ECONOMIC RESEARCH REPORTS

EVALUATING THE COST OF CONSCRIPTION IN THE NETHERLANDS

BY

Guido Imbens
and
Wilbert van der Klaauw

RR # 93-24 *May, 1993*

C. V. STARR CENTER FOR APPLIED ECONOMICS

NEW YORK UNIVERSITY
FACULTY OF ARTS AND SCIENCE
DEPARTMENT OF ECONOMICS
WASHINGTON SQUARE
NEW YORK, N.Y. 10003

Evaluating the Cost of Conscription in the Netherlands[1]

Guido Imbens – Harvard University[2] and NBER
Wilbert van der Klaauw – New York University[3]

February 1993

[1]Research support was provided by the C.V. Starr Center for Applied Research at New York University. We are grateful for comments and suggestions by Chris Flinn and Robert Moffit. We also wish to thank P. Mulders from the Defense Department (Ministerie van Defensie) and B. Grubben from the Central Bureau for Statistics (Centraal Bureau voor de Statistiek) for providing us with the data used in the research reported here. We alone are responsible for any errors.

[2]mailing address: Department of Economics, Harvard University, Cambridge, MA 02138
[3]mailing address: Department of Economics, New York University, New York, NY 10003

Evaluating the Cost of Conscription in the Netherlands

Abstract

Since early this century the armed forces in the Netherlands get most of their recruits through compulsory conscription rather than voluntary enrollment. Recently there has been a discussion on abolishing this system of compulsory conscription and replace it with a system of voluntary enrolment in the light of the collapse of the threat from the former Soviet–Union. In this paper we focus on one aspect of the compulsory military service that has received little attention in this debate: the cost of military service in terms of its effect on future earnings.

Estimating the cost of military service to individuals is more complicated than comparing the earnings for veterans and non–veterans because of various forms of self–selection that occur even if military service is formally compulsory. Angrist (1990) dealt with similar selection problems in estimating the cost of serving in the military for Vietnam–era veterans in the United States by using the draft lottery that was used to determine priority for enrolment in the military as an instrument. We follow a similar approach using variation in the aggregate enrolment rates generated by government decisions concerning the number of people needed in particular years.

We find that the yearly cost to individuals of serving in the military was approximately 3000 Dutch Guilders (1988 Guilders, approximately $1500 in 1988 US Dollars), or 8 percent of average annual earnings for the relevant cohorts. Our estimates are relatively high compared to estimates of the returns to schooling and experience, which are approximately equal to 5.5% and 4.0% of earnings for these cohorts. This suggests that the cost of serving in the military might be more than just the effect of losing one year of potential experience or education.

For the estimation we use a large data set on earnings from the Central Bureau for Statistics containing both the mean and variance of earnings by month of birth cohorts, and data on the number of potential recruits and number of enlisted men per birth year cohort. We use a weighted version of the grouped IV estimation procedures described by Angrist (1991), with the weights proportional to the monthly earnings variances.

1. INTRODUCTION

Since early this century the armed forces in the Netherlands get most of their recruits through compulsory conscription rather than voluntary enrollment. Recently there has been a discussion on abolishing this system of compulsory conscription and replace it with a system of voluntary enrolment in the light of the collapse of the threat from the former Soviet–Union. In this paper we focus on one aspect of the compulsory military service that has received little attention in this debate: the cost of military service in terms of its effect on future earnings. An exception is Duindam (1992) who discusses the total cost to society of military service, including both the direct cost in terms of earnings foregone during the service as well as the earnings effect in years following the period of active service. He takes estimates of the latter from those reported by Angrist (1990) who estimated these earnings effects for Vietnam–era veterans in the United States. In this paper we estimate the effect of military service on future earnings, i.e. earnings in years following the period of active service, using Dutch earnings data.

Estimating the cost of military service to individuals is more complicated than comparing the earnings for veterans and non–veterans because of the self–selection that occurs even if military service is formally compulsory. One selection effect that might lead to an understatement of the cost of military service, or an overstatement of the benefits, is that recruits have to pass medical and psychological examinations. Another selection effect, with an ambiguous effect, is that men who are married and who are the main source of income

in their family, and men who are indispensible in their work (mostly men working in family businesses), are exempt from military service. A third source of potential selection bias is the system of temporary exemptions granted to men who are enrolled in higher education. These temporary exemptions often lead to full exemptions because there is also an age limit after which one cannot be called for military service.

An additional complication in estimating the return to military service at the individual level is the lack of large national surveys in the Netherlands with both individual level earnings data as well as information on military service records.

Angrist (1990) dealt with similar selection problems in estimating the cost of serving in the military for Vietnam–era veterans in the United States by using the draft lottery that was used to determine priority for enrolment in the military as an instrument. We follow a similar approach using variation in the aggregate enrolment rates generated by government decisions concerning the number of people needed in particular years. We use two specific sources of variation. First we use the fact that one birth year cohort in the late fifties was exempted from military service. Second, for other years the government changed the standards that had to be met by potential recruits in physical and psychological examinations, to manipulate the number of recruits that would eventually be inducted. This led to considerable variation in the proportion of cohorts that actually served, ranging from 0% for the cohort born in 1959 to 46% for the cohort born in 1960.

We use these sources of variation in two different ways. First we use the aggregate

percentage of men in a particular birth cohort that actually served in the armed forces as an instrument for individual enrolment status in a log earnings regression. In a second attempt we use the percentage of people who were eligible according to the 'normal' standards, but not according to the standards for a particular year as an instrument. This group was designated 'special exemption' (buitengewoon dienstplichtig) and was relieved from military service except in case of a national emergency. This designation was known to the recruits, and records on the actual size of these groups are available.

We estimate the cost of military service using both instruments separately. In both cases we find that the yearly cost to individuals of serving in the military was approximately 3000 Dutch Guilders (1988 Guilders, approximately $1500 in 1988 US Dollars), or 8 percent of average annual earnings for the relevant cohorts. These estimates are comparable to those of Angrist who finds a cost of approximately 15 percent for two years of service. Our estimates are relatively high compared to estimates of the returns to schooling and experience, which are approximately equal to 5.5% and 4.0% of earnings for these cohorts. This suggests that the cost of serving in the military might be more than just the effect of losing one year of potential experience or education.

For the estimation we use a large data set on earnings from the Central Bureau for Statistics containing both the mean and variance of earnings by month of birth cohorts, and data on the number of potential recruits and number of enlisted men per birth year cohort. We use a weighted version of the grouped IV estimation procedures described by Angrist

(1991), with the weights proportional to the monthly earnings variances. We analyze the sensivity of our results to alternative specifications of the age profile, inclusion and exclusion of additional years of data, and in particular to dropping the 1959 data where the entire cohort was exempt. This sensitivity analysis suggests that the qualitative findings of our study are robust to the particular specification chosen.

2. The Draft in The Netherlands

For a long time the armed forces in The Netherlands have relied on compulsory military conscription to fill their ranks. During a representative year about 50% percent of the armed forces consist of conscripts[2], with the other 50% made up of 40% professional soldiers and 10% civilians working for the armed forces, the latter mostly in jobs at the Defense department. These numbers understate the importance of conscripts for the armed forces since the reserves that can be called in times of emergency consist almost entirely of conscripts, implying that in times of war the percentage of conscripts in the armed forces would go up to about 80%. By law all able bodied men[3] have to serve 14 months in the armed forces, and although over time a number of exemptions have been written into this law, during the early eighties typically at least 40% of a given birth cohort of men would actually serve in the armed forces.

All men are called for medical and psychological examinations during the year in which they turn seventeen[4]. On the basis of these examinations the potential recruits are divided

[2]This differs depending on the the type of armed forces, with conscripts making up 70% of the personnel in the army, 10% in the airforce and 20% in the navy.

[3]Women are exempt from military service in The Netherlands.

[4]Before 1979 these examinations were done in the year they turn eighteen.

into a number of categories. About 30% fall in categories that immediately exempt them from military service, some of them for serious medical conditions, but others for reasons less obviously related to fitness to serve, such as height exceeding 200cm (6ft 5in). Those that fall into categories that do not exempt them from military service are not immediately called to perform their military service. Most potential recruits have at least temporary exemptions based on enrolment in school. Once all (temporary) exemptions are exhausted a recruit is available for service. For about 80% of the recruits this occurs in July or August when the academic year ends. Because the armed forces prefer a steady flow of recruits few people are called immediately following the end of their exemptions. In fact the average time between becoming available for service and actually entering the military is five to six months. During this waiting time recruits can be called at any moment, so taking on permanent employment is difficult, and one may expect the cost of military service to be affected by this waiting period. A non–trivial percentage (varying from 0% to 8% over the years for which we have data) have not been called 14 months after first availability. This group is then granted a permanent exemption.

Once called to serve, recruits typically serve 14 months[5]. About 15% of the recruits are invited, and subsequently accept the invitation, to be trained as officers. This means slightly better pay and working conditions, but also serving for 16 rather than 14 months. During the 14 or 16 months of service recruits receive basic training for two and a half months, and

[5]Recently this was lowered to 12 months, but during the period for which we have data the old rules were still in force.

are on active duty for the remainder of the period.

Following their service conscripts are placed in reserve, which entails few obligations in peace time.

The tax aspect of the military service has never received much attention in The Netherlands (the exception being Duindam, 1992). If the entire population serves, one might worry about the efficiency of such a tax, but at least there is an equality of the burden. However, the fact that only about 40% percent of a given cohort of men serves, suggests that the tax is not equally distributed across the male population. In particular, exemptions granted on the basis of enrolment in higher education have led to a redistribution of the tax burden away from the higher educated towards the lower educated. In this respect it is important to note that while enrolment in higher education typically gives the right only to a temporary exemption, in practice any delay increases considerably the chances of an eventual permanent exemption and consequently men with higher education are underrepresented among recruits (Duindam 1992). In addition to these educational exemptions there are exemptions for those who are 'indispensable', often in family run businesses, and those who reside abroad, outside the European Community. Another category that has increased in importance in recent years is that of conscientious objectors. While formally one has to convince a committee that one's objections to military service are legitimate, in practice this is not a difficult hurdle to take. Once accepted as a legitimate conscientious objector, one still has to perform public service for 16 months, but there is considerable freedom in

finding a place that is willing to take someone on for 16 months as a low paid conscientious objector. In fact, these jobs often lead to permanent positions. The cost associated with this alternative kind of social service is likely to be much lower than that of regular military service, in which 75% of the recruits report being bored for more than two hours a day out of eight hours of work (Duindam, 1992).

Estimating the cost of military service is therefore of importance in determining the regressiveness of this tax, and as an argument in the debate on the merits of a conscription–based versus a volunteer army. Here we focus on only one aspect of this tax, namely the effect of military service on earnings in the years following the period of active service.

3. Identifying the Cost of Conscription

Estimation of the return to military service, or the cost or benefits of conscription is complicated by two problems. The first is the lack of a large survey based data set containing information both on earnings and on military service histories in The Netherlands. While there are a number of large cross section data sets, and some recent panel data sets, that have information on earnings and current military service status, there are none that have questions on life histories going back far enough to determine whether someone has actually performed his military service. The second problem is that even if such data were available, selection problems would severely constrain their usefulness. Whether someone serves is only partially determined by exogenous factors, and determined to a considerable extent by personal motivation and by variables directly related to earnings, such as health

status and decisions on education. In fact there are a number of lawyers specializing in military conscription, with clients willing to spend up to 5000 Dutch Guilders to stay out of the military. This is evidence both of an awareness of the cost of military service, and of the recognition that induction is not a predetermined, exogenous event. This implies that comparing differences in average earnings for treatment and control groups, i.e. those who served and those who did not serve, would not lead to credible estimates of the cost or benefit of military service.

We attempt to address what is typically referred to as the selection problem, or the endogeneity or non–ignorability of the treatment indicator, by focussing on changes at the aggregate level of treatment (i.e. military service) over time. While there is considerable evidence that the individual cohort member's enlistment decision is endogenous, we assume that the average percentage of conscripts in a cohort is not, or at least less, affected by the arguments advanced before for the endogeneity of the military service indicator. The idea of aggregating to a level where the selection problem disappears has a long tradition in evaluation methodology. See Cook and Campbell (1979) for a comprehensive survey.

One might expect that there is typically little variation over time in the proportion of men who actually serve in the armed forces. In that case, even if one believed that the aggregate rate of service is not subject to the selection problems discussed above, there would be little point in trying to estimate the returns to military service in that manner. However, during the seventies and eighties there was considerable variation in the aggregate conscription rates

due to government policies that lead to the kind of exogenous variation that we are looking for. During this period, two things happened. First of all, the government lowered the age at which potential recruits are called for the medical examinations from eighteen to seventeen. This did not have much affect on the average age at which people actually perform their military service, since most potential recruits have at least temporary exemptions at the time of their medical examinations as most of them are still in school. However, a more dramatic outcome of this policy change was that a complete birth year cohort, all men born in 1959, were completely exempt from military service.

The second government–induced variation in the aggregate rate of service stems from the fact that the military were confronted with a surplus of able bodied recruits[6]. Rather than strengthen the requirements for passing the medical and psychological examations, it was decided to create a new category, 'special exemption' ('buitengewoon dienstplichtig'). Every year a number of the categories that were acceptable according to the normal guidelines, were given this designation, with the explicit idea of limiting the number of recruits that would actually be called. Someone who fell in this category would not be drafted, unless in case of emergency (i.e. war). At the beginning of each year projections were made of the number of recruits needed, and the number of men that would fall in each of the (old) categories. On the basis of these predictions some of the old categories would then be designated 'special exemption'.

[6]The excess supplies in these years were mainly due to fluctuations in demand caused by political decisions on the defense budget and a changing role of The Netherlands within the NATO.

In an instrumental variables interpretation of the estimation strategy we could therefore define as instrumental variables a full set of dummies for all the categories, defined as outcomes for the medical examinations, indicating whether each category was designated 'special exemption' in a given period. The variation over time in these dummy variables identifies the effect of military service without being affected by selection bias because it was set by the government without reference to potential outcomes.[7] Unfortunately, we do not know all the categories and proportions of the cohorts that fell in each of them for all the years. However, the above discussion implies that we may treat the proportion of the cohort that actually serves as exogenous, in other words, as not subject to the selection problems plagueing comparisons of veterans and non–veterans at the individual level. As an alternative we also used the annual proportion of each cohort that was in the category 'special exemption' as the (exogenous) instrument for the indicator for military service. In each case we identify the effect of military service on earnings from the variation over time in the proportion of people that actually serves, rather than compare within a given cohort men who served with men who did not serve.

A problem with both approaches is that the period during which this exogenous variation occurred is so recent that the people affected are currently still on a relatively steep part of their life time earnings profile. We therefore have to take special care in estimating the age profile of earnings.

[7]The government did try to predict the number of people in each category but there is little evidence that there was much variation in this over time. This is one of the reasons for investigating the robustness of our results to the inclusion of cohort size in the regressions.

The main specification for the log earnings regression that we estimate is

$$\ln Y_i = \beta_0 + \beta_1 \cdot A_i + \beta_2 \cdot A_i^2 + \beta_3 \cdot A_i^3 + \beta_4 \cdot S_i + \varepsilon_i, \tag{1}$$

where Y_i are the earnings for individual i, A_i is his age and S_i is a dummy variable, equal to one if person i served in the military, and zero otherwise. As instruments for S_i we use either $\bar{S}_i$, the proportion of person i's birth cohort that served, or E_i, the proportion of person i's birth cohort that was designated 'special exemption'.

4. The Data

We use two sources of data. First, we obtained from the Defense Department the proportions of each birth cohort that actually served in the military, the proportion that was designated 'special exemption', the percentage that has not yet fulfilled his obligations towards military service, which we will denote as the percentage 'active'[8], as well as the size of each cohort.

The second data set consists of the mean and variance of earnings in 1989 by month of birth calculated from the 'inkomen panel onderzoek', a data set collected by the Central Bureau for Statistics in The Netherlands. These data form a random sample of about 1% of social security data for the relevant cohorts. For men in the age cohorts we are analyzing the social security data represent about 95% of all men in these cohorts. The data are not topcoded, which is part of the reason that we were not given access to the individual level

[8]This is a category that is important for the recent cohorts, and consists mainly of people enrolled in higher education.

data.

In Table 1 we give summary statistics for both data sets.

Table 1: ESTIMATES OF THE COST OF SERVING IN THE MILITARY

Birthyear	% Served	% Spec. Exemp.	% Active	Cohort Size	Ave. Earn. in fl.
1956	44.2%	–	0.0%	122,121	45,752
1957	34.1%	–	0.0%	120,188	44,602
1958	41.3%	4.6%	0.7%	121,901	43,927
1959	0.0%	–	0.0%	121,000	42,100
1960	45.8%	2.3%	0.5%	122,948	39,355
1961	36.9%	16.8%	0.3%	127,315	38,418
1962	32.3%	23.3%	0.2%	127,991	37,137
1963	45.3%	3.2%	0.5%	130,763	34,470
1964	44.8%	3.3%	0.6%	132,378	32,458
1965	33.3%	27.4%	0.7%	126,392	29,127
1966	33.4%	29.9%	1.4%	125,011	25,616
1967	36.2%	21.5%	3.2%	129,983	21,519
1968	38.8%	8.4%	6.5%	125,512	16,310
1969	38.7%	3.1%	10.4%	131,029	12,749
Average	36.2%	–	1.8%	126,038	30,900

A simple estimate of the returns to, or cost of, military service can be obtained by comparing the average earnings for the exempt year (1959, with average earnings of fl. 42,100), to the average of the two years around that year (1958 with fl. 43,927, and 1960 with fl. 39,355, which gives an average of fl. 41,641). Dividing the difference by the average probability of serving in 1958 and 1960 gives fl. (42,100-41,641)/0.44=1,043 (approximately $500) as a rough estimate. Our estimates in the next section are aimed at obtaining more accurate estimates by using the variances of the earnings data, and by utilizing the variation

in the percentage served in other years besides 1958–60.

5. Estimation Results

To evaluate the effect of military service on earnings, we estimated a log–earnings equation in which the logarithm of average earnings for each monthly cohort is related to the percentage of cohort members that served in the army and a cubic function in age, to capture the general age–earnings profile. The estimation sample includes the average 1989 earnings of all monthly birth cohorts in the period of january 1956 to december 1966. We choose to use the data from only these years mainly on the basis of the proportion of the cohorts that was in the category 'active'. In Section 6 we investigate the sensitivity of our results to the exclusion of additional years. The original data consist of the mean μ_Y and variance σ_Y^2 of earnings. We transform this into the mean and average of the logarithm of earnings assuming a log normal distribution for earnings. In that case the mean of the log of earnings is $\mu_{\ln Y} = 2\ln\mu_Y - (1/2)\ln(\mu_Y^2 + \sigma_Y^2)$, and $\sigma_{\ln Y}^2 = \ln(\mu_Y^2 + \sigma_Y^2) - 2\ln\mu_Y$. Since all regressors are identical for all individuals born in a given month, the individual level regression specified earlier is therefore equal to a regression with the month of birth as the unit of observation. The actual regression equation we estimate is therefore not equation (1) but

$$\overline{\ln Y}_t = \beta_0 + \beta_1 \cdot A_t + \beta_2 \cdot A_t^2 + \beta_3 \cdot A_t^3 + \beta_4 \cdot \bar{S}_t + \varepsilon_t, \tag{2}$$

where all variables are indexed by month of birth running from January 1956 to December 1966. We estimate this regression by weighted least squares in order to use the variances in

the earnings dataset. The weighted least squares estimates are presented in Table 2. The statistically significant parameter estimate of -0.080 implies that military service leads on average to a 8% reduction in earnings. Given that average earnings in 1989 equal fl.36,600 (or about $18,000), this implies an annual cost of approximately fl.3,000 or $1,500 in 1989 dollars.

To evaluate the fit of this model, figure 1 shows the predicted average (log) earnings as well as the actual mean and 95% confidence interval of the (log) earnings by age. The model fits the data fairly well and the predicted values of the average log earnings almost always lie within twice the standard deviation of the actual cohort average.

Next, as an alternative to the actual percentage of a cohort that served in the armed forces, we used the proportion of a cohort that was exempt from military service and was designated 'special exemption' as an instrument for individual military service completion. The same average log–earnings equation was estimated but now with the 'percentage special exemption' used as instrument for 'percentage served'. As data on this instrument was missing for the first two years in our data, 1956 and 1957, only monthly cohort data for the 1958 to 1966 period were used. Further, for the year 1959, in which the entire birth year cohort was exempt of military service, the percentage of individuals exempt according to the usual special exemption criteria was set at 80%. This number was arrived at by regressing the 'proportion served' on the 'proportion special exemption' for the years for which data were available, and then calculating the 'percentage special exemption' that would correspond to

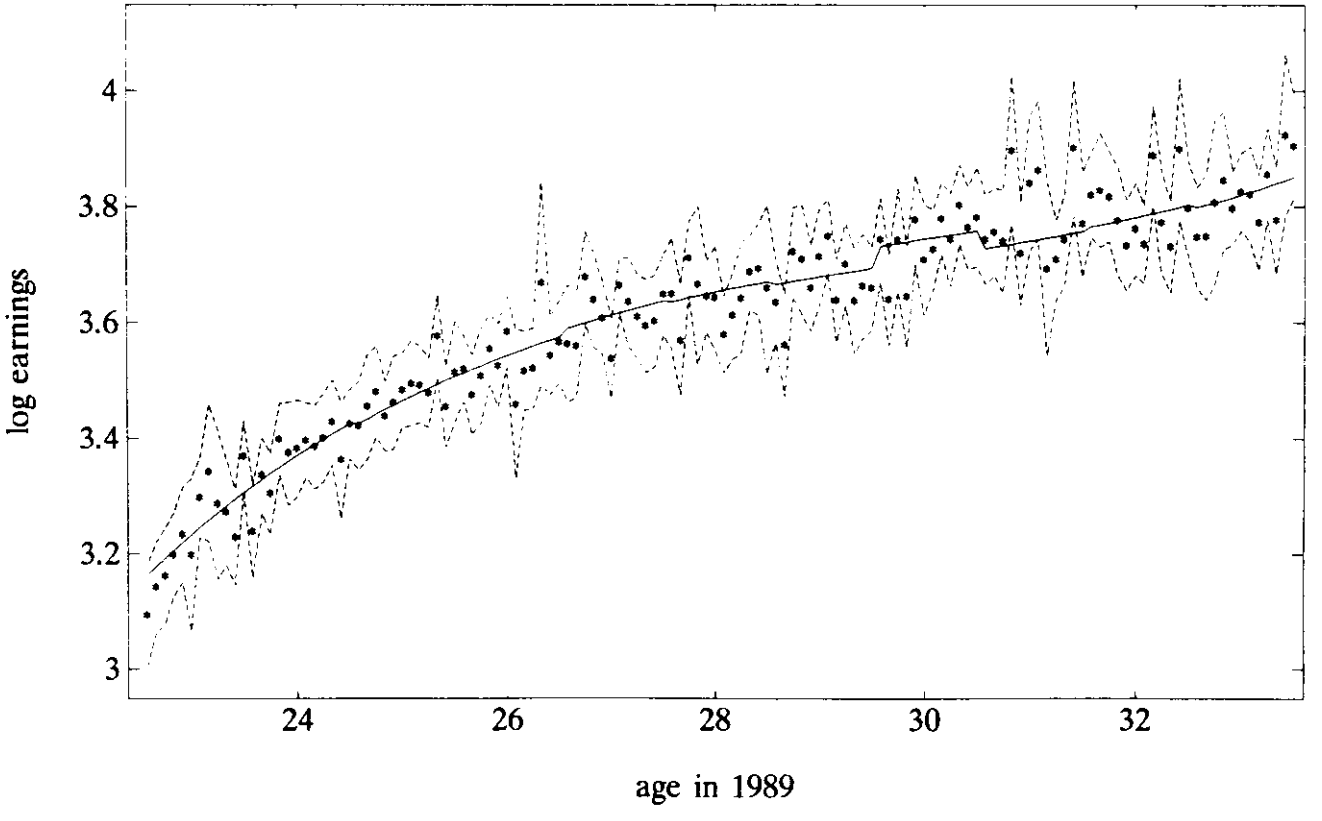
Table 2: ESTIMATES OF THE COST OF SERVING IN THE MILITARY

Var	Coeff.	(s.e.)	T-value
Intercept	3.661	0.014	267.3
Percentage Served	-0.080	0.035	-2.3
Age	0.426	0.033	12.7
Age Squared	-0.633	0.050	-12.6
Age Cubed	0.916	0.178	5.2
N=132			

Table 3: PROPORTION EXEMPT AS INSTRUMENT FOR THE PERCENTAGE SERVED

Var	Coeff.	(s.e.)	T-value
Intercept	3.618	0.007	506.7
Percentage Served	-0.078	0.035	-2.2
Age	0.379	0.041	9.3
Age Squared	-0.481	0.086	-5.6
Age Cubed	1.514	0.324	4.7
N=108			

Figure 1. expected and actual log earnings in 1989



a 'percentage served' of 0%.

As shown in Table 2, the IV estimate of 0.79 implies that the cost to individuals belonging to the included cohorts of serving in the military was approximately 7.9% of annual 1989 earnings, which is almost identical to our previously found estimate of 8%. The closeness between the two estimates increases confidence in our result. Below we investigate the sensitivity of these results to the way in which we imputed the proportion 'special exemption' for the year 1959.

Our estimated annual cost of conscription corresponds well with the 15% reduction in earnings estimated by Angrist (1990) for two years of service by US Vietnam veterans, which is surprising because here we evaluate the cost of service during peace time instead of serving in a war. Comparing this cost with an average return to experience for these cohorts of 4% and average return to education of 5.5%[9], we find the individual cost of military service in the Netherlands to be greater (though not significantly so) than the cost associated with losing 14 months of potential experience or education. This suggests that one year of service also has an indirect or additional effect on an individual's career and education decisions and outcomes. Military service may have a demoralizing effect on subsequent decisions to enrol in college and individuals who graduated from high school or college before entering military service may find their human capital depreciated and their probability of finding a job reduced. In addition, as discussed before, upon leaving school or college there often

[9]These estimates for the return to education and experience are reported in Kalwij (1992).

exists a waiting period of on average 5 to 6 months before one can actually enter the military to start the 14 month service. Taking this into account our estimate of 8% comes close to the foregone earnings gains associated with two years of work experience.

An interesting comparison can be made with the average loss in earnings **during** the year of service for which Duindam reports an estimate of fl. 5,100 ($2,500). This fl. 5,100 is a one time cost, whereas the 3,000 or 8% is a yearly tax incurred over many years. The main part of the cost therefore seems to be in the effect on future earnings, rather than the reduction in earnings during the year of service.

We estimate the returns to military service not by taking mean differences of treatment and control groups, but rather by comparing mean differences for groups that face different risks of being drafted. The effect we estimate is therefore not an estimate of the average causal effect of the entire cohort, but an average for the group in the cohort that is affected by the instrument, i.e. the changing rules of eligibility, as discussed in Imbens and Angrist (1991). The military has set up the selection process in such a way that this group is the one on the margin of being acceptable/not acceptable under normal selection. If one has great faith in the effectiveness of the examinations/screening process the potential recruits are subjected to, this means that the group affected, i.e. the group that would have been designated 'special exemption' in years in which that was a large group, but not in years where that was a small group, was relatively unfit for military service. If this is associated with relatively low earnings and low returns to experience, and if the cost of military service

is indeed caused by foregone experience, this would imply that our estimates of the cost are lower than the average cost for most recruits.

6. Sensitivity Analysis

To study the sensitivity of our findings to alternative earnings function specifications and to the composition of our estimation sample, Table 4 reports the estimates of the coefficient on the 'percentage served' variable and the implied cost in guilders of military service for different model specifications and estimation samples.

First, when average annual earnings are used as dependent variable instead of its logarithm, the estimated decrease in earnings due to the draft is about 3375 Dutch guilders, or 9% of annual average earnings. When no weights are used in the grouped data estimation procedure, the effect of military service in the baseline log-earnings specification is only 5% or 1875 guilders and it is no longer statistically significant at the 95% level. This finding accentuates the need to account for changing group sizes in grouped data estimations. In this case it leads to an underestimation of the actual effect.

Next, the sensitivity to the age dependence structure was tested by including a second, fourth and fifth degree polynomial in age instead of a third degree one. Given the strong significance of the cubic effect in Table 2, it can be expected that omission of this term will lead to biases. Indeed, excluding the third degree term in age gives rise to a zero effect of military service on earnings. The fourth and fifth degree age dependence structure both give slightly lower, but still significant, cost estimates than our baseline estimate. However, the

Table 4: ROBUSTNESS OF ESTIMATES OF THE COST OF MILITARY SERVICE

Specification	Coeff.	(s.e.)	T-value	N	Cost in fl.
baseline	-0.080	0.034	-2.3	132	−2,932
levels	-3.214	1.374	-2.3	132	−3,214
unweighted	-0.050	0.039	-1.3	132	−1,832
2nd Degree Age Dependence	0.007	0.033	0.2	132	257
4th Degree Age Dependence	-0.075	0.035	-2.2	132	−2,748
5th Degree Age Dependence	-0.068	0.035	-1.9	132	−2,492
'59 Excluded	-0.124	0.075	-1.6	120	−4,485
1956–1965	-0.071	0.035	-2.0	120	−2,706
1956–1967	-0.081	0.034	-2.4	144	−2,838
1956–1968	-0.098	0.034	-2.9	156	−3,240
1956–1969	-0.091	0.034	-2.7	168	−2,812
1957–1966	-0.077	0.035	-2.2	120	−2,762
Served minus Active	-0.079	0.034	-2.3	132	−2,895
Cohort Size Included	-0.079	0.037	-2.1	132	−2,895
Women Included	-0.118	0.150	-0.7	132	−4,324

Baseline: Logs, Weighted, 3th degree Age Dependence, '59 included, Years from '56 to '66
Proportion Served as instrument, no extra regressors

Table 5: SENSITIVITY OF INSTRUMENTAL VARIABLE ESTIMATES

	80% in 1959		50% in 1959		1959 excluded	
Var	Coeff.	(s.e.)	Coeff.	(s.e.)	Coeff.	(s.e.)
Intercept	0.464	0.008	0.488	0.024	0.455	0.008
Proportion 'Special Exemption'	-0.565	0.025	-0.802	0.102	-0.485	0.046
R Squared	0.983		0.872		0.925	
IV Estimates	-0.078	0.035	-0.088	0.037	-0.158	0.098

coefficients of the fourth and fifth degree terms are all insignificant.

In the estimations reported above we used the variation in the aggregate military enrolment rates to estimate the individual cost of conscription. It is likely that one of the main sources of variation, the complete exemption of the 1959 birth cohort, will play a large role in the identification of the cost estimate. To test this, we re–estimated our baseline specification after excluding all 1959 observations from our sample. The resulting cost estimate is 12.4% of annual earnings and is significant only at the 90% level. While this finding confirms the importance of the exemption year 1959, it also shows that variation in enrolment rates in other years leads to estimates of the cost of military service that are comparable to those based on data including 1959.

The baseline specificaton was estimated using data from the 1956–1966 period. We did not include later cohorts because a considerable fraction of their members are still completing their education in 1989 and are therefore not included in the calculation of the average annual earnings. In Table 4 estimates of the cost of military service are reported when the 1966 monthly birth cohorts are excluded and when the additional birth cohorts of 1967, 1968 and 1969 are added to the estimation sample. The cost estimates vary from about 7% to 10%. Further, omitting the 1956 cohort has essentially no effect on the cost estimate.

Related to this issue, it could be argued that our variable 'percentage served' is incorrectly measured because of our treatment of those temporarily exempt, but still eligible to be called in 1989 (this group was labelled 'active' in Table 4). These individuals are primarily students

and are therefore not part of the labor force in 1989. A more appropriate measure might therefore be one where the number of birth cohort members that served is divided by the total number of cohort members that are no longer 'active', i.e. no longer eligible for military service, in 1989. The estimated coefficient for this redefined variable is basically identical (0.079) to the one previously found (0.080).

If the proportion of a cohort that is drafted is correlated with birth cohort size, then it is possible that our cost estimate is picking up a cohort size effect on average earnings. As found by Welch (1979) an increase in cohort size can depress average earnings. However, when cohort size is included as regressor in our baseline specification, we find it to have a very small and insignificant effect on earnings, while leaving the cost estimate essentially unchanged.

Finally we estimate the same model including earnings data for women. Since women are exempt from military service we expect that our instruments have no direct effect on their average earnings. It is however possible that there are indirect effects. There could be possible income and substitution effects of lower husband's earnings levels. In the estimation we allow for differences in average earnings by including a dummy variable for gender, and interact the 'percentage served' variable with an indicator for men. This estimation strategy is essentially a 'difference in differences' strategy where changes in earnings for the control group (women in this case) are subtracted from changes in earnings for the treatment group (men) before interpreting the latter as average causal effects. The estimate reported in

Table 4 is the coefficient on the percentage served interacted with an indicator for men. The coefficient for percentage served itself was 0.01. The standard error was very high in this regression, partly reflecting the very different pattern of women's earnings. In fact, for the birth cohorts 1956 to 1966, the average earnings for women were fl. 24,181 in 1988, compared to fl. 36,648 for men from the same cohorts. Another potentially important problem with the earnings data for women is that in the calculation of the average earnings, only women with positive incomes were included. Unfortunately we do not have data on female participation rates for these birth cohorts, making it impossible for us to perform a stronger test of our result.

In the weigthed IV estimations reported in Table 2, we were required to assign a value to the (missing) proportion of the 1959 cohort that had been exempt from military service because of altered exemption criteria (ie. excluding those who would have been categorized as permanently unfit for military service). To test the sensitivity of our cost estimate to the particular value assigned, we re-estimated the model with the proportion exempt set at 50% instead of 80% and also when all 1959 observations were altogether excluded from the sample. In Table 5 we report both the estimates for the first stage regression of the 'proportion served' on the proportion 'special exemption', and the IV estimates of the cost of service from the second stage. As the estimates in Table 5 indicate, the cost estimate differs only slightly, -0.088 compared to -0.078 in the case when the proportion exempt is set at 50%, despite a relatively large change in the effect of the variable 'percentage exempt'

on the variable 'percentage served' as shown in Table 5. When the 1959 birth cohort is left out, the IV estimate is 15.8% as compared to 12.4% when the actual percentage that served was used as instrument for individual military service enrolment with the 1959 data removed.

At the same time, the estimates and the high explanatory power of the instrument in Table 5 highlight the effectiveness of the government's policy of changing each year's criteria for exemption in influencing the proportion of each cohort that will actually serve in the military. Increasing the size of the group 'special exemption' by one percentage point decreases the size of the group that actually serves by about six tenths of a percentage point.

7. Conclusion

In this paper we have analyzed an important aspect of the compulsory military service in The Netherlands: its impact on the future earnings of individuals who had their careers interrupted to serve in the army. Using two sources of data and weighted least squares and IV grouped-data estimation procedures, we find an annual cost of 8% of earnings or fl.. 3000 ($ 1500) in 1989 (on average nine years after completing the service). This estimate is found to be robust to a variety of alternative specifications of the earnings function and the selection of our estimation sample.

A comparison of this cost with the returns to one year of work experience indicates that serving in the military is roughly equivalent to the cost of losing two years of potential work experience. We should point out however that with the available data it was not possible to

confirm this explanation of the earnings cost.

The relevance of our results is limited by the fact that we only measure the earnings cost of military service at one point in an individual's subsequent career. In future work we hope to obtain additional earnings data (by birth month cohort) for later years. This will allow us to estimate the change and possible depreciation in these cost in earnings over time.

In addition to its use in the discussion on abolishing the system of compulsary conscription, our study sheds new light on the usefulness of natural experiments and of grouped–data estimation in evaluation studies and the importance of investigating the sensitivity of such estimates.

References

ANGRIST, J.D., (1990). "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records," *American Economic Review*, June 1990.

—, (1991), "Grouped-Data Estimation and Testing in Simple Labor-Supply Models", *Journal of Econometrics*, 47, 243–266.

CENTRAAL BUREAU VOOR DE STATISTIEK, (1990), "Handboek voor de Statistiek", Voorburg, The Netherlands.

COOK, T. D., AND D. CAMPBELL, (1979), *Quasi-Experimentation, Design and Analysis Issues for Field Settings*, Rand McNally College Publishing Company, Chicago.

DUINDAM, S., (1992), "Defensie, De Kosten van Dienstplicht", *De Economist.*

KALWIJ, (1992), "Returns to Schooling and Educational Attainment in The Netherlands", mimeo, Tilburg University.

IMBENS, G., AND J. ANGRIST, (1991), "Identification and Estimation of Local Average Treatment Effects", NBER Technical Working Paper no 118, December.

WELCH, F., (1979), "Effects of Cohort Size on Earnings: The Baby Boom Babies' Financial Bust", *Journal of Political Economy*, 87(5), s65-s97.