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Long-term Effects of Conscription: Lessons from the UK

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Abstract

The effect of military service on subsequent earnings has not been taken into account in the decennial debate on the abolition of military conscription in Western Europe. This paper provides evidence of the long-term effects of conscription on subsequent earnings. In our analysis, we use a regression-discontinuity approach to obtain unbiased estimates. The RD design used is based on a sharp discontinuity due to the abolition of military conscription in the UK in 1960. We study the effects of the abolition of compulsory military service in the UK on labour market outcomes. Our analysis sheds light on the importance of early labour career phase on labour market outcome.

JEL Classification: J30, J31, J24, I20

Keywords: Regression discontinuity approach, Quasi-natural experiment, Military service, wages

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1. Introduction

Compulsory military service (CMS) represented an obliged stage for most European young men's early labour life. During the last decade a growing number of countries in continental Europe, including Belgium, Netherlands, France, Spain and Italy have abolished conscription,[1] while other countries such as Germany and Israel still have mandatory military service for men.[2]

It has been argued that military service has a negative effect on subsequent labour market outcomes. For instance, Angrist (1990) and Imbens and van der Klaauw (1995) study the effect of veteran status for the US and for the Netherlands.

In this paper we study the effect of the abolition of the National Service in the UK on subsequent earnings and we explore whether the abolition of the National Service has affected subsequent earnings via the education channel influencing the demand for education of the cohorts exempt from CMS. Conscription in the UK was introduced in 1939 and continued after the Second World War (WWII). It was formalized in peacetime by the National Service Act 1948. From January 1949, every man aged 18 or more was expected to serve in the armed forces for two years. From the introduction of the National Service in 1949 more than 2,5 million young men were called up (Hickman, 2004). The National Service officially ended on 31st December 1960 and the last intake of National Servicemen took place in 1960. The abolition of military conscription affected all men born in 1943 and after.

[1] Belgium abolished compulsory military service in 1994, Netherlands in 1996, France in 1996, Spain in 2001 and Italy in 2005.
[2] The only exception is represented by Israel that has mandatory military service for both men and women.

Our results show that the effect of military conscription on subsequent earnings is significant and long-lasting. Males who served for two years in the National Service earn on average between 4 and 7 percentage points less than the immediately subsequent cohort exempt from compulsory military service. We find very little evidence that military service affects subsequent earnings through its effect on education achievement. In fact, our estimates show that exempt cohorts accumulate on average only a quarter of year of additional education. On the other hand, we find that compulsory military service affected the probability for young men of being enrolled in school when 18 or older between 2 and 4 percentage points. In other words, males exempt from CMS because born after 1943 were more likely to leave full-time education later than males drafted.

We contribute to the literature that studies the effects of military conscription on earnings with the aim to disentangle the channels through which CMS affects subsequent earnings.

Previous contributions have mainly focused on the causal relationship between subsequent earnings and veteran status (Angrist, 1990, 1998; Angrist and Kruger, 1994), while little attention has been devoted to the effect of compulsory military service (Imbens and var der Klaauw, 1995). Angrist (1990), using the Vietnam draft lottery as a natural experiment, estimates that in the early 1980s more than ten years after the end of Vietnam war, the earnings of white veterans were approximately 15% lower than the wages of non-veterans. Angrist and Krueger (1994) find that WWII veterans, if anything, earn less than non-veterans. Imbens and van der Klaauw (1995), addressing an issue similar to ours, find that CMS in the Netherlands reduced earnings by 8 percent-

age points for individuals who had their careers interrupted to serve in the army for one year relative to the non-drafted men of the same cohort.

Instead, only few studies analyze the effect of military service on college enrolment and graduation rates. Cipollone and Rosolia (2006), exploiting the exemption from CMS for few cohorts of men living in the area of southern Italy hit by an earthquake in 1980, show that exemption raised males' high school graduation by 3 percentage points.

Card and Lemieux (2001) measure the effect of draft avoidance behaviors comparing college enrolment and graduation rate of men from cohorts at risk of being drafted and females during the Vietnam era. They show that draft avoidance increased college enrolment by 4-6 percentage points in the late 1960s and the share of men born in the mid-1940s with college degree by about 2 percentage points.

More recently, Bauer *et al*. (2005) and Maurin and Xenogiani (2005) study respectively the effect of the introduction of conscription in Germany after 1955 on labour market and the abolition of CMS in France in 1997 on the demand for education and labour market outcomes.

Our findings shed light on the importance of early career phase on labour market outcomes, CMS represents a career interruption and may prevent the acquisition of labour experience in early labour life. As stressed by Oreopoulos *et al*. (2005) the early career phase represents a crucial stage for a worker's success as mature labour market participant. In the first 10 years of work, individuals experience 70% of overall wage growth, change jobs frequently, and find a career occupation and employer. The existing literature suggests that interruption or early career events at the individual level have persistent effects (Kletzer and Fairlie, 2003; Devereux, 2004 and Von Wachter and Bender, 2005). Albrecht *et al*. (1999) study the link between career interruptions and subsequent

earnings and find that human capital depreciation is not the only factor that explains the negative effect of career interruptions on future wages.[3]

The paper proceeds as follows. The next section describes the history and the characteristics of CMS in the United Kingdom. Section 3 presents the data and provides a discussion on the validity of the research design. Results are presented in Section 4 and discussed in Section 5. Section 6 concludes with a discussion about why CMS seems to generate long-term effect on subsequent earnings.

2. National Service in the United Kingdom

The National Service Act 1948 introduced compulsory military service during peacetime in the United Kingdom (excluding Northern Ireland) coming into effect on 1st January 1949. The National Service remained in place until 1960 and the last National Serviceman was demobbed in 1963. The first cohort exempt from serving in the armed forces was the 1943 cohort.

The act initially stipulated 18 months of active service with four years in the reserve, and it was extended to two years because of the war in Korea in 1950. All young men reaching the age of 18 were required to start the National Service and each had to register with the local branch of the Ministry of Labour and National Service.[4] To ensure that no one could avoid the draft, schools and employers were obliged to communicate to the Ministry of Labour the names of the males reaching the age of 18 and anyone who did not register was traced through the National Health records. Very few young men

[3] The impact of unemployment spell on subsequent earnings for British men is the focus of the paper by Gregory and Jukes (2001). They find consistent evidence of scarring. In particular, the effect of unemployment duration is permanent and proportional to the length of the spell. This is due to the lower level of work experience accumulated because of unemployment

[4] Britain's 18 to 20-year-olds represented about five per cent of the workforce.

were exempt from the National Service.[5] It was possible to ask for a deferment until the age of 21 for those who were in apprenticeships or had started university degrees. A few people conscientiously objected to the National Service.[6]

It is worth to notice that from the introduction of the CMS in 1949 to its end in 1960 more than 2,5 million young men were drafted. In those years in the United Kingdom, including Northern Ireland that was excluded from the CMS, young men aged 18 years were on average 320,000.[7] On average more than 70% of British young men served in the National Service.

Two weeks after registering at the local branch of the Ministry of Labour and National service the conscript received a notice to attend a medical examination.[8] He was then interviewed by a Military Interviewing Officer (MIO) in order to match him to the service that would be the most suitable for his skills and experience.[9]

In 1948 the basic pay for conscripts was 1.4 pounds net a week, while the average civilian weekly wage in 1951 was 8.4 pounds. Pay for conscripts rose to 1.9 pounds in 1960 well below the average weekly wage for men in 1961 (15.5 pounds).

After two years of military life the transition to civilian life was problematic for most of National Servicemen. Going back into office jobs or to study, or even finding an occupation to suit them was very difficult. In 1955 nearly one quarter of demobbed con-

[5] In particular, clergymen, police cadets, coal miners, merchant seamen, fishermen, agricultural workers involved in essential food production and graduate science teachers

[6] All conscientious objectors had to register as such, motivating their reasons to object. Their case was then heard by a local tribunal which decided whether their objection was valid. Only those objecting on religious grounds succeeded and the number never rose above four per cent.

[7] Data kindly provided by the Population Estimates Unit of UK National Statistics.

[8] The reasons for exoneration from National Service were strictly coded and quite restrictive. They typically required main physical disabilities or serious mental disorders.

[9] One of the most common complaints among men who did National Service was that MIOs did not take account of their civilian skills and experiences. As result, many National Servicemen were assigned to jobs for which they had no experience.

scripts reported that they had had difficulties in settling back into civilian life – around 35 per cent changed jobs at least once in the first year after demob (Hickman, 2004).

3. Research Design

3.1 The Cost of Conscription

Military conscription may affect subsequent labour market outcome in several ways.

First, through a direct effect. In fact, CMS, representing a career interruption at early stage in labour life, prevents young men from acquiring relevant professional experience and causes the depreciation of recently acquired job-specific skills.[10] Furthermore, firms are usually reluctant to hire and train young men unless they have finished or are exempt from military service. This implies that before having accomplished their military obligations young men may experience difficulties in finding a job (Oi, 1967) and this has clearly a negative effect in terms of lower job training, experience and occupational specific skills.

Second, through an indirect effect. Military conscription may affect human capital formation and in particular educational attainment (see Cipollone and Rosolia, 2006). CMS may represent an interruption of the educational life and since it normally occurs after the completion of the high school it may prevent individuals from continuing their studies.

In the UK the first, direct effect, is likely to be particularly relevant since the duration of the National Service has been much longer than in other European countries. For ex-

[10] As briefly discussed above, most of the times conscripts are not matched to suitable jobs while in the armed forces and this may lead to a depreciation of their human capital stock during their time of conscription.

ample, the length of mandatory military service in Germany in 1957 was 12 months, in Netherlands was 14 months and in France 10 months.

On the other hand, the CMS is unlikely to have affected educational attainment: first, high-school graduates could have asked for a deferment until the age of 21 whether enrolled in a university, second, and more importantly for our purpose, military conscription was hardly binding for cohorts born between 1933 and 1952. As shown in Figure 1 over 80% of individuals left full-time education before 18 years, the age when they had to serve in the army.

In this paper, we try to empirically differentiate between these two channels. On the one hand, we explore a simple reduced-form relationship between log real earnings and compulsory military service. On the other hand, we explore a simple reduced-form relationship between educational attainment and compulsory military service, and exploit existing evidence on returns to schooling to translate the change in schooling into differences in earnings.

3.2 Data Description

The data we use come from two different sources. The first dataset used is derived combining 15 UK General Household Surveys (GHS) from 1983 to 1998[11]. In the GHS earnings were coded exactly, furthermore education is recorded as the age when an individual left full-time education. GHS does not contain the month of birth, while it contains the year of birth. The final sample from GHS contains 52,219 individuals (28,073 males and 24,146 females) born between 1933 and 1952.

[11] For 1997 the GHS has not been conducted. For a detailed description of the dataset we use see Oreopoulos (2006).

The second data set is obtained combining 8 UK Labour Force Surveys (LFS) from 1984 to 1991. From 1984 to 1991 the LFS was carried out annually, while starting from 1992 it has been conducted on a quarterly basis.[12] LFS does not contain earnings, but it contains education as the age an individual left full-time education, furthermore month and year of birth are recorded. The final sample from LFS comprises 314,504 individuals (155,724 males and 158,780 females) born between 1933 and 1952. The LFS allows us a further investigation into the effect of the abolition of the CMS on educational attainment for cohorts immediately before and after there was the change in the legislation.

Descriptive statistics are presented in Table 1. The table shows substantial differences in real earnings for men born in 1942 and in 1943; while age left full-time education is not statistically different for the two cohorts. As far as women are concerned the comparison suggests that both real earnings and education do not differ for women born in 1942 and in 1943.

3.3 Identification strategy

Identification of the causal effect of military service on subsequent earnings requires that serving in the army is not affected by self-selection problems that may typically affect the decision to voluntarily serve in the armed forces. The abolition of the CMS allows us to address this issue. In fact, the sharp discontinuity introduced by the abolition of the CMS represents the source of identifying information (see van der Klaauw, 2002, for a detailed description of RD approach)

[12] The LFS is based on a systematic random sample design which makes it representative of the whole of Great Britain. Starting from 1992 Each quarter's LFS is made up of 5 'waves' and each wave is interviewed in 5 successive quarters. As a result, there is an 80% overlap in the samples for successive quarters.

In our analysis, we take a regression-discontinuity (RD) approach to consider the effect of the abolition of military conscription on subsequent wages and educational attainment by comparing earnings and education level of cohorts born immediately before and after 1943. Our research design is based on the sharp discontinuity due to the abolition of National Service at the end of 1960. All young men born before 1943 were required to serve in the National Service, as discussed above very few men were exempt, while males born in and after 1943 were exempt from CMS.

The advantage of this approach is that we can focus on cohorts right before and right after 1943. These cohorts are presumably similar, as shown in Table 1. Note that this approach assumes that there were no other discontinuous jumps in unobserved characteristics in the years when the abolition of CMS took place.

Thus, the comparison between the cohort born before 1943 and the cohort born in and after 1943 enables us to identify the causal effect of CMS on subsequent wages and educational attainment. Figure 2 reports the evolution of the log average real earnings for males born between 1933 and 1952.

The main specification for the log earning regression that we estimate is:

$$\ln W_i = \alpha_0 + \beta CMS_i + \varepsilon_i \quad (1)$$

where W_i is the real earning of individual i, CMS_i is a dummy variable, equal to zero if the individual was born before 1943 and one otherwise, ε_i is an i.i.d. shock.

Analogously, the main specification for education attainment regression is:

$$S_i = \alpha_0 + \beta CMS_i + \varepsilon_{it} \quad (2)$$

where S_i is the age at which individual i left full-time education.

First, we estimate equations (1) and (2) for the different subsamples of men born within arbitrarily narrow bands close to the threshold (Angrist and Lavy, 1999; van der

Klaauw, 2002). We start using the subsample of individuals born between 1938 and 1947 (± 5 years from the discontinuity), we further restrict our sample respectively to individuals born between 1941 and 1944 (± 2 years) and between 1942 and 1943 (± 1 year).

We can expect individuals born just before 1943 to be very similar on average to individuals born on or just after that date. Since cohorts born just before and after the threshold had very similar characteristics, then comparing earnings of both groups will control for all omitted factors and should therefore provide a good estimate of the effect of the abolition of CMS.

Second, we estimate equation (1) and (2) for the whole sample of men born between 1933 and 1952. In this second set of regressions we control for polynomial in birth cohort, polynomial in age and survey year fixed effect.

Following Card and Lemieux (2001), we make the counterfactual assumption that in the absence of National Service both wages and educational attainment of men and women will follow a smooth inter-cohort trend. In other words, we claim that the effect of the abolition of CMS, if any, should only affect men, while women's earnings and educational attainment should be unaffected. Figure 3 reports the evolution of log real earnings for male and female.

We estimate the two specifications above both for men and for women. Since women were exempt from military conscription we expect to see no effect as a result of the abolition of CMS both on earnings and educational attainment.

4. Results

4.1 Effect on Subsequent Earnings

We begin by analyzing the effect of the abolition of CMS using the GHS data on log real earnings. Table 2 reports estimates obtained using the full sample for individuals born between 1933 and 1952. In these specifications we control for trend in birth cohort and age, and the last three column of Table 2 include a control for education.

Our estimates suggest that the abolition of CMS is associated with a higher wage by about 5 percentage points for men who did not serve in the army, this results is very robust across all specifications. The inclusion of polynomial control in birth cohort, control for age and education does not substantially alter both the magnitude and the significance of our estimates coefficients.

Then we perform our estimates restricting our sample, as mentioned in section 3.3. In column (1) of Table 3 we use the sample of males born between 1938 and 1947, while in column (2) and (3) we restrict our sample respectively to individuals born between 1941 and 1944 and between 1942 and 1943. We aim at testing the effect of the abolition of CMS for a wide sample of men (± 5 years from the discontinuity) and then restricting our analysis to cohorts right before and right after the discontinuity, since, as previously explained, these cohorts are presumably similar and this allows us to control for omitted factors.

Estimates substantially confirm results previously obtained. The abolition of CMS leads to an increase in earnings for men exempt from conscription very close to 7 percentage points. Results are robust to the inclusion respectively of polynomial control in age (columns 4, 5 and 6) and age at which individuals left full-time education (columns 7, 8 and 9).

4.2 Effect on educational attainment

We are interested in testing whether the effect of the abolition of CMS on wages is through educational attainment or is mainly due to human capital depreciation and missed experienced in the early labour life. We adopt the same specification used for earnings. LFS recorded month and year of birth, this allows us to look at the direct effect of the abolition of military service on education by comparing educational attainment of cohorts immediately before and after the discontinuity.

Table 4 reports estimates of the effect of abolition of CMS on age at which individuals left full-time education. Columns from (1) to (3) use the same time span previously described for Table 3, column (4) restricts the sample to individuals born in the last two quarters of 1942 and in the first two quarters of 1943, while column (5) consider individuals born in the last quarter of 1942 and in the first one of 1943.

Results suggest that the abolition of CMS affects the age at which individual left full-time education by about a quarter of a year for men when we consider individual born immediately before and after the discontinuity. This effect is very small in magnitude and as discussed in the previous section this has to be related to the fact that the large majority of individuals born between 1933 and 1952 left full-time education before the age of 18. In this context, military conscription would have not affected educational attainment. Furthermore, young high-school graduates were allowed to ask for a deferment until they were 21 years old whether enrolled in a university.

Exploiting the sample size of the LFS, we are able to test more specifically whether military conscription affects the probability of leaving full-time education at different ages. The evidence provided above suggests that the abolition of CMS is more likely to have influenced the probability of leaving full-time education at 18 or older ages, while

we should see no effect on the probability of leaving full-time education at younger ages.

We define three education dummies that equal 1 if age individual left full-time education is respectively bigger than 16, 17 and 18 years (dummy=1 if ageleft >X). We estimate a linear probability model regressing the education dummies on the dummy for the abolition of CMS.

Table 5 present results for males. In columns (1), (2) and (3) the dependent variables used are respectively the education dummies for individuals who left education after 16, 17 and 18 years old. The abolition of National Service positively affects the probability of leaving full-time education at different ages and as expected its effect is stronger when we consider individuals who left education after 18. The effect on enrolment rate is between 2.4 and 4.3 percentage points for males. In other words, males exempt from CMS because born after 1943 were more likely to leave full-time education later than males drafted.

4.3 Robustness checks

We now perform a set of robustness checks to provide further evidence the results obtained in the previous section are not just a chance occurrence.

First, we test the robustness of our results estimating the effect of the abolition of conscription on women. As discussed in section 3.3 we claim that the effect of the abolition of conscription, if any, should only affect men, while since women were exempt from CMS their earning and educational attainment should be unaffected.

As expected, our estimates (Table 6 and 7) show that the abolition of CMS does not have any effect on female earnings. The effect of the abolition on female earnings

seems to be significant for cohorts relatively far from the discontinuity (± 5 years) while it completely disappears when we consider cohorts of women born between 1941 and 1944. This latter finding strengthens our results, since, as expected the abolition of CMS did not affect females earnings, while is significantly and positively correlated to males earnings.

The same holds for educational attainment. The abolition of military conscription does not affect cohorts of females born close to the discontinuity.

Second, we create two fictitious dummies for military conscription assuming that instead of affecting young males born in and after 1943 they will affect respectively young men born in and after 1942 and in and after 1944. The aim of this robustness exercise is to provide additional elements showing that no other factors drive our main results.

Table 9 presents estimates of the effect of fictitious CMS on earnings for men born between 1933 and 1952. In the first three columns we use a fictitious dummy for 1942 birth cohort; while in the last three columns we use a fictitious dummy for 1944 birth cohort.

Our claim is that if the research design is correctly specified we should observe no effect of fictitious abolitions of CMS both on subsequent earnings and on educational attainment.

Results presented in Table 9 and in Table 10 show that the effect of fictitious dummies for the abolition of CMS is not statistically different from zero for both earnings and age at which individuals left full-time education, giving support to our main results.

We conclude that the findings of the previous section are unlikely to be driven by factors other than the abolition of CMS.

5. Discussion

Our results suggest that the effect of military conscription on subsequent earnings is significant and long-lasting. According to our estimates earnings of males who did not serve in the two-year National Service are on average 4 to 7 percentage points higher than the immediately previous cohorts for which military service was compulsory. Our results are in line with previous studies that quantify the effects of military conscription on subsequent earnings. Angrist (1990) estimates that in the early 1980s more than ten years after the end of Vietnam war, the earnings of white veterans were approximately 15 percentage points lower than wages of non-veterans, while Imbens and van der Klaauw (1995) find that military conscription in the Netherlands reduced earnings for individuals who had their careers interrupted to serve in the army for one year by 8 percentage points relative to the non-drafted men of the same cohort.

We find very little evidence that the effect of military service on subsequent earnings occurs through its effect on educational attainment. In fact, our estimates show that the abolition of CMS is associated with an increase in educational attainment by a quarter of year, on average. We also find that compulsory military service affected the probability of being enrolled in school at the age of 18 or more between 2 and 4 percentage points. This result is consistent with Cipollone and Rosolia (2004) who find that exemption from conscription in Italy raised high school graduation rate of boys by about 3 percentage points by comparing high school graduation rates of young exempt men and older not exempt men.

We provide evidence that the abolition of CMS has a positive effect on earnings for exempt cohorts. On average men exempt from military conscription earn from 4 to 7 percentage points more than men who did the National Service; around 1-1,5% is explained by its effect through education[13], while the remaining part is due to the fact that compulsory military service prevents the acquisition of labour experience in early labour life and causes human capital depreciation.[14]

6. Concluding remarks

The effect of military service on subsequent earnings has not been taken into account in the decennial debate on the abolition of military conscription in Western Europe.

This paper provides evidence of the long-term effects of conscription on subsequent earnings. In our analysis, we use a regression-discontinuity approach to obtain unbiased estimates. The RD design used is based on a sharp discontinuity due to the abolition of military conscription in the UK in 1960. All men born in and after 1943 were exempt from CMS. We argue that a comparison of men born close to but before 1943 with men born right after the threshold enables us to identify the causal effect of the abolition of CMS on wages and educational attainment.

The abolition of CMS is shown to have increased earnings of cohort of men exempt by about 4 to 7 percentage points. We further explore whether the effect of military service on earnings occurs through its effect on education. The exemption from CMS increases age men left full-time education by a quarter of a year, suggesting that this ef-

[13] Several studies consistently estimate the returns to education in the UK to be between 5 and 7% for men (Harmon and Walker, 1995 and 1997; Dearden, 1998)

[14] Using our sample we estimate a Mincer equation for men born between 1933 and 1952. Results show that returns to education are very close to 6%, consistent with previous results, and that returns to experience are around 4%.

fect on education is very small in magnitude. Instead, we find that the abolition of CMS affected the probability of being enrolled in school at the age of 18 or more between 2 and 4 percentage points.

We conclude that the long-lasting effects of the abolition of CMS are only partially explained by an increase in education, while the direct effect through labour experience acquisition seems to be more important and relevant. This latter result is in line with the literature on interruptions in early labour career.

We perform a set of controls and robustness checks. First, we estimate the effect of abolition of CMS on women. As expected, since women were exempt from military conscription, we find no effect both on earnings and educational attainment.

Second, we test that using fictitious dummies for the abolition of CMS has no effects both on earnings and age left full-time education.

The research design and a set of robustness checks guarantee our findings are due to the abolition of National Service and not to other factors.

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Fig. 1 – Distribution age left full-time education
(average birth cohort 1933-1952)

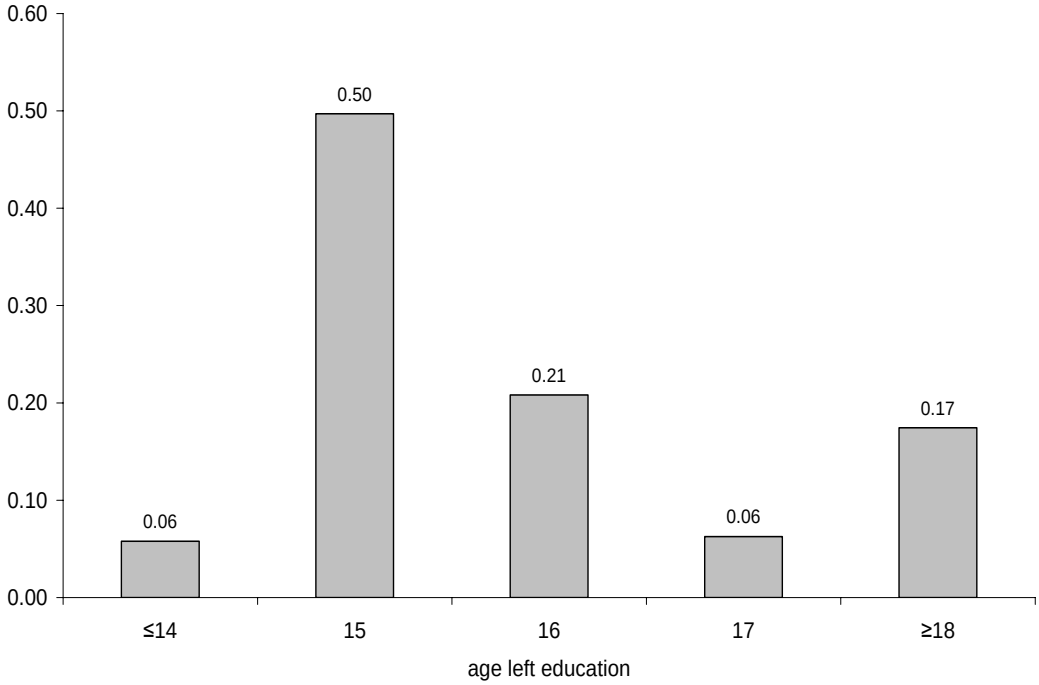

Fig. 2 – Male Log Real Earnings by birth cohort

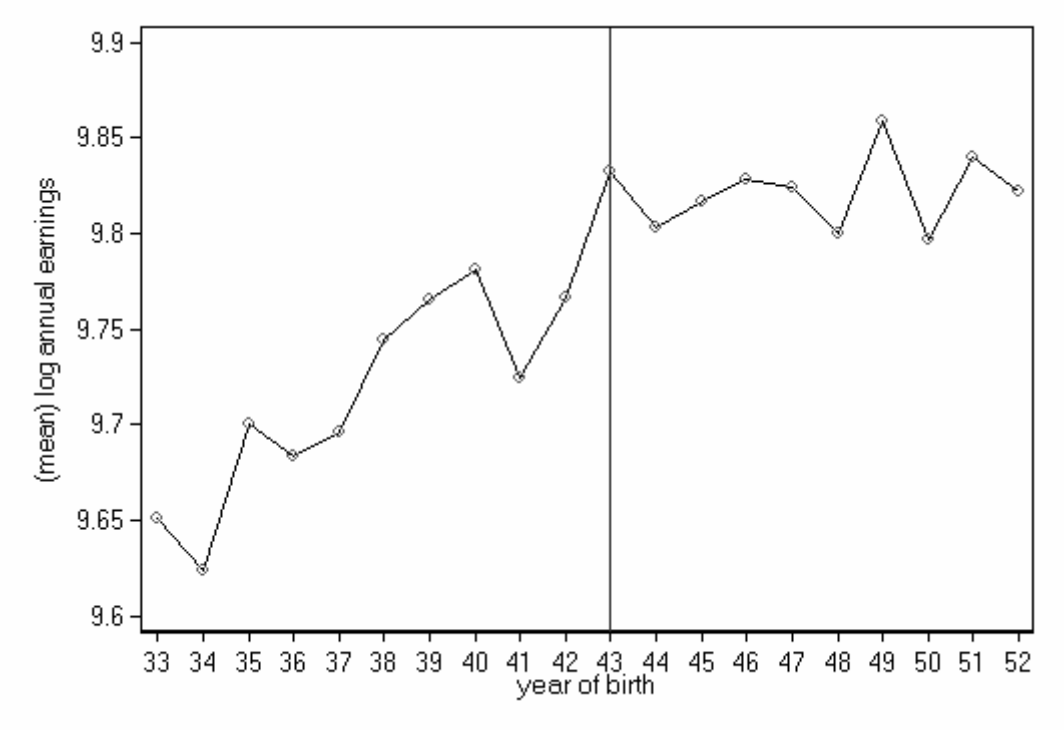

Fig. 3 – Male and Female Log Real Earnings by birth cohort

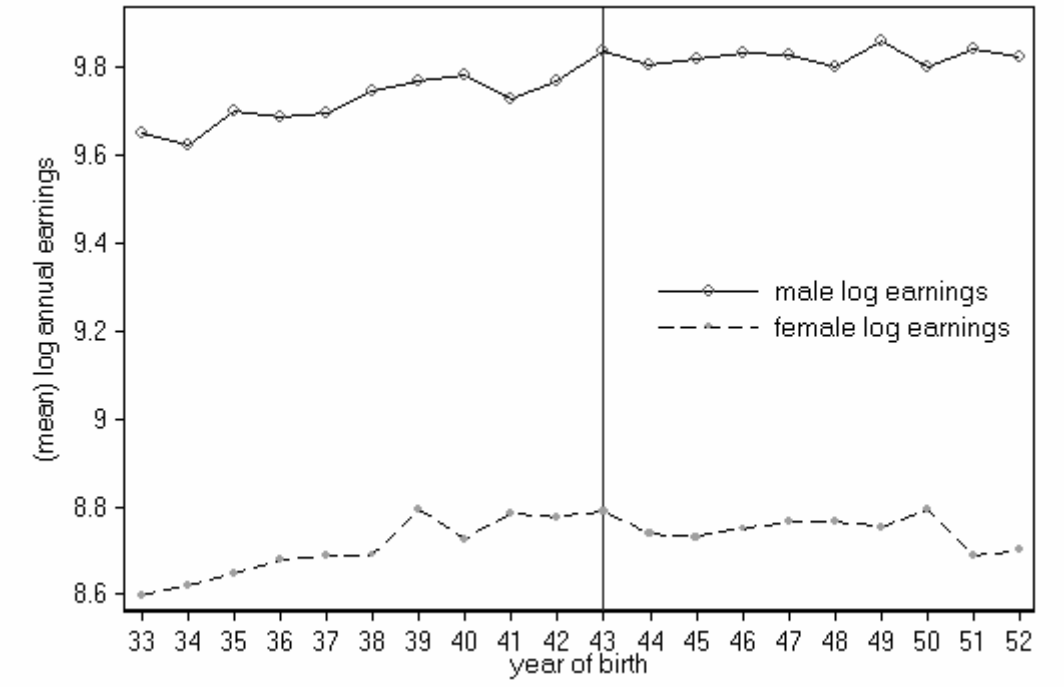

Table 1 – Descriptive Statistics

	Full Sample		Cohort 1942	Cohort 1943		Cohort 1942	Cohort 1943	
	Male	Female	Male	Male	T-ratio for diff in sample means	Female	Female	T-ratio for diff in sample means
Age	45.34 (6.4)	45.41 (6.2)	47.22	46.29	7.85	47.30	46.36	6.94
Age Left Educ	17.15 (3.6)	16.94 (3.4)	16.88	16.89	0.15	16.72	16.79	0.56
Log Earning	9.78 (.63)	8.73 (.99)	9.76	9.83	2.81	8.76	8.79	0.37
Sample size	28,073	24,146	1,334	1,416		1,238	1,307	

Table 2 – Effect of Abolition of CMS on Males Log Annual Earnings
Birth cohort 1933-1952

	(1)	(2)	(3)	(4)	(5)	(6)
CMS	0.0471 [0.0205]**	0.0494 [0.0205]**	0.0441 [0.0202]**	0.0492 [0.0198]**	0.0514 [0.0198]***	0.0465 [0.0196]**
Age Left Educ				0.0408 [0.0012]***	0.0405 [0.0012]***	0.0384 [0.0012]***
Birth Cohort Polynomial Controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
Age Polynomial Controls	None	Quartic	None	None	Quartic	None
Survey Year Fixed Effect	No	No	Yes	No	No	Yes
N. Obs.	28,073	28,073	28,073	28,073	28,073	28,073
R^2	0.01	0.02	0.03	0.06	0.07	0.08

Notes: The dependent variable is log annual earnings. CMS dummy is equal to 1 for individuals born at and after 1943 and 0 otherwise. Each regression includes a control for a birth cohort quartic polynomial. Column (2), (3), (5) and (6) also include age controls: a quartic polynomial and a survey year fixed effect whether indicated. We use the full sample of men born between 1933 and 1952. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3 – Effect of Abolition of CMS on Male Log Annual Wage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	± 5 years	± 2 years	± 1 years	± 5 years	± 2 years	± 1 years	± 5 years	± 2 years	± 1 years
CMS	0.0642	0.0702	0.0666	0.0268	0.0751	0.0732	0.0111	0.0676	0.0786
	[0.0105]***	[0.0169]***	[0.0237]***	[0.0126]**	[0.0174]***	[0.0234]***	[0.0122]	[0.0168]***	[0.0225]***
Age Left Educ							0.0432	0.0457	0.0438
							[0.0016]***	[0.0028]***	[0.0040]***
Age Polynomial Controls				Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
N. Obs.	14,545	5,484	2,750	14,545	5,484	2,750	14,463	5,450	2,733

Notes: The dependent variable is log annual earnings. CMS dummy is equal to 1 for men born at and after 1943 and 0 otherwise. Columns (4), (5) and (6) include a fourth order polynomial in age. Columns (7), (8) and (9) also include age at which individuals left full-time education. For regression in columns (1), (4) and (7) we use a subsample of men born between 1938 and 1947, while for regression in columns (2), (5) and (8) we further restrict our sample to men born between 1941 and 1944. Columns (3), (6) and (9) present regression including the sample of men born in 1942 and 1943. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4 – Effect of Abolition of CMS on Males Age Left Education

	(1) ± 5 years	(2) ± 2 years	(3) ± 1 year	(4) ± 6 months	(5) ± 3 months
CMS	0.3091 [0.0170]***	0.1357 [0.0271]***	0.0875 [0.0381]**	0.1427 [0.0531]***	0.2575 [0.0775]***
Constant	16.0766 [0.0124]***	16.1624 [0.0192]***	16.2170 [0.0273]***	16.1775 [0.0383]***	16.1442 [0.0533]***
N. Obs.	77,625	30,410	15,639	7,896	3,701

Notes: The dependent variable is the age individual left full-time education. CMS dummy is equal to 1 for men born at and after 1943 and 0 otherwise. For regression in columns (1), we use a subsample of men born between 1938 and 1947, while for regression in columns (2) and (3) we restrict the subsample respectively to men born between 1941 and 1944 and 1942 and 1943. In columns (4) and (5) we further restrict to the subsample of men born between June 1942 and May 1943 and between October 1942 and March 1943. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5 – Effect of Abolition of conscription on the males probability of being enrolled at different ages

	June 1942 – June 1943			October 1942 – March 1943		
	(1)	(2)	(3)	(1)	(2)	(3)
	Ageleft>16	Ageleft>17	Ageleft>18	Ageleft>16	Ageleft>17	Ageleft>18
CMS	0.0176	-0.0088	0.0246	0.0078	-0.0037	0.0437
	[0.0088]**	[0.0055]	[0.0084]***	[0.0131]	[0.0080]	[0.0124]***
Constant	0.1808	0.0663	0.1572	0.1918	0.0641	0.1523
	[0.0064]***	[0.0041]***	[0.0060]***	[0.0094]***	[0.0059]***	[0.0086]***
N. Obs.	7,896	7,896	7,896	3,701	3,701	3,701

Notes: The dependent variable is a dummy equal to 1 if age finished full time education is respectively bigger than 16, 17 and 18 (dummy=1 if ageleft>X) and 0 otherwise. CMS dummy is equal to 1 for men born at and after 1943 and 0 otherwise. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6 – Effect of Abolition of CMS on Females Log Annual Earnings
Birth cohort 1933-1952

	(1)	(2)	(3)	(4)	(5)	(6)
CMS	-0.0444	-0.0457	-0.0443	-0.0449	-0.0464	-0.0457
	[0.0330]	[0.0328]	[0.0327]	[0.0320]	[0.0318]	[0.0318]
Age Left Educ				0.0786	0.0788	0.0761
				[0.0019]***	[0.0019]***	[0.0019]***
Birth Cohort Polynomial Controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
Age Polynomial Controls	None	Quartic	None	None	Quartic	None
Survey Year Fixed Effect	No	No	Yes	No	No	Yes
N. Obs.	24,146	24,146	24,146	24,146	24,146	24,146
R^2	0.00	0.02	0.03	0.07	0.09	0.09

Notes: The dependent variable is log annual earnings. CMS dummy is equal to 1 for individuals born at and after 1943 and 0 otherwise. Each regression includes a control for a birth cohort quartic polynomial. Column (2), (3), (5) and (6) also include age controls: a quartic polynomial and a survey year fixed effect whether indicated. We use the full sample of women born between 1933 and 1952. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7 – Effect of Abolition of CMS on Females Log Annual Wage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	± 5 years	± 2 years	± 1 years	± 5 years	± 2 years	± 1 years	± 5 years	± 2 years	± 1 years
CMS	-0.0011	-0.0183	0.0138	0.1198	0.0504	0.0513	0.0900	0.0347	0.0456
	[0.0170]	[0.0270]	[0.0367]	[0.0194]***	[0.0279]*	[0.0376]	[0.0189]***	[0.0273]	[0.0369]
Age Left Educ							0.0771	0.0720	0.0675
							[0.0025]***	[0.0040]***	[0.0061]***
Age Polynomial Controls				Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
N. Obs.	12,961	5,035	2,545	12,961	5,035	2,545	12,801	4,967	2,508

Notes: The dependent variable is log annual earnings. CMS dummy is equal to 1 for women born at and after 1943 and 0 otherwise. Columns (4), (5) and (6) include a fourth order polynomial in age. Columns (7), (8) and (9) also include age at which individuals left full-time education. For regression in columns (1), (4) and (7) we use a subsample of women born between 1938 and 1947, while for regression in columns (2), (5) and (8) we further restrict our sample to men born between 1941 and 1944. Columns (3), (6) and (9) present regression including the sample of women born in 1942 and 1943. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8 – Effect of Abolition of CMS on Female Age Left Education

	(1) ± 5 years	(2) ± 2 years	(3) ± 1 year	(4) ± 6 months	(5) ± 3 months
CMS	0.2575	0.0669	0.0039	-0.0274	-0.0242
	[0.0139]***	[0.0220]***	[0.0304]	[0.0433]	[0.0642]
Constant	15.9438	16.0226	16.0543	16.0781	16.1621
	[0.0101]***	[0.0159]***	[0.0224]***	[0.0326]***	[0.0480]***
N. Obs.	78,906	30,771	15,913	7,963	3,864

Notes: The dependent variable is the age individual left full-time education. CMS dummy is equal to 1 for men born at and after 1943 and 0 otherwise. For regression in columns (1), we use a subsample of women born between 1938 and 1947, while for regression in columns (2) and (3) we restrict the subsample respectively to women born between 1941 and 1944 and 1942 and 1943. In columns (4) and (5) we further restrict to the subsample of women born between June 1942 and May 1943 and between October 1942 and March 1943. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9 – Effect of Abolition of conscription on the females probability of being enrolled at different ages

	June 1942 – June 1943			October 1942 – March 1943		
	(1)	(2)	(3)	(1)	(2)	(3)
	Ageleft>16	Ageleft>17	Ageleft>18	Ageleft>16	Ageleft>17	Ageleft>18
CMS	0.0151	0.0077	-0.0089	0.0038	0.0177	-0.0053
	[0.0091]*	[0.0062]	[0.0078]	[0.0130]	[0.0091]*	[0.0116]
Constant	0.1989	0.0786	0.1462	0.2043	0.0800	0.1550
	[0.0066]***	[0.0044]***	[0.0058]***	[0.0094]***	[0.0063]***	[0.0085]***
N. Obs.	7,963	7,963	7,963	3,864	3,864	3,864

Notes: The dependent variable is a dummy equal to 1 if age finished full time education is respectively bigger than 16, 17 and 18 (dummy=1 if ageleft>X) and 0 otherwise. CMS dummy is equal to 1 for individuals born at and after 1943 and 0 otherwise. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9 – Fictitious CMS dummies on Log Earnings

	CMS 1942			CMS 1944		
	(1)	(2)	(3)	(4)	(5)	(6)
CMS	0.0184	0.0216	0.0191	-0.0062	-0.0051	-0.0094
	[0.0223]	[0.0223]	[0.0220]	[0.0200]	[0.0199]	[0.0197]
Birth Cohort Polynomial Controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
Age Polynomial Controls	None	Quartic	None	None	Quartic	None
Survey Year Fixed Effect	No	No	Yes	No	No	Yes
N. Obs.	28,073	28,073	28,073	24,146	24,146	24,146
R^2	0.01	0.02	0.03	0.00	0.02	0.03

Notes: The dependent variable is log annual earnings. Fictitious CMS dummy in column (1), (2) and (3) is equal to 1 for men born at and after 1942 and 0 otherwise, while in columns (4), (5) and (6) it is equal to 1 for men born at and after 1944 and 0 otherwise. Each regressions include a control for a birth cohort quartic polynomial. Column (2), (3), (5) and (6) also include age controls: a quartic polynomial and a survey year fixed effect whether indicated. For regression in columns (1), (2) and (3) we use the full sample of men born between 1933 and 1952, while for regression in columns (4), (5) and (6) we use the full sample of women born between 1933 and 1952. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10 – Fictitious CMS dummies on Age Left Full-time Education

	CMS 1942		CMS 1943	
	(1)	(2)	(3)	(4)
	± 6 months	± 3 months	± 6 months	± 3 months
CMS	0.1078	0.0132	-0.0056	-0.0758
	[0.0552]*	[0.0762]	[0.0542]	[0.0780]
Constant	16.1466	16.1698	16.2874	16.3454
	[0.0392]***	[0.0556]***	[0.0386]***	[0.0556]***
N. Obs.	7,482	3,697	7,673	3,735

Notes: The dependent variable is age left full-time education. Fictitious CMS dummy in column (1) and (2) is equal to 1 for men born at and after 1942 and 0 otherwise, while in columns (3) and (4) it is equal to 1 for men born at and after 1944 and 0 otherwise. For regression in columns (1) and (3) and (3) we use the sabsample of men born between June 1942 and May 1943, while for regression in columns (2) and (4) we restrict the sample to men born between October 1942 and March 1943. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%.