ROYAL STATISTICAL SOCIETY
DATA | EVIDENCE | DECISIONS

WILEY

Social stratification and out-of-school learning
Author(s): Christian Andersson and Per Johansson
Source: *Journal of the Royal Statistical Society. Series A (Statistics in Society)*, Vol. 176, No. 3 (JUNE 2013), pp. 679-701
Published by: Wiley for the Royal Statistical Society
Stable URL: http://www.jstor.org/stable/43965656
Accessed: 02-07-2018 20:01 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at http://about.jstor.org/terms

JSTOR

Royal Statistical Society, *Wiley* are collaborating with JSTOR to digitize, preserve and extend access to *Journal of the Royal Statistical Society. Series A (Statistics in Society)*

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

J. R. Statist. Soc. A (2013)
176, *Part* 3, *pp.* 679–701

Social stratification and out-of-school learning

Christian Andersson and Per Johansson

Uppsala University, Sweden

[Received August 2010. Final revision May 2012]

Summary. To study the effects of out-of-school learning we use data on children who stayed in boarding houses while attending public elementary schools in Sweden in the 1940s. The out-of-school environment at the boarding houses could be considered as being more conducive to learning than the home environment: the pupils at the boarding houses had daily scheduled time for doing their homework with the assistance of a junior school teacher and, in addition, they had access to a small library. The placement at boarding houses was based on the distance from their place of residence to the nearest school and thus had no direct connection to the pupils' skills, which simplifies the empirical analysis based on register data. We find that the more conducive learning environment equalized skills at school leaving age, and the effect was greater for children with poor initial ability.

Keywords: Early interventions; Homework; Pedagogic personnel

1. Introduction

We study the importance of out-of-school learning for both the scholastic achievement and the labour market outcomes of the labour market careers of children. This is made possible by using data on boarding house children in the 1940s who attended elementary public schools close to their boarding houses. The out-of-school environment for these children could, in general, be considered as more conducive to learning than a home environment. The boarding house pupils had daily scheduled time for doing their homework with the assistance of a qualified teacher as well as access to a small library.

This study relates to a great extent to the growing field of literature on the effects of after-school programmes (see Bae *et al.* (2010) and Lauer *et al.* (2006) for reviews). After-school programmes were established in the USA in the early 1900s as a way to increase the safety and care of children who lived in unsafe neighbourhoods. The number of programmes increased after the 1940s as a response to the need for child care due to the growth in maternal employment (Halpern, 2002). To date there is an increasing number of children participating in out-of-school programmes that are school based or community sponsored. De Kanter (2001) reported that the number of schools offering after-school programmes had doubled since 1994. The objective of the programmes is to care for children who lack after-school care at home. The idea is that the enriching experiences will improve children's socialization and reduce maladaptive behaviour. The after-school programmes have also recently been seen as a way of raising student achievement (No Child Left Behind Act of 2001). This has generated a greater interest in after-school tutoring programmes and increased government investment in the programmes (Lauer *et al.*, 2006). Lauer *et al.* (2006) concluded from their metastudy of 35 out-of-school time programmes

Address for correspondence: Per Johansson, Department of Economics, Uppsala University, IFAU, Box 513, Uppsala 751 20, Sweden.
E-mail: per.johansson@ifau.uu.se

© 2012 Royal Statistical Society 0964–1998/13/176679

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

(i.e. after-school programmes and summer schools) that there is a small, but statistically significant, effect on both reading and mathematics achievement for students. The effects are greater for programmes which provide tutoring in reading.

This paper is also related to two other strands of literature: the early intervention literature (see for example Currie (2001), Heckman and Masterov (2007) and Almond and Currie (2011) for reviews) and the 'summer gap' literature (see, for example, Heyns (1978), Cooper *et al.* (1996), Fryer and Levitt (2004), Entwisle *et al.* (2007) and Lindahl (2007)).

The early intervention literature focuses on effects of preschool interventions directed at disadvantaged children under 5 years of age. The conclusion in Currie (2001) and Almond and Currie (2011) is that the programmes lead to economically and statistically significant short- and medium-term benefits on cognition as well as long-run effects such as improving educational attainment, earnings, reducing welfare dependence and crime. In general, the effects are greater for the most disadvantaged children.

In the summer gap literature, the effects of the out-of-school environment on scholastic achievement are studied by making use of the gap in test scores between semesters (spring–autumn). The general finding is that the gap in test scores between advantaged and disadvantaged pupils widens during the summer (between semesters) and that schooling (during semesters) compensates pupils from disadvantageous family backgrounds.

The main contribution of our paper is that the estimation of out-of-school programme effects makes use of an assignment that is basically exogenous. In addition, since the treatment stems from 70 years ago it allows us to study potential long-run effects. In this respect this paper also makes a significant contribution to the existing evidence from the summer gap literature and early intervention literature.

The main drawback with our study is that the treatment did not involve just a more conducive to learning, out-of-school environment. In some cases the children, some only 6 years of age, stayed at their boarding houses for a very long period of time without seeing their families. The rules at the boarding houses were strict and the children had to do some chores. Being separated from their families might have been potentially traumatic for the children and might, thus, have had a negative effect both at the time that they were boarded and also later on in life. These potential effects are most likely to attenuate any effect from tutoring and extra pedagogical stimuli provided at the boarding houses.

Placement at the boarding houses was based on the distance to the nearest school. Children who lived more than 4 km from a school were sent to a boarding house whereas children living closer were not allowed to lodge in the boarding houses. It is possible that the children who lived further from a school differed from other children in other respects which might also have had an effect on their schooling. This fact makes simple comparisons in mean outcomes between the two groups of pupils impossible. Unfortunately, we lack information on the distances to the nearest school for the non-boarding-house children, which is why a regression discontinuity approach, where we would have compared the outcomes of the non-boarding children with the boarding children at the 4-km threshold, is not possible. Instead we control for differences in skills in the first grade by means of regression. To account for selection we have access to the socio-economic status of the mothers and fathers, the grades achieved in the first semester and the name of the pupil's school. This allows us to perform extensive sensitivity checks and it also allows us to use within-school variation in the estimations.

Our results suggest that out-of-school learning has positive effects on student achievement. Staying at the boarding houses for 6 years or more raised the grade point average (GPA) in the sixth grade by 6% on average. There is some evidence of long-run effects as well. Future educational attainment and earnings were all unaffected by being lodged at the boarding houses.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

However, pupils with longer stays at the boarding houses benefited compared with pupils, with the same background, who stayed for a shorter period of time. We find that boarding had a greater effect on children with a low GPA in their first semester. Consistent with this, there are some indications of long-run positive effects on educational attainment and earnings for those who had a low GPA in the first semester. For those with poor initial skills, the positive effect of tutoring, books etc. outweighed the negative effects of being separated from the home environment.

The rest of the paper has the following structure. Section 2 describes the boarding houses. Section 3 presents the data and definitions of the variables. Section 4 provides a first descriptive look at the data and Section 5 the estimation results. The paper is concluded in Section 6.

2. Norrbotten County and the boarding houses

Sweden was not a rich country (see for example Maddison (2001)) in the early 20th century. This was especially true for Norrbotten County, a county bordering Finland in the north, where the boarding houses were situated. From 1848 to 1958, compulsory education was organized locally and the level of education was low. This meant that the quality of teaching and the number of years and number of days in a year that children actually attended school varied from one county to the next. The general pattern in the first half of the 20th century was that both the quality and the quantity of schooling were better in cities than in rural areas and that the quality and quantity of schooling decreased as the distance from the capital, Stockholm, increased (Statistics Sweden, 1974). In 1903, 7% of children between the ages of 7 and 14 years in Norrbotten County did not receive any schooling. The corresponding figure for Sweden as a whole was 3%.

In 1902 and 1903 the crops in the northern part of Sweden failed and this resulted in severe famine. As a consequence, a special type of after-school centres ('arbetsstugor') was established in Norrbotten County to which children were sent to board. From here on, we denote these centres as boarding houses. The primary goal of the boarding houses was to relieve children from acute poverty. However, they were soon seen as a solution to the problem that many children did not receive any schooling because of the long distances and poor roads between homes and schools.

Access to the boarding houses was free but voluntary. However, from 1935 it was made conditional on not living too close to a school. The children at the boarding houses attended regular schools close to the boarding houses. The schools were supposed to provide formal education, whereas the boarding houses were to provide the children with food and lodging, but also to see to the upbringing and development of the children by accustoming them to work, tidiness and obedience. As food and lodgings were free for the families with children in the homes, families gained financially from the placement, but as the children could no longer help out with chores at home it is possible that placement created some problems especially for families with crofts and farms. All in all we believe that the financial gains were higher than the potential costs. The emotional cost for the parents of sending their young children to boarding houses must, however, have been high.

The children in the homes had scheduled time for homework after school and from the mid-1930s it was required that the director of the boarding house had a secondary school certificate or was educated as a junior primary school teacher. Every boarding house had its own small library, where the pupils could borrow books. This kind of availability of books must have been exceptional at this time in Norrbotten County.

Most of the children came to a boarding house at the age of 7 years and stayed there for their

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

entire elementary schooling. In the academic year 1939–1940, time spent at the boarding house was extended from 8 to 9 months per year. Most children only visited their parents during the summer and Christmas holidays.

2.1. Schooling and organization

7 years of schooling became mandatory in Sweden in 1936 and, by 1940, 97% of the children attended full-time elementary schooling (see for example Andersson (2007) for more details). From 1935 onwards the boarding houses were funded by government grants. The grant was subject to the *proviso* that the child's place of residence was at least 4 km from the nearest school. In the 1940s there was a strong drive to centralize school units, which implied an increased requirement to transport or to send children living far from the nearest school to a boarding house (Statens Offentliga Utredningar (1945), pages 14–15). However, along with the improvement of the economy and the construction of new and better roads, it became more common for children living far from schools to have access to regular transportation. After the academic year 1953–1954, the Foundation of the Norrbotten Boarding Homes finally decided to close down the 15 remaining boarding houses. Seven of the homes that are examined in this paper were closed down in 1954. One closed as early as 1951. The closure of the boarding houses implies that children who were born in 1941 lived in a boarding house for a maximum of 12 semesters. Between the years 1903 and 1954, 5484 children lived in one of in total 20 boarding houses in Norrbotten County.

3. Data and variable specifications

The population of interest in this study consists of individuals who were born between 1932 and 1941. The choice of cohorts implies that the oldest individuals started elementary schooling in the academic year 1939–1940 and the youngest individuals in the academic year 1948–1949. The population is divided into two groups: those who lived in the boarding houses (the treatment group) and those who did not (the comparison group).

All individuals in the treatment and comparison groups attended one of eight schools in four different municipalities in Norrbotten County. We collected information on all the pupils from the eight schools from each municipality's school archives and from handwritten yearbooks, where teachers kept the daily records of the pupils at the school. From these yearbooks we have information on each pupil's name, date of birth, whether they had to repeat the first grade, their grades in all the subjects that they were taught and their parents' names. Grades are available in the handwritten books for all semesters, but we decided to collect the grades in the first and the 12th semesters only. To calculate GPAs, the letter-based grades have been transformed into numeric values. For three subjects there are a few missing values and in these cases these subjects have been disregarded when calculating individual GPAs (see Appendix A for details).

We have constructed three different GPAs. The first, *GPAca*, is intended to measure cognitive skills: it is the average of the grades in

(a) *mathematics*,
(b) *writing and language* and
(c) *speech and reading exercises*,

subjects that were taught during the entire period of elementary schooling. The second, *GPAsa*, is intended to measure social skills and is the average of the grades in

(a) *order* and
(b) *conduct.*

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

The last, *GPAo*, is the average of the grades in the rest of the subjects that were taught.

Boarding house pupils have been identified via the *Folkrörelsernas* archive in Luleå. The archive preserves the original registers from the homes. We collected information from these handwritten registers about each pupil's name, date of birth, the name and occupation of their parents, home district and in which semester they first arrived at the boarding house. Information about the number of semesters that children stayed at the boarding houses and information on distances to the nearest school were also collected. This latter information is unfortunately available for only 88% of the 428 boarding house individuals.

The eight schools that were included were small, with mean pupil cohorts of around 30. The share of boarding house pupils varied between schools. The share of boarding house pupils varied from 17% to 50% between schools. The total number of individuals is fairly evenly distributed over the sample period. However, since transporting children to school became more common over time, there were more boarding house pupils in the earliest years (for details see Table B2 in the on-line supplement). Thus, the boarding house individuals are, on average, older than the comparison group of individuals.

We have information on the date of birth as well as the children's names. Using the date of birth as well as the names of individuals, Statistics Sweden could retrieve the personal identifier for more than 90% of all individuals. Using the personal identifier, information on family background, educational attainment and annual earnings were matched to the data. There are two reasons why it is not possible to find all individuals. First, it is not possible to find an individual who died before 1947 or after 2003. Second, individuals who have emigrated from Sweden are not registered in the Statistics Sweden database. One potential reason for emigration was the reimmigration of Finnish children who lived in Sweden during the latter part of World War II (see De Geer (1986)). The final sample contains 1457 individuals (born between 1932 and 1941), 428 of whom lived in one of the boarding houses.

To obtain information on the pupils' family backgrounds we used the *multigenerational register*. This register contains information on both the biological mothers and fathers of individuals who were born after 1932 and registered as living in Sweden from 1961 onwards.

From the population censuses in 1960 and 1970 we obtained information on the socio-economic status as well as the educational level of the parents. The socio-economic status is based on the individual's occupation and it is divided into 12 categories. Information on marital status, citizenship, municipality and parish is measured at the end of 1968 and is collected from population registers. Information on educational attainment in 1990, 1995 and 2006 is collected from the *Swedish register of education*. Annual earnings and wealth in 1968, 1978, 1988 and 1998 are collected from the *income and tax register*.

4. Descriptive statistics and some preliminary evidence

In Section 4.1 we describe the observed family backgrounds for the two groups of pupils and in Section 4.2 we describe the observed individual characteristics for the two groups of pupils.

4.1. Family background

Unfortunately, all parental variables are measured after the pupils started their elementary education (1939–1948): socio-economic status is measured in 1960, earnings, wealth and marital status in 1968, and educational attainment in 1970. Since adult education at this time was extremely rare, the educational attainment in 1970 is most probably a correct measure of the parents' level of education in 1939–1948.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 1. Socio-economic status SEI of parents in 1960†

SEI	*Boarding house fathers*		*Comparison fathers*		*Boarding house mothers*		*Comparison mothers*	
	Observations	*%*	*Observations*	*%*	*Observations*	*%*	*Observations*	*%*
1	82	28.5	135	19.3‡	108	34.5	151	19.0‡
2	59	20.5	62	8.8‡	55	17.6	54	6.8‡
3	6	2.1	53	7.6‡	5	1.6	58	7.3‡
4			9	1.3§§			7	0.9§§
5			1	0.1	1	0.3		
6	5	1.7	66	9.4‡	7	2.2	90	11.3‡
7	70	24.3	218	31.1§	53	16.9	195	24.5§
8			17	2.4§	2	0.6	56	7.0‡
11			2	0.3				
12	66	22.9	138	19.7	82	26.2	185	23.2
Total	288	100	701	100	313	100	796	100

†The SEI-codes are 1, crofters and farmers, 2, workers in farming or forestry, 3, entrepreneurs in industry, trade, transport or service occupations, 4, entrepreneurs in 'free' occupations, 5, business executives (employed), 6, civil servants and salaried employees, 7, blue-collar workers (other than category 2), 8, employees in service occupations, 11, students, and 12, unemployed.
‡Significant at 1%.
§Significant at 5%.
§§Significant at 10%.

The distributions of socio-economic status SEI of mothers and fathers in the treatment group in 1960 are shown in Table 1. The mean differences of the parents' SEI between the boarding house and comparison groups are all statistically significant, except for business executives (employed), students and unemployed for both parents.

Under the assumption that parental social class is stable over time we can conclude that both mothers and fathers of the boarding house pupils are overrepresented in the group of crofters and farmers (SEI = 1). This group is likely to consist of quite poor families that in most cases lived off a small piece of land that they cultivated.

The parents of the boarding house pupils are also overrepresented in both the socio-economic group of workers in farming and forestry (SEI = 2) as well as the unemployed (SEI = 12) compared with the comparison group. The parents of the comparison pupils are more frequently found in the socio-economic groups entrepreneurs in industry, trade or service occupations (SEI = 3), civil servants (SEI = 6) and blue-collar workers (SEI = 7). They are also more often employed in service occupations (SEI = 8) than the parents of the boarding house pupils. All in all, this picture indicates that, in general, parents of boarding house pupils belonged to lower socio-economic groups than parents of pupils in the comparison group.

Table 2 presents wealth, annual earnings and marital status in 1968 for the two groups of parents. On average, the parents of the boarding house pupils have less wealth, earn less and are married to a lesser extent than the parents of the comparison pupils. It is evident that the socio-economic background of the boarding house group is poorer than that of the comparison group.

4.2. Individual variables

The distribution of distances to the nearest school for the boarding house pupils is displayed in Table 3. We see that the 4-km limit for being allowed admission to the boarding houses was strict. A mere 3.2% of the boarding house individuals had a place of residence that was less than

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 2. Wealth, earnings and marital status of parents in 1968†

Parent	*Boarding house (SEK)*		*Comparison (SEK)*	
	Observations	*Mean (standard deviation)*	*Observations*	*Mean (standard deviation)*
Wealth				
Fathers	239	6175 (67359)	622	39359 (230652)
Mothers	213	495 (5100)	535	16370 (116898)
Earnings				
Fathers	239	93012 (57406)	622	108661 (104988)
Mothers	213	39017 (29629)	535	56668 (57406)
Married = yes				
Fathers	192	83.5%	544	90.2%
Mothers	188	67.1%	544	71.9%

†Wealth and earnings are in Swedish kronor adjusted to 2006 values (US $1 ≈ SEK 7.5).

Table 3. Distance to the nearest school for boarding house pupils†

Distance (km)	*Observations*	*%*
0–2	0	0
3	12	3.2
4–5	108	28.5
6–10	113	29.8
11–15	25	6.6
16–20	36	9.5
21–25	23	6.1
26–30	34	9.0
31–35	9	2.4
36–40	12	3.2
⩾41	7	1.8
Total	379	100

†For 49 boarding house individuals (about 13%) there is no information on the distance to the nearest school. Originally the number of missing values was 136, but on the basis of the information on place of residence we have imputed distances to the nearest school for 87 individuals.

4 km from the nearest school. No one lived closer than 3 km from the nearest school. For the majority of the boarding house individuals the distance to school was between 4 and 10 km. The rest of the individuals are quite evenly distributed up to a distance of 30 km. Note also that many pupils had very long distances to travel from their residence to the nearest school. More than 20% of the pupils lived more than 20 km from the nearest school.

We have compared the individuals with observed distances to the nearest school with those for whom the information on distance is missing with respect to grades, parents' earnings and socio-economic status. The results from this exercise show small, and not statistically significant

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

(at the 10% level), differences between the two groups. The same exercise has been carried out for those living within 4 km of the nearest school. Four of these individuals had peers in the boarding house from the same home village; hence the distance rule seems to have been strict. However, we cannot rule out that a few exceptions might have been made on the grounds of poverty.

Since the number of boarding houses was restricted, the distance to the nearest school for the children is not always the same as the distance to the nearest boarding house where the child actually boarded. The distances to the nearest boarding house (which are not displayed) are in general further than the distances to the nearest school. Less than 10% of the boarding house pupils were lodged less than 10 km from their home. This suggests that very few children could meet their parents apart from at Christmas and during the summer holidays.

Table 4 presents the number of semesters spent at the boarding house by age of entry to the home. We have information on the number of semesters for 389 of the boarding house pupils. We lack information on the number of semesters for 39 boarding house pupils. The SEI-value of these individuals' parents indicates that they were unemployed to a greater extent than the rest of the parents. One potential reason for the missing information is that the pupils lodged in a boarding house for just a very short period of time due to residential mobility caused by the parents' unemployment status. We can conclude that most pupils entered the boarding house at 7 years of age. Most pupils who entered at 6 or 7 years of age stayed in the boarding houses throughout their elementary schooling period; see 11–12 and 13–14 semesters in Table 4. Many pupils, however, left the boarding houses early. We do not know the reason for this. However, since transportation improved over time and since we find an increasing fraction leaving early over time the most likely explanation is that the majority of the 'dropouts' were subsequently transported to school instead of being sent to a boarding house. About half of the boarding house individuals spent more than 5 years at the boarding house. The mean number of semesters spent at one of the boarding houses is almost 10 semesters. As a result of the closure of the boarding houses in 1954, the last cohort of pupils, those born in 1941, could stay at the boarding home for a maximum of 12 semesters. One boarding house was closed down already

Table 4. Number of semesters at the boarding house by age at entry†

Semesters	*Results for the following ages (years) when started boarding:*								*Total*	*%*
	6	*7*	*8*	*9*	*10*	*11*	*12*	*13*		
1–2	4	21	5	2	1	0	3	2	38	9.8
3–4	8	20	2	3	0	1	3	1	38	9.8
5–6	2	19	4	0	2	3	0	0	30	7.7
7–8	9	34	5	3	0	2	0	0	53	13.6
9–10	6	26	2	3	1	0	0	0	38	9.8
11–12	11	48	7	1	0	0	0	0	67	17.2
13–14	15	89	8	1	0	0	0	0	113	29.0
15–16	3	7	2	0	0	0	0	0	12	3.1
Total	58	264	35	13	4	6	6	3	389	100
%	14.9	67.9	9.0	3.3	1.0	1.5	1.5	0.8	100	100

†Own calculations based on data from the *Folkrörelsernas arkiv*. Information on the number of semesters is missing for 39 individuals (9%).

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 5. Grade repetition and gender distribution†

	Boarding houses		*Comparison*	
	Observations	*%*	*Observations*	*%*
Grade repetition (1st grade)	22	5.2	61	5.9
Female	7	3.6	13	2.6
Male	15	6.5	48	9.0
BHs: boarding $\geqslant$ 1 semester but $\leqslant$2 years	4	5.3		
BHm: boarding > 2 years but < 6 years	5	4.1		
BHl: boarding $\geqslant$ 6 years	7	3.7		
Female	196	45.8	494	48.0
Male	232	54.2	535	52.0

†The number of semesters is missing for six of the boarding house pupils.

Table 6. GPA in the first semester and at the end of the sixth grade†

	Boarding house		*Comparison*	
	Observations	*Mean*	*Observations*	*Mean*
Total population				
GPA (cognitive)—1st semester	367	3.29 (0.55)	969	3.39 (0.60)‡
GPA (social)—1st semester	369	6.37 (0.99)	972	6.57 (0.89)‡
GPA (other)—1st semester	367	3.12 (0.39)	969	3.15 (0.41)
Population with grades in 6th grade				
GPA (cognitive)—1st semester	97	3.42 (0.62)	321	3.34 (0.53)
GPA (social)—1st semester	97	6.21 (1.02)	322	6.46 (0.99)§
GPA (other)—1st semester	97	3.19 (0.49)	321	3.08 (0.32)§§
GPA (cognitive)—6th grade	111	3.93 (0.84)	326	3.90 (0.83)
GPA (social)—6th grade	111	6.82 (0.64)	326	6.86 (0.61)
GPA (other)—6th grade	111	4.03 (0.58)	326	4.07 (0.59)

†Standard deviations are given in parentheses.
‡Significant at 1%.
§Significant at 5%.
§§Significant at 10%.

in 1951, which implies that individuals born in 1941 could only lodge in this boarding house for a maximum of 3 years. Some, but far from all, of the pupils went through 7 years of elementary schooling despite the fact that compulsory school attendance at this point in time was 7 years. Individuals who spent 14 semesters at a boarding house were those who continued to the seventh grade of elementary schooling or had to repeat grades. The reason that some pupils stayed for 16 semesters at a boarding house is that they had to repeat at least one grade.

About 5% of the boarding house pupils had to repeat the first grade (Table 5). The corresponding figure for the comparison group pupils is about 6%. Thus the difference in grade repetition is small and far from statistically significant. The proportion of boys who repeated a grade was higher than for girls. We also note that the proportion of boys in the boarding houses group is greater than in the comparison group, but this difference is again not statistically significant.

Three different indices of GPAs are calculated for the first semester as well as for the sixth

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 7. Educational attainment in 1995 and annual earnings in 1968, 1978, 1988 and 1998 by group†

	Boarding house		*Comparison*		*p-value*
	Observations	*Mean (standard deviation)*	*Observations*	*Mean (standard deviation)*	
Compulsory schooling < 9 years	151	0.404	278	0.303	0.00
Compulsory schooling 9 years	7	0.019	47	0.051	0.01
Upper secondary maximum 2 years	134	0.358	309	0.337	0.47
Upper secondary 3 years	32	0.086	92	0.100	0.41
University < 3 years	29	0.078	77	0.084	0.70
University > 3 years	21	0.056	107	0.117	0.00
Doctoral studies	0	0.000	6	0.007	0.12
Earnings (SEK)					
1968	348	128921 (90593)	825	149981 (105118)	0.00
1978	411	168850 (176218)	987	189037 (232818)	0.01
1988	397	195181 (388238)	965	207683 (455076)	0.02
1998	370	214437 (840982)	908	219373 (871288)	0.56

†Educational attainment is obtained from the Swedish register of education. Earnings are measured from the tax register. Earnings include, for example, income from self-employment, sickness insurance, unemployment insurance and pensions (old age and early retirement). Standard deviations are given in parentheses. *p*-value denotes the probability of rejecting equal means.

grade of elementary schooling. Unfortunately, grades in the sixth grade are available for only four schools. Table 6 displays mean GPAs for each subclass of skills (cognitive, social and other) for the two groups of pupils. It is clear that the GPA in the first semester for the boarding house individuals is lower than for the comparison group on all three accounts.

The differences in cognitive and social skills between groups are statistically significant. The difference in grades for the other subjects is not statistically significant, however. When we turn to the sample for which we have data on grades in the sixth grade, the differences in social skills are again statistically significant but now the boarding houses pupils have a higher GPA in the other subjects. The boarding house pupils also perform marginally better than the comparison group when it comes to cognitive skills in the sixth grade. On the other two GPA measures they perform marginally worse in the sixth grade.

Educational attainment in 1995 and earnings in the years 1968, 1978, 1988 and 1998 for the two groups are displayed in Table 7. Boarding house individuals achieve, on average, a lower educational attainment than the comparison group. About 42% of the boarding house individuals have only compulsory schooling, whereas the corresponding figure for the comparison group is 35%. There are also differences at the other end of the distribution. The share who graduated from university is 7 percentage points higher for the comparison group than for the boarding house group.

Earnings for the comparison group are higher than for the boarding house pupils at all points in time. The differences in income for the period 1968–1988 are quite stable at around SEK 20000. In 1998, the difference is much smaller and not statistically significant. The most likely explanation for the change in income differences is that by 1998 some individuals had left the labour market. In 1998 the age of the oldest cohort was 66 years, and the mandatory retirement age in Sweden was at the time 65 years. Also, 31% of our sample was on early retirement schemes

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 8. Parameter estimates of the selection to boarding houses and the number of semesters†

Dependent variable	*Boarding houses*		*ln(number semesters)*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Socio-economic status of mother: reference category SEI(3–5,8,11)					
SEI(1–2)	0.47‡	0.51‡	0.15	0.15	−0.08
	(0.18)	(0.18)	(0.13)	(0.13)	(0.13)
SEI(6–7)	−0.56‡	−0.48‡	0.31§	0.30	0.03
	(0.18)	(0.19)	(0.19)	(0.19)	(0.18)
SEI(12)	−0.88‡	−0.83‡	0.10	0.13	−0.39
	(0.29)	(0.30)	(0.20)	(0.21)	(0.36)
SEI(M)	0.01	0.11	0.03	−0.05	−0.06
	(0.13)	(0.15)	(0.12)	(0.15)	(0.10)
Socio-economic status of father: reference category SEI(3–5,8,11)					
SEI(1–2)	0.01	0.03	−0.21	−0.24	−0.10
	(0.20)	(0.20)	(0.13)	(0.15)	(0.13)
SEI(6–7)	0.38§§	0.39§§	−0.35§§	−0.36§	−0.07
	(0.19)	(0.20)	(0.18)	(0.19)	(0.16)
SEI(12)	−0.11	−0.11	−0.07	−0.09	0.13
	(0.29)	(0.30)	(0.16)	(0.18)	(0.18)
SEI(M)	0.04	−0.15	−0.06	−0.07	−0.04
	(0.29)	(0.17)	(0.11)	(0.12)	(0.10)
Female	−0.00	−0.00	−0.15‡	−0.14§§	−0.08
	(0.08)	(0.08)	(0.06)	(0.07)	(0.08)
GPAca	−0.27‡	−0.27‡	−0.15	−0.13	−0.08
	(0.10)	(0.10)	(0.10)	(0.10)	(0.120)
GPAsa	−0.01	−0.01	0.02	0.02	0.04
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
GPAo	0.18	0.18	0.09	0.09	−0.04
	(0.14)	(0.15)	(0.12)	(0.13)	(0.15)
Income (SEK) father/10000		0.04		0.01	
		(0.04)		(0.05)	
Income (SEK) mother/10000		−0.28§§		−0.03	
		(0.12)		(0.10)	
Father's income missing		0.28§§		0.00	
		(0.14)		(0.11)	
Mother's income missing		−0.08		0.11	
		(0.10)		(0.09)	
Distance (km)/100					0.50
					(0.50)
Controls for school and year	Yes	Yes	Yes	Yes	Yes
Observations	1336	1336	335	222	222
Pseudo-R^2/R^2	0.16	0.164	0.24	0.24	0.29

†Estimation of columns (1) and (2) and (3)–(5) is performed with probit maximum likelihood estimators and ordinary least squares estimators respectively. Robust standard errors are given in parentheses.
‡Significant at 1%.
§Significant at 5%.
§§Significant at 10%.

in 1998. Since income from pensions is a fraction of previous earnings this will imply a reduction in the difference in income between the two groups.

Ordinary probit regression models are used to describe the selection to the boarding houses. Log-linear regression models are used in the description of the number of semesters of boarding. In these regressions, we include family background variables (SEI and income) and GPA in the

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

first semester. We have regrouped the SEI-code into five new classes. The first group, SEI(1–2), contains parents with SEI-codes 1 and 2; the second group, SEI(6–7), consists of parents with SEI-codes 6 and 7. The third group, SEI(3–5,8,11), consists of parents with the SEI-codes 3, 4, 5, 8 and 11. The final group, SEI(12), consists of unemployed parents, i.e. those with SEI-code 12. We have also created the group SEI(M) for those parents for whom we lack information about socio-economic status. The results from these regressions are shown in Table 8.

Columns (1) and (2) in Table 8 give the results from the probit regressions on boarding houses. In column (1) we do not control for family income. In comparison with the omitted category, SEI(3–5,8,11), consisting of entrepreneurs, business executives, service industry workers or students, boarding house pupils are relatively speaking more likely to have mothers working in farming and forestry; their mothers are less likely to be civil servants, blue-collar workers (SEI(6–7)) or unemployed (SEI(12)). They are also more likely to have fathers working as civil servants or blue-collar workers (SEI(6–7)). Further, they have lower cognitive GPAs in the first semester. The results when controls for the income of the mothers and fathers in 1968 are included are displayed in column (2). We impute missing incomes with the median income and in the regression we control for the imputation by adding two dummy variables. We obtain the same picture as in column (1) on the SEI and on the first-semester cognitive GPA but now boarding house pupils are also more likely to have had a father with missing information on income and a low income mother.

Turning to the boarding house sample and the determinants of the number of semesters we find very few significant associations. According to column (3), based on 335 boarding house students, the number of semesters increases by 31% if the mother is a civil servant or a blue-collar worker, SEI(6–7); in contrast, the number of semesters decreases by 35% if the father is a civil servant or a blue-collar worker. The number of semesters is 15% lower for girls. The result when we control for (imputed) income is presented in column (4). We can see that the parameter for the income variable is not statistically significant and that the inference basically stays the same. The result when we add the distance to the nearest school is displayed in column (5). The sample is now reduced to only 222 individuals and no parameter in the regression equation is statistically significant.

All in all,

(a) pupils from the boarding houses seem to have come from somewhat less advantageous circumstances than the comparison pupils, and
(b) we cannot observe any apparent signs of selection of length of stay at the boarding houses.

We also performed the same analysis as above on the sample for which we have data on grades in grade 6. Since the sample size is smaller there are fewer statistically significant differences. However, the general pattern of the boarding house pupils being negatively selected remains for this sample.

5. Analysis and results

We first study the short-term effect of boarding by estimating the effect of the number of semesters spent at the boarding houses on GPA in the sixth grade (cognitive, social and other subjects separately); thereafter we turn to the long-term effects on educational attainment and earnings. We control for age when starting compulsory school, gender and school, using fixed effects. We include parental variables measured in 1960 and 1968 as control variables, and in some specifications we also control for the potentially endogenous first-semester grades.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

5.1. Short-term effects

In this section we analyse the effect of boarding on the pupils' GPA in the sixth grade. Unfortunately, we could only observe these grades for four schools. However, since there are very small differences in the observed covariates between the two groups, we believe that the results for the short-term effects can be generalized to the group of boarding house pupils.

Instead of just using a boarding house indicator variable we make use of the information that treatment intensity varies (see Table 4 for the distribution of the number of semesters). If there is a conducive learning out-of-school effect from living in a boarding house, we would expect a monotonously increasing effect from the number of semesters. Even if there is selection to boarding houses, in general, we can use treatment intensity to test for a conducive learning out-of-school effect. Note that any effect from treatment intensity from the more conducive learning environment could be strengthened because of positive peer effects. Note also that our treatment intensity effect would be biased downwards if weaker students repeat grades and therefore stay in the boarding house for a longer period of time.

To ensure that the results are not driven by functional form, we classify the number of semesters into three groups; those with 2 years or less at the boarding house, *BHs*, more than 2 but less than 6 years, *BHm*, and, finally, those with 6 years or more, *BHl*. The first group of individuals consists of 76 pupils, whereas the second and third groups consist of 122 and 191 pupils respectively. For 39 pupils there is missing information on the number of semesters. These pupils were excluded from this analysis.

The results from the estimations with a complete set of control variables are found in Table 9 (all estimated parameters, with different sets of control variables, are for completeness displayed in Tables B3–B5 in the on-line supplement). The results suggest a monotonously increasing effect of the number of semesters on the cognitive GPA. We find a positive and statistically significant effect on the cognitive GPA for pupils with 6 years or more at a boarding house. This effect is statistically significant when controlling for year, school and the SEI-status of the parents. The estimate in column (1) in Table 9 implies that 6 years or more at a boarding house would

Table 9. Effect of the number of boarding house semesters on GPA in the sixth grade (cognitive, social and other)†

	(1), cognitive	*(2), social*	*(3), other*
BHs: boarding $\geqslant$ 1 semester but $\leqslant$ 2 years	−0.391	−0.081	−0.224
	(0.254)	(0.255)	(0.253)
BHm: boarding > 2 years but < 6 years	−0.052	−0.051	−0.158
	(0.164)	(0.132)	(0.114)
BHl: boarding $\geqslant$ 6 years	0.277‡	0.086	0.170§
	(0.136)	(0.085)	(0.090)
Controls for grades in 1st semester	Yes	Yes	Yes
Controls for school and year	Yes	Yes	Yes
Controls for socio-economic status of parents	Yes	Yes	Yes
Observations	417	417	417
R^2	0.35	0.18	0.36

†Estimation by ordinary least squares. Robust standard errors are given in parentheses. The controls for socio-economic status consist of five dummy variables for both fathers and mothers. Interaction terms between these variables are also included in the specifications.
‡Significant at 5%.
§Significant at 10%.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

lead to an increase in the cognitive GPA of about 0.28 units, 0.34 standard deviations, or about 7% (0.28/3.90) for the average student. We find no effect of boarding houses on our measure of social skills GPA (see column (2)). Still the estimates are monotonously increasing with respect to the number of semesters. One potential explanation for the insignificant results is that there is too little variation in the measure. From Table 6 we can see that the coefficient of variation is about half the coefficient of variation for the other GPAs. It is, however, interesting to note that the social skills measure picks up some variation in behaviour as the magnitude of the estimate of the girls' dummy variable is the same as for the cognitive skills GPA measure (see Table B4 in the on-line supplement).

The effect on our GPA measure of other subjects is also monotonously increasing with the number of semesters at the boarding houses (see column (3)). The effect on those with 6 years or more at a boarding house is positive and statistically significant at the 6% level. This effect is somewhat smaller than the effect found on cognitive skills.

It is worth emphasizing that the results that are displayed in Table 9 are not contingent on the inclusion of GPA in the first semester (see columns (3) and (4) in Tables B3–B5 in the on-line supplementary material). If unobserved heterogeneity had been driving the results, we would have expected that adding the first-semester GPA would have affected the estimate on boarding.

We have seen that there are positive effects on the GPA for pupils with 6 or more years at a boarding house, and these results are not contingent on the inclusion of the GPA in the first semester. From Table 8, columns (3)–(5), we can observe that the numbers of semesters are not correlated with initial ability (i.e. GPA in the first semester). Hence, there seems to be no sorting on initial ability on the number of semesters at a boarding house. However, the result of an increasing effect could, potentially, be due to endogenous sorting. In other words, pupils who gain from boarding stay on whereas those who do not drop out. It is difficult to discriminate between a pedagogical effect and effects from endogenous sorting. However, the families seem to have had little choice, i.e. the placement was based on the distance to the closest school. Since there is an increasing fraction leaving early over time the most likely explanation is that the majority of dropouts from the analysis were subsequently transported to school instead of boarded. Endogenous sorting could potentially stem from geographic mobility. This option is not very likely either, since most of the families were poor, putting financial restrictions on their mobility.

Under the assumption that the effect of pedagogical stimuli is higher for individuals with poor initial skills than for individuals with high initial skills the hypothesis of a pedagogical effect can be tested given that GPA in the first semester is a good measurement of initial ability. Note that any differences in effects over the initial GPA distribution cannot stem from endogenous sorting since there is no correlation between the cognitive GPA in the first semester and the number of semesters at a boarding house (conventional χ^2- and non-parametric regressions were used in the test).

Table 10 presents heterogeneous treatment effects by initial skills. The effects in all boarding house groups are now positive. The effects on cognitive and other GPAs are statistically significant for the boarding house pupils with more than 2 years at a boarding house and the effects decrease with better initial skills. The effect on GPA would be positive for more than 5% of the pupils if they stayed 2 years or less at a boarding house (i.e. 5.3% of the pupils have less than 2.4 on their cognitive GPA in the first semester, which is the cut-off value when the effect becomes negative). The effect would be positive for more than 40% of the pupils if they stayed for more than 2 years, but less than 6 years (41.3% have a cognitive GPA in the first semester that is lower than a cognitive GPA of 3.2). Finally, the effect would be positive for more than 90% of the pupils if they stayed 6 years or more at the boarding house (90.3% have a cognitive GPA in the first semester which is lower than a cognitive GPA of 4.1).

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 10. Heterogeneous treatment effects in the sixth grade by initial skills†

	(1), cognitive	*(2), social*	*(3), other*
BHs: boarding $\geqslant$ 1 semester but $\leqslant$2 years	1.082	1.067	0.319
	(1.732)	(1.555)	(2.232)
BHm: boarding > 2 years but < 6 years	1.457§	0.194	1.510‡
	(0.666)	(0.462)	(0.385)
BHl: boarding $\geqslant$ 6 years	1.653‡	−0.065	0.955§
	(0.569)	(0.455)	(0.425)
BHs×(GPA cognitive skills in 1st semester)	−0.444	−0.346	−0.162
	(0.552)	(0.510)	(0.669)
BHm×(GPA cognitive skills in 1st semester)	−0.455§	−0.071	−0.501‡
	(0.214)	(0.151)	(0.126)
BHl×(GPA cognitive skills in 1st semester)	−0.409§	0.044	−0.235§§
	(0.165)	(0.122)	(0.123)
GPA cognitive skills in 1st semester	0.916‡	0.018	0.507‡
	(0.106)	(0.087)	(0.073)
GPA social skills in 1st semester	0.032	0.057§§	0.020
	(0.038)	(0.034)	(0.027)
GPA other skills subjects in 1st semester	−0.134	−0.086	0.094
	(0.132)	(0.136)	(0.093)
Female = 1	0.216‡	0.281‡	0.237‡
	(0.077)	(0.062)	(0.055)
Controls for school and year	Yes	Yes	Yes
Controls for socio-economic status of parents	Yes	Yes	Yes
Observations	417	417	417
R^2	0.37	0.18	0.38

†Estimation by ordinary least squares. Robust standard errors are given in parentheses. The controls for socio-economic status consist of five dummy variables for both fathers and mothers. Interaction terms between these variables are also included in the specifications.
‡Significant at 1%.
§Significant at 5%.
§§Significant at 10%.

Previous research (see Section 1) focused on effects on socially disadvantaged children. To increase our understanding of the results from the heterogeneity analysis further we estimated the same model as above but with an indicator equal to 1 if at least one of the parents was unemployed and 0 if neither of the parents was unemployed. The precision of the estimates (which is not displayed) is lower but the pattern is the same as displayed above. Hence, this suggests that targeting children according to their socio-economic status for, for example, after-school programmes is most probably justified.

5.1.1. Sensitivity analyses

The descriptive statistics on parental earnings, wealth and SEI-categories show that the boarding house pupils came from environments with worse prerequisites than the non-boarding pupils. We have controlled for these observed differences and, in addition, we have also controlled for grades in the first semester. If there is still some unobserved ability remaining, this is potentially negatively correlated both with being boarded and GPA. This therefore implies that we would underestimate the effect of boarding (or pedagogic stimuli).

There is one potential source of upward bias in our estimates. Some pupils had Finnish as their mother tongue. If more boarding house pupils had Finnish as their mother tongue compared with the comparison pupils, our 'value-added' approach may be biased upwards, since initial

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

cognitive GPA (mathematics, reading and writing) may be low just because of poor initial knowledge of Swedish. To test for this potential problem, we have regressed the number of boarding house semesters on the grades in mathematics only. This is done since grades in mathematics are less likely to be affected by pupils' knowledge of Swedish. The effect for the pupils with more than 6 years at a boarding house is 0.32 and statistically significant at the 10% level. Hence, we believe that our estimates are not biased because of there being more Finnish-speaking children among the boarding house pupils than among the comparison group.

We have performed several sensitivity checks, all of which provide evidence of an effect of boarding on grades. Among others, we have included

(a) models with just a dummy indicator for boarding house (the results are in accordance with those reported in Table 9 but they are less precise (p-value less than 0.2)),
(b) models where we control for the GPA of all subjects in the first semester, which produces exactly the same results as above, and
(c) a model with the GPA for all subjects as the dependent variable (the effect, for the pupils with 6 years or more at a boarding house, is 0.18 (p-value less than 0.04)).

Finally, we estimated sibling fixed effects models. Unfortunately we only have 27 boarding house students with a non-boarding sibling, which is why the precision of the estimates is low. The point estimates are, however, very close to the estimates that are displayed in Table 9 and we cannot, on the basis of this analysis, reject the results that are displayed in Table 9.

All in all, we have found that boarding has an effect on cognitive GPA for those who stayed 6 years or more at a boarding house. We also found that the effects of boarding are greater for those with a poor initial ability (measured as GPA in the first semester). This effect supports the idea that the estimated average effect stems from an increase in pedagogical stimuli at the boarding houses.

The problem of only short- or medium-term effects of policy interventions is well documented (see for example Currie (2001) and Heckman and Masterov (2007)). One potential problem with our analysis is that the observed effects of boarding for 6 years or longer are simply because the pedagogical effects are short lived, i.e. those with the fewest number of boarding semesters have, on average, had the treatment at an earlier stage than those with the most semesters. However, since we also are studying long-run effects this leaves room for testing for the potential possibility of just short-lived effects. If the effect of boarding is short lived, we shall not observe differences in long-run effects of the number of semesters of boarding.

5.2. Long-term effects

We first estimate the effect of the number of semesters on educational attainment by using an ordered probit model. We then examine how earnings are affected by the number of semesters at the boarding houses. In these estimations we make use of the complete data set; thus we do not restrict the estimation to the subsample where we observe sixth-grade outcomes (results based on the restricted sample are qualitatively the same as the results based on the complete sample).

The distribution of educational attainments for the two groups is displayed in Table 7. Pupils in the comparison group were overrepresented among those with a higher level of education. The results in Table 11 have the same flavour. The ordered probit coefficients for the two groups with less than 6 years at a boarding house are negative (p-value less than 0.001). For the group with 6 years or more the estimate is negative, but not statistically significant at any reasonable level of risk. By and large, these patterns remain when we add more control variables; for the pupils with more than 6 years at a boarding house the effect becomes positive, albeit not statistically significant at any reasonable level of risk.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 11. Effect of the number of boarding house semesters on educational attainment in 1995†

	(1)	*(2)*	*(3)*	*(4)*
BHs: boarding $\geqslant$ 1 semester but $\leqslant$ 2 years	−0.445‡	−0.342§	−0.327§	−0.369§
	(0.141)	(0.145)	(0.148)	(0.162)
BHm: boarding > 2 years but < 6 years	−0.491‡	−0.323‡	−0.291§	−0.282§
	(0.115)	(0.119)	(0.122)	(0.138)
BHl: boarding $\geqslant$ 6 years	−0.116	−0.025	0.025	0.002
	(0.090)	(0.098)	(0.102)	(0.107)
Female = 1		0.181‡	0.173‡	0.033
		(0.061)	(0.062)	(0.068)
GPA cognitive skills in 1st semester				0.392‡
				(0.086)
GPA social skills in 1st semester				0.012
				(0.043)
GPA other skills in 1st semester				0.383‡
				(0.122)
Controls for school, year, age and age^2	No	Yes	Yes	Yes
Controls for socio-economic status of parents	No	No	Yes	Yes
Observations	1257	1257	1257	1159
Pseudo-R^2	0.01	0.03	0.04	0.06

†Estimation by ordered probit maximum likelihood. Educational attainment is an ordered variable with seven levels: 1, compulsory schooling < 9 years, 2, compulsory schooling 9 years, 3, upper secondary maximum 2 years, 4, upper secondary 3 years, 5, university < 3 years, 6, university > 3 years, and 7, doctoral studies. Descriptive statistics are provided in Table 7. Robust standard errors are given in parentheses. The controls for socio-economic status consist of dummy variables for both fathers and mothers together with interactions.
‡Significant at 1%.
§Significant at 5%.

To obtain an idea of the magnitude of the effect of the intensity of treatment on the educational attainment that is displayed in Table 11, we calculated the counterfactual probability at each level of education for an individual who boarded for 6 years or more and looked at what would have happened if they had been boarders for a shorter period of time. This is done by calculating the marginal effects at each level and subsequently dividing by the proportion of individuals at each level of education (see Table 7). All in all, we found that living in a boarding house for 6 years or more had a great effect. For example, the probability of completing less than 9 years of education was found to be 32% ($100 \times 0.13/0.404$) higher if a pupil spent under 2 years at a boarding house and 25% ($100 \times 0.10/0.404$) higher if a pupil spent 2–6 years in a boarding house. The probability of gaining a university degree on a 3-year course was reduced by 42% ($100 \times (-0.033/0.078)$) and 33% ($100 \times (-0.026/0.078)$) for a pupil in either of the two groups whereas the probability of gaining a university degree requiring over 3 years was reduced by around 73% ($100 \times (-0.041/0.056)$ and 61% ($100 \times (-0.034/0.056)$).

Again we have performed some sensitivity checks. For instance we have considered broader educational groups and we have estimated ordinary least squares linear regression models. These sensitivity checks all yielded the same result: negative effects among the pupils with less than 6 years of boarding and no effects of boarding on the pupils with 6 years or more at a boarding house.

Table 12 presents ordinary least squares estimates from regressions of log-earnings, pooled over the years 1968, 1978, 1988 and 1998, on the number of boarding house semesters for various sets of control variables. The first thing to note is that adding control variables does very little to the estimates. The two groups with less than 6 years at a boarding house have lower earnings

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 12. Effect of the number of boarding house semesters on earnings†

	(1)	(2)	(3)	(4)
BHs: boarding $\geqslant$ 1 semester but $\leqslant$ 2 years	−0.253	−0.167	−0.159	−0.192
	(0.180)	(0.120)	(0.124)	(0.129)
BHm: boarding > 2 years but < 6 years	−0.233	−0.261§	−0.224§	−0.212‡
	(0.155)	(0.107)	(0.109)	(0.121)
BHl: boarding $\geqslant$ 6 years	−0.034	−0.017	0.030	0.078
	(0.118)	(0.088)	(0.092)	(0.094)
Female = 1				−0.738§§
				(0.062)
GPA cognitive skills in 1st semester				0.167‡
				(0.077)
GPA social skills in 1st semester				0.014
				(0.038)
GPA other skills in 1st semester				0.135
				(0.100)
Controls for school, year, age and age^2	No	Yes	Yes	Yes
Controls for socio-economic status of parents	No	No	Yes	Yes
Observations	5075	5075	5075	4676
R^2	0.001	0.535	0.538	0.545

†Estimation by ordinary least squares. The earnings regressions have been run on pooled data for the years 1968, 1978, 1988 and 1998. Standard errors, estimated by clustering at the individual level, are displayed in parentheses. The controls for socio-economic grouping consist of five dummy variables for both fathers and mothers. Interaction terms between these variables are also included in the specifications.
‡Significant at 10%.
§Significant at 5%.
§§Significant at 1%.

compared with the comparison group. The effects are statistically significant at the 10% level for the boarding house pupils with more than 2 but less than 6 years for the model with the full set of control variables (see column (4)). The estimated effect on the group with 6 years or more at a boarding house is positive when we control for the socio-economic status of the parents as well as when we control for both GPAs in the first semester and the socio-economic status of the parents (see columns (3) and (4)). The effects are, however, far from statistically significant. It may also be worth noting that the lifetime income of the pupils who were boarders for 6 years or more is around 28% higher than the income of the other children who were boarders (p-value less than 0.09 for those with less than 2 years and the p-value less than 0.06 for those with 2 years but under 6 years).

Turning to heterogeneous long-term effects, the results on educational attainment and lifetime earnings are presented in Table 13. There is some evidence of a statistically significant (p-value less than 0.05) effect on educational attainment (see column (1)) for those boarding house individuals with low grades in cognitive subjects who stayed between 2 and 6 years at their boarding house. For these individuals the effect is, on average, positive for the pupils with the 12.1% lowest first-semester cognitive GPAs (GPA less than 2.8). For those with longer periods of boarding the estimate is positive for pupils with the 46.2% lowest GPAs (GPA less than 3.3). This estimate is, however, not statistically significant (p-value less than 0.25).

Regarding the estimated effects on earnings (see column (2) in Table 13) there are no significant effects of boarding. There is a positive estimate for the boarding house pupils who stayed more than 6 years at the boarding house, but the effect is not statistically significant (p-value less than 0.2).

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Table 13. Heterogeneous long-term effects on educational attainment and earnings†

	(1), educational attainment	*(2), earnings*
BHs: boarding $\geqslant$ 1 semester but $\leqslant$ 2 years	−1.679	−0.878
	(1.179)	(1.042)
BHm: boarding > 2 years but < 6 years	1.530‡	−0.358
	(0.737)	(0.720)
BHl: boarding $\geqslant$ 6 years	0.712	0.618
	(0.627)	(0.488)
BHs × GPA cognitive skills	0.384	−0.164
	(0.340)	(0.142)
BHm × GPA cognitive skills	−0.551‡	0.043
	(0.220)	(0.213)
BHl× GPA cognitive skills	−0.213	0.206
	(0.185)	(0.307)
GPA cognitive skills	0.445§	0.168‡
	(0.092)	(0.081)
GPA social skills	0.015	0.016
	(0.043)	(0.038)
GPA other subjects	0.392§	0.146
	(0.122)	(0.101)
Female	0.023	−0.742§
	(0.068)	(0.062)
Controls for school, year, age and age^2	Yes	Yes
Controls for socio-economic status of parents	Yes	Yes
Observations	1159	4676
R^2/pseudo-R^2	0.05	0.54

†Estimation by ordered probit maximum likelihood (column (1)) and ordinary least squares (column (2)). Robust standard errors are given in parentheses. The controls for socio-economic status consist of five dummy variables for both fathers and mothers. Interaction terms between these variables are also included in the specifications.
‡Significant at 5%.
§Significant at 1%.

5.2.1. *Sensitivity analyses*

We have performed extensive sensitivity checks. We have

(a) controlled for the total GPA in the first semester instead of the subdivisions into cognitive skills, social skills and other subjects,
(b) estimated regression models with earnings instead of the models with log-earnings and
(c) estimated models with the number of boarding house semesters and semesters squared.

The results above are all robust to these respecifications. Finally, we have estimated within-sibling fixed effects models. The point estimates are very similar to the estimates that are shown in Tables 11 and 12. The confidence intervals are, however, almost three times as great.

5.2.2. *Summing up*

We found no evidence of positive long-term effects of boarding *versus* not boarding. The boarding house pupils with 6 years or more have less advantageous family backgrounds than the non-boarding pupils. Despite this difference they did not achieve a less formal level of education or reach a lower level of income (see columns (4) in Tables 11 and 12). Hence, the increased pedagogical stimuli might have equalized the family background differences. The

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

lack of long-term effects may possibly be due to liquidity constraints or it may, perhaps, be a result of a reduced demand for future studies stemming from possible vocational training obtained from doing advanced chores like sewing, knitting and carpentering at the boarding houses.

To test for the possible effects of liquidity constraints, we added the income and wealth of the mothers and fathers to the regressions, but the results from the regression were basically unaffected. The problem with these estimates is that only 393 observations (out of in total 1159) include information on parental income and, hence, we cannot exclude the possibility that the results that are given in Tables 11 and 12 are explained by financial constraints.

The interpretation that the nil effect stems from liquidity constraints is supported by the observed positive effect on educational attainment and income for the pupils who were boarders for 6 years or more when compared with the other boarding house pupils (i.e. those who stayed for less than 6 years). The reason for this conclusion is that we could not discover any difference with respect to parental income and socio-economic status for the two groups of boarding house students (see Table 8). Thus, it is likely that both groups faced the same liquidity constraints and therefore, as a result of the increased cognitive ability that is obtained from staying longer at the boarding house, we can observe an increase in both educational attainment and income. Thus, it is likely that this long-term effect stems from the increase in cognitive ability due to the more conducive learning environment at the boarding houses compared with the children's relatively disadvantaged rural house environment. This fact also provides strong evidence that the effects on scholastic achievement that were found in Section 5.1 are not just a result of a short-lived pedagogical effect.

6. Conclusions

This paper examines the effects of an environment that is conducive to learning (tutoring, books and time for homework) in elementary school on grades, educational attainment and earnings. We use data on boarding house pupils, born between 1932 and 1941, who attended regular public schools close to the boarding houses. At these boarding houses pupils had daily scheduled time for doing their homework. Hence, the boarding house pupils were exposed to a pedagogical influence that the other pupils in the same school were not. The placement at the boarding houses was based on the distance from their place of residence to the nearest school and had no direct connection with the pupils' skills.

We find a 7% or 0.34 standard deviation increase in the GPA in the sixth grade for pupils who lived in boarding houses for 6 years or more. For the boarding house pupils who stayed for a shorter period of time we could not find any effect on the GPA in the sixth grade. In a heterogeneity analysis, where we estimated the effect of boarding houses conditionally on the cognitive GPA in the first semester, we found greater effects for pupils who initially had poor cognitive skills. This supports the idea that the boarding houses had a pedagogical effect that enhances cognitive skills. We also found some weak evidence that pupils with poor initial skills and many semesters at the boarding house also increased their educational attainment. In accordance with this result, we also found that the children of unemployed parents gained more from being boarders than the children with employed parents. Thus, this result suggests that targeting children by using socio-economic status for, for example, out-of-school programmes is most probably justifiable.

The effect of increasing the GPA by 0.34 standard deviations corresponds well to the results on effects of out-of-school time programmes. Lauer *et al.* (2006) performed a meta-analysis

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

of 35 studies on out-of-school time programmes directed at disadvantaged US K-12 students. The average effect of 30 studies on reading and 22 studies on mathematics are estimated at 0.13 and 0.17 standard deviations respectively. Studies, however, vary enormously. 15 and 13 of the studies on reading and mathematics respectively find effects that are greater than 0.34 standard deviations. In their estimation, Lauer *et al.* (2006) controlled for different mediators, e.g. duration of programme, quality, sample and size. Interestingly, the most important mediator is, at least for mathematics, the duration of time spent in the programmes.

All in all, this paper supports the results that are found in Entwistle *et al.* (2007) and Rønning (2011) about the importance of the family environment (the presence of books, magazines and parents reading and tutoring) but also the claim that early interventions (see Heckman and Masterov (2007) for a survey) are important for equalizing skills in elementary schooling. However, at the time of our data (in the 1950s) there were only very small effects (if any) on educational attainment and lifetime earnings.

How should this last result be interpreted? It is clear that individuals who stayed in boarding houses for 6 years or more had less advantageous family backgrounds compared with the non-boarding individuals. Despite this difference they did not achieve a lower level of formal education or earned a lower income than the latter. Hence, the increased pedagogical stimuli might have equalized the differences in family background. Because of liquidity constraints, individuals from poorer socio-economic circumstances needed to have a greater ability, on average, than students from better socio-economic circumstances to motivate themselves to pursue further studies after the compulsory 6 or 7 years of elementary school.

This interpretation of a liquidity constraint for further education is supported by the fact that the boarding house individuals who boarded for 6 years or more gained a higher educational attainment and a higher income than their peers (with similar family backgrounds) who stayed at the boarding houses for a shorter period of time. Thus, it is highly likely that this effect stems from the increase in cognitive ability due to the more conducive learning environment at the boarding houses compared with the home environment.

There seems to be a general understanding today, at least in the developed parts of the world, that financial restrictions are less important than the socio-economic environment (see Kane (1994), Dynarski (2000), Cornwell *et al.* (2006), Penn and Kyle (2007), Chapman (2005), Baumgartner and Steiner (2006) and Heckman and Masterov (2007)). However, in the 1940s and 1950s, income liquidity constraints seem to have been important for further studies, particularly higher education. During this time period, the Swedish study grant system was very diverse and there was no general system for financing higher education. There existed a variety of options and even though the different systems were expanded during the 1940s it must have been difficult for students from poor backgrounds to finance higher studies. In 1944 the government study grant amounted to SEK 3 million. In 1963 this amount had increased to SEK 145 million (Statens Offentliga Utredningar (1963), page 74). A general study grant was introduced in Sweden in 1965.

What can be learnt from this paper in terms of the current policy discussion? If the aim is to reduce social stratification in education, there are two policy options: either

(a) remove existing homework or
(b) offer qualified assistance in out-of-school time programmes to help students to do their homework.

These two conclusions are supported by recent data on after-school time programme studies (see for example Lauer *et al.* (2006)) and Rønning (2011) who convincingly show that homework amplifies existing inequalities through complementarities with home inputs.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Acknowledgements

The authors are grateful for helpful comments from the Joint Editor, Per-Anders Edin, Peter Fredriksson, Bertil Holmlund, Mikael Lindahl, Fay Lundh Nilsson, participants at the 7th Research Training Network Meeting, December 2008, in Budapest and two referees. Archival help from the municipality archives is gratefully acknowledged. We are also most grateful to Staffan Johansson at the *Folkrörelsernas arkiv* in Luleå and Åke Jalo at Statistics Sweden in Örebro. Financial support from the Swedish Council for Working Life and Social Research is acknowledged.

Appendix A

Grades were based on letters and given on a seven-point scale except for the subjects conduct and order, where grades were given on a three-point scale (the grades in these subjects were A, B and C). The highest grade on the seven-point scale was A which corresponds to a pass with distinction. C was the lowest grade and corresponds to a fail. The grade scale in descending order was A, a, AB, Ba, B, Bc and C. The grades have subsequently been converted into numeric values to be able to create GPAs for each individual. The highest grade, A, is given the numeric value of 7. This continues for all grades until grade C, which is given the numeric value 1. Teachers could also reward pupils with either a plus or a minus for each subject grade. A grade with a plus is rewarded with a one-third extra credit when calculating the GPA. If there is a minus, a third is deducted from the relevant score. In some cases there are missing values in individual subjects and in these cases that subject is disregarded when the GPA is calculated.

References

Almond, D. and Currie, J. (2011) Human capital development before age five. In *Handbook of Labor Economics* (eds O. Ashenfelter and D. Card), vol. 4B, pp. 1315–1486. New York: North-Holland.

Andersson, C. (2007) Teachers and student outcomes: evidence using Swedish data. Department of Economics, Uppsala University, Uppsala.

Bae, S., Oh, H., Kim, H., Lee, C. and Oh, B. (2010) The impact of after-school programs on educational equality and private tutoring expenses. *Asia Pacif. Educ. Rev.*, **11**, 349–361.

Baumgartner, H. J. and Steiner, V. (2006) Does more generous student aid increase enrolment rates into higher education?: evaluating the German student aid reform of 2001. *Discussion Paper 563*. German Institute for Economic Research, Berlin.

Chapman, B. (2005) Income contingent loans for higher education: international reform. *Discussion Paper 491*. Centre for Economic Research, Australian National University, Canberra.

Cooper, H., Nye, B., Charlton, K., Lindsey, J. and Greathouse, S. (1996) The effect of summer vacation on achievement test scores: a narrative and meta-analytic review. *Rev. Educ. Res.*, **66**, 227–268.

Cornwell, C., Lee, K.-H. and Mustard, D. B. (2006) Student responses to merit scholarship retention rules. *J. Hum. Resour.*, **40**, 895–917.

Currie, J. (2001) Early childhood education programs. *J. Econ. Perspect.*, **15**, 213–238.

De Geer, E. (1986) *De Finländska Krigsbarnen i Sverige 1941-1948*. Stockholm: Föreningen Norden.

De Kanter, A. (2001) After-school programs for adolescents. *Natn. Ass. Second. Schl Princps Bull.*, **85**, 12–21.

Dynarksi, S. (2000) Hope for whom?: financial aid for the middle class and its impact on college attendance. *Working Paper 7756*. National Bureau of Economic Research, Cambridge.

Entwistle, D. R., Alexander, K. L. and Steffel Olson, L. (2007) Lasting consequences of the summer learning gap. *Am. Sociol. Rev.*, **72**, 167–180.

Fryer, Jr, R. G. and Levitt, S. D. (2004) Understanding the black-white test score gap in the first two years of school. *Rev. Econ. Statist.*, **86**, 447–464.

Halpern, R. (2002) A different kind of child development institution: the history of after-school programs for low-income children. *Teach. Coll. Rec.*, **104**, 178–211.

Heckman, J. J. and Masterov, D. V. (2007) The productivity argument for investing in young children. *Rev. Agric. Econ.*, **29**, 446–493.

Heyns, B. (1978) *Summer Learning and the Effects of Schooling*. New York: Academic Press.

Kane, T. (1994) College entry by blacks since 1970: the role of college costs, family background, and the returns to education. *J. Polit. Econ.*, **102**, 878–911.

Lauer, P. A., Akiba, M., Wilkerson, S. B., Apthorp, H. S., Snow, D. and Martin-Glenn, M. (2006) Out of school time programs: a meta-analysis of effects for at-risk students. *Rev. Educ. Res.*, **76**, 275–313.

Lindahl, M. (2007) Achievement, schooling and family background: evidence for Sweden. *Mimeo*. Swedish Institute for Social Research, Stockholm University, Stockholm.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms

Maddison, A. (2001) *The World Economy: Historical Statistics*. Paris: Organisation for Economic Co-operation and Development.
Penn, D. and Kyle, R. (2007) The Tennessee Education Lottery Scholarship: a reward for past achievement or motivator for future performance? *Working Paper*. Department of Economics and Finance, Middle Tennessee State University, Murfreesboro.
Rønning, M. (2011) Who benefits from homework assignments? *Econ. Educ. Rev.*, **30**, 55–64.
Statens Offentliga Utredningar (1945) *1940 års Skolutrednings Betänkanden och Utredningar*, vol. IV, *Skolpliktti-dens Skolformer*. Stockholm: Ecklesiastikdepartementet.
Statens Offentliga Utredningar (1963) *Rätt till Studiemedel*, vol. IV, *Studiesociala Utredningen*. Stockholm: Ecklesiastikdepartementet.
Statistics Sweden (1974) *Pupils in Compulsory Schools in Sweden 1847–1962*. Stockholm: Statistics Sweden.

Supporting information

Additional 'supporting information' may be found in the on-line version of this article. Please note: Wiley–Blackwell are not responsible for the content or functionality of any supporting materials supplied by the authors. Any queries (other than missing material) should be directed to the author for correspondence for the article.

This content downloaded from 129.108.9.184 on Mon, 02 Jul 2018 20:01:32 UTC
All use subject to http://about.jstor.org/terms