Evaluating the Labor Market Effects of Compulsory Military Service: A Regression-Discontinuity Approach

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Abstract

Within the current discussion on replacing the system of compulsory conscription by a system of voluntary enrolment in the army, proponents and opponents of this change in paradigm tend to base their arguments on a cost-benefit analysis. Surprisingly, these cost-benefit calculations typically ignore the costs and benefits of military service for the conscripts. This paper seeks to improve our knowledge on these costs (benefits) by analyzing the potential effects of compulsory military service on subsequent labor market outcomes. Based on a large administrative data set – the IAB employment sample (IAB_S) merged with a supplementary file that allows for tracing back men's labor market history – we use a regression-discontinuity (RD) approach to obtain unbiased estimates of the wage effects of compulsory military service. The RD design is based on a discontinuity for subsequent German birth cohorts in the probability to be drafted for compulsory military service arising at the time when the new armed forces have been created in the second half of the fifties. In particular, identification of the causal effect of military service on wages is obtained by comparing men born before July 1, 1937 (the so-called *White Cohort*) with men who were born on or shortly after this threshold date.

Keywords: Regression-discontinuity approach; Causal effects; Quasi-natural experiments; Military service

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I. Introduction

Based on the Paris Treaties of October 1954, West Germany became member of the NATO in May 1955, resulting in the creation of new armed forces, the *Bundeswehr*, in 1956. At that time the West German parliament decided that these armed forces should primarily comprise recruits in their compulsory military service. Until the 1990s, the West German Bundeswehr consisted of about 495,000 soldiers of which around 50% were conscripts. With the end of the Cold War the size of West German army has been reduced to about 340,000 soldiers. An increasing involvement of the German armed forces in UN missions of the United Nations in recent years as well as the desire to reduce the defense budget have generated the necessity to reconsider the organizational structure of the Bundeswehr. In particular, many observers request that the system of compulsory conscription should be replaced by a system of voluntary enrolment.

Proponents and opponents of this change in paradigm tend to base their arguments on a cost-benefit analysis. Surprisingly, the various cost-benefit calculations typically ignore the costs and benefits of military service for the conscripts. Yet, two important types of potential costs and benefits arise to the conscripts, (i) opportunity costs in terms of foregone earnings during military service[1] and (ii) potential effects of compulsory military service on subsequent labor market outcomes. This paper emphasizes the latter, indirect implications of the German draft army. Specifically, we employ a regression-discontinuity design to provide evidence on the effects of compulsory military service on subsequent labor market outcomes. In contrast to the US, these costs and benefits, albeit potentially important, have so far largely been neglected in the debate on the organization of the German armed forces.

Identifying the effects of military service on subsequent labor market outcomes is a difficult task. First, many of the individual level data accessible to researchers do not contain the necessary information about military service. Second, the limited data material comprising this information do typically not allow the identification of the *causal* effects of military service on labor market outcomes. Specifically, because of potential selection and omitted variable biases, a simple comparison of the average labor market outcomes of conscripts and those who did not serve would

[1] For Germany estimated opportunity costs are provided by Schleicher (1996) and Schäfer (2000). In the US, the direct costs of military service for those who served received increasing attention already in the 1960s. See, among others, Bailey and Cargill (1969), Davis and Palomba (1968), Fisher (1969), Hansen and Weisbrod (1967), Oi (1967), and Spencer and Woroniak (1969).

not be reliable. Whether someone is drafted for compulsory military service is partially determined by factors which are directly related to labor market outcomes, such as, for example, health status. Access to information on characteristics which determine draft status would enable the researcher to identify the causal effects of military service by correcting for selection bias. These variables, however, are usually not observable to the econometrician.

In reaction to such deficiencies, previous studies on the individual costs and benefits of military service relied on instrumental variable (IV) estimates to identify the effects of military service on earnings (Angrist 1990; Angrist and Krueger 1994; Imbens and van der Klaauw 1995). These studies exclusively find negative effects of military service on subsequent earnings. For instance, using year of birth as an instrument for the probability to be drafted for the Vietnam war, Angrist (1990) estimates that 10 years after the war white veterans realize approximately 15 percent lower annual wages than non-veterans. These wage differentials between nonwhite veterans and non-veterans are, however, not statistically significant.

Based on instrumental variable estimates that utilize the relationship between date of birth and veteran status, Angrist and Krueger (1994) find that World War II veterans, if anything, earn less than non-veterans. Imbens and van der Klaauw (1995) estimate that 10 years after compulsory military service in the Netherlands, those who did serve earn on average 5% less than those who did not serve. Similar to Angrist (1990) and Angrist and Krueger (1994), Imbens and van der Klaauw (1995) use variations of the probability to be drafted for compulsory military service across birth cohorts to instrument for military service.

In our analysis, we use a regression-discontinuity (RD) approach to obtain unbiased estimates of the effects of compulsory military service on subsequent labor market outcomes. This approach, which has originally been introduced by Thistlethwaite and Campbell (1960), exploits discontinuities in the probability of receiving treatment to identify treatment effects. The RD design has recently been used, for example, by van der Klaauw (1997) to investigate the effects of financial aid on college enrollment, by Black (1999) to evaluate the effect of elementary school quality on housing prices, and by Angrist and Lavy (1999) to identify the effect of class size on schooling attainment. Furthermore, Hahn, Todd, and van der Klaauw (1999) analyze the effect of an anti-discrimination law on minority employment in small U.S. firms, DiNardo and Lee (2002) estimate the effect of unionization on establishment closure, and Lee (2001) analyzes whether political incumbency provides an advantage in elections to public office.

The RD design used here is based on a discontinuity in the probability of subsequent German birth cohorts to be drafted for compulsory military service. When creating the new armed forces in the second half of the fifties, all men born before July 1, 1937, were exempted from compulsory military service. Yet, men born on July 1, 1937, and after faced a realistic chance to be drafted into the Bundeswehr. Based on this rule, we use observations on the so-called *White Cohort*, i.e. men who neither served during World War II nor were required to perform compulsory military service, to identify the labor market effects of military service in Germany. We argue that a comparison of men who were born close to but before July 1, 1937, with men who were born at or shortly after this threshold date enables us to identify the causal effect of compulsory military service on subsequent labor market outcomes.

In this paper we use a large administrative data set, the *IAB employment sample (IAB_S)* that is merged with a supplementary file that allows for tracing back men's labor market history in the past. Different to other data sets in Germany, this data set allows to identify persons served in the Bundeswehr. In addition we have exact information on the day of birth which allows us to separate persons in the *White Cohort* from persons who faced a positive probability of being drafted for military service. Finally, the data set is large enough to have sufficient observations around the threshold day of July 1, 1937 to apply the RD approach.

The paper is organized as follows. Section 2 presents a short description of the history and the organization of compulsory military service in Germany. Section 3 provides a short theoretical discussion of the potential effects of conscription on subsequent labor market outcomes. A formal description of the RD-approach is offered in Section 4. The data used in the empirical analysis and a discussion of the validity of the RD design in our context are discussed in this section also. Section 5 presents the estimation results and derives their economic implications. Section 6 concludes with a reflection of the current political debate.

II. Compulsory Military Service in Germany

As a consequence of the Paris Treaties of October 1954, West Germany entered the NATO in May 1955. This membership implied the creation of new armed forces, the Bundeswehr, with a projected maximum strength of some 500,000 soldiers. In

January 1956, the first 1,000 volunteers entered these new armed forces. The West German parliament decided at this time, however, that the Bundeswehr should comprise to a large part recruits performing their compulsory service rather than volunteers. The corresponding law regulating the introduction of compulsory military service passed the parliament in July 1956. According to this law, all able-bodied men born after June 30, 1937, have to serve in the armed forces.[2] This birthday threshold plays the central role in our empirical strategy.

In particular, we use the so-called *White Cohort*, i.e. men born before the July 1, 1937, who neither served during World War II nor were required to serve in the Bundeswehr, to construct a comparison group. This construction allows us to obtain consistent estimates of the effects of compulsory military service on subsequent labor market outcomes. The first 10,000 conscripts entered the Bundeswehr in May 1957. They had to serve in the armed forces for 12 months. In 1962, the duration of compulsory military service was extended to 18 months and has changed several times since then. For conscientious objectors the German constitution as well as the law on compulsory military service provides the opportunity to perform an alternative type of service. This alternative service mainly comprises auxiliary activities in the health care sector. In the birth cohorts relevant for the empirical analysis, however, the fraction of conscientious objectors is negligible.[3]

The regulation of the selection of conscripts in Germany are similar to the procedures in other countries with compulsory military service.[4] All men of a particular birth cohort are called for medical and psychological examinations around their 18^{th} birthday. Based on these examinations, they are divided into three groups: those with a good health condition are categorized as being fit for service; men with smaller health problems are temporarily deferred from service; and men with severe health problems are immediately exempted from military service. Even if the medical and psychological examinations state that a person is fit to serve, there are various other factors that can lead to an exemption from military service. First, volunteers to the Police, the Federal Border Guard, Peace Corps workers, and the disaster control as well as priests are exempted. Furthermore, those who have been sentenced to prison

[2] In West Germany, women have been exempted from compulsory military service. Women have been allowed to work voluntarily for the military only in medical professions. Since a few years, women are also allowed to volunteer for service in the fighting troops.

[3] The first 340 conscientious objectors started their service in April 1961. Until the seventies, the annual number of conscientious objectors stayed at a negligible level (Haberhauer and Maneval 2000).

[4] See, for example, Imbens and van der Klaauw (1995) for the Netherlands.

because of high treason or the endangering of outer security are not drafted.

In the early years of conscription in Germany, additinal reasons could lead to an exemption. These are of particular importance for the birth cohorts at the center of interest in our analysis. Sole sons of a soldier killed in World War II or a soldier still missing were exempted from military service. This rule applied to about 10% of the men in the birth cohorts from 1937 to 1944 (Wehrstruktur-Kommission 1971). Those who lost all brothers or all sisters, or one or both parents during World War II could also request to be exempted from military service. In addition to these special exemptions, persons who are examined to be fit for service could defer the draft if they face a personal, economic or occupational situation in which the draft would be a serious hardship. This temporary deferment could result in a permanent exemption when these persons reached the maximum age limit for military service of 25. According to the Wehrstruktur-Kommission (1971), about 3.5% of all men in a birth cohort received a permanent exemption from military service in the 1960s because they were older then 25.

The end of the Cold War in conjunction with an increased involvement of the German army in UN missions such as in former Jugoslavia and – more recently – in Afghanistan, as well as increasing efforts of the German government to reduce the defense budget have led to a new situation. A heated discussion ensued whether the system of the recruitment of conscripts should be replaced by an all-volunteer force. The debate on the pros and cons of an all-volunteer force versus the practice of conscription has mainly been dominated by historical, socio-political, defense-political and military-practical arguments. Economic arguments, however, have been largely neglected. More importantly, existing relative cost-benefit-analyses of the two recruitment systems typically do not take all relevant costs into account. Specifically, these analyses tend to ignore the costs accruing to the conscripts themselves.

The existing economic literature on the individual effects of compulsory military service in Germany concentrates predominantly on the implicit income tax placed on the conscripts. This tax equals the difference between the income that those who serve could earn in the civilian market and the (usually lower) income during military service. Using aggregate data for 1995, Schleicher (1996) calculates, for example, that the direct monthly opportunity costs of service for the average German conscript are approximately Euro 1,950. The total costs of compulsory military and civil service resulting from this implicit tax amount to more than 27% of the official defense budget in 1995. Similar numbers are reported by Schäfer (2000). Because of the various exemptions from military service, this implicit income tax is not equally distributed among the male population.

One can even argue that compulsory military service leads to a reallocation of welfare to the detriment of the conscripts, because those who are not drafted also benefit from the increased outer security. Although these opportunity costs are important, here we do not aim to estimate them. We rather follow the studies by Angrist (1990), Angrist and Krueger (1994), and Imbens and van der Klaauw (1995) who focus on the potential effects of compulsory military service on labor market outcomes following the service.

These long-term effects of compulsory military service on subsequent labor market outcomes could be expected to be numerically at least as important as the direct opportunity costs of service, most likely even more important. Several channels might lead to this implication. Usually, potential recruits are not immediately drafted after either their health examinations or the end of their temporary exemptions. Typically, they will not be able to foresee the exact date of this draft, since they can be called for service any time. Due to this insecurity, it may be difficult for them to find a permanent employment position. They may be obliged to accept casual employment and are more likely to be unemployed (Oi 1967). This insecure labor market status during the waiting period may have detrimental effects on the human capital of potential recruits in terms of lower job training or occupational specific skills. Hence it may result in relatively lower earnings and employment prospects even after military service.

It has further been argued that conscripts suffer from a deprivation of their human capital stock during their time of service (see, for example, Spencer and Woroniak (1969)). Since the wages paid by the armed forces to the conscripts are usually lower than their market wage, armed forces that rely on conscription usually suffer from military overmanning. In other words, the relatively low labor costs in the military sector lead to an inefficient low capital-labor ratio. This in turn suggests that the armed forces have no incentive to use the human capital of the conscripts in an efficient way and to allocate them to appropriate jobs within the military sector.

Military service may, however, also have positive effects on the human capital stock of the conscripts. During military service, for example, conscripts may acquire soft skills, such as working in a team and in a strict hierarchical structure. Assuming that these skills are rewarded by the labor market, they may have positive effects on the labor market position of those who served. Finally, performing compulsory military service may be used by potential employers as a signal for a good health status or a high sense of responsibility resulting in positive statistical discrimination of veterans. Employers may also positively discriminate veterans for other reasons such as patriotism.

Overall, this discussion shows that the long-term labor market effects of compulsory military service are not easily predictable. In our empirical analysis we therefore estimate the net effects of compulsory military.

III. Econometric Framework: The Regression Discontinuity Approach

Investigating the effects of compulsory military service on subsequent labor market outcomes presents a classical evaluation problem. Since men are either drafted or not drafted for compulsory military service, we do not observe a single individual in both states. Consider any generic labor market outcome of interest, for instance lifetime earnings. Suppose that this outcome is determined by the equation:

$$Y_i = \alpha_i + \beta M_i + \epsilon_i, \tag{1}$$

where Y_i denotes lifetime earnings of individual i, and M_i is the treatment indicator that equals unity if individual i performed compulsory military service and zero otherwise. If treatment is nonrandom, simple OLS-estimates of $\beta \equiv E[Y|M = 1] - E[Y|M = 0]$ will be inconsistent since $E[\epsilon|M] \neq 0$. Such a selection bias in the estimates of β could occur, for example, because the health status of an individual affects both the probability to be drafted for compulsory military service and lifetime earnings Y_i.

The existing literature on the labor market effects of military serve rely on instrumental variable estimations to correct for a potential bias the estimates of β (Angrist 1990; Angrist and Krueger 1994; Imbens and van der Klaauw 1995). These studies either use variations in the probabilities of being drafted across birth cohorts that are based on institutional regulations such as, for example, the draft-lottery for the Vietnam war (Angrist 1990) or special exemptions from military service resulting from an over-supply of potential conscripts (Imbens and van der Klaauw 1995). In the following empirical analysis we present IV-estimates similar to those of Imbens and van der Klaauw (1995) for the Netherlands using variations in the average probability of being drafted for different birth cohorts as an instrument for M_i.

In addition to modeling the selection bias resulting from nonrandom treatment assignment using an IV-approach, we employ also a regression-discontinuity (RD) approach to estimate the causal effect of compulsory military service on subsequent labor market outcomes. In the RD-approach, which has been introduced by Thistlethwaite and Campbell (1960), additional information about discontinuities in the probability to be selected into treatment, $P[M_i = 1|X_i]$, is used to obtain consistent estimates of β (van der Klaauw 1997; Hahn, Todd, and van der Klaauw 2001).

Apart from the treatment indicator M_i, let W_i be in indicator variable that equals one if a men was born at or after July 1, 1937, and zero otherwise. In the specific problem analyzed in this paper, one could differentiate three different sub-groups: men born before July 1, 1937 (the *White Cohort*) who were altogether exempted from military service (M_i=0, W_i=0); men born at or after July 1, 1937 who could in principle be drafted for military service but who were exempted due to one of the reasons described in the last section (M_i=0, W_i=1); and men born at or after July 1, 1937 who were drafted for compulsory military service (M_i=1, W_i=1). Hence, for the full sample of individuals the conditional probability of receiving treatment M_i, $f(B) \equiv E[M_i|B_i = B] = P[M_i = 1|B_i = B]$, is known to be discontinuous at a threshold value $\bar{B}$, which in our case is the birthday that separates the *White Cohort* from the birth-cohorts facing a positive probability of being drafted for compulsory military service.

The literature on the RD-approach typically differentiates between two types of discontinuities.[5] The case were treatment M_i is known to depend on B_i in a deterministic way is called the sharp design, the case were M_i is known to depend on B_i in a stochastic way is called the fuzzy design. Note, that our case could be considered as a mixture between the fuzzy and the sharp design. Let $\bar{B}$ denote the threshold birthday July 1, 1937. We know that men born before $\bar{B}$ are exempted from military service, i.e. $M_i = E[M_i|B_i < \bar{B}] = 0$, whereas the probability to be drafted for military service is a function of some individual characteristics X_i, such as health status, for those born at or after $\bar{B}$, i.e. $M_i = E[M_i|B_i \geq \bar{B}] = f(X_i) + \eta_i$.

[5]See van der Klaauw (1997) and Hahn, Todd, and van der Klaauw (2001) for a detailed discussion.

The RD-approach makes use of the expectation that individuals who are just below the threshold value $\bar{B}$ are similar in their observed and unobserved characteristics to individuals who are just above the threshold value $\bar{B}$. Therefore, in the absence of treatment effects individuals close to the threshold value $\bar{B}$ could be expected to have similar outcomes Y_i in the case of treatment as well as in the case of non-treatment. Stated differently, comparing a sample of individuals within a very small range around the threshold value $\bar{B}$ will be similar to a randomized experiment at the threshold value $\bar{B}$. In our empirical analysis, we argue that the distribution of observed and unobserved characteristics of individuals born just before July 1, 1937, will be arbitrarily close to the distribution of observed and unobserved characteristics of individuals born at or just after the July 1, 1937. Hence, for individuals born near the threshold date it could be expected that $E[\alpha_i|B_i = \bar{B}+\Delta] \cong E[\alpha_i|B_i = \bar{B}-\Delta]$, where Δ denotes an arbitrarily small number.

To see this, assume that both $E[\alpha_i|B_i = B]$ and the conditional mean function $E[\epsilon_i|B]$ are continuous at $\bar{B}$. Assuming further that the treatment effect is constant across different individuals, it can be shown that the average treatment effect is identified by (see van der Klaauw (1997), Hahn, Todd, and van der Klaauw (2001)):

$$\beta = \frac{lim_{B\to\bar{B}^+}E[Y_i|B_i = B] - lim_{B\to\bar{B}^-}E[Y_i|B_i = B]}{lim_{B\to\bar{B}^+}E[M_i|B_i = B] - lim_{B\to\bar{B}^-}E[M_i|B_i = B]} = \frac{Y^+ - Y^-}{M^+ - M^-}. \qquad (2)$$

Because $Pr[M_i = 1|B_i = B]$ is discontinuous at $\bar{B}$, the denominator is nonzero. Note again that our case represents a mixture between the sharp and the fuzzy RD-design, because M^- in equation (2) always equals zero. Hence, in our case the average treatment effect is identified by

$$\beta = \frac{Y^+ - Y^-}{M^+}. \qquad (3)$$

A limitation of the RD-approach is, however, that the treatment effect can only be identified for persons with values of B_i that are near the threshold value $\bar{B}$.

Allowing the treatment effect to be heterogeneous across individuals requires the additional assumption that treatment M_i is independent of β conditional on being near the threshold $\bar{B}$ (local conditional independence assumption), or, in other words, that the potential costs or returns from treatment do not change the behavior of individuals in a way they try influence the treatment probability. Under this assumption, the ratio defined in equation (2) identifies a local average treatment effect (LATE) in the sense proposed by Imbens and Angrist (1994), i.e. equation (2) identifies the marginal treatment effect for the sub-group of individuals for whom the probability of treatment changes discontinuously at $\bar{B}$.

In our case it could be reasonably argued that the local conditional independence assumption is very likely to be valid. Most reasons leading to an exemption from military service could either not be influenced by an individual or are by far too costly for an individual to change behavior in a way to affect treatment. Popular folklore maintains that some individuals are able to temporarily affect their health status around the medical examinations, inducing exemption. The administration responsible for these examinations, however, denies that this is indeed the case.

Assuming that the unknown parameter η_i in $M_i = E[M_i|B_i] = f(X_i) + \eta_i$ is an independent assignment error, i.e. $cov(u_i, \eta_i) = 0$, the treatment parameter β can be consistently estimated by implementing the regression:

$$Y_i = \alpha + \beta M_i + g(B_i) + \nu_i, \tag{4}$$

where $g(B_i)$ is a control function that is continuous in S and represents a specification of $E[\epsilon|B]$. In the estimations below, we will specify $g(B_i)$ as a higher order polynomial in the month of birth of an individual i. If, however, η_i is not independent of u_i given M_i, estimation of equation (4) will lead to biased estimates of β. The resulting selection bias depends on $cov(T_i, u_i|M)$ and may take any sign (see van der Klaauw (1997)).

The literature on the RD-approach suggests two solutions to this selection bias problem. van der Klaauw (1997) considers a two-step estimation procedure. Let $m_i = 1$ if $B_i > \bar{B}$ and $m_i = 0$ otherwise. In the first-step the propensity score is estimated using

$$E[M_i|B_i] = f(B_i) + \delta m_i, \tag{5}$$

where $f(B_i)$ is some function of B that is continuous in $\bar{B}$ and could be obtained by a semi- or nonparametric estimator. In the second stage, M_i in equation (4) is replaced by the first-stage estimates of $E[M_i|B_i] = Pr[M_i = 1|S_i]$, i.e.

$$Y_i = \alpha + \beta E[M_i|B_i] + g(B_i) + \nu_i. \tag{6}$$

If the functions for $g(B_i)$ and $f(B_i)$ are specified correctly, equation (6) produces consistent estimates of the treatment effect β. If $g(B_i)$ and $f(B_i)$ are estimated parametrically using the same functional form, this two-stage procedure reduces to a standard TSLS IV-estimator, where m_i and $f(B_i)$ are used as instruments. In this sense, those existing empirical studies on the labor market effects of military service which employ variations of the probability to be drafted across different birth-cohorts as instruments (see (Angrist 1990), (Angrist and Krueger 1994), (Imbens and van der Klaauw 1995)) implicitly apply a RD-approach.

In our empirical analysis, we will estimate the system of equations (5) and (6) using a piecewise-linear function of age for $f(B_i)$ and higher order polynomials of the difference of a person's age to the threshold date measured in days for $g(B_i)$. Equation (6 further includes a linear term of the age of an individual, three quarter-of-birth dummies and two dummies measuring the education of an individual. An advantage of this two-step approach is that the identification of β is not restricted to persons with values of B_i near the threshold value $\bar{B}$. A limitation of this approach is that it may be more fragile towards misspecification, because consistent estimates of β crucially depend on the correct specification of $f(B_i)$ and $g(B_i)$.

Hahn, Todd, and van der Klaauw (2001) propose a nonparametric approach to estimate the limits in equation (2). In particular, they suggest to estimate Y^+, Y^-, M^+, and M^- using one-sided uniform Kernel regression. The authors show this procedure to be numerically equivalent to a local Wald estimator. The advantage of this approach, if compared to the two-step estimation procedure suggested by van der Klaauw (1997), is that it does not require the specification of $f(B_i)$ and $g(B_i)$.

A disadvantage is, however, that the treatment effect is only identified for persons near the threshold $\bar{B}$ and that the nonparametric estimates of the limits in equation (2) requires the choice of a Kernel-function and of an appropriate bandwidth. Because of the poor boundary problems of standard kernel estimators, Hahn, Todd, and van der Klaauw (2001) propose to use local linear regression suggested by Fan (1992) instead of one-sided uniform Kernel regression to estimate the limits in equation (2). Here we apply both one-sided Gaussian Kernel regressions and local linear regressions to estimate the limits in equation (2).[6]

IV. Data Construction

In order to evaluate the causal effect of compulsory military service in Germany by an RD approach, we need data following individuals born around 1937 for a

[6] A more detailed description of both estimators is given in the Appendix.

long time period. The empirical basis for the following analysis is the *IAB Employment Sample (IAB_S)* together with a supplementary file that trace men's labor market history back into the past. The supplementary file facilitates obtaining a detailed characterization of the labor market status of an individual. In particular, we can distinguish between regular employment, unemployment and other 'non-active' states such as maternity leave, illness, disability, full-time education and military service.

The IAB_S represents a 1% random sample of all employees in Germany who pay social security contributions and who have been gainfully employed at least for one day between 1975 and 1995 (Bender, Haas, and Klose 2000). The data set is based on the integrated notifying procedure for health insurance, statutory pension scheme and unemployment insurance, which was introduced in 1973. To comply with legal requirements, employers must provide information to the social security agencies on all employees in jobs falling under mandatory social security provisions. These notifications are required for the beginning and ending of any employment relationship. In addition, employers are obliged to provide an annual report for each employee who is covered by social insurance and is employed on the 31^{st} December of each year. Note that the IAB_S is a so-called event data set. Hence, we are able to obtain labor market information and information on labor market transitions basically on a daily basis.

From the original sample of the IAB_S, we deleted all females, East Germans, and all persons that lived outside of West Germany at one point in time, including foreigners and ethnic German immigrants. We further restrict our analysis to persons born between January 1, 1934 and December 31, 1940 who worked at least for one day in our sample period. The latter selection was mainly driven by the desire to capture a symmetric range of birth-cohorts around the threshold year 1937 on the one hand, and to limit the probability that a non-negligible fraction of the individuals in our sample serves in alternative functions other than military service. Furthermore, individuals in the sample should not serve more than twelve months on the other hand. Thus, we are able to concentrate our empirical analysis on those persons who really served in the Bundeswehr. Professional soldiers and those who started compulsory military service and then decided to become professional soldiers have been deleted from our sample.

Figure 1 shows the average number of conscripts by month of birth. The figure shows that about 18% of the men born in July 1937 have been drafted for military service. The share of conscripts is slightly decreasing for men born between July 1937 and August 1938 and is sharply increasing for men born after the mid of 1938. This development could be explained by temporary friction sin building up the Bundeswehr in the first years, the special exemptions for potential conscripts which have been described above, and the gradual intensification of the cold-war.

As outcome variables Y_i we consider the real daily gross wage at different points in the life-cycle of an individual in order to obtain a complete picture of the wage effects of military service. As a composite indicator of labor market success we further consider the cumulative real earnings from age 25 to age 55. Differences in real wages at different points in the life-cycle and cumulative earnings might be the result of differences in accumulated labor market experience due to distinct probabilities of becoming unemployed, sick or leaving the labor market. Therefore, we also investigate the effects of compulsory military service on average cumulative earnings from age 25 to 55, defined as cumulative earnings divided by the cumulative days of employment. All outcome variables used in the empirical analysis are measured at the respective birthday of an individual.

Table 1 and Figures 2 and 3 report some descriptive statistics of the variables used in our analysis. The wages reported in Table 1 show the typical concave pattern over the life-cycle that could be observed in almost all developed countries. Individuals from the *White Cohort* have about 0.1 percent lower average earnings than those born at or after July 1, 1937; the difference is, however, not statistically significant. Throughout the life-cycle, individuals that served in military earn more than both individuals from the *White Cohort* and individuals born after July 1, 1937 who were not drafted from military service. These patterns are confirmed by Figures 2 and 3. Figure 2(a) and Figure 2(b) show the age earnings-profiles for the three groups (Individuals from the white cohort, individuals born at or after July 1, 1937 who served in the Bundeswehr, and individuals born at or after July 1, 1937 who did not serve in the Bundeswehr) for the full-sample and the sample of individuals born in 1937, respectively. These figures show again, that those who served in the Bundeswehr earn more than those that did not serve and than individuals in the White Cohort. Note that the respective earnings differentials are smaller if we restrict our sample to individuals born in 1937. A similar picture emerges if we look at cumulative lifetime earnings, which are depicted in Figure 3(a), and the average lifetime wages that are shown in Figure 3(b). As already discussed above, however, these simple comparison might be misleading due to selection into treatment.

The validity of the RD-approach largely rests on the assumption that individuals near the threshold value $\bar{B}$ are similar with regard to their observed and unobserved characteristics. Unfortunately, the IAB_S provides only limited information on those characteristics of the individuals which are determined before a potential draft. These are the traits unaffected by military service. The only variable available in the IAB_S which largely satisfies these requests is the educational attainment of the individuals in our sample. For most men, schooling is already completed at the time of their draft. However, for those planning to graduate from university, the draft usually occurs between the completion of high school and the start of university education. For these persons, the draft defers university education but does not necessarily affect the decision to obtain a university degree.[7]

Table 2 reports mean schooling levels of persons from different birth cohorts. Table 2 shows that there are statistically significant differences in educational attainment between the White Cohort and the Post-White Cohort when using the full sample, with the latter showing slightly higher shares of individuals with middle or higher education. Note, however, that these differences disappear as soon as we restrict our sample to those born in 1937.

V. Estimation Results

The estimated returns to military service obtained by simple OLS regressions are shown in Figure 4 and Table 3. The estimated coefficients in Figure 4 are obtained from OLS regression that include a dummy variable indicating whether an individual served in the Bundeswehr, two education dummies and year-of-birth dummies. The figure shows that for all three sub-samples depicted in this figure, the returns to military service are increasing over time. Using the full sample (see Figure 4a) the estimated return to military service varies between 0.027 in 1967 and 0.030 in 1987. For all years, the estimated coefficients are statistically significant. Restricting the sample to those born at or after the threshold date July 1, 1937 (see, Figure 4b), the returns appear to be very similar to those obtained when using the full sample, varying between 0.029 in 1967 and 0.031 in 1987. Restricting the sample only

[7] Those persons who are categorized to be fit for service and start their university education before being drafted could apply for a temporary exemption from military service until they receive their university degree. According to the law, however, they have to expect to be drafted before the maximum age limit of 25, irrespective of whether they finished their degree. Hence, there are strong incentives to enter military service before starting university education. The law further limits the possibilities to decide to go to university just to avoid the draft to military service.

to individuals born in 1937, however, provides a somewhat different picture (see, Figure 4c). First, the estimated returns to military service show a higher variance and are statistically insignificant for the years 1967-1971. These differences may be explained by the smaller sample size.

Table 3 shows the estimated returns to military service when using average lifetime wages and cumulative lifetime earnings as dependent variables. Using the full sample, the results suggest that over their entire lifecycle those who served in the Bundeswehr have about wages that are about 4.5 percent higher than the wages of those who have been exempted or are born before July 1, 1937 (see column 3 of Table 3). This advantage amounts to a cumulative lifetime earnings advantage of about 15 percent. The results appear to be very similar when restricting the sample to those born at or after July 1, 1937 and somewhat smaller, but still statistically significant, when restricting the sample to those born in 1937.

As discussed in section III, it is very likely that the estimated returns to military service are biased due to a non-random selection of the conscripts. The existing literature on the wage effects of military service tried to solve this problem by using instrumental variable estimation. Here we follow Imbens and van der Klaauw (1995) by using the average conscription rate by month of birth as an instrument for M_i. The results from these instrumental variable estimations are reported in Table 4. Note that the estimations include only individuals born at or after July 1, 1937. The returns to military service are positive and somewhat higher than the corresponding OLS-estimates in Table 3. However, none of the estimated coefficients is statistically significant at conventional levels.

Table 5 shows the results when using the discontinuity of the probability to be drafted to identify the causal effects of military service on subsequent wages. Panel A of Table 5 shows the results of 2SLS estimations when using a dummy variable that takes the value one if an individual is a member of the post-white cohort, and zero otherwise. Using this approach the returns to military service become negative. However, they are estimated very imprecisely.

In Panel B of Table 5 we apply the two-stage estimation strategy suggested by van der Klaauw (1997). We estimated the system of equations (5) and (6) using a piecewise-linear function of age for $f(B_i)$ and higher order polynomials of the difference of a person's age to the threshold date measured in days for $g(B_i)$. Equation (6 further includes a linear term of the age of an individual, three quarter-of-birth dummies and two dummies measuring the education of an individual. Similar to Panel

A of Table 5 the returns to military service are estimated to be negative. However, they are much smaller in absolute values if compared to the corresponding coefficients in Panel A. Again, none of the estimated coefficients is statistically significant at conventional levels.

Nonparametric estimates: To be written.

VI. Conclusions

To be written.

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A Wald Estimator based on Kernel Regression

One way to get estimates of the limits Y^+, Y^-, M^+, and M^- in equation (2) is to use one-sided kernel regression, which are given by

$$\hat{Y}^+ = \frac{\sum_{i=1}^n Y_i m_i K(u)}{\sum_{i=1}^n m_i K(u)},$$

$$\hat{Y}^- = \frac{\sum_{i=1}^n Y_i (1-m_i) K(u)}{\sum_{i=1}^n (1-m_i) K(u)},$$

$$\hat{X}^+ = \frac{\sum_{i=1}^n X_i m_i K(u)}{\sum_{i=1}^n m_i K(u)},$$

$$\hat{X}^- = \frac{\sum_{i=1}^n X_i (1-m_i) K(u)}{\sum_{i=1}^n (1-m_i) K(u)},$$

where $m_i = 1$ if $B_i > \bar{B}$ and $m_i = 0$ otherwise; $K(u)$ is the uniform kernel, i.e. $K(u) = 1/2$ if $|u| \leq 1$ and $K(u) = 0$ otherwise; $u = (B_i - \bar{B})/h$; and h is an appropriate bandwidth parameter. Hahn, Todd, and van der Klaauw (1999) show formally that this estimator is numerically equivalent to an IV-estimator of a regression of Y_i on M_i, which uses m_i as an instrument, applied to the subsample for which $\bar{B} - h_- < B_i < \bar{B} + h_+$.

B Local Linear Smoother

The local linear estimate of the conditional mean of y given x could be expressed as a pointwise weighted least square estimator:

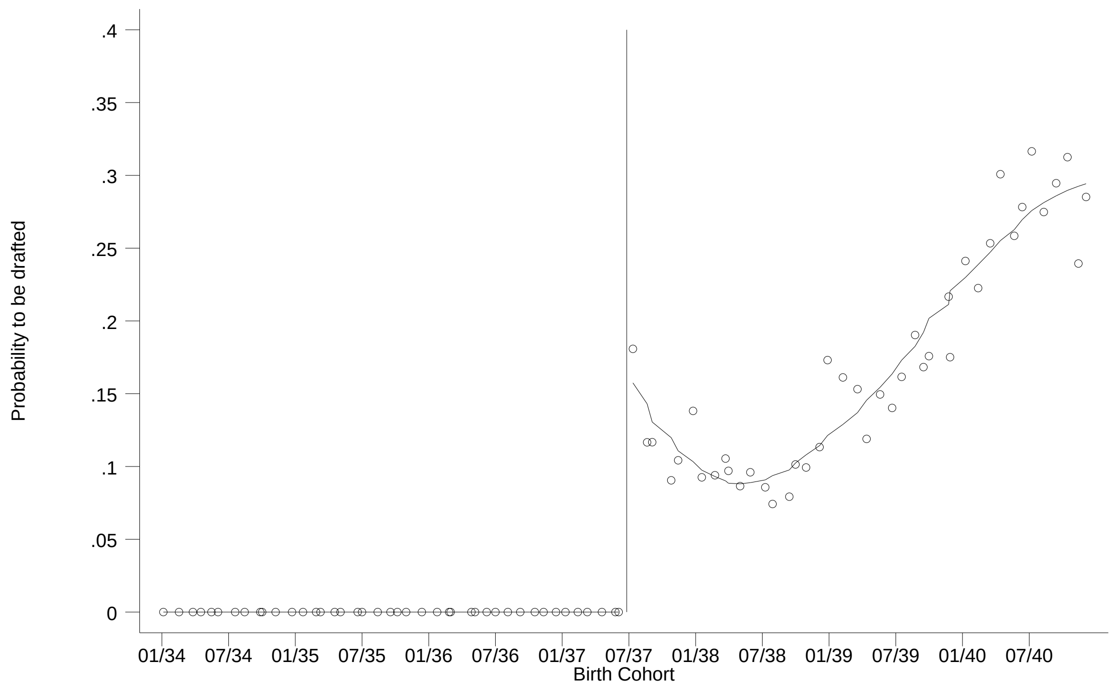
$$E[y_i|x] = \hat{a} + \hat{b}x,$$

where

$$(\hat{a}, \hat{b}) = argmin_{a,b} \sum_{i=1}^n (y_i - a - bx)^2 K(u)\, m_i,$$

with $u = (B_i - \bar{B})/h$. h is a suitable bandwidth parameter. Note that if the nearest neighbor method is used instead of a fixed bandwidth, one obtains the Lowess estimator (Cleveland 1979). The local linear estimator have been shown by Fan (1992) to have better boundary properties than a standard kernel regression estimator.

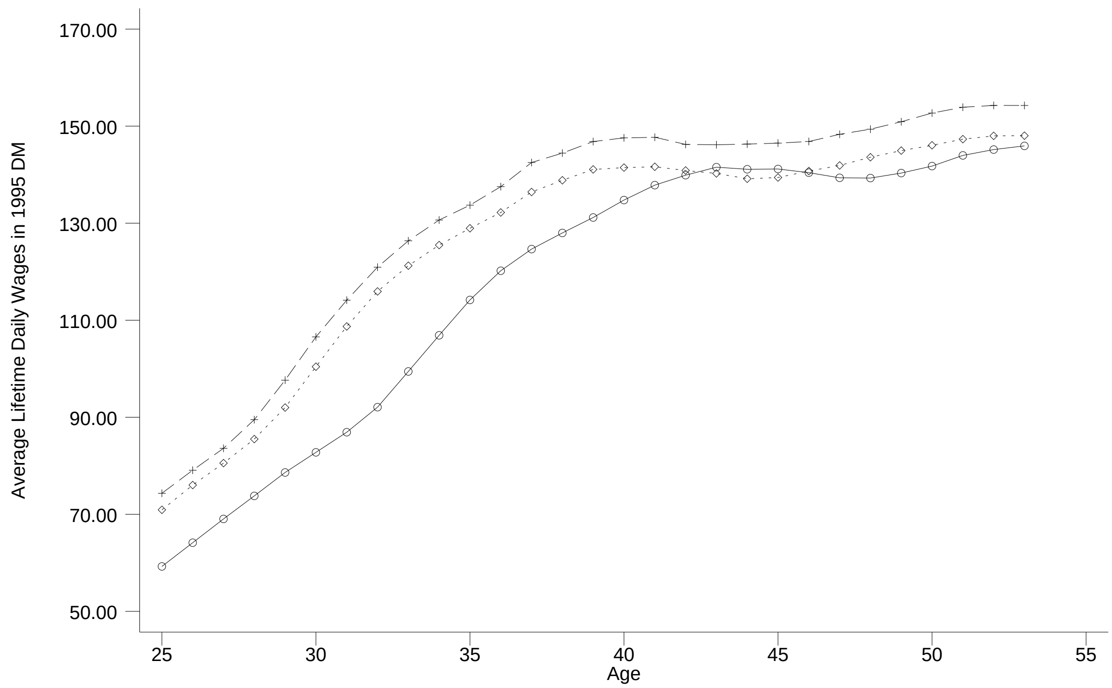
Figure 1: Probability to be Drafted for Military Service



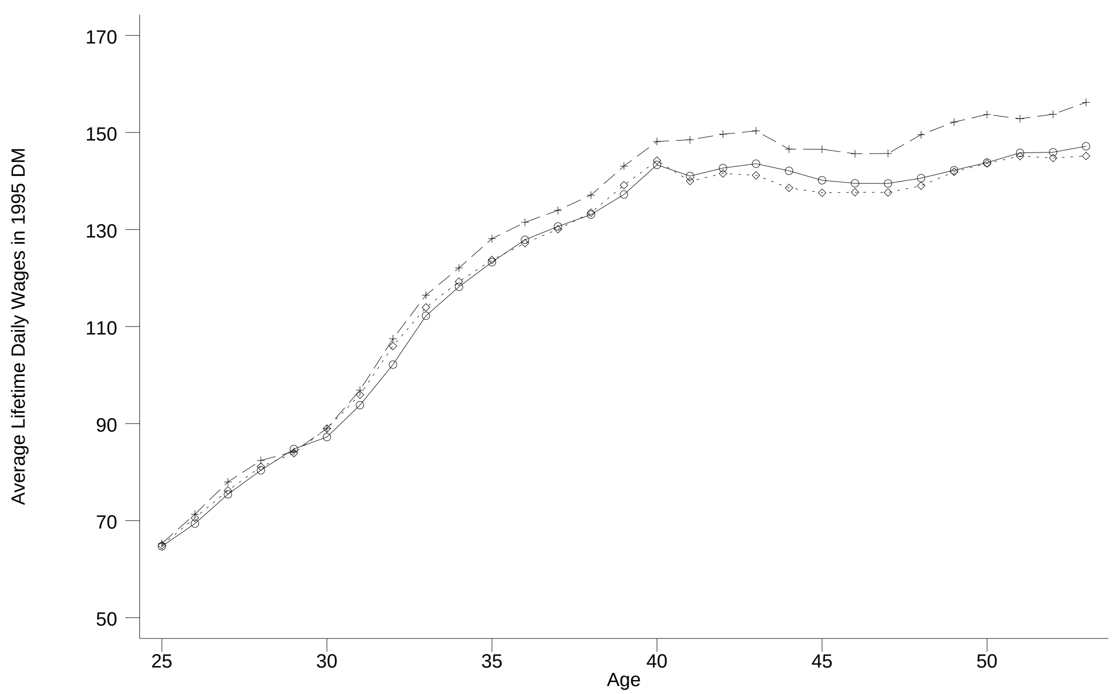
Notes: IAB$_S$ supplementary file; own calculations.

Figure 2: Age Earnings Profiles by Birth Cohort and Military Service

(a) Full Sample

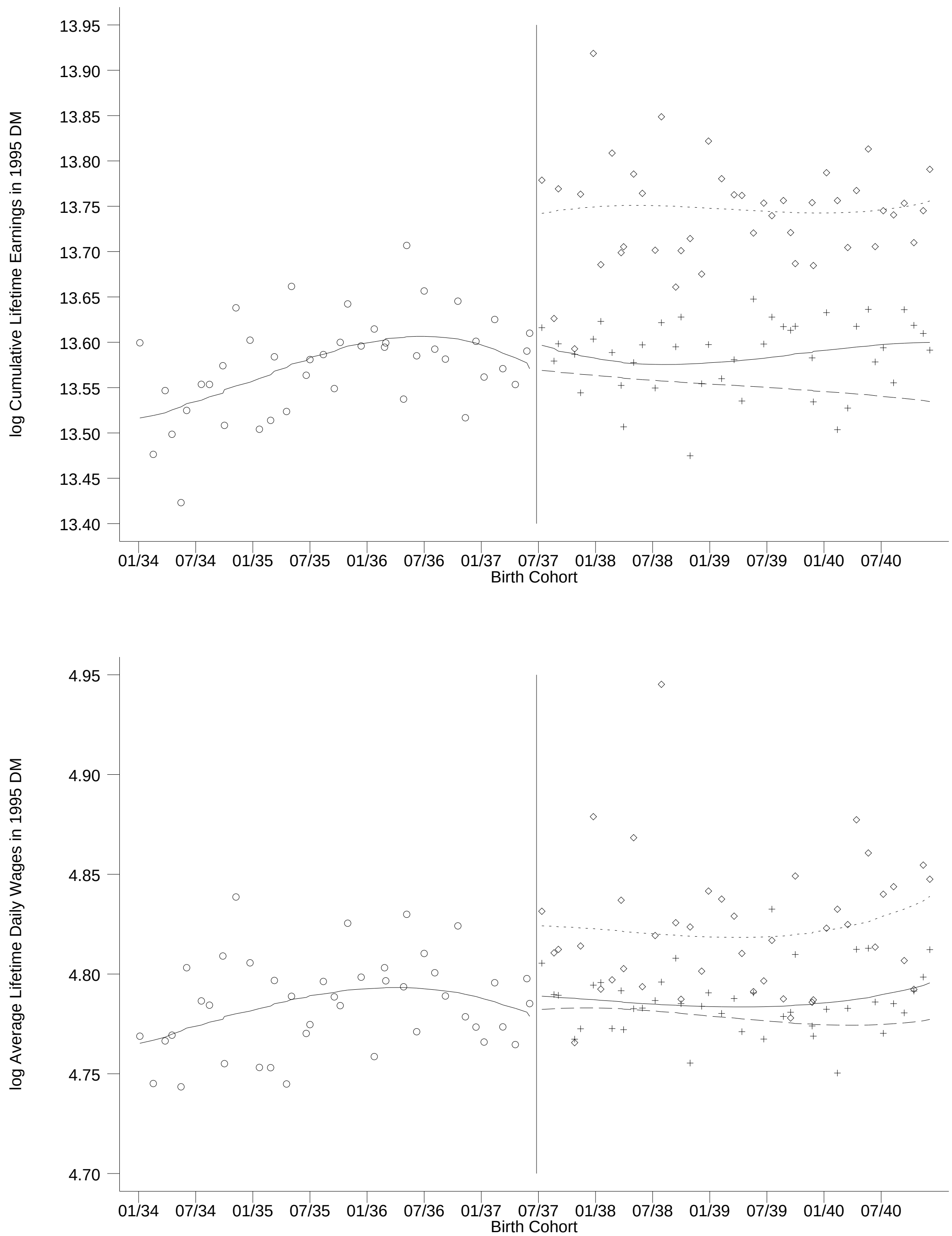


(b) Birth-Cohort 1937



Notes: IAB_S supplementary file; own calculations. o: White Cohort; +: Not White Cohort, Military Service; ⋄: Not White Cohort, No Military Service. *Polynomial fit:* —: White Cohort; - - -: Not White Cohort, No Military Service; · · ·: Not White Cohort, Military Service;

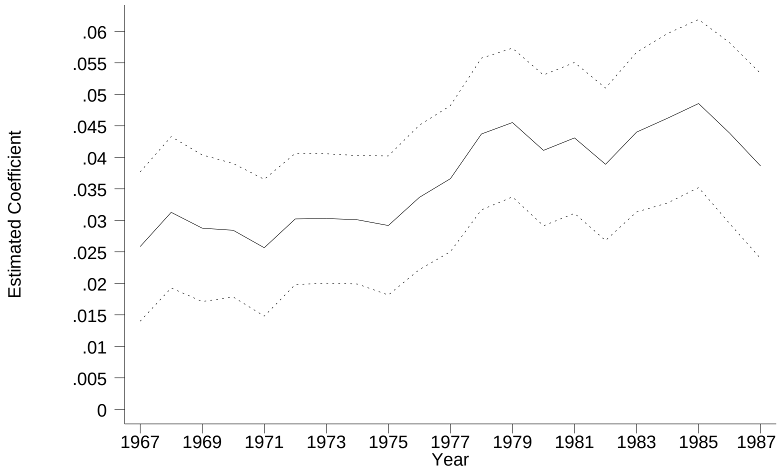
Figure 3: Lifetime Earnings Profiles by Birth Cohort and Military Service



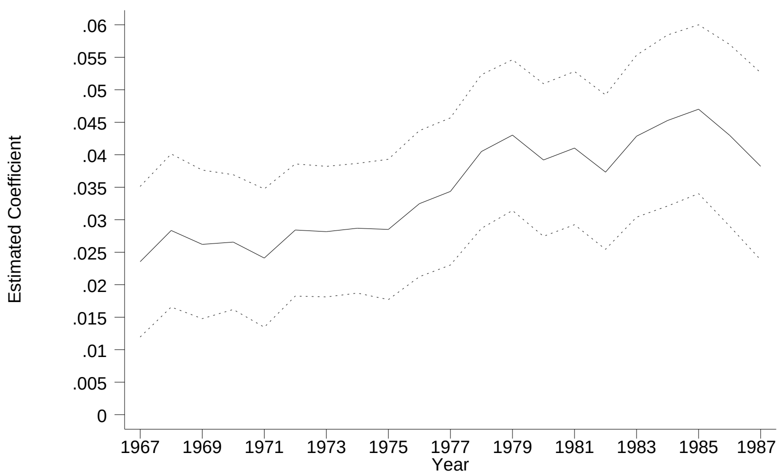
Notes: IAB_S supplementary file; own calculations. o: White Cohort; +: Not White Cohort, Military Service; ◇: Not White Cohort, No Military Service. *Polynomial fit:* —: White Cohort; - - -: Not White Cohort, No Military Service; ···: Not White Cohort, Military Service;

Figure 4: OLS-Estimates of the Return to Compulsory Military Service

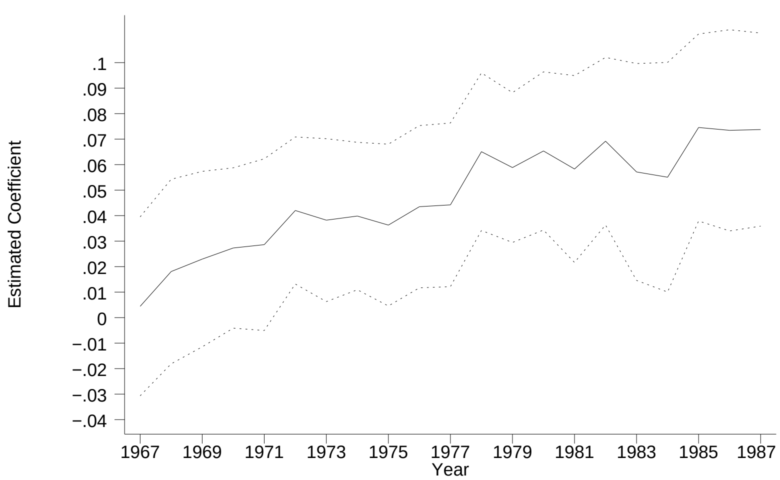
(a) Full Sample



(b) Birth-Cohort July 1937 - December 1940



(c) Birth-Cohort 1937



Notes: IAB_S supplementary file; own calculations.

Table 1: Descriptive Statistics

	Birth Cohort January 1934 - June 1937	*Birth Cohort July 1937 - December 1940*		
		Total	*No Military Service*	*Military Service*
log(Real Daily Wage) at Age 25	4.026	4.215	4.209	4.240
	(0.250)	(0.239)	(0.244)	(0.216)
log(Real Daily Wage) at Age 35	4.685	4.841	4.834	4.872
	(0.258)	(0.226)	(0.231)	(0.200)
log(Real Daily Wage) at Age 45	4.982	5.017	5.010	5.047
	(0.285)	(0.299)	(0.307)	(0.262)
log(Real Daily Wage) at Age 55	5.081	5.033	5.022	5.079
	(0.381)	(0.389)	(0.406)	(0.305)
log(Cumulative Wage) Age 25-55	13.823	13.891	13.861	14.023
	(0.670)	(0.692)	(0.718)	(0.550)
log(Average Cumulative Wage) Age 25-55	4.754	4.846	4.833	4.900
	(0.273)	(0.271)	(0.278)	(0.231)

Notes: IAB_S supplementary file; own calculations. Standard deviations in parentheses.

Table 2: Pre-Determined Characteristics by Birth-Cohort

	White Cohort	*Post-White Cohort*	*Difference*
A: Full Sample			
Low Education	0.280 (0.004)	0.246 (0.004)	0.034** (0.006)
Middle Education	0.665 (0.005)	0.686 (0.004)	-0.023** (0.006)
High Education	0.054 (0.002)	0.066 (0.002)	-0.011** (0.003)
Observations	10,234	12,175	22,409
B: Birth-Cohorts: 1936-1938			
Low Education	0.277 (0.007)	0.249 (0.006)	0.028** (0.009)
Middle Education	0.664 (0.007)	0.687 (0.007)	-0.023* (0.010)
High Education	0.059 (0.003)	0.065 (0.004)	-0.005 (0.005)
Observations	4,697	4,772	9,469
C: Birth-Cohorts: January 1937 - December 1937			
Low Education	0.284 (0.011)	0.262 (0.012)	0.022 (0.016)
Middle Education	0.656 (0.012)	0.681 (0.012)	-0.026 (0.017)
High Education	0.060 (0.006)	0.057 (0.006)	0.003 (0.008)
Observations	1,658	1,443	3,101
D: Birth-Cohorts: April 1937 - September 1937			
Low Education	0.283 (0.016)	0.266 (0.016)	0.016 (0.023)
Middle Education	0.654 (0.016)	0.683 (0.017)	-0.029 (0.024)
High Education	0.063 (0.008)	0.050 (0.008)	0.013 (0.012)
Observations	842	736	1,578
E: Birth-Cohorts: June 1937 - July 1937			
Low Education	0.285 (0.028)	0.243 (0.026)	0.043 (0.038)
Middle Education	0.650 (0.029)	0.713 (0.027)	-0.063 (0.040)
High Education	0.065 (0.015)	0.044 (0.012)	0.021 (0.020)
Observations	263	272	535

Notes: IAB_S supplementary file; own calculations. Standard errors in parentheses.
**: Statistically significant at least at the 1%-level. One-sided test
*: Statistically significant at least at the 5%-level. One-sided test

Table 3: Estimates of the Returns to Compulsory Military Service: OLS-Results

log (Average Lifetime Daily Wages) in 1995 DM						
	Full Sample		*Not White Cohort*		*Birth Cohort 1936 - 1938*	
	(1)	*(2)*	*(1)*	*(2)*	*(1)*	*(2)*
Military Service	0.054**	0.045**	0.056**	0.047**	0.043**	0.037**
	(0.006)	(0.005)	(0.006)	(0.005)	(0.011)	(0.011)
Apprenticeship/High-School	-	0.186**	-	0.183**	-	0.189**
		(0.004)		(0.006)		(0.006)
University	-	0.364**	-	0.349**	-	0.381**
		(0.008)		(0.011)		(0.013)
Observations	22,108	22,108	12,028	12,028	9,361	9,361

log (Cumulative Lifetime Earnings) in 1995 DM						
	Full Sample		*Not White Cohort*		*Birth Cohort 1936 - 1938*	
	(1)	*(2)*	*(1)*	*(2)*	*(1)*	*(2)*
Military Service	0.185**	0.150**	0.192**	0.159**	0.161**	0.130**
	(0.014)	(0.013)	(0.014)	(0.014)	(0.029)	(0.029)
Apprenticeship/High-School	-	0.417**	-	0.418**	-	0.429**
		(0.014)		(0.020)		(0.023)
University	-	0.501**	-	0.554**	-	0.530**
		(0.026)		(0.030)		(0.041)
Observations	22,108	22,108	12,028	12,028	9,361	9,361

Robust standard errors in parentheses. Columns 2 and 3, and columns 4 and 5 further include 6 and 3 year-of-birth dummies, respectively.

Table 4: 2SLS-Estimates of the Returns to Compulsory Military Service
Instrument: Average Conscription Rate by Month of Birth
Sample: Birth Cohort July 1937 - December 1940

	Average Daily Wages		Lifetime Earnings	
	(1)	*(2)*	*(1)*	*(2)*
Military Service	0.088 (0.066)	0.106 (0.061)	0.191 (0.189)	0.233 (0.181)
Apprenticeship/High-School	-	0.178** (0.008)	-	0.412** (0.025)
University	-	0.351** (0.011)	-	0.558** (0.031)
Observations	12,028	12,028	12,028	12028

Robust standard errors in parentheses.

Table 5: RD-Estimates of the Returns to Compulsory Military Service

	Average Daily Wages		**Lifetime Earnings**	
	Full Sample	*Birth Cohort 1936 - 1938*	*Full Sample*	*Birth Cohort 1936 - 1938*
Panel A: Without control function $g(B_i)$				
Military Service	-0.150	0.001	-0.742	-0.097
	(0.145)	(0.090)	(0.451)	(0.273)
Observations	22,108	9,361	22,108	9,361
Panel B: With control function $g(B_i)$				
Military Service	-0.019	-0.016	-0.192	-0.221
	(0.069)	(0.120)	(0.214)	(0.304)
Observations	22,108	9,361	22,108	9,361

Robust standard errors in parentheses.