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ABSTRACT

Long-Term Effects of Childhood Nutrition: Evidence from a School Lunch Reform*

We examine the long-term impact of a policy that introduced free and nutritious school lunches in Swedish primary schools. For this purpose, we use historical data on the gradual implementation of the policy across municipalities and employ a difference-in-differences design to estimate the impact of this lunch policy on a broad range of medium and long-term outcomes, including lifetime income, health, cognitive skills, and education. Our results show that the school lunch program generated substantial long-term benefits, where pupils exposed to the program during their entire primary school period have 3 percent greater life-time earnings. In addition, we find the effect to be greater for pupils that were exposed at earlier ages and for pupils from poor households. Finally, exposure to the school lunch program had substantial effects on educational attainment and health and these effects can explain a large part of the return to school lunches.

JEL Classification: I12, I38, J24

Keywords: nutrition, early life, childhood, long-term, income, causal

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1 Introduction

Recent research has shown beneficial long-term effects of early childhood interventions (Currie & Almond 2011a; Currie & Almond 2011b). We know much less, however, about the long-term effects of government policies targeting children in the phase between early childhood and adulthood, such as school meal programs (Almond et al. 2017). The period between childhood and adolescence is believed to be a critical period for diets of high nutritional quality and, today, many Western countries feed their school-age children through extensive government sponsored school-meal programs (WHO 2006). In the U.S., for instance, the National School Lunch Program (NSLP) alone serves approximately 30 million children (57% of the student body) at a cost of $13.6 billion.[1]

Despite school lunch programs being around since the 1940s in rich countries such as Sweden and the U.S., it has been difficult to evaluate these programs' effectiveness. Since the U.S. school lunch program is federal, with no or little variation across areas, commonly used quasi-experimental approaches are not easily applied (Hoynes & Schanzenbach 2015). Hence, it is unclear how effective school lunch programs are relative to other related social reforms, such as food stamps, HEAD start, or increasing income transfers to poor families, in improving long-term outcomes (Ludwig & Miller 2007; Dahl & Lochner 2012; Hoynes et al. 2016). Moreover, the quality of school meal programs has recently been questioned in many Western countries, where the increasing prevalence of overweight and obese children has put school lunches at top of the debate (Schanzenbach 2009). This has led governments to impose stricter nutritional standards on school lunches but evidence on the long-term impacts of such initiatives is lacking.

This study provides evidence on the medium and long-term economic benefits of an extensive school lunch program. More specifically, we ask whether a policy that introduced free and nutritious school lunches in Swedish primary schools in the 1950s and 1960s improved children's medium and long-term economic, cognitive, educational, and health outcomes. The policy imposed strict nutritional standards on the meals served, which were to provide a third of the daily caloric need; contain strictly specified amounts of proteins, vitamins, calcium and iron; and contain a maximum fat content. Interestingly, these standards are very similar to the ones introduced in more recent meal programs, such as the "School Meals Initiative for Healthy Children" that was passed by the U.S. congress in 1995 in response to evidence questioning the nutritional quality of the NSLP program.[2] Evaluating the Swedish program therefore constitutes a unique opportunity to improve our understanding of the potential long-term effects of more recent initiatives to serve children more healthy school meals.[3]

The main motivation behind the Swedish school lunch program was that the nutritional

[1] https://www.ers.usda.gov/topics/food-nutrition-assistance/child-nutrition-programs/national-school-lunch-program/.

[2] The nutritional standards of the NLSP were further updated in 2012 under the "Hunger-Free Kids Act" but the Trump administration recently scaled back these standards in a proclamation signed by the Secretary of Agriculture Sonny Perdue (see http://edition.cnn.com/2017/05/02/health/school-lunch-changes/index.html).

[3] The Swedish school lunch program also shares other characteristics with recent programs, including (partial) reimbursement of costs to schools, controls that schools follow the program, and the provision of education and training of school food personnel.

quality of the meals consumed during the school lunch break was deemed inadequate and, in particular, fell short in terms of vitamins A, C and D, protein, and certain minerals. Food shortage and hunger was uncommon in Sweden during the 1950s and 1960s and the program was aimed at improving nutritional standards rather than improving access to food or caloric intake. Another motivation behind the policy was to ease the economic burden on the households by freeing them from the task of providing school lunches (SOU 1945). An interesting feature of the program was therefore that it provided improved nutrition, but potentially also improved household finances.

To estimate the impact of the program, we use newly collected historical data on its gradual implementation across municipalities in Sweden between the years 1959 and 1969. During this time period, almost 300 municipalities introduced the program, with a fairly equal number of municipalities per year. We have linked the data on the program's introduction to administrative records on approximately 1.5 million individuals, covering the population of primary school pupils during the study period. Using a difference-in-differences design, we then estimate the impact of the program on a broad range of outcomes measured from a variety of registers, including income and education registers, the military enlistment register, the medical birth register, and hospitalization and mortality registers.

Our results show that the Swedish school lunch program generated substantial long-term benefits, where pupils exposed to the program during their entire primary school period have 3 percent greater life-time earnings compared to pupils who were never exposed. We also find interesting heterogeneity in the effects, where children from poor households benefit the most from the program, although children from all households, except the richest, benefit to some extent. In addition, we show evidence of a clear dose-response relationship, where earlier exposure is associated with greater effects. Importantly, the school lunch program did not affect the probability of being obese or overweight at age 18, suggesting that the school lunch program did not provide an unhealthy excess of calories.

To shed light on possible mechanisms, we use data from multiple sources. Using data from the education register, we find that greater exposure to school meals had large and positive effects on years of schooling that can explain one half of the income effect. Using data from the military enlistment records and the medical birth register, we find that males and females who were more exposed to the meals were taller and that males were assessed to be healthier at time of enlistment, suggesting that improved nutrition, which was specifically at focus in the school lunch program, is an important mechanism. Using data from the hospitalization and mortality registers, we find no long-term effects on mortality or morbidity, however.

Since the school lunches were provided free of charge, an alternative mechanism behind our results is improvements in household income due to reduced food expenditures. Moreover, the school meal program may have affected female labor supply since it was no longer necessary for mothers to stay home and cook during the lunch break. When we run our analysis on closely spaced siblings, who should all be exposed to such potential improvements in household income, the estimates remain at least as strong. Moreover, when we calculate the potential savings in food expenditures of the household from free school lunches, we conclude that they are too small to generate long-term effects.

School lunches may also affect earnings through school attendance. In the Swedish context, we can rule out such effects since primary school was mandatory and since attendance rates were high already before the lunch program was introduced.

We address a number of threats to our empirical design. To assess the parallel trend assumption, we compare pretreatment trends across early, mid, and late adopters of the reform and find that they are parallel. In addition, an event-study analysis show that the effect arises for cohorts exposed to the lunch program but not for unexposed cohorts. The results are also robust to the inclusion of linear and quadratic municipality-specific trends. In addition, we check for any systematic migration of families in response to the program and find no evidence for such strategic behavior. The main results are also insensitive to using alternative income measures and to controlling for other potentially important reforms that were rolled out during the same time period.

Our paper makes several contributions to the literature. First, it fills a gap in the literature on the effect of early life policies on long-term outcomes. While previous studies have established that policies such as food stamps can have important long-term effects for those exposed in utero, we know less about the effect of policy-driven improvements during later phases of childhood and adolescence. Knowing if there are critical periods during which interventions are particularly effective is useful for policy-makers (van den Berg et al. 2014; Chetty et al. 2016; Hoynes et al. 2016).

Second, we provide new and rare evidence on the long-term effects of a policy-driven improvement in early-life nutrition. Most of the existing evaluations of school meal programs are of a short-term nature and evaluate meal programs where the food served is of questionable nutritional quality.[4] Third, by focusing on a policy that affected all children in school, we can investigate heterogeneity in the effect of the school lunch program across the entire parental income distribution. Fourth, whereas many of the policies studied previously, such as food stamps and welfare benefits, affect only a subset of the population, and therefore can be stigmatizing and create incentives for parents to modify their behavior in order to remain eligible for the program benefit, we study a universal policy where such effects can be ruled out.

The paper proceeds as follows: In the next section, we discuss the literature on the long-term effects of early life policies, including school meal policies, and discuss the different channels through which school meal programs may affect long-term outcomes. Section 3 describes the Swedish policy and setting in more detail. Section 4 describes our data, and Section 5 introduces our empirical design. Section 6 shows our main results whereas Section 7 shows the results on potential mechanisms through which the long-term effects may appear. Section 8 show the results across generations and Section 9 provides a set of robustness checks. Section 10 provides a cost-benefit calculation of the school-lunch program, and Section 11 concludes.

[4]We review the relevant literature in Section II.

2 Literature background

2.1 Early life policies

The negative long-term effects of various shocks in utero or infancy are now well established, and the economic literature has instead turned to analyzing the long-term effects of early life *policies*. Since this latter literature is closely related to our paper, we briefly review some of it below before turning to studies that have specifically analyzed the effect of school meal policies.[5]

An important literature evaluates the long-term effects of various welfare programs specifically targeted toward poor households. Hoynes et al. (2016) analyze the introduction of the Food Stamp Program across counties in the U.S. and show that the program led to a reduction in the incidence of metabolic syndrome and to increased women's self-sufficiency for cohorts that were exposed in childhood (ages 0-5). The authors argue that the program should mainly be viewed as an income transfer program rather than as a nutrition program. Earlier work by the same authors showed health gains at birth for babies whose mothers had access to food stamps during pregnancy, thus pointing to nutrition as an important pathway for the effects; see Hoynes et al. (2011).[6] Aizer et al. (2016) show that children in poor families who had access to cash transfers had favorable outcomes in terms of mortality, education, and earnings, and Ludwig & Miller (2007) show that the introduction of the Head Start program to poor children aged three to five (and their parents) increased educational attainment and reduced child mortality rates. Dahl & Lochner (2012) study the effect of the earned income tax credit on child outcomes and find positive effects on children's test scores, and Milligan & Stabile (2011) find similar effects in Canada.

In addition, more specific policies with the potential to affect fetuses, infants and small children have proven to be important for later life outcome. Nilsson (2017) shows that increased access to alcohol during pregnancy has negative long-term effects on affected children's earnings and education during adulthood. Other papers find positive long-term effects of specific health interventions such as treatments for low birth weight (Almond et al. 2010; Bharadwaj et al. 2013), breastfeeding interventions (Fitzsimons & Vera-Hernández 2016), improved access to maternal health clinics (Bhalotra et al. 2017; Hjort et al. 2017), expansions of Medicaid (Wherry & Meyer 2016; Brown et al. 2015), improved access to hospitals (Chay et al. 2009), and improved access to water fluoridation (Neidell et al. 2010).[7]

Most of the papers mentioned above concern policies affecting children in utero or the first years of life. For later phases of childhood, there is still very limited evidence and our paper extends the literature by focusing on the long-term effects of a policy targeting children at ages 7 to 16. A notable exception is Chetty et al. (2016), who analyze the long-term impact of the Moving-To-Opportunity (MTO) Program and show that children who were exposed to the program, and therefore moved to a lower-poverty area, before the age

[5]For a thorough literature review on the effect of early life policies in developed countries, see Almond et al. (2017).

[6]Nutritional interventions have been studied in the context of developing countries. Hoddinott et al. (2008) and Maluccio et al. (2009) studied the effect of a childhood nutritional intervention in Guatemala and found large and positive long-term effects on educational outcomes and adult income.

[7]Other papers evaluate policies that indirectly affect early life health, such as measure to reduce pollution (Nilsson 2009; Isen et al. 2017).

of 13, faced substantial long-term economic benefits.[8][9]

2.2 School lunch policies

Most of the literature on the impact of school meals focuses on various programs in the United States, such as the National School Lunch Program (NSLP) or various school breakfast programs.[10] It should be noted that the Swedish school lunch program differed from the historical NSLP in several ways. First, while the Swedish program provided specific nutrition guidance, as described in Section 3 below, similar U.S. guidelines were introduced as late as 1995. In line with the lack of specific nutrition guidelines, several early analyses of the nutritional content of the NSLP lunches revealed that NSLP participants were not nutritionally better off compared to non-participants (Paige 1972; Hanes et al. 1984). Later evaluations of the NSLP found that the program increased the intake of fat and important minerals, vitamins, and proteins but no long-term evaluations of this later phase of NSLP are available (see Hopkins & Gunther (2015) for a literature review). Second, the Swedish school lunches were, and still are, served free of charge for all pupils whereas the NSLP was, and still is, subsidized only for low-income families.

The findings regarding the effect of the NSLP are mixed and almost exclusively of a short-term nature. In a recent literature overview on the effect of various U.S. food and nutrition programs, Hoynes & Schanzenbach (2015) noted: "We have much more to learn about the potential benefits of these programs on health and wellbeing in the long run, and when in the life cycle is the most important time to provide these benefits."

Several studies have analyzed whether the NSLP contributes to the overweight and obesity "epidemic". Schanzenbach (2009) exploits sharp discontinuities in eligibility for reduced-price lunch to show that childhood obesity increases with exposure to the NSLP program. Similar findings were reported by Millimet et al. (2010), using panel data, but studies by Mirtcheva & Powell (2013) and Dunifon & Kowaleski-Jones (2004), also using panel data, and Gundersen et al. (2012), using a non-parametric bounds analysis, find opposite or no effects.

Short-term educational outcomes were analyzed in a U.K. study by Belot & James (2011), who evaluated the "Feed Me Better" campaign run by the British celebrity chef Jamie Oliver. The program aimed at improving the nutritional standards of school lunches in the U.K and the results showed a short-term decrease in absences and improved school grades following the introduction of the program. Using data from Californian public schools, Anderson et al. (2017) find positive effects on student achievement tests for students at schools who had contracts with healthy school vendors. Analyzing the short-term effects of a school lunch program in Chile aimed at increasing caloric intake, McEwan (2013) found no effects on

[8] Oreopoulos (2003) found weaker effects when studying differences in long-term outcomes between children assigned to substantially different housing projects in Toronto. The mean age at assignment was higher than in the MTO experiment, however. Jacob et al. (2015) found no long-term effects on child outcomes from a program that randomized housing vouchers in Chicago.

[9] In the economics of education literature a number of papers have analyzed the long-term impact of various educational interventions such as class size reductions; see Burgess (2016).

[10] See Galer-Unti et al. (1995) for a discussion on the introduction of NSLP.

attendance or grades.

We are aware of only one paper that evaluates the long-term impact of free school lunches. Hinrichs (2010) uses a change in the formula used by the federal government to allocate funding to the states in order to study the long-term impact of exposure to the NSLP program. He finds no effects on health but finds large and positive effects on educational attainment, where an increase in NSLP exposure in a state by 10 percentage points increases completed education by nearly one year on average for males and one-third of a year for females. As discussed by Hinrichs (2010), the impact of the NSLP on educational attainment may reflect increased attendance in schools following the access to school lunches together with an increase in calories consumed. To the best of our knowledge, there are no studies that estimate the long-term impact of a policy specifically targeted at increasing the quality of the school lunch.

Another related strand of literature evaluates the impact of various school *breakfast* programs, again mostly in the short run. Several studies report short-term positive effects of the program in terms of dietary quality, reduced obesity, school achievement, and attendance rates (Bhattacharya et al. 2006; Millimet et al. 2010; Frisvold 2015; Leos-Urbel et al. 2013). Evaluating a breakfast in classroom program (BIC), Imberman & Kugler (2014) found no effects on grades or attendance, however. Re-analyzing data from a randomized trial on a BIC program, Schanzenbach & Zaki (2014) report a few positive impacts on measures of dietary quality, and no positive impacts on behavior, health or achievement.[11]

The only paper on the long-term effects of serving a nutritious school breakfast is a recent paper by Bütikofer et al. (2016), where the long-term impact of a program that replaced the traditional hot meal at the end of the school day with a nutritious breakfast is evaluated. The results show a positive effect of the school breakfast on long-term economic outcomes. We differ from their paper in that we estimate the effect of introducing healthy school lunches whereas they study the impact of replacing a late meal with an earlier (and healthier) one. Moreover, their study takes place in the 1920s in Norway, where the meals were supposed to reduce malnutrition and increase caloric intake.

2.3 Mechanisms

Nutritious school lunches in primary school can affect later life outcomes through multiple channels. First, the quality of the meals can have a direct impact on children's health and growth. Nutrition is believed to be the most important environmental factor affecting height and affects growth more in the postnatal period than in the prenatal period (Silventoinen, 2003). The single most important nutrient for height growth is protein, followed by minerals and vitamins A and D, and a recent study by Grasgruber et al. (2014) found that variation in the consumption of high-quality proteins from milk, pork, fish, and wheat explained most of the variation in height across 45 European countries. Vitamin D deficiency affects the mineralization of bones since vitamin D is important for the absorption of calcium, and this problem is more severe in northern regions due to a shorter light period in winter time (Silventoinen 2003). In addition, a number of studies have shown that calcium, phosphorus,

[11] Kremer (2005) analyze a program in Kenya that randomized access to subsidized breakfasts in preschools. The results showed positive effects on school attendance and test scores.

magnesium, zinc and iron are of importance for human growth (Allen 1994; Prentice & Bates 1994).[12] Interestingly, the Swedish school lunch program aimed at increasing the intake of exactly these nutrients and in our empirical analysis we will therefore focus on height as an important marker for health and improved nutrition but also analyze other important health outcomes.

A poor diet can also lead to overweight and obesity with related problems such as high blood pressure, diabetes, heart disease, and risk of stroke (see for example Hoynes & Schanzenbach (2015)). While school meals can be a remedy for poor diet, they may also contribute to overweight and obesity, as discussed above, and in our analysis we will therefore study the effect of the Swedish school lunch program on measures of body size.

Second, poor nutrition in early life can directly affect the formation of cognitive skills and educational attainment. Deficiencies in zinc, iodine, iron and folate have been linked to worse cognitive development of school-aged children in developed countries (Pollitt & Gorman 1994; Delange 2000; Bryan et al. 2004; Feyrer et al. 2017).[13] School lunches may also affect educational attainment and test scores through making pupils more attentive and raising their energy level. Figlio & Winicki (2005) found that schools at risk of sanctions if students under-performed in tests and that served meals richer in carbs on the days of the tests improved their scores in several subjects, a finding that is also supported in experimental research (Wyon & Abrahamsson 1997).

Third, free school meals can affect outcomes through mechanisms not directly related to nutrition. The provision of free meals can make it more attractive for some students to attend school and lead pupils to attend more classes (Hinrichs 2010). Such a mechanism is unlikely to be of importance in our context since primary school was mandatory and since rates of non-attendance were low already before the program was introduced.[14]

Finally, the introduction of free school lunches can affect household income since food expenses are reduced and since more mothers were able to enter the labor market. The latter effect may have arised because mothers did no longer have to be at home and cook during the lunch break. To the extent that increases in income benefit the children in the household, as suggested in the study by Dahl & Lochner (2012), this represents another mechanism through which long-term outcomes may be affected. In our empirical analysis we will address this possibility by estimating the effect of the school lunch program within families. If increased household income is an important mechanism, we expect to see much smaller effects within families since all children should gain from such income increases.

[12]While healthy school meals can offer a remedy through a more balanced diet, they could in principle also worsen things through a mismatch between early and late nutritional supply. If the supply of nutrients during the pre-natal or early post-natal is scarce, the body predicts that the future will also be nutritionally deprived and invokes biological mechanisms to adapt to the future environment (Barker 1997; Gluckman & Hanson 2005). This mechanism is referred to as the Barker hypothesis and predicts that when nutrients are plentiful in later period, but not in an earlier period, the early metabolic adaptions are a bad fit for the later environment and the risk of metabolic disorders increases.

[13]See Gómez-Pinilla (2008) for a review on some of the biological mechanisms that link diet quality and nutrition to cognitive outcomes.

[14]We show that this is the case in Section 7.7.4.

3 The Swedish school lunch reform

We next provide an overview of the Swedish school lunch program. In 1946, the Swedish social democratic government signed a proclamation, Författningssamling (1946), guaranteeing comprehensive state subsidies for all municipalities that introduced universal and free school lunches in primary school.[15] The motivation for this government program came from a number of studies on the nutritional standards of the food consumed by school children. Surveys showed that breakfast for most children in primary school comprised coffee, tea, or hot chocolate, in combination with white bread (SOU 1945). Only one third of the children received porridge or "välling" (a milk-based cereal hot drink). The surveys revealed that 50 percent of the children did not receive any hot meal (other than porridge, etc.) until the evening. There were also concerns about the fact that one quarter of the children were not able to eat their lunch at home because of long travelling distances and instead had to bring food prepared at home to their school. Researchers analyzing the nutritional content of the food brought from home, as well as the food consumed at home during lunch, concluded that there were serious problems with the content, which negatively affected almost all school children and, in particular, frail and sick children.[16] In particular, the intake of vitamins A, C and D, protein, and certain minerals was perceived as inadequate (SOU 1945). The concerns were only related to the quality of the meal whereas the quantities consumed were believed to be adequate.

At the start of the program, every municipality that informed the regional school board about their intent to serve school meals for free automatically became eligible for the subsidies as long as they complied with the recommended nutritional standards of the meals (Virgin 1970). The government program for providing free and nutritious school lunches included a provision of grants to cover both salary and ingredient expenses and, more important for our period of study, a large institutional "framework" aimed at helping and controlling municipalities to provide the best possible school lunch for the tax money being spent. While the coverage of costs for school lunches by government grants dropped from 70 percent in 1948 to 22 percent in 1955 (SOU 1958) the institutional provision of help and control had increased in a similar magnitude. In SOU (1958), a detailed description is given of this extensive program, hence, providing us with information on the actual treatment occuring.

Important for our study is that the school board was actively taking part in the work of providing lunches.[17] First, the school board collected statistics annually from each municipality on their costs or providing free lunches, on their organization, and their facilities. Second, they provided guidance on how to best organize these school lunches by actively visiting these schools and producing brochures including good examples, including what lunch

[15]Some limited state subsidies were introduced already in 1937 but few municipalities applied for those, and the subsidies were targeted to the areas that were most severely hit by the economic crisis in the 1930s. School lunches were therefore rare before 1946, and in 1940/41, only 13% of primary school pupils were served a school lunch free of charge.

[16]Similar concerns were expressed in the U.S. in the 1940s, where it was questioned whether children would be nourished adequately if they brought a lunch to school or ate lunch at home (Hinrichs 2010).

[17]The school board (Skolöverstyrelsen in Swedish) was a central office for school issues, formed in 1920. The school board was originally responsible for primary schools and educational institutions, but subsequently, it also ruled over elementary and upper secondary schools. The school board was replaced in 1991 by the National Agency of Education.

ingredients to buy locally.[18] Third, in another brochure called "Kosthållet vid skolmåltider", they provided guidelines for the nutritional content of a typical meal being served, with a 3-week menu template being the main feature in these guidelines; see the template of a typical 3-week school lunch menu in Figure A1 in the appendix.[19] However, the nutritional standard of the meal was not for the municipality to decide, and hence, these 3-week menus were more than just guidelines.[20] Fourth, the school board realized that having such high requirements for the local authorities also required a lot of help in terms of providing education in these matters. These education programs involved several levels with initial education programs of kitchen staff lasting up to two months, while continuing education could be weeklong courses. The school board also frequently visited the schools, and at these visits, further education was given through discussion groups etc.[21] To conclude, the overall impact of this extensive government program resulted in practically all schools serving the so called A-meal, i.e., the one being presented in the 3-week menu templates, including a hot meal, sandwiches and milk, by 1955.[22] Hence, there is supporting evidence that practically all students living in a municipality having adopted the free school lunch program were served a nutritious school lunch.

Important for the interpretation of our results (intent to treat effects) is the school lunch participation rate. Early on in the program, schools were required to keep daily track of the number of pupils eating school lunches. However, for the later period of the program, pupil counts were no longer mandatory, since subsidies were much lower, but, among the municipalities that did report such figures, the school lunch participation rate was always above 90 percent. In addition, only approximately 4 percent of the pupils were absent from the school lunch when the program was evaluated in the late 1950s (SOU 1958). These participation rates are remarkably high considering that a low rate of school meal attendance is a common problem when studying the effect of school meals, making our empirical estimates close to being interpreted as treatment on the treated effects (Kristjansson et al. 2007; Bernstein et al. 2004; Evans & Harper 2009).

The government investigations on the quality of meals consumed before the school lunch program led to the conclusion that the school lunch should comprise freshly cooked hot food with an adequate amount of micronutrients, together with milk and bread. Detailed guidelines were provided by the National Medical Board regarding the amount of vitamins A, B, and C, protein, calcium, iron, phosphorus, and egg white, and the A-meal were to provide a third of the daily need of calories (>800 calories), compared to a caloric intake of approximately 500 calories if only eating two cheese sandwiches and drinking 3 dl of milk.[23]

[18] Since salaries of kitchen staff amounted to approximately 1/3 of total costs improving the productivity of producing meals was important; see Table 6 in SOU (1958).

[19] The content of the menus was decided in collaboration with the National Agencies for Medicine and Public Health.

[20] On page 25 in SOU (1958) it is stated that this brochure contains typical menus, which provide a benchmark for the standard of school meals.

[21] To obtain some idea about the scale of these programs it is stated that each year 2-300 kitchen staff were given the education (pp. 24-26, SOU 1958).

[22] Table 10 in SOU 1958 shows that 98% of the students were served an A-meal. The other 2 percent were served a B-meal (Oslo breakfast), while very few were served a C-meal (porridge).

[23] The guidelines were as follows: protein: 32 grams. Calcium: 0.4 grams. Phosphorus: 0.8 grams. Iron: 7 grams. Vitamin A: 2000 I.E. Vitamin B: 0.5 mg. Vitamin C: 25 mg. Egg white: 65 grams. These amounts correspond to half of the average daily need of vitamins, minerals, and egg white for school-age children (SOU 1945).

These nutritional guidelines were implemented and expressed through the 3-week school lunch menus, often comprising meat-based stews, vegetable-based soups, fish and meat or egg-based dishes, and fruit, berry, or vegetable-based dishes (SOU 1945). With each lunch, 30 cl milk and rye bread with butter were to be served. Hence, one way to think about the change in the lunch at school before and after the reform is as adding a hot nutritious meal to the typical pre-reform milk and sandwich package. The guidelines also included recommendations about maximum fat content, illustrating an early awareness of the risks of being overweight.

Two features of the school lunch program are of particular importance for our empirical strategy. First, when school lunches were introduced in a municipality, it covered all pupils in all grades in primary school. During the time period considered, children started school in the year they turned seven and the number of years that a pupil was exposed to the program therefore depended on the grade the student was in when it was implemented. In our data, we therefore have cohorts that were never exposed to free school lunches, cohorts that received school lunches during their entire primary school period, and cohorts that were partly exposed for one or more years. Second, the school lunch program was introduced gradually across municipalities. In a government report from 1958, it was concluded that lack of proper facilities was the major bottleneck for municipalities, which partly explains the differences in the timing of the uptake (SOU 1958). In section 5, we conduct comparisons between early and late adopters of the school lunch programs to assess whether the different groups faced similar pre-treatment trends.

Finally, an additional motivation behind the introduction of school lunches was to ease the household work burden for women and to encourage females to enter the labor market. It was believed that the provision of school lunches would be an important policy in this regard, since women would no longer need to stay at home during daytime to cook lunch (SOU 1945). In the empirical analysis we address this potential mechanism.

3.1 Sweden during the 1960s and other reforms

To put our results into context, some knowledge about Swedish society in the 1960s is useful. Sweden was not occupied during WWII, and the postwar period was a period of strong economic growth and a strong expansion of the welfare state. It is important to understand that hunger and malnutrition were not of great concern in the 1950s and 1960s, when the school lunch program was introduced. The fraction of children growing up in poor households during the 1960s was not very different from later rates and in 1968, for instance, approximately 18 percent of children were growing up in liquidity constrained households, which was similar to the rate in 2000 (Jonsson & Mood 2013). With a GDP per capita in 1960 of 11,871 USD (in PPP 2005 USD), Sweden belonged to the 5 richest countries in the world.[24] The environment in which the school lunches were served was thus very different from the situation in the 1920s and 1930s, when the limited school meal programs in place were targeted towards poor regions where malnutrition and food shortages were real concerns. During this time period, there were also no concerns about being overweight or about obesity in the Swedish society. From the military enlistment records, we know

[24]The corresponding GDP per capital in the U.S. was 15,644 USD.

that only approximately 1 percent of men born during the 1950s and 1960s were classified as obese at age 18 (Neovius et al. 2008).

Another large reform that occured in Sweden during the 1950s and 1960s was the compulsory schooling reform. The reform increased the mandatory years of primary schooling from 7 to 9 years and was rolled out across municipalities between 1949 and 1969.[25] In our regressions, we include indicators of exposure to the schooling reform to not confound any effect of school lunches with those of the reform. Most municipalities had already introduced the schooling reform by 1959, and our results are not affected by controlling for reform exposure or by dropping municipalities that had not yet introduced the schooling reform. In addition to the schooling reform, there were no other reforms that were rolled out gradually across municipalities during the same time period.

4 Data

4.1 Exposure data from archives

We collected and digitized information on the introduction of school lunches across municipalities from the Swedish National Archive. In the archive, the information was kept on paper forms, where each municipality, for each year, reported whether they served school meals, the number of pupils served, and their costs for the school meals. In January 1960, Sweden had 1031 municipalities, and we managed to collect data on 1004 of them.[26]

In our empirical analyses, we focus on the municipalities that introduced the school lunch programs between 1959 and 1969, in total 265 municipalities. The reason for focusing on this period is that the National Archive does not hold forms sent by the municipalities in earlier years. Before 1959, the records were kept by the National School Board ("Skolöverstyrelsen") but the records from this earlier period were thrown away more than 20 years ago.

In Figure 1, we illustrate how the school lunch program was introduced between 1959 and 1969. We see that the number of municipalities that introduced the program gradually increases during the period, with approximately 25 municipalities added per year on average. In Section 5 below, we conduct some comparisons on the characteristics of the early, mid, and late adopting municipalities and with municipalities that already had introduced the program by 1959. As it turns out, we find little evidence of any systematic patterns in the timing of the introduction of the program across municipalities.

In 1959, when our data series starts, approximately 750 municipalities had already introduced free school meals while 281 had not (of which we have information on 265). We lack information on which year these 750 municipalities introduced their school lunches but we do know that individuals in these municipalities who started school in fall 1959 or later must have been exposed to school lunches during their entire school period. Individuals born 1952 and onwards from these municipalities, who entered the first grade at age 7, are therefore included in our data set and empirical analyses. The treatment for those pupils is

[25] See Lundborg et al. (2014b) and Holmlund (2008) for a description of the reform.

[26] Twenty-one municipalities lacked an introduction date as of 1969. Some of them are likely to have introduced school meals earlier but failed to report it properly.

equal to the number of years they spent in primary school, i.e., 7 or 9 years, depending on whether they were exposed to the 9-year compulsory schooling reform.

For the remaining municipalities that introduced the school meal program between 1959 and 1969, we include individuals born between 1942 and 1965. Their exposure to school meals differs according to which year they started school, which year the municipality started serving free school meals, and which year their municipality of residence introduced the compulsory school reform. Among these individuals, there will be individuals with between zero and nine years of exposure. Table 1 shows the distribution of school lunch exposure in our sample. Here, we see that 83 percent of our sample was exposed to school lunches for 9 years and 8 percent were never exposed. The fractions that were exposed between 1 and 8 years are much smaller since there will only be one cohort in each municipality that will be partially exposed for a certain number of years. For instance, only those who were in grade 6 when the school lunch program was introduced will have been exposed for 4 years. The fraction exposed for 7 years is larger, at 4 percent, reflecting that certain cohorts went through the "old" 7-year primary school.

4.2 Register data and census data

In our empirical analyses, we employ data from a number of administrative registers that have been linked to census data and the historical data on school lunch exposure. Our starting point is the Register of the Total Population (RTB), where we select every Swedish citizen born between 1942 and 1965. We obtain information on municipality of residence during school age from the censuses in 1960 and 1965. For those born between 1942 and 1954, we use the 1960 census to determine their municipality. They will then be a maximum 18 years of age, and we know from previous studies that most individuals aged 18 and below lived in the same municipality as that in which they went to school (Holmlund 2008). For cohorts born after 1954, we obtain the information on municipality of residence from the 1965 census.

We obtain data on income from the income and taxation register (IoT). Our main income measure includes labor market earnings plus all taxable benefits such as unemployment benefits, sickness pay and welfare pay. The records start in 1968 and end in 2011 and are present at the yearly level. These records come from the equivalent of W2 records in the United States and are reported by employers to the tax authorities. To focus on long-term outcomes, we construct a measure of lifetime earnings, defined as the mean of yearly incomes between 1968 and 2011.[27] In our sensitivity analysis we will also test the robustness of our results to the exclusion of taxable benefits. The income measures are also used to create indicators of labor market participation of parents, defined as having any labor income a given year.

For data on educational outcomes, we use the education register (utbildningsregistret, UREG). These data are reported directly from educational institutions in Sweden and are available from 1990 and onwards. We use the latest available measure in the data and impute years of schooling from the highest obtained degree reported in the register. We also consider alternative education margins, such as transitioning into secondary and tertiary

[27]The year 1968 is the earliest year for which we observe earnings data in the income register.

education. To measure parents' schooling, we use the 1970 census with self-reported level of education since the education register lacks reliable data for some of the older generations.

We obtain additional outcome measures for males from the Swedish military enlistment register. The register includes data on test results at the military enlistment, which occurs for males the year they turn 18. The register includes information from tests on cognitive skills as well as data from physical measurements, such as height and weight, and overall health assessments. The data exist for cohorts born 1951 and onwards, meaning that there are few cohorts that are entirely unexposed to the school lunch program and the analyses on this sample will therefore not include any municipality-specific pretreatment trends.

The cognitive ability test is a traditional IQ test, similar in style to the AFQT in the US. The test includes four sub-tests, Instructions, Synonyms, Metal Folding, and Technical Comprehension. The cognitive skill variable that we use in our analyses is a standardized version of the composite cognitive measure calculated by the military enlistment service. Previous studies have established that the measure of cognitive skills is a strong predictor of adult earnings (see, for example, Lundborg et al. (2014b)).

To measure the overall health of males we use data from the health examination at the military enlistment. Each male was given a score on a 13-step scale ranging from more or less perfect health, which is necessary for "high mobility positions" (such as light infantry or pilot) to very poor health, meaning that the individual is not allowed to undergo military training. The score is based on the health examination and on doctor certificates proving the existence of any health conditions. In our analyses, we use a binary indicator of being in more or less perfect health as an outcome. In some analyses, we also use data on diagnoses recorded at enlistment.

To study height, weight, and smoking behavior among women we use data from the Swedish Medical Birth Register, covering all births in Sweden. The same register is used to study outcomes of the children of the mothers, including the birth weight of babies. Note that the register only covers women in our sample that gave birth from 1973 and onwards, and we will in our empirical analysis therefore test how sensitive our results are to this potential source of sample selection.

Finally, we measure long-term health outcomes through data from the national hospital register and cause-of-death register. The former register covers all hospitalizations in Swedish hospitals together with diagnoses, and we use data from 1995 to 2013 while the latter covers all deaths, together with causes, from 1968 to 2013.

4.3 Selection of data and descriptive statistics

Selecting individuals born between 1942 and 1965 and who were registered in the census of 1960 if born in 1954 or before and in the census of 1965 if born in 1955 or after, leaves us with a population of 2,177,167 individuals. Since we include individuals born between 1942 and 1965 in municipalities that introduced the school lunches in the period 1959-1969, and individuals born between 1951 and 1965 in municipalities that already had introduced the lunches in 1959, this further limits our sample to 1,073,451 individuals.

In our regressions, the number of observations varies somewhat across regression as the fraction of missing data varies across outcomes. In Table 2, we show descriptive statistics on the outcomes studied and on the background characteristics used in the regressions.

5 Empirical method

The timing of the implementation of the school lunch program across municipalities was not random and depended, as discussed above, on the availability of kitchens and staff. We address the non-randomness of reform exposure by exploiting a difference-in-difference (DiD) design. The identifying assumption is that conditional on municipality and birth cohort fixed effects, school lunch exposure is as good as random. If our identifying assumption holds, we obtain unbiased estimates of the effect of school lunch exposure on the outcomes studied.

We employ two different DiD specifications in our empirical analyses. Our first specification specifies the treatment variable as a linear years-of-exposure variable and can be written as:

$$y_{itm} = \beta_1 S_i + \beta_2 M_i + \beta_3 N_i + \beta_4 CSR_i + \lambda_t + \alpha_m + \epsilon_i \tag{1}$$

where y_{itm} is the outcome of interest, such as schooling or earnings of an individual i at time t in municipality m, S indicates the number of years for which the individual was exposed to school lunches, M is an indicator of being male, N an indicator of being born in Sweden, λ_t denotes birth cohort fixed effects, and α_m denotes municipality fixed effects. Since the compulsory school reform that increased mandatory years of schooling from 7 to 9 was also implemented during the study period we also include a variable, CSR, indicating whether the individual was exposed to the compulsory schooling reform.

Our second specification is more flexible in that we include 8 dummy variables, one for each number of years of exposure. The reference group in this specification is those who were never exposed to school lunches in primary school. This specification allows for non-linearities in the effect of treatment intensity and, in addition, it allows us to illustrate the point estimates graphically.

In our analyses, we also estimate models where we control for municipality-specific linear or quadratic time trends. For the trends to reflect pretreatment trends, we estimate them on a sample of individuals born between 1933 and 1941, who would have finished primary school before 1959, and who were thus not affected by the school lunch reform.[28] Using this sample, we run regressions on the different outcomes studied as a function of year of birth, municipality, and linear or quadratic municipality-specific trends. We then predict pre-reform trends for our main sample and include them as control variables in our DiD regressions.[29]

5.1 Internal and external validity

An important assumption in any DiD-analysis is that pretreatment trends in the outcomes studied are parallel across treatment and control groups. We can provide direct evidence on the validity of this assumption by comparing pre-trends in outcomes between early, mid, and late adopting municipalities of the reform. At any point in time, the control

[28] It is particularly important in our context that the trends reflect pre-treatment trends since it would be easy to confuse any gradually increasing effect for cohorts that were increasingly exposed to the reform with post-treatment municipality-specific trends. Recall that the oldest cohort exposed to the reform in a given municipality was exposed for only one year, the next-to-oldest for two years, etc.

[29] For a discussion about this approach to control for trends, see, for example, Wolfers (2006), Holmlund (2008), and Lee & Solon (2011).

group will comprise individuals in municipalities that did not yet implement the reform, and it is therefore of importance that individuals in early, mid, and late adopting municipalities face similar pretreatment trends. In Panels A to C of Figure 2, we show pre and post-treatment cohort trends in adult income and education where we have divided the sample to individuals in early, mid, and late adopting municipalities. Those are defined as municipalities that introduced school lunches in 1959-1962, 1963-1965, and 1966-1969, respectively. In the same graphs, we plot the trends for municipalities that already introduced the reform by 1959 to shed light on the representativeness of the municipalities that introduced the reform between 1959 and 1969. The x-axis represents birth cohorts.

To measure income at the same age for all cohorts when constructing these plots, we focus on mean income during ages 35-45. The graphs in Panels A-C illustrate two important things. First, and focusing on the unexposed cohorts born prior to 1952, we see that the absolute levels of income and education are strikingly similar between early, mid, and late adopting municipalities and municipalities that already implemented the program. This suggests that there was no strong selection in the timing of the implementation of the reform with respect to these factors, reducing concerns about external validity. Second, the trends are similar across the four groups, suggesting that the parallel trend assumption is supported in our context.

In Panel D of Figure 2, we show trends in average municipality income for early, mid, and late adopting municipalities by *year*. Here, our data goes back to 1952, which means that we can study trends in municipality characteristics for 7 years before the first cohort was exposed in our sample. Trends and absolute levels are strikingly similar when we study average income per capita.[30]

In the robustness section below, we provide further evidence on the validity of the parallel trend assumption by performing an event study analysis on the effect of reform exposure on earnings.[31] Morever, we test for a number of additional threats to our DiD design. One such threat would be parents moving with their children across municipalities in response to the introduction of the lunch program, and in the robustness section, we check for any evidence of systematic moving patterns.

6 Results

We begin our empirical analysis by estimating the long-term economic impact of the school lunch program. We then turn to potential mechanisms in Section 7 and to intergenerational effects in Section 8.

6.1 Lifetime income

Table 3 shows estimates of the long-term effect of the school lunch program on adult income. Panel A shows results from the specification that enters the number of years of school lunch exposure linearly whereas the specification in Panel B instead uses a set

[30]We have also looked at trends in average real estate value, and the findings are similar (available on request).

[31]We have also run placebo-like DiD regressions where we regress the number of years of school lunch exposure on parents' years of schooling. Taking the average of the parents' schooling in the census of 1970, which is the earliest year for which we have data on parental schooling, we obtain a small and insignificant estimate of -0.00008 for the effect of parents' schooling on their children's number of years of school lunch exposure. These results are available on request.

of dummies to capture any non-linearities in the effects. Such non-linearities can arise, for instance, if the effect of school lunch exposure strongly depends on at what age the exposure starts. Recall, however, that we cannot distinguish the effect of treatment intensity from that of age at first exposure to school lunches, as earlier exposure always means greater cumulative exposure to school lunches. This arises from the feature of the program where the treatment stays on once it starts.[32] All specifications control for municipality fixed effects, cohort fixed effects, whether born in Sweden, gender, and schooling reform exposure. In addition, we show specifications that include municipality-specific linear and quadratic trends (columns 2 and 3).

The results in column 1 of Panel A show that one additional year of exposure to the school lunch program increases adult income by 0.35 percent. If we extrapolate this effect to 9 years of exposure, we obtain an effect of approximately 3 percent for those who were exposed during their entire primary school period. The estimate is robust to controlling for linear and quadratic municipality specific trends, as shown in columns 2 and 3.

We next run regressions where we include a full set of dummy variables for each number of years of exposure, with zero years being the reference category. The results are shown in Panel B, where the dummy indicating 9 years of exposure is significant and positive, suggesting that exposure during the entire primary school period is associated with almost 3 percent higher adult income. This estimate comes close to the implied effect of 9 years of exposure that we obtained in Panel A. Exposure to school lunches for between 6 to 8 years also significantly raise income, but the effects decline in magnitude and significance as the number of years of exposure decline. For those who were exposed only between 1 and 5 years, the estimates are insignificant and much smaller in magnitude. The pattern in the point estimates suggests a clear dose-response relationship where greater exposure is related to greater effects, consistent with previous findings where earlier exposure to a positive intervention have greater effects than later exposure (van den Berg et al. 2014; Chetty et al. 2016).

To illustrate the dose-response relationship visually, we plot the point estimates in Figure 3. Here, the point estimates are close to zero for those exposed for between 1 and 3 years, corresponding to a onset of treatment at ages 13-15. The estimates start increasing thereafter but the largest effects are seen for those exposed for 7 to 9 years, corresponding to treatment starting at ages 7 to 9.

While the estimates in Table 3 are intention-to-treat estimates, they likely come close to average treatment effects, as the participation rate in the school lunch program was above 90 percent. The high participation rate of the Swedish school lunch program is an interesting feature of the reform and makes it different from other programs, such as the U.S. food stamp program, where the participation rate was approximately 43 percent (Hoynes et al. 2016).[33]

The increase in earnings by 3 percent for those fully exposed to the program can be regarded as a substantial effect. We can relate it to estimates of the returns to schooling in

[32]The school lunch program shares this feature with other social programs, such as the food stamp program and the Moving to Opportunity Program (Hoynes et al., 2016; Chetty et al., 2016).

[33]Since we lack data on individual participation in the program, we are unable to provide instrumental variables estimates of the effect of participation in the school lunch program on earnings.

Sweden (in terms of lifetime income). Studies by Black et al. (2015) and Lundborg et al. (2014a), for instance, provide such estimates, where the effect of one additional year of schooling amounted to 4-5 percent greater earnings. The effect of 9 years of school lunch exposure on earnings thus corresponds to the effect of an additional two thirds of a year of schooling.

Since there are no other studies that estimate the long-term effect of access to school lunches on adult income in developed countries, it is difficult to compare our findings to other studies. The size of the effect is in the same ballpark, however, to the estimated effect of access to school breakfast in Norway, where the long-term effect on earnings was estimated to 2-4 percent (Bütikofer et al. 2016). We can also relate our finding to other school-based interventions; Fredriksson et al. (2013), for instance, find that a one-pupil reduction in class size in Swedish schools increases adult earnings by 1.2 percent, suggesting that the effect of school lunches corresponds to that of reducing class size by 2.5 pupils.

6.2 Timing of school lunch exposure

As explained above, our estimates capture the combined influences of length of exposure to school lunches and age at onset. An alternative specification of our empirical model is to nonparametrically estimate the effect of age at onset such as done in Hoynes et al. (2016). For this purpose, we re-run our main DiD-specification and include dummies indicating the individual's age at which school lunches were introduced. The omitted reference category is being above the school leaving age when school lunches were introduced in one's municipality.

Figure 4 plots the estimates from the age-at-onset regression. The effects drastically declines when moving beyond age 9, and it appears critical to receive the school lunch at ages 7-9, i.e., an early onset. Beyond these ages, there is a minimal effect of obtaining access to school lunches. The effects are thus similar to what we found above, where the greatest effects were obtained for those exposed to school lunches for 7 to 9 years. This is unsurprising since the age-at-onset effects are almost a mirror of the length-of-exposure effects.[34] Although we are unable to distinguish the effect of age at onset from that of length of exposure, we are still able to conclude from our results that the school lunch program produces larger benefits for children who were young when first exposed.

6.3 Heterogeneity by parental income

So far, we have focused on the mean impact of the school lunch program and we next move on to examine whether some pupils were affected more than others. Policy-makers at the time were particularly concerned about the nutritional intake of poor and sick pupils, and we therefore next investigate whether there is important heterogeneity in the effect of the program by parental income. Moreover, we investigate whether there are differential effects by gender since some previous studies suggest gender differences in the effect of school lunch programs (Hinrichs 2010).

In columns 1 to 4 of Table 4, we show the effect of school meal exposure on earnings

[34] They do not exactly mirror each other, however, since individuals with the same number of years of exposure can have different age-at-onsets in the new and old school systems. A person who has 7 years of exposure in the 9-year school was exposed to school lunches from grade 3 and onwards whereas 7 years of exposure in the 7-year school system meant having school lunches from grade 1 and onwards.

by parental (mean household) income quartiles.[35] Children of parents who belonged to the lowest quartile of the income distribution (column 1) were affected to a greater extent than other children. The point estimate, 0.0057, is more than double in size compared to the estimates obtained for children whose parents belonged to the second and third quartiles of the income distribution. The lowest quartile also maps onto the definition of relative income poverty, amounting to having a disposable income less than 50% (OECD) or 60% (EU) of the median (Jonsson & Mood 2013).[36] At the fourth quartile of the parental income distribution, the point estimate is much smaller and statistically insignificant. Children from low-income families thus seemed to gain more from the school lunch program, which may reflect that increase in school lunch quality was greatest for the poorest children.[37]

In columns 5 and 6, we turn to results by gender and find that the effect of school lunches on earnings is similar across men and women.

7 Mechanisms

We go on to examine the effect of the school lunch program on a number of intermediate outcomes, with the aim of shedding light on potential mechanisms behind our estimated income effects. We first focus on short-term outcomes measured for both males and females including height, body mass index (BMI), and overweight. For males, these outcomes are measured at age 18 during the military enlistment for cohorts born between 1951 and 1960, as no digitized records exist for earlier cohorts. For females, we can study height and obesity through information taken from the medical birth register from 1973 and onwards. In addition, we observe the outcome of cognitive skills tests and health assessments for males but not for females.

The data obtained from the military enlistment and from the medical birth register restrict our analysis in certain ways. First, we cannot include municipality-specific trends in the regressions since almost all cohorts are exposed to some extent and since we cannot predict pre-reform trends using unexposed cohorts. We take some comfort in the fact that our estimated effects of the school lunch program on income were insensitive to the addition of linear and quadratic municipality-specific trends. Second, we only observe height and weight for females who had a birth in 1973 or later. We thus lose those who had all their births prior to 1973 and those who never had children and, below, we therefore analyze to what extent school lunch exposure affects the likelihood of being observed in the medical birth register.

7.1 Height, BMI, and overweight/obesity

Table 5 shows estimates of the effect of years of exposure to school lunches on height, BMI, and overweight/obesity.[38] The latter two outcomes are motivated by U.S. studies,

[35] We measure parental income in 1968 when approximately 70 percent of the children were still in school.

[36] The lower threshold using 50% of the median is 85,000 SEK, while the larger threshold (60%) is 102,000 SEK. The threshold for the lowest quartile in our data is 93,000 SEK. Using the 60% threshold we obtain a significant point estimate of 0.0063.

[37] We obtain less strong evidence of heterogeneity in the effects by parental education. The point estimate for those with low-educated parents (both parents having only primary school education) is similar to that of those with more educated parents, although it is significant only in the former case. The similarity can be explained by the relatively compressed wage distribution for the parent generation in our data.

[38] Those with a BMI above 25 are defined as overweight or obese.

where the results suggest that exposure to school lunches increases the probability of being obese (Millimet et al. 2010; Schanzenbach 2009). Including these outcomes also allows us to measure whether the school lunch program affected the amount of calories consumed, in addition to nutrition.

In column 1 of Table 5, we see that school lunch exposure has a positive and significant effect on male's height where one additional year of exposure increases height by 0.07 cm. If we extrapolate this effect to 9 years of exposure, we obtain an implied increase in height of approximately 0.6 centimeters. This is a large effect since it corresponds to 6-7 years of secular height growth in Sweden during the 1940s and 1950s (Werner 2007). We can also relate this finding to estimates of the (male) height premium in Sweden, where a 0.6 centimeter growth in height would imply 0.4 percent greater earnings, suggesting that the increased height following the school lunch program could explain approximately 15 percent of the adult income effect (Lundborg et al. 2014b).

In Panel B, we see some evidence of non-linearities in the height effect, where the magnitude jumps and becomes statistically significant at the 5 percent level for those with 4 or more years of exposure. In magnitude, the greatest effect is obtained for those with 9 years of exposure, where the effect is to increase height by 0.8 cm. In Panel A of Figure 5, we illustrate these patterns graphically.

In column 2, we show the results for females based on height data from the birth records.[39] Similar in magnitude to the effect among males, each year of school lunch exposure adds 0.05 cm of height. We see some evidence of non-linearities in Panel B, also illustrated in Panel B of Figure 5, where those exposed for 9 years gain 0.65 cm of height. This is a somewhat smaller effect than the one we obtained for males, but the relative effect is larger given that females are shorter on average.

We can relate the findings for height to the content of the school lunch program, where an important aim of the lunches was to increase protein and vitamin intake. Protein is believed to be an important input in height growth, and a recent study relates the variation in height across European countries largely to variation in protein intake (Grasgruber et al. 2014).

In columns 3 to 6, we show the effect of school lunches on BMI and overweight/obesity, defined as having a BMI above 25. The results do not provide any evidence that the Swedish school lunch program increased the risk of being overweight or obese, and the point estimates are in most cases close to zero.[40] Moreover, we obtain no evidence of any non-linearities in the effect, as demonstrated in Panels C to F in Figure 5.[41]

Our results on body size differ from results obtained in several U.S-based studies, and one likely reason for the divergence in results is that the Swedish program followed strict

[39]For females who we observe several times in the birth register, we take the average of their height measures, and each woman appears only once in the regressions. Since birth records on height are available from 1973 and onwards, we observe height for only 30 percent of the female sample. When we a run regression on the effect of school lunch exposure on the probability of observing height we obtain a small but significant and positive effect of 0.002, suggesting that 9 years of exposure increase the chance of observing height by 2 percentage points from a baseline of 30 percent. We should therefore view the height results for females with some caution.

[40]For women who are observed multiple times in the medical birth register we use their average BMI as an outcome.

[41]We have also studied the effect of school lunches on the probability of being underweight (BMI<18.5). We find small and insignificant effects (results available on request).

guidelines regarding nutritional content and fat content.

7.2 Health assessment at military enlistment

We next study the effect of school lunches on the assessment of overall health for males, measured during the military enlistment. The outcome is a dummy indicating having near perfect health in relation to typical tasks performed in the army. The results in column 1 of Table 6 show that school lunch exposure improves health, where those exposed for 9 years have a 4 percentage points greater likelihood of being assessed in near perfect health, corresponding to a 6 percent increase at the mean. In Panel A of Figure 6, we see that the shape of the relationship between school lunch exposure and health resembles the corresponding one between school lunch exposure and height, providing additional support that nutritious school lunches had positive effects on health, at least in the short run.

The military also records specific diagnoses at the enlistment, although there is limited variation in the data since 18-year-old males are generally in good health and since the unhealthiest males do not enlist (Carlsson et al. 2015). In Table A1 in the appendix, we study some diagnoses that could potentially be linked to improved nutrition and that may map into the overall health assessment: Type II diabetes, conditions of the digestive system, and visual impairment. While the incidence of diabetes is below 1 percent in the sample, 10 percent are classified as having a condition of the digestive system and 2 percent as having a visual impairment. Moreover, we study the effect of the probability of having any diagnosis and on the probability of having a congenital disorder (birth defect), where the latter should not be affected and thus acts as a placebo outcome.

In column 1 of Table A1, we see that school lunch exposure decreases the likelihood of having any diagnosis. We also find that school lunches decrease the risk of being diagnosed with diabetes at age 18, although it should be noted that the estimate is significant only at the 10 percent level. For digestive conditions and visual impairment, we also find beneficial effects of school lunches, and this time the estimates are significant at the 1 percent level. Finally, as our placebo test, column 6 reports that school lunches had no effect on an outcome that should not be affected: congenital disorders.

7.3 Cognitive skills

In column 2 of Table 6, we show results for cognitive skills among males, as measured through the combined score of the cognitive ability tests at military enlistment. There is a positive estimate for years of exposure to school lunches, but it is imprecisely estimated. Panel B, or even clearer in Panel B of Figure 6, also reveals no sign of any positive relationship between years of exposure and cognitive skills. We have also elaborated with splitting the cognitive test score measure into two different types of intelligence, crystallized and fluid intelligence, as defined in Carlsson et al. (2015), without a change in results.

An absence of an effect on cognitive skills might reflect that the school lunch policy was introduced at ages beyond critical development periods for cognitive skills. A large literature suggests that cognitive skills are developed early in life, although the exact critical ages are a matter of debate Heckman (2007).

7.4 Behavioral changes

Another question of interest is whether the school lunch program led to any permanent behavioral changes, such as adopting a healthy lifestyle. As a marker for this, we examine the smoking behavior of mothers, recorded in the medical birth register.[42] Admittedly, smoking does not reflect all aspects of lifestyle, and we lack data on other important markers such as physical exercise, drinking, and adult eating habits. As shown in column 3 of Table 6, we obtain no evidence that school lunches led to any changes in lifestyle, as the effect on smoking was small and insignificant. We analyze another potential behavioral outcome in Section 8, where we look for effects across generations.

7.5 Education

We next turn to educational attainment as a possible mechanism through which exposure to school lunches can lead to higher adult earnings. In column 4 of Table 6, we focus on years of schooling but in Table A2 in the appendix, we also show results for entering university. Since education data are available for our entire sample, and not only for the military enlistment sample, we can study the effects for all cohorts between 1942 and 1965, and for both males and females.

We see that school lunch exposure positively affects schooling, where the effect of one additional year of exposure is to increase years of schooling by 0.03. In Panel B, 9 years of exposure increase years of schooling by 0.28. The estimates are illustrated graphically in Panel C of Figure 6. Given estimates of the returns to schooling in Sweden of 4-5 percent, an effect of 0.28 can explain almost half of the effect of school meal exposure on life-time income.

In column 1 of Table A2 in the appendix, we see that exposure to school lunches also increased the propensity to enter university. Those exposed for 9 years have a 1.5 percentage points greater likelihood of entering university, and the estimates are illustrated in Panel D of Figure 6.[43]

7.6 Long-term health outcomes

We continue by analyzing whether exposure to the school lunch program had long-term effects on health, as measured through adult morbidity and mortality. In column 1 of Table A3 in the appendix, we see that school lunch exposure had only a small and insignificant effect on mortality, measured through the probability of not surviving until 2013, which is the last year for which we have data from the mortality register. The finding is not surprising since the average age in 2013 is 56 in our sample and only 5 percent had died by then. As a measure of morbidity, we examine whether the individual had been hospitalized at any point between 1995 and 2013. The results, shown in column 2 of Table A3, again reveal

[42]The outcome variable takes on the value 1 if the mother is observed to smoke at any time of measurement in the birth register. Smoking status is observed for 32 percent of our women and, again, school lunch exposure is associated with a slight increase in the likelihood of being observed.

[43]In columns 2 to 3 of Appendix Table A2, we show the results for years of schooling and university attendance with controls for municipality-specific trends included. This does not change the results to any important extent.

small and insignificant effects of school lunch exposure. Since we lack data on outpatient care, we are unable to examine less severe health outcomes that do not require hospital care.

In addition, we have analyzed the effects on cause-specific morbidity where we focus on broad categories of diseases that could potentially be linked to school lunches such as cardiovascular disease, cancer, and respiratory disease. As shown in columns 3-6 of Table A3, we do not obtain any significant effects, suggesting that the school lunches did not affect the likelihood of experiencing severe health shocks during the period from young adulthood to middle age.[44]

7.7 Alternative mechanisms

As discussed above, introducing free school lunches can affect the income of the households in two ways. First, since the school lunches were provided free of charge, their introduction meant that parents had to spend less money providing lunches for their children. If some of the money was instead spent on inputs that affected the health and human capital of their children, this may explain part of the positive effect of school lunches. Second, an important goal of the school lunch program was to free women from the task of providing lunches to their school-aged children and thereby increase female labor market participation. If the labor supply effect was substantial, increased household income would be one possible mechanism through which the introduction of school lunches affected school children. Dahl & Lochner (2012), for example, found that income increases among poor families in the U.S. increased their children's math and reading scores. In the following section, we provide evidence on the potential importance of these alternative mechanisms.

7.7.1 How much did households save from free school lunches?

One way to understand the magnitude of any increases in household income coming from reduced food expenditures is to calculate how much a typical household would save. We know from the data collected exactly how much the schools did spend per student. If we assume that the cost per pupil represents an upper bound for what the meals that were previously served at home, or brought to school, would cost, we can make some rough calculations about potential cost savings. The yearly average spending per pupil was 3,730 SEK, which corresponds to approximately 1% of the average income of an household in 1968 (272,000 SEK).[45] We can assess whether a 1 percent increase in household income per child could generate the effects we have estimated above by looking for evidence in studies that have estimated the effect of household income on child outcomes. Dahl & Lochner (2012) used changes in the earned income tax credit program in the U.S. to estimate the effect of family income on child outcomes. They found that a $1000 increase in household income, corresponding to a 3 percent increase at the mean, leads to a 6 percent of a standard deviation increase in math and reading test scores. If we would translate these findings to the Swedish context, where the increase in household income at most amounted to one

[44]We have also analysed the effect on cause-specific morbidity by parental income but find no evidence of heterogeneity.

[45]3,730 SEK corresponds to approximately 420 USD (in 2017).

percent per child, it is doubtful that the savings at the household level would explain any important part of the effects of the school lunch program that we estimate.

7.7.2 Did the free lunches affect maternal labor supply?

We next estimate the effect of the school lunch program on mothers' labor supply in 1970, measured as having any labor income in that year.[46] The results in the fifth column of Table 6 show that having a child fully exposed to the school lunch program increases maternal labor market participation by 2.8 percentage points from a baseline rate of 53 percent.[47] This result shows that one of the aims of the reform, to increase maternal labor supply, was accomplished to some extent.

Among fathers, labor market participation was already high, and since mothers were usually the ones providing lunches at home, we expect less of an effect among fathers. As a placebo test, we therefore run the same analysis on male labor supply where we indeed obtain small and insignificant estimates; see sixth column of Table 6.

7.7.3 Effects within families

Another way to shed some light on the role that increased household income, either through the effect of reduced household expenditures on food or through increased maternal labor supply, play for our estimated school lunch effects is to look at the effect of school lunch exposure within families. That is, we add a mother fixed-effect to our main DiD regression model, meaning that we identify the effect of differential exposure to school lunch exposure across siblings. If the positive effect of school lunches is explained by a positive household income effect, other children in the family might benefit as well.[48] If such an income increase was evenly distributed across the children, all children may benefit to at least some extent, and we would expect a smaller, or non-existent, school lunch effect within families. A smaller effect would also result if there were strong (positive) spillover effects between exposed and unexposed siblings, i.e., if the SUTVA assumption was violated.[49]

The estimate from models that add family fixed effects would still reflect increased household income if older siblings who were not exposed to school lunches had moved out of the household and, thus, were not affected by the income increase. Moreover, siblings unexposed to school lunches but who remained in the household would in any case be exposed to any income increase at the household level for a shorter period of time compared to their younger exposed siblings. To reduce these concerns, we run our family fixed regressions on a sample of closely spaced siblings, born at most 5 years apart.[50] In column 1 of Table 7, we first run our baseline DiD specification on families where the first and second born siblings are born at most 5 years apart in order to assess the external validity of the siblings

[46] Approximately 70 percent of the children in our sample were still in school in 1970.

[47] The results do not change if we add municipality specific trends, as shown in column 4 of Table A2 in the appendix.

[48] A positive household income effect would also arise if local farmers gain from the school lunch program because of an increase in the demand for their products.

[49] Alternatively, there could be knowledge spillover effects in meal provision, so that parents also start serving healthier meals. In such a case, we would expect that siblings unexposed to the school lunch program would also gain.

[50] In these regressions we include families of any size but only use the first and second born children in the estimation. The average birth spacing in this sample is 3.7 years.

sample. The results are similar to our main findings in Table 3. When we add family fixed effects, shown in column 2, the point estimate is similar in magnitude and remains positive and significant. We obtain a similar point estimate when we run the analysis on second and third born children, as shown in column 3, but it is somewhat less precisely measured.

In column 4, we study male height as an outcome using within family variation and restricting the sample to first and second born brothers born at most 5 years apart. The point estimate is similar to the one we obtained without family fixed effects in Table 4 but the estimate is insignificant due to the much smaller sample size. For females, the sample size shrinks substantially, resulting in an insignificant point estimate of 0.033, which is not very different in magnitude from the corresponding estimate we obtained without fixed effects, however. In column 6, we show that the estimate of school lunch exposure on health assessed at military enlistment increases in magnitude when we add family fixed effects and remains significant at the 5 percent level.

Taken together, these results do not suggest that our main estimates are driven by income increases at the household level following from the introduction of school lunches. Any income gains in terms of reduced expenditures on food or from increases in labor supply should benefit all children in the household, at least for closely spaced siblings. The fact that the family fixed estimates are similar to our main estimates also implies that parental reinforcing or compensating behavior appears less important in our context. This differs from Almond et al. (2009), for instance, who found evidence of reinforcing behavior when studying the effect of radioactive fallout in Sweden on child outcomes. One reason for the difference may be that parents in our case were only able to adjust their investments in their children from age 7 and onwards whereas the parents in Almond et al. were able to react to child outcomes already from the birth of the child. If there are critical periods for parental investments, this might thus explain the difference.

7.7.4 Did the lunches affect school attendance?

If children were more likely to attend school after the introduction of free school lunches, this may constitute one mechanism through which long-term income was affected. We consider this to be an unlikely mechanism in our context, as primary school was compulsory and since non-attendance rates were remarkably low in Sweden during the time period studied. Cattan et al. (2017) report that for cohorts born between 1930 and 1935, i.e., before the school lunches were introduced, students in grade 1 missed only 1.7 days on average for reasons other than sickness. The corresponding number in grade 4 was 3.3 days.

Truancy rates were also low in later periods (Jönsson, 1990). In a survey from 1967, truancy was almost non-existent for pupils attending 3rd grade, while increasing to 1 percent in 6th grade and to almost 4 percent in 8th grade. A similar rate, a truancy rate of 3 percent for 6th graders, was found in a study conducted in 1953, where many schools had not yet introduced school lunches. Another study conducted in 1968 found that truancy rates in 8th grade were 3.6 percent for boys and 1.6 percent for girls, see Jönsson (1990) for more details.

The low rates are not surprising given that the Swedish compulsory schooling law strongly prohibits truancy, and if children do not comply, fines will be given to their parents.

Moreover, there was a strong focus on order and conduct in the school system at the time, and at the end of the school year, days of absence for different reasons (e.g., sickness, truancy etc.) during the school year were presented along with the grades in different subjects. It thus appears unlikely that increased attendance plays an important role for our results.

8 Effects across generations

We next analyze whether the school lunch program had effects across generations. Medical evidence suggests that inadequate intake of vitamin D can cause adverse pregnancy outcomes including intrauterine growth restriction and neonatal low birth weight (Pérez-López et al. 2015), and if greater exposure to school lunches meant that food habits were affected permanently, we may see an effect on birth outcomes. For this analysis, we use data from the medical birth register, where we observe the birth weight of children born to mothers in our main sample.[51]

Table 8 shows the results on child birth weight. In column 1, we see that children whose mothers were more exposed to the school lunch program were not different from other children in terms of birth weight. When we focus on indicators of having at least one child born with low or very low birth weight, we obtain negative but insignificant point estimates (column 2 and 3).

9 Robustness analyses

In this section, we present the results of several sensitivity checks. First, we perform an event-study analysis, where we look for the existence of pre-trends in the data that would violate our parallel trend assumption. Second, we test for strategic relocation across municipalities in response to the school lunch program. Third, we check whether our results are robust to the income measure we use and to dropping those that were exposed to the old 7-year primary school system. Finally, we investigate whether our estimated income effects are also obtained on our restricted samples from the military enlistment register and from the medical birth register.

9.1 Event-study results

The key identifying assumption in our DiD specification is that of parallel trends. To shed further light on the validity of this assumption, we next perform an event-study analysis where we use the same specification as in our main analysis but add a number of dummy variables representing cohorts that were not exposed to the school lunch program. If the assumption of parallel trends is valid, we expect that the coefficients of these pretreatment dummies are zero.

[51]For mothers with multiple births, we take the average of the children's birth weight in our regressions so that each mother appears only once in the data. When we study indicators of low birth weight (<2,500 grams) and very low birth weight (<1,500 grams), we use indicators of observing at least one child with low or very low birth weight. We observe the birth weight of at least one child for 40 percent of our women. A (DiD) regression on the likelihood of observing birth weight as a function of school lunch exposure produces a small and insignificant estimate of 0.0009.

We plot the point estimates from the event study regression in Figure 7. In the graph, years of exposure coincide with cohorts. Zero years of exposure correspond to the cohort that had just left school when school lunches were introduced. Minus 1 corresponds to those that left school more than 1 year before the school lunches were introduced, etc. Negative event time thus reflects the cohort distance in years from the first cohort treated with school lunches. In the graph, 1 year of school lunch exposure represents the oldest cohort exposed to school lunches, i.e., those who began the last grade when school lunches were introduced and who, thus, were only exposed for one year. Positive years of exposure thus represent both the intensity of school lunch exposure and the distance in years from the first cohort that was treated with school lunches.

The pattern in the figure shows no evidence of any pre-trends; conditional on common cohort effects and municipality effects, the point estimates of the pre-treatment dummies are all close to zero and insignificant. After treatment sets in, however, we see a gradual increase in the estimates, reflecting the gradual increase in treatment intensity for post-treatment cohorts. These results, together with the ones reported in Section 5, provide evidence on the validity of the parallel trend assumption.

9.2 Strategic relocation

If some parents decide to move with their children in response to the introduction of school lunches in a municipality, our DiD estimates might be biased. Since most individuals in our sample were registered in both the censuses of 1960 and 1965, we can check whether they moved as children and whether such moves influenced their school meal exposure. In particular, we are interested in whether certain types of families moved, and for that purpose, we regress a dummy for moving to, and then a dummy for moving from, a municipality that had introduced the school meal program as a function of parental education and birth year. The results suggest that years of parental schooling significantly affect the probability of moving both to and from a municipality that introduced school lunches. The probability of moving to a municipality that would increase the total number of years that the person had school meals was 0.2 percentage points higher for someone whose parents had the maximum amount of schooling in the sample compared to someone whose parents had the least. The corresponding effect of moving to a municipality that would decrease the number of years of exposure was 0.4 percentage points. The coefficients are in both cases thus minute and the effect works in both directions. Rather than strategic relocations, we therefore conclude that these effects reflect the fact that people with higher education were in general somewhat more mobile.

9.3 Alternative income measures and dropping those in the 7-year school system

In our main analysis, we use a measure of income that includes labor income and all other taxable benefits, such as sickness insurance benefits and unemployment benefits. We next test whether our income results are robust to an alternative measure of income that excludes taxable benefits. This measure captures the "pure" labor income and does not include any compensation paid by the social welfare system, such as unemployment benefits or sickness benefits.[52] If school lunches make some people healthier and more productive in the long run, and thereby reduce sickness insurance take-up, such an effect would to some extent be hidden when we use an income measure that includes sickness insurance benefits. As seen in column 1 of Table 9, the effect of the school lunch program is almost unchanged when we use this alternative measure. This result is in line with our previous results of no long-term health effects of school lunches.

In column 2, we instead use average earnings during ages 35-45 as an outcome, allowing us to measure earnings at the same ages for all cohorts. The effects of school lunch exposure is still positive and significant at the 1 percent level, although the point estimate is smaller in magnitude, suggesting that the full benefit of school lunches has not yet fully materialized at these ages.

In column 3, we show regressions where we drop those that were exposed to the "old" compulsory schooling system with only 7 years of schooling as mandatory. This leaves our estimate of the effect of school lunches practically unchanged.

9.4 Income effects for those in the enlistment register and medical birth register

In our analyses on potential mechanisms, we used data from the military enlistment register and from the medical birth register. We next examine whether the positive effect of school lunch exposure on income that we observed above also exists for the individuals observed in these registers. For the analysis on males, we use register data on all males born between 1951 and 1965. The reason is that we do not have linked income data in the military enlistment register. We have enlistment data for 87 percent of this sample, which explains the smaller number of observations in the regressions on enlistment outcomes. While we cannot observe in the population income register which males actually enlisted, column 4 shows that we obtain similar point estimates for the male cohorts born between 1951 and 1965 as well as for the cohorts born between 1942 and 1965, which we used in our main analysis. We also obtain similar income effects for women for whom we observe height and the birth weight of their children in the medical birth register; see columns 5 and 6.

10 Did the benefits outweigh the costs of the program?

The Swedish school lunch program was extensive, and an important question is whether the benefits of the program exceeded its costs. Since we have detailed data on the costs of the program, we are in a good position to conduct such an analysis.

Our estimates suggest that exposing a child to 9 years of healthy school meals will increase the child's lifetime earnings by approximately SEK 102,000 ($11,700), discounting future earnings at a 3-percent interest rate and counting earnings from age 21 to 65. This is almost four times the total discounted cost of the program, which amounts to SEK 26,900 ($3,080). The benefit-cost ratio increases substantially if we single in on the children in poor families (bottom quartile of household income). For these children, 9 years of exposure increased earnings by 5.45 percent, meaning that the discounted benefits were seven times

[52] Moreover, it does not include income from self-employment.

larger than the discounted costs. One should keep in mind that this simple back-of-the-envelope calculation only counts the income benefits of the school lunch program and that other important benefits are not accounted for.

11 Concluding remarks

In this paper, we provide estimates of the long-term economic benefits of a program that offered free, universal, and nutritious school to children in Swedish primary schools in the 1950s and 1960s. For this purpose, we use historical data on the rollout of the program across Swedish municipalities in the 1960s. We employ a difference-in-differences empirical design, where our identifying assumption is that exposure to school lunches is as good as random, conditional on birth cohort fixed effects and municipality fixed effects. We provide a number of specification checks that support this assumption.

Our results show substantial long-term economic benefits of the program. Those exposed to school lunches during their entire primary school period increased their life-time income by approximately 3 percent. This result is robust to several alternative specifications of the exposure variable and is similar in magnitude for both genders. The results also point to that earlier, and thereby longer, exposure leads to greater effects and that children from poor households gain most from the program, although children from all households except the richest benefit to some extent.

To shed light on the role of nutrition as a mechanism behind the income effect, we turn to data from the military enlistment and from the medical birth register, where we find that exposure to school lunches has large and positive effects on height and overall health. In addition, we obtain substantial effects on educational attainment that could explain half of the school lunch premium.

Our results emphasize the importance of childhood conditions and complement the growing literature that analyses the effect of interventions and circumstances at different phases of childhood. We contribute by analyzing the effect of a policy-driven change in nutrition in the phase between early childhood and adulthood where little evidence on effective policy interventions exist.

Although we study the part of the Swedish school lunch program that was rolled out during the 1950s and 1960s, we believe our results are still relevant for Western countries today. The program was introduced in a relatively wealthy country where school children did not face food insecurity or malnutrition. The school lunches rather changed the nutritional content of the meals and made pupils switch from less nutritious lunch bags or lunches at home. This is of relevance for many countries today that plan to improve, or have improved, the nutritional content of the food served in schools.

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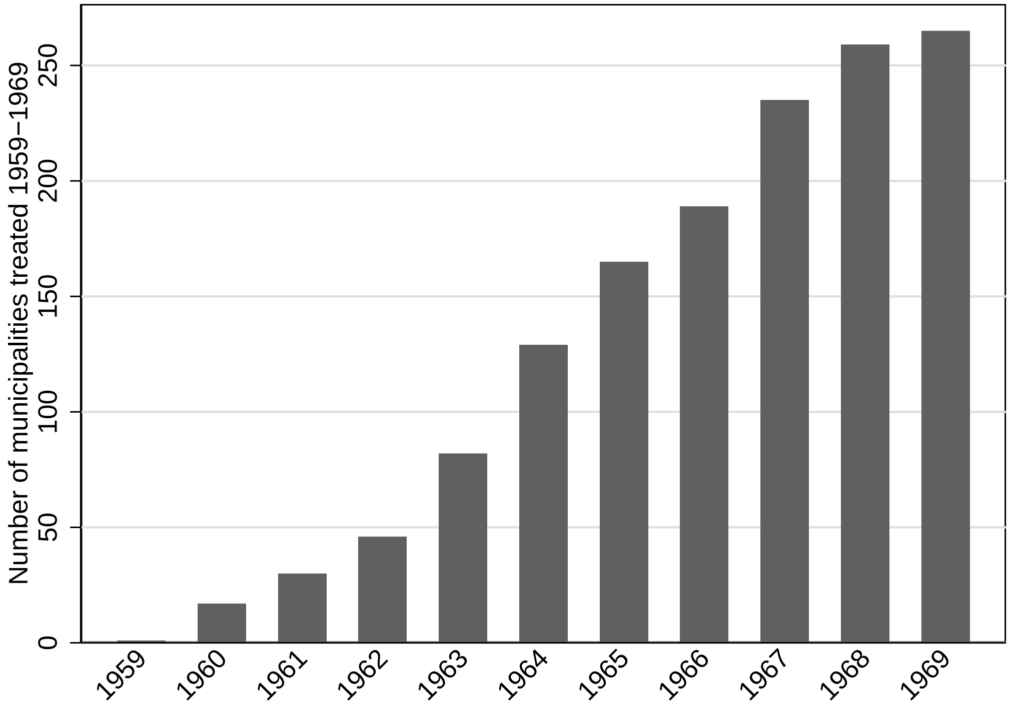
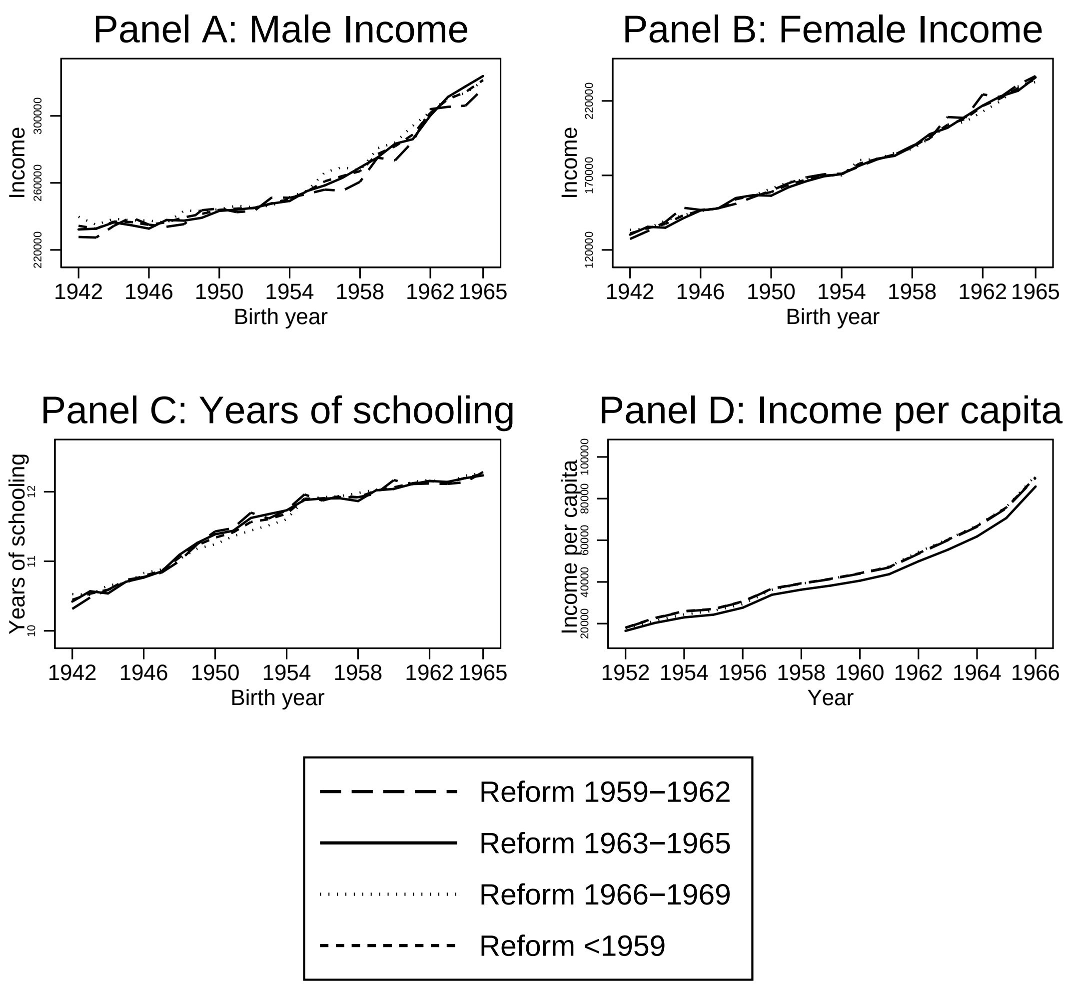
Figure 1: Cumulative number of municipalities introducing the school lunch program between 1959 and 1969.



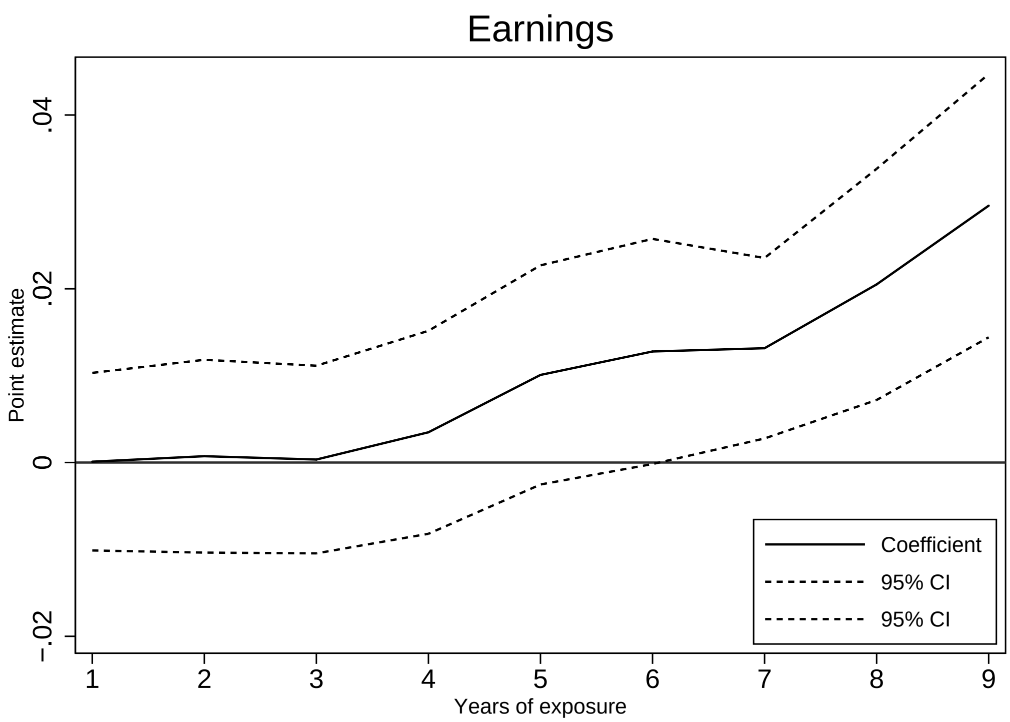
Notes: The figure shows the cumulative number of municipalities that introduced the school lunch program between 1959 and 1969. See text for details.

Figure 2: Trends in income, schooling, and average municipality income for early, mid, and late adopting municipalities, and for municipalities that already had introduced the school lunch program by 1959.



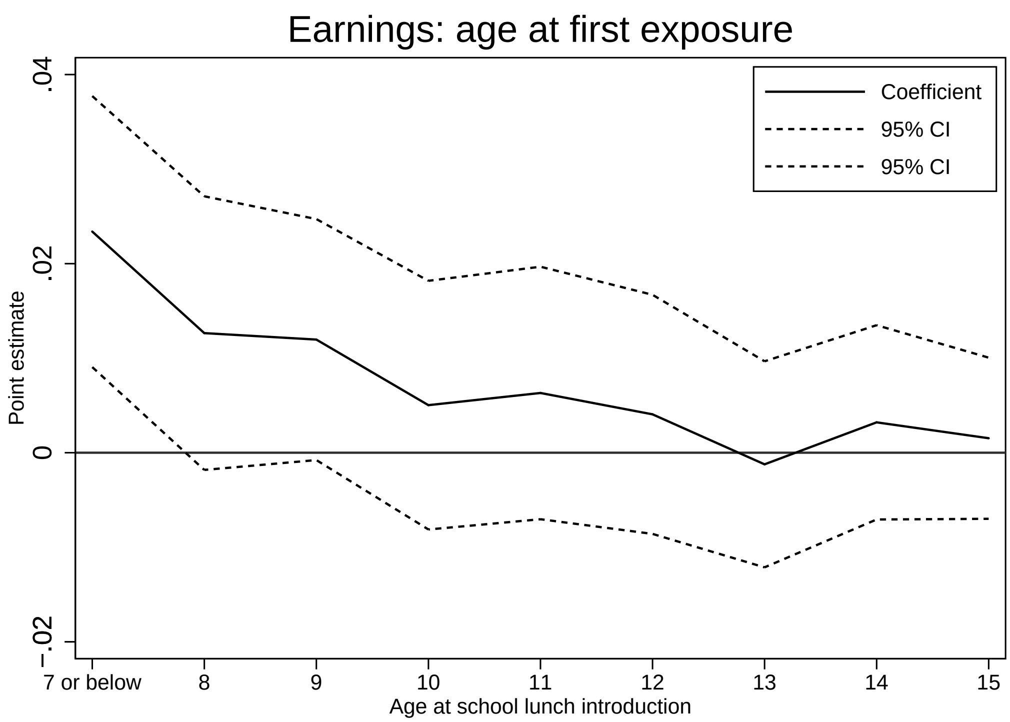
Notes: Panels A-C show average income between ages 35 and 45 and years of schooling for individuals born between 1942 and 1965 in municipalities that introduced the school lunch program between 1959 and 1969 or who had introduced it already by 1959 (reform <1959). The different lines distinguish between individuals in early, mid, and late adopting municipalities of the school lunch program and between individuals in municipalities that already had introduced the program by 1959. Panel D shows average income at the municipality level between 1952 and 1966. Income is measured in 2010 prices.

Figure 3: Effect of years of exposure to school lunches on lifetime income



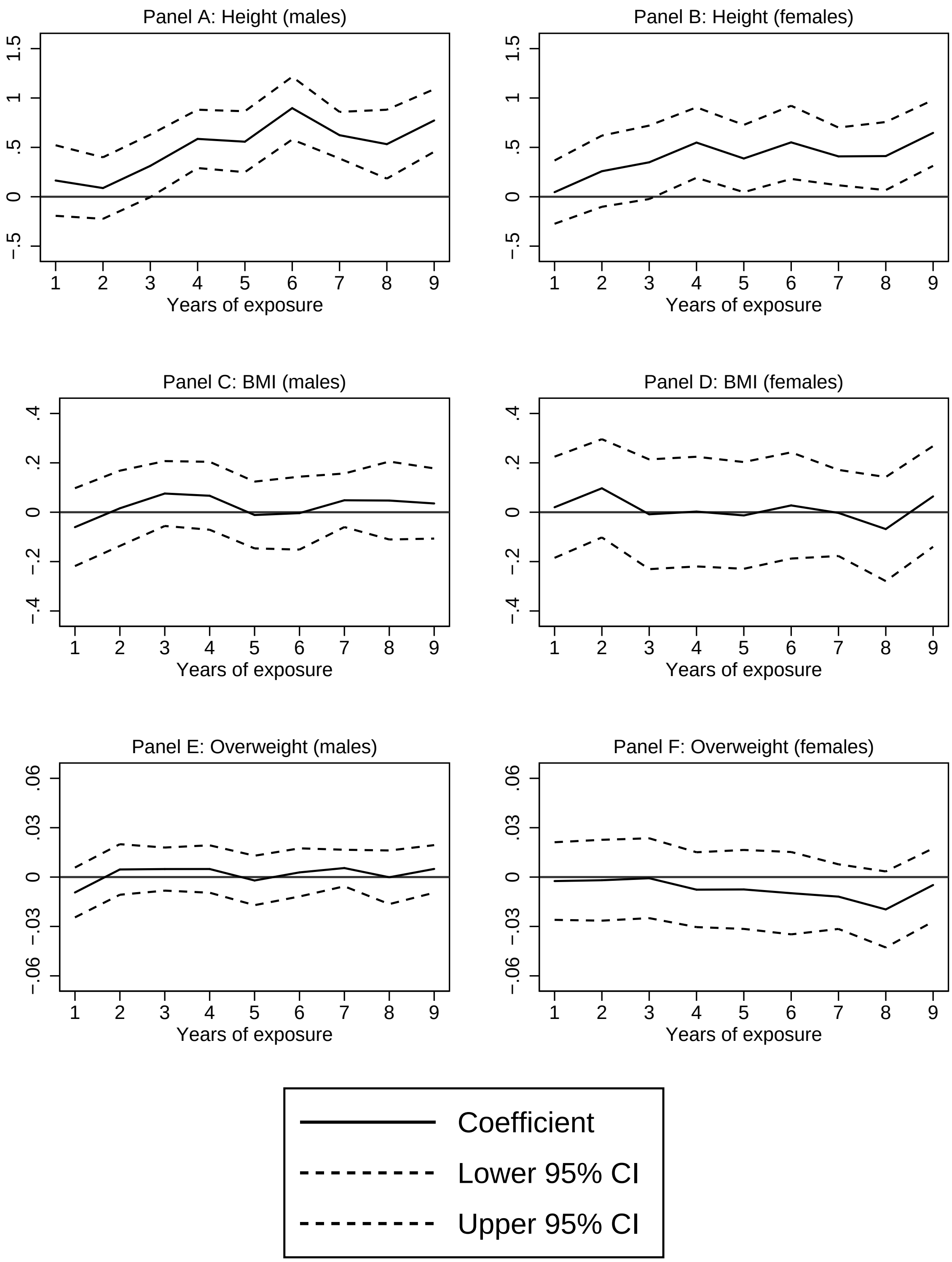
Notes: The figure plots estimates and 95 percent confidence intervals from a regression on the effect of years of exposure to school lunches on average lifetime income. The reference category is 0 years of exposure. Control variables include cohort fixed effects, municipality fixed effects, sex, whether born in Sweden, and whether the individual was exposed to 9 years of mandatory primary school. See text for details.

Figure 4: Effect of age at first school lunch exposure on lifetime income



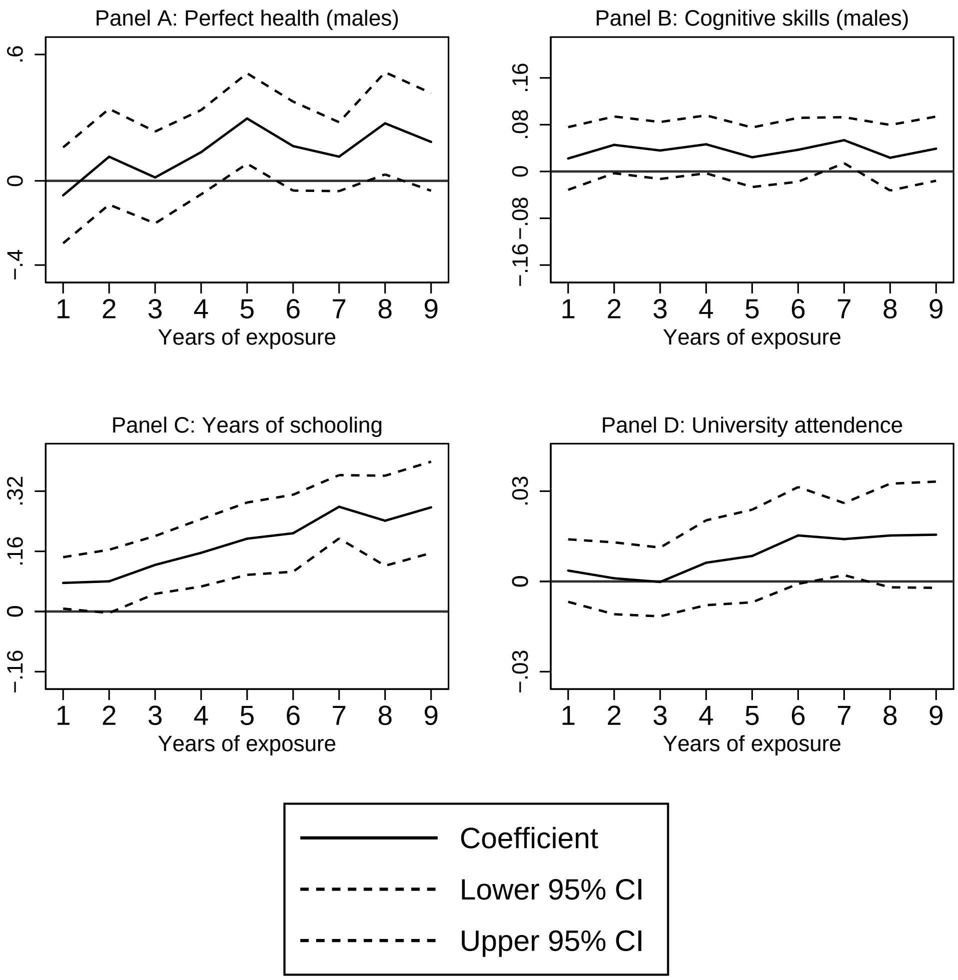
Notes: The figure plots estimates and 95 percent confidence intervals from a regression on the effect of age at first school lunch exposure on average lifetime income. Control variables include cohort fixed effects, municipality fixed effects, sex, whether born in Sweden, and whether the individual was exposed to 9 years of mandatory primary school. See text for details.

Figure 5: Effects of years of exposure to school lunches on various health outcomes at enlistment



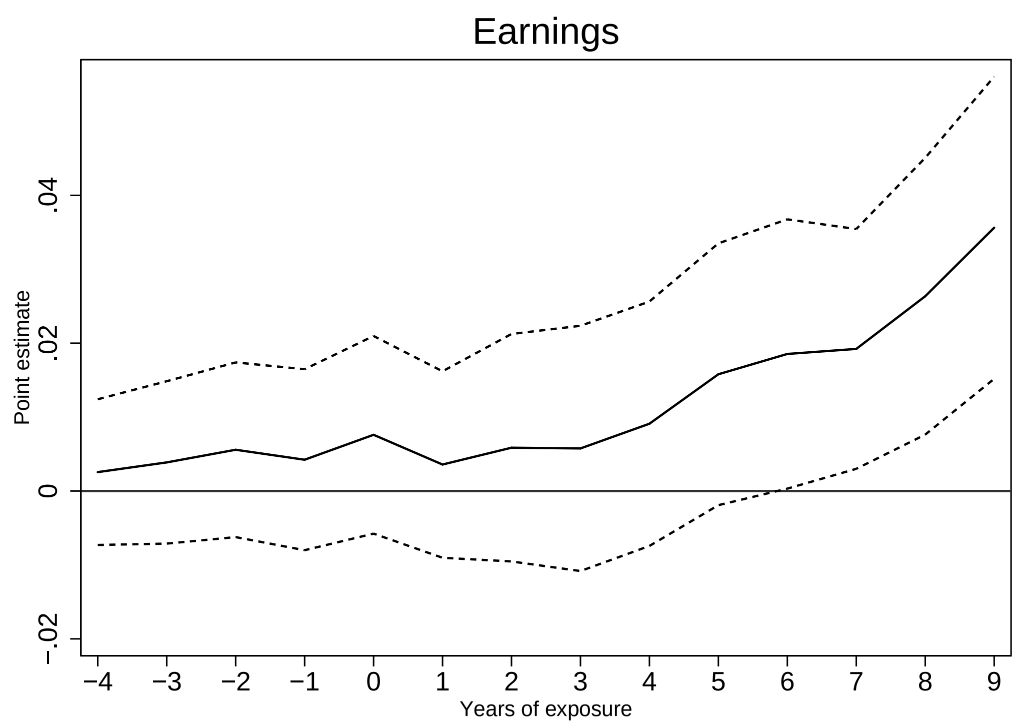
Notes: The figure plots estimates and 95 percent confidence intervals from regressions on the effect of years of exposure to school lunches on height, BMI, and overweight/obesity. Control variables include cohort fixed effects, municipality fixed effects, whether born in Sweden, and whether the individual was exposed to 9 years of mandatory primary school. See text for details.

Figure 6: Effects of years of exposure to school lunches on health, cognitive skills, and educational outcomes



Notes: The figure plots estimates and 95 percent confidence intervals from regressions on the effect of years of exposure to school lunches on health, cognitive skills, and educational outcomes. Control variables include cohort fixed effects, municipality fixed effects, sex (except for enlistment outcomes where only males were observed), whether born in Sweden, and whether the individual was exposed to 9 years of mandatory primary school. See text for details.

Figure 7: Event study graph of the effect of school lunch exposure on earnings



Notes: The figure plots estimates and 95 percent confidence intervals from an event study regression. In the graph, years of exposure coincide with cohorts. Zero years of exposure corresponds to belonging to the cohort that just had left school when school lunches were introduced. Minus 1 corresponds to those that left school more than 1 year before the school lunches were introduced, etc. Negative event time thus measures the cohort distance in years from the first cohort that was treated with school lunches. In the graph, 1 year of school lunches corresponds to the oldest cohort exposed to school lunches, i.e., those who began the last grade when school lunches were introduced and who, thus, were only exposed for one year. Positive years of exposure thus represent both the intensity of school lunch exposure and the distance in years from the first cohort that was treated with school lunches. Control variables include cohort fixed effects, municipality fixed effects, sex, whether born in Sweden, and whether the individual was exposed to 9 years of mandatory primary school. See text for details.

Table 1: Distribution of school lunch exposure.

	Observations	Fraction
Meals 0 year	124,987	0.082
Meals 1 year	9,610	0.006
Meals 2 years	8,932	0.006
Meals 3 years	9,090	0.006
Meals 4 years	9,379	0.006
Meals 5 years	9,521	0.006
Meals 6 years	10,253	0.007
Meals 7 years	59,473	0.039
Meals 8 years	11,399	0.007
Meals 9 years	1,277,116	0.835
Observations	1,529,760	

Notes: This table shows the distribution of school lunch exposure in our sample of individuals born between 1942 and 1965.

Table 2: Descriptive statistics.

	Mean	SD	N
Outcomes			
log(Earnings)	12.106	0.493	1,529,760
Years of schooling	12.176	2.513	1,443,114
University attendence	0.335	0.472	1,443,114
Maternal labor supply	0.619	0.486	1,529,760
Paternal labor supply	0.905	0.293	1,529,760
Height (males)	178.958	6.511	625,630
Height (females)	166.671	5.853	463,157
BMI (males)	21.49	2.707	621,394
BMI (females)	22.762	3.419	424,573
Overweight (males)	0.09	0.286	621,394
Overweight (females)	0.209	0.406	670,694
Perfect health	0.630	0.483	634,275
Cognitive skills	5.154	1.939	619,652
Smoking (females)	0.295	0.456	492,549
log(Birth weight)	8.146	0.160	609,638
Low birth weight	0.082	0.275	609,638
Very low birth weight	0.015	0.120	609,638
Background factors			
Birth year	1957.636	5.265	1,529,760
Female	0.487	0.5	1,529,760
Swedish	0.984	0.126	1,529,760
Compulsory school reform	0.875	0.331	1,529,760

Notes: This table shows descriptive statistics for our sample of individuals born between 1942 and 1965. See text for details on the sample selection.

Table 3: Effect of school lunch exposure on ln(income).

	(1) Income	(2) Income	(3) Income
Panel A			
Years of exposure	0.0035	0.0029	0.0027
	(0.0008)***	(0.0007)***	(0.0006)***
Panel B			
Meals 1 year	0.0001	-0.0009	-0.0012
	(0.0052)	(0.0052)	(0.0052)
Meals 2 years	0.0007	-0.0005	-0.0008
	(0.0057)	(0.0056)	(0.0056)
Meals 3 years	0.0003	-0.0014	-0.0017
	(0.0055)	(0.0054)	(0.0054)
Meals 4 years	0.0035	0.0015	0.0010
	(0.0060)	(0.0059)	(0.0059)
Meals 5 years	0.0101	0.0076	0.0070
	(0.0064)	(0.0063)	(0.0063)
Meals 6 years	0.0128	0.0098	0.0091
	(0.0066)*	(0.0064)	(0.0064)
Meals 7 years	0.0132	0.0096	0.0086
	(0.0053)**	(0.0051)*	(0.0050)*
Meals 8 years	0.0205	0.0162	0.0153
	(0.0068)***	(0.0065)**	(0.0065)**
Meals 9 years	0.0296	0.0234	0.0218
	(0.0077)***	(0.0064)***	(0.0063)***
Observations	1,529,760	1,529,760	1,529,760
Birth year FE	YES	YES	YES
Municipality FE	YES	YES	YES
Linear trends	NO	YES	NO
Quadratic trends	NO	NO	YES

Notes: This table shows DiD estimates of the effect of school meal exposure on (ln)lifetime income (average income between ages 25-65). Column 1 show results from our main DiD equation (1). Column 2 adds municipality-specific linear trends. Column 3 instead includes municipality-specific quadratic trends. In addition to birth cohort and municipality fixed effects, all regressions control for sex, whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Panel A shows the effect of school meals measured as a continuous treatment between 0 and 9 years. Panel B shows the effect of school meals when measured through a set of dummy variables, with 0 years being the reference category. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Heterogeneity analyses: effect of school lunch exposure on income by parental income quartile and gender.

	(1) Income quartile 1	(2) Income quartile 2	(3) Income quartile 3	(4) Income quartile 4	(5) Men	(6) Women
Years of exposure	0.0057	0.0023	0.0026	0.0015	0.0031	0.0040
	(0.0012)***	(0.0011)**	(0.0011)**	(0.0018)	(0.0010)***	(0.0010)**
Observations	380,015	380,017	380,016	380,016	780,163	739,901
Birth year FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Notes: This table shows DiD estimates of the effect of school lunch exposure on (log) lifetime income. Columns 1 to 4 shows the effect within quartiles of mean parental income (in 1968). Column 5 shows the effect for males and column 6 shows the effect for females. In addition to birth cohort and municipality fixed effects, all regressions control for sex (columns 1-4), whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The effect of school lunch exposure on height, BMI, and overweight/obesity.

	(1)	(2)	(3)	(4)	(5)	(6)
	Height		BMI		Overweight/obesity	
	Males	Females	Males	Females	Males	Females
Panel A						
Years of exposure	0.0678	0.0504	0.0044	0.0036	0.0007	-0.0004
	(0.0141)***	(0.0158)***	(0.0064)	(0.0090)	(0.0006)	(0.0010)
Panel B						
Meals 1 year	0.1644	0.0469	-0.0603	0.0387	-0.0094	-0.0009
	(0.367)	(0.1632)	(0.453)	(0.1041)	(0.226)	(0.0125)
Meals 2 years	0.0881	0.2588	0.0159	0.0852	0.0046	0.0015
	(0.579)	(0.1836)	(0.838)	(0.0994)	(0.555)	(0.0130)
Meals 3 years	0.3126*	0.3489	0.0758	-0.0203	0.0049	0.0056
	(0.053)	(0.1895)*	(0.258)	(0.1107)	(0.465)	(0.0129)
Meals 4 years	0.5861***	0.5489	0.0668	-0.0070	0.0049	-0.0056
	(0.000)	(0.1817)***	(0.341)	(0.1126)	(0.504)	(0.0124)
Meals 5 years	0.5577***	0.3874	-0.0112	0.0117	-0.0021	-0.0031
	(0.000)	(0.1735)**	(0.871)	(0.1100)	(0.788)	(0.0129)
Meals 6 years	0.8974***	0.5511	-0.0036	0.0353	0.0028	-0.0063
	(0.000)	(0.1888)***	(0.962)	(0.1076)	(0.703)	(0.0135)
Meals 7 years	0.6237***	0.4090	0.0484	-0.0061	0.0055	-0.0091
	(0.000)	(0.1490)***	(0.381)	(0.0876)	(0.332)	(0.0104)
Meals 8 years	0.5328***	0.4121	0.0475	-0.0672	-0.0001	-0.0161
	(0.003)	(0.1757)**	(0.555)	(0.1050)	(0.989)	(0.0123)
Meals 9 years	0.7726***	0.6469	0.0355	0.0579	0.0050	-0.0007
	(0.000)	(0.1700)***	(0.624)	(0.1018)	(0.500)	(0.0118)
Observations	625,630	463,157	621,394	424,573	621,394	424,573
Birth year FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Notes: This table shows DiD estimates of the effect of school lunch exposure on various medium-term health outcomes. Columns 1 and 2 show effects on height (cm). Columns 3-4 show effects on BMI, and columns 5 and 6 on overweight/obesity (BMI>25). In addition to birth cohort and municipality fixed effects, all regressions control whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Panel A shows the effect of one additional year of school meal exposure, that is, exposure as a linear measure, whereas Panel B shows the effect of school meals measured through a set of dummy variables, with 0 years being the reference category. Robust standard errors in parentheses. *** p$<$0.01, ** p$<$0.05, * p$<$0.1.

Table 6: The effect of school lunch exposure on health, cognitive skills, schooling, and labor supply

	Full sample					
	(1) Perfect health (males)	(2) Cognitive skills (males)	(3) Smoking (females)	(4) Schooling (all)	(5) Maternal LMP	(6) Paternal LMP
Panel A						
Years of exposure	0.0041	0.0032	-0.0013	0.0294	0.0034	0.0003
	(0.0013)***	(0.0026)	(0.0011)	(0.0063)***	(0.0008)***	(0.0004)
Panel B						
Meals 1 year	0.0043	0.0223	0.0048	0.0760	-0.0025	-0.0044
	(0.0125)	(0.415)	(0.0131)	(0.0349)**	(0.0061)	(0.0039)
Meals 2 years	0.0158	0.0455*	-0.0102	0.0803	0.0021	-0.0043
	(0.0121)	(0.066)	(0.0136)	(0.0428)*	(0.0070)	(0.0040)
Meals 3 years	0.0124	0.0359	0.0095	0.1240	0.0021	0.0023
	(0.0120)	(0.148)	(0.0142)	(0.0392)***	(0.0066)	(0.0040)
Meals 4 years	0.0078	0.0465*	0.0090	0.1561	0.0191	-0.0043
	(0.0113)	(0.066)	(0.0142)	(0.0457)***	(0.0077)**	(0.0039)
Meals 5 years	0.0324	0.0244	-0.0073	0.1937	-0.0016	-0.0043
	(0.0130)**	(0.348)	(0.0142)	(0.0490)***	(0.0078)	(0.0046)
Meals 6 years	0.0268	0.0371	0.0163	0.2081	0.0114	-0.0009
	(0.0127)**	(0.183)	(0.0137)	(0.0522)***	(0.0080)	(0.0045)
Meals 7 years	0.0280	0.0535***	-0.0079	0.2786	0.0187	-0.0007
	(0.0093)***	(0.008)	(0.0109)	(0.0429)***	(0.0069)***	(0.0036)
Meals 8 years	0.0330	0.0235	-0.0075	0.2415	0.0074	-0.0010
	(0.0148)***	(0.410)	(0.0138)	(0.0610)***	(0.0085)	(0.0043)
Meals 9 years	0.0394	0.0390	-0.0026	0.2770	0.0280	0.0001
	(0.01384)***	(0.163)	(0.0131)	(0.0619)***	(0.0083)***	(0.0041)
Observations	634,275	619652	492,549	1,443,114	1,529,760	1,529,760
Birth year FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Notes: This table shows estimates of the effect of school lunch exposure on various potential mediators. Column 1 shows regressions on a dummy indicating perfect health, as assessed at military enlistment. Column 2 shows effects on cognitive skills, while column 3 shows effects on smoking among females. Column 4 shows effects on years of schooling, whereas columns 5 and 6 show effects on mothers' and fathers' labor supply, measured through having positive labor income in 1970. In addition to birth cohort and municipality fixed effects, all regressions control for sex (columns 4-6), whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Panel A shows the effect of one additional year of exposure to school lunches. Panel B shows the effect of school meals measured through a set of dummy variables, with 0 years being the reference category. Robust standard errors in parentheses. *** p$<$0.01, ** p$<$0.05, * p$<$0.1.

Table 7: Effect of school lunch exposure on ln(income). Family fixed effects models.

	(1)	(2)	(3)	(4)	(5)	(6)
	Income	Income First and second born	Income Second and third born	Height (males)	Height (females)	Perfect health (males)
Years of exposure	0.0040	0.0048	0.0040	0.0417	0.0333	0.0068
	(0.0009)***	(0.0015)***	(0.0023)*	(0.0347)	(0.0631)	(0.0035)**
Observations	710,946	710,946	292,865	172,487	224,872	174,508
Family FE	NO	YES	YES	YES	YES	YES
Birth year FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Notes: The table provides robustness checks on our main DiD model of the effect of school lunch exposure on (log) lifetime income. Column 1 shows estimates of the effect of the number of years of school lunch exposure when restricting the sample to families where the first and second born children were born at most 5 years apart. Column 2 shows estimates from the same model as in column 1, this time with familiy fixed effects added. Column 3 shows estimates from re-running the model as in column 2, but restricting the data to second and third born children, born at most 5 years apart. Column 4 and 5 show estimates from family fixed effects regressions with height as an outcome, restricting the sample to first and second born siblings, born at most 5 years apart. Columns 6 show estimates from a family fixed regression with health at enlistment as an outcome, restricting the sample to first and second born siblings, born at most 5 years apart. In addition to birth cohort and municipality fixed effects, all regressions control for sex (columns 1-3), whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. Standard errors clustered at the family level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Effect of school lunch exposure on the birth weight of children.

	(1)	(2)	(3)
	(ln)Birth weight	Low birth weight	Very low birth weight
Years of exposure	0.0000	-0.0005	-0.0004
	(0.0003)	(0.0006)	(0.0002)
Observations	609,638	609,638	609,638
Birth year FE	YES	YES	YES
Municipality FE	YES	YES	YES

Notes: The table provides DiD estimates of the effect of school lunch exposure on the birth weight of children. Column 1 shows the effect of the number of years of school lunch exposure on log birth weight. Columns 2 and 3 show the effects on the probability of having at least one child born with low birth weight (<2500 grams) and very low birth weight (<1500 grams). In addition to birth cohort and municipality fixed effects, all regressions control for whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Robustness tests.

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor income	Income 35-45	Income School reform=1	Income (males) Enlistment	Income (females) Height sample	Income (females) Birth weight sample
Years of exposure	0.0039	0.0023	0.005	0.0025	0.0021	0.0022
	(0.0010)***	(0.0009)***	(0.001)***	(0.0011)**	(0.0011)*	(0.0009)**
Observations	1,522,710	1,502,320	1,336,921	720,408	463,157	609,638
Birth year FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Notes: This table shows the results of various robustness checks. Column (1) shows estimates from re-running our main specification, this time with labor earnings (excluding taxable benefits) as an outcome. Column (2) shows results using average earnings between ages 35-45 as an outcome. Column (3) shows results when only including individuals exposed to the 9-year schooling reform, see text for details. Column (4) shows estimates of the effect of school lunch exposure on earnings for male cohorts included in the military enlistment register. Column (5) shows the corresponding effect for females in the medical birth register (for those for which we observe birth weight). In addition to birth cohort and municipality fixed effects, all regressions control for sex (columns 1-3), whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Tables and Figures For Online Publication

Figure A.1: Example of a school lunch menu

Med utgångspunkt från de på sid. 27 föreskrivna typrätterna gives i det följande exempel på matsedel för sex veckor. *Denna matsedel skall vara normgivande för måltidernas standard.* Vid en skola kan man dock få ha en tre- eller fyraveckors matsedel.

1:a veckan.

Måndag:	Rökt korv, kokt blomkål eller annan grönsak, tomatsås, potatis.
Tisdag:	Blandad grönsakssoppa. Smörgåspålägg: ost.
Onsdag:	Stekta fiskfiléer, citron, potatis. Frukt.
Torsdag:	Ägg med spenatstuvning, tomat. Smörgåspålägg: leverpastej.
Fredag:	Leverbiff, råsallad, potatis.
Lördag:	Blandad risrätt, bönor. Frukt.

2:a veckan.

Måndag:	Spenat- eller grönkålssoppa, ägg eller korv. Smörgåspålägg: ost.
Tisdag:	Kalops, rödbetor, potatis. Frukt.
Onsdag:	Grönsaksfat, ost- eller skinksås, potatis.
Torsdag:	Inkokt strömming, skarpsås, sallad, potatis.
Fredag:	Senapsbrynt fläskkorv, rotmos. Frukt.
Lördag:	Makaronipudding, smörsås, rårivna morötter.

3:e veckan.

Måndag:	Stuvade fiskbullar, ärter, potatis. Frukt eller morotsbit.
Tisdag:	Skånsk kålsoppa. Smörgåspålägg: ost.
Onsdag:	Köttfärs, sås, tomat eller annan grönsak, potatis.
Torsdag:	Potatisgratin. Frukt. Smörgåspålägg: korv.
Fredag:	Leverstuvning, äppel- och vitkålssallad, potatis.
Lördag:	Pannkaka. Frukt.

Source: SOU (1958). See text for details.

Table A.1: School lunch expsoure and health conditions at enlistment (males)

	(1)	(2)	(3)	(4)	(5)
	Any diagnosis	Type II diabetes	Digestive conditions	Visual impairment	Congenital disorder
Years of exposure	-0.0119	-0.0002	-0.0059	-0.0058	-0.0001
	(0.0015)	(0.0001)*	(0.0006)***	(0.0009)***	(0.0003)
Observations	635,773	635,773	635,773	635,773	635,773
Birth year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES

Notes: This table provides DiD estimates of the effect of school lunch exposure on various health conditions, diagnosed at military enlistment. In addition to birth cohort and municipality fixed effects, all regressions control whether born in Sweden and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Effect of school lunch exposure on alternative education measures and from specifications including municipality-specific linear trends.

	(1) University attendence	(2) Schooling	(3) University attendence	(4) Maternal LMP
Panel A				
Years of exposure	0.0019	0.0294	0.0018	0.0026
	(0.0009)**	(0.0062)***	(0.0009)*	(0.0010)**
Panel B				
Meals 1 year	0.0036	0.0760	0.0036	-0.0012
	(0.0053)	(0.0349)**	(0.0053)	(0.0079)
Meals 2 years	0.0010	0.0803	0.0011	0.0025
	(0.0061)	(0.0428)*	(0.0061)	(0.0096)
Meals 3 years	-0.0002	0.1239	-0.0002	0.0035
	(0.0058)	(0.0391)***	(0.0058)	(0.0098)
Meals 4 years	0.0062	0.1560	0.0062	0.0225
	(0.0072)	(0.0455)***	(0.0072)	(0.0103)**
Meals 5 years	0.0085	0.1936	0.0085	-0.0017
	(0.0078)	(0.0487)***	(0.0078)	(0.0105)
Meals 6 years	0.0153	0.2080	0.0153	0.0027
	(0.0082)*	(0.0516)***	(0.0082)*	(0.0108)
Meals 7 years	0.0141	0.2786	0.0137	0.0154
	(0.0061)**	(0.0429)***	(0.0061)**	(0.0086)*
Meals 8 years	0.0153	0.2413	0.0152	0.0105
	(0.0088)*	(0.0596)***	(0.0088)*	(0.0109)
Meals 9 years	0.0155	0.2768	0.0153	0.0221
	(0.0090)*	(0.0602)***	(0.0091)*	(0.0106)**
Observations	1,443,114	1,443,114	1,443,114	1,513,596
Birth year FE	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Linear trends	NO	YES	YES	YES

Notes: This table shows DiD of the effect of school lunch exposure on various educational outcomes and maternal labor supply. Column 1 shows the effect of school lunch exposure on the probability of entering college. Columns 2 and 3 show the effects on years of schooling and college attendance in regressions where we control for municipality-specific linear trends. Column 4 show the effects on maternal labor supply with municipality-specific trends. In addition to birth cohort and municipality fixed effects, all regressions control for sex, whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Panel A shows the effect of school meals measured as a continuous treatment between 0 to 9 years. Panel B shows the effect of school meals measured through a set of dummy variables, with 0 years being the reference category. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Effect of school lunch exposure on long-term morbidity and mortality.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mortality	Morbidity	Heart disease	Mental disease	Cancer	Endocrine conditions	Digestive conditions
Years of exposure	0.0008	-0.0001	0.0004	0.0000	-0.0005	-0.0000	-0.0001
	(0.0006)	(0.0003)	(0.0004)	(0.0003)	(0.003)	(0.0002)	(0.0004)
Observations	1,538,518	1,538,518	1,538,518	1,538,518	1,538,518	1,538,518	1,538,518
Birth year FE	YES	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES	YES

Notes: The table provides DiD estimates of the effect of school lunch exposure on long-term mortality (not surviving until 2013) and morbidity (having any hospitalization betweeen 1995 and 2013). Column 1 shows the effect of the number of years of school lunch exposure on the probability of not surviving until 2013. Column 2 show estimates of the effect of school lunch exposure on the probability of having any hospitalization. Columns 3-7 show regressions on specific health conditions as diagnosed in the hospitalization register between 1995-2013. In addition to birth cohort and municipality fixed effects, all regressions control for sex, whether born in Sweden, and whether the individual was exposed to 9 years of primary school. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.