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Do Guns Displace Books?

The Impact of Compulsory Military Service on Educational Attainment

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Do Guns Displace Books? – The Impact of Compulsory Military Service on Educational Attainment

Abstract

Compulsory military service typically drafts young men when they are at the height of their learning ability. Thus, it can be expected to depress the demand for higher education since skill atrophy and the delayed entry into the civilian labor market reduce the returns to human-capital investments. Attending university, however, might open the possibility to avoid the draft, leading to an increase in the demand for tertiary education. To estimate the causal effect of conscription on the probability to obtain a university degree, we use a regression-discontinuity design that employs special regulations associated with the introduction of conscription in Germany in 1956. We estimate conscription to increase the probability of having a university degree.

JEL Classification: I28, J24

Keywords: Regression discontinuity; conscription; career interruption; skill atrophy; TS2SLS

May 2011

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1 Introduction

Conscription into the armed forces is still actively enforced in many countries. Typically, men are called around the time they are 18 years old for a medical examination, after which they will either be declared fit to serve in the armed forces or exempted (temporarily or permanently) from service. Upon being drafted, the selected individuals are to serve in the armed forces for a specified period of time, usually for about two years. Military service can therefore be construed as a disruption of the human-capital-acquisition process of young men.

In several respects, this interruption is an important factor in an individual's demand for higher (i.e., tertiary) education. As Keller, Poutvaara and Wagener [2009] and Di Pietro [2009] point out, military service negatively affects the stock of human capital because academic skills acquired before military service may depreciate when these skills lie fallow during active duty. Furthermore, skills learned in the armed forces may not be necessarily transferable to the civilian labor market.

Moreover, due to their time in service, conscripts graduate from any higher educational facility at a later age than those graduates who have not been drafted. Hence, if there is a mandatory retirement age, as in Germany, conscripts have less time for reaping the returns to human-capital investments. This effect directly translates to a decline in the quantity of education demanded. Keller, Poutvaara and Wagener [2009] show that the number of university graduates indeed tend to be lower in countries that rely on compulsory military service (CMS) when compared to countries with an all-volunteer force, while adjusting for other factors that could plausibly influence the decision to seek university training.

In most countries, however, university students are granted a temporary exemption from military service. This temporary exemption is typically converted to a permanent one as soon as the student reaches a certain cutoff age. Even if a student graduates before reaching the cutoff age, being called into service after obtaining a university degree is not a prevalent phenomenon. These regulations might encourage young men to pursue a strategy of deliberate draft avoidance. This positive effect of conscription on educational demand has been documented for countries such as the United States [Card and Lemieux 2001] and France [Maurin and Xenogiani 2007].

This paper examines the issue within the context of Germany, where national service is still

compulsory until June 30, 2011 for men either in the form of military service (*Grundwehrdienst*) or civilian service (*Zivildienst*) for conscientious objectors.[1] Our objective is to estimate the causal impact of CMS on the probability of graduating from university. Since very special rules were governing the German draft in the 1950s, we can use a regression-discontinuity (RD) design to identify this causal effect: When Germany was allowed to rearm itself after becoming a member of NATO in 1955, men born on or after July 1, 1937 had to undergo mental and physical examinations upon coming of age to determine whether they were fit to serve in the German armed forces (*Bundeswehr*).

Those born earlier were exempted from CMS, though. Colloquially, they are called the "White Cohort" (*weißer Jahrgang*) because they neither served in World War II nor in the new German armed forces. We deliberately use the White Cohort as a control group to which we can compare the educational outcomes of those facing a positive probability of being drafted. Assuming men born on either side of the threshold date are different only in that the White Cohort did not serve in the armed forces but are identical otherwise, the difference in the educational outcomes of the White Cohort and the subsequent cohort can be construed as the causal effect of conscription.

Our analysis indicates that, for the cohorts under study, the actual causal impact of conscription on the probability of having a university degree is positive and substantial in magnitude. Exploiting the discontinuous treatment-assignment mechanism, the RD coefficient estimates based on two-sample two-stage least squares (TS2SLS) range from 12 to 15 percentage points, with the highest estimate being observed for precisely those men for whom we believe the RD estimates to be most appropriate. Therefore, we conclude that conscription increased men's university graduation rate.

The remainder of the paper is organized as follows. Section 2 presents a brief review of the relevant literature, Section 3 introduces the data, and Section 4 presents our estimation strategy and discusses the ensuing results. Finally, Section 5 concludes with a discussion of the policy implications of our findings.

[1] Conscription is enshrined in Art. 12a of Germany's constitution (*Grundgesetz für die Bundesrepublik Deutschland*). The rules and regulations are contained in the *Wehrpflichtgesetz*. Civilian service is governed by the *Zivildienstgesetz*. In the much earlier time period examined in this paper, the number of conscientious objectors had been negligible. [Haberhauer and Maneval 2000]

2 Related literature

The present study locates itself at the intersection of two established areas of the economic literature. The first examines the effects of military service on veterans, the second, the effects of career interruptions on subsequent labor-market outcomes. With regard to the former, Angrist [1990] finds that white Vietnam War veterans subsequently realized lower annual wages than nonveterans. However, updated estimates by Angrist and Chen [2008] using the same data show that the wage gap vanishes in the long run. In any case, these results suggest military service is costly for those who have to serve.

Angrist and Krueger [1994] demonstrate that the higher wages of World War II veterans compared to nonveterans are merely the result of how soldiers were selected into the armed forces, i.e., that simple cross-sectional comparisons are biased. Taking the nonrandom selection into the armed forces into account, serving in the war was not found to have a positive causal impact on wages. While these studies use the date of birth as an instrument for veteran status, Imbens and van der Klaauw [1995] specifically look at the effect of conscription (as opposed to being a veteran of any war) in the Netherlands. Using variations in the probability to be drafted across birth cohorts as an instrument for being conscripted, they show that draftees earn about 5 percent less than those who did not serve.

Methodologically, the present paper is closest to Buonanno [2006], Bauer et al. [2009], and Grenet, Hart and Roberts [2011]. These papers take advantage of the discontinuity in the probability of being drafted into military service across birth cohorts. In the United Kingdom, this was caused by the initiation of conscription in 1949 and its subsequent abolition in 1960. Buonanno [2006] uses the latter to estimate the impact of conscription on earnings and on educational attainment. He finds that conscription penalizes men but that the resulting earnings disadvantage should be attributed to lost labor-market experience and not to the educational channel. In fact, he finds little evidence to suggest that the demand for higher education is negatively affected by conscription.

Grenet, Hart and Roberts [2011] use the start of conscription in the UK to identify the effects of conscription on long-term real earnings. Their findings indicate that conscription had no significant effect on such earnings. With the distinct advantage of having precise birth date information at their disposal, Bauer et al. [2009] use the same White Cohort/non-White Cohort

distinction described in the present paper to obtain an estimate of the causal impact of CMS on long-run labor-market outcomes in Germany. The authors conclude that the superior labor-market performance of those who served in the *Bundeswehr* is merely an artifact of selection bias introduced at the time of the draft (e.g., healthier men, who are more likely to perform well in the labor market, are precisely the ones who were called to serve).

Other authors examine explicitly the effect of military service on educational outcomes. Here, the evidence is rather mixed. Using the trend in college enrollment of women as a comparison, Card and Lemieux [2001] estimate that CMS raised college attendance rates by 4–6 percentage points and the fraction of men born in the mid-1940's with a college degree by about 2 percentage points. The authors argue that these results can be explained by a draft-avoidance strategy. A similar study concerning the effect of the abolition of conscription in France was conducted by Maurin and Xenogiani [2007]. In their paper, draft-avoidance behavior was also thought to motivate young men to stay within the formal educational system. Similar to the results obtained from US data, France saw a significant decrease in the time spent in school by young and in the share of degree-holders among adult men as a result of the abolition. Bound and Turner [2002] present results indicating that state funding for college through the Servicemen's Readjustment Act of 1944 (the so-called "G.I. Bill") increased the post-secondary educational attainment of G.I.'s.

Cipollone and Rosolia [2007] exploit a natural experiment in southern Italy caused by an earthquake in 1980. This natural disaster prompted politicians to exempt certain birth cohorts of men from military service. The authors find that the exemption raised boys' high school graduation rates by over 2 percentage points. Di Pietro [2009], also using data from Italy, finds that the abolition of conscription raised university participation for individuals from more privileged family backgrounds but depressed university participation for individuals who are more disadvantaged. Finally, Keller, Poutvaara and Wagener [2009] analyze the issue at the macroeconomic level by taking 22 OECD countries and looking at the impact of CMS on the acquisition of post-secondary education. They find that enforcement intensity—measured as the share of the labor force conscripted and the duration of service—have a significantly negative impact on university enrollment figures.

In addition to these studies that are directly related to military evidence, several studies

analyze the impact of career disruptions (e.g., unemployment) on future labor-market outcomes. The conventional wisdom is that career interruptions imply foregone experience in the labor market and skill atrophy (i.e., human-capital depreciation). These disruptions apparently have a persistent effect [Kletzer and Fairlie 2003] and even their timing and type seem to matter [Kunze 2002]. This line of research concentrates predominantly on the interruptions experienced by women due to child-bearing and -rearing [Albrecht et al. 1999]. These breaks contribute to the globally observed gender wage gap because women are much more likely to experience career interruptions than men. For men, being drafted into military service represents a special form of career disruption that typically occurs between secondary and tertiary education. An interruption at such an early point in one's prospective labor-market career can have far-reaching implications.[2]

3 Data construction and description

We use two datasets for our empirical analysis. The first is provided by the state pension authorities. This dataset ("Pension Data") contains information on the service status of men and their dates of birth. This allows us to compute the probability of serving in the armed forces as a function of one's date of birth, which is crucial to our identification strategy. The second data source ("IABS") consists of administrative data provided by the Institute for Employment Research (IAB).[3] Except for some special groups, such as civil servants and the self-employed, the dataset contains the total German population that was gainfully employed for at least one day for the period 1975–1995. Observed characteristics of individuals include sex, date of birth, nationality, and educational qualification.

With respect to sample exclusions, the following observations were removed from both datasets: females, East Germans (defined as having at least one employment spell in East Germany), persons who had an employment spell outside West Germany at any point (including foreigners and ethnic German immigrants), persons born before January 1, 1934, and persons born after December 31, 1940. Professional soldiers and those who underwent conscription

[2] See, for example, the introduction of Arulampalam, Gregg and Gregory [2001] on the "scarring" effects of unemployment.

[3] See Bender, Haas and Klose [2000] for details. A new version of the IABS will be available for researchers via the Research Data Center of the German Employment Agency in the Institute for Employment Research (RDC–IAB) in 2011. See `http://fdz.iab.de/en/FDZ_Projects/BASID.aspx/`.

and then subsequently became professional soldiers were also deleted from the datasets. We excluded those people without a tertiary-system-entry certificate (*Hochschulreife*). Since these people were virtually excluded from entering university, their decision to seek out tertiary education is most likely unaffected by the introduction of conscription. For the Pension Data, this leaves us with 15,835 men born around the threshold date who, if they performed CMS, served for not more than 12 months. For the IABS, we end up with 320,620 men. Notably, in these years, only a negligible fraction of men were engaged in *Zivildienst* or other non-military services (*Wehrersatzdienst*) as a substitute for CMS.

Persons are defined as having a university degree if they earned a degree from a *Hochschule*, which can take a number of forms such as the more technical *Fachhochschule* or the traditional *Universität*. According to our data, 74 percent of men in the White Cohort, i.e., those born before July 1937, have a university degree compared to a share of 79 percent among those in the non-White Cohort. It would therefore seem that, among men who have the possibility to enter university, those who faced a positive probability of being drafted are more likely to obtain a tertiary degree.[4] For a variety of reasons (e.g., selection bias), however, a simple comparison of means will be able, at most, to give us an indication of the extent to which draft eligibility is influencing the likelihood of graduating from university.

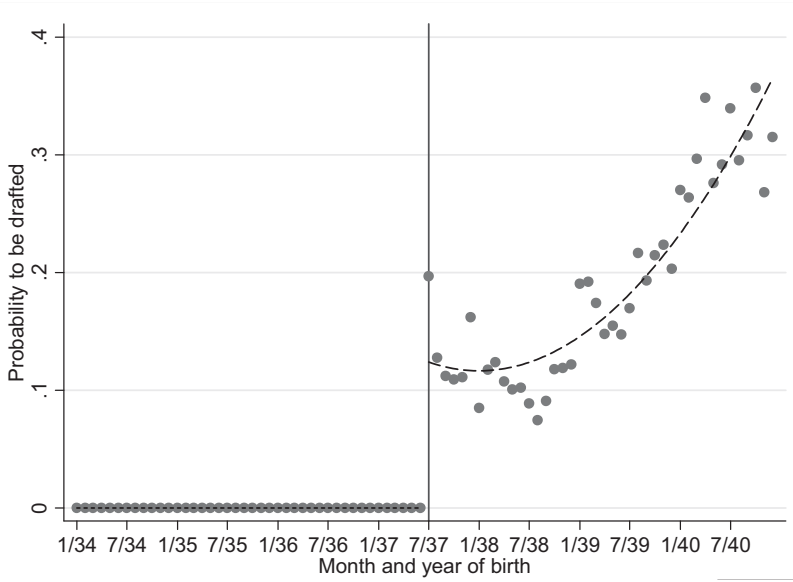
Figure 1 plots the share of conscripted men by month of birth. The White Cohort was completely exempted from the conscription and therefore the region to the left of the threshold date only contains points that lie at zero. To the right of the exemption threshold on the 1st of July 1937, we observe that the share of men called into service started at about 18 percent, which then continuously declined until about the middle of the 1938 birth cohort, after which the probability of being drafted increased until the end of the sample period.

This evolution of the draft probability can be attributed to initial experienced in the first few years of the *Bundeswehr*. First, special exemptions were granted to some men.[5] Second, the Cold War was intensifying as these men were coming of age. Third, temporary deferments of national service were granted to men who would experience serious personal or economic hardship if drafted. Some of these temporary deferments became permanent as soon as men

[4] If we include those men who do not have a tertiary-system-entry certificate, the respective shares are 8 and 14 percent.

[5] For instance, the only son of a soldier killed in World War II was exempted. People who lost siblings may also request for an exemption. Finally, those who will likely experience some form of economic hardship if drafted may also be exempted.

FIGURE 1
PROBABILITY TO BE DRAFTED FOR MILITARY SERVICE BY MONTH OF BIRTH



NOTE. Dashed lines represent quadratic fits over White Cohort and non-White Cohort observations.
SOURCE. Authors' own illustration based on Dataset 1.

turned 26 because the maximum age at which men could be drafted in Germany—except under special circumstances—is 25.[6]

4 Estimation strategy and results

Our identification strategy is anchored on the RD design of the military draft in Germany.[7] The White Cohort—men born before July 1, 1937—was exempted from conscription. Let N_i be equal to 1 if individual i does not belong to the White Cohort and 0 if he does. For those who were born on or after the threshold date, men are subjected to a comprehensive medical examination upon coming of age to determine whether they are healthy enough to serve. Those who fall short may defer national service and those who have severe health issues are immediately and permanently exempted. Although one may be declared fit for service, this does not necessarily imply that one is immediately or actually drafted into service. The treatment status may

[6] Among others, exemptions are granted to volunteers to the police, the Federal Border Guard, and priests. The special exemptions applied to about 10 percent of the men born between 1937 and 1944. About 3.5 percent of men in a particular birth cohort were exempted because they crossed the age limit. [Wehrstruktur-Kommission 1971]

[7] For references on the RD design, see Hahn, Todd and van der Klaauw [2001], van der Klaauw [2002], and Lee and Lemieux [2010].

thus be represented by M_i, which is equal to 1 if individual i was eventually called to serve in the military and 0 otherwise. Men could therefore be in one of three different possible states: ($M_i = 0$ and $N_i = 0$), ($M_i = 0$ and $N_i = 1$), and ($M_i = 1$ and $N_i = 1$).

The RD design is characterized by a treatment-assignment rule based on a known cutoff point with respect to an observable and continuous variable, which is called, *inter alia*, the forcing or running variable. As seen in Figure 1, the conditional probability of receiving treatment (i.e., serving in the *Bundeswehr*) is known to be a discontinuous function of the date of birth, B_i. Let $\overline{B}$ represent the threshold value at which point the conditional probability jumps—that is, the date of birth that distinguishes members of the White Cohort from the rest. The White Cohort is exempted from conscription so that $\mathrm{E}\left[M_i | B_i < \overline{B}\right] = 0$. On the other hand, for those born on or after $\overline{B}$, the probability to be drafted is a function of a vector of individual characteristics $\mathbf{x}_i$, i.e., $\mathrm{E}\left[M_i | B_i \geq \overline{B}\right] = f(\mathbf{x}_i)$.

The situation represented by Figure 1 is called the partially fuzzy RD design, where the jump in the conditional probability is not from 0 to 1 but rather from 0 to some value less than 1. More specifically,

$$\lim_{B \uparrow \overline{B}} \Pr(M_i = 1 | B_i = B) < \lim_{B \downarrow \overline{B}} \Pr(M_i = 1 | B_i = B),$$

where $\lim_{B \uparrow \overline{B}} \Pr(M_i = 1 | B_i = B) = 0$. The conditions for proper identification of the treatment effect in this case are similar to those required in the sharp RD design, which is the case when the jump in the conditional probability is equal to one [Battistin and Rettore 2008].

To estimate the causal impact of conscription on the probability of having a univeristy degree, we implement a two-sample two-stage least-squares (TS2SLS) approach developed by Angrist and Krueger [1992]. The reason is that we can observe B_i in both datasets but only M_i in the Pension Data and only Y_i, the outcome variable, in the IABS.[8] The sequential system of equations is

$$M_{i1} = \alpha_1 + f(B_{i1}) + \delta N_{i1} + \epsilon_{i1}$$
$$Y_{i2} = \alpha_2 + g(B_{i2}) + \tau \widehat{M}_{i2} + \nu_{i2},$$

[8] In a previous paper [Bauer et al. 2009], we were able to match individuals from both datasets. However, for this paper, we obtained the whole sample of men from the IABS, not all of whom are available in our original dataset. Therefore, we had to rely on TS2SLS to retrieve the required estimates.

where the second subscript indicates which dataset is used. The variable $\widehat{M}_{i2}$ denotes the cross-sample fitted values, which are computed by using the parameter estimates obtained from the first stage based on the Pension Data but using the observations in the IABS. The disturbance terms are represented by ϵ_{i1} and ν_{i2}. The terms $f(B_{i1})$ and $g(B_{i2})$ are some arbitrary continuous functions of B. We apply the restriction $f(B_{i1}) = g(B_{i2})$ so that the system becomes essentially a two-sample two-stage least-squares instrumental-variable estimator for the parameters in the model.[9]

Military service is therefore instrumented by an individual's date of birth, which is presumably independent of the decision to obtain a university degree. In our specification, $f(B_{i1})$ is modeled as a polynomial of the difference (measured in days) of a man's date of birth and the threshold date, i.e., $\left(B_{i1} - \overline{B}\right)$. These individual differences are interacted with N_{i1} to allow for different slope parameters on either side of the threshold. The standard errors are subsequently corrected according to the method developed by Murphy and Topel [1985] regarding generated regressors (our $\widehat{M}_{i2}$), which was shown by Inoue and Solon [2010] as the correct approach in the TS2SLS framework.

Two minor econometric issues arise in this context. First, we are estimating both equations as linear probability models; thus, we are only linearly approximating the underlying conditional expectation function. Second, one could restrict the sample for the first-stage equation to include only those men whose date of birth is later than June 30, 1937 since, by definition, those born on or before that date will have $M_{i1} = 0$. Accounting for the nonlinearity is an unnecessary complication since, in an IV framework, one does not need consistent estimates of the first-stage coefficients to retrieve consistent estimates of the coefficients in a properly specified second-stage model [Heckman 1978]. We therefore refrain from extensively interpreting the coefficient estimates in the first stage. A linear second stage has the advantage of directly providing the marginal effects. As for the partially fuzzy design, the predicted probabilities of serving in the German armed forces for the White Cohort are extremely close to zero. For instance, using the full sample, the maximum value of $\widehat{M}_{i2}$ is less than 1×10^{-12}.

The coefficient of interest is τ, which is interpreted as a local average treatment effect valid for a small neighborhood around the point of discontinuity in the conditional probability of receiving treatment [Lee and Lemieux 2010]. That is, the estimand $\widehat{\tau}$ is a consistent estimator of

[9] For details of the TS2SLS estimator, see Inoue and Solon [2010].

$\mathrm{E}\left[Y_i(1) - Y_i(0) | \text{unit } i \text{ is a complier and } B_i = \overline{B}\right]$, where $Y_i(M_i)$ represents potential outcomes and "complier" is used in the same sense as in Imbens and Angrist [1994]. Valid identification is possible as long as the conditional mean of the potential outcome $Y_i(0)$ is continuous at $\overline{B}$ [Hahn, Todd and van der Klaauw 2001]. The fundamental idea here is that, for as long as all other factors that could affect one's educational attainment affects it only in a continuous manner, the jump in the probability of being drafted at the threshold point explains the difference in educational attainment between the White Cohort and the non-White Cohort.

The control function $f(B_i)$ is constructed using polynomials of the difference in days in one's birth date and the threshold date. Fully interacting the polynomials with the indicator variable N_i permits different slope coefficients on both sides of the cutoff point. Following Angrist and Lavy [1999], the degree of the polynomial that we use in the regression decreases as the discontinuity sample becomes tighter around the threshold point.

Table 1 presents the first-stage results using the Pension Data; Table 2 shows the results of a regression of a binary variable indicating completion of a university degree on the instrument based on the IABS. All regressions include the control function $f(B_i)$ with varying degrees of the polynomial order. Table 1 shows that the probability of being drafted is positively related to one's date of birth as captured by the indicator variable for being on either side of the threshold. The coefficient estimates for the binary instrumental variable range from 9.98 to 12.36 percentage points. In particular, for the cohort of men born in 1937, we recover an estimate of 11.72 percentage points. Nevertheless, these estimated coefficients should not be given a causal interpretation for reasons already mentioned. What is rather more important is that the t- and F-statistics indicate that there is a strong relationship between one's date of birth and the probability of conscription.

Table 2 presents the reduced-form estimates based on the IABS. Here, we highlight the fact that, without instrumenting conscription, we obtain a statistically significant relationship for two subsamples (Cohorts 1935–1939 and 1937) between one's date of birth, again as captured by the binary instrumental variable for being part of the White Cohort or not, and the probability of having a university degree. This can be interpreted as the effect of the introduction of conscription, i.e., as an intention-to-treat effect. Taking the estimate from the 1937 birth cohort, the introduction of conscription raised the share of men with a university degree by about

TABLE 1
FIRST-STAGE ESTIMATES (PENSION DATA)

	Cohort			
	All	1935–1939	1936–1938	1937
Non-White Cohort	0.0998***	0.1236***	0.1193***	0.1172***
	[0.0204]	[0.0148]	[0.0120]	[0.0165]
Polynomial degree J	3	2	1	1
F-statistic	317.81	84.49	57.13	41.71
Adjusted R^2	0.1526	0.0500	0.0404	0.0533
Observations	15,835	11,099	6,668	2,171

NOTES. The dependent variable is having served in the armed forces. The regressions include a constant term. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE. Authors' own calculation.

1.7 percentage points. As the reduced form only allows us to recover the intention-to-treat effect, the estimate naturally understates the actual causal impact of conscription on draftees. This is because of "noncompliance" in the group assigned to be treated. As shown in Figure 1, less than 20 percent of men born close to the right of the threshold date were eventually drafted.

TABLE 2
REDUCED-FORM ESTIMATES (IABS)

	Cohort			
	All	1935–1939	1936–1938	1937
Non-White Cohort	0.0121	0.0129*	0.0106	0.0170**
	[0.0075]	[0.0071]	[0.0069]	[0.0080]
Polynomial degree J	3	2	1	1
F-statistic	100.83	45.74	13.27	2.46
Adjusted R^2	0.0028	0.0014	0.005	0.0001
Observations	320,620	225,233	132,121	43,197

NOTES. The dependent variable is having a university degree. The regressions include a constant term. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE. Authors' own calculation.

Our estimates are very close to the result obtained by Card and Lemieux [2001], who estimate a 2-percentage-point increase in the share of men with a university degree in the US. In line with their results, we attribute this finding to the existence of "draft dodgers" in the population. Since going to university affords male students the opportunity to avoid being drafted, higher education can be used to decrease the likelihood of serving in the armed forces. In Germany, one could still legally be drafted after reaching the maximum age for conscription (25 years old) if the reason for deferment was to complete a university degree. However, in our dataset, only 290 men were drafted after the age of 25. This means that crossing the age threshold—even if the deferment is due to university participation—resulted in a *de facto* per-

manent exemption. For draft dodgers, university participation provided a safe haven. Consequently, for the cohorts under scrutiny, draft avoidance apparently overwhelmed the possible negative effect of conscription (due to the adverse effect on the returns to human capital) on the probability of pursuing a university degree.

As in the standard "grouping estimator" of Wald [1940], to obtain an estimate of the causal effect of military service, the estimates in Table 2 are normalized by dividing them with the corresponding estimates in the first-stage regressions in Table 1. More precisely, this procedure allows recovering the local average treatment effect for observations around the threshold point [Imbens and Angrist 1994; Lee and Lemieux 2010]. The only difference that must be taken into account here is that the estimates in Tables 1 and 2 derive from different samples, which means that the standard errors must be corrected [Inoue and Solon 2010]. The consistency of the estimator, however, is unaffected by the fact that we use moment conditions from one sample and apply it to another [Angrist and Krueger 1992].

Table 3 shows results using the TS2SLS approach that exploits the RD design of the military draft. Our estimates indicate that conscription had a positive impact on the probability of obtaining a university degree. Although the statistical significance of our estimates depend on which cohorts we use, the estimate is significant for precisely that subsample for which the validity of the RD estimate is most credible. That is, since the RD estimate is valid only for those observations within a small neighborhood around the threshold, we take the estimates using only those men born in 1937 as the one with the highest internal validity. By this measure, we calculate that conscription raised the probability of having a university degree by about 15 percentage points.

TABLE 3
IMPACT OF CMS ON UNIVERSITY GRADUATION

	Cohort			
	All	1935–1939	1936–1938	1937
Military service	0.1212	0.1042*	0.0891	0.1455**
	[0.0748]	[0.0573]	[0.0577]	[0.0679]
Polynomial degree J	3	2	1	1

NOTES. Bracketed numbers are Murphy–Topel [1985] standard errors. The regressions include a constant term. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE. Authors' own calculation.

A concern is whether the compulsory schooling laws in Germany—in particular, the regulation on the age of school entry—could generate a similar effect on our outcome variable

of interest, thereby rendering the estimates suspect. Men born later in the year enter school older than men born earlier in the year. If there is an age-of-entry effect, whereby older pupils perform better in school, this may bias our estimates.[10] Simply put, men born after July 1 in the year 1937, because they are older than the rest of their school cohort, may have a higher probability of entering the *Gymnasium*, which is the type of secondary school which can supply graduates to the university system.[11] If this is true, then we should expect a higher probability of obtaining a university degree for men born in the second half of the year—for every year.

This provides us with an opportunity to test whether the age-of-entry effect is confounding our results. If we find a similar positive effect in other years, then it is likely that the previous estimates do not accurately reflect the direct impact of conscription on the probability of obtaining a university degree. We can create a dataset where the discontinuity is relocated somewhere else and then similarly re-estimate our model; we do this for the year 1938. The results are presented in Table 4. These "placebo regressions" indicate that there is no evidence of a positive effect on the probability of having a university degree for men born in later parts of the year.

TABLE 4
PLACEBO REGRESSIONS WITH SIMULATED DISCONTINUITY IN 1938

	Cohort			
	All	1935–1939	1936–1939	1937–1938
Placebo	−0.0553	−0.0269	−0.0436	−0.0483
	[0.0635]	[0.0635]	[0.0682]	[0.0617]
Polynomial degree J	4	2	2	1

NOTES. Bracketed numbers are Murphy–Topel [1985] standard errors. The regressions include a constant term. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE. Authors' own calculation.

5 Discussion and conclusion

In Western Europe, Germany became the latest country to effectively abolish compulsory military service. However, many countries still rely on conscription to staff their respective armed forces. The effects on the lives of young men of a policy relying on what is essentially in-

[10] For the literature on this, see, e.g., Angrist and Krueger [1992], Fertig and Kluve [2005], Puhani and Weber [2005], and Lincove and Painter [2006].

[11] The *Gymnasium* has a higher admission requirement in terms of the potential student's previous academic performance. Graduates of the two other types of schools, the *Realschule* and the *Hauptschule*, do not have a similarly easy way to access to tertiary education.

voluntary labor is a subject of extreme economic and political—if not moral—importance. To contribute to the debate, we examine the relationship between conscription and the demand for higher education in Germany.

We exploit the regression-discontinuity design of the military draft in Germany in the 1950s using a sample of men born between 1934 and 1940 to examine the impact of conscription on tertiary educational attainment. The law governing conscription allows us to distinguish two groups of men: those born before July 1, 1937, who belong to the so-called White Cohort, and those born afterwards. What is peculiar about the White Cohort is that its members were completely exempted from performing military service. The rest, on the other hand, faced a positive probability of being drafted. This discontinuity in the treatment assignment based on a man's date of birth is used to obtain consistent estimates of the effect of conscription on the probability of having a university degree.

Like previous country-specific studies [Card and Lemieux 2001; Maurin and Xenogiani 2007], we find that the presence of conscription increased the demand for post-secondary investments in human capital.[12] We estimate this intention-to-treat effect of conscription to be 1.7 percentage points. We attribute this to the fact that enrollment in an educational institution is an effective method to avoid the draft.

There are several reasons why men would prefer not to serve in the armed forces. In the period under study, men were growing up fresh out of World War II. Presumably, the ravages of war were vividly imprinted in their (and in their parents') minds, leading to an understandable desire not to be involved with the armed forces. Moreover, the Cold War was intensifying and the distinct possibility of another armed conflict erupting within their lifetime—or, indeed, while they were on active duty—was not at all remote, if not increasingly likely. It seemed that retreating to university and away from the military had been the optimal choice for some men.

Our estimate of the effect of conscription itself (and not simply its introduction) on the probability of obtaining a university degree is rather striking. The estimates range from about 9 to 15 percentage points, with the higher bound exhibiting the highest level of statistical significance within our range of estimates. By virtue of the fact that our estimate is much higher than the reported effects in other studies, it should be subjected to a stricter level of scrutiny. Thus, we emphasize caution in generalizing this estimated effect to the rest of the population.

[12] In contrast, Keller, Poutvaara and Wagener [2009], using cross-country regressions, find a small negative effect.

We posit three reasons to explain our findings. The first reason can be indirectly attributed to skill atrophy. Human capital—in particular, that part of one's stock that is not used in the armed forces but is rewarded in the civilian sector—tends to depreciate while on active duty. Replacing lost human capital requires additional education on the part of the former conscript. Having already finished secondary education, the next logical step would be to enter a higher educational institution, where human capital can be re-acquired [Keller, Poutvaara and Wagener 2009].

Second, human-capital acquisition post-conscription allows men to catch up with the rest of their cohort in terms of earnings. It is well known that the experience–earnings profiles are dissimilar by educational attainment. While, on average, earnings increase as a function of labor-market experience, the slope of this function is much higher for university graduates than for non-graduates. Furthermore, the age–earnings profile of a university graduate would exhibit a longer flat region before reflecting a positive slope, indicating the fact that the person has spent more years in the school system before entering the labor market. This flat region would be even longer for conscripts, which could induce them to resort to university training after their service in the hopes of quickly matching the earnings of their non-conscripted peers.

Finally, conscription has been shown to have heterogeneous effects based on one's socioeconomic standing. Using the father's occupation as a proxy for a person's socioeconomic status, Di Pietro [2009] showed that the abolition of the draft in Italy had a detrimental (that is, negative) effect on the university enrollment of men from disadvantaged backgrounds. We are unable to make the same distinction here. However, we can speculate that the positive effect we estimated may be driven by the share of men coming from disadvantaged families. In other words, conscription drove men with higher discount rates—generally a characteristic associated with men who are less educated and are disadvantaged—to invest in a university degree. Furthermore, until the recent abolition of compulsory military service, men from households with a higher average income were less likely to serve in the armed forces [Bauer and Schmidt 2010].

Taking the estimated effect at face value would lead to the conclusion that the recent abolition of conscription in Germany will reduce men's probability of investing in university-generated human capital. We are, however, tentative in elaborating on policy recommenda-

tions because of two principal reasons. First, treatment effects obtained by exploiting an RD design are extremely localized because the source of identifying variation is itself highly specific. The discontinuity in the conditional probability of being drafted into the armed forces is what allows us to credibly recover the treatment effect. But this effect is most valid only for the subpopulation of men in a shrinking neighborhood around that discontinuity point. Estimating the effect of conscription for men away from the threshold involves a certain amount of extrapolation which we hesitate to do, since the identifying assumption that makes the estimates from the RD design valid is less likely to hold the farther away one moves from the point of discontinuity.

Second, the institutions governing conscription in Germany at that time were quite different from what they are today. For instance, opting for civilian service was a viable alternative in recent years. In general, which skills are acquired and lost turns on whether one is either in the military or in the civilian sector. In the period analyzed in the present study, men who were called to perform national service would have faced severe difficulties substituting civilian service for military service.

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