

The impact of military service on criminal offending over the life course: evidence from a Dutch conviction cohort

Marieke van Schellen · Robert Apel ·
Paul Nieuwbeerta

Published online: 24 February 2012

© The Author(s) 2012. This article is published with open access at Springerlink.com

Abstract

Objectives Examination of the relationship between military service and criminal conviction, and evaluation of its sensitivity through the use of two distinct study designs.

Methods Data were collected on the conviction histories of a cohort of men convicted of a crime in 1977 in the Netherlands ($n=678$). The men were born between 1940 and 1964, and reached the age of military eligibility during an era when service was compulsory, although they could be classified as ineligible or exempt for a variety of reasons. Information on criminal conviction was collected from age 12 (the age of criminal responsibility) to calendar year 2003. The first study design—a quasi-experiment—uses panel methods and control variables to compare the risk of conviction among men who served in the military to all other men in the sample who did not serve in the military, irrespective of their reason for not serving. The second study design—a natural experiment—compares men who served in the military to men who, as a result of policies of the Dutch government, were exempt from service but otherwise would have served. For example, at various times, the Dutch government exempted an entire birth cohort from service, or exempted men through use of a lottery.

Results In the quasi-experiment, military service reduced the odds of criminal conviction by 22% in each year of the post-service period (odds ratio=0.78; $p<.01$). In the natural experiment, on the other hand, there was no relationship between military

M. van Schellen (✉)

Department of Sociology, Utrecht University, Heidelberglaan 2, 3584 CS Utrecht, The Netherlands
e-mail: m.vanschellen@uu.nl

R. Apel

School of Criminal Justice, Rutgers University, 123 Washington Street, Newark, NJ 07102, USA
e-mail: robert.apel@rutgers.edu

P. Nieuwbeerta

Department of Criminology, Leiden University, Steenschuur 25, 2311 ES Leiden, The Netherlands
e-mail: p.nieuwbeerta@law.leidenuniv.nl

service and long-term conviction risk (odds ratio=0.98). Follow-up analyses indicated that the results from the two study designs did not differ by crime type, nor were they age graded. There was suggestive evidence that military service actually worsened conviction risk among men who performed the worst on tests of military fitness (e.g., intelligence, language and mathematical skills).

Conclusions Two study designs yield two different conclusions concerning the impact of military service on long-term conviction risk. However, the authors view the results from the natural experiment as authoritative, due to the success of this design in achieving superior balance between men who served in the military and men who did not serve. Therefore, the authors conclude that military service had no causal impact on criminal conviction in a sample of high-risk Dutch men. Qualifications of the study findings include the use of conviction data from official sources, the use of a sample of men who were all convicted of a crime at some point in their lives, and the study of military service which was compulsory rather than voluntary. Attention should also be paid to the different cultural and military context of the Netherlands compared to the United States.

Keywords Military service · Criminal conviction · Life-course criminology · Panel models · Natural experiment

Introduction

During the twentieth century, a large number of young Dutch men entered the military around the age of 18, during an era when such service was compulsory. As in other Western countries, conscripts were accommodated in barracks during a large part of their service time. Their stay in barracks isolated them from their old living environments and from society as a whole. They entered a world with a distinctive language, and different norms and values, with an emphasis on discipline, uniformity, hierarchy, mutual solidarity, team spirit, and group conformism. Most men served in the land forces, with a smaller number in the air and marine forces (Bullinga 1984, 1988; Van der Meulen 1984). During their service period, conscripts followed training programs which prepared them for life as a soldier and the potentiality of war (Kors 1996).

Service in the military is considered an important transitional event that can influence an individual's life course in a variety of domains (Elder 1986; Maas and Settersten 1999; MacLean and Elder 2007). One such area concerns criminal behavior (Allen 2000; Laub and Sampson 2003; Rand 1987). In recent years, a number of empirical studies have tested different theoretical expectations about the relationship between military service and criminal behavior (e.g., Bouffard and Laub 2004; Sampson and Laub 1996; Yager et al. 1984). The results are equivocal, as some studies have found beneficial effects of military service while others report no effects at all. However, these studies refer to different historical contexts and employ a variety of research designs.

The substantive aim of the current study is to acquire insight into the possible causal effects of military service on long-term criminal behavior, using new empirical data from a unique long-term study in a different cultural context. The data come

from the Criminal Career and Life-Course Study (CCLS), a sample of individuals who were all convicted of a crime at least once during their lives in the Netherlands. The criminal histories of these individuals have been re-constructed from ages 12 to 63 for the oldest individuals in the sample. Our interest is in untangling whether a plausible causal relationship exists between military service and subsequent development of criminal behavior in this high-risk Dutch sample.

The current study also has a methodological aim of providing estimates of the “military effect” on criminal conviction from two different designs: a quasi-experiment and a natural experiment. In program evaluation, there is ongoing debate about the relationship between the quality of research designs and the validity of study outcomes (Weisburd et al. 2001; Welsh et al. 2010). By comparing two different designs, the current study will contribute further evidence to this important discussion. The two designs specifically differ in the selection of CCLS men who comprise the non-military comparison group. The first approach is a common panel data approach, relying on random- and fixed-effects models with statistical controls. In these models, men who served in the military are contrasted with all other men in the CCLS who did not serve. The second approach exploits policies of the Dutch Ministry of Defense that periodically exempted some men from military conscription. In these models, men who served in the military are compared to their CCLS counterparts who were exempted from service.

Military service and the development of criminal offending

Criminological theories are divided about the salience of life events for the unfolding of criminal behavior, leading to mixed expectations about the impact of military service on criminal behavior. Theories can generally be divided into one of two different camps (or a combination of both)—static and dynamic (or ontogenetic and sociogenic). Each theoretical perspective makes important assumptions about the influence of significant life events and transitions on criminal behavior and criminal propensity (Cohen and Vila 1996; Paternoster et al. 1997). In the paragraphs below, each of these perspectives is considered in greater detail.

In one camp are the proponents of static theories, of which Gottfredson and Hirschi (1990) are the most prominent. These theories assume that criminal behavior can be completely explained by latent personal characteristics, or a “propensity” to commit crime and other deviant behavior, in combination with opportunities to put these impulses into action. Because criminal propensity is formed fairly early in life and tends to remain relatively stable throughout the life span, significant life transitions such as marriage, employment, and incarceration are believed to have no genuine causal influence on criminal behavior. Instead, any correlation between life events and crime is spurious, as both are attributable to the same underlying criminal propensity (Hirschi and Gottfredson 1995). Applied to the question of military service, static theories would predict that this life event has no influence on criminal propensity, and only affects criminal behavior through opportunities. On the one hand, because of its emphasis on discipline and close supervision, the military might severely restrict criminal opportunities and, thus, during the period of service, be associated with reduced offending. On the other hand, the military setting might

represent an environment with newfound opportunities for crime-prone young men to continue their old way of living, and thus be associated with more criminal offending during the service period. However, any relationship between military service and criminal behavior should be observed only during the period of service, and only because of the restriction/expansion of viable opportunities for crime. No change in criminal propensity, or in criminal behavior following completion of one's military obligation, is to be expected, other things being equal.

In another camp are dynamic theories, which presume that life circumstances do indeed influence the development of criminal behavior, in childhood and adolescence as well as throughout adulthood. The most prominent criminological theorists in this tradition are Sampson and Laub (1993). According to them, important life-course events such as marriage, divorce, and children constitute "transitions" which involve changes in the opportunity structure for criminal behavior, but become "turning points" when they also involve changes in criminal propensity for better or worse. Moreover, the turning point potential of life events transcend individual differences in criminal propensity (Blokland and Nieuwebeerta 2005a, b; Laub and Sampson 2003). With respect to military service, dynamic theories are friendly to the potential for long-term changes in the risk of criminal behavior that are due to genuine changes in criminal propensity.

In line with the dynamic vision, different mechanisms have been proposed for why serving in the military would lead to reduced criminal behavior throughout the life course. First, the military cuts young men off from their pasts by forming a *new social context* with different roles and responsibilities (Bouffard 2003). Isolated from family and neighborhood friends, the army can act as a new beginning, offering new opportunities for achievement and therefore avenues for change (Elder 1986). In particular, young men from severely disadvantaged environments are removed from potentially adverse influences, such as low-status neighborhoods or delinquent friends. Those with a criminal record might even overcome the potential stigmatizing effects of their past behavior. Second, military service is ascribed a *resocializing function* that can motivate behavioral change. The military is a "total institution" with a distinctive culture characterized by discipline, subordination, formalism, responsibility, and group cohesion (Bullinga 1984, 1988; Van der Meulen 1984; Sun et al. 2007: 602). As a result, young men learn prosocial skills which they might not have learned at home, like functioning in a disciplined environment and maintaining cooperative relationships (Elder 1987). Third, military service offers *training and educational possibilities* which can improve long-term prospects in the labor market and provide a way out of crime (Laub and Sampson 2004).

On the other hand, also consistent with a dynamic perspective, military service can actually have a negative influence on crime and criminality throughout the life course. First, in the military, soldiers learn to resolve conflicts by means of *aggression and the use of weapons* (Castle and Hensley 2002; Hakeem 1946). Conscripts may resort to the same methods when they return to civilian life. Second, the *stress of military actions* could lead to psychological and behavioral problems later in life—especially in the case of war experiences, where individuals face the prospect of serious injury or even death. Third, military service can *attenuate social bonds*, for example, by straining relationships with intimate partners and family members, promoting new friendships with antisocial individuals, or causing one to miss out on good job opportunities (Jongbloed 1996).

To summarize, static and dynamic theories are actually indistinguishable with respect to the impact of military service on criminal behavior while individuals are serving. Both theories can account for positive or negative changes in behavior that occur contemporaneously with active military service. However, the two theoretical perspectives differ greatly with respect to whether military service has an impact on long-term criminal behavior. Static theories would anticipate no long-term behavioral change, net of changes in the opportunity structure, because military service cannot influence an individual's latent criminal propensity. Dynamic theories, on the other hand, expect military service to actively change an individual's criminal propensity, for better or worse, and therefore to deflect military veterans onto an alternative criminal or non-criminal pathway that is well sustained throughout adulthood. Post-service criminality is therefore the focus of the present analysis, as it allows for a crucial theoretical test.

Earlier studies of military service and criminal behavior

Table 1 shows an overview of earlier studies that have focused on the relationship between military service and criminal behavior. A weakness of many of the early studies is their reliance on cross-sectional data, which insufficiently control for so-called "unobserved heterogeneity," or time-stable characteristics that can potentially confound correlations between military service and criminal behavior. Furthermore, in some studies, very selective groups are chosen as research subjects, making it difficult to determine whether a relationship between military service and criminal behavior exists on a population scale, and if so, how this relationship unfolds. More recent studies, on the other hand, frequently employ longitudinal data, although individuals tend to not be followed for long periods of time. Additionally, personal characteristics that form good indicators for individuals' criminal propensity are more often included in the analyses as control variables. For an extensive discussion of earlier studies, we refer interested readers to Allen (2000).

The results of this research tradition are mixed. Interestingly, a number of studies have found a great deal of continuity in criminal behavior. This is true not only for older studies (Hakeem 1946; Lunden 1951) but also for more recent ones using longitudinal data (Allen 2000; Bouffard 2003). Specifically, young men who exhibited delinquent behavior during their youth tend to continue their criminal lifestyle during and after their period of military service.

However, there are also studies that have shown military service to be followed by a reduction in criminal behavior. In particular, men who served during the Second World War and the Korean War appear to have undergone positive behavioral changes. Although young delinquents do not always behave well during service, the military might actually cause a decline in criminal behavior afterwards (Mattick 1960). Studies conducted by Sampson and Laub (1993, 1996; Laub and Sampson 2003, 2004) showed similar results. Sampson and Laub studied the life courses and criminal careers of 500 delinquents from early youth well into adulthood. Qualitative as well as quantitative analyses showed that military service had a suppressing effect on the development of criminal behavior. The training and educational possibilities during service were largely responsible for this effect. The same applies to serving

Table 1 Studies on the effects of military service on criminal behavior

Author	Year	Sample	<i>n</i> ^a	Service period	Measurement of criminal behavior
Cross-sectional studies					
Hakeem	1946	Boys who have been sent to prison after service for the first time	510	Second World War	Official and self-report data before and during service
Lunden	1951	Boys who have been imprisoned after service	156	First and Second World Wars	Official and self-report data during and after service
Mattick	1960	Boys who have been imprisoned before service and a group consisting of "normal" conscripts	924	Second World War	Official data during and after service
Resnick et al.	1989	Boys who served in the military and have war-related psychological problems	118	Vietnam War	Self-report data before, during and after service
Shaw et al.	1987	Boys who have served, and of whom some were imprisoned after service	61	Vietnam War	Official data after service
Yager et al.	1984	National sample of boys who were of age to go into military service during the Vietnam War	1,342	Vietnam War	Self-report data after service
Greenberg et al.	2007	Men in state and federal prisons in 1997 and 2000 census data on male veterans and nonveterans	1,075,202; 75,253,678	Vietnam War and voluntary period	Official data after service
Longitudinal studies					
Allen	2000	Three birth cohorts and a panel study consisting of boys who did and did not serve	155; 243; 565; 4,570	Vietnam War and voluntary period	Official data until the age of 35
Bouffard	2003	Two birth cohorts	565; 243	Vietnam War	Official data until the age of 30
Bouffard	2005	A panel study consisting of boys who did and did not serve	5406	Voluntary period	Self-report data in the year after service

Table 1 (continued)

Author	Year	Sample	<i>n</i> ^a	Service period	Measurement of criminal behavior
Bouffard and Laub	2004	Three birth cohorts and a panel study consisting of boys who did and did not serve	155; 243; 565; 4,570	Vietnam War and voluntary period	Official data until the age of 35
Laub and Sampson	2003	Delinquent boys originating from a disadvantaged background	52; 419	Second World War and Korean War	Official and self-report data until the age of 32 and 70
Laub and Sampson	2004	Delinquent boys originating from a disadvantaged background	52	Second World War and Korean War	Self-report data until the age of 70
Rand	1987	A birth cohort	106	Vietnam War	Official data until the age of 30
Rohlfis	2009	White and black American-born men born between 1940 and 1955	1,434	Vietnam War	Official data before, during and after service
Sampson and Laub	1993	Delinquent and non-delinquent boys originating from a disadvantaged background	590	Second World War and Korean War	Official and self-report data until the age of 32
Sampson and Laub	1996	Delinquent and non-delinquent boys originating from a disadvantaged background	590	Second World War and Korean War	Official and self-report data before and during service
Wright et al.	2005	Boys in the second year of high school	667	Vietnam War	Official and self-report data until the age of 30

^a In some cases, different sample sizes are mentioned because use is made of various data sources

overseas; this aspect of military service also brought about a decrease in deviant behavior during service (Sampson and Laub 1996).

During the Vietnam War, on the other hand, military service appears to have had a stimulating effect on criminal behavior. This applies particularly to the conscripts who experienced real war situations during their service period (Resnick et al. 1989; Shaw et al. 1987). For example, Yager et al. (1984) found that violent experiences during the Vietnam War caused various emotional and behavioral problems later in life. In addition, war experiences stimulated post-traumatic stress disorder (PTSD) that in turn was related to antisocial behavior following military service (see also Wilson and Zigelbaum 1983). The study of Rohlf's (2010) revealed that combat exposure increased the number of violent acts committed for black men, and found suggestive evidence for a similar relationship for white men and for certain types of crimes. Wright et al. (2005) found that extensive drug abuse was a crime-stimulating factor for many enlistees during the Vietnam War.

Recent longitudinal research showed that the effect of military service also differed between persons (Bouffard 2005). Although, in general, military service did not bring about any changes in criminal behavior, the effect turned out to be different for people in disadvantaged situations; specifically, ethnic minorities, individuals originating from lower social strata, and individuals with a delinquent history. For example, for African-American men, military service resulted in a decrease in criminal behavior, whereas in other groups an increase in criminal behavior was observed.

Like the theoretical approaches, the results of previous studies yield very different findings about the possible effects of military service on criminal behavior. In the end, clarity can only be obtained by conducting additional empirical studies using prospective, long-term data which explicitly take into account the circumstances under which military service was fulfilled. Although the military has been a salient institution in Dutch society and in the lives of many Dutch men, until now no study has been conducted which aimed to determine the effects of military service on later life outcomes. The aim of this study is to enlarge our knowledge about the influence of military service on crime by examining this life-course event in a context that has not previously been investigated.

Agenda for the current study

The primary aim of this study is to evaluate the claim that military service is a turning point in the criminal careers of a Dutch conviction cohort. We test the claim empirically by contrasting two different approaches to causal identification: a quasi-experiment and a natural experiment. The comparison of findings from multiple designs and methods has a long tradition in program evaluation. For example, results from non-experimental designs are often compared to those obtained from the randomized "gold standard." In one such kind of study, the researcher performs a meta-analysis of effect sizes from experimental and non-experimental studies, and determines what non-experimental design features yield mean effect sizes that are closest to experimental designs (Heinsman and Shadish 1996; Weisburd et al. 2001; Welsh et al. 2010). In another kind of study, the researcher utilizes data from a field experiment and merges it with large-scale, non-experimental datasets to conduct a search for the comparison group

and statistical method(s) that provide the best approximation to the experimental design (Heckman and Smith 1999; LaLonde 1986). The conclusions from these research traditions tend not to inspire optimism about the prospects for non-experimental designs and methods (see Weisburd 2010). For instance, Weisburd et al. (2001) conclude from their review of criminal justice interventions that there is generally an inverse relationship between the quality of research design (*vis-à-vis* internal validity) and the magnitude of the effect size under study.

Nonetheless, a set of practical guidelines emerges from this research tradition on how to optimize non-experimental designs for causal identification, and which guide the present inquiry. Heinsman and Shadish (1996) provide two, in particular, that they suggest contribute to a well-designed, non-experimental study. First, subjects should not be allowed to select themselves into different conditions. Second, differences between subjects on the outcome variable at “pretest” should be minimized. With these recommendations in mind, we utilize two approaches in this study, differing in one key respect—the composition of the group of non-military subjects who are selected to provide a “counterfactual” conviction probability. In the first case, all men in the CCLS who did not serve in the military, irrespective of their reason for not being drafted, are included in the comparison sample. In the second case, a subset of these men, who were exempt from military conscription for reasons that were plausibly exogenous, are retained in the comparison sample.

Military service in the Netherlands was compulsory during the period covered by this study. Men could be deemed ineligible for service for any number of reasons that can confound estimates of the military effect on crime, including intellectual deficiency, psychiatric instability, physical handicap, and even prior criminal behavior. However, men could also be deemed exempt from service by virtue of their birth year when there was an overabundance of young men, having older brothers who had already been drafted, or by lottery. Our data allow us to distinguish men who did not serve because they were ineligible from those who did not serve because they were exempt. In our analysis of the quasi-experiment, we contrast men who served in the military to all men who did not serve. In our analysis of the natural experiment, we contrast the men who served to the subset of men who were deemed exempt from service.

Because our data also contain criminal history information from age 12 onward, we are able to test for “balance” or “pretest equivalence” between men who served in the military and these two different groups of men who did not serve, prior to the age (18) at which all men were eligible for conscription. In other words, we are in a unique position to test whether the two groups being contrasted are truly equivalent in certain respects prior to the transition of interest, and thus the extent to which each non-military sample is a justifiable comparison for the military sample. This will potentially have implications for understanding the source of any discrepancy in findings about the military–crime association between the two approaches, and then provide guidance about which set of findings is more defensible under the circumstances.

Data

To gain more insight in the effects of military service, we used a supplement to the large-scale Criminal Career and Life-Course Study (Nieuwebeerta and Blokland

2003), known as the CCLS. The CCLS research subjects were selected by taking a 4% random sample of all cases of criminal offenses tried in the Netherlands in 1977, with over-sampling of serious offenses (e.g., murder, rape, drug offenses) and under-sampling of common offenses (e.g., drunk driving). This resulted in a total sample of 5,164 offenders. Abstracts from the General Documentation Files (GDF) of the Criminal Records Office were used to reconstruct the entire criminal careers of all these 5,164 persons before and after 1977: from the age of 12 (the minimum age of criminal responsibility in the Netherlands) until calendar year 2003 (the end of data collection). The GDF contain information on every criminal case registered by the police at the Public Prosecutor's Office. Although the GDF contain information on all offenses that have led to any type of judicial action, in this study information is used only on those criminal offenses followed by a conviction or a prosecutorial disposition due to policy reasons, thereby excluding non-criminal offenses (traffic offenses, for example) and cases that resulted in acquittal or a prosecutorial disposition due to technicalities. This means that only the offenses that most likely have been committed by the sample subjects are included in the study.

To measure life circumstances, the judicial data have been supplemented with data from the population registration records (GBA). This registration system contains information on marriage and fertility history as well as date of death for all Dutch inhabitants since 1938. For individuals in the sample who had died before 1994 (prior to electronic registration), personal record cards were retrieved from the Centre of Genealogy and Heraldry (CBG). Based on personal details from the 1977 offense, we were able to trace 89.4% of the original sample, leaving a total of 4,615 individuals in the sample (4,187 men and 428 women). Because the mean age of these individuals was about 27 years in 1977, these data reach far into adulthood for the larger part of the sample. For example, while the sample was followed until a mean age of 50.9 years (median = 49 years), the age of follow-up for almost 6% of the sample is 70 or older.¹

Military examination and military service

As part of this study, the CCLS has been expanded to include data on military careers, which have been collected from the Ministry of Defense. These supplementary data were collected for a subsample of the men in the original CCLS sample. First of all, data collection was limited to men who were born after 1940 ($n=3,515$). The reason for doing so is that insufficient information about the military examination and service record was available in the archives of the Ministry of Defense for men from earlier birth cohorts. Because of limited resources, a random subsample of these men was selected ($n=736$). As no information on military examination or military service could be located for 47 men, the resulting sample is comprised of 689 men for whom so-called B-cards (brother's service cards) and military files have been collected. This group can be considered representative of all men who were convicted in 1977 and were at that time between 12 years (minimum age of criminal responsibility) and

¹ In recent years, the CCLS has been used to study various topics, among them the development of individual criminal careers (Blokland et al. 2005; Petras et al. 2010), the impact of life course events—marriage, children, divorce—on criminal offending (Blokland and Nieuwbeerta 2005a, b), the impact of imprisonment on life outcomes (Apel et al. 2010), offenders' causes of death (Nieuwbeerta and Piquero 2008), and the criminal careers of female offenders (Block et al. 2010).

37 years of age (because of birth year 1940 or later). In the analyses below, professional soldiers are removed because their military experience differs considerably from men who completed compulsory military service, and because our interest is in post-military differences in criminal conviction. This leaves us with a total of 678 research subjects.

The test results and military files yield various kinds of information not only about the possible service period but also about personal characteristics of the subjects. Prior to the 1990s, military service in the Netherlands was compulsory (beginning in the 1990s, the Dutch adopted an all-volunteer military policy), and all men were expected to be tested before they were drafted into service. This examination, which usually took place at 18 years of age, consisted of three phases. The first phase was a medical examination, the results of which were summarized in the so-called ABOHZIS formula. This abbreviation is short for “algemene fysieke toestand” (general physical condition), “bovenste ledematen” (upper part of the body), “onderste ledematen” (lower part of the body), “horen” (hearing), “zien” (eyesight), “intelligentie” (intelligence), and “psychische stabiliteit” (psychological stability). The following scores could be obtained: 1 (very good), 2 (good), 3 (reasonable), 4 (bad), and 5 (very bad). Persons who scored a 5 were declared unfit for military service. The second phase of the examination consisted of a Morse test, and a number of written tests aiming to measure practical insight, intelligence, and mathematical and linguistic skills. The test scores that could be obtained ranged from 1 (very good) to 6 (very bad). The third phase of the examination consisted of a personal conversation with a selection officer. During this conversation, topics such as education, occupation, study plans, hobbies, and preferences for certain divisions of the armed forces were discussed. The outcome of the three examination phases was a rating of “eligible” or “(temporary) ineligible.” Those considered ineligible were not drafted into military service.

Men could also be excluded from service for reasons other than poor performance in the military examination. First, exclusion could occur when men had been unconditionally sentenced for a criminal offense 6 months or more before their examination. These men were considered to be too great a “company risk.” Second, men could be exempted from military service. Personal indispensability, being the breadwinner of a family, brother’s service (two older brothers already served), occupying a clerical office, working in a developing country, or exemption on grounds of conscience, could form possible grounds for this decision. Finally, large numbers of men were eliminated from the draft altogether at various points. The number of new conscripts was not only determined by the demand for new forces but also by the supply of young men. Due to demographic changes, the supply of eligible recruits—especially at the end of the twentieth century—was sometimes larger than necessary. In such years, conscripts were selected by drawing lots. In these periods, it was also easier to be exempted from service (Van der Meulen 1984). By the end of the 1970s, the number of young men 18 years of age was so large that the draft for the entire 1959 birth cohort was eliminated.

Methods

When examining the effects of life-course events such as military service, a major problem is that such life events do not arise at random. As described above, a criminal

past constitutes one reason to deny men the opportunity to serve in the military. Also, men with certain personal characteristics, for example deficient intelligence or psychological instability, have a higher risk of exclusion from military service. These characteristics strongly correlate with criminal behavior. This selection process ensures that men with a lower criminal propensity have a higher likelihood of serving in the military, making it more difficult to determine possible causal effects of the military. An observed difference in criminal behavior after service will be a combination of the difference of having served in the military (as far as the service has an effect) and differences in criminal propensity that already existed before that time.

The problem can be illustrated using the following cross-sectional model as a point of departure:

$$f(\text{Convict}_i) = \alpha_0 + \alpha_1 \text{Mil}_i + \alpha_2 X_i + e_i$$

This represents a generalized linear model (GLM) of the effect of having served in the military on criminal conviction (on the GLM, see McCullagh and Nelder 1989). In it, Convict_i is a measure of criminal conviction that can be modeled non-linearly, Mil_i is a dichotomous indicator for having served in the military, X_i represents a vector of characteristics thought to be correlated with military service and conviction and which may or may not be observed, and e_i is a residual with the usual properties.

An unavoidable inferential problem arises because military service is determined, in part, by individual choices, which themselves are partially determined by characteristics of the choice maker. The coefficient for the “military effect” in the above equation, α_1 , will yield an unbiased causal effect of service on conviction under only two conditions. First, the military effect is unbiased if all of the covariates summarized in X_i are “balanced” (i.e., equivalent) between those who serve in the military and those who do not, rendering it unnecessary to statistically control for any variable in X_i . This condition would be achieved if military service could be randomly assigned, as randomization achieves balance in expectation on all observed and unobserved variables by design. Unfortunately, assigning individuals randomly to an experience like military service is more easily achieved in theory than in practice.²

Second, in the absence of randomization, the military effect on conviction in the above equation is unbiased if (1) all the relevant confounders of the military-conviction association are observable and included in X_i , or else (2) pseudo-random variation in the population can be isolated that renders military service, for all intents and purposes, randomly assigned. The former represents a “quasi-experiment” whereas the latter represents a “natural experiment.” We employ both strategies in this study and describe them in more detail in the next two sections.

² It is not, however, impossible. We note that this has indeed been done before in the United States. Between the years 1970 and 1972, a draft lottery was implemented in which induction into the military was determined by a random number assigned by birth date. Men who did not qualify for a deferment and whose draft number was below the threshold were inducted into the military. The Vietnam-era draft lottery has been exploited by a number of analysts to estimate the effect of military service on such outcomes as mortality (Hearst et al. 1986), earnings (Angrist 1990), and arrest (Rohlfis 2010).

Panel models of the effect of military service on criminal conviction

We begin by taking advantage of the longitudinal nature of the CCLS data, which provides conviction histories from age 12 to 49 for the average research subject (the interquartile range of the follow-up age is 45–54). Using dates of entry into and exit from the military, we can then partition each subject's military experience into the periods before, during, and after his service in the following way:

$$f(\text{Convict}_{it}) = \beta_0 + \beta_1 \text{PreMil}_{it} + \beta_2 \text{InMil}_{it} + \beta_3 \text{PostMil}_{it} + \beta_4 X_{it} + u_i + e_{it}$$

where i indexes individual subjects and t indexes time periods. In this model, Convict_{it} represents criminal conviction as before; PreMil_{it} (pre-military), InMil_{it} (in military), and PostMil_{it} (post-military) are time-varying dichotomous indicators for military service period among those who served (contrasted with not serving)³; and X_{it} represents observed covariates used to control for potential confounding between military service and conviction. The military service dummies are contrasted with non-military men of the same age. The coefficient of primary interest is β_3 , quantifying the lasting influence of military service on criminal conviction, relative to having never served in the military, and net of the control variables. The model additionally adjusts for exposure or “street time,” referring to the proportion of each year that an individual is not incarcerated (on exposure adjustments, see Piquero et al. 2001).

With panel data, it is possible to partition the residual into the component which varies across subjects (u_i) and the component which varies within subjects (e_{it}). The error component u_i is included to capture all unobservables that are immutable, that is, stable over time. This refers to so-called “unobserved heterogeneity.” An assumption that u_i varies randomly in the population gives rise to the random-effects model (also known as a random-intercept model), while an assumption that u_i has no well-defined distribution but can be conditioned out of the model gives rise to the fixed-effects model. We estimate both sets of models but, on the basis of specification testing which is explained in the [Appendix](#), we only present the results from the random-effects model (fixed-effects results are, of course, available upon request).

To estimate the aforementioned models, a person-year file was constructed with records containing information on each subject in each year. For every individual, the records begin at age 12—the minimum age of criminal responsibility in the Netherlands—and ends in the year 2003 (the end of data collection) or in the year prior to death (in this way, we account for “false desistance” caused by mortality). The fully constructed data file contains information on 25,104 person-years for 678 subjects, 174 (26%) of whom served in the military and 504 of whom did not serve. Because of missing information on the date of entry into the military for 10 individuals who served, our estimation sample is actually based on 24,664 person-years for 668 subjects.

In the model described above, we compare individuals who served in the military to “everyone else,” meaning all other individuals in the sample who

³ For instance, PreMil_{it} is coded “1” in all years during which subjects who will serve in the military are not yet eligible to serve, and “0” otherwise. Similarly, InMil_{it} is coded “1” during all in-service years, and PostMil_{it} is coded “1” in all years following the completion of their military obligation.

did not serve in the military. The results provided by these models can be interpreted as the causal effect of military service if selection into the military is only on the basis of time-invariant unobservables (which are swept out of the fixed-effects model) or is otherwise fully captured by the control variables included in the model. The control variables that we have available in the data include both time-invariant and time-varying observables. The time-invariant predictors include year of birth and a composite score from the military exam (the mean score obtained on the following tests: intelligence, practical insight, mathematical skills, language skills, and general knowledge). The time-varying predictors include age (modeled as a third-order polynomial) and dummy indicators for marital and parental status (coded “1” in each year that subjects are married or have a child co-residing, and “0” in all other years). We also include an exhaustive set of time-varying controls for criminal history. These include a dummy indicator for having been convicted in the previous year, the total number of convictions accumulated as of two years ago, a dummy indicator for having been incarcerated in the previous year, and the total calendar time in prison accumulated as of two years ago. The purpose of the criminal history measures is to control as rigorously as possible for time-varying selection effects.

Exemption from military service as a natural experiment

Our second empirical strategy relies on the same longitudinal model of criminal conviction. However, this approach fundamentally differs in the selection of a comparison group of non-military subjects. Rather than compare subjects who serve in the military with all other non-military subjects, we take advantage of the fact that 170 (34%) of the 504 non-military subjects were exempt from military conscription. These individuals were eliminated by lottery, by virtue of their year of birth (recall from earlier discussion that the entire 1959 birth cohort was eliminated from service), or because they were otherwise exempted from service (e.g., because older siblings were already drafted). The most important thing to note is that these men were not excluded from the draft because of physical, intellectual, or psychological deficits or any other exclusion criteria (e.g., criminal record). The subsample of eliminated and exempted non-military subjects therefore includes a large share of subjects who *would have served in the military*, but for historical circumstances, birth order, or luck of the draw.

This situation constitutes a compelling “natural experiment” for the study of the effect of military service on conviction. Meyer (1995) observes that “natural experiments are studies in which there is a transparent exogenous source of variation in the explanatory variables that determine the treatment assignment” (p. 151). In this study, “treatment assignment” refers to whether an individual is drafted into the military or not. A distinct advantage of this type of design is intimate knowledge about the *assignment mechanism*, or the process by which individuals are selected into military service. The natural experiment in this case represents a setting in which the policies of the Dutch government unwittingly (for its part) introduced variation in military service that was arguably random, in the sense that individuals were exempted from military service for reasons that were completely independent of the ongoing

process that determines criminal conviction.⁴ We argue that this type of variation provides a unique opportunity to test the claim that military service has a causal relationship with criminal behavior, and to evaluate the sensitivity of more common quasi-experimental approaches.⁵

For this set of analyses, the fully constructed data file contains the conviction records of 12,837 person-years for 344 subjects, 174 (51%) of whom served in the military and 170 of whom did not serve. However, missing information on the date of entry into the military for 10 individuals who served restricts the analysis to 12,397 person-years for 334 subjects.

Results

Before we turn to our empirical models, we begin with descriptive statistics, provided in Table 2. In total, 174 of the 678 research subjects (25.7%) passed the military examination and went to service, while 504 men (74.3%) did not serve. A large share of the men who did not go into the military were declared unfit: 20.2% because of intellectual deficiency, 19.2% because of psychiatric instability, and 15.5% because of physical handicap. Also, a criminal past was an important reason why some men did not serve—7.7% of the non-military subjects were not drafted into military service for this reason. Another common reason why subjects did not serve—37.3% of the non-military subjects—was exemption (e.g., on the grounds that two older brothers had already served), elimination by lottery, or elimination by birth year (e.g., the 1959 birth cohort). For economy, we will henceforth refer to this group of men as “exempt from service.”

For men who did serve in the military, information not shown in Table 2 reveals that, for the overwhelming majority (86.0%), age of entry was between 19 and 21 years, with a mean length of service (including furlough) of 12 months. These men served between 1959 and 1985. The vast majority were drafted into the land forces (about 90%) and mostly served in the infantry and artillery; respectively about 30 and 15% of the total drafted. On several occasions, conscripts were promoted during their period of service, although in most cases they maintained a lower rank. The highest promotion in most cases was “first-class soldier,” one

⁴ An anonymous reviewer was critical of the fact that we did not take advantage of this natural experiment to provide instrumental variable (IV) estimates of the military-crime association, using exemption as an exogenous instrument. Unfortunately, this was not possible in the present study. To perform an IV analysis, we would need a measure of exemption *eligibility* in order to model the probability of actually being exempt from the draft. For example, we would need to know, for all subjects (those who did not serve as well as those who did) whether they had older brothers who were drafted and which made them eligible for an exemption. We only know this information for subjects who were actually exempted (because we have information on the reason for exemption).

⁵ Granted, some of these men would undoubtedly have been considered ineligible for the draft had they not been exempted outright. There is thus likely to be some “contamination” of our proposed natural experiment. This can be observed in the mildly worse military test scores of the exempt men relative to men who served, at least those for whom test scores were available (see Table 2). Yet they are by far the most comparable when contrasted with men who did not serve in the military for other reasons. Therefore, we argue that any remaining endogeneity in military service is far smaller when the exempt men constitute the comparison sample relative to any other group of non-military men.

Table 2 Characteristics of the sample

Variables (range)	Total sample		Non-military subsample				Exempt from service
	Military subsample	All non-military	Criminal record	Intellectual deficiency	Psychiatric instability	Physical handicap	
<i>n</i>	678	504	39	102	97	78	170
% of total sample	100.0%	74.3%	5.8%	15.0%	14.3%	11.5%	25.1%
% of non-military subsample	–	100.0%	7.7%	20.2%	19.2%	15.5%	33.7%
Year of birth	1953.6	1952.5	1955.6***	1952.8	1953.7	1952.3*	1955.2***
Criminal history, 12–17							
Convicted (0–1)	44.0%	34.5%	61.5%***	49.0%***	55.7%***	46.2%*	39.4%
Total number of convictions (0–32)	1.2	0.8	3.6***	1.3*	1.6***	0.9	0.9
Military test scores							
Valid test score (0–1)	45.1%	69.0%	41.0%***	41.2%***	27.8%***	43.6%***	35.6%***
Intelligence (1–6)	3.9	3.3	4.0***	5.6***	4.0***	3.4	3.9***
Practical insight (1–6)	4.1	3.6	4.7***	5.2***	4.5***	3.9*	4.0**
Mathematical skills (1–6)	4.4	4.0	4.4***	5.4***	4.6***	4.5***	4.4***
Language skills (1–6)	4.2	3.8	4.4***	5.5***	4.0†	3.8	4.4***
General knowledge (1–6)	4.3	3.9	4.4***	5.4***	4.6***	3.9	4.2**
Total test score (1–6)	4.2	3.7	4.4***	5.7***	4.5***	3.9*	4.2***

Means of binary variables are shown as percentages. Higher values on the military test scores indicate less successful performance. Descriptors are not shown for the 18 men who did not serve in the military for reasons that are unknown (2.7% of the total sample; 3.6% of the non-military subsample), although they are still included under the “All non-military” column. Subjects included under the “Exempt from service” column were eliminated from the draft by lottery or by virtue of their year of birth, or were otherwise exempt because their older sibling(s) served in the military. Significance stars are from *t* tests, and contrast each group with the military subsample

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

rank higher than the rank assigned to a beginning soldier. Only a small number of individuals obtained the rank of sergeant. Most were dismissed from service because they fulfilled their military obligation and received the equivalent of an “honorable discharge.”

Table 2 also provides some background characteristics of the 678 men in the sample. With a mean 1.2 total convictions apiece prior to age 18, collectively these men accumulated a total of about 804 convictions (1.2×678). Not shown in the table is that 1 in 9 (11.3%, or 91 of 804) of these convictions is for a violent offense. The 504 men who did not serve in the military have a criminal history that is clearly more extensive than the 174 men who did serve. The likelihood of conviction prior to age 18 is 47.2% compared to 34.5%, respectively. The likelihood of conviction for a violent offense is also lower for men who went into the service (8.6 vs. 11.3%). Important differences are also observed *within* the group of men that did not serve in the military. Men who were excluded from the draft because of a criminal record accumulated the largest share of convictions before age 18 (3.6, on average). In fact, while these men constitute just 7.7% of the non-military sample, they account for 21.4% of all of the convictions among non-military men prior to age 18. However, men who were excluded because of psychological instability or intellectual deficiency were also criminally active during this period, respectively garnering a mean number of 1.6 and 1.3 convictions during their youth. Men who were exempt from service are much more comparable to those who served in the military. Before the age of 18, they were convicted 0.9 times, on average, which is not significantly different from the military men.

Other notable differences are observed when we examine the test scores received during the military eligibility examination. Men who served in the military scored significantly lower on these tests, which is to say that they scored significantly better than men who did not serve. It should be noted, however, that for a little over half of the subjects (54.9%), information on the test score was unavailable.⁶ When we consider only the men for whom test scores are non-missing, the group who served in the military has a mean total score of 3.7, while the group who did not serve has a mean total score of 4.7. In other words, men who achieved lower (i.e., better) scores on the military examination were more likely to be drafted into the military. For the various separate tests (practical insight, intelligence, mathematics and language skills, and general knowledge), a similar pattern arises. The men who were exempted from military service have a mean total score of 4.2 which, although superior to the average for all other non-military subjects (and significantly so), still differs significantly from men who served in the military.⁷

⁶ In the empirical models, we impute a value of “0” for men who are missing their military test score. We then construct a dummy variable to flag these imputed cases, and include the missing data dummy as an additional control variable in the model. We might also impute the sample mean or median, but we note that any value chosen is completely arbitrary. Because the imputed value is a constant, it will render the same interpretation of the coefficient estimated for individuals with valid military test scores.

⁷ It will therefore be important to control for the military test score in the model that compares men who served in the military to men who were exempt from service. Additionally, the fixed-effects model is well suited to eliminating any unobserved, time-stable differences between the two groups that might create non-equivalence.

In Fig. 1, we provide the age distribution of the probability of criminal conviction for three groups of men: all men who served in the military ($n=174$), all men who did not serve in the military ($n=504$), and the subset of non-military men who were exempt from military service ($n=170$). (Note that the age distribution of conviction frequency has an identical pattern as the age distribution of conviction probability.) Until their mid-50s, men who did not serve in the military have, as a group, a far higher risk of conviction compared to men who served. In fact, while it is not shown in the figure, 95% confidence intervals show that non-military men have a conviction probability that is significantly larger than military men until age 45, after which the confidence intervals overlap. By way of example, the peak conviction likelihood for non-military men is 0.26 at age 27, while the same figure for military men is 0.16 at age 28. This provides *ex ante* evidence that selection into military service is determined, at least in part, by the same underlying characteristics that determine criminal conviction. In words, men who serve in the military appear to be at much lower risk of criminal behavior well before they actually serve.

When we consider the men who were exempt from military service, there are far fewer differences in conviction risk compared to men who served. The peak conviction likelihood for exempt, non-military men is 0.19 at age 26. Until their early 30s, these men have a slightly higher conviction probability than men who served in the military but, from their mid-30s on, a slightly lower conviction probability. However, 95% error bars around the age distributions (which are not

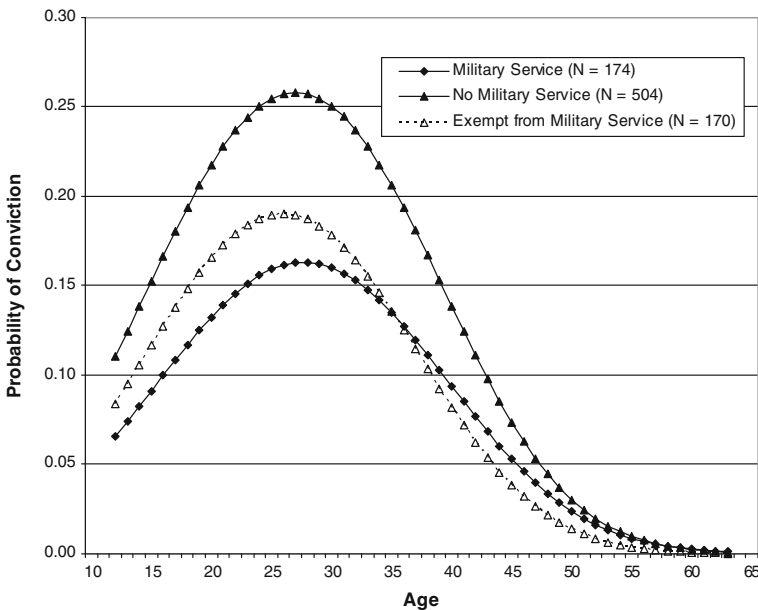


Fig. 1 Age distribution of criminal conviction probabilities, by military service experience. Individuals who are exempt from military service are a subset of the individuals who did not serve in the military. The age-crime curves are estimated using a separate random-effects logit model for each group with a quadratic age function

shown in the figure) show that the differences in conviction risk are never statistically significant at any age.^{8,9}

The impact of military service on convictions

Table 3 summarizes the results from two random-effects logit models. The first model retains all 504 non-military subjects as the comparison group (Model A), while the second model uses only the 170 non-military subjects who were exempt from service as the comparison group (Model B). We tabulate the results from random-effects models rather than fixed-effects models based on the coefficient from a specification test known as the Durbin–Wu–Hausman test for the key military service indicator (post-service period), which is shown in the bottom row of the table (refer to the [Appendix](#)).

Before turning to the military service indicators, we first summarize the remaining coefficients, noting that Models A and B are virtually identical with respect to the magnitude and significance of the control variables. All the age coefficients are statistically significant and produce the well-known age–crime curve. Marital status, a time-varying covariate, is inversely correlated with conviction. This means that periods in which men are married correspond with lower conviction risk, net of age, compared to periods in which men are not married. Parenthood, on the other hand, is positively and significantly correlated with conviction. This may support the idea that men commit crimes as a way to provide for their children (Blokland and Nieuwebeerta 2005a, b). The military examination score, a time-invariant covariate represented here as the mean score, is positively correlated with the probability of conviction. Because higher values on this score indicate lower performance on the military examinations, this suggests that men who perform poorly in the examination have a significantly higher risk of conviction. A subject’s criminal history is also strongly correlated with conviction. Specifically, men who were convicted or incarcerated in the previous year, and have more accumulated convictions or prison time (Model A only) as of two years ago, are at far higher risk of being convicted in the current year.

We next turn to the military service indicators. In Model A, the indicator for the pre-service period replicates the finding from Fig. 1, which shows that men who will serve in the military have lower conviction risk *prior to their service* than men of the same age who will not serve in the military. Specifically, at each age prior to serving, men who will serve in the military have an odds of conviction that is 33.6% lower than all other non-military men ($e^{-.409} - 1 = -.336$). The risk of conviction for military men remains unchanged *during their service*. Most relevant for our purposes, the indicator for the post-service period is also negative and significant, and this indicates

⁸ Based on Fig. 1, we can anticipate one potential flaw of the quasi-experimental method that compares military men to all non-military men. This approach assumes that the two groups follow the same age-graded pathway prior to the life event under study. This is clearly not the case, as the age-conviction curve is steeper during the teenage years for non-military men. Thus, the “military service effect” estimated in the panel model is likely to be confounded with other age-related changes that determine conviction risk and that are unobserved (this is known as “dynamic selection”). Note that this is true irrespective of whether a random- or fixed-effects model is chosen.

⁹ Note that for all three distinguished groups the peak age is older than the peak of the well-known age-crime curve (Piquero et al. 2003). This is because the CCLS data do not measure criminal behavior in general, but only convictions.

Table 3 Random-effects logit models of the impact of military service on conviction

	Model A Military + all non-military <i>b</i> (SE)	Model B Military + exempt from service <i>b</i> (SE)
Age	0.865 (.040)***	0.752 (.062)***
Age squared	-0.026 (.001)***	-0.023 (.002)***
Age cubed	0.000 (.000)***	0.000 (.000)***
Year of birth	-0.012 (.008)	-0.000 (.012)
Married	-0.629 (.059)***	-0.638 (.086)***
Child	0.144 (.060)*	0.199 (.091)*
Mean military test score	0.281 (.050)***	0.299 (.066)***
Missing test Score	1.272 (.239)***	1.237 (.313)***
Convicted at <i>t</i> -1	0.727 (.047)***	0.645 (.074)***
Total no. convictions as of <i>t</i> -2	0.013 (.003)***	0.015 (.007)*
Incarcerated at <i>t</i> -1	0.397 (.065)***	0.464 (.108)***
Total time incarcerated as of <i>t</i> -2	0.074 (.027)**	0.041 (.062)
Pre-service period	-0.409 (.131)**	-0.219 (.148)
In-service period	-0.082 (.171)	0.202 (.178)
Post-service period	-0.249 (.092)**	-0.016 (.106)
<i>n</i>	668	334
Total observations	24,664	12,397
Durbin-Wu-Hausman test	-0.431	-1.589

The reference group for the three military service dummies is the non-military sample at the same age. The models adjust for differential exposure due to incarceration in the current period. The Durbin-Wu-Hausman (DWH) specification test is a *z* test for the post-service period coefficient. A significant DWH test means that the fixed-effects model is preferred on consistency grounds, whereas a non-significant DWH test means that the random-effects model is preferred on efficiency grounds. See the [Appendix](#) for details

* $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

that men who serve in the military have a lower risk of conviction *after their service* compared to men who do not serve. In fact, military men have an odds of conviction that is 22.0% lower at each age in their post-service period than men who did not serve ($e^{-.249} - 1 = -.220$). The findings therefore show that military service does indeed lead to a lower likelihood of criminal conviction.

In Model B, we restrict the non-military subjects to those who were exempt from the draft for reasons that were essentially random, for example, by lottery or year of birth. There are two key findings from this model. First, the coefficient for the pre-service period remains negative but is no longer statistically significant. To be precise, soldiers-to-be have a 19.7% lower odds of conviction prior to being drafted than exempt non-military men ($p = .138$). This conforms to what we observed in Fig. 1, in which exempt non-military men have a slightly higher conviction risk that does not achieve statistical significance. Second, and most interesting, is the fact that the coefficient conforming to the post-service period is no longer statistically significant, nor is it remotely distinguishable from zero in a substantive sense. Our

conclusion from this model is thus that military service exerts no discernible impact on criminal conviction after the service period.

The empirical results thus point to two different conclusions regarding the impact of military service on conviction. On the one hand, a traditional quasi-experimental approach (Model A), comparing military men to all non-military men, suggests that military service is quite beneficial for high-risk men and helps promote desistance from criminal behavior. On the other hand, a natural experimental approach (Model B), comparing military men to non-military men who most likely would have served were they not exempt from the draft, suggests that the “military service effect” is nothing more than an artifact of a conscription process that tends to draft lower-risk men into the service.

Follow-up analyses

We estimated the same models using convictions for violent offenses as the dependent variable. While these results are not shown, the conclusions are generally unchanged from the previous table. In this case, military service significantly reduces convictions for violence when all non-military subjects are included as the comparison group. On the other hand, there is no relationship between military service and violence when only the exempt, non-military subjects are included as the comparison group.

We also tested for an age-graded relationship between military service and criminal conviction, in order to ascertain whether military service has short- or long-term effects on conviction, and whether the effects grow or decay with the passage of time. We estimated the same random-effects logit models, but this time we only estimated the models up to a certain age (in 5-year intervals, e.g., 25, 30, 35, 40, and so on). There was no clear age-graded pattern in the impact of military service. When all non-military subjects constitute the comparison group, the coefficients suggest that the strongest impact is felt in the first several years following discharge, but while the impact modestly declines in magnitude, it nevertheless stabilizes and persists well into adulthood. When only the exempt, non-military subjects comprise the comparison group, there is no evidence that military service has any beneficial impact on the likelihood of criminal conviction, in either the short run or long run.

We also eliminated the “criterion conviction” from the analysis, referring to the conviction that brought each subject into the CCLS sampling frame. Because of the way that the CCLS data were collected, all subjects are convicted at least once during their lives (for most, this conviction was in either 1976 or 1977). Because the military examination process tends to recruit lower-risk men, subjects who do not have any criminal convictions before they serve in the military must, by construction, have at least one conviction during or after their service period. This could distort estimated military service effects on criminality, specifically by watering down any beneficial impacts. To avoid this, we excluded the criterion conviction altogether. The results were unchanged.

In an additional set of follow-up analyses, at the urging of an anonymous reviewer, we evaluated the sensitivity of the results to the composition of the exempt, non-military men. For example, we limited our attention to men who were born in 1959, men who had older brothers who had been drafted, and men who were eliminated

from the draft by lottery. In all instances, the results did not substantially differ by the reason for exemption.

In a final empirical model, we allowed the impact of military service to vary by the subject's test score on the military examination. This allows us to test if the effect of military service differs between groups of offenders with different personal characteristics. Based on earlier results, we limit the comparison sample of non-military subjects to those who were exempt from the draft. Although we do not show the full results in a table, we provide a figure that highlights the relevant findings. In Fig. 2, we plot the relationship between the mean score on the military test scores and the probability of criminal conviction, and do so separately by a subject's military experience. One caveat to mention is that these results can only be based on subjects with valid data on the military test score, which represents just over half of the sample of military and exempt non-military subjects. The results should therefore be interpreted with due caution to the potential selectivity of individuals with valid military test scores. We interpret the findings as strictly illustrative rather than conclusive.

Figure 2 shows that, for both groups, there is a positive (and statistically significant) relationship between an individual's mean test score and the likelihood of conviction. Because higher scores on the military examination indicate poorer overall performance, this suggests that individuals who are less fit to serve in the military are at higher risk of criminal conviction, irrespective of whether or not they are drafted. Yet the relationship between the military examination score and criminal conviction is much more pronounced among subjects who were actually drafted into the military. In the empirical model, the coefficient for the interaction is positive and statistically significant ($p < .001$). This seems to suggest that, among individuals who are least fit

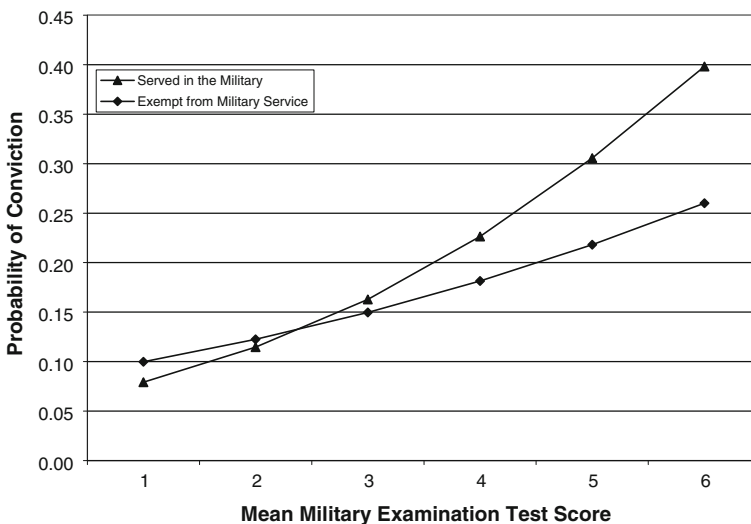


Fig. 2 Effect of military test scores on the probability of conviction, by military service status. $n=334$; Total observations=12,397. The estimates are derived from random-effects logit models of conviction, as in Model B of Table 3, and include an interaction term between military service and the mean score on the military examination. This interaction is statistically significant. The estimates shown are predicted probabilities for a hypothetical subject who is 25 years of age, born in 1950, unmarried with no children, with no criminal history, and in the post-service period (for those who served in the military). Higher values on the military test scores indicate less successful overall performances

to serve in the military—because of intellectual deficits, mathematical or language difficulty, etc.—military service is particularly strongly correlated with later conviction. Apparently, military service worsens criminal behavior for some veterans in our sample.

Discussion

Our concern in this study has been to determine whether service in the military has any causal effects on post-service criminal behavior. We made use of data of the Criminal Career and Life-Course Study (CCLS), a longitudinal study of a Dutch conviction cohort, which allowed us to prospectively study the relationship between military service and criminal conviction and to do so with a long-term follow-up period. Our strategy involved the use of two distinct research designs. The first design was a quasi-experiment, in which we contrasted men who served in the military to all other men in the CCLS who did not serve, and attempted to control for observables and unobservables using traditional panel models (e.g., random- and fixed-effects models). The second design was a natural experiment, in which we contrasted men who served in the military to a group of men who, in all likelihood, would have served in the military but for historical circumstances, birth order, or luck of the draw.

One key finding from this study is that the conclusion about the impact of military service on criminal conviction appears to depend on one key factor: the selection of individuals that comprise the non-military comparison group. When we compare men who served in the military to all other men who did not serve, we are consistently led to believe that the military experience has benefits with respect to criminal conviction that reverberate well throughout adulthood. Thus, military service would appear to be a very positive turning point in the lives of this sample of high-risk Dutch men by deflecting them onto a more conventional pathway compared to their non-military counterparts. Yet, when we compare men who served in the military to men who most likely would have served in the military (were they not exempt from the draft), we are led to believe that military experience has no relationship with criminal conviction. Among high-risk Dutch men, therefore, military service neither exacerbates nor ameliorates their criminality. Moreover, the differences between these two approaches are quite robust, and withstand a variety of different sensitivity analyses and model specifications.

Invariably, the question arises about which of these two sets of results is the most persuasive. We are inclined to place more weight on the findings from the natural experiment as opposed to the quasi-experiment, for two primary reasons (see Heinsman and Shadish 1996). First and foremost, the natural experiment provides greater assurance that men who avoided military service did so for reasons that were plausibly exogenous, or at least for reasons that were less endogenous than the quasi-experiment. The key consideration is that their avoidance of military conscription was not due to individual choice, but was instead due to policies of the Dutch government that selected them out of the pool of eligible military men. A convincing case can be made that a large share of these men would in fact have served in the military if not for these policies. Second, the natural experiment is far superior in contrasting groups which are observationally comparable with respect to criminal

conviction prior to the transition of interest. While not equivalent in all respects to men who served in the military (e.g., the military test scores), the men who were exempt from military service are indistinguishable from the military men in their criminal history prior to the age when the latter group began their military training (see Table 2 and Fig. 1). Relative to all other men who did not serve, the group of exempt, non-military men give us greater assurance in the “all else equal” assumption that non-experimental research designs require for causal identification.¹⁰ Our conclusion, therefore, is that the overall effect of military service on post-service convictions is a selection artifact. This harmonizes with static theories concerning the impact of life events on criminal behavior, and, at first glance, would seem to contradict dynamic theories.

A second key finding from this study, however, is actually supportive of dynamic theories and would seem to contradict static theories. When we consider individuals with valid data on their military test scores, we find that military service tends to exacerbate criminal behavior in the long run for individuals who performed worst on the military tests. Although this finding is and must remain speculative because of missing data, this implies an interaction between personal characteristics and life events, in this case, between “military fitness” and military service. Individuals who are the least fit to serve in the military experience long-term increases in criminal behavior as a result of their service, compared to similarly less fit individuals who were exempt from service. It could, for example, be the case that these individuals are more receptive to the violent army culture, and the use of aggression and weapons to resolve conflicts. Interestingly, then, the only evidence for a “military service effect” on criminal behavior in this study is negative—under no circumstances does military service appear to be beneficial with respect to long-term criminal behavior.

We wish to qualify our conclusions by pointing out that the data used in this study relate to military service in a specific context: the Netherlands. Earlier studies of the effects of military service on criminal behavior have focused exclusively on the United States. The experience of Dutch conscripts might differ from American conscripts in a number of ways. First, most Dutch men did not experience war situations during their service, and consequently did not have to cope with the stress accompanying military actions. Second, in contrast to the U.S., Dutch veterans did not have access to additional educational and health insurance benefits. Therefore, they might not have benefited from military service in the same way as American veterans benefited from such programs as the G.I. Bill. Third, although the duration of Dutch service was about 24 months at the beginning of the twentieth century, it decreased to 6 months in the 1990s when the draft came to an end. A short service period might limit the potential for military service to redirect an individual’s crime trajectory. That

¹⁰ As an aside, this analysis also demonstrates that fixed-effects models, in and of themselves, are not necessarily the panacea for selection bias that they are often touted to be. While these models are robust to unobservables that do not change over time, they are biased by unobservables that do change over time. Our long-term panel data allowed us to test the oft-overlooked assumption that the two “treatment groups” being contrasted are following the same (specifically, a parallel) criminal pathway prior to the “intervention” of interest. We found that military men and all other non-military men follow different pathways, whereas military men and exempt, non-military men do indeed follow parallel pathways. On its face, then, exemption from military service constitutes a more appropriate comparison group, as the groups are “balanced” on their criminal history prior to military eligibility.

being said, we believe it is notable that, in our quasi-experimental results, we still observe a beneficial effect of military service on crime, as a number of other studies have observed (Mattick 1960; Sampson and Laub 1993, 1996). This is in spite of the very different cultural and military context of the Netherlands.

Additionally, while the CCLS is unusually rich with respect to allowing reconstruction of long-term criminal histories, the reliance on information from official registries limits our ability to control for other time-varying influences that can confound the military–crime association. It is therefore unknown whether our model successfully balances military veterans and exempt civilians on a variety of other pre-18 characteristics, such as other measures of the social bond (e.g., employment) and peer affiliation. Furthermore, because we study official measures of criminality (criminal conviction), we inevitably underestimate total criminal activity. If the degree of underestimation differs systematically for individuals who serve in the military compared to those who do not, biases will plague our findings. On the other hand, it should be noted that the use of official data also has an important advantage, by allowing us to avoid the problems of social desirability and faulty memory, as would have been the case with self-report data.

Our findings also apply to Dutch men who are officially convicted of a crime at some point in their lives, and not necessarily to all Dutch men. The sampling design ensured that the men in this study were convicted in the late 1970s, at least. It is therefore advisable to interpret the estimates as the impact of military service on the long-term, official criminal behavior of high-risk Dutch men, and not to the general population of Dutch males. Relatedly, as pointed out helpfully by an anonymous reviewer, the findings reported here do not indicate whether (or how) military service prevents the initiation of a criminal career—specifically, does military service prevent the onset of criminality? Because all the men in the CCLS initiate a criminal career at some point in their lives, the analysis is only informative about whether military service can shorten the criminal careers of such men—specifically, does military service hasten the desistance process? Although our results suggest that the answer to this latter question is “no,” this analysis cannot rule out an answer of “yes” to the former question.

Finally, we unfortunately do not have a sufficient number of cases to study distinct historical periods of military service, or to evaluate the sensitivity of our estimates to the changing conditions of military service during the period spanned by our data. For example, prior studies have shown that service during the Second World War and the Korean War produce beneficial outcomes compared to service during the Vietnam War (Wright et al. 2005). Nor do the findings in this study relate to contemporary military service, either in the Netherlands or in other comparable Western countries. Recall that the sample studied here was drafted into the Dutch military during an era in which such service was compulsory. The degree to which the current findings generalize to voluntary service is therefore unknown.

Conclusion

Notwithstanding the foregoing limitations, we regard the current study as an important step in the study of the causal impact of military service on long-term

criminal behavior. While the findings from the natural experiment indicate no discernible causal effect on crime for the average Dutch military veteran included in this study, there is suggestive evidence that veterans who were the least fit to serve in the military experienced long-term escalation in their criminal activity due to their service. Untangling the underlying mechanism for this effect, to the extent that it is real rather than apparent, remains a task for future research.

This study also has implications for the design of other non-experimental studies, as the divergence of findings from the two distinct designs is instructive. In the absence of experimental randomization, causal identification in a study such as ours hinges on the ability to ensure that “all else is equal” when individuals who serve in the military are contrasted to individuals who do not serve—that is, to show that the groups are “balanced.” Were we to conduct a typical quasi-experimental study relying on panel methods alone, we would be led to conclude that military experience is a long-lasting turning point in one’s criminal career. Two features of our data allow us to test the plausibility of this “all else equal” assumption of the quasi-experimental approach. First, we have data on criminal behavior spanning several years prior to one’s eligibility to serve in the military. These data, in combination with the military examination scores, unambiguously show that individuals who do not serve in the military differ, on average, from military veterans in a quite pronounced way. Furthermore, these differences are not strictly time-invariant, meaning they cannot be overcome by sweeping them out of the model using a fixed-effects transformation.

Second, our data include a number of instances in which individuals did not serve in the military for reasons which, we argue, are independent of the ongoing processes that underlie criminal behavior. Military service in the Netherlands was compulsory during the period covered by this study. Some men avoided conscription because they were ineligible to serve (due to intellectual deficiency, psychiatric instability, physical handicap, and prior criminal behavior), whereas other men did not serve because they were exempt. The latter contain a large share of men who would have served in the military but for historical circumstances, birth order, or luck of the draw. Comparisons between military veterans (prior to their conscription) and the men who were exempt from service show unquestionably superior balance, at least on the measures which we have at our disposal, which includes early criminal behavior. This gives us greater confidence in the findings from this “natural experiment,” which yields no evidence of reductions in criminal behavior for the average military veteran compared to the average exempt civilian.

Our findings therefore seem to confirm Weisburd et al.’s (2001) and Welsh et al.’s (2010) observation that more internally valid designs—that is, designs which achieve better balance or equivalence between the two groups being contrasted—tend to produce more conservative “treatment effects” in settings that are of interest to criminologists. Our study demonstrates this to be true for the impact of military service on criminal behavior among Dutch men who are convicted at some point in their lives. Future inquiry concerning this and related questions would benefit from comparisons of findings from different research designs and statistical models.

Open Access This article is distributed under the terms of the Creative Commons Attribution License which permits any use, distribution, and reproduction in any medium, provided the original author(s) and the source are credited.

Appendix

Details on the Durbin–Wu–Hausman Specification Test

As a point of departure, suppose that we estimate the following panel model, simplified from the substantive model used in the analysis for the purpose of illustration:

$$f(\text{Convict}_{it}) = \alpha + \beta \text{Mil}_{it} + \gamma X_{it} + u_i + e_{it}$$

in which the key coefficient of interest is β , quantifying the impact of military service on criminal conviction. Random- and fixed-effects models make qualitatively different assumptions about the correlation between the unobserved, individual-specific error component of this model, u_i , and the regressors included in the model, including military service. This unobserved error component is assumed to capture variables that are time stable. In the random-effects model, it must be assumed that u_i is uncorrelated with military service. By way of example, it must be assumed that some trait such as intelligence or ability, which is not believed to vary over time, is not correlated with whether or not an individual serves in the military. Violation of this assumption renders the random-effects estimate of β biased and inconsistent. On the other hand, the fixed-effects model relaxes this assumption and treats any such time-stable trait as a nuisance that can be swept out of the model by a transformation that, effectively, gives each subject his own constant (when the model is linear, that is).

It would thus seem that a fixed-effects model is always preferred under these circumstances, because one can never be certain that time-invariant unobservables are truly uncorrelated with military service. While appealing, however, there is an important tradeoff between random-effects (RE) and fixed-effects (FE) models that is often overlooked. Although it is true that FE is consistent in the presence of correlation between the unobserved component u_i and military service, it is also inefficient compared to RE. It can be so inefficient, in fact, that it increases the risk of type II errors of statistical inference. Fortunately, various specification tests can adjudicate between RE and FE. One such test, known as a Durbin–Wu–Hausman (DWH) test (or more parsimoniously, the Hausman test), formalizes the tradeoff between efficiency and consistency (Durbin 1954; Wu 1973; Hausman 1978). It is represented as follows:

$$DWH = \frac{b_{FE} - b_{RE}}{\sqrt{s_{b_{FE}}^2 - s_{b_{RE}}^2}}$$

The DWH test is distributed as a standard normal random variable (i.e., it is a z test). The numerator contrasts the consistency of the two estimators, whereas the denominator contrasts the efficiency of the two estimators. Because FE is always less efficient than RE, the denominator will always be greater than zero. If this statistic exceeds a threshold such as ± 1.96 , it indicates that RE is sufficiently inconsistent that FE overcomes its inefficiency. A “large” statistic thus constitutes a rejection of RE in

favor of FE on consistency grounds. On the other hand, a “small” statistic means either that RE is consistent or that FE is so inefficient that its consistency advantage is undermined.

References

- Allen, L. C. (2000). *A life course analysis of the relationship between military service and criminal behavior*. Dissertation. University of Maryland, College Park.
- Angrist, J. D. (1990). Lifetime earnings and the Vietnam era draft lottery: evidence from social security administrative records. *American Economic Review*, *80*, 313–336.
- Apel, R., Blokland, A., Nieuwebeerta, P., & van Schellen, M. (2010). The impact of first-time imprisonment on marriage and divorce: a matched samples comparison in a Dutch conviction cohort. *Journal of Quantitative Criminology*, *26*(2), 269–300.
- Block, C. R., Blokland, A. A. J., van der Werff, C., van Os, R., & Nieuwebeerta, P. (2010). Long-term patterns of offending in women. *Feminist Criminology*, *5*(1), 73–107.
- Blokland, A. A. J., & Nieuwebeerta, P. (2005a). The effects of life circumstances on longitudinal trajectories of offending. *Criminology*, *43*, 1203–1240.
- Blokland, A. A. J., & Nieuwebeerta, P. (2005b). The effects of life circumstances on longitudinal trajectories of offending. *Criminology*, *43*, 1203–1240.
- Blokland, A. A. J., Nagin, D., & Nieuwebeerta, P. (2005). Life span offending trajectories of a Dutch conviction cohort. *Criminology*, *43*, 919–954.
- Bouffard, L. A. (2003). Examining the relationship between military service and criminal behavior during the Vietnam era: a research note. *Criminology*, *41*, 491–510.
- Bouffard, L. A. (2005). The military as a bridging environment in criminal careers: differential outcomes of the military experience. *Armed Forces and Society*, *31*, 273–295.
- Bouffard, L. A., & Laub, J. H. (2004). Jail or the army: Does military service facilitate desistance from crime? In S. Maruna & R. Immarigeon (Eds.), *After crime and punishment: Pathways to offender reintegration* (pp. 129–151). London: Willan.
- Bullinga, M. (1984). *Het leger maakt een man van je: homoseksualiteit, disciplineren en seksueel geweld. [The army turns you into a man: homosexuality, discipline, and sexual violence]*. Amsterdam: SUA.
- Bullinga, M. (1988). *Tegen-geweld. De VVDM over het bestrijden en voorkomen van intimidatie en geweld in de krijgsmacht. [Against violence: The VVDM about controlling and preventing intimidation and violence in the armed forces]*. Utrecht: VVDM.
- Castle, T., & Hensley, C. (2002). Serial killers with military experience: applying learning theory to serial murder. *International Journal of Offender Therapy and Comparative Criminology*, *46*, 453–465.
- Cohen, L. E., & Vila, B. J. (1996). Self-control and social-control: an exposition of the Gottfredson-Hirschi/Sampson-Laub Debate. *Studies on Crime and Crime Prevention*, *5*, 125–150.
- Durbin, J. (1954). Errors in variables. *Review of the International Statistical Institute*, *22*, 23–32.
- Elder, G. H., Jr. (1986). Military times and turning points in men’s lives. *Developmental Psychology*, *22*, 233–245.
- Elder, G. H., Jr. (1987). War mobilization and the life course: a cohort of World War II veterans. *Sociological Forum*, *2*, 449–472.
- Gottfredson, M. R., & Hirschi, T. (1990). *A general theory of crime*. Stanford: Stanford University Press.
- Greenberg, G. A., Rosenheck, R. A., & Desai, R. A. (2007). Risk of incarceration among male veterans and nonveterans. *Armed Forces & Society*, *33*, 337–350.
- Hakeem, M. (1946). Service in the armed forces and criminality. *Journal of Criminal Law and Criminology*, *37*, 120–137.
- Hausman, J. (1978). Specification tests in econometrics. *Econometrica*, *46*, 1251–1271.
- Hearst, N., Newman, T. B., & Hulley, S. B. (1986). Delayed effects of the military draft on mortality: a randomized natural experiment. *New England Journal of Medicine*, *314*, 620–624.
- Heckman, J. J., & Smith, J. A. (1999). The pre-programme earnings dip and the determinants of participation in a social programme: implications for simple programme evaluation strategies. *Economic Journal*, *109*, 313–348.
- Heinsman, D. T., & Shadish, W. R. (1996). Assignment methods in experimentation: when do non-randomized experiments approximate answers from randomized experiments? *Psychological Methods*, *1*, 154–169.

- Hirschi, T., & Gottfredson, M. R. (1995). Control theory and the life course perspective. *Studies on Crime and Crime Prevention*, 4, 131–142.
- Jongbloed, A. (1996). *Voor Koning(in) en Vaderland. Dienstplicht door de jaren heen. [For King and Country. Military Service throughout the Years]*. Zutphen: Alpha.
- Kors, A. (1996). *t' Is plicht dat ied're jongen. Geschiedenis van de dienstplicht in Nederland. [It's every boys duty. The history of compulsory military service in the Netherlands]*. Utrecht: Kwadraat.
- LaLonde, R. J. (1986). Evaluating the econometric evaluations of training programs with experimental data. *American Economic Review*, 76, 604–620.
- Laub, J. H., & Sampson, R. J. (2003). *Shared beginnings, divergent lives. Delinquent boys to age 70*. Cambridge: Harvard University Press.
- Laub, J. H., & Sampson, R. J. (2004). In K. W. Schaie & G. H. Elder Jr. (Eds.), *Historical influences on lives and aging* (pp. 1–31). New York: Springer.
- Lunden, W. A. (1951). Military service and criminality. *The Journal of Criminal Law, Criminology and Police Science*, 42, 766–773.
- Maas, I., & Settersten, R. A., Jr. (1999). Military service during wartime. Effects on men's occupational trajectories and later economic well-being. *European Sociological Review*, 15, 213–232.
- MacLean, A., & Elder, G. H. (2007). Military service in the life course. *Annual Review of Sociology*, 33, 175–196.
- Mattick, M. (1960). Parolees in the army during World War II. *Federal Probation*, 24, 49–55.
- McCullagh, P., & Nelder, J. A. (1989). *Generalized linear models* (2nd ed.). New York: Chapman and Hall.
- Meyer, B. D. (1995). Natural and quasi-experiments in economics. *Journal of Business and Economic Statistics*, 13, 151–161.
- Nieuwbeerta, P., & Blokland, A. A. J. (2003). *Criminal career and life-course study (documentation and codebook)*. Leiden: NSCR.
- Nieuwbeerta, P., & Piquero, A. (2008). Mortality rates and causes of death of convicted Dutch criminals 25 years later. *Journal of Research in Crime and Delinquency*, 45(3), 256–286.
- Paternoster, R., Dean, C. W., Piquero, A., Mazerolle, P., & Brame, R. (1997). Generality, continuity, and change in offending. *Journal of Quantitative Criminology*, 13, 231–266.
- Petras, H., Nieuwbeerta, P., & Piquero, A. (2010). Participation and frequency during criminal careers across the life span. *Criminology*, 48, 607–637.
- Piquero, A. R., Blumstein, A., Brame, R., Haapanen, R., Mulvey, E. P., & Nagin, D. S. (2001). Assessing the impact of exposure time and incapacitation on longitudinal trajectories of criminal offending. *Journal of Adolescent Research*, 16, 54–74.
- Piquero, A., Farrington, D. P., & Blumstein, A. (2003). The criminal career paradigm. *Crime and Justice: a Review of Research*, 30, 359–506.
- Rand, A. (1987). Transitional life events and desistance from delinquency and crime. In M. E. Wolfgang, T. P. Thornberry, & R. M. Figlio (Eds.), *From boy to man, from delinquency to crime* (pp. 134–162). Chicago: University of Chicago Press.
- Resnick, H. S., Foy, D., Donahoe, C. P., & Miller, E. N. (1989). Anti-social behavior and post-traumatic stress disorder in Vietnam veterans. *Journal of Clinical Psychology*, 45, 860–866.
- Rohlf, C. (2010). Does combat exposure make you a more violent or criminal person? Evidence from the Vietnam draft. *Journal of Human Resources*, 45, 271–300.
- Sampson, R. J., & Laub, J. H. (1993). *Crime in the making: Divergent pathways and turning points through life*. Cambridge: Harvard University Press.
- Sampson, R. J., & Laub, J. H. (1996). Socioeconomic achievement in the life course of disadvantaged men: military service as a turning point, circa 1940–1965. *American Sociological Review*, 61, 347–367.
- Shaw, D. M., Churchill, C. M., Noyes, R., & Loeffelholz, P. L. (1987). Criminal behavior and post-traumatic stress disorder in Vietnam veterans. *Comprehensive Psychiatry*, 28, 403–411.
- Sun, I. Y., Sung, H., & Chu, D. C. (2007). Collateral gains from the military? A cross-national analysis of the armed forces-crime relationship. *International Journal of Offender Therapy and Comparative and Comparative Criminology*, 51, 599–614.
- van der Meulen, J. (1984). *De taal van de dienstplichtcultuur—omstandigheden, principes, normen en ambities. [The Language of the Military Service Culture—Circumstances, Principles, Norms, and Ambitions]*. Assen: Van Gorcum.
- Weisburd, D. (2010). Justifying the use of non-experimental methods and disqualifying the use of randomized controlled trials: challenging folklore in evaluation research in crime and justice. *Journal of Experimental Criminology*, 6, 209–227.

- Weisburd, D., Lum, C. M., & Petrosino, A. (2001). Does research design affect study outcomes in criminal justice? *Annals of the American Academy of Political and Social Science*, 578, 50–70.
- Welsh BC, Sullivan CJ, Olds D (2010) When early crime prevention goes to scale: a new look at the evidence. *Prev Sci* 11:115–125
- Wilson, J. P., & Zigelbaum, S. D. (1983). The Vietnam veteran on trial: the relation of post-traumatic stress disorder to criminal behavior. *Behavioral Sciences & the Law*, 1, 69–83.
- Wright, J. P., Carter, D. E., & Cullen, F. T. (2005). A life-course analysis of military service in Vietnam. *Journal of Research in Crime and Delinquency*, 42, 55–83.
- Wu, De-Min. (1973). Alternative tests of independence between Stochastic Regressors and disturbances. *Econometrica*, 41, 733–750.
- Yager, T., Laufer, R., & Gallops, M. (1984). Some problems associated with war experience in men of the Vietnam generation. *Archives of General Psychiatry*, 41, 327–333.

Marieke van Schellen is a Ph.D. student in sociology at the Department of Sociology of the Utrecht University (The Netherlands). Her research specialties include criminal careers and marriage.

Robert Apel received his Ph.D. in criminology and criminal justice from the University of Maryland in 2004 and is currently an associate professor in the School of Criminal Justice at Rutgers University. His research specialties include employment and criminal behavior, incarceration and employment, and violent victimization and injury.

Paul Nieuwbeerta received his Ph.D. in sociology from the University of Nijmegen (The Netherlands) in 1995 and is currently a professor in the Institute for Criminal Law and Criminology at Leiden University (The Netherlands) and in the Department of Sociology of the Utrecht University (The Netherlands). His research specialties include criminal careers and life courses, effects of incarceration, and homicide.