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Compulsory Military Service in Germany Revisited

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Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI)
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Alfredo R. Paloyo[1]

Compulsory Military Service in Germany Revisited

Abstract

This paper estimates the causal impact of compulsory military service on men in Germany using social security and pension administrative data for the cohort of individuals born in the period 1932–1942. Due to the combination of laws restricting conscription only to men born on or after July 1, 1937, difference-in-differences estimates of the effect of conscription on average daily wages can be computed using cohorts of women as a comparison group. The results indicate that conscription had no significant impact on a draftee's labor-market performance, validating an earlier result using an alternative identification strategy.

JEL Classification: J31

Keywords: Conscription; difference in differences; German Federal Defense Force

September 2010

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1 RWI, Ruhr-Universität Bochum and Ruhr Graduate School in Economics (RGS Econ). *– Access to the data was generously provided by Stefan Bender of the Institut für Arbeitsmarkt- und Berufsforschung (IAB) in Nürnberg. Thanks to Thomas K. Bauer, Daniel Baumgarten, Colin P. Green, and Timo Mitze for their comments. I gratefully acknowledge the financial support provided by the RGS Econ.* *– All correspondence to Alfredo R. Paloyo, Ruhr Graduate School in Economics, c/o RWI, Hohenzollernstr. 1–3, 45128 Essen, Germany; E-Mail: paloyo@rwi-essen.de.*

1 Introduction

Germany is among the dwindling number of countries, especially in Europe, that still use conscription to staff its armed forces. Most recently, Sweden abolished compulsory military servcie (CMS) after 109 years of obligating young men (and, since 1980, young women) to serve in the armed forces.[1] Various other countries are seriously considering a transition to an all-volunteer force. In Germany, the issue of ending conscription has recently been raised by the Defense Minister, Karl-Theodor zu Guttenberg, who irked some of his colleagues by going against his party's long-standing support for obligatory military service (*Wehrdienst*).[2] The reason is more economic than strategic. Faced with a tightening fiscal constraint brought on by the economic and financial crisis that started in 2007, the German federal government is becoming increasingly creative in ensuring fiscal balance. According to one estimate, the government will save almost €500 million per year by abolishing conscription—not a small amount of money considering that it wants to save €80 billion by 2014.[3] The potential annual savings from conscription represents about 1.6 percent of total military expenditure in 2008 (€31.921 billion).[4]

Surprisingly, however, there is a dearth of studies looking at the impact of military service, compulsory or otherwise, on the people who actually serve or have served in Germany. While the issue has been examined in the United States in a series of papers by Joshua Angrist, the pecuniary and non-pecuniary impacts of having been in the armed forces have largely been neglected in Germany.[5] This problem is made more acute in the context of the reform currently being contemplated by the Defense Minister. For such a major transformation of a country's armed forces and defense strategy, it becomes imperative to reach well-informed conclusions founded on credible policy evaluation. Thus, one aim of the present study is to provide additional evidence on the effects of military service

[1] Agence France-Presse, "Sweden ends compulsory military service", Defense News, 1 July 2010. Accessed 6 August 2010. `http://www.defensenews.com/story.php?i=4694162`.

[2] Fröhlingsdorf, Michael, Sven Röbel, and Christoph Scheuermann, "Killing time: for conscripts, German military service is a battle against boredom", Der Spiegel, 24 June 2010. Accessed 6 August 2010. `http://www.spiegel.de/international/germany/0,1518,702665,00.html`. See also Demmer, Ulrike, Kerstin Kullmann, René Pfister, and Christoph Schwennicke, "Dodging the draft: conscription debate divides German conservatives", Der Spiegel, 29 July 2010. Accessed 6 August 2010. `http://www.spiegel.de/international/germany/0,1518,708905,00.html`.

[3] Joyner, James, "Germany can't afford military conscription", Atlantic Council, 29 July 2010. Accessed 6 August 2010. `http://www.acus.org/new_atlanticist/germany-cant-afford-military-conscription`. See also "Radical cutbacks: German government agrees on historic austerity program", Der Spiegel, 7 June 2010. Accessed 6 August 2010. `http://www.spiegel.de/international/germany/0,1518,699229,00.html`.

[4] Military expenditure was obtained from the Stockholm International Peace Research Institute.

[5] For the US, see Angrist [1990], Angrist [1993], Angrist and Krueger [1994], Angrist and Chen [2008], and Angrist, Chen and Frandsen [2009], all of which give a modern econometric treatment to the issue. For Germany, see Schleicher [1996] and Schäfer [2000]. These studies, however, focus on the implicit tax that is imposed on conscripts during the performance of their civic duty. An exception is a previous paper by the current authors; see Bauer et al. [2009].

to guide policymakers not only in Germany but also in those countries contemplating changes to the way their armed forces are organized.

Varying results have been reported by researchers in different jurisdictions. For example, for those who actually served in wars, Angrist [1990] shows that Vietnam War veterans earned approximately 15 percent lower annual wages than non-veterans although updated estimates from Angrist and Chen [2008] find no long-term impacts. Furthermore, Angrist and Krueger [1994] indicate no effect of serving in World War II on the labor-market performance of veterans. There are also estimates for national military service that does not involve serving in war zones. On one hand, Imbens and van der Klaauw [1995] show that conscripts in the Netherlands earn about 5 percent less than those who did not perform military service. On the other hand, an earlier paper by the current authors [Bauer et al. 2009] find no similar effect for conscripts in Germany. While Buonanno [2006] finds a small penalty for conscripts in the UK, Grenet, Hart and Roberts [2010] report no significant effects for the same country. Other outcome variables of interest, such as demand for education, have also been examined. Card and Lemieux [2001] and Maurin and Xenogiani [2007] find evidence that college enrollment increases as a result of men avoiding the draft in the US and France, respectively. This is because men enrolled in university are temporarily exempted from conscription. Cipollone and Rosolia [2007], however, find that exemption from military service raised boys' high school graduation rates in Italy (i.e., it would have been lower had the draft applied to these boys).

In a sense, conscripts were being made to pay a tax-in-kind to the State; this fact is well-known. However, one should also note that time spent in the German armed forces *Bundeswehr* equates to time not spent in the civilian labor market, where they could accumulate human capital or earn income. Moreover, the human capital that conscripts have accumulated up to the point of conscription may also depreciate during active duty—a phenomenon called skill atrophy—since the skills learned in school may not necessarily be skills that are valued in the armed forces. To be fair, however, a conscript may pick up certain skills in the armed forces that may be of use later on when they re-enter civilian life, such as the ability to work in a hierarchical environment, which could be of value in a firm. These positive and negative effects may even cancel each other out. It is therefore not clear a priori whether one should expect conscripts to overperform or underperform in the civilian labor market when compared to non-conscripts.

In a recent paper, Bauer et al. [2009] examine the causal effect of conscription on long-term labor-market performance as measured by, among others, average daily wages. Using an identification strategy based on a discontinuity in the probability to be drafted, they report returns to military service that are between 3 to 8 percent, although these values are imprecisely estimated. They therefore conclude that the observed wage differential between conscripts and non-conscripts cannot be attributed to military service. Their results are revisited here using an alternative identification strategy based on the same data source by comparing the outcome of men and women who belong to two different sets of birth cohorts defined by the law governing conscription in Germany.[6]

The exogenous variation that is exploited by Bauer et al. is a discontinuity in the probability to be drafted between different cohorts of men, which lends itself to a regression-discontinuity design [Lee and Lemieux 2010]. In the current paper, the comparison group is the sample of women during the same time period. The provision in the German constitution (*Grundgesetz*) and the implementing *Wehrpflichtgesetz* (the law governing conscription) only applied to men. Women were totally excluded from military service due to the way Article 12a.4 of the *Grundgesetz* was worded at that time.[7] It was only in January 2001 when the law allowing women to enter the *Bundeswehr* and perform combat duty on a voluntary basis was passed.[8] The differing treatment of men and women allows an alternative strategy to be employed: that of difference-in-differences to identify the causal impact of conscription.[9]

The results of this paper confirm the ones obtained by Bauer et al. While the point estimates are much lower—in this case, about 0.5 percent—the statistical significance of the estimated effect is nowhere close to conventional levels. This finding is robust to changing the sample birth cohorts, and is also validated by additional pieces of evidence that support the maintained hypothesis of the identification strategy. Therefore, the definitive conclusion—based on the current paper and on Bauer et al.—is that CMS had no impact on the labor-market performance of conscripts.

[6]The second aim and contribution of this paper is therefore a comparison of results obtained from alternative identification strategies. The value of replication in the social sciences is perhaps only recently being rightfully appreciated. However, as early as 1969, the American psychologist Donald T. Campbell already noted the importance of replication in social science. In his words, "Because we social scientists have less ability to achieve 'experimental isolation', because we have good reason to expect our treatment effects to interact significantly with a wide variety of social factors many of which we have not yet mapped, we have much greater needs for replication experiments than do the physical sciences." [Campbell 1969]

[7]In part, Article 12a.4 used to read "Under no circumstances may [women] render service involving the use of arms."

[8]This was due to a decision of the European Court of Justice: Case C-285/98, *Tanja Kreil vs. Germany*, [2000] ECR I-69. The Court ruled that Council Directive 76/207/EEC of February 9, 1967 (Equal Treatment Directive) meant that the military profession—including posts that involve the use of arms—had to be accessible to women as well. This resulted in an amendment to the *Grundgesetz* to comply with the ruling. The amended part of Article 12a.4 now reads "[Women] may under no circumstances be required to bear weapons." Compulsory military service, however, is still restricted to men.

[9]See Imbens and Wooldridge [2009] for an overview of modern microeconometric evaluation techniques.

2 Related studies on compulsory military service

In the realm of economics, comparisons between a well-regulated standing army and a "militia" (formed by conscripts) go as far back as Adam Smith [1776] in *The Wealth of Nations*. Smith was of the opinion that national defense was better left to a professional army. In Book V, Chap. I, Part I, he says, "A militia, however, in whatever manner it may be either disciplined or exercised, must always be much inferior to a well-disciplined and well-exercised standing army."[10] More recently, other researchers have taken up the task of identifying the effects of conscription on the conscripts themselves.

This endeavor is not without difficulties. The identification of the causal effect of conscription or military service in general is confounded by a number of factors that are outside the control of the investigator. For example, a regression of some labor-market outcome, say, wages on an indicator of having performed CMS will hardly say much about the effect of conscription other than the magnitude of the correlation between wages and military service. The reason is that selection into the armed forces involves tests that measure a potential conscript's capacity to perform well in the military. Typically, these tests are based on health and mental acuity, and the results of these tests are also often unavailable to the researcher. The problem of course is that health and intelligence are precisely those factors, among others, that simultaneously determine wages and the probability of conscription. Without being able to directly control for such influences, the estimated parameter in the basic regression model will necessarily be biased, and will be of little use for policy.

Various strategies have been employed by applied economists to overcome the identification problem described above. These strategies can be broadly categorized into three econometric evaluation approaches: (i) instrumental-variable strategies, (ii) regression-discontinuity designs, and (iii) difference-in-differences models. The approaches correspondingly identify three different types of treatment effects. They are, respectively, a local average treatment effect [Imbens and Angrist 1994], a local average treatment effect valid for observations that are arbitrarily close to the point of discontinuity [Hahn, Todd and van der Klaauw 2001], and the average treatment effect on the treated [Ashenfelter and Card 1985; Abadie 2005]. Heckman and Vytlacil [2005] have shown that these treatment effects are all versions of what they call the "marginal treatment effect"; the difference depends only over which subpopulation the marginal treatment effect is averaged. The extent to which these

[10] Smith, Adam. An Inquiry into the Nature and Causes of the Wealth of Nations. Edwin Cannan, ed. 1904. Library of Economics and Liberty. Accessed 13 August 2010. `http://www.econlib.org/library/Smith/smWN20.html`.

various treatment effects can be credibly recovered crucially depends on the institutional framework in which the approach is applied.

In a series of related papers [1990; 1993; 1994; 2008; 2009], Angrist and his co-authors exploit the draft lottery in the US to study the effects of military service on World War II and Vietnam War veterans. The outcomes of interest are labor-market performance, health, and educational attainment. Since the probability of being drafted was randomly allocated through a lottery system, the authors could isolate the impact of military service on a variety of outcomes using the assigned lottery number as an instrument for actually performing military service. Imbens and van der Klaauw [1995] and Bauer et al. [2009] use the variation in the probability to be drafted for CMS per cohort in the Netherlands and Germany, respectively, as an instrument for being drafted for CMS.

As mentioned in the introduction, Bauer et al. employ the regression-discontinuity design in their paper (apart from the aforementioned instrumental-variable strategy). Other studies using the same approach include Grenet, Hart and Roberts [2010] and Buonanno [2006] for the UK. In these papers, cohorts of men facing different probabilities to be drafted for CMS are compared against each other to identify the effect of conscription. In its fuzzy (as opposed to a sharp) design, exploiting discontinuities in the conditional probability can be interpreted as using an instrument. That is, a variable indicating on which side of the threshold an observation is can be used as a binary instrument for a variable indicating treatment status. The interpretation of the estimated treatment effect is therefore similar to those obtained using the instrumental-variables approach, with the added caveat that the estimate is only valid for observations close to the cutoff point (although, for an alternative interpretation, see Lee and Lemieux [2010]).

In terms of methodology, this paper is closest to Card and Lemieux [2001], Maurin and Xenogiani [2007], and Cipollone and Rosolia [2007], all of whom exploit the fact that conscription only affected men and not women. In these studies, the authors examine the relationship between conscription of men and the demand for education in the US, France, and Italy, respectively. Card and Lemieux and Maurin and Xenogiani particularly use difference-in-differences models to identify the impact of conscription on educational attainment while Cipollone and Rosolia use the difference between men and women as an instrument to identify peer effects.

3 Institutional setting and data

3.1 Women in the German armed forces

The *Bundeswehr* was created in response to the admission of West Germany into NATO. The law governing conscription was enacted by the parliament in July 1956.[11] All able-bodied men born after June 30, 1937 were required to serve in the *Bundeswehr* (although several exemptions were also applied). The first batch of conscripts entered the service in May 1957. At the beginning, the conscripts were required to serve for 12 months. Since then, the duration of military service has varied over time. The longest duration of service occurred in the period 1962–1971, when conscripts had to serve for 18 months.[12] Since January 2002, the duration of service has been fixed at nine months (three months of basic training or *Allgemeine Grundausbildung* followed by six months posted at a military barracks) although a proposal to shorten it further to six months was just recently implemented.

In Germany, women were only recently allowed to enter the *Bundeswehr* for combat duty. This was due to an amendment to the *Grundgesetz* brought about by a decision of the European Court of Justice. In *Kreil vs. Germany*, the Court held that the Equal Treatment Directive precluded the application of laws, such as what was originally written in the *Grundgesetz*, which proscribed the employment of women in military positions involving the use of arms.[13] Currently, there are 17,388 female soldiers serving in the German armed forces. However, while women were allowed to voluntarily enter the *Bundeswehr* after *Kreil*, only men are compelled to undergo national service. This distinction was affirmed in *Alexander Dory vs. Germany*, where the Court held that European community law "does not preclude compulsory military service being reserved to men."[14]

The exclusion of women from CMS means that one can compare their labor-market performance to that of men's. However, this approach remains inadequate because men and women have, in any case, different outcomes in the labor market, i.e., the observed difference between their performances is not due solely to the fact that men performed military service. One way to solve this

[11] Also, Article 12a.1 of the German constitution or basic law (*Grundgesetz der Bundesrepublik Deutschland*) reads: "Men who have attained the age of 18 years may be required to serve in the Armed Forces, in the Federal Border Guard, or in a civil defense organization."

[12] Officially, conscripts during the period 1986–1988 also had to serve for 18 months but this extension was suspended. See Federal Ministry of Defense, "History of compulsory military service", at `http://www.bmvg.de/`.

[13] See Footnote 8. In fact, this applied not just to the *Bundeswehr* but to the whole public sector as well.

[14] European Court of Justice, Case C-186/01, *Alexander Dory vs. Germany*, [2001] ECR I-2479. The extremely fine distinction, according to the Court, is as follows: "decisions of the Member States concerning the organization of their armed forces cannot be completely excluded from the application of Community law" (reiterating *Kreil*) but "it does not follow that Community law governs the Member States' choices of military organization for the defense of their territory or of their essential interests." [*Dory*, para. 35]

problem is to exploit yet another difference in outcomes due to a policy that was randomly assigned among men. Luckily, one such policy is the *Wehrpflichtgesetz*, whereby only men born after June 30, 1937 are required to undergo national service. Those born on or before—unofficially called the *weißer Jahrgang* or the White Cohort (presumably because they were not tainted by violence)—were totally exempted from national service. These two differences form the basis of the two-group mean-comparison tests that underlie the regression framework described further in Section 4.2.

3.2 Data description

The dataset used for the following analysis is constructed by merging data provided by the Institute for Employment Research (IAB) and supplemental information provided by state pension authorities. The information is available for a 1-percent random sample of the total German population that was gainfully employed and covered by social security for at least one day during the period 1975–1995.[15] For these people, the entire employment history can be recovered.

For purposes of estimation, the following were excluded from the dataset: East Germans, persons who lived outside West Germany at any point, persons born outside the period 1932–1942, professional soldiers, as well as men who performed national service for more than 12 months. It should be noted that, while nowadays, the majority of men performing national service do so in the form of civilian service (*Zivildienst*), most men covered by the period of analysis for this study were engaged in military service. The final sample consists of 57,494 unique individuals, of which 33,138 or about 58 percent are men. There are 24,407 individuals born before July 1, 1937, of which 14,424 are men.

Average cohort characteristics are presented in Table 1. One can see that the average real daily wage is following an upward trend: the 1932 cohort started with 105 DM (€53.69) while those born ten years after in 1942 were earning 112 DM (€57.26) on a daily basis, which is about a 7 percent increase. Education also seems to have become more important for these cohorts over time. In particular, both shares of people with an *Abitur* and a degree from an institution of higher learning (*Universtität* or *Hochschule*) have doubled in the span of a decade.[16] Also worth noting is the fact

[15] The dataset is briefly described here. For a more detailed account, see Bauer et al. [2009] and Bender, Haas and Klose [2000]. A new version of the IAB-S 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/`.

[16] Passing the *Abitur* is the most common way for students to enter the university system. The certificate one obtains after passing the *Abitur* is thus functionally equivalent to a school-leaving certificate which enables students to pursue further education in the form of university training. Those who have been coded as having obtained a university degree means that they have completed tertiary education.

that the number of women in the sample is consistently less than men, both in total and by cohort. Recall that the sample is constructed from the population of Germans who were employed for at least one day during the period of analysis. While the labor-force participation of women in Germany has been increasing over time [Jaumotte 2003], it is still significantly lower than that of men's, then and now.

TABLE 1
AVERAGE COHORT CHARACTERISTICS

Cohort	Variables					
	Size	Abitur	University	Average daily wage	Females	Males
1932	3,609	0.04	0.03	104.80 [39.50]	1,381	2,228
1933	3,616	0.04	0.03	103.83 [39.99]	1,447	2,169
1934	4,509	0.04	0.03	105.05 [40.47]	1,842	2,667
1935	4,808	0.04	0.03	103.74 [40.73]	2,061	2,747
1936	5,023	0.05	0.04	105.46 [40.40]	2,048	2,975
1937	5,393	0.05	0.04	105.35 [40.93]	2,281	3,112
1938	5,819	0.06	0.05	105.03 [40.63]	2,461	3,358
1939	6,287	0.05	0.04	105.47 [41.06]	2,737	3,550
1940	6,747	0.06	0.05	107.60 [41.41]	2,972	3,775
1941	6,412	0.07	0.05	110.22 [42.96]	2,838	3,574
1942	5,271	0.08	0.06	112.49 [43.25]	2,288	2,983

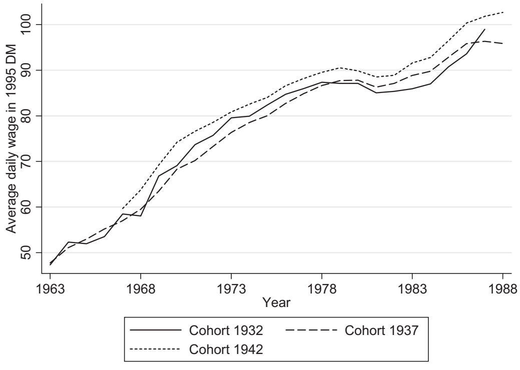
NOTES.—Bracketed numbers are standard deviations. The variables pertaining to educational attainment are indicator variables and therefore the values presented should be interpreted as shares in decimal form. The average daily wage is quoted in 1995 DM.
SOURCE.—Author's calculation based on IAB-S 1975–1995.

Figures 1(a), 1(b), and 1(c) show the evolution of the average daily wage for females, for males, and for everyone in the dataset, respectively.[17] While initial wages vary depending on the birth cohort, it is quite obvious that irrespective of one's sex, daily wages tend to follow a positive trend over the lifecycle. This observation is comparable to other studies that look at cohort-specific labor-market performance over the lifecycle (e.g., Bachmann, Bauer and David [2010] and the references cited therein). The average daily wages per cohort of females have increased over time, as can be seen clearly from Figure 1(a), where the dashed line for the cohort of women born in 1942 strictly dominates the equivalent lines for the other two cohorts. This can probably be attributed to the gradual institutional changes in German society that define the role of women and how they can productively participate in society.

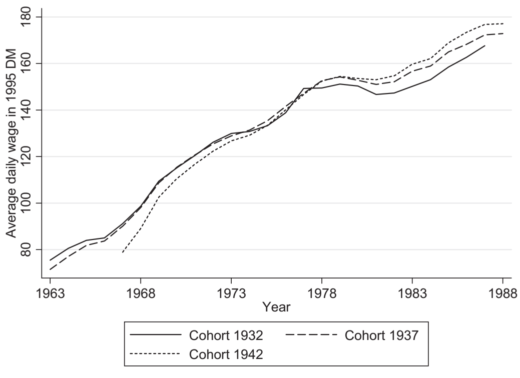
However, one can also see in Figures 1(a) and 1(b) that women still earn significantly less than

[17] Only the cohorts 1932, 1937, and 1942 are drawn for aesthetic reasons. The other cohorts follow a similar trend of rising average daily wage over their careers. The complete graphs can be made available upon request although it will be difficult to distinguish one cohort from the others as the lines overlap substantially. Note also the difference in scales in the ordinates.

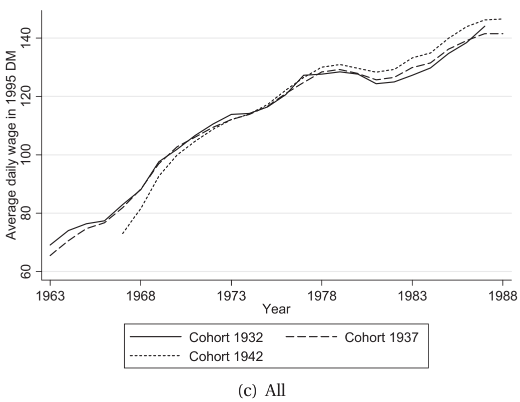
FIGURE 1
EARNINGS PROFILES BY BIRTH COHORT



(a) Females



(b) Males



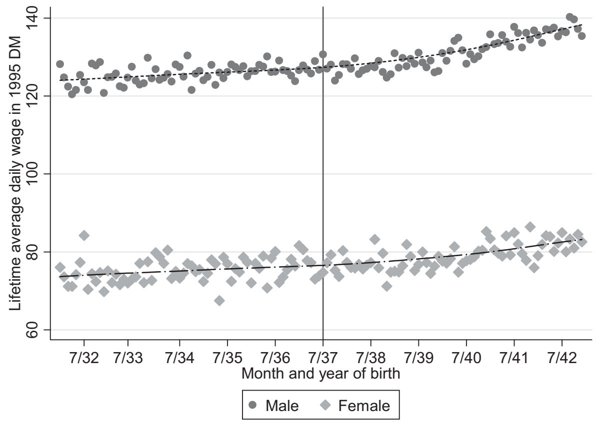
(c) All

NOTES.—For clarity, only the cohorts 1932, 1937, and 1942 are presented here. Similar trends can be observed for the remaining cohorts.

SOURCE.—Author's own illustration based on the IAB-S 1975–1995.

men. At 46 years old, the average woman born in 1942 was earning a little over 100 DM (€51.13). Her male counterpart was already earning roughly the same amount in his late 20s. An alternative way to represent this is shown by Figure 2, where the difference in lifetime average daily wages[18] between men and women is clearly seen. The points on the graph represent the mean lifetime average daily wage computed for each month–year cohort and separately for each sex. For those born before July 1, 1937, the difference remained constant at about 50 DM (€25.56). While both groups show an increasing lifetime average daily wage by cohort—consistent with what has already been shown in Figure 1—the evolution of this measure of labor-market performance seems to have accelerated for men as indicated by the slightly steeper line segment for cohorts born after the threshold date.

FIGURE 2
LIFETIME AVERAGE DAILY WAGE BY SEX AND BIRTH COHORT



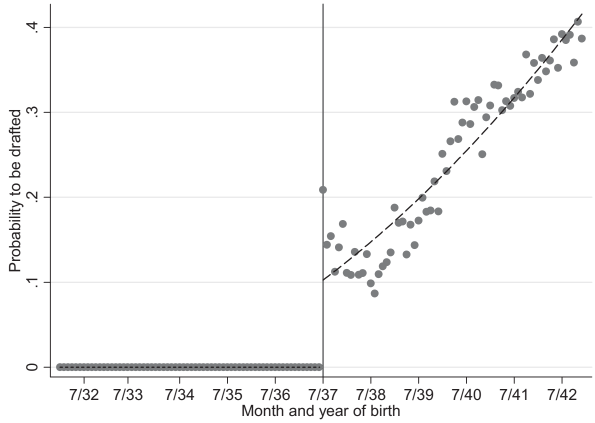
NOTES.—The points on the graph represent the mean lifetime average daily wage by month–year cohort and sex. Dashed lines are lowess curves estimated separately for each sex.
SOURCE.—Author's own illustration based on the IAB-S 1975–1995.

Figure 3 show the probability of being drafted by month of birth for males. The points on the graph represent the share of men who were drafted for each month–year cohort. For the White Cohort and for women, conscription was unnecessary and therefore the probability to be drafted was zero. While this was the case for both men and women born before the threshold date, men born after June 30, 1937 exhibited an increasing probability to be drafted for military service. In fact, those born in 1942 were facing a conscription probability of roughly 40 percent, most likely due to the demands of the Cold War. The relatively low conscription rates for the birth cohorts 1937–1939 (between about

[18] Lifetime average daily wage is computed as the cumulative real earnings from 1963 to 1988 divided by the total days of employment.

10–20 percent) is due to the special exemptions accorded to certain men. These were especially relevant at the time the law on conscription took effect. For example, sole sons of soldiers killed in World War II or those who lost all their siblings qualified for such an exemption.

FIGURE 3
PROBABILITY TO BE DRAFTED FOR MILITARY SERVICE BY MONTH OF BIRTH



NOTES.—The points on the graph represent the share of men who were drafted by month–year cohort. Dashed lines represent the quadratic fit estimated separately for both sides of the threshold.
SOURCE.—Author's own illustration based on the IAB-S 1975–1995.

4 Estimation strategy and results

4.1 Descriptive regressions

The figures and tables discussed in Section 3.2 can also be represented in a regression framework. For this purpose, one could regress the (log) average daily wage, $\ln w_i$, against indicator variables for a person's birth cohort and sex. More specifically, consider the following regression model, where $i = 1, 2, \ldots, N$ indexes individuals:

$$\ln w_i = \alpha + \delta m_i + \boldsymbol{\lambda}' \mathbf{c}_i + \epsilon_i, \tag{1}$$

where m_i is a dichotomous variable that equals 1 if the individual is male, $\mathbf{c}_i$ is a vector of variables indicating a person's birth cohort (i.e., cohort fixed effects), and ϵ_i is a typical stochastic disturbance term; α, δ, and $\boldsymbol{\lambda}$ are parameters or vectors of parameters to be estimated.

The regression estimates based on Equation (1) are presented in Table 2, specifically Column (2).

For the sake of completeness, Equation (1) is supplemented with additional covariates that control for a person's level of educational attainment. This is shown in Column (3) while Column (1) is a parsimonious regression of the (log) average daily wage rate only on cohort indicator variables and a constant. The regressions confirm what can already be gleaned from the figures and tables. That is, (i) later birth cohorts and (ii) men exhibit a higher average daily wage. Moreover, one learns that, as expected, there are significant returns to completing an apprenticeship, having an *Abitur* (completing secondary education), and obtaining a university degree. These translate to a wage premium of about 19, 30, and 39 percent, respectively, where the reference category is having no secondary-education degree or its equivalent.

Restricting the sample only to men makes it worthwhile to estimate the following variant of the regression model:

$$\ln w_i = \alpha + \gamma d_i + \rho n_i + \epsilon_i, \tag{2}$$

where d_i is a binary variable that equals 1 if the man performed CMS and n_i is an indicator that equals 1 if the individual does not belong to the White Cohort; γ and ρ are additional parameters to be estimated. Using Equation (2), one obtains the following estimates: $\widehat{\alpha} = 4.559$, $\widehat{\gamma} = 0.351$, and $\widehat{\rho} = -0.019$, with associated standard errors of 0.003, 0.004, and 0.004, respectively. Taken at face value, conscription is associated with a 35-percent wage premium.

The estimate of γ above is certainly too high for it to be interpreted as the causal effect of conscription; it is higher than the estimated returns to secondary education. However, this is also not surprising, considering that none of the possible biases have been taken into account simply by using Equation (2). For instance, selection into the *Bundeswehr* involves a physical and mental examination. Those who fail such a test—and therefore, to more likely perform worse in the labor market later on—are exempted from conscription. Conversely, those who pass (i.e., those who are healthier and mentally superior) are more likely to excel anyway in the labor market compared to their peers. In other words, to the extent that both intelligence and health have a positive impact on labor-market performance, men who performed CMS can reasonably be expected to have earned higher wages—but, importantly, not necessarily because of conscription.

TABLE 2

DESCRIPTIVE REGRESSIONS OF (LOG) AVERAGE DAILY WAGE

Variables	(1)	(2)	(3)
1933	−0.012	−0.003	−0.007
	[0.010]	[0.008]	[0.008]
1934	−0.001	0.015*	0.007
	[0.010]	[0.008]	[0.008]
1935	−0.017*	0.010	−0.001
	[0.010]	[0.008]	[0.008]
1936	0.004	0.018**	0.005
	[0.010]	[0.008]	[0.007]
1937	0.001	0.024***	0.007
	[0.010]	[0.008]	[0.007]
1938	−0.002	0.022***	−0.003
	[0.010]	[0.008]	[0.007]
1939	0.002	0.033***	0.010
	[0.010]	[0.007]	[0.007]
1940	0.024***	0.058***	0.033***
	[0.009]	[0.007]	[0.007]
1941	0.046***	0.081***	0.049***
	[0.010]	[0.007]	[0.007]
1942	0.068***	0.098***	0.061***
	[0.010]	[0.008]	[0.007]
Male	—	0.582***	0.536***
		[0.003]	[0.003]
Apprenticeship	—	—	0.193***
			[0.003]
Abitur	—	—	0.297***
			[0.017]
University	—	—	0.085***
			[0.018]
Constant	4.563***	4.204***	4.113***
	[0.008]	[0.006]	[0.006]
R^2	0.0029	0.3924	0.4417
F-statistic	16.64	2,942.06	2,973.23
Observations	57,494	57,494	57,494

NOTES.—The results are based on OLS regressions of the (log) average daily wage against a set of covariates as described in Equation (1). Columns (1), (2), and (3) differ only in the covariates. The reference cohort, sex, and educational attainment are 1932, female, and no secondary-education degree, respectively. Bracketed numbers are heteroskedasticity-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

SOURCE.—Author's calculation based on IAB-S 1975–1995.

4.2 Identification of the effect of conscription

To recover the impact of conscription on a generic outcome variable y_i, one could execute a standard difference-in-differences (DD) model of the following form:

$$y_i = \alpha + \delta m_i + \theta n_i + \tau (m_i \times n_i) + \epsilon_i. \tag{3}$$

The parameters or vector of parameters α, δ, θ, and τ are then typically estimated via ordinary least squares. Denote $(m_i \times n_i)$ as the DD regressor; the estimated parameter $\widehat{\tau}$ represents the treatment effect. The maintained hypothesis here is that the difference in y_i between men and women would have remained the same in the absence of the treatment.[19]

The main advantage of Equation (3) over Equations (1) and (2) is that the former model eliminates any time-invariant unobserved differences between the two sexes, and consequently, any biases that may arise out of such unobservables. One could go one step further and account for the observed compositional differences between males and females by augmenting Equation (3) with additional covariates:

$$y_i = \alpha + \delta m_i + \theta n_i + \tau (m_i \times n_i) + \boldsymbol{\beta}' \mathbf{x}_i + \epsilon_i, \tag{4}$$

where $\mathbf{x}_i$ is a vector of control variables and its coefficient $\boldsymbol{\beta}$ is a vector of parameters to be estimated. In the conditional DD representation of Equation (4), the impact of CMS is corrected for the influence of time-varying observable individual characteristics, albeit in a linear fashion. Moreover, the addition of covariates should improve the precision of the estimates in terms of lower standard errors [Galiani, Gertler and Schargrodsky 2005].

The results of the DD estimation, where y_i is the (log) average daily wage as in Section 4.1, are presented in Table 3. DD estimates with year fixed effects—necessitating the deletion of θn_i from the model—are also presented. That is, the following models are estimated, where Equation (6) merely

[19] An alternative identification strategy is that employed in Bauer et al. [2009], where the regression-discontinuity design of military service in Germany is exploited. Using men born around the threshold date of date of June 30, 1937, the assumption is that men born on either side of the cutoff are different only in that the non-White Cohort had a positive probability of being drafted.

corrects for additional covariates:

$$\ln w_i = \alpha + \delta m_i + \tau (m_i \times n_i) + \boldsymbol{\lambda}'\mathbf{c}_i + \epsilon_i \tag{5}$$

$$\ln w_i = \alpha + \delta m_i + \tau (m_i \times n_i) + \boldsymbol{\lambda}'\mathbf{c}_i + \boldsymbol{\beta}'\mathbf{x}_i + \epsilon_i, \tag{6}$$

where $\boldsymbol{\lambda}$ is a parameter vector to be estimated. The results are in Columns (3) and (4) of Table 3, respectively. Regressions based on Equations (3) and (4) are presented in Columns (1) and (2), respectively. The covariates included in the vector $\mathbf{x}_i$ are the following: (i) indicator variables for the quarter of birth, where the first quarter is the reference category; and (ii) indicator variables for the educational attainment (completed an apprenticeship, *Abitur*, or university), where the reference category is having no secondary-education degree.

TABLE 3
DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE TREATMENT EFFECT

Variables	(1)	(2)	(3)	(4)
DD regressor	−0.001	0.007	−0.001	0.005
	[0.007]	[0.006]	[0.006]	[0.006]
Male	0.582***	0.531***	0.583***	0.533***
	[0.005]	[0.005]	[0.005]	[0.005]
Non-White Cohort	0.045	0.021***	—	—
	[0.006]	[0.006]		
Apprenticeship	—	0.194***	—	0.193***
		[0.003]		[0.003]
Abitur	—	0.299***	—	0.298***
		[0.017]		[0.017]
University	—	0.087***	—	0.085***
		[0.018]		[0.018]
Constant	4.215***	4.119***	4.203***	4.115***
	[0.005]	[0.005]	[0.007]	[0.007]
Cohort fixed effects	None	None	All	All
Quarter of birth	None	All	None	All
R^2	0.3901	0.4403	0.3924	0.4418
F-statistic	11,217.83	4,780.17	2,848.36	2,412.83
Observations	57,494	57,494	57,494	57,494

NOTES.—Columns (1), (2), (3), and (4) correspond to Equations (3), (4), (5), and (6), respectively. Bracketed numbers are heteroskedasticity-robust standard errors clustered at the date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Author's calculation based on IAB-S 1975–1995.

Across all specifications, one cannot reject the null hypothesis that the treatment effect is equal to zero. That is, there is no significant effect of conscription on the average daily wages of the cohort of men born on or after July 1, 1937. Take the most-preferred estimate presented in Column (4) based on Equation (6). Here, $\widehat{\tau}$ is equal to 0.005, which is much less than the naive estimate $\widehat{\gamma} = 0.351$ obtained from Equation (2). The DD estimates in Table 3 are much more reasonable compared to the estimates that ignore the selection into treatment. This highlights—in a rather extreme way, considering that $\hat{\tau}$ is less than 2 percent of $\hat{\gamma}$—the importance of correcting for selection bias introduced by the institutional arrangements surrounding the induction of conscripts into the *Bundeswehr*.

Nevertheless, all the other estimated coefficients presented in Table 3 are significant at the 1-percent level (except the cohort indicator in Column (1)) and are of reasonable magnitudes. For example, one can see that there are positive returns to education, which are similar to the estimates presented in Table 2. This is also the case for the estimate of the wage gap between men and women which, in Column (4), is about 53 percent (and consistent with the reported wage gap at that time; see, e.g., Maier [2007]). Correcting for the time-invariant unobservables that simultaneously affect the probability of serving in the *Bundeswehr* and future labor-market performance, such as innate health and intelligence, seems to have affected only the estimates of the treatment effect (i.e., $\hat{\tau}$ and $\hat{\gamma}$, although the latter is obviously biased).

The parameter of interest, τ, is typically interpreted in DD models as the average treatment effect on the treated (ATET) because it is the coefficient of the indicator variable formed by interacting a treatment indicator with an indicator identifying the treatment period (before or after). Here, however, the variable n_i only identifies whether one was born before the threshold date of June 30, 1937, not whether or not one served in the *Bundeswehr*. In this particular context, τ is thus properly interpreted as the intention-to-treat effect (ITTE).

The ITTE is less than the ATET unless all men born on or after July 1, 1937 served in the *Bundeswehr*, in which case the estimates of the ITTE and the ATET would be equal to each other. More specifically, ATET is equal to $\tau/(1-r)$, where r is the share of men who do not belong to the White Cohort and served in the *Bundeswehr*. Therefore, the estimate of the ATET is directly proportional to the compliance rate, holding the estimated ITTE constant. The number of men in the sample born after the cutoff date is $18,714$, of which $4,524$ performed CMS. This implies that the compliance rate is about 24 percent. Using the aforementioned equality, the recovered ATET of conscription is thus 0.006.

While the definitions of and relationship between the ATET and ITTE are somewhat mechanical, their interpretations are not. This crucially depends on the question that is posed, i.e., in which effect one is principally interested. If one is concerned with the effect of the introduction of CMS, then τ

can be directly construed as the ATET. If the interest lies instead with the effect of conscription itself and not merely its introduction, then τ can only properly be interpreted as the ITTE.

The set of point estimates in the current instance is much lower than what was previously obtained in Bauer et al. [2009], although both are statistically insignificant. This is likely due to a variety of reasons, not the least of which is the difference in comparison groups. Here, women constitute the control group while the previous paper uses men born just before the threshold date. Moreover, because of the instrumental-variable nature of the regression-discontinuity design, the estimate is only valid for those men who are arbitrarily close to the threshold and, even then, only for those whose treatment status is influenced by their date of birth. This is most certainly a subset of men for whom the estimates in this paper are valid. Nevertheless, it remains comforting that the statistical significance is unchanged when alternating identification strategies.

4.3 Further results

In order to obtain a consistent estimate of the treatment effect, it must be the case that the relative outcomes for men and women from the same birth cohort would have followed the same trend but for a sex-specific treatment or policy change. In other words, had the *Wehrpflichtgesetz* not been enacted into law, the difference in outcomes between men and women born in the same year would have remained the same for those born before and after the threshold date. If this parallel- or common-trends assumption is not likely to hold, the parameter estimate is unfortunately rendered inconsistent.

This is an assumption about the counterfactual and therefore cannot be statistically tested. However, there are strong indications that the evolution of average daily wages for both men and women would have followed the same trend if obligatory military service were not introduced in Germany. Figure 2 already indicates that average daily wages were exhibiting the same pattern for men and women born before the cutoff date. Evidence from this visual inspection can at least be statistically verified. The idea is that, if the average daily wages for the two sexes were following the same trend before the policy intervention, it is much more likely that they would have continued to develop similarly in the absence of such an intervention.

To this end, one could drop all individuals born after the threshold date. Then, one could perform a regression based on Equation (5) but with an altered definition of n_i to simulate a policy change in July 1933. If there were no sex-specific treatment at that time, the estimate of the coefficient of the DD regressor should be insignificant. These "placebo regressions" could be performed for each subsequent year until 1936. The results of such regressions are presented in Table 4. None of the simulated policy changes were found to have a significant impact on average daily wages. This means that before the actual implementation of the *Wehrpflichtgesetz*, the relative labor-market outcomes for men and women (in terms of average daily wages, at least) were not on divergent paths.

TABLE 4
PLACEBO REGRESSIONS BASED ON SIMULATED POLICY CHANGES

Variables	1933	1934	1935	1936
Placebo	−0.001	0.000	0.006	−0.011
	[0.010]	[0.008]	[0.008]	[0.009]
Male	0.583***	0.583***	0.580***	0.585***
	[0.009]	[0.007]	[0.006]	[0.005]
Constant	4.203***	4.203***	4.205***	4.202***
	[0.010]	[0.009]	[0.008]	[0.008]
Cohort fixed effects	All	All	All	All
R^2	0.3869	0.3869	0.3869	0.3869
F-statistic	1,883.69	1,883.77	1,885.32	1,884.68
Observations	24,407	24,407	24,407	24,407

NOTES.—Placebo regressions are based on Equation (3). Policy changes were simulated for the years 1933, 1934, 1935, and 1936. Only individuals born before July 1, 1937 are included in the regressions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Author's calculation based on IAB-S 1975–1995.

Nevertheless, one should not discount the fact that the role of women in society was also gradually changing during that time. Not only were wages steadily increasing (although still far below their male counterparts), labor-force participation was also evolving and, concomitantly, so was fertility [Bredtmann, Kluve and Schaffner 2009; Jaumotte 2003]. If performing *Wehrdienst* had a positive impact on men's labor-market performance, but women's labor-market performance was improving more rapidly than men's, the DD approach would underestimate the impact of conscription. Of course, men's participation rate might also have been changing towards the opposite direction (see, for example, Fitzenberger, Schnabel and Wunderlich [2004]). These developments in the labor market suggest that one should be cautious in using women as a comparison group for men since these sex-specific developments may be driving the result.

Although the placebo regressions have already established that the difference in outcomes between men and women remained relatively stable over the pre-treatment period, one could alleviate any residual concerns by taking yet another approach to address this issue. Hence, the sample is refined by trimming both sides of the cohort spectrum, incrementally reducing the sample size and running the same regressions that generated Table 3. The rationale behind this approach is that changes in women's labor-force participation and fertility are gradual developments brought about by changes in society's institutions and mores, which themselves normally transform at a glacial pace. By narrowing the period of analysis from the original 1932–1942 to incrementally shorter periods in between, the bias introduced by such developments will unlikely materialize.

Furthermore, it has been shown that DD estimates over long sample periods can seriously suffer from underestimated standard errors [Bertrand, Duflo and Mullainathan 2004]. This is due to the serial correlation in the error terms, which, when unaccounted for, translates to rejections of the null hypothesis of no effect that occur more often than it correctly should. In this study, unobservables may persist over time, affecting adjacent cohorts similarly. The error term can thus reasonably be expected to be correlated over cohorts. A proposed solution is to collapse the period of analysis into just two periods: one before and one after the policy intervention. This limits the scope of the problem due to serial correlation. Narrowing the sample band described above therefore addresses this issue as well.

Regression results based on a strategy of trimming the sample are presented in Table 5. The estimated coefficients remain relatively stable across all specifications, i.e., close to zero. These are comparable to the previously estimated coefficients based on the full sample presented in Table 3. Of course, the reduction in the sample size consequently leads to higher standard errors but the increase is not dramatic. One could thus be confident with the estimated treatment effects. The ITTE of 0.005 from Column (4) of Table 3 and its implied ATET of 0.006 fall roughly in the middle of the range of estimates presented both in Tables 3 and 5. These pieces of evidence lend credibility to the appropriateness of the identification strategy employed in this study.

Finally, heterogeneous treatment effects are examined by estimating the DD model using only individuals without a secondary-education degree. This group is special because they represent the low-skilled segment of the labor force. Absent the training provided by formal educational institutions, the *Bundeswehr* may act as a substitute by instilling discipline and perhaps certain other skills that might be relevant for the civilian labor market later on. The results of these regressions are presented in Table 6, where no significant impact of conscription on low-skilled workers is recovered.

TABLE 5
REGRESSIONS BASED ON TRIMMED SAMPLES

Variables	1934–1940				1936–1938			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DD regressor	0.004	0.009	0.003	0.006	0.002	0.005	0.000	0.002
	[0.008]	[0.007]	[0.007]	[0.007]	[0.012]	[0.012]	[0.009]	[0.009]
Male	0.579***	0.530***	0.580***	0.532***	0.582***	0.533***	0.583***	0.535***
	[0.006]	[0.006]	[0.006]	[0.005]	[0.009]	[0.008]	[0.008]	[0.008]
Non-White Cohort	0.018***	0.003	—	—	0.000***	−0.010	—	—
	[0.007]	[0.007]			[0.011]	[0.010]		
Apprenticeship	—	0.193***	—	0.193***	—	0.197***	—	0.197***
		[0.004]		[0.004]		[0.006]		[0.006]
Abitur	—	0.308***	—	0.309***	—	0.315***	—	0.315***
		[0.020]		[0.020]		[0.032]		[0.032]
University	—	0.096***	—	0.095***	—	0.097***	—	0.097***
		[0.022]		[0.022]		[0.035]		[0.035]
Constant	4.222***	4.123***	4.220***	4.123***	4.225***	4.124***	4.228***	4.124***
	[0.005]	[0.006]	[0.007]	[0.008]	[0.007]	[0.009]	[0.005]	[0.009]
Cohort fixed effects	None	None	All	All	None	None	All	All
Quarter of birth	None	All	None	All	None	All	None	All
R^2	0.3894	0.4412	0.3901	0.4417	0.3895	0.4439	0.3895	0.4439
F-statistic	7,606.53	3,308.08	2,872.72	2,141.86	3,135.28	1,428.51	2,352.24	1,288.30
Observations	38,586	38,586	38,586	38,586	16,235	16,235	16,235	16,235

NOTES.—Regressions are similar to those depicted in Table 3 except that smaller samples are used. Columns (1)–(4) are based on a sample from 1934–1940 while Columns (5)–(8) are based on a sample from 1936–1938. Bracketed numbers are heteroskedasticity-robust standard errors clustered at the date of birth. For brevity, the regression results based on the periods 1935–1939 and 1936–1938 (excluding 1937) are not presented here. The results are similar to what are shown above. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Author's calculation based on IAB-S 1975–1995.

TABLE 6
IMPACT OF CONSCRIPTION ON LOW-SKILLED INDIVIDUALS

Variables	(1)	(2)	(3)	(4)
DD regressor	−0.009	−0.009	−0.011	−0.012
	[0.010]	[0.010]	[0.009]	[0.009]
Male	0.542***	0.542***	0.544***	0.543***
	[0.007]	[0.007]	[0.013]	[0.007]
Non-White Cohort	0.033	0.033***	—	—
	[0.008]	[0.008]		
Constant	4.110***	4.113***	4.110***	4.113***
	[0.006]	[0.007]	[0.011]	[0.011]
Cohort fixed effects	None	None	All	All
Quarter of birth	None	All	None	All
R^2	0.3510	0.3511	0.3525	0.3527
F-statistic	3,920.80	1,962.47	990.75	793.39
Observations	19,361	19,361	19,361	19,361

NOTES.—Columns (1), (2), (3), and (4) correspond to Equations (3), (4), (5), and (6), respectively. Only individuals with no secondary-education degree are included in the regression. Bracketed numbers are heteroskedasticity-robust standard errors clustered at the date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
SOURCE.—Author's calculation based on IAB-S 1975–1995.

5 Conclusion

This paper revisits compulsory military service in two senses. First, it looks back at the labor-market performance of men who underwent conscription when it was introduced in Germany during the second half of the 20$^{\text{th}}$ century. Second, it reexamines the results first reported by Bauer et al. [2009] by subjecting the data to an alternative but equally plausible identification strategy. The natural experiment here is anchored on a combination of laws which exempted women and a particular cohort of men called the *weißer Jahrgang* from military service.

Difference-in-differences estimates are computed by comparing the average daily wages of men and women born before and after the date of birth on which conscription was enforced. The point estimate of the semi-elasticity of the average daily wage is 0.5 percent but with an associated standard error of 0.6. The results therefore indicate that conscription had no significant impact on the labor-market performance of draftees in terms of the average daily wage. This is consistent with the results found in Bauer et al. and Grenet, Hart and Roberts [2010]. Both studies find that conscripts were neither penalized nor rewarded in the civilian labor market.

Current policy discussions in Germany involve serious proposals to abandon conscription altogether or to at least substantially change the way the armed forces are staffed and organized. This paper adds to that discussion by highlighting the effect of conscription on precisely those men that have been drafted to serve their country once before. Thus, while the paper reflects on the past to comprehend the present, it is also forward-looking in that it contributes to policy conclusions that shape Germany's future national defense strategy.

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