Sources of Generational Persistence in Education and Income*

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Abstract

In this paper, we find that the benefits of a low-cost early-life health intervention transfer from one generation to the next. Importantly, we show that there are only significant education and income spillovers if the mothers were exposed. We provide novel evidence that improved marriage outcomes of mothers, but not fathers, are an important source of the estimated intergenerational spillovers. Moreover, we document that the early-life healthcare policy significantly increased intergenerational mobility across three generations making it a key candidate to contribute to reducing cycles of poverty.

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1 Introduction

A rapidly expanding literature documents that programs that improve children's nutrition or access to healthcare have long-lasting effects on their later health, human capital, marriage outcomes, and economic success (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). These types of programs are therefore strong candidates to investigate whether public investments in children have intergenerational spillovers. A handful of studies documents such intergenerational spillovers: if women gain access to healthcare early in life through Medicaid expansions (East et al., 2022), the Civil Rights Act (Almond and Chay, 2006), or universal home visiting programs (Baker et al., 2022) the birth outcomes of their offspring are improved. Since birth outcomes such as birth weight have been shown to positively affect future earnings (Royer, 2009; Black et al., 2007), it is likely that the benefits of these early interventions also extended to the next generation's human capital and economic outcomes.

As yet, we know very little about the long-term consequences on educational and labor market outcomes of the offspring generation and the processes by which intergenerational spillovers might occur. The transmission from the affected generation to their children could operate through a direct effect of the programs on parental resources through improved health, human capital, and income or through an indirect marriage market channel if the program has effects on assortative mating with consequences for fertility choices and child ability.

While economic status is highly persistent across generations (see, e.g., Clark, 2014; Lindahl et al., 2015; Solon, 2018), recent research suggests that at least part of this intergenerational association is causal. That is, external or policy-induced changes in parents' economic circumstances such as job loss also affect their offspring's economic success (Bastian and Michelmore, 2018; Aizer et al., 2016; Hoynes et al., 2016; Akee et al., 2010; Oreopoulos et al., 2006, 2008). In addition, some studies show effect heterogeneities concerning socioeconomic characteristics. In particular, effects are often larger for individuals from the lower end of the income and education distribution (Bütikofer et al., 2019). However, we do not know whether intergenerational spillovers are larger for disadvantaged families or whether transmission mechanisms operate differently for high and low-income families.

Research to date has either looked into questions on intergenerational spillovers (see, e.g., Barr and Gibbs, 2022; East et al., 2022; Rossin-Slater and Wüst, 2020) or genera-

tional persistence and assortative mating (see, e.g., Ermisch et al., 2006; Collado et al., 2022; Fernández and Rogerson, 2001). This paper provides a unified framework to study the effects of an early-life intervention on these important dimensions combined to get a better understanding of the complexity of generational persistence. Estimating intergenerational spillovers of policies and understanding the pathways of these spillovers has proven challenging for several reasons. One problem is exogenous variation in widespread early-life policies. Another major challenge is data availability across multiple generations.[1] We overcome these challenges by analyzing the multi-generational effects of a universal infant healthcare program using national registry data linking families across three generations, and capitalizing on variation in program exposure that resulted from Norway's staggered adoption of mother-child healthcare centers. The centers, which were established during the 1930s, 1940s, and 1950s provided free medical exams during the first year of life, as well as information about nutrition, disease, and other aspects of early child development. Bütikofer et al. (2019) document that children who gained access to the centers had substantially better health and economic outcomes in adulthood.[2] Importantly for our study, the next generation did not face the same geographic differences in healthcare access as infant healthcare became a nationwide offer before the offspring generation was born. Hence, the offspring generation faced universal healthcare access, making this an ideal natural experiment with which to explore the presence of multi-generational spillovers of early healthcare intervention and the processes that generated them.

Our paper offers three sets of key results. First, there is a substantial spillover effect on the education and income of the second generation. These spillovers are only observed from mothers to their offspring; there are no significant spillovers on individuals whose fathers were exposed. The effect on years of education for the first-generation women with children is 0.09 and for the second generation with a treated mother, it is 0.08. This results in an intergenerational persistence of about 0.89. This finding is surprising, both because of the large magnitude and because the reform had larger effects on first-generation men

[1] In light of these challenges, most of the evidence on generationally persistent effects of early life environments is based on negative in utero shocks such as disease and malnutrition (see, e.g., Almond et al., 2010; Painter et al., 2005; Richter and Robling, 2016) or exposure to radioactivity (Black et al., 2019). All of these studies find evidence that these types of extreme shocks to the in utero environment produce effects that persist to later generations. One study by Bütikofer and Salvanes (2020) considers the impacts of a health intervention that occurs in adolescence, and finds that a Norwegian tuberculosis intervention reduced generational persistence in educational attainment. Another related study by Colmer and Voorheis (2020) finds improved educational outcomes among the grandchildren of cohorts who benefited from reductions in pollution exposure following the 1970 Clean Air Act Amendments.

[2] Studies 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).

than women.

Second, our results show that the large spillover effects can be explained by a combination of how the policy affected a) the resources of parents and b) partner choice. In particular, we provide novel evidence that improved marriage market outcomes are an important source of the intergenerational spillovers we estimate by studying how the reform changed the characteristics—both innate and post-policy—of the partners of the treated individuals. We show that there are large gender differences in the choice of partner. Treated women are more likely to choose a partner with more education and higher earnings, while we do not observe this for men. This means that children of treated mothers will grow up in higher-resource families both directly through mothers' increased human capital and through the higher resources of the partner. We illustrate this by studying the reform effect on the households' total education (the sum of years of education of the treated individual and their partner's education). We find no significant change in the total household education for children who grow up in households with treated fathers. On the other hand, our findings show that a child with a treated mother grows up in a household with 0.28 years of additional education. Hence, while the first-generation effect on education of treated women is only 0.09, the household-level effect is substantially larger. Compared to the second-generation effect, this results in an intergenerational persistence of about 0.24 on the household level. The large difference between the intergenerational persistence between the mother and child of 0.89 and the household and the child of 0.24 can thus be explained by the treated mother's choice of partner.

In further analysis, we also provide empirical evidence of assortative mating on innate skills proxied by pre-reform data on the education of the parents of the partners the treated individuals choose. This education measure cannot have been affected by the reform as it is pre-determined. Reweighting the second-generation estimates by the pre-policy educational distribution of the first-generation partner, we find that up to one-third of the spillover effect can be explained by assortative mating on innate skills. Through a mediation analysis, we then show that increased resources can likely explain the rest of the spillover effect.

Third, the policy reduced intergenerational educational persistence both in the first and second generations if mothers were exposed. We find that the intergenerational correlation of years of education is reduced by 6 percent between the parents of the exposed generation and the exposed generation, while the correlation is reduced by 8 percent between the parents of the exposed generation and the exposed generations' offspring. Overall, these

results suggest that intergenerational mobility in education is significantly altered across three generations by an early-life healthcare policy. Hence, we identified a policy that can contribute to breaking cycles of poverty over generations.

The paper offers important contributions to previous literature on intergenerational persistence in education and income. First, we quantify the average intergenerational spillover effects of an early-life health intervention on the second generation's long-term outcomes. We distinguish between spillovers through exposed mothers and exposed fathers and show that it is key to separate between mothers and fathers to understand the sources of persistence. Second, we make significant progress over previous literature on the processes by which intergenerational spillovers might occur. In particular, we decompose our treatment effects into a direct component coming from the effects of the policy on family resources and an indirect component from the policy-induced changes in assortative mating. We provide empirical evidence that the gender differences in the choice of partners explain why we only find a spillover when the mother is treated. Assortative mating also explains why we observe large intergenerational spillover effects compared to the impact of the reform on the treated generation. Overall, both assortative mating on innate skills and more resources in the family are shown to contribute to the overall estimated spillovers, however, the former is the most important channel. Finally, we investigate the cycles of poverty by quantifying the effect of early life health intervention for intergenerational persistence in education across three generations. Our results demonstrate that early-life health interventions are key policy candidates for countries that are aiming at reducing intergenerational persistence in education.

2 Background and Data

2.1 Infant Healthcare 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-the-art 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 Oslo were receiving check-ups and the centers were an important part of the city's universal healthcare services (Schiøtz, 2003).

On average, a child would visit an infant healthcare 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.

By 1960, most infants had access to free infant check-ups. Starting in the 1960s, centers began to switch from private to public ownership, and since 1972, all Norwegian municipalities are obliged by law to provide public healthcare 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.

In Bütikofer et al. (2019) they studied the effect of the rollout on infant mortality and causes of death and found a substantial effect on infant mortality caused by diarrhea (see figure 1, panel D in Bütikofer et al. (2019)). These effects are consistent with the centers' main purpose of improving nutrition in children's first year of life.

While access to infant healthcare varied substantially across geographic areas during the initial rollout period, this was not the case by the time the treated cohorts gave birth to their children. Hence, our estimates will capture an indirect effect of "first generation" healthcare access on the second generation.

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, and earnings, as well as a set of demographic variables. In addition, a multi-generational register matches Norwegian children to their parents and grandparents.[3] As a result, we can link earnings, education, and birth outcome data over several generations. The historical data on the healthcare 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 healthcare centers are two surveys that the Norwegian Women's Public Health Association (NKS) sent to all healthcare 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 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 healthcare 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 rollout of the mother and healthcare centers across Norwegian municipalities between 1935–1955. Bütikofer et al. (2019) provide a more detailed description of the historical data.

2.3.1 Administrative Data

The central population register contains individuals' municipality of birth. We assign access to healthcare centers during the primary years of the centers' rollout (1936–1955)

[3]We have information on schooling for about 65% of the first generation's parents. Because this information is only available for individuals who survived until the late 1950s, however, analyses of the sample of first (second) generation individuals whose parents' (grandparents') education is available would be contaminated by selection.

based on the municipality and year in which the directly exposed generation was born.[4] We call these birth cohorts "first generation". The main sample of interest consists of the children of the first generation, whom we call the "second generation." This second-generation sample includes individuals born between 1961 and 1988 and consists of 500,765 individuals (and their parents), where 91,037 have a treated mother, 68,269 have a treated father, 205,632 have two treated parents, and 135,827 have no treated parent. Limiting the second generation to cohorts born between 1961 and 1988 simultaneously ensures that no individuals in the second generation were directly impacted by the rollout that continued until it became mandatory for municipalities to offer these services in the 1960s while including all births that occurred to the first generation between the ages of 25 and 33.[5] We focus on generational spillovers onto common measures of socioeconomic success including measures of educational attainment and earnings. We also use the education of the parents of the first generation and denote these individuals as generation zero.

Educational attainment for the first and the second generation 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 (vs. vocational) track during high school. The data are based on school reports sent directly from educational institutions to Statistics Norway, thereby minimizing any measurement error due to misreporting. For generation zero, we use education data from the 1960 Census. Note that these measures are therefore only available for first-generation individuals who are still alive in 1960.

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 generations, 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 changes in fertility patterns, partner choice, and migration decisions. We examine standard measures of infant health that are available from the Medical Birth Registry,

[4]Note that the first center opened in Oslo in 1914. Only 9 municipalities, including Oslo, had an infant healthcare center before 1936 and we therefore focus on the rollout after 1936.

[5]This sample restriction means that our analyses do not include second-generation individuals whose 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 estimates are very similar when we restrict the analyses to second-generation individuals whose mothers were older than 25 when they gave birth.

which contains records for all births since 1967.[6] 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 have the potential to influence our second-generation results either directly, or indirectly through selection into the sample.

We also investigate whether program exposure influenced marriage market outcomes using information on the partner's years of education, likelihood of enrolling in an academic high school track, average earnings, age difference, and the partner's treatment status, all of which are provided in the registries described above. As a substantial share of Norwegian couples are cohabiting 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 the population records.

Finally, we conduct several exercises that are based on municipality-level variables. First, we consider whether the program influenced the first generation's location decisions. Specifically, we consider whether an exposed parent was more likely to move to one of the seven largest urban areas in Norway before the first child was born,[7] and whether they were more likely to move out of a low-income municipality. Information on individuals' municipality of residence is available annually from the central population register. A low-income municipality is a municipality with an average income below the median. The average income for each municipality is taken from the 1930 population census.[8]

3 Empirical Strategy and Sources of Generational Persistence

Our identification strategy is based on variation in healthcare access resulting from the program's staggered rollout across municipalities. For the first generation, we estimate the

[6]Restricting the second generation sample to cohorts born after 1967 does not substantively change our main results.

[7]These urban areas are: Oslo, Bergen, Trondheim, Stavanger, Drammen, Fredrikstad, and Skien.

[8]There are four municipalities lacking information on the average income in 1930: Ski, Ringeriket, Arendal, and Hammerfest.

following reduced form model:

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

where y_{itc} are the outcomes of interest for individual i born in municipality c at time t. D_{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.[9] Individuals born a year 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 D_{ct} being 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.

Assuming that the rollout is quasi-random and that the treatment effects are homogeneous across treatment groups, Equation 1 will produce unbiased estimates. We consider the importance of this assumption using the method proposed by Callaway and Sant'Anna (2020), and obtain similar results for most of our estimates.

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 exposed mothers and exposed fathers:

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

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 healthcare 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

[9]Contrary to Bütikofer et al. (2019), who only include individuals in ever treated municipalities in their main specification, we include never treated municipalities as part of the control group to estimate the differential effect by parental treatment status in the second generation. Additionally, we classify an individual as treated if she was born in the year before a center opened because infants were eligible for the services until the age of one year. Hence, many infants born in the year before a center opening had access to infant healthcare during their first year of life. Bütikofer et al. (2019) classify individuals as treated if she was born the same year or after a center opened in their municipality of birth.

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.

In addition, we estimate a reduced form model for the second generation that considers the differential effect of the mother's and the father's treatment status for the second generation:

$$\begin{aligned} Y_{jt_mt_fc_mc_f} = \alpha_j + \eta * TreatMother_{c_mt_m} + \rho * TreatFather_{c_ft_f} + \sigma * TreatBoth_{c_mc_ft_mt_f} \\ + \beta X_{jt_mt_fc_mc_f} + \lambda_{c_m} + \lambda_{c_f} + \theta_{t_m} + \theta_{t_f} + \varepsilon_{jt_mt_fc_mc_f}, \end{aligned} \tag{3}$$

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 shows the effect of only having a mother with access to an infant healthcare center, ρ shows the effect of having only a father with access to infant healthcare, and σ shows the effect of having both parents exposed to an infant healthcare center. X_{jct}, the weights, and the clustering of the standard errors are the same as for Equation 2. Different from Equation 2, we include municipality fixed effects for the mother's and father's municipality of birth λ and cohort fixed effects for the mother's and father's birth year θ. Importantly, Equation 3 captures both the reform effect and the endogenous sorting of couples. To understand this better we produced Figure 2 which shows how we group the children by couple types. Staring at the lowest row, there are four different groups of children: those having both parents treated, those having a treated mother, those having a treated father, and those having no treated parents. If the reform affects partner choice in the second generation (second row), there is no pure control group anymore in this specification. That is, couples, where both parents are non-treated, could be different in the pre- and post-period. Indeed we will later show that this is the case. Even though we cannot interpret the estimates in this specification as a causal effect of the reform (violation of the SUTVA assumption), we can learn about the relative effects by comparing the different groups. We will also use the framework to decompose the sources of endogenous sorting (on observables) from the reform effect as we will now describe in more detail.

3.1 Potential Channels for Intergenerational Transmission

As shown in Bütikofer et al. (2019) and replicated in this paper, the reform affected the education, income, and health of the first generation. When studying the second generation, there are therefore different sources of educational inequality that can explain potential persistence in education across generations (Blanden et al., 2023). Before turning to these

sources of transmission, we discuss a few sources that are not relevant in our context. First, the first generation was treated after birth. Hence, the reform did not affect the first generation's innate skills. Second, all second-generation 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. Hence, differential healthcare access across municipalities in the second generation is unlikely to drive our results.

We distinguish between two main sources of generational persistence. First, the reform affected parental inputs such as education, health, and income. These improved parental inputs likely have a positive impact on the child's education through increased monetary investments, increased time allocations, knowledge, and information that is transmitted to the children, or changes in parental preferences or aspirations that affect children's educational choices. Second, the reform could change partner choice with implications for fertility, child skills, and household resources.

4 Results

In this section, we present our three main findings. First, we show that the first-generation results on education and earnings from Bütikofer et al. (2019) replicates in our slightly modified sample and estimation strategy. Second, we document spillover effects on the educational attainment of the second generation and reveal that there are only significant and large spillover effects for individuals with a treated mother. For individuals with treated fathers, there are no significant reform effects. Last, we present evidence that the reduction in intergenerational persistence in education found for the first generation is further reduced for the second generation. Several robustness checks confirm the findings.

4.1 Direct Effects on First Generation

Before analyzing generational persistence, we present the results for the directly affected generation—the first generation. Table 1 displays the estimated effects using Equation 1. Panel A includes only individuals born in municipalities that eventually got treated as in Bütikofer et al. (2019). Consistent with their findings, the reform increased years of education by 0.19 years which is about a 2 percent increase relative to the pre-reform mean, the likelihood that an individual completes an academic high school track increases by 3.2 pp, and average earnings between 1967-2017 are increased by about 2.4 percent. In Panel B, we show that these results are very similar if we extend the sample to also include untreated

municipalities as control groups. The effect size is slightly smaller.[10] The main reason to extend the specification to untreated municipalities is the possibility of understanding the full dynamics of the marriage market and observing treated individuals who have children with both individuals in eventually treated and never-treated municipalities. Consistent with findings in Bütikofer et al. (2019), we find that the effects are smaller for women than men (see Online Appendix Table A.1). Finally, Panel C shows that results are very similar if we restrict the sample to those who have children between 1961 and 1988 (about 60 percent of the original sample in Panel B). We make this restriction on second-generation cohorts because we aim to focus on children born when the full rollout was completed in 1960 and there was no regional variation in access to mother and child healthcare centers anymore. In addition, we cannot look at cohorts born after 1988 as we want to estimate the effect on education and income as an adult (30 and older).

Although the small differences in the coefficients between Panels B a C in Table 1 already indicate that the sample of individuals with and without children are not very different, we also estimate the effects of the reform on fertility. The results in Online Appendix Table A.2 indicate that the reform only had minor effects on the first generation's fertility. For women, there is no significant effect on ever having a child or on the age at first birth. Completed fertility, however, is slightly reduced, but the effect size is minor. In addition, the regressions for the second generation are weighted such that each first-generation parent gets equal weight (see Section 3). Hence, our results for the second generation are therefore not biased by differences in fertility across treatment and control groups.[11]

To investigate the presence of pre-trends, Online Appendix Figure A.1 displays the results from stacked event-study regressions as proposed by Callaway and Sant'Anna (2020). The plots show that there are no pre-trends for the three outcomes discussed in Table 1 and that the estimated effect size is mostly increasing year-by-year after individuals in a municipality gain access to infant healthcare. The increasing effect size is in line with the increasing uptake of the health checkups described in Bütikofer et al. (2019).

[10]The smaller effects could be because some municipalities classified as untreated also got a center from another provider than the NKS but there is no information about this in our data sources.

[11]In Online Appendix Table A.3 we show that the results are robust to limiting the sample to individuals that stay in the municipality of birth before having their first child (non-movers) and we follow a specification check that was also performed in Bütikofer et al. (2019) accounting for infant mortality by bounding the effect in Online Appendix Table A.4. Both tests suggest that the results for the first generation are robust to mobility concerns and selection due to reform effects on infant mortality for the first generation.

4.2 Spillovers to Second Generation

Table 2 presents the results for the second generation. Note that the estimations include the offspring of individuals born in eventually treated and untreated municipalities (as in Panels B and C of Table 1). In Panel A of Table 2, we split the sample by gender of the parent in the first generation. We estimate the effect of the reform using Equation 2. We see a significant spillover to the second generation of 0.08 years of education if mothers are treated.[12] Compared to a direct effect on mothers of .09 years of education (Online Appendix Table A.1, Panel D), this gives a persistence parameter of 89 percent of the original treatment effect. There are also significant positive and meaningful effects on the likelihood that the second generation completes an academic track in high school (4 percent relative to pre-reform mean) and on their average earnings from 1967-2017 (2 percent relative to pre-reform mean). Hence, the estimated persistence parameter for completed academic high school is about 67 percent of the effect on first-generation mothers. As there is a small and insignificant effect on first-generation mothers' income, the persistence parameter for earnings is very large. In Section 5, we will return to the main mechanisms behind our findings and explain the reasons for these large persistence parameters. Looking at the spillovers of fathers' reform exposure, we find no significant effects. This is despite large direct treatment effects for men with children (see Online Appendix Table A.1, Panel C). Hence, only children of exposed mothers seem to reap the benefits from parental exposure to infant healthcare. This finding is consistent with Black et al. (2005) who find that children benefit more from increases in mothers' rather than fathers' education.

Results from stacked event-study regressions as proposed by Callaway and Sant'Anna (2020) are shown in Online Appendix Figure A.2 and document that there are no pre-trends for years of education (Panel a), academic high school track (Panel b) and average earnings 1967-2017 (Panel c) for the second generation with an exposed mother. We see though a clear and significant increase in the estimated effects for the schooling outcomes for children with exposed mothers, while results for the yearly estimates on earnings are positive but not significant. Contrary to the results for children of treated mothers, the estimates for children with exposed fathers are different and not significantly positive (Online Appendix Figure A.3). Hence, the different results for children with exposed fathers versus exposed mothers are reflected in these event-study figures.

As illustrated in Figure 2, we have four types of children in the second generation: those

[12] We have also tested if spillovers are different for girls and boys in the second generation and found no significant differences. Results are available on request.

with both parents treated, those with only mother treated, those with only father treated, and those with no parents treated. Panel B of Table 2 displays the estimated effects based on Equation 3 using children with no treated parents as a control group. As couple formation might be directly affected by the reform, these results need to be interpreted with caution as we condition on a potentially endogenous variable. We still find the results useful illustration as we can compare the relative effects across groups and as we discuss potential assortative mating in Section 5.2. The results in Panel B document that the spillover to the second generation on total years of education is driven both by children having a treated mother only or both parents treated. Although the coefficient for having a treated father is also significant for the likelihood of attending academic high school, the coefficients are larger when mothers are treated. For average earnings, we only find significantly positive effects when both parents have access to infant healthcare. Overall, the results support the earlier findings that the treatment of mothers is most important for generational persistence of the reform effect. Importantly, there is substantial generational persistence through mothers even in the absence of a treated father.

Comparing the size of the estimated earnings effect of 1.4-1.7 percent to effects of other policies or studies from Nordic countries affecting similar cohorts, our findings are similar to the size of the effect of a 2 pupil reduction in class size (Fredriksson et al., 2013) or a 14 percent increase in birth weight (Black et al., 2007). Moreover, the long-term effect of job loss for similar cohorts in Norway is about 3% (Huttunen et al., 2011). Hence, the indirect effect we estimate on the second generation is substantial.

4.3 Persistence in Educational Inequality

Table 3 explores the intergenerational mobility in years of education across the three generations: the directly exposed first generation, their parents (generation zero), and their offspring (second generation). In particular, we study the direct and indirect intergenerational effects of infant healthcare access.

We regress the education of the first and the second generation on their parents' or grandparents' education including a dummy variable for the reform exposure of the first generation and an interaction term between parental/grandparental education and the reform exposure. This allows us to measure whether the reform can weaken the ties between generations in terms of educational persistence. In Panel A, we show that the reform lowers the intergenerational persistence in education between generation zero and the first generation. That is, the correlation between education of the directly exposed generation

and their parents is lower, if an individual had access to infant healthcare. The persistence in education is lowered by 6 percent in the sample of individuals with children in the second generation. Overall, this suggests that the reform had a larger impact on children from a lower socioeconomic background and is in line with the findings by Bütikofer et al. (2019). In Panel B, we focus on the link between generation zero and their grandchildren. As we only find significant spillover effects if mothers are treated, we distinguish between maternal grandfathers in Column (1) and paternal grandfathers in Column (2). While intergenerational persistence in education is substantially lower across two than across one generation (0.05 instead of 0.4), we find an estimated decrease in persistence if the mother was exposed to the reform. The persistence across two generations is lowered by 20 percent if the mother had access to infant health care as a child. For fathers, the coefficient of the interaction term is not significant. [13] This implies that after the first generation, there is an additional reduction in educational persistence for the second generation if the mother is exposed.

Our results differ from Nybom and Stuhler (2013) who document that a Swedish educational reform increased intergenerational mobility in education from parents to the directly affected children, but decreased intergenerational mobility in educational outcomes in the following generation. Our findings are more in line with Bütikofer et al. (2022) who show that the Norwegian oil boom in the 1970s lowered the persistence in intergenerational persistence across three generations. Overall, the results in this paper suggest that early-life health policies can improve economic mobility across three generations contributing to reducing the cycle of poverty.

5 Mechanisms for Spillovers

In this section, we investigate suggestive mechanisms, showing i) that health outcomes at birth are not a main channel for the longer run outcomes for the children in the second generation, ii) that the reform has a large effect on partner choice and partner characteristics, especially for females, iii) that positive assortative mating on innate skills only explains about a third of the spillover effects in education, iv) that the large persistence in education and income found between treated mothers and their children is explained by the larger treatment effect on the household (through the partner choice) than on the individual mother, and, v) that the bundle of family resources (both through treatment

[13] We control for G1 years of education, however, results are very similar without controlling.

status and partner choice) explains the major share of the spillover effects.

5.1 Birth Outcomes

Previous studies document that access to infant healthcare reduced their likelihood of experiencing metabolic syndrome and cardiovascular disease in adulthood (Bütikofer et al., 2019; Hjort et al., 2017). The presence of such health conditions among mothers, when they get pregnant, can put their offspring at risk for poor birth outcomes (Catalano and Ehrenberg, 2006) and thereby negatively impact individuals' later life education and success in the labor market (see, e.g., Almond et al., 2018; Black et al., 2007; Figlio et al., 2014). Therefore, we investigate whether the program's effect on second-generation economic outcomes is a result of program-induced improvements in their early life health. Online Appendix Table A.5 provides results from examining the program's impact on measures of the second generation's health as birth weight, the likelihood of being born below the low birth weight threshold (2500 grams), and the likelihood of being born prematurely. We find no evidence of intergenerational spillovers on second-generation birth outcomes. The lack of persistent health effects may reflect the continued evolution of healthcare in Norway: by the 1960s, infant check-ups following national guidelines had been established in all municipalities, and the vast majority of births occurred in hospitals. Hence, the regional variation in infant healthcare that we exploit in the first generation is evened out for the second generation. The second generation's equal healthcare access may therefore partly compensate for differences between treated and untreated mothers' health and human capital.[14]

5.2 Partner Choice and Assortative Mating

In Table 4, we investigate whether the reform affects the type of partner treated individuals choose. We look at a range of characteristics of the partner including total years of education, academic high school track, average earnings 1967-2017, relative age, whether the partner is also treated, and whether the partner was born in an urban area. In Panel A, we see that treated women are more likely to have children with higher-educated partners and partners with higher average earnings. In addition, the age gap between the two partners is smaller for treated women and they are more likely to choose a partner who

[14]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 spillover results in Section4.2.

is treated. Overall, treated women have children with partners with higher financial and human capital resources. In Panel B, we show that this pattern is different for men. They do not choose better-educated or higher-earning women to have their children with. The only significant differences between treated and untreated men in their choice of partner are that the partner is more likely to be treated and more likely to be born in an urban area. These results indicate that the reform led to large gender differences in the type of partner men and women choose. Women choose higher-resource men, men do not seem to react the same way. In sum, children of treated mothers will grow up in higher-resource families both directly through their increased human capital and through the higher resources of their partner.

Nevertheless, all of these partner characteristics are measured after the reform. Hence, the increased resources could be a combination of drawing a partner from a different place in the innate skill distribution or the same type of latent partner who was also treated and therefore has higher education and income. To measure innate skills that were determined before the individuals gained healthcare access, we use two different proxy variables: the education of the parents of the first generation and whether the first generation is born in a poor municipality. Table 5 shows that there is positive assortative mating on innate skills for treated mothers only, proxied by the education of the partner's parents (G0). We see no differences between the innate skills of partners between treated and untreated fathers consistent with the results in Table 4. Moreover, we do not find a significant impact on the likelihood that the partner is born in a poor area. In sum, this indicates that treated women positively select their partners also on pre-determined characteristics not directly affected by the reform.

To determine to what extent this selection process based on innate characteristics explained our findings, we re-estimate the effects on the second generation by weighing observations by the pre-policy education distribution of the treated parent's partner. This gives a decomposition of how much of the effect of the second generation is due to the sorting in the marriage market on the pre-determined education distribution. Online Appendix Table A.6, displays the results. The effects on years of education and academic high school track are about a third lower when using the weights and less precisely estimated. The estimates for earnings are more similar to previous estimates but less precise. Hence, while there is some indication that the spillover effect on years of education is driven by positive assortative mating, better innate characteristics of partners cannot explain the total spillover effects we find.

5.3 Changes in Family Resources and Interpreting the Persistence Parameter

So far, we demonstrated that exposed mothers are having children with more positively selected partners both in terms of innate skills but also in terms of education and income that might be influenced by the policy change. Hence, the spillover effects on the second generation might also be driven by policy-induced changes in family resources. In Table 6, we investigate this by taking the perspective of the second generation and estimating the effect of the reform on the total resources of the household second-generational individuals grow up in.[15] The results show striking benefits of having a treated mother. While we do not find any significant effects of individuals with a treated father, individuals with a mother who had access to a mother-child healthcare center grow up in a household with significantly higher total household education (1 percent), academic high school track choices (6 percent), and household average earnings (2 percent).

Importantly, the effects on the household level for individuals with a treated mother are substantially larger than the coefficients for the mothers' outcomes alone. That is, we likely overestimate the generational persistence of the reform effect when focusing on the effects on mothers' outcomes only and do not consider mothers' partner choice. Hence, we compare the results for the second generation (from Panel A of Table 2) to effects on the households' outcomes in Table 6 to adjust the generational persistence parameters for assortative mating. Considering the impact of the reform on the household, the generational persistence of the policy for years of education is .29 (.08/.28), for academic high school track it is .62 (2.16/3.46), and for earnings is .47 (4959/10664). This shows that the large persistence effects when comparing the effect on children to treated mothers were driven by the partner choice. While these findings highlight the importance of accounting for assortative mating when interpreting intergenerational policy spillovers, there is still a substantial generational persistence of the policy effect when taking partner choice into account that should be considered in cost-benefit analysis for early-investment policies.

Overall, the persistence parameter we estimate for education is similar to correlation coefficients between the completed years of education of children and the years of education of their parents in Nordic countries (see, e.g., Black et al., 2005; Karlson and Landersø, 2021). In addition, our estimate is also in the same ballpark of the generational persistence

[15]Note that taking the perspective of the second generation to look at outcomes for the first generation will is somewhat different from studying the first generation directly due to households with multiple siblings.

Black et al. (2019) estimate for the effect of in utero exposure to nuclear fallout on cognitive ability.

In the last step, we perform a mediation analysis where we estimate Equation 2 and step-wise control for different parental and grand-parental characteristics. Table 7 shows the results. In Panel A, we control for the education of grandparents—our proxy for innate skills. The estimated effects do not change much. This confirms our earlier result in Section 5.2 that sorting on innate skills exists but is not the main driver for our results. In Panel B, we control for parental education. Controlling for education lowers all estimated spillover effects of the reform substantially and almost none of them is still significant. Controlling for household earnings also lowers the estimated spillover effects of the reform (Panel C), but less than for education. There are still significant spillover effects if mothers are treated. When adding all of the controls from Panels A–C, there are few spillovers left. Hence, including the potential channels through which the generational might occur reduces the spillover effects substantially and suggests that financial and human capital resources through both individual reform effects and partner choices are important drivers of intergenerational persistence in reform effects.

6 Conclusion

Do the benefits of early-life health interventions transmit across generations and thereby decrease intergenerational persistence in education? Both researchers and policymakers are increasingly intrigued by the potential intergenerational effects of policy interventions—in particular early-life interventions for which public investments are increasing. However, there are limited opportunities to empirically measure the intergenerational spillovers, the channels through which they occur, and their implications for intergenerational mobility in education. The rollout of mother and child healthcare centers in Norway that offered infant check-ups offers a unique opportunity to explore these questions. The centers were established in the 1930, 1940, and 1950s and, therefore, substantial time elapsed since its initial implementation to study generational spillovers. While there is substantial geographic variation in healthcare access for infants during the initial rollout, infant healthcare becomes a nationwide offer before the offspring generation is born. Hence, the offspring generation faces a different regional distribution of healthcare access, making this an ideal natural experiment with which to explore the presence of multi-generational spillovers and the processes that generate them. In addition, the Norwegian registry data allows us to link

individuals across three generations to their mothers and fathers. This allows us to present novel evidence regarding the intergenerational transmission of effects resulting from early childhood health intervention, the mechanisms behind the spillovers, and the consequences of the policy economic mobility across three generations.

We find compelling evidence indicating that the positive impacts of access to infant check-ups are transmitted across generations. Our results suggest a consistent pattern of increased educational achievements and earnings both in the directly affected first and the indirectly affected second generation. These effects translate to an estimated 1% improvement in years of education. Importantly, we show that there are only significant intergenerational spillovers if the mothers were exposed. For children with exposed fathers, there are no significant spillovers. This asymmetry suggests that the increased educational attainment and earnings of parents, stemming from their access to infant healthcare, cannot be the only channel by which the policy benefits are transmitted. We provide novel evidence that improved marriage market outcomes of mothers, but not fathers are an important source of the intergenerational spillovers. Treated individuals are more likely to have children with better education and higher-earning partners from families with higher socioeconomic backgrounds. Hence, it is important to account for partner choice when estimating the generational persistence of reform effects—while the second-generation education effect is 89 percent of the education effects on mothers, it is only 29 percent of the education effect of the household in total. Nevertheless, the generational persistence of the policy effect is still substantial when taking partner choice into account and important for cost-benefit analysis when making policy decisions. Last, we document that the policy reduced intergenerational educational persistence in the first and second generations. Overall, these results suggest that intergenerational mobility in education is significantly altered across three generations by an early-life healthcare policy.

The key takeaways are that the benefits of a low-cost early-life health intervention appear to transfer from one generation to the next, that mothers and their choice of partners is an important channel, and that such policies with large benefits for individuals from poorer backgrounds have the potential to lower the persistence in education across three generations. Hence, the availability of infant healthcare appears to have effectively disrupted the cycle of poverty. Overall, these findings suggest that cost-benefit analyses of early childhood interventions undervalue the benefits by disregarding the transmission of positive effects across generations which has important policy implications for the optimal allocation of resources to such programs.

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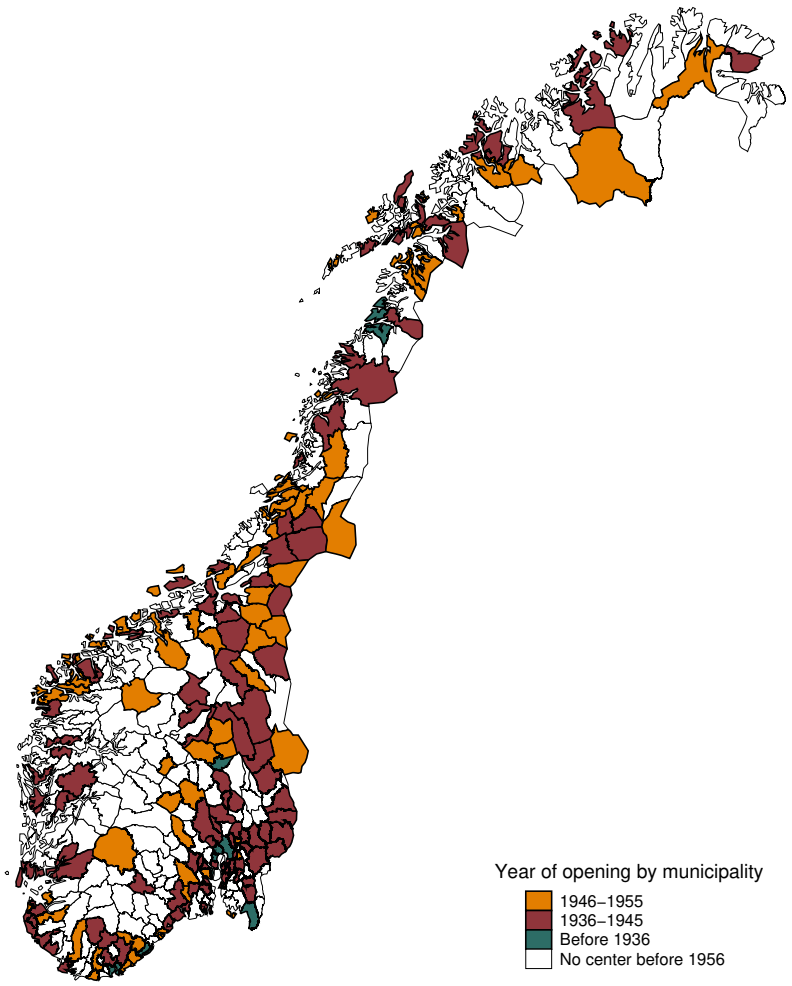
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7 Tables and Figures

Figure 1: Rollout of Mother and Child Healthcare Centers



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 in the period of interest.

Figure 2: Family Types

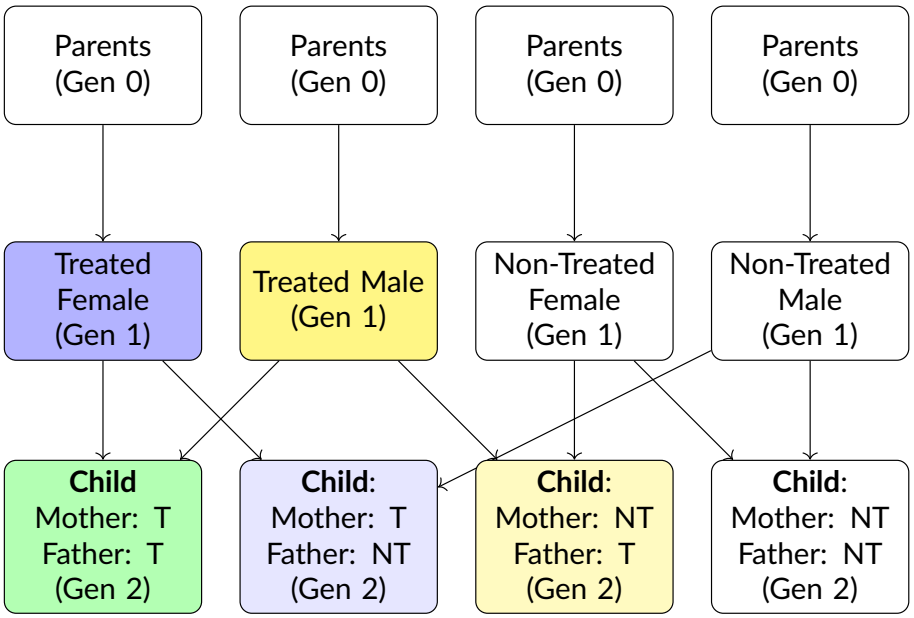


Table 1: First Generation Education and Labor Market Outcomes

	Education registry		Earnings registry
	Total years of education	Academic high school track	Average earnings 1967–2017
	(1)	(2)	(3)
	Panel A: Eventually treated municipalities		
Center exposure	.19***	3.23***	4909.2***
	(.04)	(.85)	(1715.4)
Observations	348,511	348,511	349,741
Pre-reform mean	11.1	32.0	207981
	Panel B: Untreated municipality as control group		
Center exposure	.14***	1.85***	4436.2***
	(.04)	(.62)	(1660.7)
Observations	781,355	781,355	784,365
Pre-reform mean	11.5	38.0	227212
	Panel C: Sample with children in second generation		
Center exposure	.16***	2.40***	4916.0**
	(.05)	(.75)	(2089.0)
Observations	442,134	442,134	442,842
Pre-reform mean	11.9	45.6	253339

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Earnings presented are 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. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 2: Second Generation Education and Labor Market Outcomes

	Education registry		Earnings registry
	Total years of education	Academic high school track	Average earnings 1967–2017
	(1)	(2)	(3)
	Panel A: Separate groups		
Treated mother	.08**	2.16***	4959.2**
	(.03)	(.65)	(2162.9)
Observations	492,207	492,207	492,872
Pre-reform mean	12.9	57.6	303783
Treated father	.01	-.14	1397.2
	(.03)	(.59)	(1868.6)
Observations	492,207	492,207	492,872
Pre-reform mean	12.8	56.3	299546
	Panel B: All groups		
Treated mother/untreated father	.10***	3.18***	3281.1
	(.03)	(.71)	(2029.9)
Treated father/untreated mother	.04	1.16*	-2344.3
	(.03)	(.65)	(2384.4)
Both parents treated	.07**	1.81**	5079.0**
	(.04)	(.77)	(2288.6)
Observations	492,207	492,207	492,872
Pre-reform mean	12.9	55.9	305094

Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for the treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, second generations year-of-birth fixed effects, and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 3: Policy-Induced Changes in Intergenerational Mobility in Education

	Total years of education	
	Panel A: Generation 0 to 1	
Father's years of education	.406***	
	(.007)	
Center exposure	.427***	
	(.106)	
Center exposure×Father's years of education	-.023***	
	(.009)	
Observations	291,126	
	Panel B: Generation 0 to 2, controlling for G1 education	
	Maternal grandfather	Paternal grandfather
Maternal/or paternal grandfather's education	0.054***	0.044***
	(0.004)	(0.004)
Treated mother/or father/untreated	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	502,451
	Panel C: Generation 0 to 2, controlling for G1 education	
	Maternal grandfather	Paternal grandfather
Maternal/or paternal grandfather's education	0.039***	0.029***
	(0.005)	(0.004)
Treated mother/or father/untreated	0.077	-0.040
	(0.061)	(0.045)
Treated mother/or father×G0's education	-0.005	0.004
	(0.005)	(0.005)
Observations	309,409	323,290

Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for the treated generation are shown in parentheses. Second-generation regressions are weighted by the number of siblings. 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. Column 1 in Panel A represents the full sample. Column 2 in Panel B represents a sample of first-generation individuals with children in the second generation. Column 1, Panel B represents intergenerational estimates for children with treated versus untreated mothers, and Column 2, Panel B represents intergenerational estimates for children with treated versus untreated fathers. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 4: First Generation Partner Characteristics

	Total years of education (1)	Academic high school track (2)	Average earnings 1967–2017 (3)	Relative age (4)	Partner treated (5)	Born in urban area (6)
	Panel A: Mother's partner					
Center exposure	.11**	1.26*	6119.74**	.06	14.77***	0.41
	(.05)	(.70)	(2848.77)	(.05)	(1.62)	(.68)
Observations	221,225	221,225	222,174	222,295	222,295	222,295
Pre-reform mean	11.9	38.1	316968	2.1	37.4	14.2
	Panel B: Father's partner					
Center exposure	.03	.38	737.43	.17***	12.75***	1.82***
	(.03)	(.678)	(1235.78)	(.05)	(1.32)	(0.58)
Observations	220,238	220,238	220,299	221,037	221,037	221037
Pre-reform mean	11.3	42.3	154146	-2.7	42.1	14.2

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Sample is based on individuals with children in second generation (born between 1961–1988). Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). Earnings presented are 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. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 5: Selection on Innate Skills of Partner

	Partner G0 education (1)	Partner parent's born in poor area (2)
Treated mother	.14**	-.01
	(.05)	(.01)
Observations	237,393	359,687
Treated father	.05	.01
	(.07)	(.01)
Observations	237,393	359,687

Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for the treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, second-generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 6: Second Generation: Household Resources

	Households total years of education (1)	Household academic high school track (2)	Household average earnings 1967–2017 (3)
Treated mother	.28**	3.46***	10664.13**
	(0.12)	(1.05)	(4769.22)
Observations	493,426	493,426	493,426
Pre-reform mean	23.1	55	464134
Treated father	-.01	-0.53	-799.84
	(.10)	(.85)	(3151.72)
Observations	493,426	493,426	493,426
Pre-reform mean	23.1	55.4	462170

Notes: Each column is from a separate regression on access to a mother and child healthcare center for the treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for the treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 to 1955 (Oslo in 1914). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, second-generations year-of-birth fixed effects, and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table 7: Mediation Analysis: Second Generation Education and Labor Market Outcomes

	Education registry		Earnings registry
	Total years of education	Academic high school track	Average earnings 1967–2017
	(1)	(2)	(3)
	Panel A: Controlling for G0 education		
Treated mother	.11***	3.13***	5648.11**
	(.03)	(.78)	(2301.46)
Observations	492,207	492,207	492,872
Treated father	.02	.22	1652.4
	(.03)	(.74)	(2006.55)
Observations	492,207	492,207	492,872
	Panel B: Controlling for G1 education		
Treated mother	.03	.99*	3779.29*
	(.03)	(.60)	(1966.84)
Observations	492,207	492,207	492,872
Treated father	.01	-.10	1413.21
	(.02)	(.45)	(1654.74)
Observations	492,207	492,207	492,872
	Panel C: Controlling for G1 earnings		
Treated mother	.05*	1.47**	3436.46*
	(.03)	(.59)	(1921.62)
Observations	492,207	492,207	492,872
Treated father	.02	-.01	1611.35
	(.02)	(.50)	(1648.17)
Observations	492,207	492,207	492,872
	Panel D: Controlling for G0 education, and G1 education and earnings		
Treated mother	.03	1.17**	2877.91
	(.03)	(.56)	(1823.87)
Observations	492,207	492,207	492,872
Treated father	.02	.03	1458.41
	(.02)	(.44)	(1535.98)
Observations	492,207	492,207	492,872

Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for the treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, this second-generations year-of-birth fixed effects, and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

A Online Appendix

Table A.1: First Generation Education and Labor Market Outcomes by Gender

	Education registry		Earnings registry
	Total years of education	Academic high school track	Average earnings 1967–2017
	(1)	(2)	(3)
	Panel A: Men		
Center exposure	0.20***	2.27***	6957.92***
	(.05)	(.61)	(2585.84)
Observations	397,364	397,364	399995
Pre-reform mean	11.8	36.9	302303
	Panel B: Women		
Center exposure	.07*	1.46*	1606.45
	(.038)	(.78)	(1160.32)
Observations	382,287	382,287	382,670
Pre-reform mean	11.2	39.2	149567
	Panel C: Men with children in second generation		
Center exposure	0.18***	1.91***	3759.68
	(.06)	(.69)	(2403.043)
Observations	220,307	220,307	221,043
Pre-reform mean	12.3	43.9	339312
	Panel D: Women with children in second generation		
Center exposure	.09**	2.93***	1372.86
	(0.05)	(1.1)	(1493.99)
Observations	220,899	220,899	220,871
Pre-reform mean	11.6	47.3	167660

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). Earnings presented are 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. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.2: First Generation Fertility

	Ever having a child (1)	Completed fertility (2)	Age at first child (3)
	Panel A: Men		
Center exposure	.70*	-.04**	-.2*
	(.39)	(.02)	(0.11)
Observations	400,916	343,502	343,502
Pre-reform mean	85.4	2.5	26.6
	Panel B: Women		
Center exposure	-.09	-.05**	-0.12
	(.27)	(.02)	(.08)
Observations	387,515	349,927	349,927
Pre-reform mean	90.8	2.5	23.5

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936 and 1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). All outcomes, except "Ever having a child", are based on a sample of individuals having a child. All specifications include a full set of cohort and municipality fixed effects. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.3: First Generation Education and Labor Market Outcomes: Non-Movers

	Education registry		Earnings registry
	Total years of education (1)	Academic high school track (2)	Average earnings 1967–2017 (3)
Center exposure	.16***	1.96**	4530.89**
	(.05)	(.93)	(2000.34)
Observations	371,686	371,686	372,205
Pre-reform mean	11.3	35	229068

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). Earnings presented are 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. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.4: Infant Mortality Bounds: First Generation's Total Years of Education

	Baseline	Dropping 60th percentile	Dropping 70th percentile	Dropping 80th percentile	Dropping 90th percentile	Dropping 100th percentile
	(1)	(2)	(3)	(4)	(5)	(6)
Center exposure	.16***	.16***	.16***	.14***	.15***	.16***
	(.05)	(.05)	(.05)	(.05)	(.05)	(.05)
Observations	442,134	437,689	438,396	437,595	437,658	439,304

Notes: Each column is from a separate regression on access to a mother and child healthcare center. Robust standard errors adjusted for clustering at the level of the municipality of birth are shown in parentheses. Birth cohorts are born between 1936–1955. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, municipality-specific pre-treatment trends, and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.5: Second Generation Health Outcome

	Birth weight	Low birthweight	Premature
	(1)	(2)	(3)
Treated mother	2.36	-.11	-.09
	(7.91)	(.23)	(.33)
Observations	422,246	422,246	406,136
Pre-reform mean	3505	4	8.6
Treated father	2.50	-.25	.12
	(7.46)	(.20)	(.29)
Observations	422,246	422,246	406,136
Pre-reform mean	3501.5	4.2	8.9

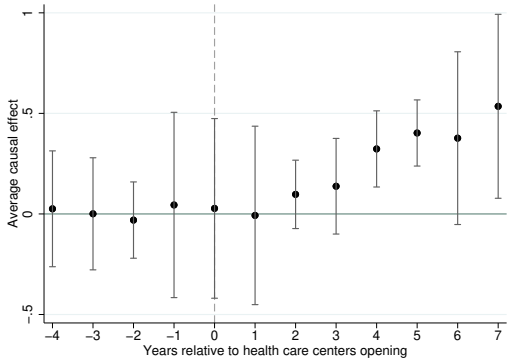
Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). All specifications include a full set of cohort and municipality fixed effects, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.6: Second Generation Education and Labor Market Outcomes: Weighted

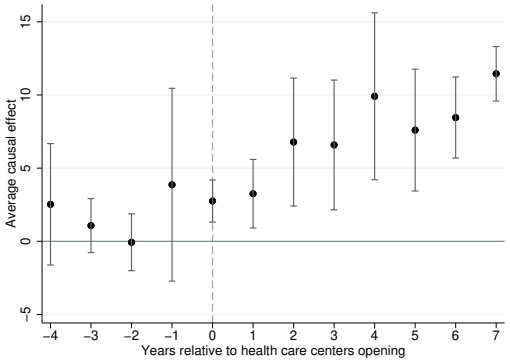
	Education registry		Earnings registry
	Total years of education	Academic high school track	Average earnings 1967–2017
	(1)	(2)	(3)
Treated mother/untreated	.05	1.9*	4459.25
	(.04)	(1.05)	(2788.8)
Observations	309,409	309,409	309,708
Treated father	.00	0.01	2362.05
	(.03)	(.82)	(1999.94)
Observations	323,290	323,290	323,592

Notes: Each column is from a separate regression on access to a mother and child healthcare center for treated generation. Robust standard errors adjusted for clustering at the level of the municipality of birth for treated generation are shown in parentheses. All regressions are weighted by the number of siblings. Birth cohorts for second generation are born between 1961–1988. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Earnings presented are average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, this generations year-of-birth fixed effects and a dummy variable controlling for the gender of the individual. Significance levels: *** 1% level, ** 5% level, * 10% level.

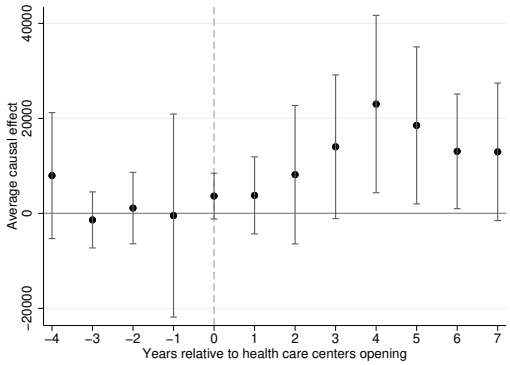
Figure A.1: First Generation Education and Labor Market Outcomes: Stacked Event-Study Regressions using Callaway and Sant'Anna



(a) Total years of education

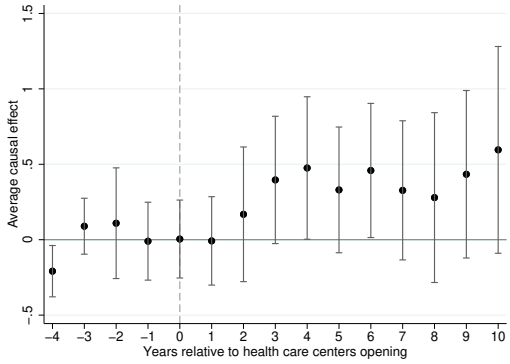


(b) Academic high school track

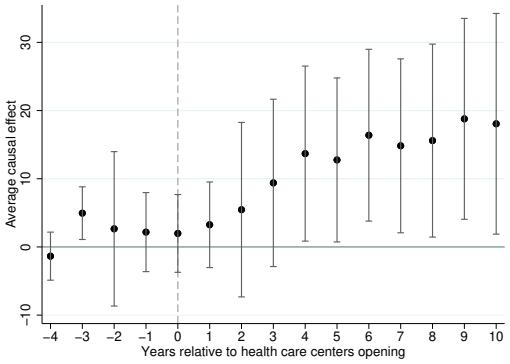


(c) Average earnings 1967–2017

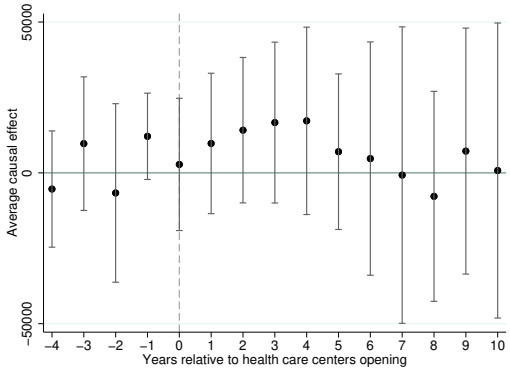
Figure A.2: Second Generation Education and Labor Market Outcomes for Treated Mothers: Stacked Event-Study Regressions using Callaway and Sant'Anna



(a) Total years of education

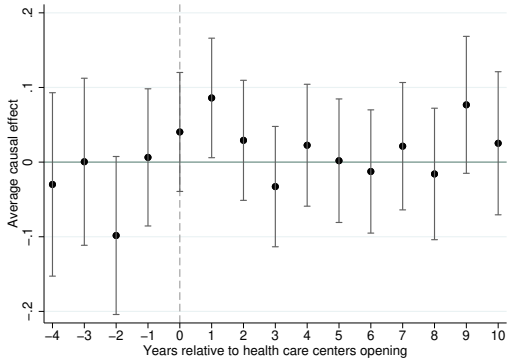


(b) Academic high school track

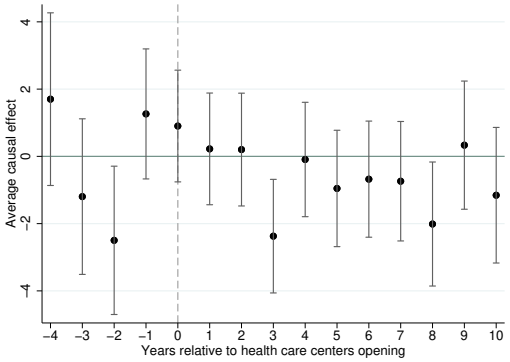


(c) Average earnings 1967–2017

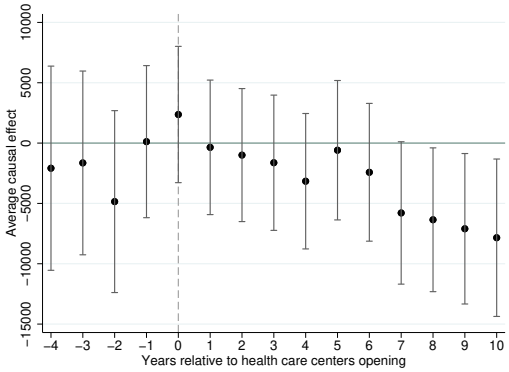
Figure A.3: Second Generation Education and Labor Market Outcomes for Treated Fathers: Stacked Event-Study Regressions using Callaway and Sant'Anna



(a) Total years of education



(b) Academic high school track



(c) Average earnings 1967–2017