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ABSTRACT

Infant Health, Cognitive Performance and Earnings: Evidence from Inception of the Welfare State in Sweden*

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 microdata that track individuals through five 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 and occupational choice at age 16-20, 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 an additional year of exposure was associated with improved reading and writing skills in primary school, and increased enrolment in university and apprenticeship in late adolescence. These changes are larger and more robust for men, but we find increases in secondary school completion which are unique to women. In the longer run, we find very substantial increases in employment (especially in the public sector) and income among women, alongside absolutely no impacts among men. We suggest that this may be, at least in part, because these cohorts were exposed to a massive expansion of the Swedish welfare state, which created more jobs for women than for men.

JEL Classification: I15, I18, H41

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

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1 Introduction

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 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. Increasing attention has been paid to the pre-school environment, parenting styles, and the role of stimulation (Heckman, 2006; Attanasio et al., 2014; World Bank, 2015), relatively less to the potential role of pre-school health.

In the first part of this paper, we estimate causal impacts of an intervention targeting infant health on cognitive performance in grades 1 and 4 of primary school. Since we also have records of sickness-related absence from school, we are able to (weakly) differentiate the impacts of contemporaneous from early life health on test scores. Our first contribution is hence to a fairly thin strand of the literature that seeks to link early life health interventions to cognitive attainment. Analysis of health *interventions* include Chay et al. (2009) who study black-white convergence in test scores as a function of hospital de-segregation in America, Bharadwaj et al. (2013) who document impacts of neonatal care on school test scores in Chile, and Bhalotra and Venkataramani (2013) who investigate impacts of early life exposure to a clean water programme in Mexico on cognitive attainment in middle and late adolescence. *Accidental* exposure to radiation from the Chernobyl disaster is examined in Almond et al. (2009). Other important 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 the 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, we analyse impacts of an intervention. This is important because, as they 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).

A second, possibly unique contribution of this paper is that we are able to track individuals from birth, through to retirement. So, for the individuals for whom we observe cognitive performance in primary school, we also observe their subsequent educational and occupational choices, employment, earnings and pension income. While there is evidence from twin-comparisons that IQ and earnings in adulthood are both increasing in birth weight (Black et al., 2007), evidence of causal impacts of IQ on earnings is scarce. In fact, 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 non-cognitive rather than cognitive skills (Chetty et al., 2011; Heckman et al., 2013). So, 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., 2015; 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 studies are able to link data on skill acquisition and early career choices to earnings in adulthood, a niche that we are able to fill.

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 programs in each of the three Scandinavian countries in the mid-1930s (Hjort et al., 2014; Bütikofer et al., 2015). 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, a healthy diet and monitoring of child health. Similar early childhood home visiting programmes are increasingly being introduced in developing countries (Engle et al., 2007), and there is growing emphasis on universal health coverage (Gorna et al., 2015). The programme had an antenatal and postnatal component and we consistently model exposure of the individual child to both. Since we find no impacts of the antenatal programme, our discussion focuses upon the postnatal (infant) component.

We exploit three features of the programme to aid identification of causal impacts. Most important, we use the eligibility criterion, which was essentially that children aged 0-12 months at any time in the window for which the programme was available, were eligible. Second, programme documents highlight that the government selected seven medical districts where the trial was implemented to be representative of the country. We nevertheless use the (pre-intervention) 1930 census to create matched controls at the parish level. In a robustness check, we also obtain estimates from mother fixed effects. Third, since the trial was of less than two years duration, and announced to be so, we may expect that endogenous fertility and migration play a limited role. The intervention was introduced simultaneously across the seven districts, and we are unaware of any exactly contemporaneous events.[1]

We assimilated longitudinal individual data that track individuals through five stages of the life-course, from birth to retirement. In order to model exposure to the infant programme, we purposively digitised individual birth certificate data from historical parish records to obtain a census of births in every treated parish (or city) in the seven trial districts, and in every matched control. The birth sample contains 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. The birth registers also provide indicators of the socioeconomic status (occupation) of parents.

We gathered school catalogues from regional archives, from which we digitised school grades by subject for grades 1 and 4 (age 7 and 10), and records of sickness-related absences from school. We then linked the birth registers to the primary school registers. We also linked the birth records to the 1950 and 1970 census files and tax records to gain information on the educational and occupational choices of the sample cohorts at ages 16-20, their labour market outcomes at ages 36-40, and their pension income at age 71. The linking of these administrative data was done by first and last name, birth date and birth parish. Linkage of school test scores to earnings allows us to investigate robustness of our estimates for test scores to anchoring (Bond and Lang, 2013; Cunha et al., 2010).

In a companion paper, we matched birth to death registers and showed 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 (24% of baseline risk) and a 2.56 percentage point decline in the risk of dying by the age of 75 (7.0% of baseline risk); see Bhalotra et al. (201x). For the purposes of the current analysis, the result for infant mortality validates the effectiveness of the infant intervention. To the extent that morbidity scales with mortality (Bozzoli et al.,

[1]In contrast, analyses of the nationwide rollout of a similar programme in Denmark (Hjort et al., 2014) and Norway (Bütikofer et al., 2015), 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 the staggered rollout of a programme across regions of a country which is potentially endogenous, while we rely upon individual date of birth which determines eligibility and which is very unlikely in our setting to be endogenous (though we do test for this; see Bhalotra et al. (201x).

2009), we have evidence that programme exposure improved the health of surviving children. Improvements in nutrition, which increase a child's capacity to fight infections are associated with more rapid neurological 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). Our finding that the infant health intervention was associated with lengthened life expectancy and, in particular, lower risks of death from cancer, cardio-vascular disease and infections, is also pertinent here. It suggests that, over and above any impacts that flow from better cognitive performance in school, the infant intervention may have impacted investments in higher education, earnings and pension income through lowering adult morbidities and extending the horizon over which returns to investments accrue (Oster et al., 2013; Soares, 2005).

We now present our findings for each stage of the life cycle. First, we estimate that a year of exposure to the infant health intervention was, on average, associated with a 0.11 s.d. improvement in reading and writing skills at age 10, although there were no discernible effects of exposure at age 7. Mean impacts are 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 (especially the 50th to 80th percentile), but we identify significant effects of similar magnitude among girls in the top 30% of the distribution of scores. Impacts on math scores are small and not significantly different from zero. Boys had lower baseline language skills and a year of programme exposure is estimated to close about half of the reading gap and about a quarter of the writing gap between boys and girls at baseline. The magnitude of the effects we identify is large relative to other interventions.[2] We find no impacts on health indicated by sickness absence. While not conclusive, since sickness absence is a partial indicator of health, this is consistent with the cognitive advantage of exposed children stemming from exposure in infancy.

The sample cohorts (born 1930-1934) are age 16-20 in the 1950 census, where we observe their early career choices. The baseline means indicate that 66% of women and 80% of men were working at this age, 2.5% of both genders were at university and 3 to 4% were in apprenticeships (men) or vocational training (women). We find positive impacts of exposure to the infant health intervention on university enrolment and apprenticeship, which are larger and more robust for men. For men, exposure is associated with a 1.5 percentage point increase in university enrolment

[2] Also while cognitive gains stemming from some pre-school interventions fade (e.g. in Bitler et al. (2016), by grade 1), we find cognitive gains at age 10.

and a 3.2 percentage point increase in apprenticeship, which are large changes relative to the baseline means. The coefficients for women are not significantly different, but are smaller and less precisely estimated. Yet, when these cohorts are observed in the 1970 census at age 36-40, we find large and robust impacts of exposure to the infant programme on secondary school completion, which are only statistically significant for women. One year of exposure is associated with a 3.5 percentage point increase in the chances that women complete secondary school, while the coefficients for men are small and negative. The baseline rates are 20% for women and 17% for men. Since admission to secondary schooling was to a great extent based on primary school grades, this result is consistent with the finding that female gains in primary school performance were concentrated at the top.

Investigating impacts of the infant health intervention on labour market outcomes at ages 36-40, we find large and statistically significant increases in employment (7.6 percentage points, a 20.5% increase relative to a mean of 37%) and income (19.5%) among women,[3] alongside absolutely no impacts among men. To illuminate these results, we investigated impacts of programme exposure on labour force participation, public-private sector jobs, and occupational sorting. A year of exposure among women is associated with a 5 percentage point (13.5%) increase in labour force participation, 4.9 percentage point (20.5%) and 3.4 percentage point (66.5%) increases in the chances of municipal or central government sector jobs, and increases in the probability of being in (a) a scientific, medical or technical occupation and (b) an accounting or administration occupation of 4.6 and 4.3 percentage points respectively. In the pre-intervention period, 92.5% of men were employed and there was no significant change in participation, or in public sector employment as a result of the programme. The only change in occupational sorting among men was an increase in the chances of working in sales of 2.1 percentage points.

We investigate pension income at age 71 as an insensitive measure for income since in 1970 there could have been career interruptions due to childbearing etc. Furthermore, it represents earnings at advanced stages of the career. Consistent with earnings in middle adulthood, we find that a year of exposure to the infant programme was associated with a 7.1% increase in women's pension income. Now, rather than no change, we find that exposure is associated with a reduction in men's pension income. Exploring this further, we find that it can be explained by survival selection, consistent with the marginal survivor being negatively selected.

[3]This is large but it includes incomes rising from zero as our results mark extensive margin changes for many women. Also this effect size is not out of line with other estimates in the literature of the long run impact of early life health conditions on earnings.

To summarize, although programme exposure led to a larger fraction of men than women experiencing increases in test scores in primary school, a larger fraction of women completed secondary school. The test score gains among men narrowed the gender gap in reading and writing skills (which favoured women at baseline) without quite closing it, while increases in secondary schooling widened the pre-intervention gap, which also favoured women. In the 1940s and 1950s, men were more likely to leave school to enter an apprenticeship or to directly join the labour force. From the 1950s onwards, there was a rapid expansion of the welfare state in Sweden which created more jobs for women than for men. These were "women friendly" public sector jobs, including nursing, pre-school and school teaching and our estimates suggest that they disproportionately favoured women exposed to the infant health programme. There was no similar growth in the demand for male workers – and labour market policies tended to reduce male skill premiums.

Our findings reinforce a small literature demonstrating causal effects of infant health on cognitive performance in the school years, but highlight that the earnings payoff to cognitive skills is uncertain, being dependent upon early career choices (which "set the path") and on demand conditions at the time of entry to the labour market. While previous work has discussed changes in the relative demand for female (vs male) labour stemming from recession or technological change, we provide a new perspective on gender-differentiated demand that may be of 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 discusses potential mechanisms. Section 6 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, but in the 1920-1930 period no further improvement in infant mortality took place.[4] This stagnation, together with increasing maternal

[4]Less than 5% of pregnant women actually went to a doctor before giving birth although it was not uncommon to give birth in a maternity ward.

mortality rates from 1920 onwards, and an increasing number of deaths of children occurring during the first few days of life, gave rise to an intense public debate in Sweden how to improve conditions for expectant mothers and newborns.[5] The solution suggested was a field trial.

The field experiment started on 1 October 1931 and ended on 30 June 1933. It involved 7 health districts (Lidköping, Hälsingborg, Harad, Råneå, Jokkmokk, Pajala and Mörtfors) which received free ante- and neonatal care during this period. The goal was to choose health districts that together mirrored the situation in the country as a whole. Importantly, the districts were chosen based on variables like population density and living standards to reflect the diversity in local conditions and not due to the outcome variables we analyse. The aim of the intervention was to modernise maternal and infant care and involved funding of SEK 30,000 (USD 133,000 in current prices) in order to finance the activities to meet this purpose.

To ensure uniform standards, a 5 day long educational event including lectures and courses for the participating staff was organised in Stockholm in July 1931. One of these standards demanded that doctors and nurses had to keep records of all activities they exerted, using utilisation forms for every child and mother who enrolled. In each of the 7 districts a health center with regular office hours at 2-3 occasions per week was started and outreaching activities, including announcements in local newspapers, churches and oral announcements by midwives and nurses (Stenhoff, 1934), to inform the population about the existence of the services took place. The activities were decentralised to the district level and led by physicians in order to meet different local demands.

The main activities focused on preventive care and included guidance services and examinations at the surgeries, home visits and information campaigns. Beside of this, the maternal care provided anamnesis, antenatal examinations like temperature testing or the analysis of haemoglobin and urine samples, and gave dietary recommendations. The antenatal care furthermore included recommendations for outdoor exercise, personal hygiene and sex, and general information on maternal and infant care was provided by the National Board of Health. The visits to the health centres when the child was born implied further check-ups and services, like weighting of the child and the referral of sick children to the doctor. Mothers were encouraged to breastfeed their children and with the neonatal care they received written and illustrated details on adequate nutrition of the child at its various stages of development. When the baby

[5]The situation with worsening maternal mortality was not unique to Sweden. Rather maternal mortality did not decrease in any Western country between 1920 and the mid 1930's. The reasons for this development are still unclear, but some parts of the negative trend might be ascribed to higher rates of septic complications following higher rates of abortions.

was born nurses made home visits aimed at giving advice on hygiene, sanitation and cleanliness in the household. Furthermore, these visits aimed at ensuring that the families followed the recommendations they were given and at getting an idea of the environment and circumstances the child was exposed to.

The eligibility of the programme was determined by birth date. Children that were not older than 12 months when the intervention begun were eligible until they turned one year old, and children born during the intervention became eligible by birth. Expectant mothers were authorised to participate during the whole time of the intervention, irrespective of their stage of pregnancy.[6] 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. The project was well received in the test districts with health centres receiving on average 2.8 visits per child and about 3.9 receiving a home call.

[**Insert Figure 1 about here**]

In total about 2,000 mothers and 2,600 children enroled which represents a majority of eligible individuals. In general, the take-up in the two urban areas was lower than in the rural areas and the utilisation of the infant intervention was slightly higher than that for the maternal intervention.[7] The trial was positively evaluated and a similar scheme was rolled-out nationwide in 1937.[8] Physicians as well as auditing reports (Stenhoff, 1934) attributed improvements in maternal and infant health, respectively behavioural changes among participants to the intervention. 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. Likely inspired by the field trial in Sweden Norway (cf. Bütikofer et al., 2015) and Denmark (cf. Wüst, 2012; Hjort et al., 2014) rolled out similar programs from 1936 and 1937 onwards.

3 The Swedish Educational System

In the 1930's schooling in Sweden started in the year an individual turned seven and was compulsory for six years. Students went to the so-called *Folkskolan* and the attendance rate was almost

[6]There are only very few exceptional cases where these rules of maximum eligibility of 12 months for children were not strictly enforced, which do not threat the reliability of our results.

[7]For detailed information on the field experiment and the utilisation of the services see Bhalotra et al. (201x).

[8]Looking into historical archive data there is no evidence that the parishes and cities selected for the trial were the first early birds in the national roll out.

100%.[9] Most students ended their educational career after these years of compulsory schooling. Secondary schooling however became more and more popular and widely spread geographically in the 1940s.[10]

In 1919 a central education plan, the so called *utbildningsplanen* was introduced to overcome variations and differences in content and format of primary schools across school districts.[11] These guidelines were published by the Department of Ecclesiastical Affairs and included time-tables, syllabuses for compulsory schooling and defined the possible forms a school could have. The curriculum did not change between 1919 and 1950.

Although most students attended school full time (i.e. roughly eight months per year), some school districts in rural areas provided half time reading in order to meet the demands from the agrarian sector. Yet, in the beginning of the 1940s half time reading was only somewhat more important in the regions of *Halland* (7.6% of pupils), *Gothenburg and Bohus* (15.3%) and *Älvsborg* (24.1%). In our sample half time reading accounts for less than 0.5%. Besides the possibility of half time or full time reading, *Folkskolan* was divided into *The Main forms* and *The Exception forms*. The main forms consisted of the forms A and B. Both of these required full time reading and an appropriate teacher degree (*folkskollärareexamen*) of the teacher but while a teacher in the A-forms only supervised one grade, a teacher in the B-forms supervised several grades. The exception forms consisted of forms C and D. These were either characterised by half time reading (C and D) or by the fact that the teacher did not have an appropriate degree to teach in *Folkskolan* (only D-forms). 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). Appendix Table F5 provides an overview on the proportion of the school forms in the school year 1940/1941 in comparison to the proportions in our sample.

Teachers recorded marks and sickness absence (among other things) of all children in exam

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

[10] At the time Sweden had a tracking system where students taking on secondary schooling either left *Folkskolan* after grade 4 or after grade 6. 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. Specifically a reform implemented in 1927 improved the access to secondary education for girls as they from this year were granted access to all state-led grammar schools and could study the same curriculum as boys. Before 1927 girls could take on secondary education, but only in private schools implying a higher cost and that the social selection of girls was narrower than that of boys. For an elaborated discussion on the reforms and the expansions of secondary education, see Lindgren et al. (2014); Kyle and Herrström (1972); Stanfors (2003).

[11] The country was divided into about 2,400 school districts during this time.

catalogues. Official marks were given on a seven-point grading scale ranging from A (passed with great distinction) to C (failed). In order to ensure high reliability and comparability, several marking principles were established. For example teachers should mark the quality of knowledge and not the quantity and take notes throughout the year. This ensured that grading did not only take place retrospectively at one point in time. Thus, the marks we analyse differ from regular test scores (which can depend on the pupil's condition on the day of the test) and should reflect the general knowledge of the students quite well. Furthermore, teachers should not allow for mark inflation as pupils progress to higher grades and school form should not be adjusted for.[12] Nevertheless we control for school form as a quality indicator in our analyses. From 1943/44 onwards, the national school authorities explicitly recommended teachers to use standardised tests in Swedish and math to calibrate the final marks each year.[13] All these measures indicate that standardisation and reliability of school marks was a big issue during our period of investigation. The reliability of the marks will be confirmed in a robustness check in Section 6 where we validate our results anchoring the grading scale to household income in 1970.[14]

Beside some changed marking instructions, the Swedish educational system experienced some quantitative changes concerning compulsory school years and the length of the school year, whereas system itself remained unchanged. We are able to control for all of these changes in our analyses. For further details on these reforms see Fischer et al. (2013).

4 Data and Empirical Strategy

4.1 Matching Procedure

Since the intervention took place in seven medical districts consisting of 2 cities and 57 rural parishes, 2 control cities and 57 control parishes (belonging to 38 different health districts) were identfied using observable parish characteristics of the 1930 census. Figure 2 visualises these

[12]According to the guidelines, the main forms should be marked according to the same principles. For C schools a different (lower) marking standard was allowed, but since these were so rare it is hardly an issue.

[13]Sweden was neutral during the Second World War and historical sources do not indicate educational disruptions. 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. However, schools were allowed to have shorter breaks in case of limited energy supply, and schools could cancel regular schooling in case of a threat which 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, 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.

[14]So far it can be stated that marks seem to be informative, although the most recent ones are probably more reliable than the older ones which were given before the recommendations took place. Since we observe marks between 1937-1947, this statement holds true for our sample as well.

areas on the municipality level. The best matches in terms of observable characteristics (denoted $\mathcal{J}_M(i)$) were identified using the Mahalanobis distance metric, defined as

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

where X_i is a vector of observable characteristics for a parish belonging to a test district, in our case, average income; net wealth; employment shares in manufacturing and agriculture; population density; proportion of fertile married women; and a dummy variable for urban locations, and 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. In fact, our matching procedure is based on similar information that was available to the decision makers at the time of the intervention. The matching was done in random order and without replacement. The upper panel of Table 1 shows 1930 census summary statistics and the standardised difference (Imbens and Woolridge, 2009) between treated and (matched) control districts. 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.[15]

[Insert Figure 2 about here]

[Insert Table 1 about here]

4.2 Data Sources

The dataset is unique and was developed by merging five different individual level datasets; two of them were purpose-built for analysing the effects of early life health interventions. The first individual dataset contains *data from all births* in the identified treated and control regions for the cohorts of 1930-1934. It stems from high-quality administrative church records on all newborns. Sweden is one of few countries with high-quality vital statistics at the parish level covering the whole population from the 18$^{\text{th}}$ century onwards (Pettersson-Lidbom, 2015). In our treatment and control regions there were in total 24,710 deliveries (25,029 individual children) during this time which resulted in 24,390 births. For all these individuals we observe various background characteristics, e.g. children's sex, marital status of the mother and mother's age. We also observe paternal occupational status which was translated into occupational classes

[15] For further information on the identification of the control group and the underlying matching procedure see Bhalotra et al. (201x).

based on the HISCO classification (Leeuwen et al., 2002), allowing us to control for socio-economic status. To the main data we add information from several other data sources via matching procedures which were carefully executed and validated (cf. Bhalotra et al., 201x). The dataset thus also includes information on death dates from merging with the Swedish Death Index (cf. Fischer et al., 2013).

The second individual-level dataset contains information on *school performance and sickness absence* for individuals from the treated and control regions for the school years 1937–1947. Standardised exam catalogues containing individual information for each pupil in each school year were gathered from historical archives and then digitised. Figure 3 shows an example of an exam catalogue which consists of several pages. We mainly observe our cohorts in primary school in grade 1 and grade 4. The reason for not collecting data on later grades is that children continuing to secondary schooling could chose to do so after grade 4 (or after grade 6). For about half of our sample we have details from both grades, grade 1 and grade 4. The other half of the sample is either observed in grade 1 or in grade 4. Due to the possibility of grade retention there are a few cases where we observe pupils more than once per grade.

The school dataset includes detailed information on academic performance for three cognitive subjects: 'math', 'writing' and 'reading and speaking', and for one non-cognitive subject: 'Christianity'. Given that marks in 'Christianity' are more likely to be achieved due to inheritance of religious skills from the parental home rather than effort we refer to 'Christianity' as placebo estimate since marks in that subject should not be affected by the infant intervention. In addition to information on academic performance the school dataset contains information on sickness absence in days, total absence in days, the length of the school year, school type or the name of the teacher and the name of the school.[16]

[Insert Figure 3 about here]

In order to analyse the effects of the infant health intervention on academic performance, the individuals in the birth records were matched to the school data via an algorithm based on birth parish, date of birth, forenames and surnames. Out of 22,500 individuals still alive until age 7 from the 1930-1934 cohort, roughly 16,000 were matched to our school and sickness absence data. Unmatched individuals are likely to be missing at random. For some schools the archives from which we collected the information do not have the relevant information. This incompleteness

[16]Teachers reported the type of absence in the Exam catalogues. Sickness absence accounts for about 80% of total absence. Other reasons for absence could be e.g. inappropriate clothing or weather conditions preventing children from going to school.

of archives is unproblematic if it is random which we assume to be the case. Selection, due to migration to another parish after birth, should be unproblematic since the matching algorithm already takes this into account for movement between treatment and control regions. We also collect information on movers to other parishes so that several people moving to other places than treatment and control parishes get matched as well.[17]

For the estimation of long-term effects we merge individuals in the birth records to data from the *1970 population and housing census*. It covers the whole population of Sweden on 1st November in 1970 (Population and Housing Census 1970, 1972a). It contains a number of demographic variables but also socio-economic and labour market variables like education, income and working status which we use as main outcome variables. In total we observe 20,922 individuals from the birth record dataset in 1970. 3,243 of the remaining 4,142 unmatched individuals died before the data collection of the 1970 Census.[18] Appendix Table F6 and Table 2 present descriptive statistics on our explanatory and outcome variables for each dataset.[19]

[Insert Table 2 about here]

Our fourth data source is the *1950 population census* containing information on occupation during the individuals' early labour market career which allows us to construct background variables like participation in apprenticeship, working 1950 and being a student 1950. We can match 20,326 individuals to the 1950 census. Out of the remaining ones, we have a date of death dating before 1950 for 2,794 individuals.[20]

The final dataset adds 2001–2005 pension (labour) income from *official tax registers*. These contain information on 16,194 individuals from our birth record dataset. With 7,290 individuals died before age 71 we are left with about 1,580 individuals which we cannot match to the tax registers. Thanks to some special features of the previous Swedish pension system, which is currently being phased out, this variable is very insensitive to career interruptions due to child-bearing etc. For the cohorts considered here, full pensions require thirty years of contributions

[17] 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). This is not much but it could still account for a small part of the unmatched individuals.

[18] We are consequently left with about 900 individuals which cannot be matched. We assume that they most likely emigrated abroad although they could also be dead.

[19] The earnings information in the 1970 census is confirmed to be of high quality, but females 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 seriously bias our results, we impute incomes of these females based on their qualifications and hours worked.

[20] The individuals that we cannot match likely emigrated abroad although they could also be dead. Furthermore, about 200,000 individuals of the Swedish population are not covered in the 1950 population census which likely explains the discrepancy between the 1950 and 1970 census match rates.

and the level of the pension is based on the best fifteen years (Sundén, 2006). We thus consider this variable an excellent complement to the 1970 earnings variable, and it is likely to represent earnings at advanced stages of the career quite well.

Table 3 shows descriptive statistics and p-values for the differences in means for the main outcome variables in our treatment and control regions before, during and after the intervention. Differences in means are insignificant concerning our cognitive GPA, 'reading and speaking', 'writing', the propensity to work fulltime or parttime, income, working in the governmental or municipal (public) sector and most 1950 census outcomes, while treatment and control groups seem to differ in sickness absence, 'math', secondary schooling and being a student in 1950 before the intervention took place. These differences should however not be problematic since we control for parish specific differences and parish specific time trends. In Section 6, we also conduct tests for pre-trends.

[Insert Table 3 about here]

We follow our exposed cohorts over five different points in time. The treated cohorts born 1931–1933 are mainly observed in first grade between 1938–1940 when they are 7 years old, and in fourth grade between 1941–1943 when they are 10 years old. We then track them until 1950 when they make their first labour market decisions and are between 17–19 years old, and until 1970 when they are age 37–39 and already spent some time in the labour market. We observe them for the last time in 2002–2004 when they are 71 years old.

4.3 Empirical Strategy

We estimate impacts of the infant intervention on sickness absence and academic performance in primary school, early educational and occupational choices, and eventual employment, occupation and income in adulthood. We use a difference-in-differences type strategy to compare changes for exposed cohorts in the treated regions to unexposed cohorts and births in control regions. 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 bias while on the other hand it will slightly reduce the efficiency of our estimates.

Since marks were officially given on a 7-point grading scale in the 1930s, we translate the scale into a range from 1 for the poorest mark (C) to 7 for the best mark (A). We create a measure of children's cognitive abilities by taking the mean of school performance in 'math', 'reading and speaking' and 'writing' to form a grade-point average (GPA), but also examine subjects one by one. In order to ease the interpretation of the coefficients we rescale these variables to have a standard normal distribution.[21] The effects are separately estimated for grade 1 and grade 4 and we also conduct the analysis separately for males and females. Figures 4 and 5 show the fraction of the grade point average by gender and grade. Females got better marks than males, and marks in grade 4 had a higher variation and improved in comparison to grade 1.

[Insert Figure 4 about here]

[Insert Figure 5 about here]

Since marks are given on an ordinal scale and thus only allow to define ranks instead of a cardinal interpretation, the choice of the scale can have significant implications for our findings (cf. Bond and Lang, 2013; Lang, 2010). To address this issue we anchor the 7-point grading scale to the logarithm of income in adulthood (as proposed e.g. by Cunha and Heckman, 2008) in a robustness check in Section 6. Applying this method gives us an interpretable metric and a valid scale based on income in 1970.

We gauge impacts of the infant intervention by estimating

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

where y_{ipt} is the outcome of child i born in parish p on day t, T_t is the duration of eligibility for the infant intervention for a child i born on day t in years, D_p is a dummy equal to one for treated parishes, γ_p are parish fixed effects, σ_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, marital status of the mother, a twin indicator, dummies capturing older (>35 years) and younger (<25) mothers and the household heads occupational status. Furthermore, we also control for the effects of the maternal intervention since some individuals have been eligible for both interventions. In order to allow for a different trend between treatment and control regions, we add parish specific time-trends in our last specification which is more general than treatment-group-specific trends.[22] The outstanding variety of information in our school dataset additionally enables us to include school fixed effects and to control for the length of the school

[21]We transform the variable into a z-score by taking its inverse normal distribution.

[22]We also checked for robustness using health district fixed effects and health district specific trends. Results are quite similar to parish fixed effects and parish specific trends.

year and the school form (an indicator of school quality) while exploring the effects on sickness absence and academic attainment in primary school.

Assuming a common time trend in the absence of the treatment for treatment and control regions, the parameter δ_I measures the intent-to-treat (ITT) effect of the infant intervention. It gives us the effect for each additional year of eligibility of making the service available. This effect is especially of importance 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) this ITT is a scaled version of the average treatment effect on the treated (ATT). In Appendix Table E4 we present results using alternative definitions of the treatment indicator. Instead of a continuous indicator which takes duration of eligibility into account, we try different binary variables to identify age and duration of exposure. Estimates are generally robust to the different definitions with at least 0-3 months of exposure being the most effective treatment period.

5 Results

5.1 Academic Performance

First we present estimates for the GPA across subjects and then we provide subject-specific estimates. The three columns of Table 4 display the difference-in-differences estimation results for all grade×gender combinations using the standardised grading scale. Each specification controls a set of covariates (see Table F6). While there do not seem to be any programme impacts in first grade, there is an increase in the GPA in fourth grade of about 0.08 standard deviations on average. The average effect is driven by males who exhibit an increase in marks of about 0.1 standard deviations. These results are robust to the inclusion of parish specific time trends and significant at the 5% level.

In addition to the mean effects we estimate unconditional quantile treatment effects as suggested by Firpo et al. (2009). Figure 6 shows quantile treatment effects by gender. While males experienced a positive effect across most of the distribution, it was only the upper 30% of the female distribution that benefited from the intervention.

[Insert Table 4 about here]

[Insert Figure 6 about here]

We also explore the subjects separately; once again for each grade×gender combination. Table 5 shows significant improvements in 'writing' and 'reading and speaking' in grade four, whereas there is no significant effect on academic performance in 'math', the placebo subject 'Christianity' or in any subject in grade one. The effects in fourth grade are about 0.11 and 0.12 standard deviations on average. The average gains are larger for boys at 0.13–0.18 standard deviations, relative to 0.08–0.1 standard deviations for girls. Although estimates for boys are more precise, they are not significantly different from the coefficients for girls. Appendix D shows that children born out of wedlock benefited substantially more than other children. However, there are no differences by socioeconomic status of the parents, indicated by their occupation.

To put the average gain in cognitive performance of 0.11 standard deviations overall (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, but for a selective sample of children at the low birth weight margin. Using twin fixed effects Bharadwaj et al. (201x) 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 Figlio et al. (2014) estimate that, on average, the heavier twin scores about 5 percent of a s.d. more than the lighter twin in Florida respectively. Our estimates, being from exposure to a universally available programme, look 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).

[Insert Table 5 about here]

5.2 Sickness Absence

Sickness absence rates are similar for boys and girls at about 5% of school days.[23] Table 6 shows that the infant intervention had small and mainly insignificant effects on sickness absence. A year's exposure to the intervention reduced boys' sickness absence in fourth grade by about 0.8% which is about 20% in comparison to the baseline of 4%, but we see an increase in sickness absence for girls in fourth grade, for which we have no ready explanation.

[Insert Table 6 about here]

[23]The distribution and average days of sickness absence for the sample corresponds well to the figures reported in contemporary research on the consequences of sickness absence in compulsory education, see e.g. Aucejo and Romano (2014) and Goodman (2014).

5.3 Long-Term Outcomes

5.3.1 Education and Early Career

Table 7 shows estimates of the impact of eligibility for the infant health intervention on education and early career labour market outcomes. About 19% of the individuals born between 1930 and 1934 continued into secondary education after primary school, and although the pre-intervention mean is somewhat larger for girls, it is not significantly different from that for boys. The results show that an additional year of exposure 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. This difference may be explained by the fact that at the upper end of the test score distribution, improvements in performance were larger for girls than for boys, with baseline performance being stronger among girls. Figure 7 is illustrative, showing the mapping between primary school test scores and secondary school completion.

[Insert Figure 7 about here]

Using the 1950 census we generate outcome variables that allow us to capture the early career choices of our sample cohorts, who were 16-20 when enumerated in the census. They were making or had made decisions on going straight onto the labour market, continuing in education, or gaining vocational training through an apprenticeship, which involved additional education for three to four years (Nilsson, 2013). At this time, the private sector offered good career opportunities so, at the margin, the treated individual may have preferred to gain a competitive edge through harnessing greater skills. Given gender segregation in the labour market at this time, we distinguish apprenticeships, that led to craftsmanship or skilled manufacturing work, and that attracted men from vocational training that was tuned to women's jobs as for instance midwives, nurses and pre-school teachers. We find no increase in employment at this age for either sex, consistent with programme exposure encouraging skill acquisition. We find positive coefficients on university enrolment and apprenticeship that are not significantly different between men and women but that are larger and more robust for men. Treated males are 1.5 percentage points more likely to study 1950 (59% increase in comparison to the baseline) and about 3.2 percentage points more likely to be in an apprenticeship (100% increase in comparison to the baseline).[24] We do not see any improvements in vocational training for female-specific

[24]We checked whether our cohorts are late starters but 1950 is exactly the time where they are supposed to be in an apprenticeship. Usually they are between 15 and 20 years old.

jobs.[25]

[Insert Table 7 about here]

5.3.2 Labour Income

We find very substantial impacts of the infant health intervention on labour income in 1970. Table 8 shows that eligibility for a year enhanced income by 7.3% on average. This result is completely driven by women who experienced an increase in income of about 19.5%, in contrast to no gain among men. These results are robust to the inclusion of parish specific time trends. The estimate for women are large but given that they are also 20.5% more likely to be working fulltime, they are plausible.

Since 1970 income depends a lot on fluctuations in the labour market we also look at pension income at age 71. Although slightly smaller, estimates suggest an increase in income of about 7% for women. For males we now see a negative significant effect of 4%. A possible explanation of this is endogenous survival selection since we estimate that 36.5% of our sample cohorts do not survive to age 75, and the marginal surviving individual is negatively selected post-intervention Bhalotra et al. (201x). To investigate this, we re-estimated programme effects on income in 1970 for different survival age samples. In Table 9 we display results for subsamples of individuals surviving until age 40, 50, 60, 70 and 75. We see no selection among females until age 75 when there appears to be some positive selection. However, among men we see negative selection from age 60 onwards and estimates progressively become lower than the main result. This is in line with our expectation of pension income for males being an under-estimate because of negative selection issues.

[Insert Table 8 about here]

[Insert Table 9 about here]

5.3.3 Employment

To estimate the effects on employment status we create dummy variables indicating whether an individual was working part-time (≥20 hours per week <35) or full-time (≥35 hours per week)

[25] This undermines a potential mechanism driving the longer-term labour market outcomes discussed next, which is that the nurses and midwives working in the intervention became role models and inspired participating parents to promote a similar career for their daughters.

in 1970.[26] We find an increase in the propensity to work fulltime of 7.6 percentage points for women exposed to the intervention for a year. In comparison to the baseline of 37% fulltime work among women, this implies an increase of 20.5%. There are no impacts for men, 92.5% of whom worked fulltime and no impacts on part-time work for men or women.

5.3.4 Occupation

At the time when women from our sample cohorts were joining the labour market, there was increasing participation among married women. Women with children often prefer public sector jobs where conditions are typically more woman-friendly. In order to explore this, we created indicators of employment in lower level municipal and higher level (central) government employment (Table 10). We see an increase in the probability that women work in municipal public sector jobs of 4.9 percentage points which is a large increase of 20.5% relative to the baseline of about 24%. We also see an increase in the likelihood of working in central governmental of 3.4 percentage points or 66.5% of the baseline mean.

Since programme exposure is associated with greater skilling (secondary school completion) among women (but not men), we examined occupational sorting with a view to identifying whether programme-led increases in women's employment were concentrated in high-skilled sectors, and this is what we find (see Table 11). Women who were exposed to the intervention (for a year) are 4.6 percentage points (27.2% in comparison to the baseline) more likely to work in the scientific, medical and technological branch and 4.3 percentage points (34.9% referring to the baseline) more likely to work in the accounting, banking and administrative branch. In contrast, there is only a small positive effect for men in the sales branch. Disaggregating the occupational categories that attracted women further, we found that it was women working in medical jobs as e.g. midwifes or nurses that drives the first result. Since we found no increase in acquisition of these qualifications in the 1950 census, it appears that nurses and midwives who may have dropped out of the labour force after having children increased their labour force participation, although this may have been supplemented by programme-led increases in general (secondary and university) education among women. Importantly, we see a reduction in the *out of the labour force* group for women (and not men).

[Insert Table 10 about here]

[26] We are aware that employment status in the 1970 population and household Census is exposed to measurement error but since parttime and fulltime work are underestimated (cf. Population and Housing Census 1970, 1972b) our results likely estimate lower bounds.

[Insert Table 11 about here]

5.4 Discussion

In this section we discuss the differences in programme effects identified for men vs women. First, we exclude the possibility of gender differences due to higher uptake of utilisation for females by studying physician records on individual utilisation (see Appendix C). We shall argue that the long-term effects, as observed in the 1970 census and pension earnings, are driven by a combination of intervention-led effects on school performance, educational choices, and the expansion of the welfare state which raised the relative demand for women's labour, alongside general trends in the labour market that may have cramped returns for men in our sample cohorts.

We found that the cognitive gains in primary school from the intervention are distributed differently for males and females: males significantly improve their scores between percentiles 50–80, whereas females experience a significant improvement near the very top of the distribution (percentiles 70–90). This is consistent with investments, possibly by parents or teachers, reinforcing the intervention in accordance with the expected returns to these investments. In Sweden in the 1930s, the returns to years of schooling were in general larger for women than for men (Bång, 2001). According to the National Census of 1930 there was a limited supply of well-educated women as only six per cent of all women above age 16 had secondary education or more. As described above, girls did not have access to state-led secondary schools until 1927 (in turn implying a higher cost and a narrower social selection into secondary schooling for girls than boys) which was the starting point for a series of reforms which came to increase general access to secondary education in the following decades. Returns to education declined over the following decades but gender differences are still visible in 1970. In Table 12 we regress income on marks in grade four and on the secondary schooling variable. Columns 2 and 3 suggest that returns to school grades were slightly larger for females than for males, but more importantly the estimated secondary schooling premium is significantly larger for females than for males, even after controlling for primary school performance (see also Björklund and Kjellström, 1994). It is thus striking that female improvements in primary school performance were concentrated exactly in the parts of the distribution that were most likely to proceed to secondary school (cf. Figures 6 and 7).

[Insert Table 12 about here]

The gender bias in returns to education may also explain why treated males increased their likelihood of becoming apprentices instead of continuing on the academic track. Moreover, the labour market gave males a comparative advantage in physical activities (*brawn* rather than *brain*).[27]

However, intervention effects on female earnings in 1970 are one order of magnitude larger than could reasonably be attributed to the increase in secondary schooling. Besides, we see that males enjoyed no long-term benefits from the intervention, despite having acquired more vocational qualifications. We argue that both outcomes may be explained with reference to general trends on the labour market, which tended to augment the female advantage and diminish the male advantage.

We propose that the expansion of the Swedish welfare state and the public sector (mainly employing women) starting in the 1960s (Sundin and Willner, 2007) is a potential mediating mechanism for our long-term findings. Figure 8 shows that female employment rapidly increased from about 800,000 employed women in 1950 to about 1,200,000 in 1970 while male employment stayed rather constant over time. While until the 1950s mainly single women participated in the labour market, also married women were integrated in the Swedish labour force in the mid-1960s (Stanfors, 2003) when our cohorts were about 30 years old. Alongside the development of other parts of the welfare state, there were specifically large investments in publicly provided child care for pre-school children (Datta Gupta et al., 2006). By the expansion of child care a lot of female labour was released (Bergh, 2009). Women could join the labour force due to relief in child raising, but the expansion also generated new workplaces for women – see e.g. Figure 9 illustrating the increase in pre-school teachers among females 1960-1975. Public sector employment thus offered possibilities to combine career and family responsibilities (Datta Gupta et al., 2006).[28] Overall, it seems likely that the expansion of the Swedish welfare state pulled women into the labour force from 1960 and onward. Although education only explains about 10% of the income effect it seems plausible that the better educated females, being more competitive, were the ones that benefited from the expanded demand for women. In comparison to singles or low educated women they were not obliged or "pushed" to work in order to gain their living, but were probably rather career oriented and attracted by attractive "women friendly"

[27] Saaritsa and Kaihovaara (2016) explore schooling decisions of females and males in Finland in the early 20$^{\text{th}}$ century and find quite similar developments. 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.

[28] As discussed by Kyle (1979) and Berntsson (2002) the expansion of child care institutions was largely steered by labour market interests in the female labour force, not by the needs of women and children.

working conditions in the public sector (Stanfors, 2003). This is in line with our finding for employment in the medical branch which demands highly educated labour and Figure 9 showing the development of some typical public sector jobs mainly executed by women. Thus, better educated women could take better advantage of the expansion of the Swedish welfare state.[29]

[Insert Figure 8 about here]

[Insert Figure 9 about here]

The rapid expansion of the welfare state also provides an explanation for why we only find long-run effects for women in the Swedish context, while Bütikofer et al. (2015) find that access to well-child visits significantly impacts later life labour outcomes for both men and women in Norway (and even more so for men). The economic transformation from agriculture to industry and from rural to urban was faster and more fundamental in Sweden compared to Norway and Denmark, leading to a faster development and implementation of the welfare state. While both countries experienced a women-friendly development regarding working conditions over the years, several features of the welfare state advanced later in Norway than in Sweden. For e.g. publicly provided child care was established with substantial increase in subsidies already in the mid 1960s in Sweden and income during maternity leave was compensated for a longer period of time (6 months in comparison to 12 weeks in Norway). Similarly, the growth of married women's labour participation took place about ten years earlier in Sweden than in Norway (Eeg-Henriksen, 2008).[30] Maternity leave, which was introduced in Sweden in 1939 (Eeg-Henriksen, 2008) and granted employment security for mothers, and income compensation during maternity leave brought about that women who had secondary education were not forced to choose between employment and family formation to the same extent as before. Thus, they were able to leverage their investments into labour market human capital to a larger extent even if having a child.

Labour market trends worked out quite differently for males who had selected into an apprenticeship due to the intervention. As evident from Figure 10 there was no comparable surge in the demand for craftsmen. On the other hand, the first two postwar decades were characterised by an almost chronic shortage of manufacturing workers. These shortages did, however,

[29]Similarly, in the case of India Klasen and Pieters (2012) show that women with lower education tend to be pushed into work while women with higher levels of education are pulled into the labour force due to attractive working conditions and economic opportunities.

[30]In 1970 about 40% of Swedish married women were working compared to only 25% in Norway (Eeg-Henriksen, 2008).

not translate into an increased skill premium among blue-collar workers. Instead, there were several forces working in the opposite direction. Official labour market policies had the explicit aim of reducing wage differentials. In addition, skilled workers born in the 1930s were facing increasing competition from other groups. It is likely that these two processes seriously impacted the returns to their qualifications. Alongside this development technological change, bringing about more industrialised and mechanised production methods, shifted demand from higher skilled people holding an apprenticeship towards less specialised skills.

[**Insert Figure 10 about here**]

Starting with public policies, the cohorts covered in our study had their early careers in an era characterised by the implementation of the so-called Rehn-Meidner model. First presented in 1951, this model for fiscal, monetary and labour market policies was designed to reach the four goals of low inflation; full employment; high growth; and low income inequality in a small open economy (cf. Karlsson and Karlsson, 2002, for a general introduction to the model and its implementation). The model, which was only partly implemented in the end, entailed a number of policy measures – the most important one from our point of view being the 'solidarity wage policy'. According to this wage policy, all firms should provide equal pay for equal work, irrespective of the profitability of the company. This policy was implemented, and led to a rapid compression of blue collar workers' wages, especially in the private sector (Alexopoulos and Cohen, 2003). Between 1962 and 1970, the dispersion of blue-collar wages was reduced by half. Similar reductions were notable among white-collar workers and in other segments of the labour market, but they were generally smaller and happened somewhat later (Hibbs and Locking, 2000; Hibbs, 1990; Persson, 1990).

This implementation of the Rehn-Meidner model coincided with two other trends which might have hurt skilled male blue-collar workers in particular. First, there was a large influx of migrants from other European countries. This change was also the result of public policies: the Nordic countries introduced a common labour market in 1954, waiving the requirement of residence and work permits for migrants from these countries. At the same time, the Swedish labour market was made accessible for migrants from other European countries (Hammarstedt and Palme, 2012). These measures led to more than 200,000 migrants from Nordic countries, and 100,000 migrants from other European countries, entering the country between 1950 and 1970

(Nilsson, 2004).[31] These were disproportionately working in larger manufacturing companies. Some of them came as individual job-seekers, but a large share were actively recruited either by government agencies or by larger companies. These efforts and the surrounding regulations generally targeted skilled workers (Lundqvist, 2004).

Second, skilled workers born in the 1930s faced increasing competition from another source: starting with the cohorts born in the late 1930s, an ever larger share of the labour force had their formal education within a new school system that was introduced as a roll-out starting in the 1949/50 academic year. This new school system was comprehensive with nine years of compulsory schooling (Meghir and Palme, 2005). Thus, younger cohorts entering the labour market would have significantly more general education than their older peers (Nilsson, 2013). In addition, the school reform changed secondary schooling. Vocational training now became a part of the formal school system, and a wide range of vocational two-year secondary schooling programmes were introduced, to which students could proceed after having finished the 9-year comprehensive schooling (Holmlund, 2008b). This upgrade of vocational training is likely to also have negatively affected the demand for skilled workers from older cohorts.

It is beyond the scope of this paper to assess the relative importance of these various influences on male skill premiums. However, the hypothesis that returns to apprenticeship diminished over time is supported in our data. Table 13 shows the correlation of 1970 earnings with different types of qualifications – with a complete absence of qualifications being the reference category. There is no evidence of a skill premium for former apprentices.

[Insert Table 13 about here]

6 Robustness Checks

6.1 Anchoring of Grading Scale

Our positive findings on academic achievement could in fact be sensitive to the choice of the applied grading scale (Bond and Lang, 2013). In order to validate our results on academic performance and to confirm the reliability of the 7-point grading scale, we anchor the scale to log income in 1970. Although school marks are given on an ordinal scale, we will show that they are interpretable by anchoring them to the cardinal scale of income in adulthood, as proposed e.g. by Cunha and Heckman (2008).

[31] These numbers underestimate the magnitudes somewhat since many workers, especially those from other Nordic countries, would leave the country again after a couple of years.

We separately regress income Y in 1970 on each mark N of our 7-point grading scale for each subject s in each grade g:

$$\ln Y_{sig} = \alpha + \beta^N \text{Mark}^N_{sig} + \varepsilon$$

to obtain the mean log income for each of the seven steps on our grading scale. Each individual i with the same mark in the same subject therefore has the same anchored mean log income which will be used as the new outcome variable. Table 14 lists the results for the subjects of 'math', 'reading and speaking' and 'writing' in grade 1 respectively grade 4 with mark 3 as reference group. Since log income increases with better marks in grade 1 and grade 4 and decreases with worse marks, the grading scale can perfectly be interpreted and seems to be quite informative and reliable. The correlations in fourth grade imply that if marks rise from 1 to 6 respectively 7 points, future earnings would rise between 95% and 126%.

Next we use the underlying anchored scales to validate our results for academic performance. Appendix Table F8 shows anchored regression results regarding the separate subjects. Since parish specific time trends demand a lot from our data and because results are relatively robust to their inclusion, all of the estimates for the subjects refer to a specification which includes all controls except for parish specific time trends. Results go hand in hand with the standardised grading scale. Marks in 'reading and speaking' and 'writing' in grade four improved by about 0.27-0.29% in comparison to the baseline but there are still no significant effects on 'math' or 'Christianity' scores. Anchoring with years of education instead of log income generates quite similar results and validates the findings.

[Insert Table 14 about here]

6.2 Common Trend Assumption

To investigate whether the treatment and control regions follow a similar trend, we test for pre-trends, looking at our outcome variables before the intervention took place. We run regressions for the pre-intervention period based on 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 our pre-intervention sample and $treated$ refers to a dummy indicating treated parishes. If β is different from 0, the treated and control regions had a significantly different pre-trend. Since outcomes are gender-specific we conduct tests separately for males and females. Table 15 shows pre-trend tests for primary school outcomes. It indicates that for most of our variables there does not seem to be a significantly different pre-trend. Only for males' sickness absence in grade 1 treatment and control regions might have followed a different trend. Table 16 shows pre-trend tests for the long-term outcomes. Neither for males nor for females we see significantly different pre-trends.

[**Insert Table 15 about here**]

[**Insert Table 16 about here**]

In addition, results are relatively robust to the inclusion of parish specific time trends. These findings make it quite likely that treatment and control regions did not experience a significantly different pre-trend.

6.3 Mother Fixed Effects

Since there could be unobserved time-invariant mother level traits which could bias our results we estimate school and long-run labour market outcomes including mother fixed effects. We lose a lot of observations performing this excercise as we can only include mothers with at least two births within the five year observation window. Moreover we need to observe the siblings at school for fixed effects on academic performance and in the long-run for later life outcomes. Since some individuals die before 1970 and we have a subsample of school grades the number of observations decreases. Tables 17 and 18 present mother fixed effects regression results on academic performance and labour market outcomes. Overall, estimates for this special subsample of individuals are bigger than our main estimates, consistent with reinforcing parental investments (cf. Almond and Mazumder, 2013). It seems likely that parents have invested more in the treated child and thus reinforced the positive intervention effect.

[**Insert Table 17 about here**]

[**Insert Table 18 about here**]

6.4 Placebos

For the 1970 Census outcomes we perform some further robustness checks using placebo estimates. First, we generated a sample of children born in treatment and control areas ten years after the infant intervention took place using information in the 1950 population census and the Swedish Death Index. The information for these cohorts born between 1940 and 1944 is much more limited than the original sample and the only covariate which is available is 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 placebo intervention took place exactly ten years after the original intervention.[32] Reassuringly Table 19 shows that the estimates for these placebo regressions are small and insignificant.

[**Insert Table 19 about here**]

We also conduct a randomisation inference test for the long-term outcomes in order to derive placebo treatment effects. We randomly assign treatment status within each treatment and control parish pair (cf. Karlsson and Pichler, 2015 for a discussion of randomisation inference in difference-in-difference settings) using 5,000 permutations. Figures 11 and 12 plot these distributions of placebo treatment effects by gender and include the actual treatment effect and the corresponding p-value. Except for part-time employment for females where the distribution does not look smooth, results are quite similar to our main estimates in Tables 7, 8 and 10.

[**Insert Figure 11 about here**]

[**Insert Figure 12 about here**]

7 Conclusion

Our analysis indicates that an improvement in early life health conditions through a universally provided low-cost and scale-able infant health intervention led to gains in academic performance in primary school and improved education and labour market outcomes, although the long term gains are restricted to women. We argue that the expansion of the Swedish welfare state starting in the 1960s "pulled" women into the labour force which explains the effects on our labour market long-term outcomes. Females who were exposed to the intervention benefited more from the favourable economic conditions during this time than non-eligible females. Males on the other hand left school earlier and backed the wrong horse when deciding to take an apprenticeship

[32]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, 2008a). 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 our 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.

because of a disadvantaged development in the private sector and industries. Intention to treat effects, at each of the five stages of the lifecourse, are large.

The findings on academic performance, on long-run outcomes and the results of Bhalotra et al. (201x) on mortality, indicate that the Swedish infant intervention of 1931-1933 seems to have had a large return on the investment. Estimates in Grantham-McGregor et al. (2007) suggest that poverty, poor health and nutrition, and deficient care will cause about 200 million children under the age of 5 not to reach their full potential in cognitive development. This disadvantage can in turn give rise to poor levels of cognition and education later in life and even to the intergenerational transmission of poverty due to reduced wages and income (Grantham-McGregor et al., 2007). Improving basic infant care could therefore result in better academic achievement and health in todays low income countries, which in turn could lead to reduced inequalities through improved labour market and socio-economic conditions.

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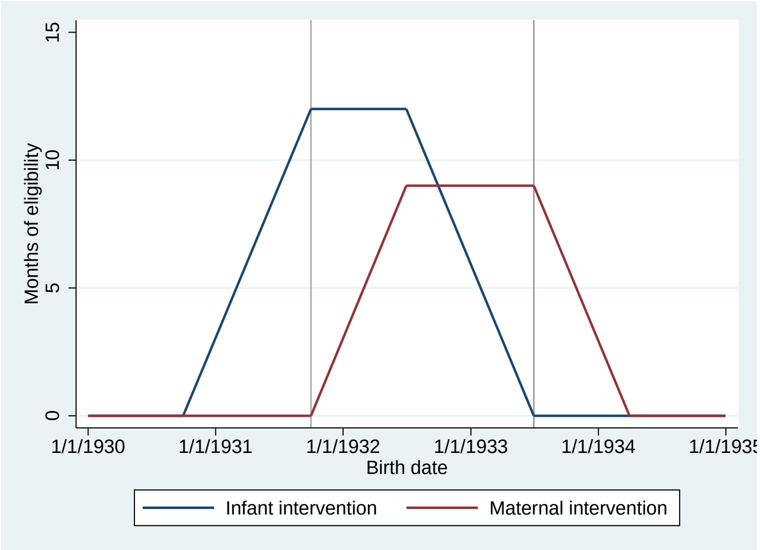


Figure 1. Duration of eligibility by birth date for maternal and infant care.

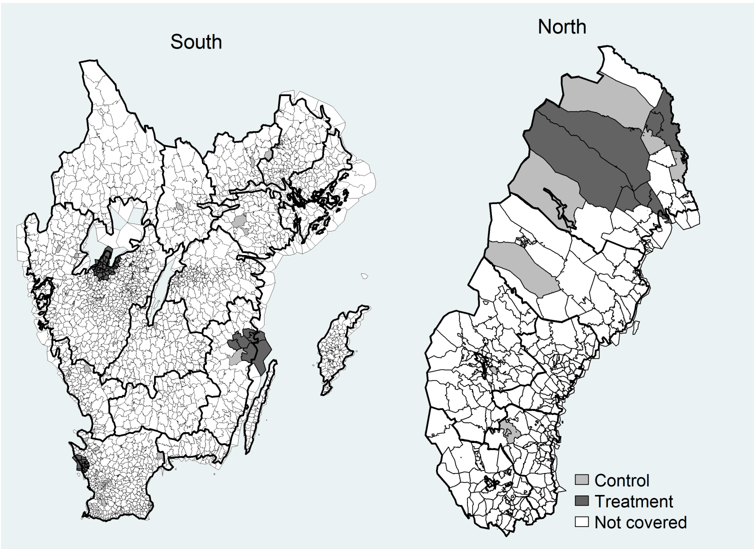
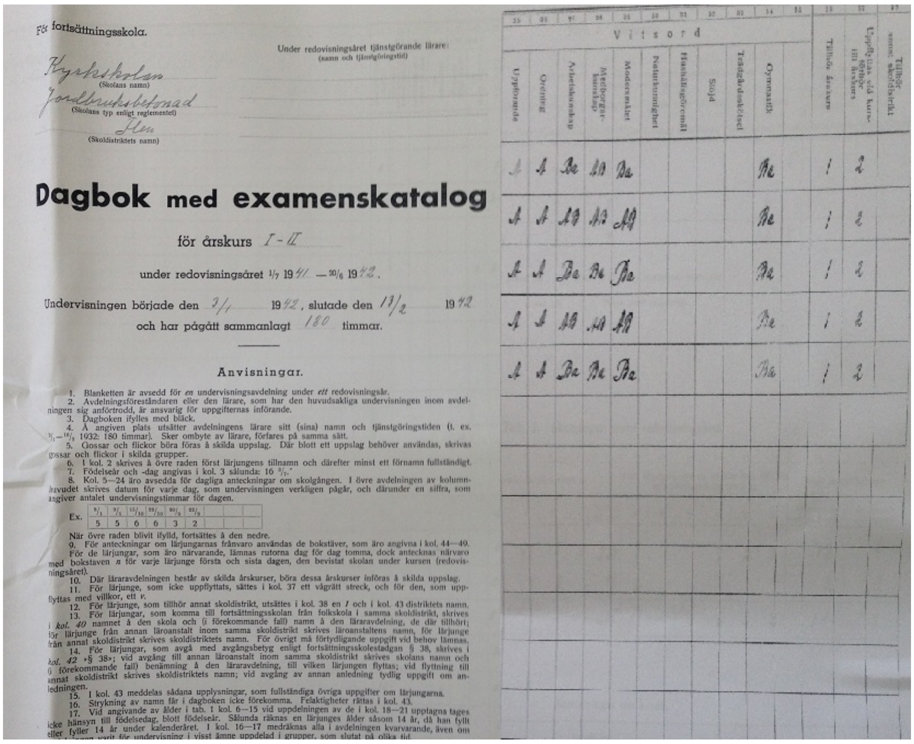


Figure 2. Municipalities containing treated and control districts.


För fortsättningsskola.

Under redovisningsåret tjänstgörande lärare:
(namn och tjänstgöringstid)

(Skolans namn)

(Skolans typ enligt reglementet)

(Skoldistriktets namn)

Dagbok med examenskatalog

för årskurs

under redovisningsåret 1/7 19 — 30/6 19

Undervisningen började den 19 , slutade den 19

och har pågått sammanlagt timmar.

Anvisningar.

Figure 3. Exam catalogue in *Folkskola*.

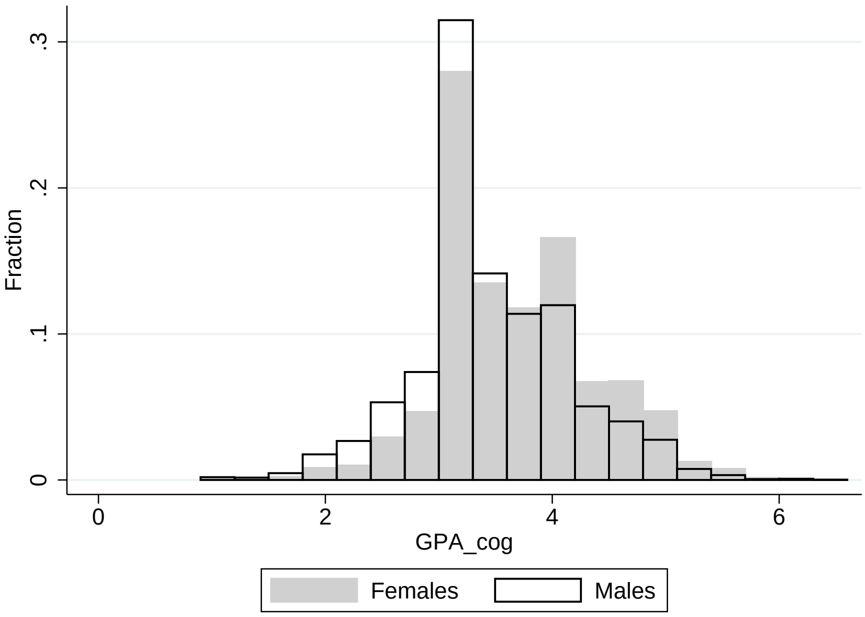


Figure 4. Fraction of GPA by gender.

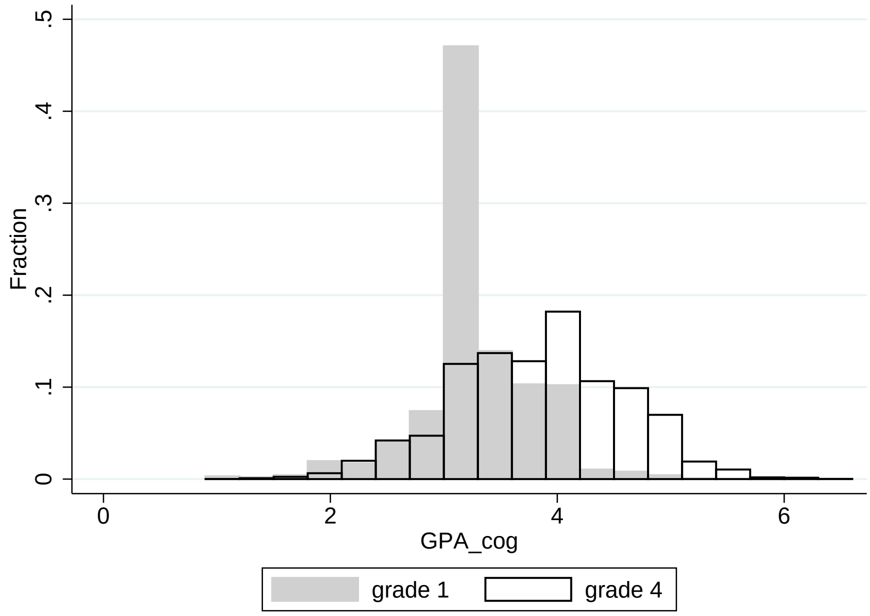
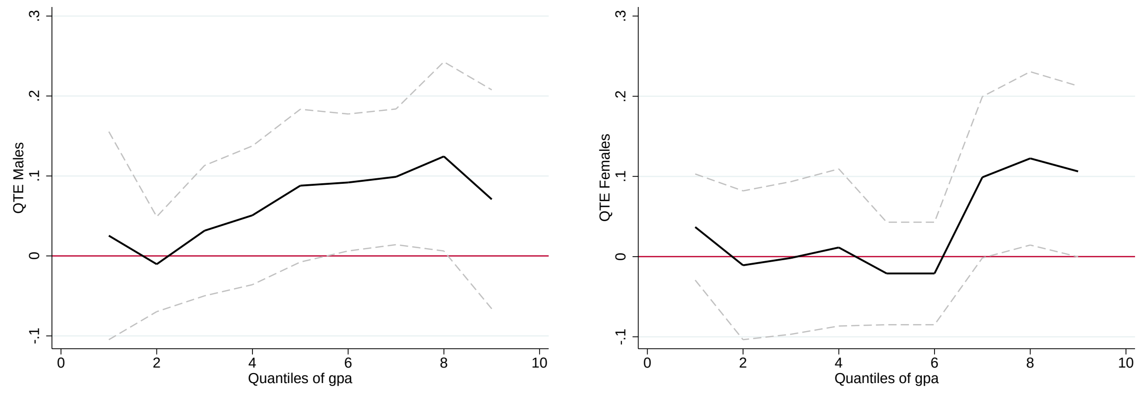


Figure 5. Fraction of GPA by grade.

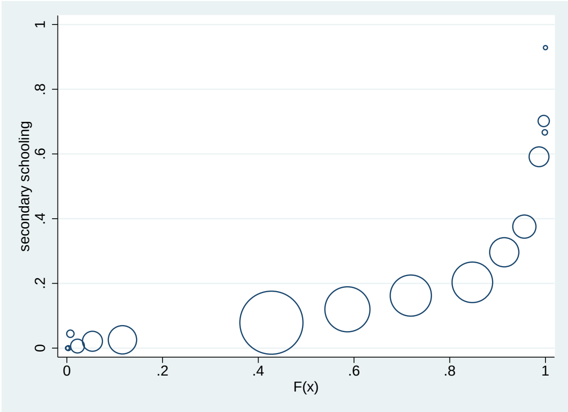


(a) Males

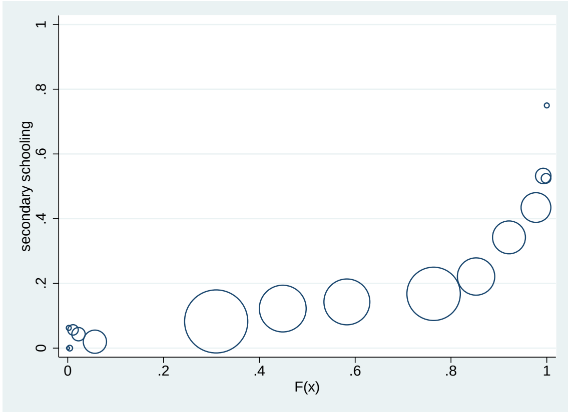
(b) Females

Note: Covariates which are included are a dummy indicating twin births, dummies capturing old (>35 years) and young (<20) mothers and a dummy for married women.

Figure 6. Quantile regression of GPA by gender.



(a) Males



(b) Females

Note: Circle size weighted by number of people in each group.

Figure 7. Correlation of marks in primary school and secondary schooling uptake.

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

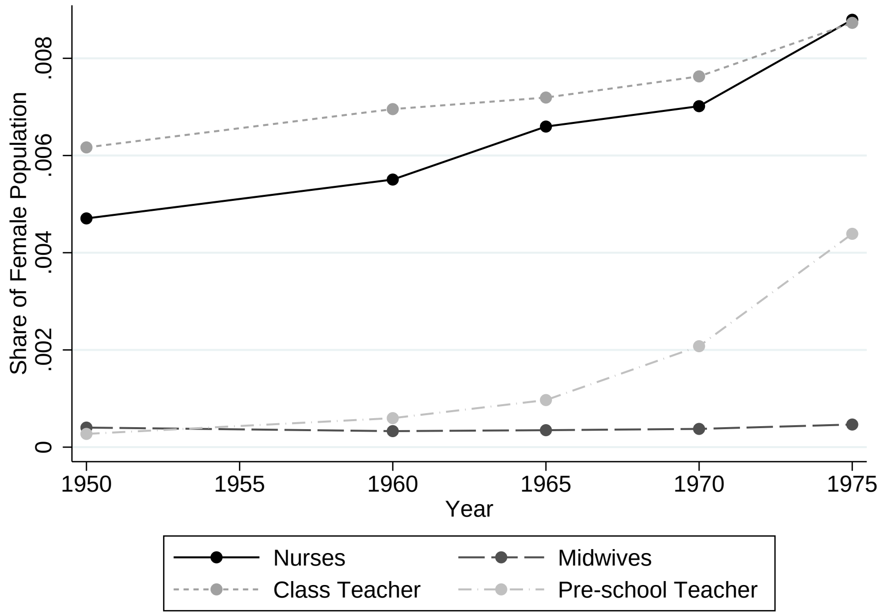


Figure 9. Females working in public sector jobs *Source: Statistiska Centralbyrån (2009).*

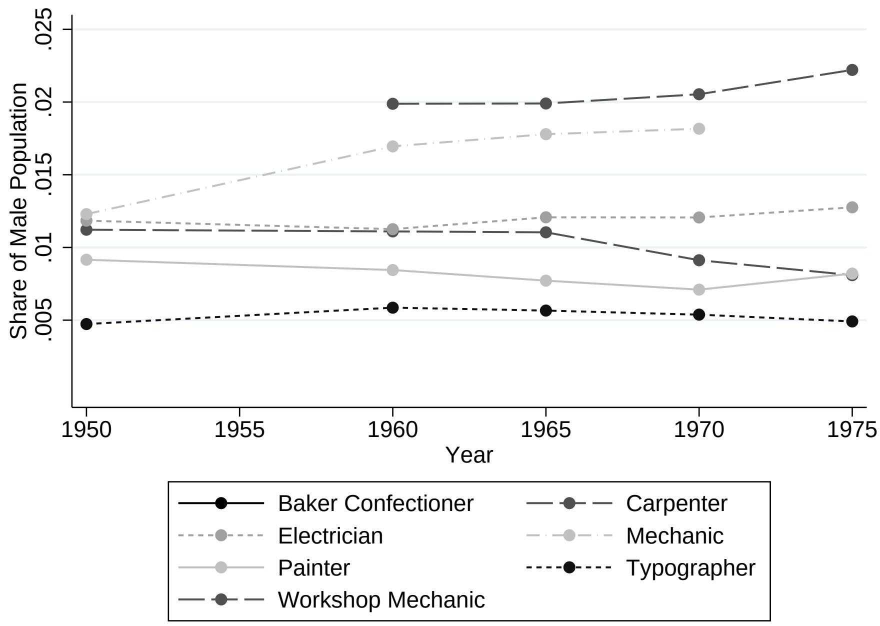


Figure 10. Males working in typical apprenticeship jobs *Source: Statistiska Centralbyrån (2009).*

Figure 11. Randomisation inference long-term outcomes males.

Figure 12. Randomisation inference long-term outcomes females.

Table 1. 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.

Table 2. Descriptive statistics outcome variables.

	Females & Males					Females		Males	
	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
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 1950									
Working 1950	20,291	0.577	0.494	0	1	9,934	0.539	10,357	0.613
Student	20,327	0.032	0.176	0	1	9,965	0.032	10,362	0.032
Apprenticeship	20,327	0.021	0.143	0	1	9,965	0.001	10,362	0.040
Vocational Female	19,456	0.023	0.150	0	1	8,940	0.050	10,516	0.000
Census 1970									
Secondary Schooling	20,911	0.182	0.386	0	1	10,298	0.188	10,613	0.176
Working Fulltime	20,723	0.635	0.481	0	1	10,257	0.336	10,466	0.929
Working Parttime	20,723	0.136	0.343	0	1	10,257	0.259	10,466	0.016
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.

Table 3. Descriptive statistics and p-values for difference in means.

BEFORE	Control Count	Mean	SD	Min	Max	Treated Count	Mean	SD	Min	Max	P-value
School Data											
Share Sickn. Abs.	1,923	0.043	0.062	0	1	2,290	0.051	0.073	0	1	0.00007
GPA_cog	1,935	3.510	0.702	1	6	2,353	3.519	0.674	1	6	0.67201
Math	1,923	3.460	0.815	1	6	2,344	3.516	0.825	1	6	0.02772
Reading/Speaking	1,931	3.567	0.768	1	7	2,349	3.556	0.711	1	6	0.61143
Writing	1,541	3.460	0.817	1	7	2,005	3.419	0.771	1	6	0.12495
Census 1950											
Working 1950	1,420	0.740	0.439	0	1	1,832	0.727	0.446	0	1	0.40378
Student	1,424	0.032	0.177	0	1	1,835	0.016	0.127	0	1	0.00275
Apprenticeship	1,424	0.013	0.115	0	1	1,835	0.018	0.133	0	1	0.29444
Vocational Female	1,381	0.020	0.141	0	1	1,804	0.020	0.140	0	1	0.94924
Census 1970											
Secondary Schooling	1,468	0.138	0.345	0	1	1,919	0.178	0.382	0	1	0.00162
Working Fulltime	1,456	0.650	0.477	0	1	1,904	0.641	0.480	0	1	0.56233
Working Parttime	1,456	0.130	0.337	0	1	1,904	0.137	0.344	0	1	0.57911
log Income	1,470	9.554	1.113	0	12	1,920	9.565	1.134	0	12	0.79037
Municipal	1,456	0.167	0.373	0	1	1,904	0.148	0.355	0	1	0.13731
Governmental	1,456	0.079	0.270	0	1	1,904	0.085	0.279	0	1	0.52419
DURING	**Control** Count	Mean	SD	Min	Max	**Treated** Count	Mean	SD	Min	Max	**P-value**
School Data											
Share Sickn. Abs.	8,399	0.044	0.059	0	1	9,961	0.050	0.066	0	1	0.00000
GPA_cog	8,383	3.536	0.717	1	6	10,042	3.545	0.717	1	6	0.37972
Math	8,360	3.488	0.847	1	7	10,020	3.506	0.844	1	7	0.15659
Reading/Speaking	8,353	3.586	0.778	1	7	10,014	3.609	0.775	1	7	0.04644
Writing	6,658	3.484	0.837	1	6	8,867	3.461	0.821	1	6	0.08680
Census 1950											
Working 1950	6,137	0.613	0.487	0	1	7,935	0.574	0.495	0	1	0.00000
Student	6,145	0.048	0.213	0	1	7,952	0.025	0.156	0	1	0.00000
Apprenticeship	6,145	0.019	0.138	0	1	7,952	0.024	0.152	0	1	0.07625
Vocational Female	5,896	0.026	0.158	0	1	7,617	0.023	0.150	0	1	0.31654
Census 1970											
Secondary Schooling	6,314	0.172	0.377	0	1	8,172	0.196	0.397	0	1	0.00028
Working Fulltime	6,254	0.638	0.481	0	1	8,096	0.630	0.483	0	1	0.31585
Working Parttime	6,254	0.135	0.342	0	1	8,096	0.136	0.343	0	1	0.86188
log Income	6,316	9.520	1.153	0	13	8,176	9.560	1.123	0	12	0.03872
Municipal	6,254	0.166	0.372	0	1	8,096	0.161	0.367	0	1	0.39611
Governmental	6,254	0.084	0.277	0	1	8,096	0.090	0.286	0	1	0.21179
AFTER	**Control** Count	Mean	SD	Min	Max	**Treated** Count	Mean	SD	Min	Max	**P-value**
School Data											
Share Sickn. Abs.	1,715	0.045	0.061	0	1	2,015	0.050	0.058	0	1	0.01283
GPA_cog	1,733	3.539	0.719	1	6	2,029	3.561	0.724	1	6	0.34638
Math	1,729	3.478	0.818	1	6	2,027	3.522	0.857	1	6	0.10834
Reading/Speaking	1,726	3.608	0.795	1	6	2,027	3.632	0.778	1	7	0.33755
Writing	1,344	3.495	0.826	1	6	1,820	3.475	0.837	1	6	0.50262
Census 1950											
Working 1950	1,282	0.378	0.485	0	1	1,685	0.313	0.464	0	1	0.00022
Student	1,284	0.041	0.199	0	1	1,687	0.019	0.136	0	1	0.00030
Apprenticeship	1,284	0.031	0.174	0	1	1,687	0.014	0.118	0	1	0.00164
Vocational Female	1,195	0.017	0.128	0	1	1,563	0.022	0.148	0	1	0.29254
Census 1970											
Secondary Schooling	1,317	0.176	0.381	0	1	1,721	0.199	0.399	0	1	0.11547
Working Fulltime	1,308	0.632	0.482	0	1	1,705	0.638	0.481	0	1	0.76575
Working Parttime	1,308	0.131	0.338	0	1	1,705	0.151	0.358	0	1	0.13434
log Income	1,317	9.501	1.158	0	12	1,722	9.572	1.089	5	13	0.08175
Municipal	1,308	0.146	0.353	0	1	1,705	0.158	0.365	0	1	0.37441
Governmental	1,308	0.083	0.275	0	1	1,705	0.077	0.267	0	1	0.60500

Note: This table shows descriptive statistics and differences in means for the outcome variables of children born before, during and after the infant intervention starting 1 October 1931 and ending 30 June 1933. Variable descriptions to this table are available in Appendix A.

Table 4. DID estimates of the infant intervention for cognitive GPA.

		Standardised Grading Scale		
		(1)	(2)	(3)
Males and Females	**Grade 1 and 4**	0.0502	0.0474	0.0460
	SE	(0.031)	(0.030)	(0.030)
	N	26,475	26,475	26,475
	Pre-mean	-0.039	-0.039	-0.039
Males and Females	**Grade 1**	0.0264	0.0124	-0.0020
	SE	(0.051)	(0.051)	(0.053)
	N	13,207	13,207	13,207
	Pre-mean	-0.032	-0.032	-0.032
	Grade 4	0.0761*	0.0691**	0.0759**
	SE	(0.039)	(0.033)	(0.036)
	N	13,268	13,268	13,268
	Pre-mean	-0.047	-0.047	-0.047
Males	**Grade 1**	0.0073	0.0055	-0.0352
	SE	(0.058)	(0.051)	(0.049)
	N	6,803	6,803	6,803
	Pre-mean	-0.093	-0.093	-0.093
	Grade 4	0.0818	0.1156**	0.1084
	SE	(0.057)	(0.055)	(0.072)
	N	6,707	6,707	6,707
	Pre-mean	-0.200	-0.200	-0.200
Females	**Grade 1**	0.0487	0.0407	0.0468
	SE	(0.074)	(0.074)	(0.084)
	N	6,404	6,404	6,404
	Pre-mean	0.027	0.027	0.027
	Grade 4	0.0701	0.0362	0.0617
	SE	(0.049)	(0.050)	(0.054)
	N	6,561	6,561	6,561
	Pre-mean	0.098	0.098	0.098
	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 5. DID estimates for subjects.

		Standardised Grading Scale							
		Math		**Reading**		**Writing**		**Christianity**	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Males and Females	**Grade 1 and 4**	-0.0181	-0.0181	0.0740**	0.0647*	0.1239**	0.1216**	0.0029	0.0121
	SE	(0.033)	(0.031)	(0.032)	(0.036)	(0.051)	(0.051)	(0.039)	(0.032)
	N	26,403	26,403	26,400	26,400	22,235	22,235	26,298	26,298
	Pre-mean	-0.042	-0.042	-0.028	-0.028	-0.040	-0.040	-0.036	-0.036
Males and Females	**Grade 1**	-0.0327	-0.0525	0.0331	0.0170	0.0937	0.0891	0.0107	-0.0223
	SE	(0.050)	(0.050)	(0.050)	(0.053)	(0.093)	(0.093)	(0.067)	(0.071)
	N	13,161	13,161	13,177	13,177	9,007	9,007	13,060	13,060
	Pre-mean	-0.058	-0.058	0.001	0.001	-0.016	-0.016	-0.027	-0.027
	Grade 4	-0.0220	0.0010	0.1179**	0.1105*	0.1239**	0.1129**	-0.0150	0.0372
	SE	(0.047)	(0.045)	(0.045)	(0.059)	(0.056)	(0.054)	(0.049)	(0.035)
	N	13,242	13,242	13,223	13,223	13,228	13,228	13,238	13,238
	Pre-mean	-0.027	-0.027	-0.056	-0.056	-0.057	-0.057	-0.044	-0.044
Males	**Grade 1**	-0.0895*	-0.1121**	0.0534	0.0094	0.0528	0.0225	0.0326	-0.0160
	SE	(0.049)	(0.045)	(0.068)	(0.071)	(0.074)	(0.075)	(0.062)	(0.066)
	N	6,779	6,779	6,794	6,794	4,608	4,608	6,723	6,723
	Pre-mean	-0.066	-0.066	-0.082	-0.082	-0.131	-0.131	-0.054	-0.054
	Grade 4	0.0193	0.0317	0.1823***	0.1649**	0.1645**	0.1291*	-0.0222	0.0247
	SE	(0.079)	(0.091)	(0.064)	(0.082)	(0.064)	(0.072)	(0.097)	(0.096)
	N	6,688	6,688	6,687	6,687	6,692	6,692	6,689	6,689
	Pre-mean	-0.082	-0.082	-0.241	-0.241	-0.275	-0.275	-0.184	-0.184
Females	**Grade 1**	0.0201	0.0014	0.0393	0.0570	0.1469	0.1719	0.0031	-0.0131
	SE	(0.089)	(0.090)	(0.062)	(0.073)	(0.126)	(0.129)	(0.097)	(0.106)
	N	6,382	6,382	6,383	6,383	4,399	4,399	6,337	6,337
	Pre-mean	-0.050	-0.050	0.082	0.082	0.091	0.091	-0.000	-0.000
	Grade 4	-0.0535	-0.0217	0.0832	0.0902	0.0859	0.1068	0.0160	0.0654
	SE	(0.051)	(0.056)	(0.057)	(0.066)	(0.081)	(0.094)	(0.052)	(0.066)
	N	6,554	6,554	6,536	6,536	6,536	6,536	6,549	6,549
	Pre-mean	0.025	0.025	0.120	0.120	0.150	0.150	0.088	0.088
	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 6. DID estimates of the infant intervention for share of sickness absence.

		(1)	(2)	(3)
Males and Females	**Grade 1 and 4**	-0.0021	-0.0032	-0.0034
	SE	(0.002)	(0.003)	(0.003)
	N	26,303	26,303	26,303
	Pre-mean	0.048	0.048	0.048
Males and Females	**Grade 1**	-0.0022	-0.0042	-0.0044
	SE	(0.004)	(0.005)	(0.005)
	N	13,165	13,165	13,165
	Pre-mean	0.052	0.052	0.052
	Grade 4	-0.0021	-0.0014	-0.0002
	SE	(0.002)	(0.003)	(0.002)
	N	13,138	13,138	13,138
	Pre-mean	0.045	0.045	0.045
Males	**Grade 1**	-0.0048	-0.0060	-0.0076
	SE	(0.007)	(0.009)	(0.009)
	N	6,792	6,792	6,792
	Pre-mean	0.051	0.051	0.051
	Grade 4	-0.0059	-0.0073	-0.0083*
	SE	(0.005)	(0.005)	(0.004)
	N	6,651	6,651	6,651
	Pre-mean	0.040	0.040	0.040
Females	**Grade 1**	0.0009	-0.0029	-0.0025
	SE	(0.008)	(0.009)	(0.008)
	N	6,373	6,373	6,373
	Pre-mean	0.052	0.052	0.052
	Grade 4	0.0018	0.0061	0.0100*
	SE	(0.004)	(0.004)	(0.006)
	N	6,487	6,487	6,487
	Pre-mean	0.050	0.050	0.050
	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. *ymean* 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 specific linear trends* allows for parish specific time trends.

Table 7. DID estimates for medium-term outcomes.

		Census 1970		Census 1950							
		Secondary Schooling		Working 1950		Student		Apprenticeship		Vocational Females	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Males and Females	DID	-0.0062	0.0027	-0.0019	-0.0281*	0.0145*	0.0142**	0.0237**	0.0169*	0.0003	-0.0015
	SE	(0.017)	(0.013)	(0.022)	(0.015)	(0.008)	(0.007)	(0.010)	(0.010)	(0.007)	(0.008)
	N	20,910	20,910	20,290	20,290	20,326	20,326	20,326	20,326	19,455	19,455
	Pre-mean	0.185	0.185	0.732	0.732	0.024	0.024	0.016	0.016	0.020	0.020
Females	DID	0.0353**	0.0350**	0.0015	-0.0133	0.0142	0.0137	0.0029*	0.0015	0.0014	-0.0021
	SE	(0.016)	(0.014)	(0.028)	(0.024)	(0.011)	(0.011)	(0.001)	(0.001)	(0.015)	(0.016)
	N	10,297	10,297	9,933	9,933	9,964	9,964	9,964	9,964	8,939	8,939
	Pre-mean	0.198	0.198	0.662	0.662	0.024	0.024	0.000	0.000	0.039	0.039
Males	DID	-0.0468	-0.0289	-0.0053	-0.0426**	0.0148*	0.0147**	0.0440**	0.0319	-0.0006	-0.0010
	SE	(0.029)	(0.021)	(0.038)	(0.020)	(0.008)	(0.007)	(0.020)	(0.019)	(0.000)	(0.001)
	N	10,613	10,613	10,357	10,357	10,362	10,362	10,362	10,362	10,516	10,516
	Pre-mean	0.172	0.172	0.802	0.802	0.025	0.025	0.031	0.031	0.001	0.001
	Parish FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
	QOB×YOB Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
	SES Rffects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
	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.

Table 8. DID estimates for long-term income.

		log Income 1970		log Pension Age 71	
		(1)	(2)	(3)	(4)
Males and Females	DID	0.0295	0.0732**	-0.0035	0.0187
	SE	(0.033)	(0.028)	(0.012)	(0.014)
	N	20,920	20,920	15,964	15,964
	Pre-mean	9.593	9.593	11.789	11.789
Females	DID	0.1204*	0.1947***	0.0293	0.0711***
	SE	(0.063)	(0.066)	(0.019)	(0.015)
	N	10,301	10,301	8,284	8,284
	Pre-mean	8.990	8.990	11.609	11.609
Males	DID	-0.0596	-0.0464	-0.0400**	-0.0400*
	SE	(0.037)	(0.036)	(0.017)	(0.020)
	N	10,619	10,619	7,680	7,680
	Pre-mean	10.222	10.222	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.

Table 9. Survival selection Log income 1970 at different ages.

		All	**Age 40**	**Age 50**	**Age 60**	**Age 70**	**Age 75**
		(1)	(2)	(3)	(4)	(5)	(6)
Females	DID	0.1947***	0.1942***	0.2184***	0.2080***	0.2062***	0.2592***
	SE	(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
	Pre-mean	8.990	8.990	8.990	8.990	8.990	8.990
Males	DID	-0.0464	-0.0459	-0.0377	-0.0532*	-0.0750**	-0.0921***
	SE	(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
	Pre-mean	10.222	10.222	10.222	10.222	10.222	10.222
	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.

Table 10. DID estimates for long-term employment.

		Working Parttime		**Working Fulltime**		**Municipal**		**Governmental**	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Males and Females	DID	-0.0201	-0.0147	0.0276	0.0349*	0.0194*	0.0295**	0.0126	0.0131
	SE	(0.017)	(0.017)	(0.017)	(0.020)	(0.011)	(0.013)	(0.013)	(0.014)
	N	20,722	20,722	20,722	20,722	20,722	20,722	20,722	20,722
	Pre-mean	0.145	0.145	0.640	0.640	0.167	0.167	0.081	0.081
Females	DID	-0.0325	-0.0244	0.0607*	0.0760**	0.0377*	0.0488**	0.0306***	0.0339**
	SE	(0.030)	(0.033)	(0.031)	(0.037)	(0.020)	(0.020)	(0.012)	(0.014)
	N	10,256	10,256	10,256	10,256	10,256	10,256	10,256	10,256
	Pre-mean	0.265	0.265	0.370	0.370	0.238	0.238	0.051	0.051
Males	DID	-0.0077	-0.0049	-0.0052	-0.0061	0.0012	0.0102	-0.0053	-0.0077
	SE	(0.007)	(0.007)	(0.014)	(0.015)	(0.014)	(0.016)	(0.019)	(0.019)
	N	10,466	10,466	10,466	10,466	10,466	10,466	10,466	10,466
	Pre-mean	0.019	0.019	0.925	0.925	0.092	0.092	0.111	0.111
	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.

Table 11. Occupational groups.

		Scientific, Medical, Technical (1)	**Admin.** (2)	**Accounting, Admin.** (3)	**Sales** (4)	**Agricultural** (5)
Females	DID	0.0462***	0.0029	0.0433*	-0.0231	0.0071
	SE	(0.016)	(0.006)	(0.024)	(0.018)	(0.007)
	N	10,256	10,256	10,256	10,256	10,256
	Pre-mean	0.170	0.006	0.124	0.083	0.026
	Mean Income in SEK	23,976	31,336	19,270	14,856	13,048
Males	DID	-0.0340	-0.0003	-0.0207	0.0213*	0.0090
	SE	(0.021)	(0.014)	(0.017)	(0.012)	(0.014)
	N	10,466	10,466	10,466	10,466	10,466
	Pre-mean	0.195	0.033	0.037	0.085	0.095
	Mean Income in SEK	43,475	51,494	33,407	34,903	24,195
	Parish FE	✓	✓	✓	✓	✓
	QOB×YOB Effects	✓	✓	✓	✓	✓
	SES Effects	✓	✓	✓	✓	✓
	School Reforms	✓	✓	✓	✓	✓
	Parish Specific Linear Trends	✓	✓	✓	✓	✓

		Mining (1)	**Transport, Commu-nication** (2)	**Crafts** (3)	**Service** (4)	**Out of Labour Force** (5)
Females	DID	0.0003	-0.0063	-0.0169	-0.0037	-0.0498*
	SE	(0.001)	(0.011)	(0.019)	(0.016)	(0.026)
	N	10,256	10,256	10,256	10,256	10,256
	Pre-mean	0.001	0.032	0.060	0.130	0.367
	Mean Income in SEK	24,679	17,745	15,715	12,054	3,522
Males	DID	0.0022	0.0152	-0.0284	0.0246	0.0112
	SE	(0.009)	(0.015)	(0.021)	(0.020)	(0.013)
	N	10,466	10,466	10,466	10,466	10,466
	Pre-mean	0.037	0.080	0.340	0.041	0.059
	Mean Income in SEK	29,450	29,329	27,212	30,874	11,712
	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.

Table 12. Returns to education.

	Males & Females (1)	**Females** (2)	**Males** (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.

Table 13. Returns to vocational training and studying.

	Males & Females (1)	**Females** (2)	**Males** (3)
Apprenticeship	0.0799*	0.9118**	0.0337
	(0.041)	(0.355)	(0.027)
Student	0.2693***	0.4504***	0.1240***
	(0.036)	(0.069)	(0.032)
Vocational Female	0.1651***	0.1443***	-0.5343**
	(0.041)	(0.053)	(0.270)
Secondary Schooling	0.5118***	0.5879***	0.4425***
	(0.017)	(0.032)	(0.016)
Female	-1.2045***		
	(0.012)		
N	18,854	8,600	10,254
R^2	0.374	0.062	0.117

Note: *** p <0,01; ** p <0,05; * p <0,1, Outcome variable is log income 1970. 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 and a dummy indicating a hospital birth. Variable descriptions to this table are available in Appendix A.

Table 14. 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 4. Reference group category 3. Marks 6 and 7 pooled due to few observations.

Table 15. 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 16. 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 17. Mother fixed effects school outcomes.

		GPA		Math		Reading		Writing	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Males and Females	DID	0.1189*	0.0893	-0.0255	-0.0497	0.2230***	0.1757**	0.1612*	0.1393
	SE	(0.062)	(0.061)	(0.067)	(0.080)	(0.075)	(0.074)	(0.088)	(0.092)
	N	5,809	5,809	5,793	5,793	5,782	5,782	5,785	5,785
	Pre-mean	-0.047	-0.047	-0.027	-0.027	-0.056	-0.056	-0.056	-0.056
Females	DID	0.0583	-0.0216	-0.1391	-0.1727	0.1151	-0.0757	0.2207	0.1832
	SE	(0.089)	(0.078)	(0.100)	(0.131)	(0.124)	(0.123)	(0.175)	(0.165)
	N	2,828	2,828	2,824	2,824	2,811	2,811	2,813	2,813
	Pre-mean	0.098	0.098	0.025	0.025	0.120	0.120	0.150	0.150
Males	DID	0.1735	0.1981	-0.0484	0.0405	0.4014**	0.3800**	0.1775	0.1711
	SE	(0.123)	(0.126)	(0.139)	(0.154)	(0.161)	(0.179)	(0.145)	(0.151)
	N	2,981	2,981	2,969	2,969	2,971	2,971	2,972	2,972
	Pre-mean	-0.200	-0.200	-0.082	-0.082	-0.241	-0.241	-0.275	-0.275
	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 18. Mother fixed effects long-run.

		Census 1970				Census 1950							
		Secondary Schooling		**log Income 1970**		**Working 1950**		**Student**		**Apprenticeship**		**Vocational Females**	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Males and Females	DID	0.0059	0.0076	0.0338	0.0718	-0.0175	-0.0403	0.0537***	0.0723***	0.0457*	0.0196*	-0.0056	-0.0027
	SE	(0.020)	(0.024)	(0.065)	(0.067)	(0.045)	(0.034)	(0.018)	(0.020)	(0.026)	(0.012)	(0.010)	(0.011)
	N	8,723	8,723	8,726	8,726	8,464	8,464	8,482	8,482	8,482	8,482	8,031	8,031
	Pre-mean	0.185	0.185	9.593	9.593	0.732	0.732	0.024	0.024	0.016	0.016	0.020	0.020
Females	DID	0.0192	0.0419	0.0596	0.1298	-0.0451	-0.0862	0.0556**	0.0603**	-0.0000	0.0000	-0.0053	-0.0160
	SE	(0.035)	(0.030)	(0.133)	(0.164)	(0.065)	(0.080)	(0.023)	(0.024)	(.)	(0.000)	(0.036)	(0.043)
	N	4,335	4,335	4,336	4,336	4,180	4,180	4,197	4,197	4,197	4,197	3,686	3,686
	Pre-mean	0.198	0.198	8.990	8.990	0.662	0.662	0.024	0.024	0.000	0.000	0.039	0.039
Males	DID	-0.0106	0.0066	-0.0407	0.0274	-0.1167**	-0.1667***	0.0733***	0.0793***	0.0718*	0.0424*	0.0003	-0.0019
	SE	(0.041)	(0.044)	(0.079)	(0.084)	(0.056)	(0.053)	(0.021)	(0.025)	(0.039)	(0.024)	(0.000)	(0.001)
	N	4,388	4,388	4,390	4,390	4,284	4,284	4,285	4,285	4,285	4,285	4,345	4,345
	Pre-mean	0.172	0.172	10.222	10.222	0.802	0.802	0.025	0.025	0.031	0.031	0.001	0.001
	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.

Table 19. 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.

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.

Christianity Mark for “Christianity” 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 1950 Population Census:

Apprenticeship Dummy variable taking on value one for someone being in apprenticeship 1950.

Vocational Females Dummy variable taking on value one for someone working as nurse, midwife, pre-school teacher or dental nurse in 1950.

Student Dummy variable taking on value one for someone studying 1950.

Working 1950 Dummy variable taking on value one for someone working 1950 (excluding students and apprenticeship).

A.4 Variables from 1970 Population and Household Census:

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.

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.

Vocational Females Dummy variable taking on value one for someone having typical female vocational training.

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.[33] 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

[33]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)

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.

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: Utilisation

Table C2 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., 201x). 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 C2. 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.

D 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 D3 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. (201x) 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 D3. Heterogeneity in treatment effects academic performance grade 4.

	GPA		**Math**		**Reading**		**Writing**	
	Single (1)	**low SES** (2)	**Single** (3)	**low SES** (4)	**Single** (5)	**low SES** (6)	**Single** (7)	**low SES** (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.

E Appendix: Treatment Indicator

- Continuous DID: As in main paper
- Binary any exposure: =1 if duration for infant eligibility>0 days; 0 otherwise
- 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 E4. Results for different treatment indicators on long-term outcomes.

	log Income 1970 (1)	**Working Parttime** (2)	**Working Fulltime** (3)	**Secondary Schooling** (4)	**Municipal** (5)	**Governmental** (6)	**Apprenticeship** (7)	**Student** (8)
Continuous DID	0.0732**	-0.0147	0.0349*	0.0027	0.0295**	0.0131	0.0169*	0.0142**
	(0.028)	(0.017)	(0.020)	(0.013)	(0.013)	(0.014)	(0.010)	(0.007)
Binary Any Exposure	0.0337	-0.0130	0.0198*	-0.0145	0.0105	0.0121	0.0091**	0.0029
	(0.033)	(0.013)	(0.012)	(0.017)	(0.012)	(0.010)	(0.004)	(0.008)
Binary Min 3 Months	0.0734***	-0.0211	0.0298**	0.0051	0.0107	0.0121	0.0092**	0.0058
	(0.022)	(0.018)	(0.014)	(0.011)	(0.009)	(0.008)	(0.004)	(0.008)
Binary at Least First 3 Months Complete	0.0854**	-0.0405	0.0626***	0.0013	0.0300	0.0117	0.0193*	0.0085
	(0.033)	(0.027)	(0.021)	(0.014)	(0.021)	(0.014)	(0.011)	(0.008)
Binary Other Treated	0.0052	0.0018	-0.0036	-0.0221	-0.0014	0.0135	0.0035	-0.0001
	(0.047)	(0.010)	(0.012)	(0.023)	(0.010)	(0.010)	(0.007)	(0.011)
Binary 12 Months/Full Eligibility	0.0507	-0.0292	0.0479**	0.0053	0.0311	0.0028	0.0196**	0.0070
	(0.037)	(0.019)	(0.022)	(0.014)	(0.023)	(0.013)	(0.010)	(0.009)
Binary Other Treated	0.0270	-0.0072	0.0095	-0.0208	0.0019	0.0165	0.0053	0.0015
	(0.039)	(0.013)	(0.011)	(0.021)	(0.010)	(0.010)	(0.005)	(0.010)
N	20,920	20,722	20,722	20,910	20,722	20,722	20,326	20,326
Pre-mean	9.593	0.145	0.640	0.185	0.167	0.081	0.016	0.024
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.

F Appendix: Tables

Table F5. 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 F6. Descriptive statistics explanatory variables.

	All Live Births N=24,390				School Data N=16,089	Census 1950 N=20,327	Census 1970 N=20,921
	Mean	SD	Min	Max	Mean	Mean	Mean
Female	0.485	0.500	0	1	0.493	0.490	0.492
Wedlock	0.895	0.307	0	1	0.921	0.902	0.902
Twin	0.026	0.160	0	1	0.023	0.023	0.023
Treated	0.566	0.496	0	1	0.551	0.564	0.565
Mother<20	0.052	0.222	0	1	0.044	0.050	0.050
Mother>35	0.226	0.418	0	1	0.238	0.222	0.223
Hospital Birth	0.295	0.456	0	1	0.253	0.300	0.298
SES Professional/Technical	0.029	0.168	0	1	0.023	0.029	0.029
SES Administrative/Managerial	0.024	0.153	0	1	0.020	0.024	0.024
SES Clerical	0.015	0.121	0	1	0.013	0.015	0.015
SES Sales	0.026	0.158	0	1	0.023	0.026	0.025
SES Service	0.027	0.163	0	1	0.019	0.026	0.026
SES Agricultural	0.381	0.486	0	1	0.405	0.386	0.386
SES Production	0.397	0.489	0	1	0.410	0.399	0.399
SES Unknown	0.101	0.301	0	1	0.087	0.094	0.095
DurationI	0.353	0.402	0	1	0.354	0.351	0.351
DurationM	0.257	0.315	0	1	0.256	0.258	0.257
Duration	0.610	0.587	0	2	0.610	0.609	0.608

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

Table F7. 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 F8. DID estimates for subjects.

		Anchored Grading Scale			
		Math	**Reading**	**Writing**	**Christianity**
		(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.