Journal of Business & Economic Statistics, April 1995, Vol. 13, No. 2

Evaluating the Cost of Conscription in The Netherlands

Guido Imbens
Department of Economics, Harvard University, Cambridge, MA 02138, and National Bureau of Economic Research, Cambridge, MA 02138

Wilbert van der Klaauw
Department of Economics, New York University, New York, NY 10003

In this article we investigate the effect of military service in the Netherlands on future earnings. Estimating the cost or benefit of military service is complicated by the complex selection that determines who eventually serves in the military: On the one hand, potential conscripts have to pass medical and psychological examinations before entering the military, and on the other hand numerous (temporary) exemptions exist that can be manipulated by young men to avoid military service. We use substantial, policy-induced variation in aggregate military enrollment rates to deal with these selection issues. We find that approximately 10 years after serving in the military former conscripts have earnings that are on average 5% lower than the earnings of members of their birth cohort who did not serve in the military. These findings are shown to be robust against a variety of specifications.

KEY WORDS: Causal effects; Grouped data estimation; Instrumental variables; Military service; Natural experiments; Selectivity bias.

Since early this century, the armed forces in the Netherlands have gotten most of their recruits through compulsory conscription rather than voluntary enrollment into the military. Recently there has been a discussion about, and subsequently a decision to abolish, this system of compulsory conscription and replace it with a system of voluntary enrollment in the light of the end of the Cold War. In this article we focus on one aspect of the compulsory military service that has received little attention in this debate in the Netherlands—the cost of military service in terms of its effect on future earnings for those who serve.

Estimating the earnings effect of military service for individuals is more complicated than comparing the earnings for veterans and nonveterans because of the 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, possibly leading to an overstatement of the cost, is due to the "breadwinner exemption": Men who are married and who are the main source of income in their family and men who are indispensable 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 for two reasons. First, there is an age limit after which one cannot be called for military service. Second, the longer the military service is postponed the more likely it is one will be exempted based on the breadwinner exemption.

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 and information on military-service records.

Angrist (1990) and Angrist and Krueger (1994) dealt with similar selection problems in estimating the cost of serving in the military for Vietnam-era and World War II veterans in the United States by using the policies that were used to determine priority for enrollment in the military as an instrument. We follow a similar approach, using variation in the aggregate enrollment rates generated by government decisions concerning the number of people needed in different years.

Using grouped data estimation techniques similar to those employed by Angrist (1991), we find that the yearly cost to individuals of serving in the military was approximately 2,000 Dutch guilders (fl.) (in 1989 guilders, which is approximately $1,000 in 1989 U.S. dollars), or 5% of average annual earnings for the relevant cohorts. These estimates are comparable to those of Angrist (1990), who found a cost of approximately 15% for two years of military service during the Vietnam era, and Angrist and Krueger (1994), who found a negative effect of serving in the military during World War II.

For the estimation we use a large data set on 1989 and 1990 earnings from the Central Bureau for Statistics in the Netherlands that contains both the mean and variance of earnings by month-of-birth cohort and data provided by the Dutch Defense Department on the number of potential recruits and number of enlisted men by birth cohort.

1. 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% of the armed forces consist of conscripts, 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 because the reserves that can be called on 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 (women are exempt from military service in the Netherlands) have to serve 14 months in the armed forces, and although over time several exemptions have been written into this law, during the early eighties of all men aged between 22 and 30, typically about 36% of a given birth cohort had actually served in the armed forces.

All men are called for medical and psychological examinations during the year in which they turn 18 years old. On the basis of these examinations, the potential recruits are divided into several 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 200 cm (6 ft., 5 in.). Those who 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 enrollment 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, not all people are called immediately following the end of their exemptions. In fact the average time between exhausting all exemptions and the time of 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 nontrivial percentage (varying from 0% to 8% over the years for which we have data) have not even been called 14 months after first availability. This group is then granted a permanent exemption.

Once called to serve, recruits typically served 14 months during the period for which we have data, although recently this has been lowered to 12 months. About 15% of the recruits are invited, and subsequently accept the invitation, to be trained as officers or noncommissioned 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 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. An exception is the work of Duindam (1992), who estimated the cost due to foregone earnings during the period of active service. He estimated the wage that those who serve would have earned in the civilian market at fl. 5,100 ($2,550) more than the pay they receive in the military. We focus on a different aspect of this tax—namely, the effect on earnings in years following the period of active service. 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. The fact that only about 36% of a given cohort of men serve, however, suggests that the tax is not equally distributed across the male population.

The inequality of this tax burden is primarily caused by the system of exemptions. First, the exemptions granted on the basis of enrollment in higher education and on the basis of low psychological and medical examination scores at age 18 may have led to a redistribution of the tax burden away from the higher educated and the lower educated toward the middle groups. In this respect it is important to note that, although enrollment 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). The lower educated, on the other hand, are overrepresented in the groups that are exempt for failing medical or psychological examinations.

A second set of exemptions exists for married men who are the main source of income in their family, for those who are "indispensable," often in family-run businesses, and for those who reside abroad, outside the European Community. Another category that has increased in importance in recent years is that of conscientious objectors. Although 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 overcome. 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 possibly very different from that of regular military service, where 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 of importance in determining the size of this unequally distributed tax and as an argument in the debate on the merits of a conscription-based versus a volunteer army. Here we focus on one aspect of this tax—namely, the effect of military service on earnings in the years following the period of active service.

2. IDENTIFYING THE COST OF CONSCRIPTION

Estimation of the monetary 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 in the Netherlands containing information on both earnings and military-service histories. Although there are several 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 fulfilled his military-service obligations. 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 is 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 several lawyers specializing in military conscription, with clients willing to spend up to 5,000 Dutch guilders ($2,500) 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—that is, 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 nonignorability of the treatment assignment (Rubin 1978), or the selection or endogeneity problem (Heckman and Robb 1985), by focusing on changes at the aggregate level of the treatment (i.e., military service) over time. Although there is considerable evidence that the individual cohort member's enlistment decision is endogenous, we assume that the average percentage of conscripts in a birth cohort is not, or at least is less, affected by the arguments advanced earlier for the endogeneity of the military-service indicator. The idea of aggregating to a level at which 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 conscription rate is not subject to the selection problems discussed previously, there would be little point in trying to estimate the returns to military service by focusing on changes at the aggregate level. During the seventies and eighties, however, there was considerable variation in the aggregate conscription rate by birth cohort due to government policies that led to the type of exogenous variation, or natural experiment, 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 19 to 18. This did not have much effect on the average age at which people actually fulfill their military service obligations because many potential recruits have at least temporary exemptions at the time of their medical examinations when most of them are still in school. A more dramatic outcome of this first policy change, however, was that a complete birth cohort, all men born in 1959, was completely exempted 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. Rather than strengthen the requirements for passing the medical and psychological examinations on a permanent basis, it was decided to create a new category, "special exemption" ("buitengewoon dienstplichtig"), on a yearly basis. Every year several of the categories that were considered "fit to serve" according to the normal guidelines were given this designation, with the explicit idea of limiting the number of recruits who would actually be called. Men 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 who would fall in each of the old categories into which all potential recruits were divided on the basis of medical and psychological examination results. Given these predictions, some of the old categories would then be designated "special exemption." Faced with a relatively constant total demand for recruits, the variation in demand for newly examined recruits was affected mainly by the randomness of the supply of recruits from older cohorts fulfilling their military-service obligations each year.

The natural instrument is therefore a vector of dummies, with the ith element indicating whether the ith category of potential recruits was "special exemption" or not (i.e., fit to serve or unfit under any circumstances) in a particular year. Specifically, for individual j—born, for example, in 1960—the vector of instruments would indicate for each category whether that category was, for birth cohort 1960, designated as "special exemption" or not. 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. We cannot directly implement this strategy because we do not have individual-level data, nor do we know all categories and their designation. An alternative strategy is to think of the government as setting a target number of recruits each year. They attempt to achieve this target by designating particular categories as "special exemption," leading to the instrumental variables (IV) strategy outlined previously. One could directly use the target number of recruits as an instrument. A problem with this strategy is that we do not know the exact target number, but as a close approximation we use the actual number or percentage of conscripts in a cohort as an instrument.

Suppose that the proportion of people medically unfit to serve in the military varies considerably over the birth cohorts. If this condition also affects earnings, then the percentage served would not be a valid instrument for veteran status because there would be a second link to earnings via health status. It implies that, although the government may set a target number of recruits that is a valid instrument, the actual number is affected by the composition of the cohort and therefore is not a valid instrument. To address this possibility we also use an alternative instrument—namely, the percentage of the cohort that was designated "special exemption." Because this category was defined each year before medical examinations took place, changes in the proportion of medically unfit individuals need not affect the number of "special exemptions." In that case the proportion "special exemption" would be a valid instrument, but proportion served would not.

We use this partly as a means of assessing the sensitivity of the estimates based on proportion served as an instrument to some form of misspecification.

An alternative interpretation of the resulting estimation strategy is to think of the cohort dummies as instruments for the military-service indicator. Such an interpretation would underline the fact that identification stems from comparisons of different birth cohorts rather than from comparisons at the individual level. We prefer the interpretation outlined previously because the choice of instrument in that interpretation reflects directly the manipulations that create the instrument. This agrees with the Angrist, Imbens, and Rubin (1993) view that instruments should themselves be potential causes or manipulable variables (such as designation of categories or target numbers) rather than fixed attributes (such as birth years).

In both IV approaches, we identify the effect of military service on earnings from the variation over time in the proportion of people who actually serve rather than directly compare within a given cohort the men who served with the men who did not serve. A problem with these approaches is that the period during which this exogenous variation occurred is so recent that the people affected are, during the years for which we have earnings data, on a relatively steep part of their lifetime earnings profile. We therefore have to take special care in estimating the age profile of earnings.

3. INSTRUMENTAL VARIABLES ESTIMATION

The log earnings regression that we wish to estimate is

$$\ln Y_i = \beta_0 + \beta_1 \cdot S_i + \beta_2 \cdot A_i + \beta_3 \cdot A_i^2 + \beta_4 \cdot A_i^3 + \beta_5 \cdot D_i + \varepsilon_i, \quad (1)$$

where Y_i are the earnings for individual i, A_i is his age, S_i is a dummy variable equal to 1 if person i served in the military and 0 otherwise, and D_i is a dummy variable equal to 1 if the earnings observation is for 1990 and 0 if the earnings observation is for 1989. For reasons discussed in the previous sections, we do not wish to make the assumption that the unexplained part of log earnings, ε_i, is independent of veteran status S_i. We therefore employ an IV strategy. As discussed previously, we use either the proportion of conscripts in a month-of-birth cohort or the proportion of special exemptions as an instrument for veteran status.

As noted before, we do not have the individual-level data to estimate regression function (1) by ordinary least squares or IV. But it turns out that we do not need individual-level data to get IV estimates of the coefficients in (1) based on the percentage served, $\overline{S}_i$, as an instrument. To see this, consider the first-stage regression of the endogenous regressor, the military-service indicator S_i, on a constant, the instrument $\overline{S}_i$, and the other exgenous regressors in regression function (1), A_i, A_i^2, A_i^3, and D_i:

$$S_i = \pi_0 + \pi_1 \cdot \overline{S}_i + \pi_2 \cdot A_i + \pi_3 \cdot A_i^2 + \pi_4 \cdot A_i^3 + \pi_5 \cdot D_i + \eta_i. \quad (2)$$

The conditional expectation of the service indicator, S_i, given the cohort indicator and all the other regressor variables in (2), is equal to the proportion of the cohort that actually served, $\overline{S}_i$, because all regressors are constant within a cohort. We therefore know the coefficients in the first-stage regression without sampling error—$\pi_0 = 0, \pi_1 = 1, \pi_2 = \pi_3 = \pi_4 = \pi_5 = 0$—because we know the population values of the covariances of the regressors with each other and with the dependent variable. This is an example of a special case that could occur when combining individual-level data with aggregate data, where the aggregate data are sufficient to pin down the coefficients on (part of) the microlevel regression function (see Imbens and Lancaster 1994). It follows that the fitted value $\widehat{S}_i$ equals $\pi_1\overline{S}_i = \overline{S}_i$. This in turn suggests a second-stage regression,

$$\ln Y_i = \beta_0 + \beta_1 \cdot \overline{S}_i + \beta_2 \cdot A_i + \beta_3 \cdot A_i^2 + \beta_4 \cdot A_i^3 + \beta_5 \cdot D_i + \omega_i, \quad (3)$$

where the compound disturbance term $\omega_i = \varepsilon_i + \beta_1 \cdot (S_i - \overline{S}_i)$ is uncorrelated with all the regressors in (3). We still cannot estimate (3) directly because of the lack of individual-level data. But, because none of the regressors ($\overline{S}_i, A_i, A_i^2, A_i^3$, and D_i) in the regression function vary within a birth cohort, this implies that we can, as an alternative to estimating (3) on individual data, estimate it on monthly birth-cohort averages:

$$\overline{\ln Y_t} = \beta_0 + \beta_1 \cdot \overline{S}_t + \beta_2 \cdot A_t + \beta_3 \cdot A_t^2 + \beta_4 \cdot A_t^3 + \beta_5 \cdot D_t + \nu_t, \quad (4)$$

where all variables are indexed by t running from January 1956 to December 1966. Weighted least squares estimation of (4), with the weights equal to 1 over the square root of the residual variances of ν_t, is as efficient as IV estimation of Equation (2) on individual-level data. If the individual-level earnings variance is constant and the cohorts are of equal size, the weighting is irrelevant and ordinary least squares estimation of (4) is also efficient. We do not actually have the variance of ν_t for each month. Rather, we have the variance of $\overline{\ln Y_t}$ for each month. Because the covariates are all constant within months, the variance of $\overline{\ln Y_t}$ would equal the variance of ν_t if the effect of military service is 0—that is, if $\beta_1 = 0$. As long as the explanatory power of military service is small in this regression, the approximation of the variance of ν_t by that of $\overline{\ln Y_t}$ will be adequate.

In a second approach we use the percentage "special exemption" as an instrument. This may avoid problems if the percentage served is not a valid instrument because there are direct cohort effects. For example, the number of people medically unfit to serve may vary over the years, which, if that had direct effects on earnings, would invalidate the use of percentage served as an instrument but not the use of the percentage of "special exemption." Ideally we would estimate regression function (1) using the percentage "special exemption" as an instrument for the military-service indicator S_i. Because we do not have individual-level data, we average the observations by monthly birth cohort. The averaged error, ν_t in the regression function given in (4), is independent of the instrument if the individual-level error, ε in (1), is, and therefore we estimate Equation (4) using the proportion "special exemption" as an instrument for percentage served.

Table 1. Summary Statistics

Birth year	% served	% special exemption	% active	Cohort size in thousands	Average earnings (in fl., 1989)	Average earnings (in fl., 1990)
1956	44.2	—	.0	122	45,752	54,364
1957	34.1	—	.0	120	44,602	51,512
1958	41.3	4.6	.7	122	43,927	50,700
1959	.0	—	.0	121	42,100	49,381
1960	45.8	2.3	.5	123	39,355	46,618
1961	36.9	16.8	.3	127	38,418	45,591
1962	32.3	23.3	.2	127	37,137	44,176
1963	45.3	3.2	.5	131	34,470	41,897
1964	44.8	3.3	.6	132	32,458	39,034
1965	33.3	27.4	.7	126	29,127	35,961
1966	33.4	29.9	1.4	125	25,616	33,394
1967	36.2	21.5	3.2	130	21,519	29,230
1968	38.8	8.4	6.5	126	16,310	23,397
1969	38.7	3.1	10.4	131	12,749	19,117
Average	36.2	—	1.8	126	36,648	44,079

4. THE DATA

We use two sources of data. First, we obtained from the Dutch Defense Department the proportion of each birth cohort that actually served as conscripts in the military, the proportion that was designated "special exemption," the percentage that has not yet fulfilled their obligations toward military service in 1989, which we will denote as the percentage "active," and the size of each birth cohort. The group "active" is a category that is important for the recent cohorts and consists mainly of people still enrolled in education. Although there are professional soldiers in the armed forces in the Netherlands, their number in any given cohort is very small. Because they receive pay comparable to wages in the civilian labor market and serve for extended periods of time, we do not consider them in this analysis.

The second data set consists of the mean and variance of earnings in 1989 and 1990 by month of birth calculated from the "Inkomen Panel Onderzoek," a data set collected by the Central Bureau for Statistics in the Netherlands. The original data form a repeated cross-section: There are no individuals whose earnings are recorded in both years. 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 top coded, which is part of the reason that we were not given access to the individual-level data.

In Table 1 we give yearly averages for some of the key variables. Note that the category "special exemption" was introduced with the birth year 1958 and is not defined for the birth year 1959 because that entire birth cohort was exempted.

A simple estimate of the returns to, or cost of, military service can be obtained by comparing the average earnings in 1989 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. $(41{,}641-42{,}100)/.436 = -1{,}043$ (approximately $-\$500$) as a rough estimate. The same calculation using the 1990 earnings gives fl. $[\frac{1}{2}(50{,}700+46{,}618) - 49{,}381]/.436 = -1{,}641]$. The validity of this estimate rests on the adequacy of a linear approximation to the age profile over the three-year period used in this calculation. Implicitly this estimate assumes that β_3 and β_4 in (4) equal 0. A similar approach was used by Angrist and Krueger (1994), who compared the middle two birth quarters against the outer two.

Our estimates in Section 5 are aimed at obtaining more accurate estimates by using several refinements— (a) by combining the two years of earnings data (1989 and 1990), (b) by using the variances of the earnings data, (c) by using the variation in the percentage served in other years besides 1958–1960, and (d) by using the percentage "special exemption" as an instrument for the percentage served. In addition to gaining precision and providing evidence on the sensitivity of the preceding estimates, these refinements allow us to free up the linear age profile implicit in the preceding calculation.

5. ESTIMATION RESULTS

To evaluate the effect of military service on earnings, we estimated the log–earnings Equation (4) in which the logarithm of average earnings for each monthly cohort is related to the percentage of cohort members who served in the army, a cubic function in age, to capture the general age–earnings profile and a dummy indicator for the 1990 earnings observations to capture inflation and other differences in general economic conditions between 1989 and 1990. The estimation sample includes the average 1989 and 1990 earnings of all monthly birth cohorts in the period of January 1956 to December 1966. We choose to use the data from only these birth cohorts mainly because of their increasing proportion of members in the "active" category. 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 lognormal

Table 2. Estimates of the Cost of Serving in the Military

Parameter instrument weights	Percentage served (1) Yes	(2) No	Percentage special exemption (3) Yes	(4) No
Pct served	−.054	−.041	−.050	−.039
	(.024)	(.027)	(.024)	(.026)
Age	.470	.480	.443	.443
	(.020)	(.022)	(.026)	(.029)
Age squared	−.634	−.610	−.592	−.566
	(.042)	(.047)	(.047)	(.050)
Age cubed	.715	.667	1.001	1.015
	(.106)	(.115)	(.189)	(.203)
1990	.124	.123	.122	.122
	(.005)	(.006)	(.006)	(.007)
$\chi^2(6)$	21.8		33.7	

distribution for earnings. In that case, the mean of the log of earnings is $\mu_{\ln Y} = 2\ln\mu_Y - (\frac{1}{2})\ln(\mu_Y^2+\sigma_Y^2)$, and the variance of the log of earnings is $\sigma_{\ln Y}^2 = \ln(\mu_Y^2+\sigma_Y^2) - 2\ln\mu_Y$.

In Table 2 we present the basic results. Columns (1) and (2) give the results based on using percentage served as an instrument for veteran status. The regressions are performed using the monthly birth cohorts as units. The first column gives the weighted least squares results, the second column the unweighted least squares results. The former are asymptotically equivalent to those obtained when using percentage served as an instrument for veteran status in a regression at the individual level, assuming that our monthly log earnings variances are good approximations of the corresponding error variances. The last row in this table tests the model by comparing the difference between the (efficient) weighted least squares estimates and unweighted ordinary least squares estimates in a Hausman test (Hausman 1978). If the weights are correctly specified, the test statistic should have a chi-squared distribution with 6 df.

Next, as an alternative to the actual percentage of a cohort that served in the armed forces, we use the proportion of a cohort that was exempt from military service and was designated "special exemption" as an instrument for individual military-service completion, using the same log–earnings equation based on monthly birth-cohort averages. Because data on this instrument were missing for the first two years in our data, 1956 and 1957, only monthly cohort data for the 1958 to 1966 period were used. In 1959 the entire cohort was exempt. One can interpret this as designating all categories that were considered "fit to serve" in normal years as "special exemption" in that year. Because this cohort was not subject to the standard medical examinations, however, we do not know the percentage "medically unfit," and therefore we cannot calculate the percentage "special exemption" for that year. We therefore estimate, regressing the percentage served on the percentage "special exemption" using the data from the other years, what the percentage "special exemption" would have to be for the percentage served to be equal to 0. This equals approximately 100% minus the average over the other years of the percentage of the cohorts who are medically unfit to serve. We estimate the latter to be about 20% and therefore impute the percentage "special exemption" for 1959 to be about 80%. Later we investigate the sensitivity of the results to this assumption.

Columns (3) and (4) give the results based on using the percentage "special exemption" as an instrument. Column (3) gives the weighted results, column (4) the unweighted results. Using either instrument, we obtain a statistically significant parameter estimate of −.05, which implies that military service leads on average to a 5% reduction in earnings. Given that average earnings in 1989/1990 for the 1958 to 1966 birth cohorts equal fl. 40,000 (or about $20,000), this implies an annual cost of approximately fl. 2,000 or $1,000. The unweighted results, using either instrument, suggest a slightly smaller cost of about 4% of earnings. The test statistic rejects the equality of the weighted and unweighted estimates, which is one of the motivations for the sensitivity analysis in Section 6.

Comparing this cost with an average return to experience for these cohorts of 4% and average return to education of 5.5% (Kalwij 1992), we find the individual cost of military service in the Netherlands to be similar to the cost associated with losing approximately one year of potential experience (or education). Fourteen months of service may also have an indirect or additional effect on an individual's career and education decisions and outcomes, however. Military service may have a demoralizing effect on subsequent decisions to enroll 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, on leaving school or college there often exists a waiting period of on average 5 to 6 months before one can actually enter the military to start the 14-month service. Our findings suggest that the hypothesis of additional costs is either not supported by the data or that the experience of serving in the military has a nonzero return.

An interesting comparison can be made with the average loss in earnings during the year of service for which Duindam (1992) reported an estimate of fl. 5,100 ($2,550). This fl. 5,100 is a one-time cost, whereas the fl. 2,000 or 5% is a yearly tax potentially 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 (conscripts) and control (nonconscripts) 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—that is, the changing rules of eligibility, as discussed by Imbens and Angrist (1994) and Angrist et al. (1993). The military has set up the selection process in such a way that this group is the one on the margin of being aceptable/not acceptable under normal selection. If one has great faith in the effectiveness of the examinations/screening process that the potential recruits are subjected to, this means that the group affected—that is, the group that would have been designated "special exemption" in years in which that was a large group but not in years in which 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 might be lower than the average cost for most recruits.

6. SENSITIVITY ANALYSIS

We do not view the particular specification of the regression equation in Section 5 as the only possible or best one. In particular we are not certain that the cubic experience profile is adequately capturing the true experience profile. In addition there might be other characteristics of individuals that should be controlled for. In this section we therefore estimate several other specifications to explore the sensitivity of the estimate of the cost of military service to different specifications. These regressions should be viewed in the spirit of the argument advanced by Pratt and Schlaifer (1988) that in establishing a causal effect one should include in the estimated equation, or in one of the estimated equations, "every optional concomitant variable that might reasonably be suspected of either affecting or merely predicting [earnings] given [percentage served]. ... The relevant test statistic for a law as opposed to a regression is not R^2 or F but the vector of changes in the estimated effect of [percentage served] on [earnings] that result when test concomitants are included in the relation" (p. 44).

Table 3 therefore 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. For each specification, we report the estimate of the coefficient of interest, its standard error, and its implied cost in guilders, evaluated at the average earnings of fl. 40,000.

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 1,924 Dutch guilders, or 5% 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 about 4% or fl. 1,600.

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 structures both give cost estimates very similar to those of our baseline estimate. The coefficients of the fourth- and fifth-degree terms are all insignificant, however.

In the estimations just reported, we used the variation in the aggregate military enrollment rates to estimate the individual

Table 3. Sensitivity Analysis of Estimates of the Cost of Military Service

Specification	*Instrument*	*Coeff.*	*s.e.*	*N*	*Effect in fl.*
Baseline	% served	−.054	(.024)	264	−2,160
Unweighted	% served	−.041	(.027)	264	−1,640
Levels	% served	−1.924	(1.083)	264	−1,924
2nd-degree age dependence	% served	.014	(.024)	264	560
4th-degree age dependence	% served	−.053	(.024)	264	−2,120
5th-degree age dependence	% served	−.047	(.025)	264	−1,880
1959 excluded	% served	−.052	(.056)	240	−2,080
1956–1965	% served	−.050	(.024)	240	−2,000
1956–1967	% served	−.056	(.024)	288	−2,240
1956–1968	% served	−.073	(.024)	312	−2,920
1956–1969	% served	−.074	(.024)	336	−2,960
1957–1966	% served	−.050	(.025)	240	−2,000
Served minus active	% served	−.053	(.024)	264	−2,120
Cohort size included	% served	−.066	(.028)	264	−2,640
Cohort education included	% served	−.054	(.025)	264	−2,160
Variance of log earnings included	% served	−.054	(.024)	264	−2,160
Unempl. rate at age 17	% served	−.054	(.024)	264	−2,160
Interaction age and percentage served	% served	−.034	(.042)	264	−1,360
Women included	% served	−.108	(.032)	528	−4,320
1989 earnings only	% served	−.080	(.035)	132	−3,200
1990 earnings only	% served	−.044	(.035)	132	−1,760
Baseline	spec. exempt.	−.050	(.024)	216	−2,000
Unweighted	spec. exempt.	−.039	(.026)	216	−1,560
Percentage exempt in 1959 at 50%	spec. exempt.	−.051	(.026)	216	−2,040
1959 excluded	spec. exempt	−.060	(.072)	192	−2,400
1989 earnings only	spec. exempt	−.078	(.035)	108	−3,120
1990 earnings only	spec. exempt	−.042	(.035)	108	−1,960

NOTE: The baseline for regression with percentage served as instrument is logs, weighted, 3rd-degree age dependence, 1959 included, years from 1956 to 1966. The baseline for regression with percentage special exemption as instrument is logs, weighted, 3rd-degree age dependence, percentage exempt in 1959 at 80%, years from 1956 to 1966.

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 reestimated our baseline specification after excluding all (12) 1959 observations from our sample. The resulting cost estimate is 5.2% of annual earnings, but it is not significantly different from 0. Although this finding confirms the importance of the exemption year 1959, it also shows that variation in military enrollment rates in other years leads to estimates of the cost of military service that are very similar to those based on data including 1959 and therefore our results are not entirely driven by the one birth-year cohort that was completely exempt.

The baseline specification 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 or 1990 and are therefore not included in the calculation of the average annual earnings. In Table 3, estimates of the cost of military service are reported when the 1966 monthly birth cohorts are excluded and also when the additional birth cohorts of 1967, 1968, and 1969 are added to the estimation sample. The cost estimates vary from about 5% to 7.5%. Furthermore, omitting the first year for which we have data, 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 1990 (this group was labeled "active" in Table 1). These individuals are primarily students and are therefore not part of the labor force in 1989. A more appropriate measure might therefore be one in which the number of conscripts in a birth cohort is divided by the total number of cohort members who are no longer "active"—that is, no longer eligible for military service—in 1989. The estimated coefficient for this redefined variable is identical ($-.053$) to the one previously found.

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. When monthly cohort size is included as regressor in our baseline specification, however, we find it to have a very small and insignificant effect on earnings, while increasing the cost estimate slightly.

In an additional attempt to investigate the presence of cohort effects that would invalidate the cubic experience profile that we use to capture cohort differences, we add the average education level of the cohort reported by the Centraal Bureau voor de Statistiek (1990) to the regression. Again this does not make much difference to the estimate of the cost of military service, suggesting that the earnings cost of military service is probably not caused by its effect on schooling choices.

In addition we investigated regressions with the level of youth unemployment at the time a cohort was first called for service as a control variable and with the variance of log earnings as a control variable. Neither led to significant changes in the coefficient of interest.

We also included an interaction between the percentage served and age. Such a variable could be expected to capture any changes in the effect of military service over the individual's lifetime. One might expect that, because the military service experience has been followed by a longer period of civilian labor-market experience, the decrease in earnings would diminish. Including this interaction made some difference for the coefficient on percentage served itself, but the standard error was relatively large. The coefficient on the interaction term, estimated at $-.078$, however, did not have the hypothesized sign. This may be due to sampling variation given the associated standard error of .136.

We also estimate the same model including earnings data for women. Because 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. For married women, for example, 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 in which 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 of $-.108$ reported in Table 3 is the coefficient on the percentage served interacted with an indicator for men and implies a cost of about fl. 4,300. The coefficient for percentage served itself was .016, suggesting that possible indirect effects for women are small. This use of women as a control group should be interpreted with caution, however, given the very different pattern of women's earnings. In fact, for the birth cohorts 1956 to 1966, the average earnings for women were fl. 24,000 in 1989/1990, compared to fl. 40,000 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.

The next set of results uses the two years of earnings data separately. In both cases we find a negative effect of military service on earnings, 8% for the 1989 data and 4% for the 1990 data. This can be interpreted, with some caution because of the standard errors, as an indication that these effects decrease over time with a longer period of subsequent civilian labor-market experience.

In the weighted 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 (i.e., 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 reestimated the model with the proportion exempt set at 50% instead of 80% and also when all 1959 observations were altogether excluded from the sample. As the estimates in Table 3 indicate, the cost estimate differs only slightly, −.050 compared to −.051. When the 1959 birth cohort is completely left out, the IV estimate is −.060 when the percentage "special exemption" is used as an instrument, compared to −.052 when the actual percentage that served was used as instrument for individual military-service enrollment with the 1959 data removed.

7. CONCLUSION

In this article we have analyzed an important aspect of compulsory military service in the Netherlands— its impact on the future earnings of individuals who had their careers interrupted to serve in the armed forces. Using two sources of data and weighted least squares and IV grouped-data estimation procedures, we find an annual cost of 5% of earnings or fl. 2,000 ($1,000) in 1989/1990 (on average about 10 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 a year of potential work experience. It is comparable to the 15% cost found by Angrist (1990) of serving in the military for two years during the Vietnam era.

In addition to its use in the discussion on abolishing the system of compulsory 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.

ACKNOWLEDGMENTS

We are grateful for comments and suggestions by Josh Angrist, Chris Flinn, Robert Moffit, and two anonymous referees and for research assistance by Adriaan Kalwij. We also 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 and for comments on an earlier version of this article and the Tinbergen Instituut in Amsterdam for their hospitality during the preparation of the current version. Van der Klaauw thanks the C.V. Starr Center of Applied Economics at New York University for research support, and Imbens acknowledges support from the National Science Foundation. We alone are responsible for any errors.

[Received March 1993. Revised October 1994.]

REFERENCES

Angrist, J. D. (1990), "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence From Social Security Administrative Records," *American Economic Review*, 80, 313–335.

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

Angrist, J., Imbens, G., and Rubin, D. (1993), "Identification of Causal Effects Using Instrumental Variables," Technical Working Paper 136, National Bureau of Economic Research, Cambridge, MA.

Angrist, J. D., and Krueger, A. (1994), "Why Do World War II Veterans Earn More Than Nonveterans?" *Journal of Labor Economics*, 12, 74–97.

Centraal Bureau voor de Statistiek (1990), "Handboek voor de Statistiek," Voorburg, The Netherlands: Author.

Cook. T. D., and Campbell, D. (1979), *Quasi-Experimentation, Design and Analysis Issues for Field Settings*, Chicago: Rand McNally.

Duindam, S. (1992), "Defensie, De Kosten van Dienstplicht," *De Economist*, 29, 427–429.

Hausman, J. (1978), "Specification Tests in Econometrics," *Econometrica*, 46, 1251–1272.

Heckman, J., and Robb, D. (1985), "Alternative Methods for Evaluating the Impact of Interventions," in *Longitudinal Analysis of Labor Market Data*, eds. J. Heckman and B. Singer, Cambridge, U.K.: Cambridge University Press, pp. 156–245.

Imbens, G., and Angrist, J. (1994), "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–476.

Imbens, G., and Lancaster, T. (1994), "Combining Micro and Macro Data in Microeconometric Models," *Review of Economic Studies*, 61, 655–680.

Kalwij, A. (1992), "Returns to Schooling and Educational Attainment in the Netherlands," mimeo, Tilburg University, Dept. of Economics.

Pratt, J., and Schlaifer, R. (1988), "On the Interpretation and Observation of Laws," *Journal of Econometrics*, 39, 23–52.

Rubin, D. (1978), "Bayesian Inference for Causal Effects: The Role of Randomization," *The Annals of Statistics*, 6, 34–58.

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