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# Infant Health, Cognitive Performance and Earnings:

## Evidence from Inception of the Welfare State in Sweden

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**Abstract**

We estimate impacts of exposure to an infant health intervention trialled in Sweden in the early 1930s using purposively digitised birth registers linked to school catalogues, census files and tax records to generate longitudinal data that track individuals through four stages of the life-course, from birth to age 71. This allows us to measure impacts on childhood health and cognitive skills at ages 7 and 10, educational choice during young adulthood, employment, earnings and occupation at age 36–40, and pension income at age 71. Leveraging quasi-random variation in eligibility by birth date and birth parish, we estimate that exposure was associated with substantial increases in earnings and (public sector) employment among women, alongside no improvements for men. This appears to be related to the intervention having made it more likely that primary school test scores for girls were in the top quintile of the distribution and, related, that they attended secondary school. The greater investments of women in education are consistent with their comparative advantage in cognitive tasks, but opportunities are also likely to have played a role. Our sample cohorts were exposed to a massive expansion of the Swedish welfare state, which created unprecedented employment opportunities for women.

**Keywords**: Infant health; early life interventions; cognitive skills; education, earnings, occupational choice, programme evaluation; Sweden

**JEL classification**: I15; I18; H41

## 1 Introduction

A growing literature documents impacts of early life health on socio-economic status in adulthood. While earlier studies modelled the impacts of shocks such as famine or influenza (Almond, 2006; Lindeboom et al., 2010), more recent studies have investigated the large-scale roll out of health interventions. We contribute to this body of research. A possibly unique contribution of this paper is that, using linked administrative data, we track individuals from birth through to retirement and observe their outcomes at different stages of the life course. In particular, we observe cognitive performance in primary school, the decision to enroll in secondary school which was the relevant margin of higher education for the sample birth cohorts, and earnings, employment and occupation in adulthood. We also observe pension income. There is growing recognition of the importance of identifying endogenous outcomes earlier in the lifecourse that may link early life health to earnings (Almond and Currie, 2011; Heckman et al., 2014; Falk and Kosse, 2016). However, it remains unusual to have data in which individuals are tracked from birth through school and into the labour market. Another feature of this paper is that it investigates distributional effects (for test scores and income), and this turns out to be a useful complement to studying mean effects in terms of explaining our finding (detailed below) that intervention-led earnings increases were restricted to women.

The intervention we analyse was a significant pillar in the emergence of the welfare state in Scandinavia. Spurred by cessation of infant mortality decline, the Swedish government trialled a mother-baby programme from 1 October 1931 to 30 June 1933. This was positively reviewed by physicians at the time, influencing the roll out of similar nationwide programmes in each of the three Scandinavian countries in the mid-1930s (Hjort et al., 2014; Bütikofer et al., 2016).

The intervention had a home visiting component, similar to that of the Nurse Family Partnership type programmes in the UK, USA and Canada but, in contrast to these programmes, it was universal. It provided information, support and monitoring of newborn health, including encouragement of breastfeeding, sanitation and a healthy diet. There is growing emphasis on universal health coverage (Gorna et al., 2015). Similar early childhood home visiting programmes are increasingly being introduced in developing countries, but there are few systematic evalations of immediate or long run impacts (Engle et al., 2007).

We purposively digitised individual birth certificate data from historical parish records to obtain a census of births in every treated parish (and city), and a set of matched controls. The birth sample contains about 25,000 births during 1930–1934 in 114 rural parishes and 4 cities. With reference to a range of economic and demographic indicators, this sample was representative of the country in 1930. Individuals are identified by first name, last name, exact birth date and place (parish) of birth.

Using these identifiers, we link the birth records to administrative school records, the 1970 census files and official tax registers. Primary school catalogues were gathered in paper format from regional archives and digitised to provide information on subject-specific school grades and sickness-related school absences for grades 1 and 4 (age 7 and 10). The 1970 census provides data on labour market outcomes at ages 36–40 and pension income at age 71 is obtained from tax registers. We match 66% of the birth sample to school data and 86% of the birth sample to the 1970 census files. Attrition is not differential by treatment status.

We primarily exploit the intervention eligibility critera to identify causal (intent-to-treat) impacts of exposure to the programme. Children aged 0–12 months at any time in the window for which the programme was available were eligible, for durations that varied with their exact date of birth. We use the (pre-intervention) 1930 census to create matched controls at the parish level, and confirm that pre-trends were not differential by treatment status. In contrast to related studies, we analyse an intervention that was explicitly announced as a trial of less than two years duration.[1] This implies that endogenous fertility and migration are likely to have played a limited role, if any.

The analysis identifies large and robust impacts of the infant health intervention on labour market outcomes at ages 36–40. We estimate that eligibility for a year in infancy enhanced earnings by 7.3% on average. However, this is completely driven by women who experienced an increase in earnings of about 19.5%, in contrast to no gain among men.[2] We estimated unconditional quantile treatment effects for income following Firpo et al. (2009). We see no gains anywhere in the distribution for men, and that income gains for women are concentrated in the upper part of the distribution. We estimate an intervention-led increase in the probability that women belong to the top earnings quintile of 8 percentage points.[3]

A reason that the average income impact is so large is that some of it arises from extensive

[1]Analyses of the nationwide rollout of a similar programme in Denmark (Hjort et al., 2014) and Norway (Bütikofer et al., 2016), of food stamps in the USA (Hoynes et al., 2016), or of the Family Health Programme in Brazil (Bhalotra et al., 2016) need to contend with the play of other events that may have created divergent trends across areas treated earlier vs later. In particular, the cited studies rely upon staggered rollout of a programme over several years, which is potentially endogenous.

[2]Since earnings for women age 36–40 are potentially sensitive to career disruptions related to child bearing or rearing, we also examine pension income at age 71. For our cohorts pensions mirror the best fifteen years in the labour market and thus represent earnings at advanced stages of the career. Again we find treatment impacts for women, and not men.

[3]Outcome quintiles are defined throughout using as reference the distribution of ineligible individuals.

margin changes, that is, from the intervention having increased employment among women. We estimate that an additional year of eligibility increased the probability that women worked full time by 7.6 percentage points, which is an increase of 20.5%. In the pre-intervention period, 92.5% of men in contrast to 37% of women were employed, and programme exposure produced no significant change in employment among men.

Further analysis reveals that almost all of the additional employment among women was in the public sector. A fascinating feature of the context in which our study is located is that, at the time our sample cohorts were making decisions about higher education and employment, Sweden was experiencing a rapid expansion of publicly provided childcare, health and education. This benefited women in two ways, raising both supply of and demand for women's work. First, the expansion of childcare and health facilities encouraged increased labour force participation among married women.[4] Second, it created jobs that were dominated by women- nursery workers, nurses and other sorts of health care workers, and administrative work that will have expanded to support this growing enterprise. While the infant health intervention raised skills among women, realization of the full impact of the health intervention on their long run economic position was very likely facilitated by favourable demand conditions.

Investigating changes in occupational sorting as a function of intervention eligibility, we find significant increases in the chances that women were in the highest-ranked occupations in terms of skills as measured by a task-based classification (Autor et al., 2003), average GPA and secondary school completion. In particular, women were 5 percentage points (29.4% relative to the baseline mean) more likely to work as managers and professionals and 4.4 percentage points (35.5%) more likely to work in accounting, banking and administration. Disaggregating these occupational categories, we find that more than half of the increase in women's employment as managers and professionals was as health care workers, and almost all of their increased participation in the accounting, banking and administration category was as office workers (administrators).

Leveraging the linkage we have to intermediate outcome data on skill acquisition, we directly

[4] Datta Gupta et al. (2006); Bergh (2009), and Stanfors (2003) discuss the expansion of pre-school childcare and the growth of married women's labour force participation in Sweden. Bhalotra et al. (2018) show that sharp declines in child mortality and morbidity in the 1930s in the United States led women to delay fertility and increase their labour market engagement. In general, there may also be direct impacts of improvements in the health of women on their labour supply.

investigated the extent to which the intervention increased skills.[5] We estimate that a year of exposure to the infant health intervention was, on average, associated with a 0.08 standard deviation (s.d.) improvement in school test scores, driven by a 0.11 s.d. increase in reading and writing skills at age 10. This is large relative to many other interventions.[6] Mean impacts were larger and only significant for boys (0.17 s.d. in reading, 0.13 in writing), for whom significant impacts are evident through most of the distribution.[7] However, girls exhibit larger increases in GPA than boys in the upper regions of the distribution. The intervention is estimated to have increased the chances of scoring in the top quintile by a significant 12.4 percentage points for girls, in contrast to an imprecisely determined 2.75 percentage points for boys.

The distributional results potentially illuminate the mechanisms at play. In this era, primary school attendance was universal by mandate (Fredriksson et al., 1971). However only about a fifth of all children progressed into secondary school, with the others undertaking vocational training or joining the labour market (SCB, 1977). Secondary schooling was therefore an important margin for the sample cohorts. Entry to secondary school was competitive and rationed, and we show that the chances of entry increased sharply at the top of the GPA distribution. In line with this we find intervention effects on secondary schooling that favoured girls. A year of exposure is associated with a significant 3.5 percentage point increase in secondary schooling for girls, and no change for boys.

Aggregate data show that the high-ranking occupations that the intervention spurred women into have a particularly high share of workers with secondary schooling. Using the individual data, we document a large overlap in intervention-related attainments sequenced over the life-course: individuals (women) who had a primary school GPA in the top quintile overlapped with women who completed secondary schooling and each of these groups overlapped with women that entered high-ranking occupations and women who experienced an increase in the probability of being in the top quintile of earners. With the important caveat that it is only descriptive, a decomposition-style exercise following Gelbach (2016) suggests that secondary schooling was a critical lever, linking test score gains arising from the infant health intervention to earnings

[5]Infancy is a period of rapid neurological development and there is evidence that net nutrition (including breastfeeding, clean water, reduced infections) in infancy can influence brain development (Doyle et al., 2009; Eppig et al., 2010; Deverman and Patterson, 2009), creating a biological mechanism for causal effects of infant health on cognition. This may be reinforced by parental investments (as in Bhalotra and Venkataramani, 2013).

[6]For instance, Baird et al. (2014) surveyed the impact of cash transfers on test scores. They conclude that impacts range between 0.04 to 0.08 s.d. across five studies, and are consistently not statistically significant.

[7]Boys had lower baseline skills than girls.

increases.[8]

Our results contribute to several strands of the literature. First, they contribute to a scarce literature providing evidence that cognitive performance contributes to earnings. Some recent studies suggest that pre-school programmes such as Project STAR and the Perry intervention may have raised long term earnings by generating sustained improvements in health and non-cognitive rather than cognitive skills (Chetty et al., 2011; Heckman et al., 2013, 2006). Thus, although a vast body of research in economics and biology documents long run benefits of early life health interventions on earnings (Almond and Currie, 2011; Bütikofer et al., 2016; Bhalotra and Venkataramani, 2013), and it is implicit that the intervening mechanism is human capital accumulation, there remains limited evidence of the importance of cognitive skills in this process. A reason for this is that few previous studies have been able to link data on skill acquisition and early career choices to earnings in adulthood.[9]

We also contribute to a still small body of evidence that early life health interventions impact cognitive attainment.[10] Research and policy concerned with improving cognitive attainment has paid increasing attention to the pre-school environment, including parenting styles, caregiver quality and the role of stimulation (Heckman, 2006; Attanasio et al., 2014; World Bank, 2015). Our estimates suggest that pre-school health interventions have the potential to raise cognitive attainment as much as many of the interventions that directly target cognitive capacity. This is of enormous significance. There is an ongoing global learning crisis affecting the developing world as well as poor families in developed countries with millions of children failing to attain their

[8] OLS estimates of the earnings returns to secondary schooling show that these were substantial, and larger for girls.

[9] The seminal study of Black et al. (2007) use twin-comparisons to show that IQ and earnings in adulthood are both increasing in birth weight. Although, like Black et al. (2007), we are unable to directly estimate a causal impact of cognition on earnings, we show that both cognition and earnings respond to the intervention. We show that school test scores (rather than IQ in adulthood) influence adult income, partly through influencing secondary school completion.

[10] Chay et al. (2009) study black-white convergence in test scores as a function of hospital de-segregation in America, Bharadwaj et al. (2013) show impacts of neonatal care facilities on school test scores in Chile and Norway, Bhalotra and Venkataramani (2013) demonstrate impacts of infant exposure to a clean water programme in Mexico on cognitive attainment in middle and late adolescence, and Almond et al. (2009) show that in utero exposure to (accidental) radiation from the Chernobyl disaster influenced cognition. Other studies that analyse impacts of early life health rather than of health interventions or shocks, include Black et al. (2007) and Figlio et al. (2014), who use twin or sibling estimators to identify impacts of birth weight on later outcomes including cognitive performance in Norway and Florida respectively. Like Figlio et al. (2014), we are able to assess impacts of infant health on cognitive scores at different ages and by the socio-economic characteristics of parents. However, while they analyse impacts of birth weight differences, we use population-level exposure to an intervention that improved infant health. This is important because, as Figlio et al. (2014) state, *"While we have strong evidence from twin comparison studies that poor initial health conveys a disadvantage in adulthood, we have little information about the potential roles for policy interventions in ameliorating this disadvantage during childhood"*; also see Heckman et al. (2014). Another advantage we have is that our data contains records of sickness-related absence from school, allowing us to analyse the relevance of contemporaneous health vs early life health impacts in producing test score gains.

cognitive potential (UNESCO, 2014). Differences in cognitive skills between individuals tend to emerge early and widen with age (Flavio and Heckman, 2007; World Bank, 2015; Attanasio, 2015), which suggests that socio-economic inequality may be rooted in early childhood. By virtue of linking infant health to both cognition and earnings, we provide evidence that it is.

A striking feature of our findings is that earnings and employment responded to the infant health improvement for women but not for men.[11] As discussed, the evidence suggests this is linked to the intervention placing more women than men at the top of the test score distribution in primary school and, hence, to more women completing secondary schooling. This pattern of results is consistent with the model in Pitt et al. (2012), premised on men having a comparative advantage in brawn, and women in tasks that are relatively intensive in cognitive function. The model predicts that exogenously delivered improvements in early life health will reinforce the advantage of men in labour-intensive occupations, leading women to obtain more education and move into more skill-intensive occupations. They also note that gender differences in time allocation will additionally depend on gender-specific labour market returns to human capital.

Broad trends in the Swedish labour market line up with this. We find higher returns to secondary schooling for girls. Our sample cohorts emerged onto the labour market at a time when the rapid growth of the welfare state was generating public sector job opportunities in which education was rewarded, and in sectors that disproportionately employed women. These findings are potentially of contemporary relevance. They suggest (a) that expansion of broad-based public services can trigger an increase in women's employment and labour force participation,[12] and (b) that realising the full impact of an intervention that increases human capital may depend upon demand conditions. In a similar spirit, Coles and Francesconi (2017) argue that an era of expanding job opportunities for women was critical to realisation of the impacts of the pill innovation on women's labour market outcomes in America, and Bhalotra and Venkataramani (2012) show that labour market segregation in the Southern states of America limited realisation of earnings gains from infant exposure to antibiotics for black but not white men.

[11] A few other studies similarly find that educational or cognitive gains flowing from early life interventions favour girls. Baird et al. (2016) identify individuals ten years after a school de-worming programme in Kenya, and find that the programme increased education only among women, while increasing labour supply among men and there were accompanying shifts in occupational sorting. Bhalotra and Venkataramani (2013) find that infant exposure to a major clean water programme (that led to sharp drops in diarrhoea exposure in infancy) in Mexico resulted in improved performance on the Raven matrix and in school test scores, but only for women. They show that parental investments in education responded to the clean water programme with a favour for girls, and that returns to education were increasing for women, who were increasingly sorting into occupations in which cognitive skills mattered.

[12] In addition to showing that the intervention led to an increase in employment, we show that it led to a decrease in the probability of being out of the labour force, again, only for women.

In a recent review of the literature, Almond et al. (2017) argue that the effects of the early life environment on long run outcomes are often heterogeneous, *"reflecting differences in child endowments, budget constraints, and production technologies"*. Here, we additionally highlight the role of opportunities. While our findings reinforce the small body of evidence demonstrating causal effects of infant health on cognitive performance in the school years, they also highlight that the average earnings payoff to cognitive skills is uncertain, being dependent upon distributional effects that may determine higher education choices, and on demand conditions. While previous work has discussed changes in the relative demand for female (vs male) labour stemming from recession, war, or technological change (Elsby et al., 2010; Acemoglu et al., 2004; Cortes et al., 2018; Bhalotra et al., 2018), we provide a new perspective, emphasising how expansion of broad-based public services will tend to change the relative demand for female labour. This is of potential relevance to understanding historical trends in women's employment, and the prospects for women in developing countries that are currently witnessing large-scale expansion in the provision of schooling, public health services and, potentially, pre-school centres.

The rest of the paper is structured as follows: Sections 2 and 3 provide background information on the intervention and on the educational system in Sweden in the early 20$^{\text{th}}$ century. Section 4 describes the data and the empirical strategy, while Section 5 presents the results and Section 6 discusses potential mechanisms. Section 7 presents robustness checks.

## 2 The Field Trial

Similar to many other developed countries Sweden experienced a decline in maternal and infant mortality at the beginning of the 20$^{\text{th}}$ century. However, between 1920–1930, the decade preceding the intervention we analyse, there were no further declines in infant mortality, and neonatal and maternal mortality increased.[13] This gave rise to an intense public debate in Sweden how to improve conditions for expectant mothers and newborns, and the intervention we analyse emerged as a potential solution.

The intervention was described as a field experiment, implemented prior to nationwide adoption. It started on 1 October 1931 and ended on 30 June 1933. It was implemented in 7 health districts (Lidköping, Hälsingborg, Harad, Råneå, Jokkmokk, Pajala and Mörtfors) chosen to be representative of the country in population density and living standards. The selection of

[13]Increasing maternal mortality at this time was not unique to Sweden; there were no declines in maternal mortality in the Western world between 1920 and the mid 1930's. The reasons for this are unclear, but higher rates of septic complications following higher rates of abortions may have contributed.

districts was not based upon infant and maternal mortality rates, the primary targets of the intervention. The trial was funded to the tune of SEK 30,000 (USD 133,000 in current prices). The 7 districts contained 59 municipalities (2 cities and 57 parishes).

To ensure uniform standards of care across the districts, a five-day long educational event including lectures and courses for participating staff was organised in Stockholm in July 1931. The activities were decentralised to the district level and led by physicians. In each of the seven districts a health centre with regular office hours 2–3 times per week was started. Outreach activities included announcements in local newspapers, churches and oral announcements by midwives and nurses (Stenhoff, 1934). In total about 2,000 mothers and 2,600 children enrolled, which represents a majority of eligible individuals. Take-up in the two urban areas (cities) was lower than in the rural areas (parishes).

The intervention focused on preventive care and included check-ups at surgeries, home visits and information campaigns. Newborn children were weighed and checked, and sick children were referred to doctors. Mothers were encouraged to breastfeed and given written and illustrated details on the nutritional needs of children at different stages of development. Home visits by nurses were designed to provide advice on hygiene, sanitation and cleanliness in the household, and to ensure that families followed guidelines published by the National Board of Health. We digitized records maintained by health professionals that indicate programme utilization. These show that the average infant made 2.8 visits to a health centre and received 3.9 home visits.

Eligibility for the infant care programme was determined by birth date. All children less than or equal to 12 months of age at the start of the intervention were eligible and eligibility ceased on their first birthday. Children born during the intervention were of course eligible from birth for the maximum duration of a year. An antenatal care programme was introduced simultaneously with the postnatal program. All expectant mothers were eligible, irrespective of their stage of pregnancy.[14] Figure 1 shows the duration of eligibility in months for the maternal and infant intervention by birthdate. The vertical lines represent the beginning and the end of the trial. In Bhalotra et al. (2017), we show that the trial reduced infant mortality but had no impact on maternal mortality. Exploiting the differential exposure of each individual to the antenatal vs the postnatal components of the programme (illustrated above), we document no short or long run effects of the antenatal care programme, alongside large and persistent effects of the postnatal intervention. In this paper we focus our attention on the postnatal (infant care)

[14] At this time, although it was not uncommon to give birth in a maternity ward, less than 5% of pregnant women consulted a doctor before giving birth.

programme but consistently control for exposure to the antenatal care programme.

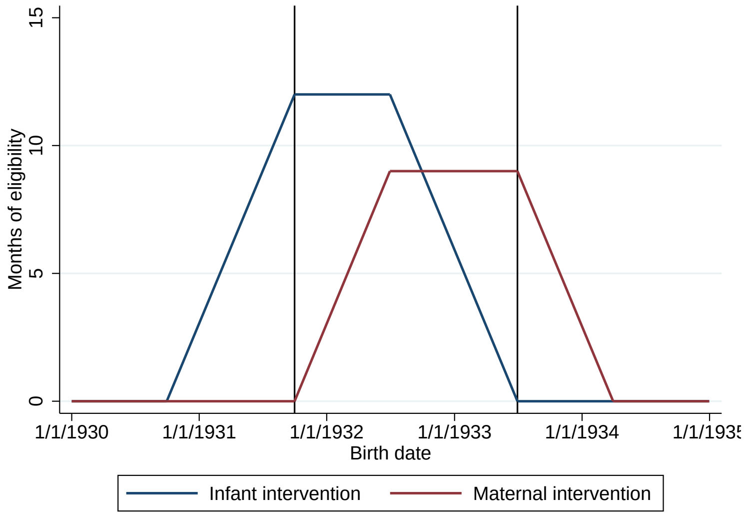



**Figure 1.** Duration of eligibility by birth date for maternal and infant care.

The trial received positive evaluations from physicians and auditors in the 1930s, who attributed improvements in maternal and infant health to positive behavioural change among participants (Stenhoff, 1934).[15] As a result of the success of the trial, a similar scheme was rolled-out nationwide from 1937.[16] Possibly inspired by the field trial in Sweden, Norway and Denmark also rolled out similar programmes from 1936 and 1937 onwards.(cf. Bütikofer et al., 2015; Wüst, 2012; Hjort et al., 2014).

## 3 The Swedish School System

In the 1930's schooling in Sweden started in the year an individual turned seven and was compulsory for six years. Primary education (*Folkskolan*) was universal.[17] Sweden had a tracking system whereby students progressing to secondary schools left *Folkskolan* either after grade 4 or after grade 6. On average barely 20 per cent of children attended secondary schools, availability of which increased in the 1940s.[18] Importantly for our purposes, the share of girls and boys attending secondary school was similar in 1930. A reform implemented in 1927 granted equal

[15]For example the audit report from the chief physician of the northern districts of Sweden states that there was a notable change in the cleanness and tidiness of childrens' beds and clothing.

[16]Historical data on rollout show no evidence that the parishes and cities selected for the trial were the first to benefit from the national roll out.

[17]Parents were legally obliged to send their children to school. Paragraph 51 of the royal decree of the Folkskola stated that parents that did not send their children to school could lose custody.

[18]In the first decades of the twentieth century education beyond the primary level was mainly for children from higher social classes. A series of reforms in the 1925–1945 period, driven by demand and a political will to reduce educational inequalities between urban and rural areas, increased access and the geographical spread of secondary schools. For an elaborated discussion on the reforms and the expansions of secondary education, see Lindgren et al. (2014); Kyle and Herrström (1972); Stanfors (2003).

access for girls to all state-led grammar schools, mandating that girls study the same curriculum as boys. Before 1927 girls could take on secondary education, but only in private schools, the higher costs of which led to lower girl enrollment. By the time our sample cohorts were secondary school age, the situation was transformed, reflecting rapid increases in girls' attendance, particularly in state schools.[19]

Teachers kept records of test scores and absences of all children in exam catalogues, which we digitized. Marks were given on a seven-point grading scale ranging from A (passed with great distinction) to C (failed). In a robustness check we anchor this scale to income in 1970 (see Section 7). The test score data are fairly reliable and comparable. The government established several marking principles (see Appendix B). For example guidelines dictated that teachers reward the quality of knowledge and not the quantity, and take notes throughout the year to ensure that grading reflected performance through the year and not at one point in time. The marks we analyse should thus be purged of day-of-test idiosyncrasies. Teachers were instructed to allow for mark inflation as pupils progressed to higher grades, and to make no adjustment for school form.

For our sample cohorts, schooling was fairly comparable across districts[20] and the curriculum did not change between 1919 and 1950. A majority of students attended school full time (i.e. roughly eight months per year), but some rural school districts provided half time reading in order to meet demands from the agrarian sector. In our sample less than 0.5% of children were enrolled on a half time reading basis. *Folkskolan* was divided into *The Main forms* and *The Exception forms.* The main forms required full-time reading and a teacher with an appropriate teacher degree (*folkskollärareexamen*). The exception forms were either characterised by half-time reading or by the teacher not having an appropriate teaching degree *Folkskolan.* The exception forms were only accepted if the local conditions allowed for no other forms and, in the beginning of the 1940s, more than 90% of all pupils in Sweden went to a school assigned to the main forms (SOU, 1944). Importantly, we can identify school form in our data and we control

[19] In the late 1920s, among birth cohorts 1915-20, just more than half of all children taking secondary education were in private schools (about 24,000 of 44,000). However, more than 90% of children in state schools were male and more than 90% of children in private schools were female. By about 1940 there was dramatic growth in state provided education, with only 10% of children attended private schools (about 5,000 of 50,000 pupils). Although private schools continued to mostly be populated by girls, there was a gender balance in state schools.

[20] In 1919 a central education plan, the *utbildningsplanen*, was introduced to overcome differences in the content and format of primary school education across Sweden's 2400 school districts. Guidelines published by the Department of Ecclesiastical Affairs included time-tables, syllabi for compulsory schooling, and a statmeent of the possible forms a school could have.

for it in the analysis. It may be thought of as a marker of school quality.[21]

Parliamentary decisions in 1936 and 1937 led to roll-out of an extension of compulsory school years and of the length of the school year, which all school districts were to have implemented by the late 1940s. The term length extension, which extended the school year by 3–5 weeks (8–13%), would affect students in all school years, and the extension of compulsory schooling from 6 to 7 years affected pupils who did not proceed to secondary schooling. In appendix Table C2 we show that these reforms are largely unrelated to the intervention studied here, but we nevertheless control for both reforms in our analyses. Further details on these reforms are in Fischer et al. (2017) and Fischer et al. (2013). Sweden was neutral during the Second World War and historical sources suggest no educational disruptions for our sample cohorts.[22]

# 4 Data and Empirical Strategy

## 4.1 Administrative Data Linkage

The dataset is unique in linking individual-level data across the life course using birth registers, school registers, the 1970 census and official tax registers. The birth and school registers were digitised by the authors.

**Birth Registers.** A census of births in 1930–1934 was taken from church records. Sweden is one of the few countries with high-quality vital statistics at the parish level from the 18$^{th}$ century onwards (Pettersson-Lidbom, 2015). Across the treatment and control parishes there were 24,710 deliveries (25,029 individual children) in the sample period, which resulted in 24,374 live births. The birth data contain sex, marital status of the mother, age of the mother and parental occupational status, which we translated into occupational classes based on the HISCO classification (Leeuwen et al., 2002) to control for socio-economic status. We merged these birth register data with data from several other sources using linking procedures that were carefully executed and validated; see Bhalotra et al. (2017) for details.

[21] Appendix Table H10 provides an overview on the proportion of the school forms in the school year 1940/1941 in comparison to the proportions in our sample.

[22] In fact the *Folkskola* was one of the main social agents for some 50,000 Finnish children (in ages one to ten) that were evacuated to foster care in Swedish families during World War II. This said, schools were allowed to have shorter breaks in case of limited energy supply, and schools could cancel regular schooling in case of a threat but any lost days had to be replaced by additional days later on, and in case a teacher was called for military service he had to be replaced by a substitute teacher (Fredriksson et al., 1971). We take care of the latter by controlling for school form and we check whether there are any structural breaks in our school data during the war years. We do not find any evidence of disruption in schooling due to the Second World War.

**Administrative School Records.** We accessed standardised exam catalogues containing pupil-level information from historical archives; see Figure H2 in the Appendix. These contain yearly information on school performance and sickness absence in primary school. We observe the birth cohorts of 1930–1934 in grade 1 and grade 4 of primary school (school years 1937–1947). Grades 1 and 4 are pivotal as grade 1 represents the first occasion at which school performance can be observed and grade 4 represents the last as some pupils leave the basic track and proceed to secondary schooling after that grade. Due to the possibility of grade retention there are a few cases where we observe pupils more than once per grade; however, at 1.6%, grade retention was rare (Hjalmarsson et al., 2015). For about half of our sample we have information on both grade 1 and 4. The other half of the sample is either observed in grade 1 or in grade 4.[23] The data contain academic performance in three cognitive subjects, math, writing and reading and speaking, as well as for one non-cognitive subject: 'religion'. Other variables include sickness absence in days, total absence in days, the length of the school year, school type, the name of the teacher and the name of the school.[24]

Individuals in the birth records were matched to school records using an algorithm based on birth parish, date of birth, forename and surname. Out of 22,500 individuals still alive at age 7, roughly 16,000 were matched to the school records. Unmatched individuals are likely to be missing at random as the missing school records often arise because some had simply not survived the roughly eighty years in the regional archive.[25] Selection due to migration to another parish after birth should also not be a problem since we collected information on those who moved outside the sample of treated and control parishes and our linking algorithm will account for them.

**Labour Market Outcomes – Census.** To summarize, after linking We merged individuals in the birth records to data from the 1970 population and housing census which covers the entire population of Sweden on 1st November 1970 (Population and Housing Census 1970, 1972a). It contains educational attainment, income, employment status and occupation. The match rate is good – of 24,390 births in 1930-34, we observe 20,922 in 1970. Upon matching birth to death

[23]The most prominent reason for missing information is that the archives of certain schools were accidentally destroyed, making it plausible that missing cases are orthogonal to treatment eligibility. Other reasons for missing information are death before reaching school age; discrepancies in name spelling; and migration between birth and school age. We could significantly reduce the matching problem related to migration by tracking migrants and collecting school records from their destination parishes.

[24]Sickness absence accounts for about 80% of total absence. Other reasons for absence could be inappropriate clothing or weather conditions preventing children from going to school. For details see (Cattan et al., 2017).

[25]Another possible explanation for unmatched individuals could be adoptions since we also match on parental surname. About 1% of children in our cohorts got adopted (Bernhardtz, L. and Klintfelt, A., 2007).

registers, we can see that 3,243 of the 4,142 unmatched individuals died before the 1970 census enumeration.[26][27]

**Table 1.** Descriptive statistics: Outcome variables.

| | Men & Women | | | | | Women | | Men | |
|---|---|---|---|---|---|---|---|---|---|
| | Count | Mean | SD | Min | Max | Count | Mean | Count | Mean |
| **School Data** | | | | | | | | | |
| Share Sickness Absence | 15,744 | 0.047 | 0.054 | 0 | 1 | 7,770 | 0.049 | 7,974 | 0.045 |
| Top GPA | 15,789 | 0.215 | 0.362 | 0 | 1 | 7,791 | 0.257 | 7,998 | 0.175 |
| GPA | 15,789 | 3.560 | 0.616 | 1 | 6 | 7,791 | 3.670 | 7,998 | 3.453 |
| Math | 15,774 | 3.518 | 0.720 | 1 | 6 | 7,780 | 3.568 | 7,994 | 3.469 |
| Reading | 15,768 | 3.618 | 0.661 | 1 | 7 | 7,779 | 3.737 | 7,989 | 3.503 |
| Writing | 14,860 | 3.519 | 0.747 | 1 | 7 | 7,341 | 3.681 | 7,519 | 3.360 |
| **Census 1970** | | | | | | | | | |
| Secondary Schooling | 20,911 | 0.182 | 0.386 | 0 | 1 | 10,298 | 0.188 | 10,613 | 0.176 |
| Working Full time | 20,723 | 0.635 | 0.481 | 0 | 1 | 10,257 | 0.336 | 10,466 | 0.929 |
| Working Part time | 20,723 | 0.136 | 0.343 | 0 | 1 | 10,257 | 0.259 | 10,466 | 0.016 |
| Top income | 20,921 | 0.205 | 0.404 | 0 | 1 | 10,302 | 0.204 | 10,619 | 0.206 |
| log Income | 20,921 | 9.545 | 1.132 | 0 | 13 | 10,302 | 8.865 | 10,619 | 10.205 |
| Municipal Employment | 20,723 | 0.160 | 0.367 | 0 | 1 | 10,257 | 0.232 | 10,466 | 0.091 |
| Governmental Employment | 20,723 | 0.085 | 0.279 | 0 | 1 | 10,257 | 0.048 | 10,466 | 0.121 |
| **Tax Registers** | | | | | | | | | |
| Log Pension Age 71 | 15,965 | 11.875 | 0.448 | 9 | 15 | 8,285 | 11.704 | 7,680 | 12.059 |

*Note:* Variable descriptions to this table are available in Appendix A.

**Pension Income – Tax Registers.** We also linked the birth records to pension (labour) income available for 2001–2005 from official tax registers. These contain information on 16,194 individuals from the birth records (7,290 individuals having died before age 71 and 1,580 individuals unmatched). An advantage of using pension income is that it is insensitive to career interruptions such as those associated with childbearing, which could influence income observed in 1970 at a prime working age. For the sample cohorts, obtaining a full pension required thirty years of contributions and the level of the pension was based upon the best fifteen years (Sundén, 2006). Table 1 and Appendix Table H11 present descriptive statistics on all explanatory and outcome variables.

**Longitudinal Individual Data: Four points in the lifecycle.** To summarize, after linking the above datasets, we track outcomes at four different points in the lifecycle. The potentially treated cohorts are born 1931–1933, and observed in first grade between the school years 1938–

[26] We are consequently left with about 900 individuals who cannot be matched. It is possible that they emigrated.

[27] The earnings information in the 1970 census is regarded to be of high quality, but women who were the partners of a small business owner or a farmer could be recorded as working full-time or part-time while having zero taxable earnings. Since this measurement error might bias our results, we impute incomes of these 2,987 women based on their qualifications and hours worked.

1940 when they are 7 years old, and in fourth grade between school years 1941–1943 when they are 10 years old. We then observe them in 1970 when they are age 37–39, a labour market active age. We match 66% of the birth sample to school data and 86% of the birth sample to the 1970 census files. Attrition is not differential by treatment status.[28] We observe pension incomes in 2002–2004 when they are 71 years old for 91% of survivors (65% of the birth sample).

**Matched Controls.** Since the intervention took place in seven medical districts consisting of 59 municipalities (2 cities and 57 rural parishes), we identified as matched controls, 2 cities and 57 rural parishes (belonging to 38 different health districts) using observable parish characteristics from the 1930 census. The best matches (denoted $\mathcal{J}_M(i)$) were identified using the Mahalanobis distance metric; details are in Appendix C, where we also present tests and descriptive statistics that validate the matches. Summary statistics for a range of relevant observables suggest that our analysis sample is representative of Sweden. Figure 2 visualises the sample areas at the municipality (parish and city) level. To ensure balance among the matching procedure variables, observations from the control group were weighted based on their population size in 1930 relative to the population size of the treated locations they were matched to. On the one hand this reduces potential bias while on the other hand it will slightly reduce the efficiency of our estimates.

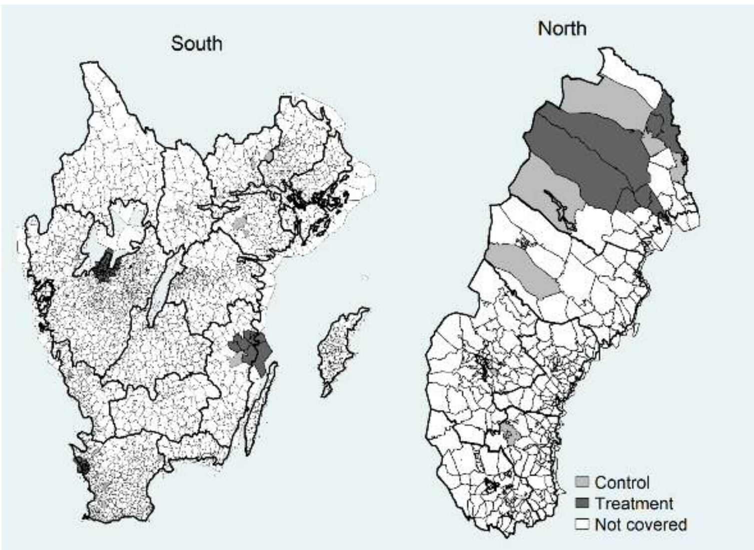



**Figure 2.** Municipalities containing treated and control districts.

[28]The main attrition difference appears for school data where the match rate in the treatment and control group is 64% and 67%, respectively.

## 4.2 Empirical Strategy

We want to estimate impacts of the infant health intervention on academic performance and sickness absence in primary school, secondary school completion, and adult employment, occupation and income. We use a difference-in-differences (DID) strategy to compare outcomes for exposed cohorts in treated regions to unexposed cohorts and control regions. In contrast to the case in most DID designs, our intervention is switched on and off, as a result of which unexposed cohorts include ineligible individuals born before and after the exposed cohorts.

We gauge impacts of the infant intervention by estimating

$$y_{ipt} = \alpha + \beta T_t + \gamma_p + \tau T_t D_p + \sigma_t + \lambda X + u_{ipt}$$

where $y_{ipt}$ is the outcome for child $i$ born in parish $p$ on day $t$, $T_t$ is the duration of eligibility for the intervention for child $i$ born on day $t$ in years, $D_p$ is a dummy equal to one for treated parishes, $\gamma_p$ are parish fixed effects, $\sigma_t$ are $Quarter\ of\ birth \times Year\ of\ birth$ fixed effects and $X$ is a vector of covariates.

Covariates that we condition on include the sex of the child, whether the child was born in a hospital, marital status of the mother, a twin indicator, dummies capturing older (>35 years) and younger (<25) mothers and the occupational status of the household head at the birth of the child. We also control for eligibility for the maternal (prenatal care) intervention since some individuals were eligible for both interventions. Since we found no impacts of the antenatal programme[29], our discussion focuses upon the postnatal (infant) component. The richness of information in the school records allows us to also control for school fixed effects, length of the school year, and school form (an indicator of school quality). In order to allow for differential trends in outcomes between treatment and control regions, we investigate robustness to including parish specific time-trends, which are more general than treatment-group-specific trends.[30] In the previous section we discussed the match rates of the birth records with school and labour market outcomes data which were exceptionally high but not complete. As a result we checked if attrition was differential by treatment status and found it was not.[31]

[29] Results available on request.

[30] We also checked that our findings are robust to including health district fixed effects and health district specific trends. Counties contain health districts which consist of parishes, which are in 99% of cases identical to school districts.

[31] This was to be expected. Consider the match of birth to school records, which was the least complete. Treatment is defined on birth date but if, for instance, a fire in a regional archive had destroyed school records for a particular parish, it would have evenly destroyed records for children born on either side of the eligible birth dates.

The parameter $\tau$ measures the intent-to-treat (ITT) effect of the infant intervention for an additional year of eligibility. This is the parameter of interest for policy makers who are unable or unwilling to make the utilisation of services mandatory. Since there were no always-takers (cf. De Chaisemartin, 2012) the ITT is a scaled version of the average treatment effect on the treated (ATT).

We conduct a number of specification checks (see Section 7). First, we use alternative definitions of the treatment indicator. Instead of a continuous indicator which takes duration of eligibility into account, we investigated the effect using binary variables to identify age and duration of exposure. Second, since marks were given on an ordinal scale, our findings are potentially sensitive to the choice of scale (cf. Bond and Lang, 2013; Lang, 2010; Cunha et al., 2010). To address this we anchor the 7-point grading scale to the logarithm of income in adulthood (as proposed e.g. by Cunha and Heckman, 2008).[32] Third, while the main specification adjusts for parish-specific trends, we used pre-intervention outcome data to test for differential pre-trends between treatment and control regions. Fourth, we implement a placebo test using a fake intervention ten years after the actual intervention. Finally, we conduct a randomisation inference test for the long-term outcomes, randomly assigning treatment status within each treatment and control parish pair. We then plot the distribution of placebo treatment effects alongside the actual treatment effect.

In Section 6, we discuss an approach to assessing mediating factors, in other words, to produce descriptive estimates of the extent to which treatment effects that acted on outcomes earlier in the life course contributed to treatment effects on outcomes later in life.

## 5 Results

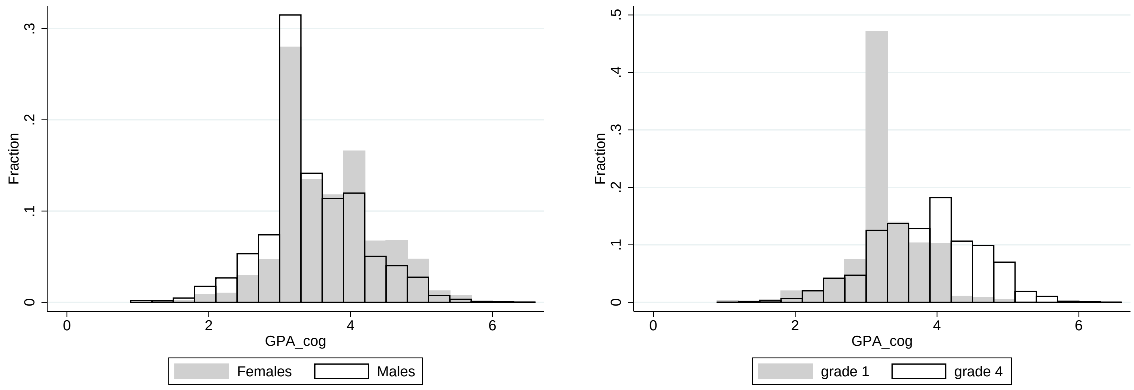
We first present results for educational and income outcomes, examining test scores (at age 7 and 10), progression to secondary school (the relevant margin for higher education in the sample period) and earnings (measured when the marginal cohort is 39, and 71). In the next section, we explore the sources of changes in these outcomes by examining sickness absence in school, and employment and occupation in adulthood (age 39).

[32] This is one more advantage of that we are able to link school test scores to earnings at the individual level.

## 5.1 Outcomes: Human Capital and Earnings

### 5.1.1 Cognitive Performance- Primary School

As discussed, we digitised school records from paper files, drawing from archives across the country, and matched them to the birth data. Since schools awarded marks on a 7-point grading scale in the 1930s, we translated the scale into a range from 1 for the poorest mark (C) to 7 for the best mark (A). We created a measure of cognitive ability by taking the mean of grades in math, reading and speaking and writing to form a grade-point average (GPA), although we shall also report subject-specific estimates. In order to ease interpretation of the coefficients we transform grades into a $z$ score using the inverse standard normal distribution. We present results separately for grade 1 and grade 4, and for boys and girls. Figure 3 shows the grade point average by gender and grade. Girls, in general, got better marks than boys, and marks in grade 4 exhibit a higher mean and greater spread than in grade 1.




(a) Girls vs. Boys

(b) Grade 1 vs. Grade 4

**Figure 3.** Distribution of Test Scores.

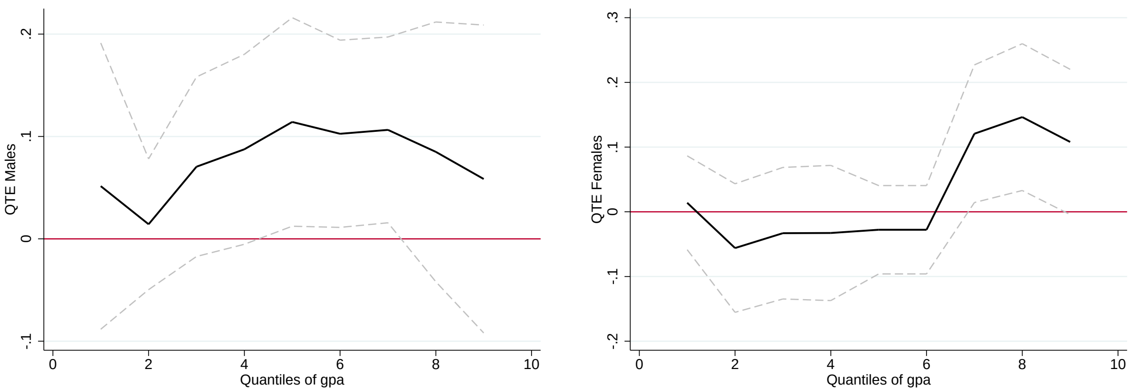
Table 2 presents estimates for each subject and for the average GPA. We see a statistically significant increase of about 0.08 standard deviations in grade 4 when the marginal cohort is age 10. The estimated coefficients are not significantly different by sex, but are larger and only statistically significant among boys, who exhibit a GPA increase of about 0.11 standard deviations. These results are robust to controls not only for parish and birth quarter × year fixed effects but also school fixed effects, the length of the school year and school form, indicators of the socioeconomic status of the parents of the child, and parish specific trends.

Disaggregating GPA by subject, we can see that eligibility for the infant health intervention led to significant improvements in 'writing' and 'reading and speaking', while having no significant effect on performance in 'math'. We also see no change in test scores in 'religion'.

Writing, and reading and speaking scores increase by about 0.11 and 0.12 standard deviations on average. Again these increases are not significantly different by gender, but are larger and only statistically significant for boys. The coefficients for boys are 0.13–0.18 standard deviations, and for girls 0.08–0.11 standard deviations.

The table also reports results for test scores in grade 1, and we see no programme impacts here. There was less variation in scores in grade 1 (see Figure 3) but in fact the standard errors are similar, and the coefficients a lot smaller. Since other studies have found that cognitive gains stemming from pre-school interventions tend to fade (see e.g. Bitler et al. (2016) and Chetty et al. (2011)), it is notable that an infant health intervention produced cognitive gains evident at age 10.

We also estimated unconditional quantile treatment effects, following Firpo et al. (2009); see Figure 4 for grade 4 GPA by gender. While boys experienced positive treatment effects across most of the distribution, it was only in the upper 30% of the distribution that girls benefited from the intervention. Moreover at the upper end the test score gains for girls exceeded the test score gains for boys.[33]




(a) Boys (b) Girls

*Note:* Covariates which are included are a dummy indicating twin births, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, Parish FE and QOB×YOB FE. 90% Confidence Intervals included.

**Figure 4.** Quantile regression: GPA in grade 4 by gender.

We investigated heterogeneity in the estimates by socioeconomic status of the family at birth. Children born out of wedlock benefited substantially more than other children but there were no differences in effects by parental socio-economic status (as indicated by their occupation); see Appendix F.

[33]We investigated whether gender differences in test score outcomes arose from differences in utilisation of the programme for sons vs daughters by studying physician records of individual utilisation of the programme (see Appendix D). We found no significant differences by gender.

**Table 2.** Cognitive Performance – Primary School

| | Boys & Girls | | | | Girls | | | | Boys | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | (1) | (2) | N | Mean | (3) | (4) | N | Mean | (5) | (6) |
| Panel A: Grade 1 | | | | | | | | | | | | |
| Top GPA | 13,207 | 0.204 | -0.0002 | -0.0057 | 6,404 | 0.223 | 0.0284 | 0.0308 | 6,803 | 0.185 | -0.0193 | -0.0302 |
| | | | (0.035) | (0.038) | | | (0.050) | (0.057) | | | (0.033) | (0.033) |
| GPA | 13,207 | -0.032 | 0.0129 | -0.0020 | 6,404 | 0.027 | 0.0406 | 0.0468 | 6,803 | -0.093 | -0.0015 | -0.0352 |
| | | | (0.050) | (0.053) | | | (0.075) | (0.084) | | | (0.050) | (0.049) |
| Math | 13,161 | -0.058 | -0.0327 | -0.0525 | 6,382 | -0.050 | 0.0201 | 0.0014 | 6,779 | -0.066 | -0.0895* | -0.1121** |
| | | | (0.050) | (0.050) | | | (0.089) | (0.090) | | | (0.049) | (0.045) |
| Reading | 13,177 | 0.001 | 0.0331 | 0.0170 | 6,383 | 0.082 | 0.0393 | 0.0570 | 6,794 | -0.082 | 0.0534 | 0.0094 |
| | | | (0.050) | (0.053) | | | (0.062) | (0.073) | | | (0.068) | (0.071) |
| Writing | 9,007 | -0.016 | 0.0937 | 0.0891 | 4,399 | 0.091 | 0.1469 | 0.1719 | 4,608 | -0.131 | 0.0528 | 0.0225 |
| | | | (0.093) | (0.093) | | | (0.126) | (0.129) | | | (0.074) | (0.075) |
| Religion | 13,060 | -0.027 | 0.0107 | -0.0223 | 6,337 | -0.000 | 0.0031 | -0.0131 | 6,723 | -0.054 | 0.0326 | -0.0160 |
| | | | (0.067) | (0.071) | | | (0.097) | (0.106) | | | (0.062) | (0.066) |
| Panel B: Grade 4 | | | | | | | | | | | | |
| Top GPA | 13,268 | 0.173 | 0.0697* | 0.0749* | 6,561 | 0.227 | 0.1000* | 0.1243* | 6,707 | 0.116 | 0.0400 | 0.0275 |
| | | | (0.039) | (0.039) | | | (0.059) | (0.071) | | | (0.033) | (0.028) |
| GPA | 13,268 | -0.047 | 0.0737** | 0.0759** | 6,561 | 0.098 | 0.0410 | 0.0617 | 6,707 | -0.200 | 0.1213** | 0.1084 |
| | | | (0.033) | (0.036) | | | (0.049) | (0.054) | | | (0.057) | (0.072) |
| Math | 13,242 | -0.027 | -0.0220 | 0.0010 | 6,554 | 0.025 | -0.0535 | -0.0217 | 6,688 | -0.082 | 0.0193 | 0.0317 |
| | | | (0.047) | (0.045) | | | (0.051) | (0.056) | | | (0.079) | (0.091) |
| Reading | 13,223 | -0.056 | 0.1179** | 0.1105* | 6,536 | 0.120 | 0.0832 | 0.0902 | 6,687 | -0.241 | 0.1823*** | 0.1649** |
| | | | (0.045) | (0.059) | | | (0.057) | (0.066) | | | (0.064) | (0.082) |
| Writing | 13,228 | -0.057 | 0.1239** | 0.1129** | 6,536 | 0.150 | 0.0859 | 0.1068 | 6,692 | -0.275 | 0.1645** | 0.1291* |
| | | | (0.056) | (0.054) | | | (0.081) | (0.094) | | | (0.064) | (0.072) |
| Religion | 13,238 | -0.044 | -0.0150 | 0.0372 | 6,549 | 0.088 | 0.0160 | 0.0654 | 6,689 | -0.184 | -0.0222 | 0.0247 |
| | | | (0.049) | (0.035) | | | (0.052) | (0.066) | | | (0.097) | (0.096) |
| Parish FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| QOB×YOB FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| School FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| SES Effects | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| Length of Schoolyear | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| Schoolform | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| Parish Trends | | | | ✓ | | | | ✓ | | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish-grade level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the household head. *Length of schoolyear* are fixed effects controlling for the reforms concerning the length of the school year. *Schoolform* are fixed effects controlling for the school form as described in Section 3 and *Parish specific linear trends* allows for parish specific time trends.

To put the average gain in cognitive performance of 0.11 standard deviations (or as much as 0.17 for boys in reading skills) in perspective, consider that Bharadwaj et al. (2013) identify effects of 0.15-0.22 s.d. in Chile and Norway using a sample of children at the low birth weight margin. Using twin fixed effects Bharadwaj et al. (2017) estimate that a 10% increase in birth weight in Chile increases outcomes in math and language scores by 0.04-0.06 standard deviations, and examining twin pairs in Florida Figlio et al. (2014) estimate that, on average, the heavier twin scores about 0.05 s.d. better than the lighter twin. Overall, our estimates, emerging from exposure to a universally available health intervention incident in infancy are sizeable. In fact, they look fairly large even in relation to educational interventions in developing countries, some of which have shown test scores gains between 0.17 s.d. to 0.47 s.d. (Duflo and Hanna, 2005; Muralidharan and Sundararaman, 2011; Banerjee et al., 2007).

The cognitive advantage of intervention-exposed individuals is plausibly linked to health improvements in their infancy. In Bhalotra et al. (2017), we show that the average duration of potential exposure to the programme in infancy led to a 1.56 percentage point decline in the risk of infant death, which is 24% of baseline risk. Infant mortality is widely used as an indicator of infant health, given that morbidity scales with mortality (Bozzoli et al., 2009). Infancy is a period of rapid neurological development and there is evidence that net nutrition (including breastfeeding, clean water, reduced infections) at this time can influence brain development (Doyle et al., 2009; Eppig et al., 2010; Deverman and Patterson, 2009), creating a biological mechanism for causal effects of infant health on cognition. This may have been reinforced by behavioural change induced by the programme, for instance in breastfeeding (Fitzsimons and Vera-Hernandez, 2015).

### 5.1.2 Secondary Education

Only about 19% of individuals born between 1930 and 1934 continued into secondary education after primary school, making secondary education the relevant margin for analysis of higher education. The estimates in Table 3 show that an additional year of exposure to the intervention resulted in a 3.5 percentage point increase in the probability that girls completed secondary school, an increase of 17.6% relative to baseline. In contrast, there is no change among boys. The pre-intervention mean is somewhat larger for girls, but it is not significantly different from that for boys.[34]

A potential explanation for treatment effects having favoured secondary schooling for girls over boys is that, at the upper end of the primary school test score distribution, improvements in performance were larger for girls than for boys (see Figure 4), with baseline performance being stronger among girls. Figure 5 shows the mapping between primary school test scores and

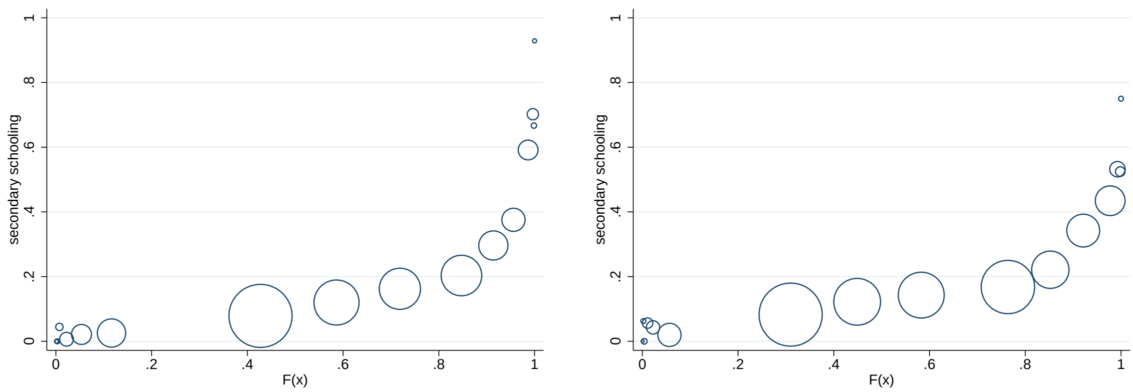
[34] As discussed in detail by Schånberg (1993) the female/male student ratio in secondary education started to increase after the 1927 reform but was close to 1 throughout the 1930's, before increasing further in the 1940–50 period.

secondary school completion.[35]

**Table 3.** Secondary Schooling

| | Boys & Girls ($N = 20,474$) | | | Girls ($N = 10,105$) | | | Boys ($N = 10,369$) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | (1) | (2) | Mean | (3) | (4) | Mean | (5) | (6) |
| Primary | 0.700 | 0.0099 | 0.0180 | 0.675 | -0.0087 | -0.0011 | 0.725 | 0.0280 | 0.0367 |
| | | (0.021) | (0.020) | | (0.032) | (0.026) | | (0.023) | (0.025) |
| Dropout | 0.114 | -0.0031 | -0.0222 | 0.126 | -0.0196 | -0.0277 | 0.101 | 0.0131 | -0.0167 |
| | | (0.016) | (0.020) | | (0.023) | (0.023) | | (0.029) | (0.027) |
| Secondary | 0.185 | -0.0062 | 0.0027 | 0.198 | 0.0353** | 0.0350** | 0.172 | -0.0468 | -0.0289 |
| | | (0.017) | (0.013) | | (0.016) | (0.014) | | (0.029) | (0.021) |
| Parish FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| QOB×YOB FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| SES Effects | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| School Reforms | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Parish Trends | | | ✓ | | | ✓ | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* allows for parish specific time trends.




(a) Boys (b) Girls

*Note:* Circle size indicates number of people in each group.

**Figure 5.** Correlation of marks in primary school and secondary schooling completion.

In 1930, the National Census showed that only six per cent of all women above age 16 had at

[35]Students seeking entry to secondary education had to take an entrance test (Wallin and Grimlund, 1933). The test was national, covered certain subjects (Swedish and math, written and oral tests) and only students who passed the test were eligible for secondary schooling. For acceptance, students also needed to pass in other subjects in primary school (Dahr, 1945). Despite an increasing number of secondary schools, there were more applicants than available seats, particularly in urban areas. According to Skolöverstyrelsen (1955), about 11 per cent of all applicants of the cohorts born 1930-1934 were rejected. This may contribute to explaining why the intervention did not raise secondary schooling for boys, even though on average they exhibited higher test scores as a result of the intervention

least secondary education, and eight per cent of men. Lifetime returns to education increased for women following a legal reform implemented in 1939 which prohibited firing women on grounds of marriage or pregnancy, similar to the lifting of marriage bars in the United States (Goldin, 1988). In the 1930s, the returns to years of schooling were greater for women than for men (Bång, 2001). We examined relative returns, using our data to regress income in 1970 on test scores in grade 4 and an indicator for completion of secondary schooling (see Table 4). We find higher earnings returns to school grades for girls, and also that the secondary schooling premium (conditional on grades) is significantly larger for girls (see also Björklund and Kjellström, 1994).

To summarize, the intervention led to higher school grades and educational attainment among girls and, for these cohorts, the returns to these skills were higher for girls.[36]

**Table 4.** Returns to education.

| | Men & Women (1) | Women (2) | Men (3) |
|---|---|---|---|
| Standardised Grade 4 GPA | 0.0841*** | 0.0905*** | 0.0798*** |
| | (0.011) | (0.021) | (0.010) |
| Secondary Schooling | 0.4645*** | 0.5379*** | 0.3837*** |
| | (0.024) | (0.042) | (0.023) |
| Female Child | -1.3704*** | | |
| | (0.016) | | |
| Constant | 10.3144*** | 8.8962*** | 10.3662*** |
| | (0.065) | (0.118) | (0.059) |
| N | 12,518 | 6,221 | 6,297 |
| $R^2$ | 0.385 | 0.045 | 0.103 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Outcome variable is log income 1970. Covariates which are included are a dummy indicating twin births, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, and dummies indicating parental SES.

### 5.1.3 Earnings

We find very substantial impacts of the infant health intervention on earnings recorded in 1970, with eligibility for a year enhancing earnings by 7.3% on average (Table 5). This result is completely driven by women who experienced an increase in earnings of about 19.5%, in contrast to no gain among men. These results are robust to all controls discussed earlier including parish

[36] Among our sample cohorts, a not insubstantial share of men were engaged in brawn-intensive activities in which males had a comparative advantage, so that women had a comparative advantage in cognition-intensive tasks. Our findings are hence in line with the predictions of (Pitt et al., 2012). Bhalotra and Venkataramani (2013) find broadly similar results. Saaritsa and Kaihovaara (2016) explore schooling decisions of females and males in Finland in the early 20$^{\text{th}}$ century and find quite similar patterns. Their conclusion is that boys were more likely to drop out of school because of lower net expected returns to schooling, while better educated girls benefited from the expansion of modern services creating attractive working conditions.

specific time trends. Below, we show that the intervention also raised employment among women, but not men. So the large earnings increase for women reflects not only wage increases but also an extensive margin increase in labour supply.

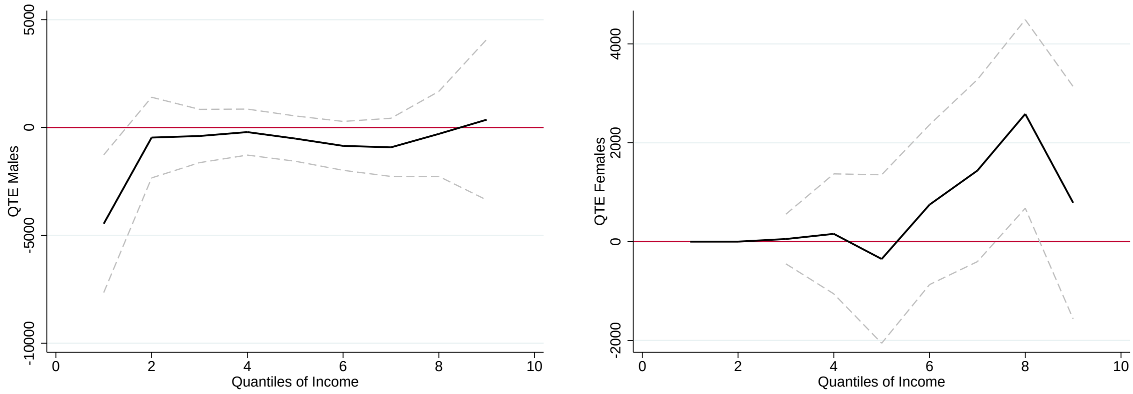
Unconditional quantile treatment effects for women's earnings show that the increases are concentrated in the upper part of the income distribution (Figure 6). We estimate that the probability of belonging to the top quintile of earners (the top 20 per cent, indicated by the variable *Top Income*) increases by 7 percentage points. This is consistent with school test scores for girls having improved in the upper tail of the test score distribution and, as we discuss below, with cognitive performance being a mediator for income effects of the health intervention.

**Table 5.** Earnings

| | Men & Women | | | Women | | | Men | | |
|---|---|---|---|---|---|---|---|---|---|
| CENSUS 1970 | N=20,920 | | | N=10,307 | | | N=10,613 | | |
| | Mean | (1) | (2) | Mean | (3) | (4) | Mean | (5) | (6) |
| Top Income 1970 | 0.228 | 0.0099 | 0.0209 | 0.244 | 0.0655*** | 0.0788*** | 0.210 | -0.0445 | -0.0361 |
| | | (0.016) | (0.013) | | (0.022) | (0.028) | | (0.034) | (0.028) |
| log Income 1970 | 9.593 | 0.0295 | 0.0732** | 8.990 | 0.1204* | 0.1947*** | 10.222 | -0.0596 | -0.0464 |
| | | (0.033) | (0.028) | | (0.063) | (0.066) | | (0.037) | (0.036) |
| PENSION AGE 71 | N=15,964 | | | N=8,284 | | | N=7,680 | | |
| | Mean | (1) | (2) | Mean | (3) | (4) | Mean | (5) | (6) |
| log Pension | 11.789 | -0.0035 | 0.0187 | 11.609 | 0.0293 | 0.0711*** | 11.995 | -0.0400** | -0.0400* |
| | | (0.012) | (0.014) | | (0.019) | (0.015) | | (0.017) | (0.020) |
| Parish FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| QOB×YOB FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| SES Effects | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| School Reforms | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Parish Trends | | | ✓ | | | ✓ | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Top income refers to belonging to the top 20 per cent of the earnings distribution. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* are parish specific linear trends.

Since income is measured at one time, in 1970 (when the marginal cohort born 1931 is 39 years old), it may be sensitive to labour market fluctuations – particularly short-term fluctuations in labour supply. We therefore investigated pension income at age 71 as an alternative measure of income. We see increases in pension income for women and not men, the increases for women being about 7%; see the lower row of Table5, which supports the results obtained with the 1970




(a) Men

(b) Women

*Note:* Covariates which are included are a dummy indicating twin births, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, Parish FE and QOB×YOB FE. 90% Confidence Intervals included. Including zero income as an alternative to the log income transformation we do in the main section in Table 5 (see Appendix A).

**Figure 6.** Quantile regression of income by gender.

data.[37]

A potential concern with the use of the pension variable is that a widow pension was available to the sample cohorts, and this could create a wedge between women's earnings and their pensions. However the results are robust to controlling for an indicator for whether the individual was in receipt of a widow pension (Appendix Table G8).

## 5.2 Intermediate Outcomes

In this section we investigate impacts of the infant health intervention on school-age and labour market outcomes that potentially mediate the observed impacts on test scores and income.

### 5.2.1 Sickness Absence in Primary School

There are two main channels through which the infant health intervention may have had the noted impacts on school performance at age 10. First, there may be a contemporaneous effect of health on academic performance, associated with sick children being more likely to be absent from school, or with their concentration being compromised when they do attend school. The

[37]However we now see a decline in pension income of 4% for men. Since we saw no decline in earnings for men at age 39 but we see a decline in pension for men at age 71, and since only 63% survive to the age of 75, this may reflect endogenous survival selection, the marginal surviving individual being negatively selected post-intervention (see Bhalotra et al. (2017)). To investigate the role of survival selection, we re-estimated programme effects on 1970 income for subsamples of individuals surviving until age 40, 50, 60, 70 and 75 respectively (Appendix Table G7). We see no selection among females until age 75, when there appears to be some positive selection. In contrast, among men, there appears to be negative selection from age 60 onwards as the earnings estimates become progressively lower the older the age group.

second channel operates through brain development and runs directly from infant health to later life cognitive performance.[38] As the school data we digitized include information on sickness absence, we used this as a marker of child health in the years in which the test scores are awarded, to discriminate between the two channels.

We focus on grade 4 as this is where we saw intervention effects on performance. We find that the intervention reduced sickness absence for boys while increasing it for girls. A year's exposure to the intervention reduced boys' sickness absence in fourth grade by about 0.8%, corresponding to 20% of the baseline rate, increasing it for girls by a broadly similar magnitude. Although the reason for this divergence is unclear, it allows us to reject the first hypothesis in favour of the second.[39]

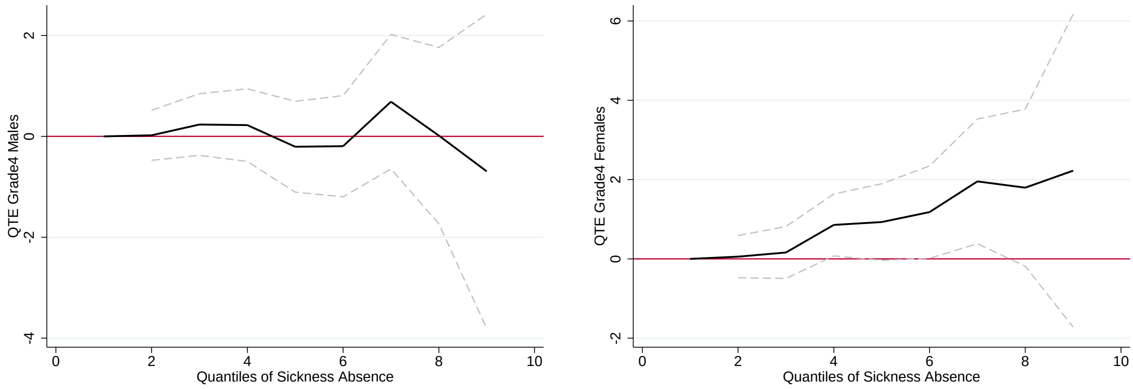
**Table 6.** Sickness Absence (Fraction of School Year) – Primary School

| | Boys & Girls ($N = 13,138$) | | | Girls ($N = 6,487$) | | | Boys ($N = 6,651$) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | (1) | (2) | Mean | (3) | (4) | Mean | (5) | (6) |
| Sickness Absence | 0.045 | -0.0014 | -0.0002 | 0.050 | 0.0071 | 0.0100* | 0.040 | -0.0080 | -0.0083* |
| | | (0.002) | (0.002) | | (0.004) | (0.006) | | (0.005) | (0.004) |
| Parish FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| QOB×YOB FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| School FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| SES Effects | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Length of Schoolyear | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Schoolform | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Parish Trends | | | ✓ | | | ✓ | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish-grade level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the household head. *Length of schoolyear* are fixed effects controlling for the reforms concerning the length of the school year. *Schoolform* are fixed effects controlling for the school form as described in Section 3 and *Parish specific linear trends* allows for parish specific time trends.

[38] For instance, biomedical evidence shows that severe or repeated infections early in life may divert nutrients away from neurological development, particularly during infancy, when it is estimated that about 85% of calorie intake is used to build brains (Finch and Crimmins, 2004; Eppig et al., 2010). In addition, the release of inflammatory molecules during an infection may directly impact the developing brain by changing the expression of genes involved in the development of neurons and the connections between them (Deverman and Patterson, 2009).

[39] Baseline sickness absence rates are similar for boys and girls at about 5% of school days. The distribution and the mean of sickness absence for this 1930s births sample corresponds fairly well to that in contemporary research, see e.g. Aucejo and Romano (2014) and Goodman (2014). See Cattan et al. (2017) for analysis of short- and long-term effects of sickness absence for our cohorts.




(a) Boys (b) Girls

*Note:* Covariates which are included are a dummy indicating twin births, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, Parish FE and QOB×YOB FE. 90% Confidence Intervals included.

**Figure 7.** Quantile Regression: Sickness Absence in Grade 4

### 5.2.2 Employment

We created dummy variables indicating whether the individual was working part-time (≥20 hours per week <35) or full-time (≥35 hours per week) in 1970.[40] We find that women exposed to the intervention for a year exhibited an increase in the propensity to work full-time of 7.6 percentage points (Table 7). As 37% of the sample of women worked full-time, this is an increase of 20.5%. There are no significant impacts on employment for men, 92.5% of whom worked full-time, nor any impacts on part-time work for men or women.

In the years when our sample cohorts were making labour market decisions, there was a trend increase in labour force participation of married women (Schånberg, 1993). Our estimates exploit a discontinuity in eligibility conditional upon general trends. It is nevertheless relevant to note that there were increasing opportunities for women, so demand conditions probably facilitated an increase in labour supply stemming from the intervention. For instance, Coles and Francesconi (2017) argue that expanding job opportunities for women was critical to realisation of the impacts of the pill innovation on women's outcomes in America. Similarly, women treated by the infant health intervention we study may have had better skills and thus higher potential on the labour market but this will need to have been complemented by (suitable) job opportunities for women in order for large increases in women's participation to be realised. To investigate the role of opportunities, albeit indirectly, we examined the sectors that women responding to the intervention joined, and linked this to historical information on sectoral growth trends. We

[40]Part-time and full-time work are thought to be underestimated in the 1970 population and household census (cf. Population and Housing Census 1970, 1972b), but this applies to men and women.

discuss these results next.

**Table 7.** Employment

| | Men & Women (N=20,722) | | | Women (N=10,256) | | | Men (N=10,466) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | (1) | (2) | Mean | (3) | (4) | Mean | (5) | (6) |
| Working Parttime | 0.145 | -0.0201 | -0.0147 | 0.265 | -0.0325 | -0.0244 | 0.019 | -0.0077 | -0.0049 |
| | | (0.017) | (0.017) | | (0.030) | (0.033) | | (0.007) | (0.007) |
| Working Fulltime | 0.640 | 0.0276 | 0.0349* | 0.370 | 0.0607* | 0.0760** | 0.925 | -0.0052 | -0.0061 |
| | | (0.017) | (0.020) | | (0.031) | (0.037) | | (0.014) | (0.015) |
| Municipal | 0.167 | 0.0194* | 0.0295** | 0.238 | 0.0377* | 0.0488** | 0.092 | 0.0012 | 0.0102 |
| | | (0.011) | (0.013) | | (0.020) | (0.020) | | (0.014) | (0.016) |
| Governmental | 0.081 | 0.0126 | 0.0131 | 0.051 | 0.0306*** | 0.0339** | 0.111 | -0.0053 | -0.0077 |
| | | (0.013) | (0.014) | | (0.012) | (0.014) | | (0.019) | (0.019) |
| Parish FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| QOB×YOB FE | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| SES Effects | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| School Reforms | | ✓ | ✓ | | ✓ | ✓ | | ✓ | ✓ |
| Parish Trends | | | ✓ | | | ✓ | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* are parish specific linear trends.

### 5.2.3 Occupation

**Public Sector Jobs.** Using indicators for employment in municipal and central government employment, we see that eligibility for the intervention for a year was associated with an increase in the probability that women work in municipal public sector jobs of 4.9 percentage points, or 20.5% relative to the baseline of about 24%. We also see an increase in the likelihood of working in central governmental jobs of 3.4 percentage points, which corresponds to 66.5% of the pre-mean(Table 7). Adding up across both categories of public sector jobs, it appears that more or less all of the additional employment of women was in the public sector.

From the mid-20$^{\text{th}}$ century, Sweden experienced a rapid expansion in the welfare state. This created more jobs for women than for men, women being predominant in publicly provided services such as midwifery, teaching and health care (Stanfors, 2003; Datta Gupta et al., 2006; Sundin and Willner, 2007). Figure 8 shows how female employment rapidly increased from about 800,000 employed women in 1950 to about 1,200,000 in 1970, while male employment stayed fairly constant over time. Until the 1950s it was mainly single women that participated in the labour market but, from the mid-1960s, married women increased their participation (Stanfors, 2003). This was facilitated by large investments in publicly provided child care for pre-school children (Datta Gupta et al., 2006; Bergh, 2009). Not only were married women able to join the labour force once child care was introduced but the expansion created new job opportunities for women (most pre-school teachers were women). Figure 9 illustrates the trend in women working in selected public sector jobs 1950–1975.[41]


**Figure 8.** Working population by gender *Source: Statistiska Centralbyrån (2009)*

**Occupation.** Since a plausible mechanism linking the health intervention to increases in labour supply and productivity (employment and wages) is increased skill accumulation, we also examined treatment effects on occupation. We find that programme-led increases in women's employment were concentrated in high-skilled sectors (Table 8). Women exposed to the intervention for a year were 5.0 percentage points (29.4% relative to the baseline mean) more likely to work as managers and professionals and 4.4 percentage points (35.5%) more likely to work in accounting, banking and administration. In contrast, we see a reduction in the share of men in the professional-management category and an increase in the share of men in sales. Table 8 reports mean earnings by occupation and these data confirm that the highest paying occupation was professional-management [42], so these findings line up with the earnings results. Consistent with the employment estimates, these results show a reduction in the *out of the labour force*

[41]A potential contributor to understanding why we find larger impacts of the infant health intervention on women than men in Sweden while Bütikofer et al. (2015) find similar impacts for men and women in Norway is that the structural economic transformation, from agriculture to industry and rural to urban progressed earlier in Sweden. In line with this, publicly provided child care and maternity leave were advanced earlier in Sweden, and married women joined the labour force about a decade earlier (Eeg-Henriksen, 2008). A full explanation is outside the scope of this study.

[42]Mining shows a higher return for women though not for men. We disregard this aberration as 0.001% of women are in mining.

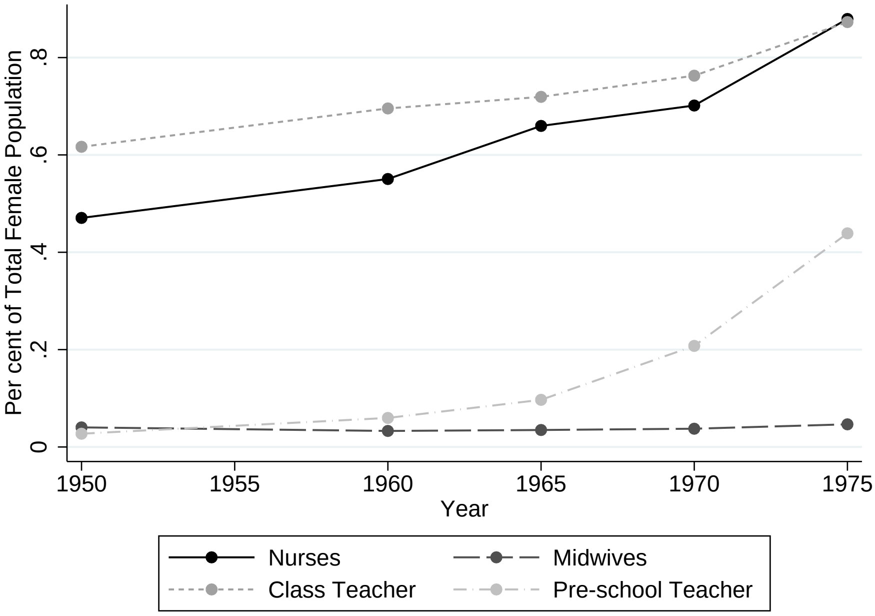



**Figure 9.** Females working in public sector jobs *Source: Statistiska Centralbyrån (2009).*

group for women but not for men. Disaggregating the occupational categories that attracted women further, we find that the largest increase in this category comes from women working in the health sector, for instance as midwifes or nurses (results in appendix table H12).

**Table 8.** Occupational Sorting

| | Men & Women (N=20,920) | | | | Women (N=10,301) | | | | Men (N=10,619) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | | | | Mean | | | | Mean | | | |
| | *Outc.* | *Earn.* | (1) | (2) | *Outc.* | *Earn.* | (3) | (4) | *Outc.* | *Earn.* | (5) | (6) |
| A. Managers, Professionals | 0.200 | 35,473 | 0.0096 | 0.0057 | 0.176 | 23,909 | 0.0427** | 0.0495*** | 0.224 | 44,196 | -0.0229 | -0.0373** |
| | | | (0.014) | (0.011) | | | (0.019) | (0.019) | | | (0.021) | (0.019) |
| B. Accounting, Admin. | 0.081 | 22,408 | 0.0121 | 0.0114 | 0.124 | 18,825 | 0.0388 | 0.0443* | 0.036 | 32,997 | -0.0141 | -0.0210 |
| | | | (0.010) | (0.010) | | | (0.027) | (0.025) | | | (0.016) | (0.017) |
| C. Sales | 0.083 | 23,504 | -0.0148 | -0.0016 | 0.083 | 13,063 | -0.0245 | -0.0226 | 0.083 | 33,742 | -0.0052 | 0.0191* |
| | | | (0.014) | (0.009) | | | (0.018) | (0.017) | | | (0.014) | (0.011) |
| D. Agricultural | 0.059 | 18,082 | 0.0090 | 0.0078 | 0.026 | 3,260 | 0.0099 | 0.0070 | 0.093 | 21,976 | 0.0081 | 0.0085 |
| | | | (0.007) | (0.008) | | | (0.007) | (0.007) | | | (0.012) | (0.014) |
| E. Mining | 0.018 | 29,146 | 0.0027 | 0.0013 | 0.001 | 24,678 | 0.0007 | 0.0003 | 0.036 | 29,266 | 0.0047 | 0.0024 |
| | | | (0.004) | (0.005) | | | (0.001) | (0.001) | | | (0.008) | (0.009) |
| F. Transport, Comm. | 0.055 | 25,173 | -0.0041 | 0.0040 | 0.031 | 17,346 | -0.0081 | -0.0062 | 0.079 | 27,522 | -0.0002 | 0.0141 |
| | | | (0.010) | (0.010) | | | (0.012) | (0.011) | | | (0.013) | (0.015) |
| G. Crafts | 0.194 | 25,075 | -0.0169 | -0.0224* | 0.006 | 31,335 | -0.0206 | -0.0161 | 0.335 | 26,632 | -0.0131 | -0.0286 |
| | | | (0.012) | (0.013) | | | (0.019) | (0.018) | | | (0.020) | (0.021) |
| H. Service | 0.086 | 16,283 | 0.0098 | 0.0104 | 0.130 | 11,288 | -0.0087 | -0.0033 | 0.041 | 29,953 | 0.0278 | 0.0238 |
| | | | (0.012) | (0.015) | | | (0.015) | (0.016) | | | (0.019) | (0.020) |
| I. Out of LF | 0.224 | 3,390 | -0.0074 | -0.0166 | 0.370 | 2,282 | -0.0301 | -0.0528** | 0.072 | 9,665 | 0.0149 | 0.0190 |
| | | | (0.012) | (0.013) | | | (0.024) | (0.026) | | | (0.014) | (0.015) |
| Parish FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| QOB×YOB FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| SES Effects | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| School Reforms | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| Parish Trends | | | | ✓ | | | | ✓ | | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. We provide means of the dependent variables as shares of men and women working in the occupational category at baseline (Outc.) and also mean earnings for each occupation (Earn.). Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* allows for parish specific time trends.

In Table 9 we provide descriptive statistics showing, for each occupation, its share and its skill content. Indicators of skill content displayed include share of workers with secondary occupation, average GPA and the average task content classified as routine vs non-routine cognitive vs non-cognitive as in Autor et al. (2003). The top panel is based on the entire sample, whereas the bottom panel excludes individuals with secondary schooling. The two highest-ranked occupational groups ('Managers & Professionals' and 'Accounting, administrative') attract the largest share of secondary schooling graduates, workers with higher GPA, and they are characterised by task profiles that are low in non-routine manual tasks and high in terms of cognitive and routine manual tasks. The 'Accounting, administrative' category also attracted highly skilled individuals from the pool of workers without secondary schooling, as indicated by the lower panel.

**Table 9.** Descriptive Statistics: Skills and Task Content by Occupation.

| | **Share** | | **Occupational Tasks** | | | | | **Grades** |
|---|---|---|---|---|---|---|---|---|
| | Occ. Group | Sec. Educ. | Nonr. Manual | Routine Manual | Nonr. Cogn. Interactive | Routine Cog. | Nonr. Cogn. Analytic | GPA |
| **Panel A: Men and Women** | | | | | | | | |
| All | 0.76 | 0.20 | 1.568 | 3.889 | 1.772 | 4.488 | 3.488 | -0.009 |
| SD | 0.42 | 0.40 | 1.375 | 1.087 | 2.596 | 3.714 | 1.950 | (0.769) |
| Managers & Professionals | 0.20 | 0.47 | 1.400 | 4.224 | 3.029 | 3.555 | 5.301 | 0.304 |
| Accounting, Admin. | 0.07 | 0.32 | 0.114 | 4.841 | 0.632 | 7.798 | 3.273 | 0.318 |
| Sales | 0.07 | 0.17 | 0.595 | 3.511 | 2.669 | 0.945 | 4.580 | 0.091 |
| Agricultural | 0.06 | 0.05 | 2.418 | 2.935 | 4.189 | 2.284 | 3.006 | -0.166 |
| Transport, Comm. | 0.06 | 0.09 | 2.882 | 3.257 | 1.191 | 2.267 | 2.162 | -0.154 |
| Crafts | 0.20 | 0.02 | 1.856 | 4.287 | 0.425 | 7.988 | 2.759 | -0.321 |
| Service | 0.09 | 0.08 | 1.511 | 2.902 | 0.990 | 1.329 | 1.798 | -0.066 |
| **Panel B: Men and Women / No Secondary Education** | | | | | | | | |
| All | 0.76 | 0.00 | 1.671 | 3.840 | 1.468 | 4.614 | 3.177 | -0.150 |
| SD | 0.43 | 0.00 | 1.409 | 1.041 | 2.376 | 3.766 | 1.810 | (0.769) |
| Managers & Professionals | 0.13 | 0.00 | 1.459 | 4.329 | 2.337 | 3.741 | 5.052 | 0.031 |
| Accounting, Admin. | 0.06 | 0.00 | 0.124 | 4.879 | 0.629 | 7.924 | 3.303 | 0.168 |
| Sales | 0.07 | 0.00 | 0.611 | 3.532 | 2.489 | 0.894 | 4.502 | 0.025 |
| Agricultural | 0.08 | 0.00 | 2.419 | 2.933 | 4.143 | 2.279 | 2.970 | -0.202 |
| Transport, Comm. | 0.06 | 0.00 | 3.004 | 3.160 | 1.103 | 2.016 | 2.084 | -0.225 |
| Crafts | 0.24 | 0.00 | 1.859 | 4.288 | 0.425 | 7.985 | 2.761 | -0.339 |
| Service | 0.10 | 0.00 | 1.495 | 2.902 | 0.972 | 1.335 | 1.777 | -0.117 |

*Note: Notes*: Descriptive Statistics for Tasks. **Columns:** (2) Share in Occ. Group 1970 (3) Share with Secondary Education Within Occupational Group (4)-(8) Average Tasks for Occupational Group (9) GPA in Primary School. *Source:* Linked 1970 Census. Own calculations. Occupational Tasks based on Autor et al. (2003).

# 6 Mediators

Identifying mediators is a central challenge in longitudinal studies of early life interventions (Heckman et al., 2013). Traditionally, mediation analysis has required either having two sources of exogenous variation or imposing strong and often implausible assumptions regarding the relationship between treatments, mediators and main outcomes. For this reason, it has been customary to report the effects of an intervention on potential mediators alongside effects on the final outcomes of interest, without attempting to weight the contributions of alternative mediators. We provided results using this approach in the preceding section.

In recent years, a number of approaches requiring less restrictive assumptions have been suggested. Identification is typically based on a sequential ignorability condition, which states that the unobserved variables that confound the relationship between the treatment and the mediator are different from those that confound the relationship between the mediator and the outcome, conditional on treatment (cf. Heckman and Pinto, 2015; Huber et al., 2017; Dippel et al., 2017). This independence assumption may be plausible in many settings, but in our case, where most outcomes considered are proxies of human capital, it seems difficult to defend such an assumption. We therefore develop a simple approach that can gauge the relatedness of the treatment effect of the intervention over different domains. In essence, we examine whether it is the same sub-populations that contribute to the treatment effects in different domains. Under some relatively plausible assumptions, it is possible to determine the extent to which the treatment effects in different outcome domains overlap.[43]

We complement this analysis with the approach developed by Gelbach (2016), which leverages the omitted variable bias formula to attribute treatment effects across potential mediators. The Gelbach approach does not have the ambition of estimating causal effects, and is essentially agnostic about the causal and temporal ordering of potential mediators. Thus, if the treatment effects on different mediators are strongly correlated, the method may deliver misleading results. For example, if one potential mediator (e.g. high-ranking occupation) is a direct consequence of a mediator that was operative at an earlier stage of the life course (e.g. secondary schooling completion) but more strongly correlated with the main outcome (e.g. earnings), then the Gelbach approach may attribute the treatment effect to the later rather than the earlier life course variable. We attempt to (partially) address this pitfall by using insights from our analysis of correlated effects to formulate a specification for the Gelbach (2016) decomposition.

[43] A similar approach has been used in Deuchert et al. (2016), but their approach requires observing the value of the mediator for treated individuals before treatment, and identification is based on this mediator having no effect on the outcome in the pre-treatment period. Thus, their approach cannot be applied to our research design.

## 6.1 Attribution of Effects

In Appendix E, we show that the estimated average treatment effect on an interaction between two binary outcomes (i.e. $Y = W \cdot Z$), denoted $\tau_Y$, carries information on how strongly the treatment effects within domains defined by the two binary outcomes W and Z relate. First, we may compare $\tau_Y$ to the benchmark value $\tau_Y^{uc}$ that it would take on if the treatment effects in the two domains were completely unrelated at the individual level:

$$\tau_Y^{uc} = \tau_W \tau_Z + \tau_W \Pr\left(Z^0 = 1\right) + \tau_Z \Pr\left(W^0 = 1\right), \tag{1}$$

where $\tau_W$ and $\tau_Z$ are the average treatment effects on the two outcomes $W$ and $Z$ and $\Pr\left(Z^0 = 1\right)$ is the (estimable) counterfactual probability of observing $Z = 0$ in the treatment group in the absence of treatment, and $\Pr\left(W^0 = 1\right)$ is analogously defined.

Table 10 shows the relatedness of the treatment effects of the intervention for a number of outcomes that exhibit economically and statistically significant results for women. The first two columns present the estimated treatment effect on the two outcomes mentioned in the leftmost column. For example, the first row shows that exposure to the intervention is associated with an increase in the probability of scoring a high GPA in primary school (grade 4 top 20%) of 10.55 percentage points, and an increase in the probability of secondary schooling of 5.2 percentage points.[44] The third column presents $\tau_Y^{uc}$ which is the benchmark value of $\tau_Y$, the effect on the interacted outcome (top GPA *and* secondary schooling), which would obtain if the treatment effects were uncorrelated. In this particular example, this is 3.4 percentage points. However, the unrestricted treatment effect for this joint outcome, presented in column (4), is almost twice that number, indicating that the treatment effects on the two outcomes are strongly correlated. The three rightmost columns present the estimated correlation coefficient between the two treatment effects.[45]

Table 10 exhibits some striking patterns. First, the estimated value of $\tau_Y$ is always well above the benchmark value $\tau_Y^{uc}$, typically twice as large, suggesting that the treatment effects are strongly correlated for all pairs of outcomes (the correlation coefficient is always greater than 0.5 for the maintained assumption on compliers).Treatment effects on earnings are highly

[44] These are essentially the results from the previous section. They are slightly different because slightly different samples are occasioned now by the requirement that both outcomes are observed for a given individual.

[45] The baseline estimate is based on the assumption that individuals who are compliers for only one of the outcomes are proportionately drawn from the populations of never-takers and always-takers in the other variable. As a sensitivity check we present estimates in square brackets that are obtained with variations in this assumption, allowing that the compliers for one outcome who are never-takers for the other outcome are either strongly under-represented or strongly over-represented; see Appendix E for details.

correlated with treatment effects on each of high-ranking occupation (0.60), secondary schooling (0.58) and top GPA (0.54).[46]

**Table 10.** Correlated Treatment Effects – Women

| Outcome 1 | Outcome 2 | (1) $\tau_1$ | (2) $\tau_2$ | (3) $\tau_Y^{uc}$ | (4) $\tau_Y$ | (5) | (6) corr $(\tau_{1i}, \tau_{2i})$ | (7) |
|---|---|---|---|---|---|---|---|---|
| Top GPA (Primary) | | | | | | | | |
| | Secondary | 0.1055* | 0.0519* | 0.0337 | 0.0664*** | | 0.7738 | |
| | | (0.062) | (0.027) | | (0.024) | [ 0.5332 | – | 0.8426 ] |
| | High Occ | 0.1044* | 0.0631 | 0.0485 | 0.0856** | | 0.9848 | |
| | | (0.063) | (0.056) | | (0.039) | [ 0.9524 | – | 0.996 ] |
| | Top Income | 0.1044* | 0.0837* | 0.0465 | 0.0704* | | 0.5420 | |
| | | (0.063) | (0.050) | | (0.041) | [ 0.1697 | – | 0.6484 ] |
| Secondary Schooling | | | | | | | | |
| | High Occ | 0.0396** | 0.0815** | 0.0276 | 0.0458*** | | 0.6121 | |
| | | (0.017) | (0.038) | | (0.014) | [ 0.2426 | – | 0.7177 ] |
| | Top Income | 0.0396** | 0.0649** | 0.0212 | 0.0392*** | | 0.5825 | |
| | | (0.017) | (0.033) | | (0.013) | [ 0.2213 | – | 0.6857 ] |
| High Occupation | | | | | | | | |
| | Top Income | 0.0817** | 0.0650** | 0.0376 | 0.0568** | | 0.6005 | |
| | | (0.038) | (0.033) | | (0.024) | [ 0.2748 | – | 0.6936 ] |

*Note:* $\tau_Y^{uc}$: benchmark value, uncorrelated effects (see Appendix E for a derivation); $\tau_1$: treatment effect outcome 1; $\tau_2$: treatment effect outcome 2; $\tau_Y$: joint treatment effect for interacted outcome $1 \times 2$; corr $(\tau_{1i}, \tau_{2i})$: Correlation coefficient between treatment effects (Bounds for alternative assumptions in square brackets; see Appendix E for a derivation).

The preceding results suggest that a plausible sequence of events leads from better primary school performance to secondary school completion and hence better occupations and higher earnings. However, the intervention had larger impacts on primary school scores, occupation and earnings than it did on secondary school completion.[47] This suggests there may be an alternative sequence leading directly from test scores to higher earnings, independent of secondary schooling. So as to discriminate between the two paths, in the next section we estimate a relatively flexible specification that introduces interactions with secondary schooling.

## 6.2 Gelbach Mediation Analysis

We used the Gelbach (2016) approach to estimate the relative contribution of endogenous outcomes at different stages of the lifecourse to earnings in adulthood. Denoting by $Y$ a $N \times 1$ vector representing log earnings and by $T$ the $N \times 1$ a vector of treatment assignment, we may compare results from two specifications; one where all potential mediators $Z$ are included as

[46] The strongest correlation in effects is found between top GPA in primary school and high-ranking occupations (defined as managers and professionals, and accounting and administration), the correlation coefficients are greater than 0.98 and robust to different assumptions regarding the distribution of compliers in the population. The next largest correlation between treatment effects for top GPA and secondary schooling, at 0.77.

[47] We showed earlier that the intervention had the following impacts: probability of a top GPA increases by about 10 percentage points and the probability of earning a top income increases by 7-8 percentage points, but secondary school completion increases by only 4 percentage points.

covariates, and a base specification which only includes the base covariates and fixed effects $X$:

$$Y = T\tau + X\lambda + \epsilon \tag{2}$$

$$Y = T\tau + Z\beta + X\lambda + \upsilon \tag{3}$$

Let $\hat{\tau}_{base}$ denote the estimate of $\tau$ based on specification (2), and $\hat{\tau}_{full}$ denote the estimate of $\tau$ based on specification (3). As shown by Gelbach (2016), their difference $\hat{\delta} = \hat{\tau}_{base} - \hat{\tau}_{full}$ represents an estimate of how much of the estimated effect can be attributed to the mediating variables $Z$. This decomposition of the effect does not have a causal interpretation since the exogeneity assumption $\mathbb{E}(\upsilon \mid T, Z, X) = 0$ may be violated even if the base specification (2) is identified. Nevertheless, the decomposition gives an indication of the quantitative importance of different potential mediators and their respective contributions to the overall treatment effect $\tau_{base}$. The contribution of variable $k$ can be quantified as $\hat{\delta}_k = \hat{\Gamma}_k \hat{\beta}_k$; where $\hat{\Gamma}_k$ represents the effect of the intervention on mediator $k$, and $\hat{\beta}_k$ is the estimate for this variable in specification (3).

Table 11 presents results for women.[48] In the top panel, we present estimates of the effects of the infant health intervention on a series of endogenous outcomes, and in the lower panel, we show how these can be attributed to potential mediators. The estimates show that intervention-led improvements in the chances of scoring in the top quintile of the primary school test score distribution account for about two-thirds (66.12%) of the increase in secondary school completion. Secondary schooling, in turn, accounts for a third (28.92%) of the increase in the chances of being in a high-ranking occupation. Secondary education seems key since having a top GPA but failing to achieve secondary schooling has a much smaller and imprecisely determined impact on high occupation. Holding a high-ranking occupation and having secondary schooling explains about half (48.39%) of the intervention-led increase in earnings. Slightly less than half of the increase in earnings is unexplained by the specification.[49]

In each case, the stated links in the chain are the strongest links and this appears to be the predominant trajectory. Although we cannot claim that this is a causal chain, the temporal ordering of the outcomes suggests that, for these early cohorts, the health intervention led to increases in cognitive ability and that the role of primary school performance in determining

[48] Results for an alternative specification which does not interact mediators can be found in Appendix Table H14 and estimates for men, who exhibited no increase in earnings, are in Appendix Table H15.

[49] We have outlined a sequence of extensive margin responses but earnings may also have responded to intensive margin changes, for example, health and cognition related increases in productivity of individuals who would have followed the secondary school pathway irrespective of the intervention.

**Table 11.** Gelbach Mediation Females.

| | Secondary Schooling | | | High Occupation | | | Earnings | | |
|---|---|---|---|---|---|---|---|---|---|
| Treatment Effect | | | 0.0484* | | | 0.0605 | | | 0.1861** |
| SE | | | (0.027) | | | (0.057) | | | (0.091) |
| N | | | 6,105 | | | 6,105 | | | 6,105 |
| Pre-mean | | | 0.189 | | | 0.318 | | | 9.036 |
| Unexplained = Treatment Effect - $\hat{\delta}$ | | | 0.0164 | | | 0.0392 | | | 0.0854 |
| | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ |
| Top GPA | 0.1073* | 0.2951*** | 0.0320* | | | | | | |
| | (0.063) | (0.016) | (0.018) | | | | | | |
| Secondary Schooling | | | | 0.0484* | 0.3652*** | 0.0177* | 0.0484* | -0.0118 | -0.0006 |
| | | | | (0.027) | (0.036) | (0.011) | (0.027) | (0.099) | (0.005) |
| Top GPA & Secondary | | | | 0.0662*** | -0.0030 | -0.0002 | 0.0662*** | 0.1540*** | 0.0102** |
| | | | | (0.024) | (0.039) | (0.003) | (0.024) | (0.034) | (0.005) |
| Top GPA & No Secondary | | | | 0.0422 | 0.0910*** | 0.0038 | 0.0422 | 0.0016 | 0.0001 |
| | | | | (0.046) | (0.018) | (0.004) | (0.046) | (0.039) | (0.002) |
| High Occ & Secondary | | | | | | | 0.0596** | 1.5088*** | 0.0899** |
| | | | | | | | (0.029) | (0.091) | (0.041) |
| High Occ & No Secondary | | | | | | | 0.0009 | 1.2271*** | 0.0011 |
| | | | | | | | (0.043) | (0.038) | (0.052) |

*Note:* $\hat{\beta}$ refers to estimates from full model of interest (dependent variable see columns); $\hat{\Gamma}$ refers to estimates from auxiliary models with each possible mediator acting as dependent variable; $\hat{\delta}$ is component of omitted variable bias estimated to be due to each variable (see Gelbach, 2016).

secondary school and the role of secondary school in determining occupational status were probably important pathways to intervention effects on earnings. These results cohere with our finding that the improvements in all of the post-primary outcomes (namely, secondary schooling, occupation and earnings) were unique to women.

# 7 Robustness Checks

**Treatment indicator.** As discussed in the Empirical Strategy section, we investigate alternative (binary) treatment indicators. The pattern of results is in general robust to these variations, and we learn that early exposure, at 0-3 months, is most effective in modifying outcomes (Appendix Table G9).

**Anchoring of grading scale**. We investigated sensitivity of the results for academic achievement to the grading scale by anchoring the scale to log income in 1970 (Bond and Lang, 2013; Cunha and Heckman, 2008).We regress income $Y$ for individual $i$ in 1970 on each mark $N$ of the 7-point grading scale for each subject $s$ in each grade $g$:

$$\ln Y_{isg} = \alpha + \beta^N \text{Mark}_{isg}^N + \varepsilon$$

This gives mean log income for each of the seven steps on the scale. Results are in Table H19; mark 3 is the reference group. The correlations in fourth grade imply that a switch of test scores from 1 to 6 points is associated with an earnings gain of 95%. Appendix Table H16 shows regression results using income. The estimates are similar to those using the grae scale. The intervention is associated with an increase in 'reading and speaking' and 'writing' performance in grade 4 by about 0.27-0.29% in comparison to the baseline but there are no significant effects on 'math' or 'religion' scores. Anchoring with years of education instead of log income generates similar results.

**Pre-trend test.** To investigate whether the outcomes of interest followed similar trends in the treatment and control regions before the intervention, we use the pre-intervention sample to estimate the following equation:

$$y = \beta(trend \times treated) + \gamma treated + \delta trend + \varepsilon.$$

$Trend$ is a trend variable based on each $month \times year$ observation in the pre-intervention sample and $treated$ is an indicator for treated parishes. Since outcomes are gender-specific we conduct tests separately for males and females. A premise of our strategy is that $\beta$ is 0. Results are in Appendix Tables H17 and H18 for primary school and long run outcomes respectively. In general we cannot reject that $\beta$ is 0.[50] The fact that our results are, in general, robust to parish specific time trends also suggests the absence of differential pre-trends.

**Placebos**. For the 1970 Census outcomes we generate placebo estimates. First, we generated a sample of children born in treatment and control areas ten years after the infant intervention, in 1940-44, using information in the 1950 population census and the Swedish Death Index. The information for these cohorts is more limited than in the original sample, the only covariate available being the individual's sex. Based on parish of birth and date of birth we generated an artificial treatment group based on the assumption that the intervention took place ten years after the original intervention. Table 12 shows that the coefficients in these placebo regressions are small and insignificant.[51]

**Randomisation Inference**. We also conduct a randomisation inference test for the long-term outcomes, in the spirit of a placebo test. We randomly assign treatment status within

[50] An exception is male sickness absence in grade 1 but, since we see no test score gains in grade 1, we do not analyse this variable.

[51] The results for the 1940–1944 cohorts have to be viewed with some caution since before 1947 the parish of birth that was reported refers to the location of the hospital they were born in and not to the place of registration of the parents (Holmlund, 2008). With a rising share of institutionalised births over time this leads to some misreporting for our placebo test cohorts. We do not face this problem for cohorts born 1930–1934 since the parish records that were digitised within this project reported the place of registration of the parents and not the place of the hospital they were born in. We also control for hospital births in our regressions.

**Table 12.** Placebo 1940-1944 cohorts long-term outcomes.

| | Secondary Schooling | | Working Fulltime | | Working Parttime | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| DID | -0.0005 | -0.0013 | -0.0002 | -0.0016 | 0.0003 | 0.0008 |
| SE | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| N | 19,110 | 19,110 | 19,339 | 19,339 | 19,339 | 19,339 |
| Pre-Mean | 0.231 | 0.231 | 0.603 | 0.603 | 0.071 | 0.071 |
| | **log Income 1970** | | **Municipal** | | **Governmental** | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| DID | 0.0017 | -0.0013 | -0.0001 | 0.0005 | -0.0007 | -0.0013 |
| SE | (0.004) | (0.003) | (0.001) | (0.001) | (0.001) | (0.001) |
| N | 19,634 | 19,634 | 19,339 | 19,339 | 19,339 | 19,339 |
| Pre-Mean | 9.391 | 9.391 | 0.140 | 0.140 | 0.079 | 0.079 |
| QOB×YOB Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parish Specific Linear Trends | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Note:* Standard errors clustered at the parish level in parenthesis. DID denotes term for placebo exposure to the infant intervention assuming it was implemented 10 years later in the same areas. Apart from *QOB× YOB effects* which indicate inclusion of quarter-of-birth dummies for each of the 20 quarters and *Parish specific trends* testing for parish specific time trends, the only control variable in all specifications is a dummy for being female.

each treatment and control parish pair using 5,000 permutations; see cf. Karlsson and Pichler, 2015 for a discussion of randomisation inference in difference-in-difference settings. In accordance with MacKinnon and Webb (2016) we present randomisation inference results based on $t$ statistics, as this is superior to inference based on coefficients. Figures 10 and 11 plot the distributions of placebo treatment effects by gender and display the actual treatment effect and the corresponding $p$ value. Except for part-time employment for females where the distribution does not look smooth (and for which we concluded earlier that there was no significant intervention effect), the results similar to the main estimates in Tables 3, 5 and 7.

# 8 Conclusion

Using fairly unique longitudinal data in which individual outcomes are observed at different stages of the life course, we identify fairly large impacts of a universal infant care intervention on school and labour market outcomes. The intervention was of low cost relative to its benefits, and it was successfully scaled up following the short trial period that we analyse. Our findings are of contemporary relevance given that poor health and nutrition and deficient early childhood care are predicted to be causing about 200 million children under the age of 5 to fail to attain their cognitive potential, and that this has been identified as a key factor in the intergenerational

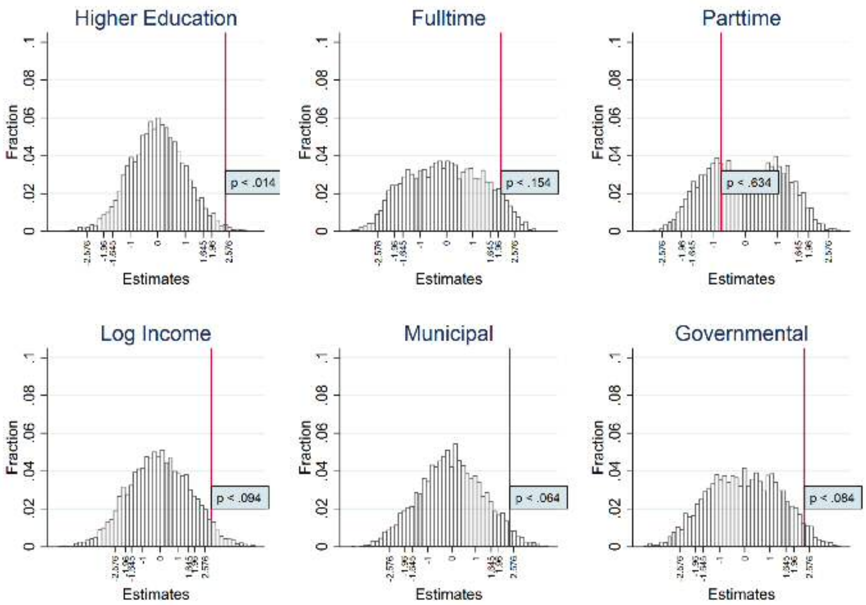



**Figure 10.** Randomisation inference long-term outcomes females.

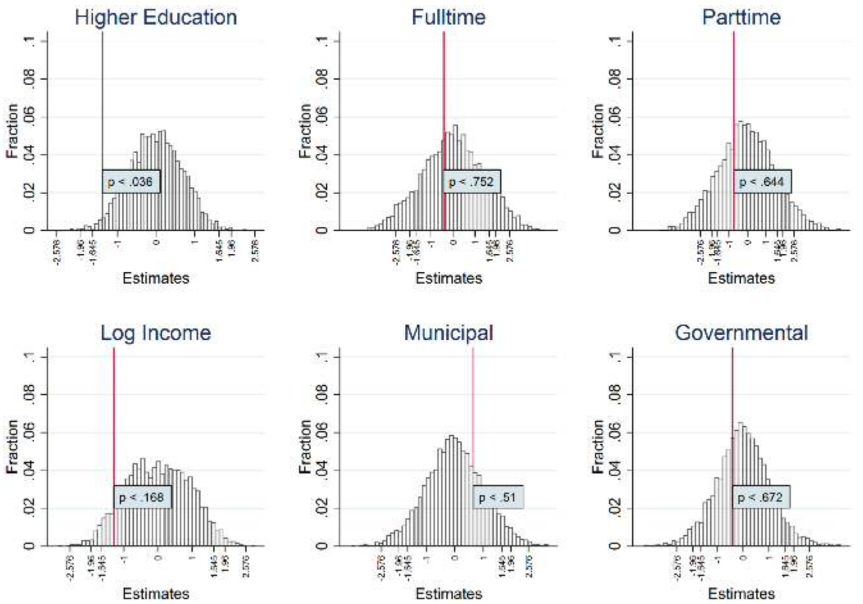



**Figure 11.** Randomisation inference long-term outcomes males.

transmission of poverty (Grantham-McGregor et al., 2007).

The infant health intervention improved cognitive performance in primary school for boys and girls, with striking distributional differences. Mean test score gains were larger for boys, but gains at the top of the performance distribution favoured girls. In this era, the probability of attending secondary school was substantially higher for children who attained test scores in the top quintile of the distribution. Consistent with this, we document intervention-led increases in secondary school completion for girls, with no increase among boys. Labour market outcome gains associated with the intervention are also restricted to women, who exhibit increases in employment, primarily in highly-paid public sector occupations, and increases in income, concentrated in the top quintile.

Intervention effects are highly correlated across outcomes, implying that it is largely the same individuals who drive the various effects. With the caveat that it is only descriptive, our analysis of mediators suggests that cognitive attainment, via secondary schooling, was an important contributor to the increase in adult earnings. The analysis also highlights the importance of institutional capacity (rationing of secondary school places) and demand conditions (demand for women's labour). Entry to secondary school was competitive and this led to more girls and not boys progressing into secondary schooling as a result of the intervention. When these women emerged onto the labour market, their participation and their movement into high-earning sectors was facilitated by substantial growth in women-friendly public sector jobs (e.g. nursing, midwifery) created by rapid expansion of the welfare state.

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# A Variable Definitions

## A.1 Information from Parish Records

**Female** Dummy variable taking on the value one for female births.

**Twin** Dummy variable taking on value one for (mono- and dizygotic) twins.

**Wedlock** Dummy variable taking on value one for children born to married mothers.

**Mother<20** Dummy variable taking on value one for mothers younger than 20 years at time of birth.

**Mother>35** Dummy variable taking on value one for mothers older than 35 years at time of birth.

**Hospital birth** Dummy variable taking on value one for child being born in hospital.

**Treated** Dummy variable taking on value one for children born in treated areas.

**TreatmentI** Dummy variable indicating eligibility for infant care intervention during at least the first three months in life.

**DurationI** Variable indicating eligibility for infant care intervention in years.

**TreatmentM** Dummy variable indicating eligibility for maternal care intervention during at least the first three months in life.

**DurationM** Variable indicating eligibility for maternal care intervention in years.

**SES** Classification of head of household profession according to HISCO 9-point scale (Leeuwen et al., 2002).

## A.2 Variables from Exam Catalogues:

**Share Sickn. Abs.** Share of school days spend absent due to sickness in grade 1 or 4.

**Writing** Mark for "writing" in grade 1 or 4.

**Reading/Speaking** Mark for "reading and speaking" in grade 1 or 4.

**Math** Mark for "math" in grade 1 or 4.

**Religion** Mark for "religion" in grade 1 or 4.

**GPA** Grade point average of subjects "math", "reading and speaking" and "writing" in grade 1 or 4.

## A.3 Variables from 1970 Population and Household Census:

**Only Primary** Dummy variable taking on value one for someone having only primary education.

**Dropout Secondary** Dummy variable taking on value one for someone who attended but did not finish secondary school.

**Secondary Schooling** Dummy variable taking on value one for someone having higher education than *Folkskola*.

**Working Fulltime** Dummy variable taking on value one for someone working at least 35 hours per week.

**Working Parttime** Dummy variable taking on value one for someone working at least 20 but not more than 34 hours per week.

**log Income** Logarithmised taxable labour earnings. Imputed an income based on qualification and hours worked for those having zero income and made a log+1 transformation for remaining zero incomes.

**Top Income 1970** Dummy variable taking on value one for someone at the upper 20% of the earnings distribution.

**Municipal (public) Employment** Dummy variable taking on value one for someone working in the municipal (public) sector. Lower (local) level of government.

**Governmental (public) Employment** Dummy variable taking on value one for someone working in the governmental (public) sector. Higher (state) level of government.

**Scientific, Medical, Technical** Dummy variable taking on value one for someone working in the scientific, medical or technical branch.

**Admin.** Dummy variable taking on value one for someone working in the administrative branch.

**Accounting, Admin.** Dummy variable taking on value one for someone working in the accounting branch.

**Sales** Dummy variable taking on value one for someone working in the sales branch.

**Agricultural** Dummy variable taking on value one for someone working in the agricultural or fishing branch.

**Mining** Dummy variable taking on value one for someone working in the mining branch.

**Transport, Communication** Dummy variable taking on value one for someone working in the transport or communication branch.

**Crafts** Dummy variable taking on value one for someone working in the crafts branch.

**Service** Dummy variable taking on value one for someone working in the service branch.

**Out of the Labour Force** Dummy variable taking on value one for someone being out of the labour force or having a non-identified job.

## B Appendix: Swedish Grading System

The grading scale used throughout the period was introduced in 1897, and was applicable to all subjects but not to behavioural marks (these had a shorter scale and much higher concentration in the highest marks). Officially marks were given on a seven-point grading scale which ranged from A (passed with great distinction) to C (failed). Teachers were also allowed to use + and - signs to express the strength or weakness of a mark. A complete list of applied marks and their meaning can be seen in Table B1. At the outset, there was some heterogeneity in how student performance was evaluated, but our investigation period falls into a period of constantly increasing comparability between schools and teachers in their marking of pupil performance.

A pass mark, i.e. at least a B, was required in theoretical subjects to proceed to the next grade.[52] There was, however, some local variation in how this rule was enforced in practice: some districts required a pass mark in all theoretical subjects; some allowed for a maximum of two fails, provided these two are not Swedish and math. Other districts allowed for very high marks in some subjects to offset low marks in other subjects.

Since from 1939 onwards, admission to secondary school was based on marks from primary school, a Royal Commission emphasised that the marking procedure should be improved and standardised much more. Therefore, guidelines for marking were prepared which were published in 1940 and became official starting with the school year 1940/41. These provided general guidelines for the marks and gave further information on individual subjects. It was stated that marks should be defined in a relative sense, meaning that *Ba* is defined as the normal mark which should encompass the middle third of a pupil's cohort. Consequently, one third of the other pupils should fall below this mark and the other third should be above. Only in really exceptional cases pupils obtained the extreme marks C or A. According to the commission, less than one percent of the pupils could be expected to have the knowledge corresponding to the top mark A, which should testify exceptional talent.

[52] There are only very few statistics on how common grade retention was at that time, but a survey in 1940 from the second largest city of Sweden Gothenburg suggests that about 3% of all pupils had to repeat a grade (Paulsson, 1946)

**Table B1.** 1897 grading scale.

| Mark | Name | English |
|---|---|---|
| A | *Berömlig* | Passed with great distinction |
| a | *Med utmärkt beröm godkänd* | Passed with distinction |
| AB | *Med beröm godkänd* | Passed with great credit |
| Ba | *Icke utan beröm godkänd* | Passed with credit |
| B | *Godkänd* | Passed |
| BC | *Icke fullt godkänd* | Not entirely passable |
| C | *Underkänd* | Fail |

*Note:* Official Swedish grading scale from 1897 as described in Section 3 and their English interpretation.

# C Appendix: Matching Procedure

This section provides more details on the matching procedure discussed in Section 4.

The Mahalanobis distance metric is defined as

$$\mathcal{J}_M(i) = \arg\min_j \sqrt{(X_i - X_j)' S^{-1} (X_i - X_j)} \tag{C1}$$

where $X_i$ is a vector of observable characteristics for a parish belonging to a test district. In our case, these are average income; net wealth; employment shares in manufacturing and agriculture; population density; proportion of fertile married women; and a dummy variable for urban locations. $S$ denotes the covariance matrix of the vector of observable characteristics. Since the matching was done before the data collection took place, it does not take our outcome variables into account. This is a virtue insofar as our matching procedure is based on information similar to that available to the decision makers at the time of the intervention. The matching was done in random order and without replacement. Further information on the identification of the control group and the underlying matching procedure is given in Bhalotra et al. (2017).

Table C2 shows 1930 census summary statistics and the standardised difference (Imbens and Woolridge, 2009) between treated districts and their matched control. The standardised difference implies balance across both groups and validates the matching procedure. The same holds for other pre-intervention characteristics from annual medical reports reported in the lower panel.

Table C3 shows descriptive statistics and standardised differences in means for the main outcome variables in our treatment and control regions in the pre-intervention period. None of the outcome variables appears to be unbalanced according to the standardised difference. The small differences that are present should also not be problematic since we control for parish

**Table C2.** Characteristics of matched and control districts.

| | **All** (1) | **Treated** (2) | **All Controls** (3) | **Std. Dif.** (2) vs. (3) | **Matched** (5) | **Std. Dif.** (2) vs. (5) |
|---|---|---|---|---|---|---|
| **Panel A: Matching Characteristics from the 1930 Census.** | | | | | | |
| Agriculture | 0.340 | 0.324 | 0.340 | -0.040 | 0.302 | 0.054 |
| Manufacturing | 0.318 | 0.340 | 0.318 | 0.096 | 0.345 | -0.018 |
| Fertile Married Women | 0.121 | 0.101 | 0.121 | -0.135 | 0.100 | 0.060 |
| Income | 811 | 839 | 810 | 0.042 | 847 | -0.013 |
| Wealth | 2,525 | 2,703 | 2,521 | 0.080 | 2,655 | 0.022 |
| Urban | 0.334 | 0.439 | 0.331 | 0.158 | 0.437 | 0.003 |
| Population | 6,271,266 | 258,418 | 6,004,052 | | 160,987 | |
| **Panel B: Other Pre-Intervention Characteristics.** | | | | | | |
| Live Birth | 0.973 | 0.974 | | | 0.979 | -0.024 |
| Wedlock | 0.836 | 0.888 | | | 0.884 | 0.008 |
| Infant Mortality | 0.055 | 0.063 | | | 0.064 | -0.002 |
| Perinatal Mortality | 0.030* | 0.017 | | | 0.021 | -0.017 |
| Infectious Disease | 0.005* | 0.005 | | | 0.006 | -0.004 |
| Other Causes | 0.020* | 0.041 | | | 0.038 | 0.011 |
| Maternal Mortality | 348.1 | 417.275 | | | 381.785 | 0.004 |
| Mother's Age | 29.45 | 29.455 | | | 29.610 | -0.017 |
| Professional, Technical | | 0.049 | | | 0.038 | 0.037 |
| Administrative, Managerial | | 0.025 | | | 0.016 | 0.046 |
| Clerical | | 0.016 | | | 0.025 | -0.045 |
| Sales Worker | | 0.029 | | | 0.023 | 0.031 |
| Service Worker | | 0.022 | | | 0.010 | 0.071 |
| Agricultural | | 0.297 | | | 0.307 | -0.015 |
| Production Worker | | 0.426 | | | 0.460 | -0.048 |
| Institutional Delivery | 0.242 | 0.335 | 0.239 | 0.151 | 0.273 | 0.096 |
| Weeks Compulsory Schooling | 226.2 | 223.8 | 226.3 | -0.244 | 223.7 | 0.012 |
| Seven Years Compulsory | 0.606 | 0.838 | 0.598 | 0.392 | 0.666 | 0.287 |

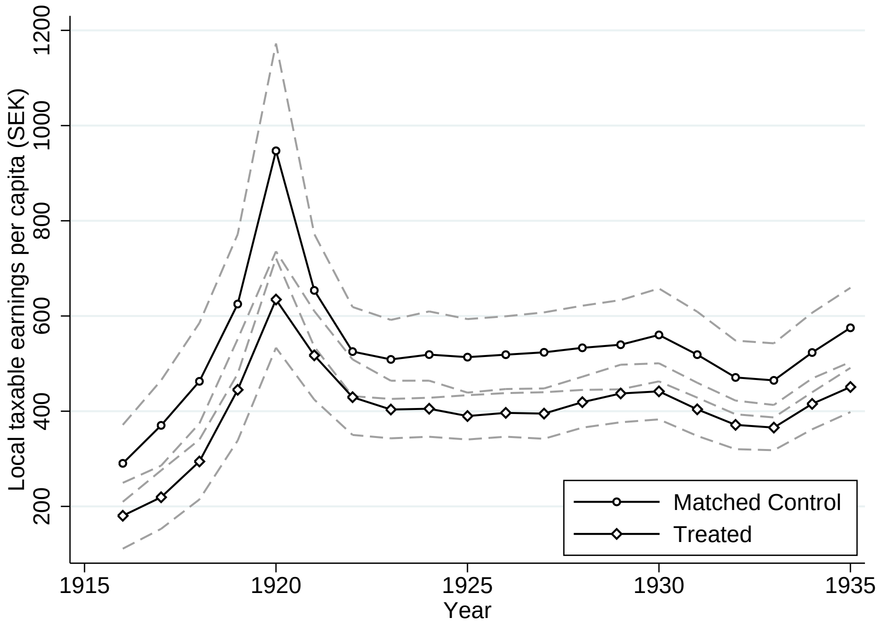
*Panel A* contains local characteristics according to the 1930 census, which were used to match treated parishes to control parishes. *Panel B* contains other local characteristics in the year 1930 which were not available in the 1930 census. Whenever possible, these characteristics are compared with the national averages; however * signifies that national and local statistics not directly comparable. '*Std Dif.*' presents the standardised difference (cf. Imbens and Woolridge, 2009); a standardised difference of less than 0.25 is generally viewed as acceptable.

specific differences and parish specific time trends.

Since we rely on difference-in-differences techniques to estimate the effect of the intervention it is desirable that a) it can be argued that the control group and the treatment group would have followed a common trend in the absence of the intervention; b) there were no contemporary and relevant changes that asymmetrically affected treated areas, and c) the intervention had no indirect impact on the local labour market. In Figure C1 we plot the average taxable earnings in treated and control areas. Even though they have been matched based on 1930 earnings according to the 1930 census, treated municipalities appear to be slightly poorer on average in terms of *taxable* earnings (although the confidence intervals overlap in all years). However, treated and control areas exhibit parallel trends throughout the 1917–35 period and there is no indication that the intervention had a contemporary effect on local earnings.

**Table C3.** Pre-Intervention Balance of Outcomes

| | Control | | | | | Treated | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Count | Mean | SD | Min | Max | Count | Mean | SD | Min | Max | Std Diff. |
| School Data | | | | | | | | | | | |
| Share Sickn. Abs. | 1,923 | 0.043 | 0.062 | 0 | 1 | 2,290 | 0.051 | 0.073 | 0 | 1 | -0.12389 |
| Top GPA | 1,935 | 0.160 | 0.366 | 0 | 1 | 2,353 | 0.210 | 0.407 | 0 | 1 | -0.12867 |
| GPA | 1,935 | 3.510 | 0.702 | 1 | 6 | 2,353 | 3.519 | 0.674 | 1 | 6 | -0.01297 |
| Math | 1,923 | 3.460 | 0.815 | 1 | 6 | 2,344 | 3.516 | 0.825 | 1 | 6 | -0.06779 |
| Reading/Speaking | 1,931 | 3.567 | 0.768 | 1 | 7 | 2,349 | 3.556 | 0.711 | 1 | 6 | 0.01555 |
| Writing | 1,541 | 3.460 | 0.817 | 1 | 7 | 2,005 | 3.419 | 0.771 | 1 | 6 | 0.05180 |
| Census 1970 | | | | | | | | | | | |
| Secondary Schooling | 1,468 | 0.138 | 0.345 | 0 | 1 | 1,919 | 0.178 | 0.382 | 0 | 1 | -0.11016 |
| Working Fulltime | 1,456 | 0.650 | 0.477 | 0 | 1 | 1,904 | 0.641 | 0.480 | 0 | 1 | 0.02018 |
| Working Parttime | 1,456 | 0.130 | 0.337 | 0 | 1 | 1,904 | 0.137 | 0.344 | 0 | 1 | -0.01934 |
| Top Income | 1,470 | 0.202 | 0.402 | 0 | 1 | 1,920 | 0.215 | 0.411 | 0 | 1 | -0.03215 |
| log Income | 1,470 | 9.554 | 1.113 | 0 | 12 | 1,920 | 9.565 | 1.134 | 0 | 12 | -0.00923 |
| Municipal | 1,456 | 0.167 | 0.373 | 0 | 1 | 1,904 | 0.148 | 0.355 | 0 | 1 | 0.05157 |
| Governmental | 1,456 | 0.079 | 0.270 | 0 | 1 | 1,904 | 0.085 | 0.279 | 0 | 1 | -0.02223 |




*Note:* Observations from control group were weighted based on their population size relative to the population size of treated locations they were matched to. Peak in 1920 due to inflation.

**Figure C1.** Local Taxable Earnings Per Capita.

## D Appendix: Utilisation

Table D4 exploits detailed utilisation data measured at the individual level to explore whether the gender driven effects could also be due to the uptake of utilisation for female children. The data stems from nurse and physician records archived for four of the seven health districts and covers a representative sample for about half of the eligible children (Bhalotra et al., 2017). We regress uptake of utilisation on duration of eligibility in years and interact this with a female dummy. Column 1 reports results for a linear model taking into account the number of visits, column 2 estimates a linear probability model with enrolment as a binary indicator and column 3 estimates utilisation conditional on enrolment and thus the intensive margin. As can be seen from the table, eligibility in years is a good predictor for utilisation but there is no higher uptake for female children. Thus, the gender specific effects are not due to gender differences in utilisation.

**Table D4.** Utilisation.

| | **OLS** (1) | **LPM** (2) | **Cond. on Enrolment** (3) |
|---|---|---|---|
| Duration of Eligibility | 3.0086*** | 0.5394*** | 2.6508*** |
| | (0.823) | (0.043) | (0.894) |
| Female Child | -0.0008 | -0.0045 | 0.4962 |
| | (0.116) | (0.049) | (0.308) |
| Female×Duration of Eligibility | 0.4126 | 0.0194 | -0.0388 |
| | (0.367) | (0.059) | (0.739) |
| In-Wedlock Birth | 0.3880 | -0.0042 | 0.8829 |
| | (0.397) | (0.035) | (0.748) |
| Twin Birth | 0.1363 | 0.0993 | -0.5591 |
| | (0.392) | (0.067) | (0.720) |
| Born to Younger Mother | -0.0427 | 0.0272 | -0.2209 |
| | (0.383) | (0.045) | (0.479) |
| Born to Older Mother | -0.0084 | -0.0125 | 0.0732 |
| | (0.105) | (0.026) | (0.248) |
| High SES | 0.4427* | 0.0063 | 0.8628* |
| | (0.251) | (0.029) | (0.514) |
| Low SES | -0.2232 | -0.0815 | 0.2107 |
| | (0.374) | (0.068) | (0.538) |
| Constant | -0.3754 | 0.1340** | 1.1748** |
| | (0.417) | (0.054) | (0.517) |
| N | 2,577 | 2,577 | 1,214 |
| r2 | 0.052 | 0.138 | 0.018 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Outcome variable is uptake of utilisation.

# E Appendix: Attribution of Treatment Effects

## E.1 Definitions

We now derive the attribution of treatment effects that has been conducted in Section 6.1. Consider two binary outcome variables $W$ and $Z$ for which, without loss of generality, the outcome 1 represents the "better" outcome. For example, we may think of $W$ as representing secondary schooling completion and $Z$ representing employment. We assume that individuals are exposed to a binary treatment $D$ with associated treatment effects $\tau_{Wi} \equiv W_i^1 - W_i^0$ and $\tau_{Zi} \equiv Z_i^1 - Z_i^0$, where $W_i^j$ and $Z_i^j$ represents the potential outcome associated with treatment assignment $j$. Clearly, $\tau_{Wi} \in \{-1, 0, 1\}$ and $\tau_{Zi} \in \{-1, 0, 1\}$.[53]

We define the average treatment effects on the treated as $\tau_W = \mathbb{E}\left[\tau_{Wi} \mid D = 1\right]$ and $\tau_Z = \mathbb{E}\left[\tau_{Zi} \mid D = 1\right]$. Clearly, we have $\tau_W \in [-1, 1]$ and $\tau_Z \in [-1, 1]$. We would like to find out the extent to which the treatment effects are correlated so that individuals who positively contribute to $\tau_W$ (i.e. the individuals who have $\tau_{Wi} = 1$) are the same as the individuals who contribute positively to $\tau_Z$ (i.e. the individuals who have $\tau_{Zi} = 1$). In order to do so, we partition the population into 16 distinct groups, depending on their values for $\tau_{Wi}$ and $\tau_{Zi}$.

**Definition 1 (Population Partition)** *Denote by $p_{kl,mn}$ the proportion of the treated population characterised by $(W^0 = k, W^1 = l, Z^0 = m, Z^1 = n)$. Thus,*

$$p_{kl,mn} = \Pr\left(W^0 = k, W^1 = l, Z^0 = m, Z^1 = n \mid D = 1\right) \forall (k, l, m, n) \in \{0, 1\}^4 . \tag{E2}$$

*Also, denote by $p_{kl}^W$ the proportion of the treated population characterised by $(W^0 = k, W^1 = l)$ and by $p_{mn}^Z$ the proportion of the treated population characterised by $(Z^0 = m, Z^1 = n)$.*

For example, the subpopulation $p_{01,01}$ consists of individuals who have $\tau_{Wi} = \tau_{Zi} = 1$ and thus experience an increase in both outcomes when treated. With the population partition, we can define explicitly the average treatment effects:

$$\tau_W = p_{01,00} + p_{01,01} + p_{01,10} + p_{01,11} - p_{10,00} - p_{10,01} - p_{10,10} - p_{10,11} \tag{E3}$$

$$\tau_Z = p_{00,01} + p_{01,01} + p_{10,01} + p_{11,01} - p_{00,10} - p_{01,10} - p_{10,10} - p_{11,10} \tag{E4}$$

[53] We have defined the treatment indicator as binary here, although a small part of our sample has treatment exposure between zero and one. It would be possible to generalise this exposition to allow for non-binary treatment by imposing the assumption that effects are linear in treatment exposure.

This composition of $\tau_W$ (and analogously for $\tau_Z$) follows immediately from the definition $\tau_W = \mathbb{E}\left[W_i^1 - W_i^0 \mid D = 1\right]$: the subpopulations with $W^0 = 0, W^1 = 1$ have size $p_{01,00} + p_{01,01} + p_{01,10} + p_{01,11}$ and the subpopulations with $W^0 = 1, W^1 = 0$ have size $p_{01,11} + p_{10,00} + p_{10,01} + p_{10,10} + p_{10,11}$. The other subpopulations are either always- or nevertakers who do not contribute to the estimated effect.

We may also derive the covariance of the two treatment effects:

$$\operatorname{Cov}\left(\tau_{Wi}, \tau_{Zi} \mid D = 1\right) = \mathbb{E}\left[\tau_{Wi} \cdot \tau_{Zi} \mid D = 1\right] - \tau_W \cdot \tau_Z \tag{E5}$$

$$= p_{01,01} + p_{10,10} - p_{01,10} - p_{10,01} - \tau_W \cdot \tau_Z \tag{E6}$$

Next, we move on to define the benchmark value of correlation between $\tau_{Wi}$ and $\tau_{Zi}$ which would obtain if the two treatment effects are completely unrelated so that $p_{kl,mn} = p_{kl}^W p_{mn}^Z \forall k, l, m, n \in \{0,1\}^4$

**Definition 2 (Uncorrelated Effects)** *We refer to the effects $\tau_{Wi}$ and $\tau_{Zi}$ as **uncorrelated** whenever $p_{kl,mn} = p_{kl}^W p_{mn}^Z \forall k, l, m, n \in \{0,1\}^4$ so that* $\operatorname{Cov}\left(\tau_{Wi}, \tau_{Zi}\right) = 0$

### E.2 Implementation

Our implementation is based on defining a third outcome by interacting the two main outcomes. We thus introduce the new outcome variable $Y = W \cdot Z$ with associated treatment effect $\tau_{Yi}$ and average treatment effect $\tau_Y$. We will now show that the value of $\tau_Y$ is well-defined under the assumption of uncorrelated effects and that the actual estimate for $\tau_Y$ can be compared to this benchmark in order to gauge how strongly correlated the effects are. In addition, we derive how the degree of correlation between treatment effects can be assessed under different assumptions regarding the underlying population.

Analogously to the definitions for $\tau_W$ and $\tau_Z$ above, the average treatment effect on $Y$ is defined as follows:

$$\tau_Y = p_{01,01} + p_{01,11} + p_{11,01} - p_{10,10} - p_{11,10} - p_{10,11} \tag{E7}$$

The first three terms represent individuals who increase their value of the interacted outcome $Y$ due to the treatment: they do so either if they increase both outcomes, or because they are compliers in one outcome and always-takers in the other. We now evaluate the treatment effects under the assumption that the treatment effects are uncorrelated.

**Uncorrelated effects**: In this scenario, the treatment effects on the two outcomes are completely uncorrelated and the estimate for $\tau_Y$ simplifies as follows:

$$
\begin{aligned}
\tau_Y^{uc} &= p_{01,01} + p_{01,11} + p_{11,01} - p_{10,10} - p_{11,10} - p_{10,11} \\
&= p_{01}^W p_{01}^Z + p_{01}^W p_{11}^Z + p_{11}^W p_{01}^Z - p_{10}^W p_{10}^Z - p_{11}^W p_{10}^Z - p_{10}^W p_{11}^Z \\
&= \tau_W \tau_Z + \tau_W \Pr\left(Z^0 = 1\right) + \tau_Z \Pr\left(W^0 = 1\right)
\end{aligned}
$$

For moderately-sized effects, this combined effect will be lower than the individual effects $\tau_W$ and $\tau_Z$.[54] Besides, it is estimable, since $\Pr\left(Z^0 = 1\right)$ and $\Pr\left(W^0 = 1\right)$ are the missing counterfactuals that we impute using the common time trend.

We have thus shown that the uncorrelated case defined above translates into a clear benchmark value for $\tau_Y$, which we denote $\tau_Y^{uc}$. A comparison of $\tau_Y$ to this benchmark reveals whether treatment effects are correlated or not. Next, we impose an additional assumption in order to derive direct estimates of the degree of correlation between effects.

**Assumption 1 (No 'defiers')** *For all subpopulations and outcomes, the treatment effect is non-negative:* $\tau_{Ki} \geq 0 \forall i, K \in \{W, Z\}$.

The implication of assumption 1 is that all subpopulations with 'defiers' have mass zero: $p_{10,10} = p_{10,11} = p_{10,01} = p_{10,00} = 0$ and $p_{11,10} = p_{01,10} = p_{00,10} = 0$. If assumption 1 is satisfied, the three average treatment effects simplify as follows:

$$
\begin{aligned}
\tau_W &= p_{01,01} + p_{01,11} + p_{01,00} \\
\tau_Z &= p_{01,01} + p_{11,01} + p_{00,01} \\
\tau_Y &= p_{01,01} + p_{01,11} + p_{11,01}
\end{aligned} \tag{E8}
$$

Our interest is in the size of the subpopulation $p_{01,01}$, since according to equation (E6) the size of this subpopulation determines how strongly correlated the outcomes are. In order to solve the system (E8) above, we introduce the two parameters $a = \frac{p_{01,00}}{p_{01,11}}$ and $b = \frac{p_{00,01}}{p_{11,01}}$. These parameters thus determine the relative sizes of the relevant subpopulations – and they lead to the solution

$$
p_{01,01} = \frac{\tau_Y - \frac{\tau_W}{1+a} - \frac{\tau_Z}{1+b}}{1 - \frac{1}{1+a} - \frac{1}{1+b}} \tag{E9}
$$

[54] It is not a lower bound, since the two treatment effects could be *negatively* correlated – in which case $\tau_Y$ would take on even lower values. In most applications this possibility can be ruled out, however.

Thus, equation (E9) provides an estimate for $p_{01,01}$ which is defined as long as $1 - ab \neq 0$. Our analysis in section 6.1 is based on finding reasonable values for $a$ and $b$ based on the baseline distribution of $W$ and $Z$ in the population: since we have $\Pr(W = 1) \approx 0.2$ for all the binary outcomes we consider, our central assumption is that $a = b = 4$. This corresponds to a situation where compliers are proportional to the relative sizes of their respective subgroups in the population: $a = \frac{p_{01,00}}{p_{01,11}} = \frac{\Pr(Z^0=0)}{\Pr(Z^0=1)}$. In order to assess the robustness of the findings, we also consider scenarios where the compliers who are nevertakers for the other outcome (i.e. $p_{01,00}$ and $p_{00,01}$) are strongly underrepresented ($a = b = 2$) or strongly overrepresented ($a = b = 8$).

## E.3 Results

Results for females are presented in Table 10 in the main text. Here we present results for males, which are of less interest since no adult outcomes are improved by the intervention – thus making the issue of correlated effects redundant. Since the "no defier" assumption cannot be satisfied for this group (due to some estimated effects being negative) we do not report the correlation coefficients of the effects.

**Table E5.** Correlated Effects Males.

| Outcome 1 | Outcome 2 | (1) $\tau_1$ | (2) $\tau_2$ | (3) $\tau_Y^{uc}$ | (4) $\tau_Y$ | (5) | (6) corr $(\tau_{1i}, \tau_{2i})$ | (7) |
|---|---|---|---|---|---|---|---|---|
| Top GPA (PRIMARY) | | | | | | | | |
| | Secondary | 0.0258 | -0.0189 | 0.0012 | 0.0111 | | | |
| | | (0.033) | (0.036) | | (0.018) | | | |
| | High Occ | 0.0243 | -0.0569 | -0.0026 | 0.0274 | | | |
| | | (0.033) | (0.036) | | (0.024) | | | |
| | Top Income | 0.0243 | -0.0252 | 0.0011 | 0.0254* | | | |
| | | (0.033) | (0.042) | | (0.014) | | | |
| Secondary Schooling | | | | | | | | |
| | High Occ | -0.0314 | -0.0702** | -0.0181 | -0.0307* | | | |
| | | (0.028) | (0.032) | | (0.018) | | | |
| | Top Income | -0.0314 | -0.0418 | -0.0126 | -0.0184 | | | |
| | | (0.028) | (0.034) | | (0.019) | | | |
| High Occupation | | | | | | | | |
| | Top Income | -0.0700** | -0.0414 | -0.0230 | -0.0521* | | | |
| | | (0.032) | (0.034) | | (0.029) | | | |

*Note:* $\tau_Y^{uc}$: benchmark value, uncorrelated effects (see Appendix E for a derivation); $\tau_1$: treatment effect outcome 1; $\tau_2$: treatment effect outcome 2; $\tau_Y$: joint treatment effect for interacted outcome $1 \times 2$; corr $(\tau_{1i}, \tau_{2i})$: Correlation coefficient between treatment effects (Bounds for alternative assumptions in square brackets; see Appendix E for a derivation).

# F Appendix: Heterogeneity

Since the programme was especially targeted at vulnerable groups like children and mothers with a relatively disadvantaged background, we also conduct heterogeneity analyses to explore whether the intervention was beneficial for children born out of wedlock and those born to families of low socio-economic status. Children born out of wedlock were of special concern since they had significantly worse health prospects than other children during that time period (Stenhoff, 1931). Table F6 shows heterogeneity results for standardised marks and standardised GPA in fourth grade. Children born to single mothers experienced larger improvements in fourth grade regarding their GPA, 'reading and speaking' and 'writing' marks. This effect is mainly driven by males born to single mothers (results not shown here). We do not find any significant heterogeneity in treatment effects for long-term outcomes or in the first grade. The improvement in marks is between 0.17 and 0.4 standard deviations if they were eligible to the intervention one more year. This effect is relatively large and three to four times the magnitude of what we find for the whole population. This is in line with the findings of Bhalotra et al. (2017) who also find significant improvements in the reduction of mortality for children born out of wedlock. Although not significant, effects for children born into a low SES environment still point to an improvement in academic performance.

**Table F6.** Heterogeneity in treatment effects academic performance grade 4.

| | **GPA** | | **Math** | | **Reading** | | **Writing** | |
|---|---|---|---|---|---|---|---|---|
| | **Single** | **low SES** | **Single** | **low SES** | **Single** | **low SES** | **Single** | **low SES** |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| DID×$Variable$ | 0.1679 | -0.0500 | 0.1853 | -0.0123 | 0.0544 | -0.0285 | 0.2733 | -0.1086 |
| | (0.143) | (0.077) | (0.170) | (0.102) | (0.168) | (0.095) | (0.173) | (0.087) |
| DID | 0.0760* | 0.1069** | -0.0182 | 0.0017 | 0.1230*** | 0.1381** | 0.1230* | 0.1813*** |
| | (0.039) | (0.041) | (0.048) | (0.067) | (0.046) | (0.056) | (0.065) | (0.065) |
| Treated×$Variable$ | -0.0138 | 0.0380 | -0.0048 | 0.0369 | 0.0636 | 0.0069 | -0.1027 | 0.0703 |
| | (0.085) | (0.059) | (0.112) | (0.067) | (0.093) | (0.078) | (0.104) | (0.054) |
| Parish FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| QOB×YOB Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| School FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| SES Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Length of Schoolyear | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Schoolform | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parish Specific Linear Trends | | | | | | | | |
| N | 13,268 | 13,268 | 13,242 | 13,242 | 13,223 | 13,223 | 13,228 | 13,228 |
| Pre-mean | -0.047 | -0.047 | -0.027 | -0.027 | -0.056 | -0.056 | -0.057 | -0.057 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the household head. *Length of schoolyear* are fixed effects controlling for the reforms concerning the length of the school year. *Schoolform* are fixed effects controlling for the school form as described in Section 3 and *Parish×Birth date effects* allows for parish specific time trends. $Variable$ refers to single mother respectively low SES mother interaction.

# G Appendix: Robustness Checks

## G.1 Selective Survival

To investigate the role of survival selection, we estimate programme effects on 1970 income for subsamples of individuals surviving until age 40, 50, 60, 70 and 75 respectively (see Appendix Table G7).

**Table G7.** Assessing survival selection: log earnings 1970 by age at observation

| | Mean | **All** (1) | **Age 40** (2) | **Age 50** (3) | **Age 60** (4) | **Age 70** (5) | **Age 75** (6) |
|---|---|---|---|---|---|---|---|
| WOMEN | | | | | | | |
| log Earnings | 8.990 | 0.1947*** | 0.1942*** | 0.2184*** | 0.2080*** | 0.2062*** | 0.2592*** |
| | | (0.066) | (0.068) | (0.065) | (0.068) | (0.071) | (0.076) |
| N | | 10,301 | 10,275 | 10,085 | 9,657 | 8,820 | 8,119 |
| MEN | | | | | | | |
| log Earnings | 10.222 | -0.0464 | -0.0459 | -0.0377 | -0.0532* | -0.0750** | -0.0921*** |
| | | (0.036) | (0.036) | (0.033) | (0.031) | (0.032) | (0.033) |
| N | | 10,619 | 10,574 | 10,177 | 9,408 | 8,041 | 7,006 |
| Parish FE | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| QOB×YOB FE | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| SES Effects | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| School Reforms | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parish Trends | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* allows for parish specific time trends.

During the time period of interest Sweden provided a large widow pension. We therefore control for a widow(er) dummy in Table G8. We also interact the widow(er) dummy with the partner's observed income in 1970 (since the pension was a function of the partner's earnings). According to these estimates, widows increase their incomes by 20 per cent on average, and it is proportional to their partner's income, suggesting we proxy it quite well. Importantly, our results are not affected by these inclusions.

## G.2 Alternative Treatment Indicators

- Continuous DID: As in main paper
- Binary any exposure: =1 if duration for infant eligibility> 0 days; 0 otherwise

**Table G8.** DID estimates pension age 71.

| | log Pension Age 71 (1) | (2) | +Widow(er) Dummy (3) | (4) | +Widow(er) x Income Partner (5) | (6) |
|---|---|---|---|---|---|---|
| **Pension Age 71** | | | | | | |
| DIDI | -0.0035 | 0.0187 | -0.0069 | 0.0135 | -0.0026 | 0.0159 |
| | (0.012) | (0.014) | (0.014) | (0.013) | (0.013) | (0.013) |
| Widow | | | -0.0122 | -0.0124 | 0.0138 | 0.0142 |
| | | | (0.024) | (0.026) | (0.017) | (0.018) |
| Widow x income partner | | | | | 0.0004 | 0.0003 |
| | | | | | (0.002) | (0.003) |
| N | 15,964 | 15,964 | 15,854 | 15,854 | 15,854 | 15,854 |
| Pre-mean | 11.789 | 11.789 | 11.789 | 11.789 | 11.789 | 11.789 |
| **Pension Age 71 Females** | | | | | | |
| DIDI | 0.0293 | 0.0711*** | 0.0271 | 0.0654*** | 0.0331* | 0.0688*** |
| | (0.019) | (0.015) | (0.021) | (0.015) | (0.019) | (0.015) |
| Widow | | | 0.2091*** | 0.2059*** | 0.0114 | 0.0116 |
| | | | (0.009) | (0.011) | (0.038) | (0.039) |
| Widow x income partner | | | | | 0.0051*** | 0.0050*** |
| | | | | | (0.001) | (0.001) |
| N | 8,284 | 8,284 | 8,225 | 8,225 | 8,225 | 8,225 |
| Pre-mean | 11.609 | 11.609 | 11.609 | 11.609 | 11.609 | 11.609 |
| **Pension Age 71 Males** | | | | | | |
| DIDI | -0.0400** | -0.0400* | -0.0447*** | -0.0445** | -0.0422** | -0.0434** |
| | (0.017) | (0.020) | (0.017) | (0.020) | (0.016) | (0.020) |
| Widower | | | -0.0124 | -0.0124 | 0.0136 | 0.0142 |
| | | | (0.025) | (0.026) | (0.017) | (0.018) |
| Widower x income partner | | | | | 0.0004 | 0.0003 |
| | | | | | (0.002) | (0.002) |
| N | 7,680 | 7,680 | 7,629 | 7,629 | 7,629 | 7,629 |
| Pre-mean | 11.995 | 11.995 | 11.995 | 11.995 | 11.995 | 11.995 |
| **Parish FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **QOB×YOB Effects** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **SES Effects** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **School Reforms** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Parish Specific Linear Trends** | | ✓ | | ✓ | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish specific linear trends* allows for parish specific time trends. Widow dummy is based on comparison of 1970 marital status and wedlock status during pension age. Income of partner in 1,000 SEK in 1970.

- Binary min 3 months: =1 if eligible to infant intervention $\geq$ any 3 months in total; 0 otherwise
- Binary at least first 3 months: =1 if birth date $\geq$ 1 October 1931 and birth date $\leq$ 31 March 1933; =2 if otherwise treated; 0 control group (omitted category).

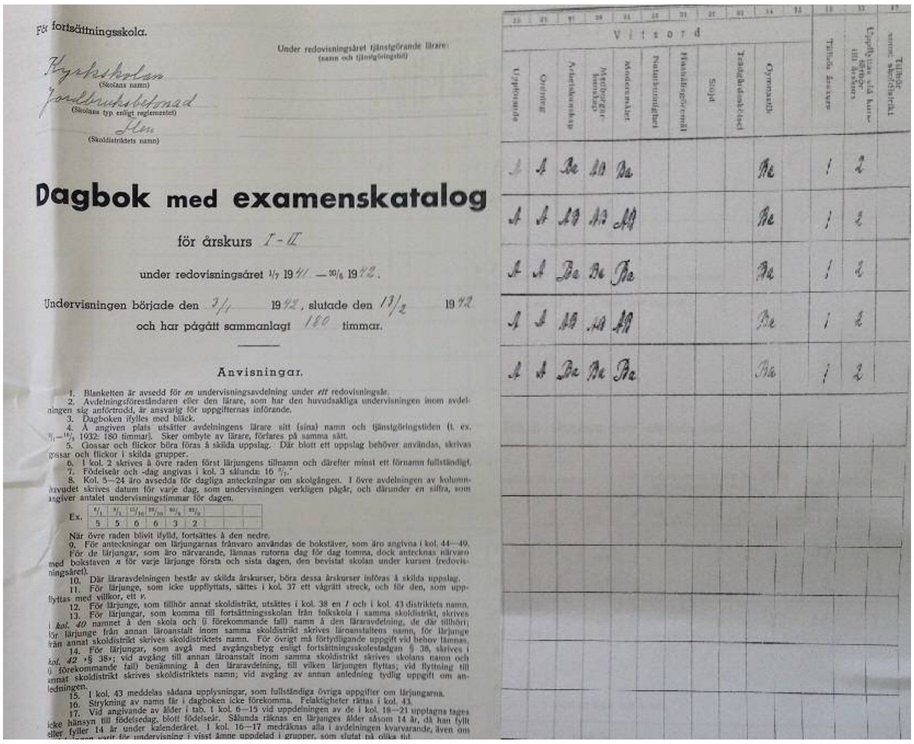
- Binary 12 months/full eligibility: =1 if duration for infant eligibility=12 months; i.e. those born between 1 October 1931 and 30 June 1932; =2 if otherwise treated; 0 control group (omitted category).

**Table G9.** Results for different treatment indicators on long-term outcomes.

| | **log Income 1970** | **Working Parttime** | **Working Fulltime** | **Secondary Schooling** | **Municipal** | **Governmental** |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Continuous DID | 0.0732** | -0.0147 | 0.0349* | 0.0027 | 0.0295** | 0.0131 |
| | (0.028) | (0.017) | (0.020) | (0.013) | (0.013) | (0.014) |
| Binary Any Exposure | 0.0337 | -0.0130 | 0.0198* | -0.0145 | 0.0105 | 0.0121 |
| | (0.033) | (0.013) | (0.012) | (0.017) | (0.012) | (0.010) |
| Binary Min 3 Months | 0.0734*** | -0.0211 | 0.0298** | 0.0051 | 0.0107 | 0.0121 |
| | (0.022) | (0.018) | (0.014) | (0.011) | (0.009) | (0.008) |
| Binary at Least First 3 Months Complete | 0.0854** | -0.0405 | 0.0626*** | 0.0013 | 0.0300 | 0.0117 |
| | (0.033) | (0.027) | (0.021) | (0.014) | (0.021) | (0.014) |
| Binary Other Treated | 0.0052 | 0.0018 | -0.0036 | -0.0221 | -0.0014 | 0.0135 |
| | (0.047) | (0.010) | (0.012) | (0.023) | (0.010) | (0.010) |
| Binary 12 Months/Full Eligibility | 0.0507 | -0.0292 | 0.0479** | 0.0053 | 0.0311 | 0.0028 |
| | (0.037) | (0.019) | (0.022) | (0.014) | (0.023) | (0.013) |
| Binary Other Treated | 0.0270 | -0.0072 | 0.0095 | -0.0208 | 0.0019 | 0.0165 |
| | (0.039) | (0.013) | (0.011) | (0.021) | (0.010) | (0.010) |
| N | 20,920 | 20,722 | 20,722 | 20,910 | 20,722 | 20,722 |
| Pre-mean | 9.593 | 0.145 | 0.640 | 0.185 | 0.167 | 0.081 |
| Parish FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| QOB×YOB Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| SES Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| School Reforms | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parish Specific Linear Trends | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable and *mean income* lists average income in each sector by gender. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish specific linear trends* allows for parish specific time trends.

# H Appendix: Tables and Graphs


För fortsättningsskola.

Kyrkskolan
(Skolans namn)
Jordbruksbetonad
(Skolans typ enligt reglementet)
Hen
(Skoldistriktets namn)

Under redovisningsåret tjänstgörande lärare:
(namn och tjänstgöringstid)

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under redovisningsåret 1/7 1941 — 30/6 1942.

Undervisningen började den 7/1 1942, slutade den 13/2 1942
och har pågått sammanlagt 180 timmar.

Anvisningar.

1. Blanketten är avsedd för *en* undervisningsavdelning under *ett* redovisningsår.
2. Avdelningsföreståndaren eller den lärare, som har den huvudsakliga undervisningen inom avdelningen sig anförtrodd, är ansvarig för uppgifternas införande.
3. Dagboken ifylles med bläck.
4. Å angiven plats utsätter avdelningens lärare sitt (sina) namn och tjänstgöringstiden (t. ex. 1932: 180 timmar). Sker ombyte av lärare, förfares på samma sätt.
5. Gossar och flickor böra föras å skilda uppslag. Där blott ett uppslag behöver användas, skrivas gossar och flickor i skilda grupper.
6. I kol. 2 skrives å övre raden först lärjungens tillnamn och därefter minst ett förnamn fullständigt.
7. Födelseår och -dag angivas i kol. 3 sålunda: 16 3/7.
8. Kol. 5—24 äro avsedda för dagliga anteckningar om skolgången. I övre avdelningen av kolumnhuvudet skrives datum för varje dag, som undervisningen verkligen pågår, och därunder en siffra, som angiver antalet undervisningstimmar för dagen.

| Vitsord | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Uppförande | Ordning | Arbetsduglighet | Modersmålet | Moder(s)målet | Naturkunnighet | Hushållsgöromål | Slöjd | Trädgårdsskötsel | Gymnastik | | Tillhör årskurs | Uppflyttas vid kursens slut till årskurs | Tillhör annat skoldistrikt |
| A | A | Ba | AB | Ba | | | | | Ba | | 1 | 2 | |
| A | A | AB | AB | AB | | | | | Ba | | 1 | 2 | |
| A | A | Ba | Ba | Ba | | | | | Ba | | 1 | 2 | |
| A | A | AB | AB | AB | | | | | Ba | | 1 | 2 | |
| A | A | Ba | Ba | Ba | | | | | Ba | | 1 | 2 | |

**Figure H2.** Exam catalogue in *Folkskola*.

**Table H10.** School form

| | Form | Sample | 1940/1941 |
|---|---|---|---|
| Full Time Attendance | A | 37.42% | 44.9% |
| | B1 | 33.81% | 26.4% |
| | B2 | 18.25% | 19.2% |
| | B3 | 2.53% | 3.3% |
| | D1 | 7.16% | 2.5% |
| | aid-class | - | 1.4% |
| Half Time Attendance | C | 0.53% | 2.1% |
| | D2 | 0.31% | 0.2% |
| | D3 | - | 0.0% |

*Note:* Occurrence of different school forms in our sample in comparison to official statistics (SOU, 1944).

**Table H11.** Descriptive statistics explanatory variables.

| | **All Live Births N=24,390** | | | | **School Data N=16,089** | **Census 1970 N=20,921** |
|---|---|---|---|---|---|---|
| | Mean | SD | Min | Max | Mean | Mean |
| Female | 0.485 | 0.500 | 0 | 1 | 0.493 | 0.492 |
| Wedlock | 0.895 | 0.307 | 0 | 1 | 0.921 | 0.902 |
| Twin | 0.026 | 0.160 | 0 | 1 | 0.023 | 0.023 |
| Treated | 0.566 | 0.496 | 0 | 1 | 0.551 | 0.565 |
| Mother<20 | 0.052 | 0.222 | 0 | 1 | 0.044 | 0.050 |
| Mother>35 | 0.226 | 0.418 | 0 | 1 | 0.238 | 0.223 |
| Hospital Birth | 0.295 | 0.456 | 0 | 1 | 0.253 | 0.298 |
| SES Professional/Technical | 0.029 | 0.168 | 0 | 1 | 0.023 | 0.029 |
| SES Administrative/Managerial | 0.024 | 0.153 | 0 | 1 | 0.020 | 0.024 |
| SES Clerical | 0.015 | 0.121 | 0 | 1 | 0.013 | 0.015 |
| SES Sales | 0.026 | 0.158 | 0 | 1 | 0.023 | 0.025 |
| SES Service | 0.027 | 0.163 | 0 | 1 | 0.019 | 0.026 |
| SES Agricultural | 0.381 | 0.486 | 0 | 1 | 0.405 | 0.386 |
| SES Production | 0.397 | 0.489 | 0 | 1 | 0.410 | 0.399 |
| SES Unknown | 0.101 | 0.301 | 0 | 1 | 0.087 | 0.095 |
| DurationI | 0.353 | 0.402 | 0 | 1 | 0.354 | 0.351 |
| DurationM | 0.257 | 0.315 | 0 | 1 | 0.256 | 0.257 |
| Duration | 0.610 | 0.587 | 0 | 2 | 0.610 | 0.608 |

*Note:* Variable descriptions to this table are available in Appendix A.

**Table H12.** Occupational Sorting

| | Men & Women (N=20,920) | | | | Women (N=10,301) | | | | Men (N=10,619) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | | | | Mean | | | | Mean | | | |
| | *Outc.* | *Earn.* | (1) | (2) | *Outc.* | *Earn.* | (3) | (4) | *Outc.* | *Earn.* | (5) | (6) |
| A1. Technical | 0.050 | 39,326 | 0.0154 | 0.0026 | 0.004 | 24,633 | 0.0022 | -0.0009 | 0.097 | 39,906 | 0.0282 | 0.0060 |
| | | | (0.014) | (0.008) | | | (0.003) | (0.003) | | | (0.028) | (0.016) |
| A2. Health care | 0.047 | 19,403 | 0.0163** | 0.0149** | 0.084 | 18,650 | 0.0324** | 0.0272** | 0.008 | 28,613 | 0.0006 | 0.0029 |
| | | | (0.008) | (0.006) | | | (0.015) | (0.012) | | | (0.006) | (0.006) |
| A3. Pedagogical | 0.061 | 38,271 | -0.0125 | 0.0010 | 0.069 | 30,822 | -0.0031 | 0.0135 | 0.052 | 47,020 | -0.0218 | -0.0114 |
| | | | (0.010) | (0.007) | | | (0.011) | (0.010) | | | (0.014) | (0.009) |
| B1. Bookkeeping | 0.021 | 21,794 | -0.0105* | -0.0038 | 0.031 | 18,565 | -0.0096 | -0.0005 | 0.011 | 33,581 | -0.0113 | -0.0072 |
| | | | (0.006) | (0.006) | | | (0.009) | (0.010) | | | (0.009) | (0.006) |
| B2. Office work | 0.059 | 22,612 | 0.0225** | 0.0152* | 0.093 | 18,917 | 0.0484* | 0.0447* | 0.025 | 32,841 | -0.0029 | -0.0139 |
| | | | (0.011) | (0.009) | | | (0.026) | (0.023) | | | (0.009) | (0.013) |
| Parish FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| QOB×YOB FE | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| SES Effects | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| School Reforms | | | ✓ | ✓ | | | ✓ | ✓ | | | ✓ | ✓ |
| Parish Trends | | | | ✓ | | | | ✓ | | | | ✓ |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish level. Letters A and B refer to the main categories in Table 8. We provide means of the dependent variables as shares of men and women working in the occupational category at baseline (Outc.) and also mean earnings for each occupation (Earn.). Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Mean* refers to the mean value of the outcome variable before the intervention took place. *QOB×YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the parental household head. *School reforms* refers to the extension of compulsory schooling and length of school year reforms and *Parish trends* allows for parish specific time trends.

**Table H13.** Outcome Variable: Education higher than Primary school

| | **Males & Females** (1) | **Females** (2) | **Males** (3) |
|---|---|---|---|
| Math | 0.0708*** | 0.0734*** | 0.0684*** |
| | (0.004) | (0.006) | (0.006) |
| Reading | 0.0466*** | 0.0456*** | 0.0481*** |
| | (0.005) | (0.007) | (0.006) |
| Writing | 0.0561*** | 0.0519*** | 0.0597*** |
| | (0.005) | (0.008) | (0.007) |
| Share Sickness Abs. | 0.1679*** | 0.0795 | 0.2779*** |
| | (0.056) | (0.077) | (0.083) |
| Female Child | -0.0397*** | | |
| | (0.006) | | |
| Born to Younger Mother | -0.0223 | -0.0141 | -0.0297 |
| | (0.015) | (0.022) | (0.022) |
| Born to Older Mother | -0.0106 | -0.0100 | -0.0113 |
| | (0.007) | (0.010) | (0.010) |
| Twin Birth | -0.0516** | -0.0456 | -0.0581** |
| | (0.021) | (0.029) | (0.029) |
| In-Wedlock Birth | 0.0691*** | 0.0664*** | 0.0717*** |
| | (0.013) | (0.018) | (0.017) |
| SES Manag/Administrative | -0.2274*** | -0.2431*** | -0.2067*** |
| | (0.030) | (0.045) | (0.041) |
| SES Clerical | -0.1420*** | -0.1211** | -0.1597*** |
| | (0.034) | (0.051) | (0.045) |
| SES Sales | -0.1999*** | -0.1589*** | -0.2315*** |
| | (0.029) | (0.043) | (0.038) |
| SES Service | -0.2612*** | -0.2543*** | -0.2601*** |
| | (0.032) | (0.045) | (0.047) |
| SES Agricultural | -0.4588*** | -0.4501*** | -0.4625*** |
| | (0.021) | (0.032) | (0.028) |
| SES Production | -0.3956*** | -0.3767*** | -0.4103*** |
| | (0.021) | (0.032) | (0.028) |
| SES Unknown | -0.3828*** | -0.3598*** | -0.4005*** |
| | (0.023) | (0.035) | (0.031) |
| Constant | 0.5170*** | 0.4698*** | 0.5212*** |
| | (0.024) | (0.036) | (0.033) |
| N | 12,241 | 6,081 | 6,160 |
| $R^2$ | 0.205 | 0.185 | 0.227 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Outcome variable is higher education than folkskola.

**Table H14.** Gelbach Mediation – Females

| | Secondary Schooling | | | High Occupation | | | Earnings | | |
|---|---|---|---|---|---|---|---|---|---|
| Treatment Effect | | | 0.0484* | | | 0.0605 | | | 0.1861** |
| SE | | | (0.027) | | | (0.057) | | | (0.091) |
| N | | | 6,105 | | | 6,105 | | | 6,105 |
| Pre-mean | | | 0.189 | | | 0.318 | | | 9.036 |
| Unexplained = Treatment Effect - $\hat{\delta}$ | | | 0.0164 | | | 0.0378 | | | 0.0938 |
| | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ |
| Top GPA | 0.1073* | 0.2951*** | 0.0320* | 0.1084* | 0.0642*** | 0.0070*** | 0.1084* | 0.0398 | 0.0043 |
| | (0.063) | (0.016) | (0.018) | (0.063) | (0.020) | (0.003) | (0.063) | (0.033) | (0.005) |
| Secondary Schooling | | | | 0.0484* | 0.3236*** | 0.0157* | 0.0484* | 0.2029*** | 0.0098 |
| | | | | (0.027) | (0.027) | (0.009) | (0.027) | (0.067) | (0.007) |
| High Occupation | | | | | | | 0.0605 | 1.2922*** | 0.0782 |
| | | | | | | | (0.057) | (0.046) | (0.070) |

*Note:* $\hat{\beta}$ refers to estimates from full model of interest (dependent variable see columns); $\hat{\Gamma}$ refers to estimates from auxiliary models with each possible mediator acting as dependent variable; $\hat{\delta}$ is component of omitted variable bias estimated to be due to each variable (see Gelbach, 2016). Reference category gpa: 1st quintile.

**Table H15.** Gelbach Mediation Males.

| | Secondary Schooling | | | High Occupation | | | Earnings | | |
|---|---|---|---|---|---|---|---|---|---|
| Treatment Effect | | | -0.0243 | | | -0.0636* | | | -0.0388 |
| SE | | | (0.036) | | | (0.035) | | | (0.039) |
| N | | | 6,121 | | | 6,121 | | | 6,121 |
| Pre-mean | | | 0.149 | | | 0.262 | | | 10.214 |
| Unexplained = Treatment Effect - $\hat{\delta}$ | | | -0.0243 | | | -0.0618 | | | -0.0108 |
| | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ | $\hat{\Gamma}$ | $\hat{\beta}$ | $\hat{\delta} = \hat{\Gamma} \times \hat{\beta}$ |
| Top GPA | 0.0213 | 0.3236*** | 0.0074 | 0.0229 | 0.0459 | 0.0010 | 0.0229 | 0.0844 | 0.0019 |
| | (0.034) | (0.039) | (0.011) | (0.034) | (0.031) | (0.002) | (0.034) | (0.053) | (0.003) |
| Secondary Schooling | | | | -0.0243 | 0.4941*** | -0.0120 | -0.0243 | 0.1174* | -0.0029 |
| | | | | (0.036) | (0.023) | (0.017) | (0.036) | (0.060) | (0.005) |
| Top GPA & No Secondary | | | | 0.0122 | 0.0862** | 0.0010 | 0.0122 | -0.0475 | -0.0006 |
| | | | | (0.023) | (0.042) | (0.002) | (0.023) | (0.053) | (0.001) |
| High Occupation | | | | | | | -0.0636* | 0.5107*** | -0.0325** |
| | | | | | | | (0.035) | (0.060) | (0.016) |
| High Occ & No Secondary | | | | | | | -0.0326 | -0.2043*** | 0.0067 |
| | | | | | | | (0.031) | (0.051) | (0.006) |

*Note:* $\hat{\beta}$ refers to estimates from full model of interest (dependent variable see columns); $\hat{\Gamma}$ refers to estimates from auxiliary models with each possible mediator acting as dependent variable; $\hat{\delta}$ is component of omitted variable bias estimated to be due to each variable (see Gelbach, 2016).

**Table H16.** DID estimates for subjects.

| | | Anchored Grading Scale | | | |
|---|---|---|---|---|---|
| | | **Math** | **Reading** | **Writing** | **Religion** |
| | | (1) | (2) | (3) | (4) |
| **Males and Females** | **Grade 1 and 4** | -0.0004 | 0.0175** | 0.0306*** | 0.0028 |
| | SE | (0.010) | (0.008) | (0.012) | (0.009) |
| | N | 26,403 | 26,400 | 22,234 | 26,298 |
| | Pre-mean | 9.907 | 9.913 | 9.906 | 9.909 |
| **Males and Females** | **Grade 1** | -0.0038 | 0.0103 | 0.0255 | -0.0001 |
| | SE | (0.016) | (0.014) | (0.022) | (0.014) |
| | N | 13,161 | 13,177 | 9,007 | 13,060 |
| | Pre-mean | 9.878 | 9.894 | 9.867 | 9.888 |
| | **Grade 4** | -0.0012 | 0.0264** | 0.0286** | 0.0015 |
| | SE | (0.013) | (0.010) | (0.012) | (0.013) |
| | N | 13,242 | 13,223 | 13,227 | 13,238 |
| | Pre-mean | 9.934 | 9.932 | 9.933 | 9.928 |
| **Males** | **Grade 1** | -0.0183 | 0.0201 | 0.0147 | 0.0062 |
| | SE | (0.015) | (0.019) | (0.019) | (0.014) |
| | N | 6,779 | 6,794 | 4,608 | 6,723 |
| | Pre-mean | 9.871 | 9.871 | 9.833 | 9.880 |
| | **Grade 4** | 0.0176 | 0.0365** | 0.0379** | -0.0011 |
| | SE | (0.021) | (0.015) | (0.015) | (0.027) |
| | N | 6,688 | 6,687 | 6,692 | 6,689 |
| | Pre-mean | 9.919 | 9.893 | 9.884 | 9.891 |
| **Females** | **Grade 1** | 0.0105 | 0.0070 | 0.0398 | -0.0039 |
| | SE | (0.028) | (0.015) | (0.030) | (0.020) |
| | N | 6,382 | 6,383 | 4,399 | 6,337 |
| | Pre-mean | 9.886 | 9.916 | 9.898 | 9.896 |
| | **Grade 4** | -0.0171 | 0.0225* | 0.0178 | 0.0088 |
| | SE | (0.013) | (0.013) | (0.016) | (0.014) |
| | N | 6,554 | 6,536 | 6,535 | 6,549 |
| | Pre-mean | 9.949 | 9.969 | 9.979 | 9.964 |
| | Parish FE | ✓ | ✓ | ✓ | ✓ |
| | QOB×YOB Effects | ✓ | ✓ | ✓ | ✓ |
| | School FE | ✓ | ✓ | ✓ | ✓ |
| | SES Effects | ✓ | ✓ | ✓ | ✓ |
| | Length of Schoolyear | ✓ | ✓ | ✓ | ✓ |
| | Schoolform | ✓ | ✓ | ✓ | ✓ |
| | Parish Specific Linear Trends | | | | |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, Standard errors are clustered at the parish-grade level. Covariates which are included in all specifications are a dummy indicating twin births, a dummy for being female, dummies capturing old (>35 years) and young (<20) mothers, a dummy for married women, a dummy indicating a hospital birth and the treatment effect of the maternal intervention. *Pre-mean* refers to the mean value of the outcome variable before the intervention took place. *QOB× YOB effects* include quarter-of-birth dummies for each of the 20 quarters. *Parish FE* are fixed effects for the parish the individual lived in at the time of the birth. *SES effects* are fixed effects for the professional group of the household head. *Length of schoolyear* are fixed effects controlling for the reforms concerning the length of the school year. *Schoolform* are fixed effects controlling for the school form as described in Section 3 and *Parish specific linear trends* allows for parish specific time trends.

**Table H17.** DID pre-trend test primary school.

| | **GPA_cog Stand. Grade 4** (1) | **Math Stand. Grade 4** (2) | **Writing Stand. Grade 4** (3) | **Reading Stand. Grade 4** (4) | **Share Sickn. Abs. Grade 1** (5) | **Share Sickn. Abs. Grade 4** (6) |
|---|---|---|---|---|---|---|
| **Females** | | | | | | |
| Interaction | 0.0003 | 0.0009 | 0.0004 | -0.0002 | 0.0000 | 0.0000 |
| | 0.001 | 0.001 | 0.001 | 0.001 | 0.000 | 0.000 |
| Trend | -0.0007* | -0.0011** | -0.0010** | 0.0001 | 0.0000 | 0.0000 |
| | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Treated | -0.1292 | -0.1222 | -0.1629 | -0.1069 | 0.0117 | 0.0040 |
| | 0.087 | 0.108 | 0.103 | 0.097 | 0.011 | 0.009 |
| Constant | 0.2558*** | 0.2069*** | 0.3591*** | 0.2033*** | 0.0409*** | 0.0440*** |
| | 0.063 | 0.078 | 0.074 | 0.070 | 0.008 | 0.006 |
| N | 1,050 | 1,048 | 1,048 | 1,049 | 1,020 | 1,048 |
| **Males** | | | | | | |
| Interaction | -0.0002 | -0.0001 | -0.0007 | -0.0000 | 0.0001** | -0.0000 |
| | 0.001 | 0.001 | 0.001 | 0.001 | 0.000 | 0.000 |
| Trend | -0.0007* | -0.0011** | -0.0003 | -0.0007 | -0.0001* | 0.0000 |
| | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Treated | 0.0065 | 0.0100 | 0.0822 | -0.0682 | -0.0130 | 0.0131** |
| | 0.087 | 0.107 | 0.104 | 0.100 | 0.009 | 0.006 |
| Constant | -0.0792 | 0.0972 | -0.2206*** | -0.1191 | 0.0584*** | 0.0310*** |
| | 0.063 | 0.077 | 0.075 | 0.073 | 0.007 | 0.004 |
| N | 1,116 | 1,110 | 1,116 | 1,115 | 1,040 | 1,105 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, *Trend* variable is based on *month* × *year* observations; *Treated* refers to a dummy indicating treated parishes; *Interaction* is the interaction of the variables trend and treated.

**Table H18.** DID pre-trend test long-term outcomes.

| | **Secondary Schooling** (1) | **Working Fulltime** (2) | **Working Parttime** (3) | **log Income 1970** (4) | **Municipal** (5) | **Governmental** (6) |
|---|---|---|---|---|---|---|
| **Females** | | | | | | |
| Interaction | -0.0000 | 0.0001 | 0.0000 | -0.0000 | -0.0000 | 0.0000 |
| | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 | 0.000 |
| Trend | 0.0001 | -0.0005** | -0.0000 | -0.0010* | -0.0003 | -0.0000 |
| | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 | 0.000 |
| Treated | 0.0384 | -0.0172 | -0.0012 | 0.0154 | -0.0345 | -0.0022 |
| | 0.038 | 0.048 | 0.044 | 0.116 | 0.042 | 0.023 |
| Constant | 0.1307*** | 0.4277*** | 0.2526*** | 9.0252*** | 0.2894*** | 0.0555*** |
| | 0.028 | 0.036 | 0.033 | 0.086 | 0.031 | 0.017 |
| N | 1,665 | 1,656 | 1,656 | 1,666 | 1,656 | 1,656 |
| **Males** | | | | | | |
| Interaction | 0.0002 | -0.0000 | 0.0001 | 0.0000 | -0.0001 | 0.0001 |
| | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Trend | -0.0002 | 0.0001 | -0.0000 | 0.0001 | 0.0001 | -0.0000 |
| | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Treated | 0.0091 | -0.0043 | -0.0082 | 0.0408 | 0.0030 | 0.0009 |
| | 0.035 | 0.026 | 0.013 | 0.059 | 0.028 | 0.031 |
| Constant | 0.1589*** | 0.9198*** | 0.0184* | 10.1674*** | 0.0851*** | 0.1079*** |
| | 0.027 | 0.020 | 0.010 | 0.045 | 0.021 | 0.024 |
| N | 1,722 | 1,704 | 1,704 | 1,724 | 1,704 | 1,704 |

*Note:* *** p <0,01; ** p <0,05; * p <0,1, *Trend* variable is based on *month* × *year* observations; *Treated* refers to a dummy indicating treated parishes; *Interaction* is the interaction of the variables trend and treated.

**Table H19.** Anchoring.

| | Grade 1 | | | Grade 4 | | |
|---|---|---|---|---|---|---|
| | **Math** (1) | **Reading** (2) | **Writing** (3) | **Math** (4) | **Reading** (5) | **Writing** (6) |
| 1 Point | -1.196*** | -0.696*** | -1.107*** | -0.319 | 0.483 | -0.681*** |
| | (0.263) | (0.262) | (0.325) | (0.252) | (0.761) | (0.254) |
| 2 Points | -0.705*** | -0.620*** | -0.645*** | -0.245*** | -0.231 | -0.331*** |
| | (0.099) | (0.105) | (0.112) | (0.087) | (0.164) | (0.087) |
| 4 Points | 0.296*** | 0.288*** | 0.275*** | 0.282*** | 0.228*** | 0.185*** |
| | (0.050) | (0.048) | (0.070) | (0.049) | (0.049) | (0.048) |
| 5 Points | 0.306* | 0.518*** | 0.552** | 0.522*** | 0.493*** | 0.415*** |
| | (0.170) | (0.164) | (0.254) | (0.059) | (0.060) | (0.061) |
| 6/7 Points | -0.837 | 0.363 | 1.051 | 0.638*** | 0.783*** | 0.533*** |
| | (0.676) | (0.500) | (1.065) | (0.129) | (0.182) | (0.173) |
| Constant | 9.865*** | 9.840*** | 9.870*** | 9.729*** | 9.722*** | 9.819*** |
| | (0.025) | (0.026) | (0.030) | (0.037) | (0.040) | (0.035) |
| $R^2$ | 0.010 | 0.008 | 0.008 | 0.011 | 0.007 | 0.008 |
| N | 12,343 | 12,356 | 8,430 | 12,479 | 12,458 | 12,463 |

*Note:* SE in parenthesis, *** p <0,01; ** p <0,05; * p <0,1. Outcome variable is log income. The point values refer to the 7-point grading scale defined in Section 3 and 4. Reference group category 3. Marks 6 and 7 pooled due to few observations.