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The effect of summer jobs on post-schooling incomes

Moudud Alam
Kenneth Carling
Ola Nääs

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Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
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The effect of summer jobs on post-schooling incomes[1]

by

Moudud Alam[2], Kenneth Carling[3] and Ola Nääs[4]

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Abstract

In part because of high youth unemployment, students' transition from school to work is an important policy and research topic. Public programs offering summer jobs or work while in high school as measures to smooth the transition is commonplace. The immediate effect of the programs on school attendance, school grades, and disposable income is well documented. However, their effect on the transition to the labor market remains unsettled, partly because of a potential selection bias in previous observational studies. In this paper, 2650 first graders of high school in Falun Council, Sweden, randomly allotted summer jobs via a program in the years of 1997-2003, are followed ten years after graduation. The program led to a substantially larger accumulation of work experience while in high school for offered (particularly weak academically performing) females, but not for offered males. Hence, the immediate program effect was heterogeneous. Females were used to estimate the causal effect of work experience while in high school on post-schooling incomes. The (statistically) significant estimate implies an elasticity of 0.4. Work experience while in high school seems to be of future benefit, but the elasticity is potentially inflated due to heterogeneous effects that we were unable to account for.

Keywords: experimental data, work experience, work while in school, selection bias
JEL-codes: C93, J24, J68

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[2] School of Technology and Business Studies, Dalarna university, SE-791 88 Falun. maa@du.se.
[3] School of Technology and Business Studies, Dalarna university, SE-791 88 Falun. kca@du.se.
[4] School of Technology and Business Studies, Dalarna university, SE-791 88 Falun. ona@du.se.

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1 Introduction

Is early contact with the labor market, through for instance summer jobs, beneficial for high school students in the long run? The answer carries important implications for labor policy makers worldwide, as it assesses whether early contact with the labor market is an advantage to high school students and therefore whether governments should consider smoothing the transition from high school to work.

Fact is that public programs promoting early labor market contacts for high school students are commonplace and they have repeatedly been advocated by the OECD. This study draws on a Swedish example, the federal Summer Youth Employment Training Program (see Leos-Urbel, 2012; Morisi, 2010) is a US example, and Parent (2006) offers an insight into the Canadian debate. Grossman (1997) reported that summer jobs were increasing in the USA as well as in many European countries, while Morisi (2010) later shows a drastic decline in summer jobs in the USA around the year 2000. In Sweden the government commenced subsidizing summer jobs in 1995 continuing until 2007; thereafter there was a temporary reintroduction in 2009 due to the financial crisis at the time. The government administers the subsidies via the municipalities which operatively, with a few exceptions, offer summer jobs to high school students.

It is widely believed that the summer job experience should be beneficial to high school students and their future labor market outcomes. Favorable arguments include that summer jobs help high school students to mature faster than otherwise; provide skills and knowledge which complement in-class education; give high school students feedback on what they have learned and offer hints on what they need to study; enhance their motivation to study; provide earnings that can alleviate poorer high school students financial constraints on future education and human capital investment; and finally, high school students may use summer jobs to smooth the transition from school to work by collecting information and establishing a social network which helps in finding their first regular job (see for example Carling and Larsson, 2005; Geel and Backes-Gellner[5], 2012; Häkkinen, 2006; Ruhm, 1997).

However, the empirical foundation in favor of summer job programs is weak since this issue has not received much attention in the literature (exceptions are Hotz, Xu, Tienda, and Ahituv, 2002; Leos-Urbel, 2012; Parent, 2006; Ruhm, 1997; Wang,

[5] Geel and Backes-Gellner (2012) and Häkkinen (2006) focus on working while enrolled at university.

Carling, and Nääs, 2006). Furthermore, the magnitude of a summer job effect also warrants empirical assessment. On the one hand, a summer job is a modest work experience and may therefore have a trivial effect on future incomes in the long run. On the other hand, in the words of Granovetter cited in Rosenbaum, DeLuca, Miller, and Roy (1999) "…early contacts may have a greater impact on later jobs than on early jobs. This effect may occur because of the accumulation of advantages that come from the initial contacts, since good initial contacts lead to more and better subsequent contacts".

Research on summer jobs or more broadly on working while in high school, has primarily focused on school attendance, school grades, and disposable income in part emphasizing potential negative consequences of summer job experiences (see the review in Ruhm, 1997). For instance, summer jobs with heavy commitment may make students too exhausted and less fit for the new semester; the perception of 'easy' money from summer jobs may detract students' from seemingly "boring" and "unproductive" in-class education; premature contacts with society may negatively affect students if they are not well protected from bad social behavior (see Lee and Orazem, 2010; Weller, Kelder, Cooper, Basen-Engquist, and Tortolero, 2003 and references therein).

It is a rather difficult task to empirically determine the effect of a summer job experience. First, there are few datasets suitable for this purpose; information about summer jobs and their holders are rarely documented. Second, the methodology to analyze this question faces some challenges; the biggest one being the issue of selection bias. A summer job is the result of an active job-searching process, and any correlation between a summer job experience and later outcomes may be due to unobserved individual abilities rather than being a causal relationship. In principle, this problem could be overcome by the appropriate conditioning of confounding variables. However access to and knowledge about such variables is often lacking. Hotz et al (2002) illustrate the methodological challenge for observational data and conclude that appropriate accounting for selection bias is of crucial importance to obtain a robust estimation of the effect of work experience on the transition from school to work.

In this paper, we estimate the effect of high school students' summer job experience on post-schooling incomes by using experimental data (cf Leos-Urbel, 2012). The data was collected in Falun, a mid-size town in central Sweden. The Falun Council randomly allocates the publicly-provided summer jobs to high school student applicants on a

lottery basis since 1995. The random allocation of the applicants to summer jobs provides a unique setting in which there is a good control of the potential selection bias. Furthermore, we have register data providing detailed background information about the applicants who were offered a summer job as well as for those who were not; variables included age, gender, grades, socio-economic status of family and income from work.[6]

This paper is organized as follows. In section two we describe the data, the processing thereof and examine the determinants for applying to the summer job program as well as being offered as a first grader. In the third section we examine the high school first graders program effect on post-schooling incomes. In the fourth section we take a look at the accumulated work experience while in high school, apply an IV-estimator to the females to account for multiple participations in the lottery, and examine the causal effect of accumulated work experience on post-schooling incomes for females. The fifth concludes with a discussion of our findings and how they relate to current literature on the topic.

2 Data and the lottery

The data comes from two sources. The first source is the Falun Council from which we received data pertaining to all applicants of its summer jobs for the years 1997 to 2003. To be an eligible applicant, in addition to be a high school student, the person also needed to be a resident in Falun. The data contains the name and contact address as well as civic registration number for all applicants. The random allocation of summer jobs among the applicants was in the form of an electronic lottery carried out by a Council official using an Excel spreadsheet. The data also contains information on who was offered a summer job based on the lottery as well as whether she accepted the offer. Only a modest fraction of students rejected the offer in which case the summer job re-entered the lottery. The rationale for using a lottery in offering the summer jobs to applicants was claimed by the officials of the Council to be fairness.

The summer jobs were within the Council such as elderly care, gardening, cleaning, and tutoring[7]. The number of summer jobs offered each year was pre-set by the Council and the jobs were intended for a period of three weeks. The Swedish high school runs

[6] Wang et al (2006) used the same experimental data but failed any sharp conclusions because of a too short follow-up period. With the progression of time, this problem is overcome.
[7] Older students tutoring younger ones in core subjects during the summer vacation.

for three years and the student commences it at about the age of 16. In the following we will refer to first, second, and third graders to denote high school students in their first, second, and third year in high school. Although high schooling is voluntary, almost all teenagers are enrolled. High school programs are divided in two categories being theoretical programs targeting university studies and vocational programs aiming at a direct transition to the labor market. However, a reform prior to the study period rendered students of the vocational programs eligible for university. About one third of the high school students are later enrolled in a university. The fall semester of the high school starts in the mid of August and the spring semester ends in mid-June. Consequently, the high school students are available for summer jobs for about a 9-week period in June to August.

The second source of data comes from Statistics Sweden (SCB). They provided data on all individuals in Sweden at the high school in the relevant period. The data contains background information such as gender, parental socioeconomic status, and school grades as well as outcome data in the form of yearly incomes[8] up to a maximum of 30 years of age (limited by data only being available up to end of 2009 at the start of our research). The Council data was added to the SCB data by using the civic registration number as the key variable.

We restrict our focus on high school students being first graders in one of the years 1997-2003. All in all they amount to 6384 of which 2650 applied for a summer job via the program and the remaining 3734 renounced applying. Presumably, the high school students considered the summer job program offered by the Council as just one of many opportunities for finding work. Why 58% of the first graders did not bother to apply for the jobs with the Council is unknown to us. There are however three explanations being put forward to us from the Council. Firstly, in the early years, there was some anecdotal evidence that the knowledge of the existence of the program was limited among students. Secondly, many students had limited interest in the categories of work offered by the Council. Thirdly, Falun is geographically extensive with some communities being more than 40 km from the town center; this resulted in students from rural areas applying proportionally less than other students. Unfortunately, the geo-coding of the students was deleted for confidentially reasons when the data was retrieved from

[8] Post-schooling incomes throughout the study are deflated by CPI to the price level of 2009.

Statistics Sweden. Figure 1 depicts into subgroups of applicants and non-applicants in the course of their high school studies. For instance, there are 2650 students applying in the first grade. Those divide, in their second grade, into 1019 re-applying, 1030 not re-applying, and 601 for whom data is missing. In the third grade, the 1019 re-applicants are divided into 49 re-re-applicants, 797 not applying in the third grade and 183 for whom data is missing in the third grade only. Data is missing primarily for those who were in the first grade in 2003 (or 2002) thereby not observed in the second (third) grade as that occurred outside the Council's data window of 1997-2003, a secondary reason is that some students moved from Falun. Note however that we still have post-schooling income (and other variables) for them retrieved from the second data source.

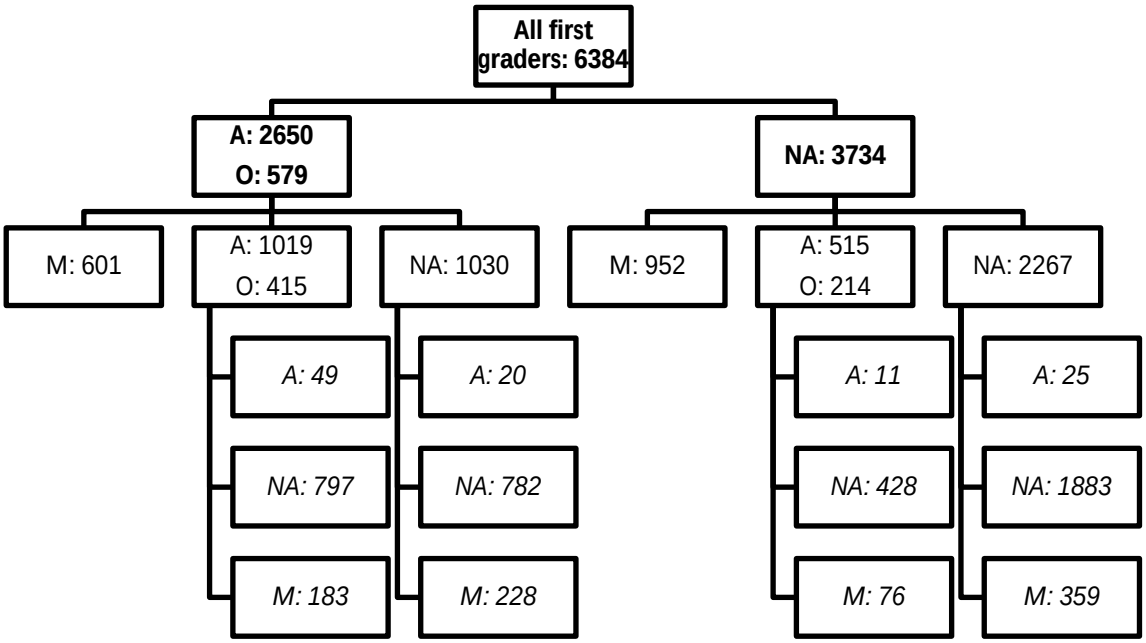

Figure 1: Applications to the summer job program of first graders in the years 1997-2003

Note: Applicants (A), Non-applicant (NA), Offered (O), and Missing (M) at first grade (top level, in bold), second grade (middle level) and third grade (bottom level, in italics).

The first thing to notice in Figure 1 is that applications in the third grade are quite unusual. There are 105 applications in the third grade which makes up only some 2% of all the 4289 applications submitted. Consequently, the program seems of no interest to the students having reached the third grade. The 3734 non-applicants in the first grade are rather unlikely to be applicants later in high school, only 515 applied in their second grade. An applicant in the first grade is however likely to re-apply in the second grade. Hence, the program involves primarily students who apply already in the first grade. For

this reason we will focus the analysis on the 2650 applicants in the first grade noting that they are the ones most affected by the existence of the program.

Table 1 shows descriptive statistics of the first graders. We have included two important variables being school grade of compulsory schooling (final grade usually at the age of 15 years) and parents' income calculated as father's income from work plus mother's income from work in SEK '000. The parental income is taken at the year the student was in first grade since the variable mostly varies between families rather than over time. Both variables presumably affect both the propensity of applying, obtaining a summer job and work experience while in high school as well as post-schooling income. A few things are worth noting. Firstly, females are more prone to apply to the summer job program. Secondly, the students have very limited work experience before high school, an experience that is on average more than tripled during the first grade. Thirdly, non-applicants have lower grades from compulsory schooling but acquire at least as much work experience as applicants.

Table 1: Descriptive statistics of first graders in the years 1997-2003 of the Falun Council. For continuous variables means are given and standard deviations are in parentheses

	Applicants		Non-applicants	
Variable	Males	Females	Males	Females
Offered (%)	18.0	25.0	--	--
Grades[a]	52.5 (26.6)	64.7 (25.1)	45.8 (26.0)	58.5 (28.2)
Parental income[b]	4.4 (2.3)	4.3 (2.2)	4.0 (2.4)	4.1 (2.5)
Summer jobs (%)	29.2	31.4	36.4	35.8
Income in first grade	5.1 (6.9)	6.7 (9.0)	6.7 (11.1)	6.4 (10.2)
Acc. income[c]:				
Mean	1.9 (3.9)	1.9 (3.7)	3.2 (6.5)	2.8 (6.2)
1st quartile	0.0	0.0	0.0	0.0
Median	0.0	0.0	0.0	0.0
3rd quartile	2.3	2.3	4.5	3.2
No.	1203	1447	1961	1773

Note: (a) Original grades from compulsory school are in different scales before and after 1998 so we used the ranked scores between 0-100 to enable a comparison over the years. (b) Parents' income when student was in the first grade (in SEK '00000). (c) Accumulated income prior to high school (in SEK '000).

Table 2 shows the estimated logistic model for the probability of applying both in the first grade and then re-applying in the second grade. Grades (and parental income) are statistically significant, but the magnitude of their effect is negligible. It seems that the time of birth in the calendar year does not affect the decision to apply, whereas the propensity of applying vary between years since the likelihood-ratio test of a no year-

effect is strongly rejected with a *p*-value less than 0.0001. Moreover, accumulated work experience lowers the propensity of applying, possibly because the student acquires a greater network of employers as the work experience accumulates. However, students offered in the first grade are more likely to re-apply in the second grade, which might follow from the offered student wishing to return to the same work place or a superstitious belief of the student as being lucky when it comes to this lottery.

Table 2 also shows the estimated logistic model for the probability of being offered a summer job by the Council in the first grade upon having applied. As expected in a random allocation of the summer job slots, none of the included variables affect the outcome (parental income is statistically significant in the model for females but of inconsequential magnitude). However, the yearly dummies are strongly significant: the number of available slots each year starting with 1997 was 84, 342, 240, 158, 269, 156 and 176 in 2003 as decided beforehand by the Council even though the number of applications per year was more stable around 800.

Table 2: Estimated logistic model for the application probability as well as probability of being offered (for applicant) first graders in the years 1997-2003. Standard errors in parentheses

	Prob. of	Prob. of	Prob. of Offer	
	Applying	Re-applying	Male	Female
Intercept	-0.786*	-0.212	-1.858*	-1.000*
	(0.108)	(0.222)	(0.313)	(0.263)
Male	-0.176*	0.144	--	--
	(0.057)	(0.100)		
Grades	0.009*	0.008*	-0.005	-0.004
	(0.001)	(0.002)	(0.003)	(0.003)
Parental income	0.012	0.063*	0.011	-0.067*
	(0.012)	(0.023)	(0.377)	(0.031)
Acc. income[a]	-0.065*	-0.063*	0.022	0.005
	(0.007)	(0.007)	(0.019)	(0.017)
Offered	--	0.395*	--	--
		(0.116)		
Birth quarter dummies	Yes	Yes	Yes	Yes
Yearly dummies	Yes*	Yes*	Yes*	Yes*
No. obs. (used)	5828	1912	1128	1351

Note: * indicates significant at 5% level. (a) Accumulated up to first grade and second grade, in SEK '000, for application model and re-application model, respectively.

It was possible to apply for summer jobs in each grade of high school and in each of the years the applicants were randomized. Consequently the quantity of summer job experience from the program while in high school is not only an outcome of the lottery

(and thereby random) but also the propensity to apply. We stress (cf Table 2) that previous work experience in the form of accumulated incomes by the first grade implies a lower re-application propensity. This means that the more experience the student gains, the less likely she is to re-apply and consequently less likely to be offered. Presumably this negative effect is countered by a greater likelihood of being offered work by other employers that may offer higher salaries or a longer duration of the work. Either way, consecutive offers are not independent outcomes since the decision of re-applying is subject to self-selection. For instance, the Council's summer job pays about SEK 7,500, and if offered while in the first grade, the model implied effect on the re-application propensity in the second grade is a decrease of about 45% (7.5 multiplied by -0.06).

3 Post-schooling incomes of students offered and non-offered in their first grade

Our main interest is not about the work experience and income while in high school, but if the early contact with the labor market accumulates into a stronger future position on the labor market manifested by e.g. higher post-schooling incomes. The summer job opportunity is open to every high school student in Falun and it is therefore reasonable to focus on the average treatment effect. We have:

(1) $\Delta_i = E\left[Y_i^S - Y_i^{\bar{S}}\right]$,

being the *i*:th student's expected post-schooling income (*Y*) in either of the cases having had the experience of summer job (*S*) or not ($\bar{S}$). Hence, Δ_i is the student's expected gain in post-schooling income from a summer job. We consider heterogeneous effects for males and females, but for the time being assume that $\Delta_i = \Delta$ for all *i*.

Clearly, it is not possible to observe the same student both with and without a summer job experience as she either will or will not have it upon entering the labor market after graduation. A crude estimator would be

(2) $\Delta_{crude} = E[Y^S|T = S] - E\left[Y^{\bar{S}}|T = \bar{S}\right]$,

where *T* is the treatment indicator of a student setting the indicator to *S* if the student had summer job experience; otherwise denoted $\bar{S}$. In the estimation, the expected values are replaced by the sample means. Such estimator has previously been found to suffer from selection bias and it is therefore invalid. Hotz et al (2002) found this estimator to

provide positive gains; a gain which was substantially reduced while attempting to render the two groups more comparable by conditioning on some of the potential aspects that influence future incomes.

Instead we focus on the program effect and compare offered and non-offered first graders who applied for a Council summer job. The offer, being based on a random allocation, ought to be independent of other aspects which may influence post-schooling incomes. An unbiased estimator of the program effect would be

(3) $\hat{\Delta}_p = E[Y|T = O] - E[Y|T = \bar{O}]$,

where the treatment indicator indicates either offered ($T = O$) or not ($T = \bar{O}$). There are however some problems with this estimator in this case as the program is opened also to second (and much less important third graders). Nevertheless we abstract from this problem initially in the interest of enabling a simple analysis.

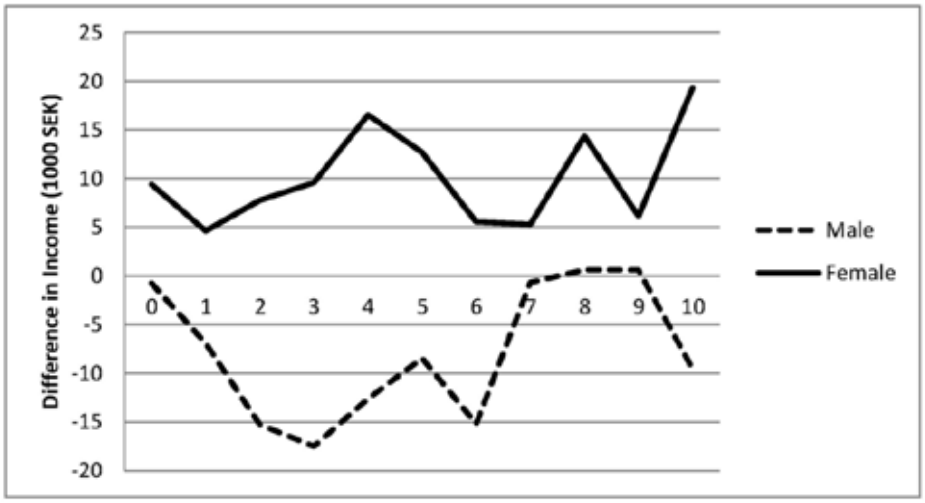

Figure 2: Evolution of difference in yearly mean post-schooling income between offered and non-offered first graders

Figure 2 shows the difference in the evolution of average yearly income up to 10 years after graduation for offered and non-offered in the first grade[9]. In Sweden in 2010, the average nominal income per year for 20-64 year olds was about 270,000 SEK. These first graders compare roughly with this figure ten years after graduation with an average of 240,000 for the males and 220,000 for the females.

[9] The proportion of students without post-schooling income is substantial in the early years after graduation as many of them entered at a tertiary schooling.

The visual impression of Figure 2, that the summer job program affects females positively and males negatively, is supported by a regression model and formal statistical testing. We regressed post-schooling incomes on an indicator variable for offered or not, including a variable which represents years after graduation as well as year of application. In the regression, we gave due account to the fact that we have repeated measures of post-schooling incomes of the applicants (Liang and Zeger, 1986 and Littell, Milliken, Stroup, and Wolfinger, 1996). The estimated program effect for males was SEK -12,100 with a standard error of 5,900 (p-value of 0.04) and SEK +8,500 for females with a standard error of 3,600 (p-value of 0.02). Furthermore, there was no indication that the effects were reduced by years after graduation. The difference in program effect for males and females is puzzling. We speculate, further discussed in the concluding section, that the program is better targeted to students opting for career in the public sector, which is much more common amongst females. Overall, there is no obvious positive program effect on post-schooling incomes as the estimated program effect based on all the first graders amounts to SEK -1,300 with a standard error of 3,200 (p-value of 0.68).

4 Estimation of treatment and dose effects

Offered or not, and indirectly, summer job or not is a crude measure of work experience. The post-schooling effect is presumably more related to the quantity of the work experience. Ruhm (1997) concludes that high school employment in the senior year, i.e. third graders here, of 20 hours/week seems to yield about 22% higher annual income some 6-9 years after graduation, an estimate susceptible to selection bias. Since the offered first graders received 3 weeks of work experience of 40 hours a week, we may expect an effect of being offered to be some 3-5% on post-schooling income (by taking Ruhm's estimate at face value). Actually, even less than this value when considering that almost half of the non-offered still received a summer job experience. In this section we consider the total work experience as the accumulated income, i.e. treatment dose, during the years of high school. We accumulate incomes up to the first semester of the third grade, since we cannot distinguish[10] between income within the third grade and a possible ensuing regular work in the fall after graduation. However,

[10] Income from work is reported as a yearly total.

the analysis is complemented by also examining accumulated income up to and including the year of graduation.

To see how the summer job program triggers work experience while in high school, Table 3 shows the accumulated income during high school by gender and grades from compulsory school (in square brackets accumulated income up to the year of graduation is given). The abundance of zero accumulated income prompted us to report median accumulated income rather than the mean of the variable. Added weeks is calculated as the difference between offered and non-offered in accumulated income while in high school and divided by SEK 2,500 which is the weekly amount the Council's summer jobs paid the offered.

Females benefit from the summer job program. The offered in first grade maintain a difference of 3 weeks more work experience compared with them not offered. The gap between offered and non-offered is further accentuated upon graduation when the difference in work experience has grown to 7 weeks (29 versus 22 weeks). Hence, Granovetter's thought of an early contact gradually accumulating in the course of time seems relevant for these female students.[11] Following up on the same female first graders eight years after graduation they are found to have 8.6% higher income. Offered males in the first grade, on the other hand, seem to lose their early advantage to fellow non-offered as the difference is only 1.0 weeks at graduation. The difference in post-schooling income eight years later is a slight 0.5%.

The first graders in Table 3 are also divided by grades from compulsory schooling. For males the effect in terms of added weeks is unrelated to those grades. However, the females differ in this regard. The females having had the lowest quartile of compulsory schooling grades are strongly affected by the summer job offer. Taken at face value, the offer led to 6 additional weeks[12] of work experience (or a 210% increase in accumulated income while in high school) and a subsequent 50% increase in post-schooling incomes, implying (approximately) an elasticity of 0.24 (0.5/2.1). While the increase in work experience for these offered females is noteworthy and the ensuing effect on post-schooling incomes is perhaps beyond expectations, it should be noted that the most

[11] Knabe and Plum (2013) find low-paid jobs to be a springboard to better paid jobs, particularly for low-skilled workers. One may speculate that summer jobs might be comparable to low-paid jobs.

[12] Again there is a growing gap between offered and non-offered females. Including the year of graduation, the difference amounts to 26 versus 11 weeks for which the most obvious explanation is the initial offer of a summer job in the early phase of high school.

striking observation is the low post-schooling income of non-offered. A yearly, average income of SEK 115,000 is even less than 50% of the national average, thereby strongly suggesting that these females (contrary to the other five subgroups of non-offered) are detached from the labor market.

Table 3: Accumulated income (median) while in high school for Offered and Non-offered first graders, the consequent increase of work experience (in weeks), and the ensuing post-schooling income (mean PS-Income) 8[a] years after graduation

Group	Offered[b]	Non-offered[b]	Added weeks	PS-Income (%)[c]
Males				
Overall	15.4 [39.5]	12.8 [36.6]	1.0 (20%)[d]	0.5 (205/204)[e]
Grades of comp. school				
Lowest (< *1st quartile*)	10.0 [34.1]	9.1 [26.5]	0.4 (10%)	13.5 (219/193)
Middle (*1st - 3rd quartile*)	17.2 [44.1]	14.0 [41.0]	1.3 (23%)	5.5 (212/201)
Highest (>*3rd quartile*)	17.2 [43.4]	13.1 [37.5]	1.6 (31%)	-13.1 (192/221)
No.	217	986		
Females				
Overall	25.4 [72.3]	17.8 [54.9]	3.0 (43%)[d]	8.6 (177/163)[f]
Grades of comp. school				
Lowest (< *1st quartile*)	21.7 [64.8]	7.0 [28.4]	5.9 (210%)	50.4 (173/115)
Middle (*1st - 3rd quartile*)	25.9 [79.0]	17.7 [59.3]	3.3 (46%)	0.0 (159/159)
Highest (>*3rd quartile*)	25.9 [65.9]	20.0 [55.0]	2.4 (30%)	16.7 (203/174)
No.	362	1085		

Note: (a) Most first graders had complete records on post-schooling incomes until 8 years after graduation. (b) Accumulated income in SEK '000, within square brackets are accumulated income up to the year of graduation. (c) In parentheses is post-schooling income 8 years after graduation in SEK '000 and the percentage gives the relative difference between Offered and Non-offered. (d) p-values <0.01. (e) p-value=0.97. (f) p-value=0.23.

The cell in Table 3 with the smallest number of observations contains only 40 students: breaking down the data into subgroups and single out an isolate year of post-schooling income means of course poor precision in any estimate and we therefore model post-schooling incomes on the dose in a two-stage framework (Angrist and Kruger, 1991)

(4) $ln(Y_{it}) = \alpha + \gamma \ln(\widehat{Dose}_\iota) + \delta t_i + \beta_1 x_1 + \cdots + \beta_k x_k + \epsilon_{it}$,

(5) $ln(Dose_i) = \mu + \lambda z_i + \omega_i$,

where $Dose_i = \sum_{t=-2}^{-1} Y_{it}$ is the accumulated income while in high school and first it is regressed on the instrumental variable represented by the model in (5). Because the random offer had no lasting effect on males we lack a useful instrumental for them and no further analysis are shown for the males. However, we attempted some analysis for

them, similar to the ones for females discussed below, but the instruments were unreliable and effect estimates small and unstable.

For the females we consider some competing instrumentals and specifications of model (5). The first instrumental is the binary variable offered or not in the first grade which should induce truly an exogenous variation in dose because of the lottery. The binary characteristic of the instrument is however a weakness (see e.g. Gelman and Hill, 2007, p. 224). We therefore also test a second instrumental variable being the ratio of the student's offers by her applications. In contrast to the number of offers, as yet another potential instrument, the ratio follows directly from the lottery and the Council's decision to allot summer jobs. However a drawback with the ratio is that it might be an invalid instrument as it might be correlated with post-schooling income via multiple applications to the summer job program during the course of the first graders' high school studies.

Table 4 gives the estimated model (5) for female first graders under three specifications.[13] As could be anticipated from Table 3, the instrumental variables Offered works well with a *F*-statistic of 37.5. An offer leads to higher expected accumulated income while in high school (i.e. a higher dose). Taken at face value, the model implies that an offer in the first grade increases the dose by 58% which compares to 43% ((25.4-17.8)/17.8) derived from Table 3.

Taking the ratio between number of offers and number of applications, thereby exploiting the multiple outcomes of the lottery, provides another well working instrument. Its *F*-statistic is 31.6. An alternative, third specification is to include background variables into model (5). Specification (5c) shows the estimates of such model where the estimate for the instrument Offer is almost unchanged to specification (5a). In estimating model (4) we consider all three specifications of model (5).

[13] The number of females used in estimating the models varies slightly due to some partially missing observations on grades and parental income.

Table 4: Estimated model (5) for female applicants in the first grade. Standard errors in parentheses

	Specification		
Variable	5a	5b	5c
Intercept	5.332* (0.082)	4.739* (0.050)	4.626* (0.160)
Offered in first grade	0.578* (0.094)	No	0.591* (0.097)
Offers-Applications ratio	No	0.668* (0.119)	No
All *X* variables from Model (4)	No	No	Yes*
F-statistics	37.5	31.6	11.8
No. Obs. (used)	1438	1438	1352

Note: * indicates significant at 5% level.

Table 5: Estimated model (4) by means of Generalized Estimating Equations (GEE). Standard errors in parentheses

	Specification		
Variable	4a	4b	4c
Intercept	3.943* (0.948)	3.646* (0.991)	4.236* (0.824)
Dose	0.404* (0.186)	0.466* (0.199)	0.430* (0.202)
Dose from OLS[a]	*0.427** *(0.104)*	*0.569** *(0.114)*	*0.548** *(0.116)*
Grades	0.004 (0.002)	0.004 (0.002)	0.001 (0.002)
Parents income[b]	-0.007 (0.015)	-0.007 (0.015)	-0.012 (0.015)
Age[c]	-0.072 (0.057)	-0.072 (0.057)	-0.072 (0.057)
Age*Age	0.019* (0.006)	0.018* (0.006)	0.019* (0.006)
Birthquarter	Yes	Yes	Yes
Birthyear	Yes	Yes	Yes
No. students included	1318	1318	1313
No. obs. (used)	6937	6937	6913

Note: * indicates significant at 5% level; (a) Estimate from least squares estimation neglecting repeated measures of the students, (b) Parents' income in 100,000 SEK, (c) Age is a counter of the number of years after graduation.

In the estimation of model (4), the predicted value of the accumulated income while in high school from model (5) serves as the *Dose* for the female students. In the example of specification 5c, we found the median predicted dose to be SEK 13,800 and the inter-quartile range to be SEK 8,600. Table 5 gives the estimated model (4) for the three choices of predicted dose given in Table 4. The model is estimated using Generalized Estimating Equations (GEE) in order to appropriately account for the repeated measures of post-schooling income for the students (see Liang and Zeger, 1986). The estimate of

the dose-effect from GEE is complemented by the corresponding OLS-estimate (in italics in Table 5)[14]. Since the estimates of the dose-effect are similar in all three specifications, we focus on the estimated model of specification (4a). The background variables are not or only weakly significant. As expected, the post-schooling income increases non-linearly by years after graduation. However, this non-linear increase is most likely a consequence of the students being followed only up to about the age of 30.

The estimated model (4) in Table 5 has afterwards undergone substantial sensitivity analysis: We have modeled subsets of students defined by their background variables such as students with low compulsory school grades, students applying in specific years or born in specific quarters as well as recoding the continuous variables into factors and estimating the model for various follow ups as for instance post-schooling income at 2, 3, 5 and 7 years after graduation.

Of most interest for this study, the dose is significant with a point-estimate of 0.40 suggesting that a one per cent increase in accumulated income while in high school causes a 0.40% higher post-schooling income.[15] Possibly the effect is slightly higher depending on the preferred instrument. Finally, one may ask whether the dose-effect wears off by time. To check that we re-estimated the model (4) including an interaction of dose and years after graduation (i.e. age), the statistically insignificant point estimate was -0.02 with a standard error of 0.06. Hence, the effect of work experience in high school seems to be strong and persistent later in the working life.

The interpretation of the elasticity for a wider group of Swedish high school students needs to be done with some care. One reason is that the model assumes a homogenous effect of summer jobs. The experimental setting as well as the modest number of students affected by the summer job program called for this assumption. However, it is perhaps to be expected that the effect varies between students, i.e. a heterogeneous effect. The experimental data is not suited for checking this, instead we regressed post-schooling income on observed logarithmic dose rather than predicted in model (4) for the two quartiles of students with lowest and highest grades from compulsory schooling, including both applicants and non-applicants in the first grade. An elasticity estimated

[14] To account for the uncertainty in dose induced by model 5, the standard errors of OLS ought to be inflated by about 10% (Gelman and Hill, 2007, p. 223-224). Appropriate inflation of the standard errors of GEE is substantially more challenging and not done here. However, the inflation of OLS can be taken as a rough approximation for GEE.

[15] We have also re-estimated the models for specification (5a) using accumulated income up to the year of graduation as the dose. The instrumental Offered in first grade worked but was a weaker instrument in this case. The dose effect was at least as large as found in Table 5 and statistically significant.

in this manner is of course susceptible to selection bias, but we found them to be comparable for the males (0.18 for those with low grades and 0.22 for those with high grades), whereas they differed substantially for the females (0.30 versus 0.14) with a standard error of only a few percentage points. This analysis suggests heterogeneity in the effect of summer jobs being the strongest for females with low grades, and these females, in turn, highly influence the estimated elasticity of 0.4 because their dose was mostly affected by the summer job program. Consequently, an elasticity averaging over all high school students is likely to be less than 0.4.

5 Conclusion

In this paper we addressed whether early contacts, in the form of summer jobs for high school students, with the labor market improved the transition from schooling to work. This issue is important since high youth unemployment rates prevail and policies of this kind have been proposed and targeted to high school students in many countries. The empirical evidence on the issue is hard to find, particularly so because the few existing studies are plagued by potential selection bias.

We studied the effect of summer jobs offered by a Swedish Council under the umbrella of a national summer job program. The Council was chosen for the study because it allotted the summer jobs amongst the applicants by means of a lottery and kept a historical record over applicants. We found no (or a modest) program effect for males, but an effect for females and a strong program effect for females with weak academic performance who seems to have tripled their work experience while in high school and increased their post-schooling incomes by some 50%. Yet, this far, the Council (or the government) has not considered targeting the program to subgroups of students. A possible, but entirely speculative, explanation to why females only benefitted from the program might be because the summer jobs were in the public sector which mostly attracts female workers, and they thereby acquired experience directly relevant for their future work tasks (the results of Geel and Backes-Gellner, 2012 suggest that employers merit rather specific work experience gained by (university) students). Note also that the post-schooling outcome of summer job programs is merely one rationale for the policy. Many other aspects have been studied in the literature of which most may be found referred to in Ruhm (1997).

The outcome of the lottery affected the females accumulated work experience while in high school. We used the lottery induced exogenous variation to examine the causal effect of accumulated income while in high school on post-schooling incomes up to ten years after graduation. We found an elasticity of 0.40. As a result this contradicts the recent findings of Hotz el al (2002) and Parent (2006) in US and Canadian settings, while the result is more in line with older non-experimental studies (see Ruhm, 1997). Ruhm (1997) reported that 20 hours/week of work experience in the last year of high school was associated with some 22% higher incomes 6-9 years after graduation. His use of senior year and another definition of dose render the comparison a bit difficult. However, the non-offered females had worked corresponding to 22 weeks fulltime (SEK 54,900) by the year of graduation (cf Table 3). Say that Ruhm's 20 hours/week of work experience in the last year of high school amounts to adding 20 weeks of fulltime work experience to the existing 22, we then get an increase in dose of some 90% that causes 36% higher post-schooling incomes assuming a linear dose-effect. Hence, our findings suggest a stronger effect of work experience than Ruhm's.

Interpreting our results as a general assessment of the importance of work experience while in high school should be done with some care. In an attempt to check for heterogeneous effects, we found indications for the effect to be substantially higher for females with the weakest academic performance, thereby suggesting that the estimated elasticity of 0.4 overestimates the average elasticity for high school students overall. The exogenous variation induced by the lottery primarily came from females with the weakest academic performance who are likely to directly opt for the labor market (of the public sector) upon graduation.

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