Did you serve? New evidence on the causal effect of conscription on wage in Germany

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RRELIMINARY DRAFT

Abstract

Using German administrative data, I estimate the causal effect of conscription on men's early career wage. My quasi-experimental design exploits a discontinuity in the probability of having served induced by the school start cutoff in connection with the suspension of conscription in 2011. Comparing men born in a narrow window around the cutoff, I find that conscription has a positive effect on wage for men from the highest schooling track. Falsification tests using men and women of different birth years show that the effect is unique to men and the birth year in which the school start cutoff induced a discontinuity in the exposure to conscription. I test several mechanisms and find suggestive evidence that conscription causes men to select into jobs requiring more team work, which are on average higher-paying.

Keywords: compulsory service, military service, draft, local randomization

JEL Codes: J24, J31, H56

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1 Introduction

I study the effect of conscription on early career wage in Germany. Conscription was active in Germany from 1957 until its suspension in 2011. In this period of more than 50 years, about 11 million young men served. An army is a public good that enables national defense and deterrence strategies. Beside its primary role, the German *Bundeswehr* provided administrative assistance in emergency situations such as the 1962 flood in Hamburg, the 2021 flood in the Ahrtal, and during the COVID-19 pandemic. Especially relevant to the German setting has been the idea that conscription can contribute to societal cohesion and a stable democracy by preventing the army of becoming a "state within the state". Maintaining a functional army is costly for a society in terms of public expenditures and opportunity costs. On the individual level, mandatory service imposes a disutility on a subset of men (and potentially women) who would have otherwise preferred not to serve. Among other economists, Hubers and Webbink (2015) describe compulsory service as an unequally distributed implicit income tax and highlight the often inefficient job-worker match in conscription systems. From a policy perspective it is relevant to learn about the wage effects of conscription, and how individual costs are distributed among men and women of different educational and socio-economic backgrounds.

My main contribution is to provide recent quasi-experimental evidence on the effect of conscription on wage in Germany using a large administrative data set. I show that conscription has a positive effect on early career wage for men from a high educational background. My quasi-experimental design exploits a discontinuity in the probability of having served induced by the school start cutoff in connection with the suspension of conscription in 2011. In my regression discontinuity design, I look at men born in a small window around the school start cutoff: Men born in June 1991 graduate after 13 years of school in 2010, still facing a positive probability of being drafted. Men born in July start and finish school one year later and were no longer drafted because of the suspension of conscription in 2011. Using population data, I show that other confounding graduation cohort differences can be ruled out.

In placebo test, the effect does not replicate for women born in the same year, who were not subject to conscription, nor for men or women born in adjacent years, in which there was no discontinuity in the probability of having served.

More broadly, I add to the literature on the causal effect of peacetime compulsory military service on wage in European countries. This is in contrast to the literature on the effect of military service during war time like for example in Angrist et al. (2011), using the Vietnam draft lottery for identification. Findings on the effects of peacetime compulsory military service in European countries are heterogeneous, which is not surprising in the light of the different settings characterized by the design of the conscription system, the education system, and the incentives provided by the legal framework. For post-war Germany and Great Britain, Bauer et al. (2012) and Grenet et al. (2011) find no causal effect of military service on subsequent wage. For Dutch men born around 1959, Hubers and Webbink (2015) identify a long-term negative wage effect, supporting earlier findings from aggregate analyses by Imbens and Van Der Klaauw (1995).

Given the institutional setting, the effect of conscription often depends on the educational background and ability of conscripts. For for high-ability Danish men born 1976-83, military service can lead to lower wage through delayed studies and forgone labor market experience (Bingley et al., 2020). For high-ability Swedish men, however, military service increases wage and the probability of rising to a civilian leadership position (Grönqvist and Lindqvist, 2012; Hjalmarsson and Lindquist, 2019). An important feature of the Swedish setting is that high-ability men are screened to undergo officer training, facing a positively selected peer group and a highly specialized training. Swedish men of low-education with a criminal history incur negative wage effects because of negative peer effects. In Portugal, men working in low-skilled jobs benefit from military service (Card and Cardoso, 2011).

I contribute recent evidence on the wage effect of conscription for German men with the highest schooling degree. Bauer et al. (2012) and Bauer et al. (2014) analyze the effect of military service on wage and educational outcomes around the time

conscription was started in post-war Germany. Since then, the legal framework and the nature of service have changed, calling for more recent evidence on the labor market effects of conscription in Germany. While in post-war Germany, essentially all conscripts served in the military, in the post-millennial years more men conducted alternative service in the health or social sector than actually went to the military. Additionally, it became harder for young men to enroll into university as a mechanism to avoid the draft.

Eberl et al. (2022) and Puhani and Sterrenberg (2022) address the effect of compulsory service on socio-economic outcomes in Germany for more recent settings using survey data. The key challenge of the literature has been to identify exogenous variation in service in combination with large data sets that accurately measure both service and wage. Naive comparisons between men who served and did not serve are biased from selection in two directions. One the one hand, healthier men are more likely to serve and also have better labor market outcomes. This is accounted for by Puhani and Sterrenberg (2022), who control for mustering categories within the subsample of men who are categorized as fit for service. On the other side, men with more education might be more likely to defer or avoid the draft. Especially men with highly-educated parents, e.g. lawyers or doctors, may have had more knowledge about how to avoid the draft. In the light of declining draft rates and the perceived unfairness of the conscription system, incentives and mechanisms for avoiding the draft played an important role. Pro-social and risk preferences play into avoidance behaviors and are also correlated with occupational choices and wage.

Another source of endogeneity arises from the selection into alternative rather than military service. Men conducting alternative service came from higher educational backgrounds (Schneider, 2003), also because conscientious objection required an extensive written explanation. Additionally, the timing of conscientious objection played an important role in the probability of service. Men who wanted to avoid service but would prefer alternative to military service could game the timing of their application for conscientious objection in order to avoid the draft.

1.1 Individual costs and benefits of conscription

Possible costs and benefits of compulsory service on educational and labor market outcomes are presented, for example, by Grenet et al. (2011) and Bach (2017). Important disadvantages can occur by the delay in tertiary education and labor market entry. This is not only because of the time in service, but also because of waiting or transitory periods. Also, the insecurity about being drafted in the near future could be a disadvantage for men in the job search and hiring processes. Another possible disadvantage of service is the depreciation of human capital during service, raising the effort costs of entering university studies after service. Theoretically, the effect of compulsory service on educational outcomes could go in both directions[1]. Empirical evidence for Turkey, France and Germany suggests that conscription causes men to acquire more education in order to avoid the draft (Torun and Tumen, 2016; Maurin and Xenogiani, 2007; Bauer et al., 2014), but empirical evidence on the effect of conscription on educational attainment is mixed (Di Pietro, 2013).

With respect to labor market outcomes, the acquisition of skills may be (though unproven) a positive side effect of compulsory service, especially in light of the rising importance of social skills in the labor market as a whole (Deming, 2017) and in particular to leadership (Hansen et al., 2021). Men of the highest schooling track might otherwise gain no or little practical experience when entering the labor market after university. In line with this, some institutions offering alternative service positions in Germany reported that conscripts from the highest schooling track had more difficulty to integrate into the work-related environment than those with vocational training (Becker et al., 2011). Skills acquired by a practical experience could not only be an advantage at labor market entry, but also increase success during university studies, generating more long-lasting advantages.

Other side effects of compulsory service include peer effects, shocks to mobility,

[1]Other arguments from the literature are that men who served acquire more education by trying to catch up on forgone earnings and the loss in human capital through service (Bauer et al., 2014). On the other hand, men could be demotivated from acquiring university education because service decreases the amortization period for human capital investments (Bach, 2017). Additionally, service might lead men to want to enter the labor market earlier, or seek paid vocational training, simply because of age effects.

and signaling. Peer effects in the German military for men from the highest schooling track could generally be positive or negative. The quarter in which men were drafted is linked to peer composition, likely increasing ex-ante inequalities. The experience of compulsory service could also increase geographical mobility. In conservative sectors such as law, signaling could still pay a role in hiring decisions even in the years shortly before the suspension of conscription (Spreng and Dietrich, 2006).

2 Institutional background

2.1 The German schooling system

Important to my identification strategy are the basic features of the German schooling system. At the time, there was a uniform school start cutoff in all German federal states (Faust, 2006). Children born in June had to start school in the calendar year they turn 6. Children born in later months could start school one year later at age 7. After 4 years of primary school, children attend one of three secondary school tracks. The general university entrance qualification (Abitur) is obtained by graduating from the highest secondary school track (Gymnasium). The duration of Gymnasium was 9 years in most federal state; the lower secondary school tracks consisted of of 5–6 grades.

2.2 Conscription in Germany before the suspension

Figure C2 shows a stylized model of the conscription process in Germany in the years shortly before the suspension. After turning 17, young men were summoned by the district recruiting office for mustering. At the mustering, men were assigned a category: fit for service, unfit, or temporarily unfit. Men categorized as unfit were exempt from compulsory service and performed neither military nor alternative service ("Zivildienst"). If fit for serve, men either served in the military, or did alternative service, or were never drafted into the military because of low demand for conscripts. Alternative service was performed by conscientious objectors, mostly

in the health and social sector.

Conscription age was from 18 to 23, or 25 in case of previous deferments. Service could be postponed in some cases, most importantly during secondary school and vocational training. Contrary to other European countries, university studies did not generally qualify for deferment, but only as of the third semester. There were a number of special exemptions. For example, third sons were, upon request, exempt from service if their two elderly siblings had already served. Also men serving in the law enforcement police were not drafted.

Conscription into military service was organized in a quarterly pattern with service starts in January, April, July, and October. Men graduating from the highest schooling track typically started military service in July, followed by October. In the January and April quarters, the share of men from lower educational backgrounds was higher. The first three months of military service consisted of basic military training (*Grundausbildung*), which was common to all conscripts in the military.

Since October 2004, total service duration was 9 months for both military and alternative service. Shortly before the suspension of compulsory service, duration was reduced to 6 months[2]. Regardless of the regular service duration, conscripts could extend the duration of their service voluntarily.

Conscientious objectors could arrange their alternative service bilaterally with institutions offering alternative service positions. The majority of services consisted in helping roles in the care of elderly, disabled, or children, in hospitals, social welfare organizations, ambulance services, or daycare facilities (Kreuter, 2010). More seldom were alternative service in fields like environment protection. In times of demographic change and personnel shortage in care jobs, conscientious objectors in the social and health care sector were in high demand.

There were also voluntary service programs accepted as surrogates to alternative service ("surrogate services"). Young men and women could participate in these

[2]The last conscripts who started military service in January 2010 served for 6 months. Conscripts who had started service in July or October 2010 were released early after 6 months, unless they requested to finish their 9-month service as originally scheduled.

programs independently of conscription. The largest among them was the voluntary social year. In 2002 it was included to count towards alternative service for conscientious objectors. Since 2005, alternative service was 9 month, and the voluntary social year had to be at least 2 month longer than alternative service in order to replace alternative service. Overall, up to 1/10 of conscientious objectors performed a surrogate services in place of alternative service in 2009 (Kreuter, 2010).

As the flowchart in Figure C2 demonstrates, the timing of conscientious objection played a large role in the probability of service in the years of declining draft rates preceding the suspension (Ludwig, 2009). Once filing for conscientious objection, men served with high probability because there was high demand for conscripts in the health and social sector. Men served with lower probability when applying for conscientious objection only in case they receive a draft notice letter. Because of low demand for conscripts in the military, some men who were fit for service were actually never drafted. When waiting with their application for conscientious objection, chances were that they would not be drafted and therefore would not need to perform neither military nor alternative service. Thus men who wanted to avoid compulsory service but would prefer alternative to military service could game the timing of their conscientious objection in order to avoid compulsory service. The risk involved with this strategy was they could be drafted during the first year of university studies, which qualified for deferment only as of the third semester.

2.3 The suspension of conscription

The suspension of conscription was preceded by a steady decrease in draft rates. In the wake of the German reunification 1990, the Two Plus Four Agreement included the downsizing of *Bundeswehr*. Since the end of the cold war and the dissolution of the Soviet Union, Germany has no longer been a front-line state. While the primary role of Bundeswehr had previously been the defense of state and alliance territory, out-of-area deployments as part of super-national peacekeeping missions now demanded a smaller, more flexible and professional army. The decreasing de-

mand for conscripts lead to several law changes, including stricter mustering rules and a lowered maximal conscription age. Because fewer and fewer men served, the conscription practice was often perceived as arbitrary and criticized as unfair.

The political decision to suspend compulsory service was made relatively short-term. The coalition contract between Christian Democratic Union and Free Democratic Party from October 2009 had included a general commitment to compulsory military service, but a commission was to propose structural changes of Bundeswehr by the end of 2010. Additionally, a duration reduction of compulsory service from 9 to 6 months was compromised upon. The minister of defense, zu Guttenberg, elaborated on downsizing and reforming the Bundeswehr in May 2010 (Kulak et al., 2018); in June, he first mentioned that he could imagine a suspension of conscription in the light of the budgetary situation (Meyer, 2010). While the Federal Chancellor, Angela Merkel, signaled openness to reform ideas ("no ban on thinking"), the topic remained highly controversial among the Christian Democratic Union in the following months (Löwenstein, 2010; Scheidges, 2010). Only at the end of September, the leaderships of the Union parties proposed reforms and suspension, followed by the party agreements in late October and mid November. On 15 December, the government sealed the suspension of compulsory service as of 1 July 2011.

In Section 4, I discuss the suspension process in the context of my identification strategy. Figure 2 maps the signals and decisions around the suspension into a timeline of the graduation cohorts included in my main sample.

3 The Data

Data I use administrative data provided by the German federal pension insurance. The data include all periods subject to mandatory pension insurance such as employment, unemployment, compulsory service, and vocational training. Not included are people under alternative pension schemes such as self-employed and civil servants.

The data set is generated by appending several samples. The 2019 data set had

been maintained as a stratified random sample of the population. Data sets 2000 onward are independent 2-percent random samples of the population. In appending the later data sets, only people are added who were not already included in any of the previous data sets. Combining the 2019–2021 data sets yields a main sample of about 900 men with Abitur born in June/July 1991.

Sample construction Table C1 shows how the main sample is obtained. My population of interest is German men with Abitur born in June or July 1991. I drop men who do not have German nationality because they are not subject to conscription. Because my identification strategy only applies to men with Abitur, I drop men for whom i) "no degree" is reported, that is, men left school without having obtained any degree, ii) a lower or "unknown" schooling degrees is reported, that is, men obtained a degree, but it is lower than Abitur and possibly difficult to categorize, and iii) the data contain a missing value in schooling degree. Finally I drop men for whom no wage is observed, that is, men who did not have any period of employment subject to social security insurance in the observation year.

Service I use an indicator of having served that takes the value of 1 if a men served for at least one day in either military or alternative service (*Zivildienst*), which are not distinguishable in the data. I expect that in my main sample, about 60 percent of conscripts conducted alternative rather than military service[3].

After the suspension of conscription, the service variable only includes voluntary military service. Unobserved in the data are voluntary services outside of the military that could generally be performed by all men and women independently of conscription.

[3]Alternative service had been on average more common than military service since 2001. Among all men starting compulsory service between 2005–08, about 55 percent did alternative service; in 2009–10, 57 percent of conscripts did alternative service (Bundeswehr, 2010; Bundesamt für Familie und zivilgesellschaftliche Aufgaben, n.d.). Additionally, alternative service was relatively more common among men from the highest school track (Kuhlmann and Lippert, 1991).

Outcome The outcome variable is average daily wage in the calendar year men (or women) reach a certain age. I look at early career wage between age 26–29. For my main sample of men born in June/July 1991 this corresponds to average daily wage in 2017-20. Average daily wage contains all wage components subject to mandatory social insurance: salary, bonuses and other one-time payments, as well as indirect components such as capital forming or pensions benefits.

Wages are capped at the social security ceiling. Up until the ceiling, contributions to the mandatory social insurance depend on wage; once wage exceeds the ceiling, contributions no longer rise in wage. Following Dauth and Eppelsheimer (2020), I additionally cap average daily wage at 4 euros below the respective ceiling, which varies by year and east/west. All wages are deflated to 2015 euros using the German consumer price index (Statistisches Bundesamt, 2023c); numbers in connection with the social security ceiling are also deflated.

4 Identification Strategy

I use the local randomization approach to regression discontinuity for discrete running variables with few mass points (Cattaneo et al., 2023), with month of birth as running variable. The local randomization approach assumes that inside a small window around the cutoff i) the running variable is assigned as in a randomized experiment, and ii) the running variable is unrelated to potential outcomes—at least approximately. The intention-to-treat (ITT) effect is calculated by the difference in outcomes between the groups on each side of the cutoff.

Figure 1 shows a discontinuous drop in draft rates by month of birth for men born in 1991 who graduated from the highest secondary schooling track. Abitur, the general university entrance qualification, was obtained after 13 school years in most federal states at the time. Men born in June 1991 enter school in 1997, the calendar year they turn 6. If attending the highest secondary school track, they graduate in 2010, still facing a positive probability of being drafted. Men born in July 1991 are not required to start school in year they turn 6. Entering school in

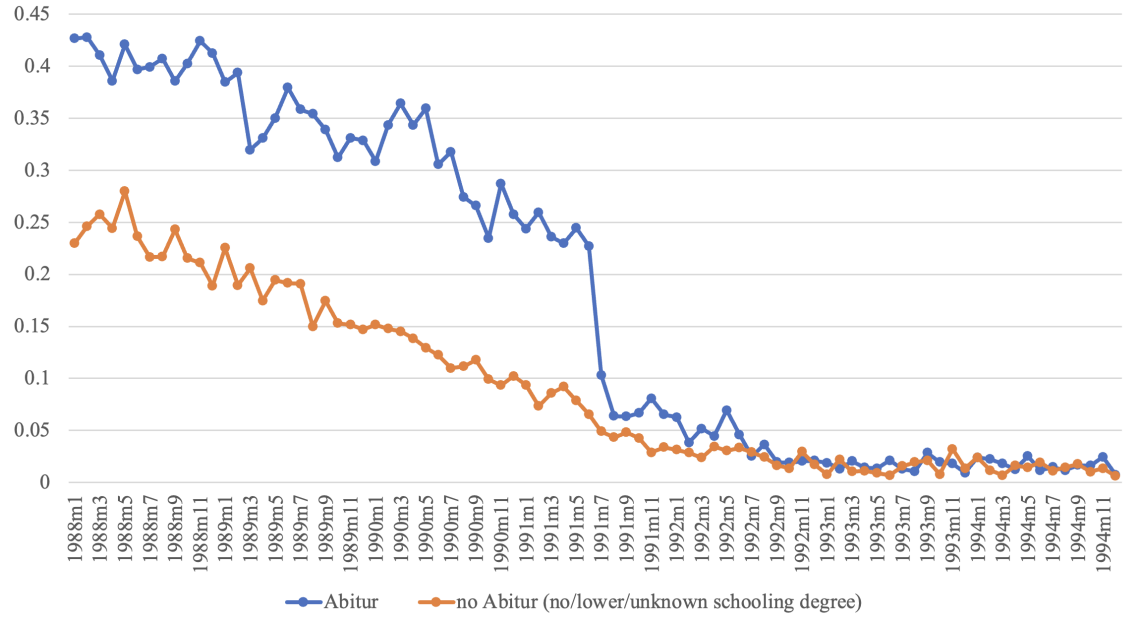
1998 at age 7, men from the highest secondary schooling track graduate in June 2011. With conscription being suspended as of 1 July 2011, men graduating in 2011 are no longer subject to the draft.

I argue that a jump occurs for men with Abitur only because they face a more homogeneous schooling trajectory. They graduated with Abitur mostly in June and typically started service between July and October of their graduation year. In more recent German settings, university studies are not an easy way to avoid the draft anymore. Since October 2004, German conscription law explicitly states that university studies quality for deferment only as of the third semester. That is, men could be drafted during the first year of their university studies. Therefore men with Abitur typically served after school and before university studies.

There is no jump in draft rates for men from lower schooling tracks because they have more heterogeneous educational trajectories. Men from lower tracks typically do vocational training after school, which qualified for deferment of compulsory service. Men from lower schooling backgrounds attend school until grade 9 or 10, with relatively more frequent grade repetitions, and typically do vocational training afterwards, which can last 2-3 years. After vocational training, they can continue with advanced vocational training, which also qualified for deferment in many cases. Thus, men from lower tracks could be born and start school in the same year but be drafted in very different years.

For the interpretation of the effect of conscription, it is crucial to understand the timing when men learn about the suspension. Figure 2 shows how the signals and the suspension decision map into the educational trajectory of men with Abitur born in June/July 1991. Men born in June who are classified as fit for service do not only differ by the higher probability of service, they also differ by the risk of being drafted after graduation. Men born in June receive a first, high-noise signal of suspension during their last month of school. The risk of being drafted could lead them to enroll into tertiary education with higher probability—or earlier than they would have in the absence of conscription—in order to defer or avoid a possible

Figure 1: Having served by month of birth



Notes: On average there are 441 men with and 770 men without Abitur per month of birth; excluded are men with missing data on schooling degrees and men without German nationality. Service includes compulsory military/alternative service, and voluntary military service.

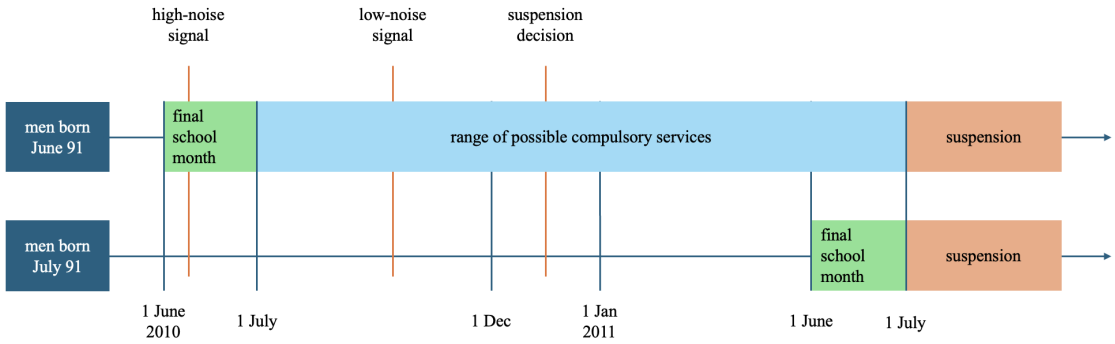
future draft. Men born in July, on the other side, are aware of the suspension of conscription already more than 6 months before their graduation—that is, likely before decisions about tertiary education are made. Because they know that they are not at risk of being drafted, conscription does not affect their decisions about whether or when to enroll into tertiary education.

I identify the intention-to-treat effect of conscription, which gives the overall policy effect of conscription on men's early career wage. The effect captures not only the experience of service for those who served, but also potential draft avoidance behaviors including decisions about tertiary education for all men classified as fit for service, or potential peer effects even on exempt men.

4.1 Exclusion restriction

The exclusion restriction of my fuzzy regression discontinuity design is that within a small window, month of birth is unrelated to potential outcomes and potential treatment, i.e., if all or none had been subject to conscription, men born in June

Figure 2: Timeline of suspension for men born in June/July 1991



Notes: The figure shows how the suspension decision maps into the educational trajectory of men who attend the 9-year secondary schooling track. Because of the school start cutoff, men born in July 1991 start and finish school one year later than those born in June 1991.

and July would have had similar early career wage and would have served with similar probability (Cattaneo et al., 2023). The assumption requires, for example, that men with Abitur born in June or July 1991 are similar in terms of socio-economic background and ability, and that school starting age and graduation cohort as intended by the cutoff rule have no important effect on early career wage. In line with this, most studies do not find an effect of school starting age on wage (Fredriksson and Öckert, 2005).

4.2 Check for balance

By construction, groups on both sides of the cutoff are identical or at least very similar in terms of age, gender, and educational degree. For men with Abitur, there are very few pre-treatment variables available in the data. I check for balance using mini jobs, which are an opportunity for pupils to earn up to then €400 per month while attending school. Mini jobs are a mandatory report in the pension data for my sample because employers are required to pay pension contributions for mini jobbers since April 1999. Having had a mini jobs at around age 17–18 contains relevant pre-treatment information because of its correlation with socio-economic background. Using data of the German Socio-Economic Panel, Geis-Thöne (2023) shows that young people of high socio-economic background have a mini job with higher probability, in particular if their parents are self-employed, work in highly

qualified jobs, or hold a leadership position—pointing to the importance of the parents' social networks. If the two groups around the cutoff differ in socio-economic background, this will likely reflect in the percentage of pupils with mini jobs.

Table 1, Panel A shows that about 32 percent of men born around the cutoff had ever had a mini job for at least one day until the calendar they turn 18, regardless of being born in June or July. When comparing men in larger windows around the cutoff, p-values become smaller. Within a 3-month window, balance fails. Within a 2-month window, men farther apart within the window become different from one another, with p-values at around 0.16 when looking at the donut window in Panel B. This violates the assumption that the running variable does not affect potential outcomes inside the window. Therefore I restrict my analyses to the 1-month window, where as-if randomization is most likely to hold.

4.2.1 Other graduation cohort differences

For my main sample of men born in June/July 1991 and graduating in 2010/11, there are no important cohort differences that could explain my result. The introduction of the bachelor/master degree system (Bologna reform) was essentially completed by the time men in my sample graduate from high school. Thus, men graduating in 2010/11 essentially faced the same undergraduate degree programs. Switches in the duration of secondary school on the federal state level (G8 reform), generated important between-cohort variation in schooling duration only for later birth years. The recession of 2008/09 in Germany was over before men from my main sample graduated from high school; the relatively small 2012/13 revision recession would be very unlikely to explain my result. In Appendix A, I discuss in detail why these reforms and recessions do not confound my results.

Table 1: Check for balance in small windows around the cutoff

	men born $\leq$ June 1991			men born $\geq$ July 1991				
	mean	SE	N	mean	SE	N	difference	p-value
Panel A. 1-month window								
served (at least one day)	0.223	0.022	368	0.094	0.015	393	0.129	0.000
mini job dummy (age 17)	0.220	0.022	368	0.239	0.022	393	-0.019	0.532
mini job dummy (age 18)	0.315	0.024	368	0.323	0.024	393	-0.008	0.814
Panel B. 2-month window								
mini job dummy (age 17)	0.232	0.015	763	0.221	0.015	789	0.011	0.590
mini job dummy (age 18)	0.330	0.017	763	0.311	0.016	789	0.020	0.404
donut: excluding 1-month window								
mini job dummy (age 17)	0.243	0.022	395	0.202	0.020	396	0.041	0.166
mini job dummy (age 18)	0.344	0.024	395	0.298	0.023	396	0.046	0.163

Notes: The table shows the fraction of men having ever had a mini job until the calendar year in which men turn 17 or 18, and the corresponding standard error. The p-values are from a two-sample test of the equality of proportions.

5 Results

I estimate linear censored regression models because my outcome, average daily wage, is right-censored at the social security ceiling (see Section 3). Effects are interpreted as usually in linear models, but refer to latent uncensored daily wage.

Table 2 suggests that conscription has a positive effect on early career wage for men with Abitur. The intention-to-treat effect emerges and grows over time. At age 26, average daily wage does not differ between men born in June and July. In the calendar year men turn 29, men born in June earn 9.5 more in daily wage, about 8 percent more than those born in July.

5.1 Falsification

Can the June-July difference be attributed to conscription? The exclusion restriction might fail, for example, if socio-economic background, absolute or relative school starting age had a direct effect on early career wage. In this case I would expect a similar pattern to occur for women or men of adjacent birth years. Furthermore, if the ITT effect were caused by socio-economic background or school starting age, it should also occur for men without Abitur.

Figure 3 shows that a significant June-July difference does not occur in any of the placebo birth cohorts. Each dot on the graph is the coefficient from the same specification as in Column 4 of Table 2, replicated for men and women born in June and July of different birth years. A significant June-July difference only occurs for men born 1991, where the school start cutoff induces a discontinuity in the probability of having served. A similar effect does not occur for women, who were not subject to conscription. In fact, the placebo ITT effect for women born in the same year is close to zero. Also, there is no similar June-July difference for men (or women) born in adjacent years, where there was no discontinuity in the probability of service. This suggests that broader phenomena like season-of-birth or school starting age effects do not drive my result.

Table C3 replicates the main results from Table 2 for placebo groups, for whom

Table 2: ITT effect of conscription on average daily wage

DV:	wage at 26 (1)	wage at 27 (2)	wage at 28 (3)	wage at 29 (4)
born June	-1.220	6.202+	6.909+	9.537*
	(3.833)	(3.763)	(3.788)	(4.277)
constant	85.28***	93.34***	108.0***	115.2***
	(2.545)	(2.471)	(2.510)	(2.976)
N (total)	638	726	772	649
N (served)	99	113	120	103

Notes: included are German men born June/July 1991 with the highest schooling degree. Wage at a given age is average daily wage in the calendar year this age is reached. Coefficients are estimated using censored linear regression models. Average daily wage is capped at 4 euros below the social security ceiling. Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

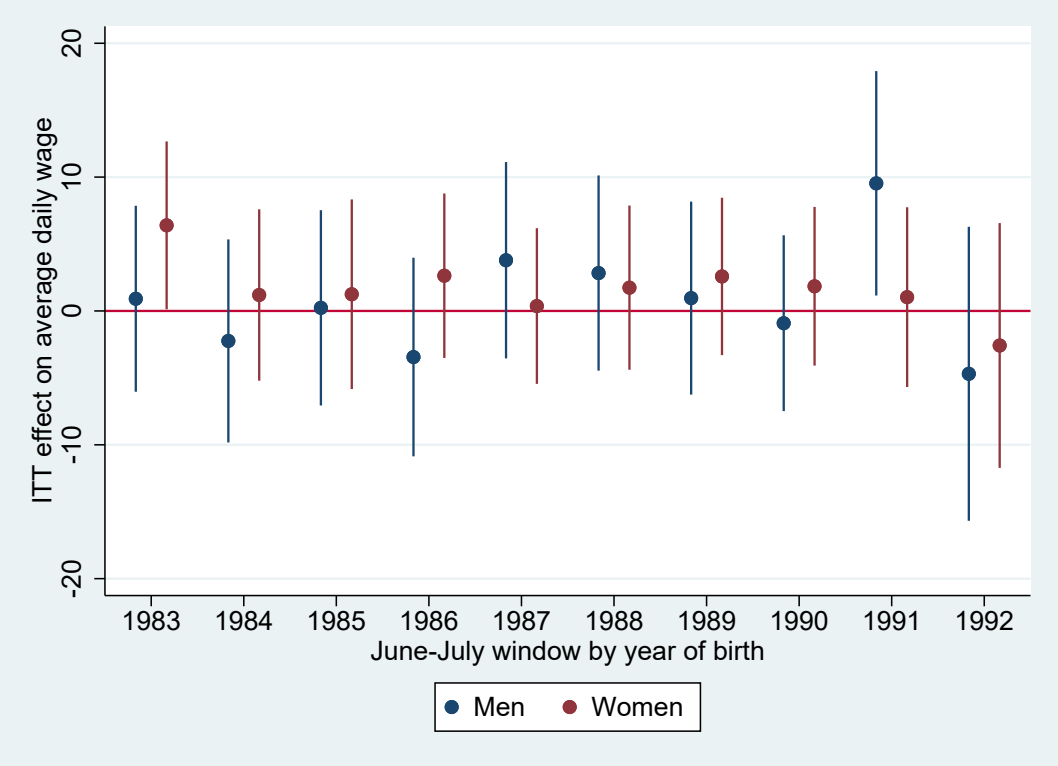
no discontinuity in the probability of service occurs: men born in June/July 1991 without Abitur, women born in June/July 1991 with Abitur, and men born in June/July 1990 with Abitur. The ITT pattern at age 26–29 observed in Table 2 occurs in none of the placebo groups. The June-July difference between age 26–29 is insignificant for women born in 1991; it is also insignificant throughout for men born in 1991 without Abitur.

5.2 Robustness

Outcome specification My results are similar when using log daily wage as outcome. The intention-to-treat effect also occurs when estimating a linear probability model with dependent variable whether wage is capped by the social security ceiling.

Part time work Results tend to be stronger and more precise when controlling for a dummy variable for part time work. If measured accurately, controlling for part time work would allow me to capture human capital better, coming closer to the ideal measure of hourly wage. However, I do not use the part time dummy in my baseline results because it contains measurement error which is potentially nonrandom. I discuss this in more detail in Section D.1.

Figure 3: Falsification of the regression discontinuity design



Notes: ITT effects are coefficients from censored linear regression models. For each birth year, the coefficients estimate the difference in average daily wage when born in June instead of July; the corresponding lines depict 95-percent confidence intervals. The dependent variable is average daily wage in the calendar year men turn 29 (in 2015 euros).

Work experience A trivial explanation for men born in June having higher wage might be that they have more work experience because they started and finished school one year earlier. The suspension of conscription, however, reduced this one-year gap on average: Men born in June served with higher probability for a duration of about 6-9 months. Men who served therefore started university in the same year as those born in July were exempt.

Table D1 shows that the pattern observed in my results is supported when controlling for lifetime work experience up until (and including) the calendar year in which average daily wage is observed. This is because there are no large long-run differences in total work experience between men born in June and July.

6 Mechanism

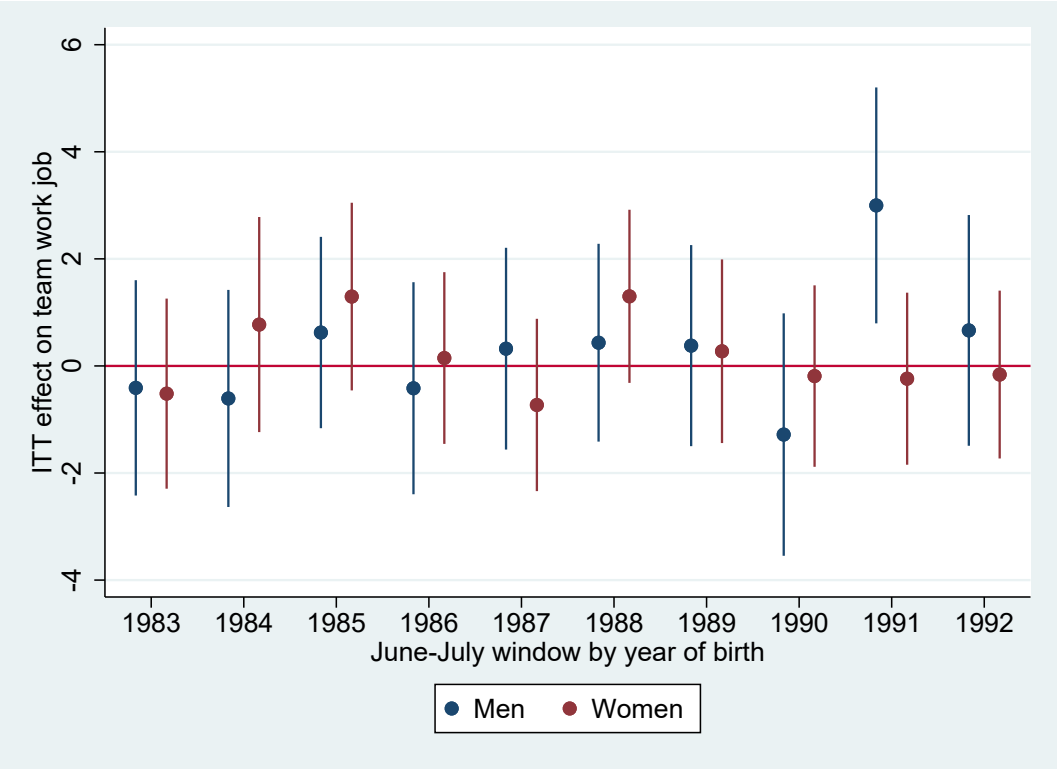
Table 3 shows the ITT effect with different outcome variables controlled in order to test whether they might drive the ITT effect.

6.1 Social skills

Social skills are of increasing importance to the labor market outcomes (Deming, 2017; Hansen et al., 2021). Social skills acquired by a practical experience could not only be an advantage at labor market entry, but also increase success during university studies, generating more long-lasting advantages. At the same time, men and women pursuing university studies after school have fewer opportunities to develop these skills than those who gain practical experience early on through vocational training. Some institutions offering alternative service positions report that conscripts with Abitur had relatively more difficulties to integrate (Becker et al., 2011). The survey of more than 200 institutions offering positions for alternative service also reports that conscripts showed the largest improvement in the areas of social skills and team work (Becker et al., 2011).

In order to test whether conscription caused men to select into team work jobs,

Figure 4: Falsification of the mechanism: selection into team work jobs



Notes: ITT effects are coefficients linear regression models; the dependent variable is the O*NET measure of team work. For each birth year, the coefficients estimate the difference in team work requirement when born in June instead of July; the corresponding lines depict 95-percent confidence intervals. Included are German men with Abitur born in June/July 1991 for whose occupational code there is an O*NET score for team work.

I use the survey-based measure by the Occupational Information Network (O*NET, 2023). The variable measures for more than 800 occupations, "How important is it to work with others in a group or team in this job?" I match the O*NET measure via the ISCO-08 crosswalk with the 5-digit KldB2010 occupational codes in my data. This yields a fine-grained measure for team work jobs for a subsample of men.

Adding the O*NET measure in Table 3, column 9 lets the ITT effect decrease strongly. (The point estimates drops from 9.5 to 7.4 as a result of decreased sample size, and from 7.4 to 3.5 as a result of including the team measure.) The falsification analysis in Figure 4 shows that men born in June 1991 are more likely to work in a job requiring team work than those born in July 1991, and that this effect is unique to my men born in 1991.

Table 3: Testing for different mechanisms of the ITT effect of conscription

	(1)	(3)	(4)	(7)	(8)	(9)	(10)
born June	9.537*	8.832*	9.576*	9.736*	8.748*	3.531	9.446*
	(4.277)	(4.099)	(4.246)	(4.034)	(4.226)	(7.281)	(4.216)
university degree		31.53***					
		(3.999)					
vocational training until Jan 2012			13.34*				
			(4.276)				
sector of employment (9 groups)				YES			
STEM occupation					19.49***		
					(4.179)		
O*NET team measure						1.146*	
						(0.464)	
leadership position							39.73***
							(9.121)
N (total)	649	649	649	649	649	224	649
N (served)	103	103	103	103	103	38	103

Notes: included are German men born June/July 1991 with the highest schooling degree. Sectors are occupational areas defined by the one-digit KldB2010. The dependent variable is wage in 2020. Wage at a given age is average daily wage in the calendar year this age is reached. Coefficients are estimated using censored linear regression models. Average daily wage is capped at 4 euros below the social security ceiling. Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6.2 Selection into education and occupational sectors

Educational choices in connection with draft avoidance behavior are unlikely to drive my result. In the more recent German setting, vocational training immediately qualified for deferment but university studies only after the first year had been completed. The easiest way to avoid service would be to do vocational training after Abitur. If conscription lead more men with Abitur select into vocational training, possibly followed by advanced vocational training or university studies, the practical background might lead to higher wage. However, adding vocational training after Abitur in column 2 of Table 3 leaves the ITT effect unchanged. Also controlling for having a university degree (according to the firm report) lets the ITT effect decrease only slightly. This suggests that educational choices in connection with draft avoidance behavior are not a key mechanism in my setting.

Also the sectors men select into do not explain my result. In general, conscription could affect occupational choices. There is anecdotal evidence that some men selected into the social sector after having made a positive experience in alterna-

tive service. However, if men born in June, who conducted alternative service with higher probability, select more often into the social sector, this should lead to lower wage. Accordingly, the ITT effect increases slightly in column 7 when controlling for a set of occupation dummies.

6.3 Alternative explanations

In this subsection I briefly discuss possible stories how my results could be explained by indirect effects of the suspension of conscription rather than the effect of conscription itself.

6.3.1 General equilibrium effects

The ITT effect could be explained by double cohorts negatively affecting men born in July. Double cohorts arise from the suspension of conscription and are reinforced by a reduction of schooling duration in 2 federal states (see Section B for a detailed explanation).

In order to explain the (placebo) ITT pattern observed, general equilibrium effects would have to be specific to men. My findings could be explained if double cohort effects drive men increasingly into higher-paying university majors, while women select into the same or similar-paying fields. Controlling for working in science, technology, engineering and mathematics (STEM) in Table 3 lets the ITT effect decrease only slightly, suggesting that gender-heterogeneous selection into STEM in response to double cohorts is unlikely to drive my result. I construct the STEM dummy using a list of STEM occupations on the level of 5-digit occupational codes (Bundesagentur für Arbeit, 2022).

I test whether more general double cohort effects drive my result by exploiting variation on the federal state level. Double cohort effects induced by the suspension of conscription are reinforced by a reduction of schooling duration leading to Abitur in 2 federal states. In Lower Saxony and Bavaria, the first 8-year-Abitur cohorts of men and women born in/after July 1992 enter universities at the same time as

the last 9-year-Abitur cohort of men born in July 1991. Therefore I expect any general equilibrium effects to be stronger in Lower Saxony and Bavaria. Exploiting variation on the federal state level, I show in Table B1 that double cohort effects are unlikely to drive my result. See Section B for more details.

6.3.2 Gift of an extra year

Another possible mechanism is that men born in July—as being among the first cohort exempt by the suspension—perceived the suspension as a gift of an extra year. Men might have used this "extra year" for activities other than tertiary education, leading to lower wage compared to those born in June, who might have followed a more regular educational trajectory.

As a sanity check, I estimate my main results of Table 2, dropping all men who served. The point estimates decrease and become statistically insignificant, suggesting that the positive effect of conscription is driven by those who were subject to it rather than those who were exempt by suspension.

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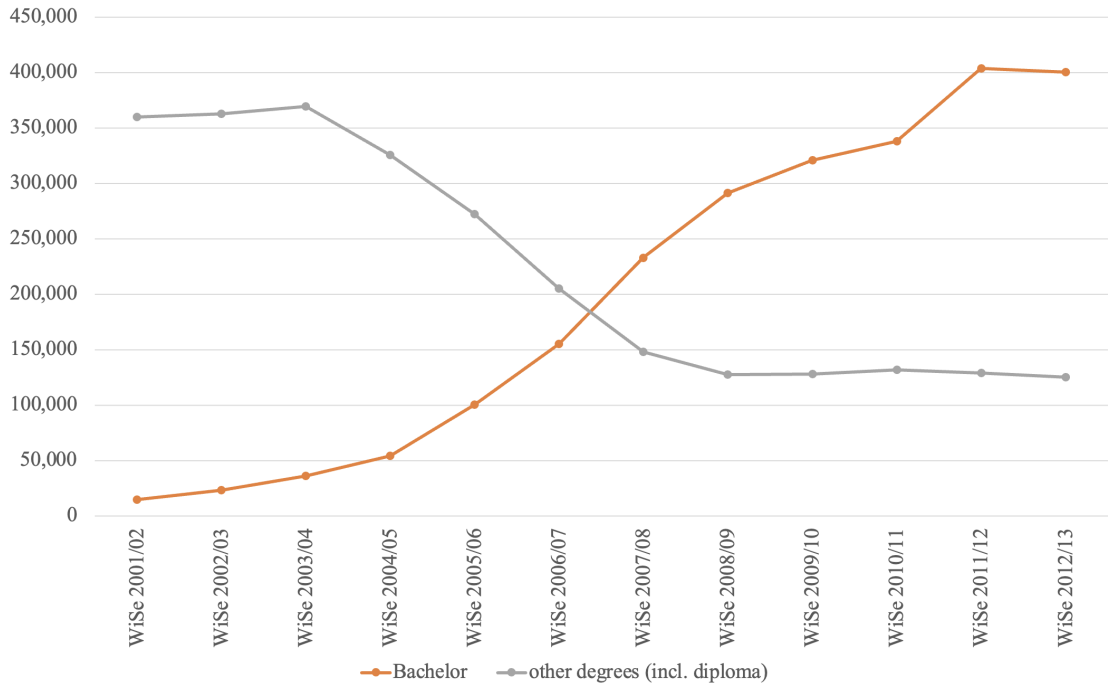
A Other graduation cohort differences

A.1 Bologna reform: introduction of bachelor degrees

Following the Bologna declaration from 1999, the German diploma was replaced by bachelor and master degrees in Germany during the 2000s. The overall goal of the reform was to establish comparable university degrees, allowing for increased student mobility within the so-called European Higher Education Area. The reform is important in my context because in the years where most study programs switched, the reform induced large graduation cohort differences in the supply of undergraduate degree programs, affecting both the decision to enroll into university and the average years of university education. The traditional German diploma was an undergraduate degree equivalent to a Master degree, with a median study duration of 11.7 semesters in 2003 (Statistisches Bundesamt, 2023b). Given the higher bar to obtain an undergraduate university degree under the diploma, fewer men selected into university education, acquired more years of education and entered the labor market later, all of which affecting early career wage.

However, the Bologna reform was essentially finalized by 2010. Hochschulrektorenkonferenz (2010) document that by 2010, less than 2 percent of university degree programs had not yet switched to the Bachelor-Master structure (for universities of applied sciences, 3 percent). Figure A1 from Hochschulrektorenkonferenz (2013) suggests that already in 2008 the reform was mostly finalized: the number of first-year students enrolling into other undergraduate degree programs steeply declined between 2003 and 2008, but remains stable between 2008 and 2012. This baseline level mainly reflects state and church examination degrees as well as degrees for teaching studies, which remained subject to state examinations in some federal states. Figure A1 suggests that men born in June/July 1990, one year before my main sample, also faced almost identical supply of Bachelor degrees. The Bologna reform would cause steep cohort differences when comparing men born in June/July 19883–87. Additional control groups born more than 2–3 years earlier than my main

Figure A1: Number of first-year undergraduate students in Germany



Notes: The data is from Statistisches Bundesamt according to Hochschulrektorenkonferenz (2013). *WiSe* is shorthand for winter semester. The category "other degrees" includes the traditional German diploma, state and church examinations, as well as all degrees of teaching studies.

sample would be invalid.

A.2 The 8-year Abitur: reductions of schooling duration

In some federal states, the highest secondary school track, Gymnasium, lasted 8 instead of 9 years ("G8"). While Thuringia and Saxony traditionally had G8, other federal states introduced the 8-year Abitur in the wake of the G8 reform between 2007 and 2016 (Huebener and Marcus, 2015). One concern would be that men graduating in different cohorts faced different schooling durations on average, which may lead to differences in ability, work experience, Abitur grades, or motivation, which also affect tertiary education outcomes and wage. Marcus and Zambre (2017) show that G8 students are less likely to enroll into university studies. If men born in July were more likely to be subject to G8, they might be less likely to obtain a university degree and have lower wage.

However, Table A1 shows that only a small subset of men in my main sample are

directly affected by the G8 reform. Men born in June and July 1991 face different secondary school durations only in Hamburg: Men born in June graduated in 2010 after 9-years of secondary school; men born in July who started school one year later graduated in the same year after 8 years of secondary school ("double cohort"). For all other federal states, the school start cutoff rule does not induce variation in schooling duration for men born June or July 1991. Until the graduation year 2011, only less populous states had switched to the 8-year Abitur, and each year at most one state featured a double cohort. For men born after 1991, the June/July cutoff would lead to variation in schooling duration in the larger and more populous federal states. Men born before 1988-90 would be valid additional control groups with respect to the G8 reform. Even if the federal states could have implemented the reform with heterogeneous success, the June/July cutoff would affect schooling duration only for a small subset of men born each year in 1988–91.

The 8-year Abitur is one of the exceptions that weaken my instrument. For federal states with the 8-year Abitur, the jump in draft rates should occur for men born in June/July 1992. However, Figure C1 does not show a second jump, likely because until then the G8 states were too few and too small to mark a jump in the data[4]. Additionally, more pupils had to repeat a grade because of the G8 reform, especially males, and therefore ended up with 9 years of secondary schooling anyways (Huebener and Marcus, 2015). A stronger instrument might be achieved by adjusting the running variable by federal state. However, I cannot do this because the federal state of residence at birth or school start is unobserved in the data.

[4]Table A1 shows total living birth by federal state, in parenthesis as percentage of total living birth in all of Germany. Out of all children born in June or July 1991, 10.8 percent would face the 8-year Gymnasium by their federal state of birth, while 87.2 would face the 9-year Gymnasium, assuming they attended Gymnasium, for the remaining 2 percent born in Hamburg, the duration of Gymnasium faced depends on their school starting age.

Table A1: Graduation year and school duration of highest secondary school track (Abitur)

Birth months	Federal states								
	SN TH	ST	MV	SL	HH	NI BY	BR B, BB BW	NRW	SH RP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Total living birth									
Jun/Jul 91	8003 (5.6%)	3104 (2.2%)	2272 (1.6%)	1966 (1.4%)	2,832 (2.0%)	37883 (26.5%)	30115 (21.1%)	33816 (23.7%)	12248 (8.6%)
Panel B. Regular duration of secondary school and graduation year									
Jul 86 - Jun 87	2005	2006	2006	2006	2006	2006	2006	2006	2006
Jul 87 - Jun 88	2006	2007	2007	2007	2007	2007	2007	2007	2007
Jul 88 - Jun 89	2007	2007	2008	2008	2008	2008	2008	2008	2008
Jul 89 - Jun 90	2008	2008	2008	2009	2009	2009	2009	2009	2009
Jul 90 - Jun 91	2009	2009	2009	2009	2010	2010	2010	2010	2010
Jul 91 - Jun 92	2010	2010	2010	2010	2010	2011	2011	2011	2011
Jul 92 - Jun 93	2011	2011	2011	2011	2011	2011	2012	2012	2012
Jul 93 - Jun 94	2012	2012	2012	2012	2012	2012	2012	2013	2013
Jul 94 - Jun 95	2013	2013	2013	2013	2013	2013	2013	2013	2014

8-year Abitur double cohort 9-year Abitur

Notes: Panel A shows total living birth by federal state, in parenthesis as percentage of total living birth in all of Germany. Panel B shows graduation year and the number of school years of the highest schooling track leading up to Abitur by month of birth and federal state. Abbreviations of the federal states: SN Saxony, TH Thuringia, ST Saxony-Anhalt, MV Mecklenburg-Hither Pomerania, SL Saarland, HH Hamburg, NI Lower Saxony, BY Bavaria, BR Bremen, BB Brandenburg, B Berlin, BW Baden-Wuerttemberg, NRW North Rhine-Westphalia, SH Schleswig-Holstein, Rhineland-Palatinate RP. Hessen is not included in Panel B because it adopted the reform over a period of three years, with double cohorts in 2012-14. Source: Huebener and Marcus (2015), Statistisches Bundesamt (2023a), own calculations.

A.3 Graduation during a recession

Among others, Oreopoulos et al. (2012) document that cohorts graduating from college during a recession have lower labor market entry wages, facing long-term losses. A concern is therefore that men graduating in adjacent cohorts could be differently affected by recessions.

The global economic crisis of 2008/09 marked the largest crisis in post-war German history, with a real gross domestic produce (GDP) growth of −5.7 percent in 2009 (Statistisches Bundesamt, 2024). The recession ended in Germany already in spring 2009 (Heilemann, 2019). Men in my main sample finish secondary school in 2010/11 and are thus no longer affected by the recession. Also men born one year earlier who finish secondary school in 2009 would not enter the labor market during a recession, even if they would not acquire tertiary education. When assuming Abitur and at least a 3-year undergraduate degree (or vocational training), men born before 1988 could be affected by entering the labor market during the 2008 recession. Another crisis of similar dimension did not occur until the outbreak of the COVID-19 pandemic in 2000, associated with a GDP growth of −3.8 percent.

Men graduating from high school in 2010/11 faced similar economic conditions at a GDP growth of around 4 percent. At the time they graduate from university, they could face different levels of positive GDP growth. The relatively small 2012/13 recession in Germany was only identified statistically in retrospect and is therefore called the "revision crisis" (Heilemann, 2019), with GDP growth at 0.4 percent in 2012 and 2013. Men born in June/July 1991 without service and with a 3-year university degree (or vocational training) entered the labor market in 2013/14. The revision crisis lasted until the first quarter in 2013 (Heilemann, 2019) such that men graduating from university in summer 2013 would not enter the labor market during the recession. And if men born in June 1991, who are more likely to serve, would face worse economic growth at labor market entry, any positive effect of service identified in my analyses would be underestimated. My result could be overestimated, if men decide to acquire more tertiary education because of the revision crisis. However,

given the revision crisis was small, it is unlikely to drive my results. When assuming tertiary education of 4-6 years for men born in June/July 1991, there are no large cohort differences in GDP growth induced by the school start cutoff.

B General equilibrium effects

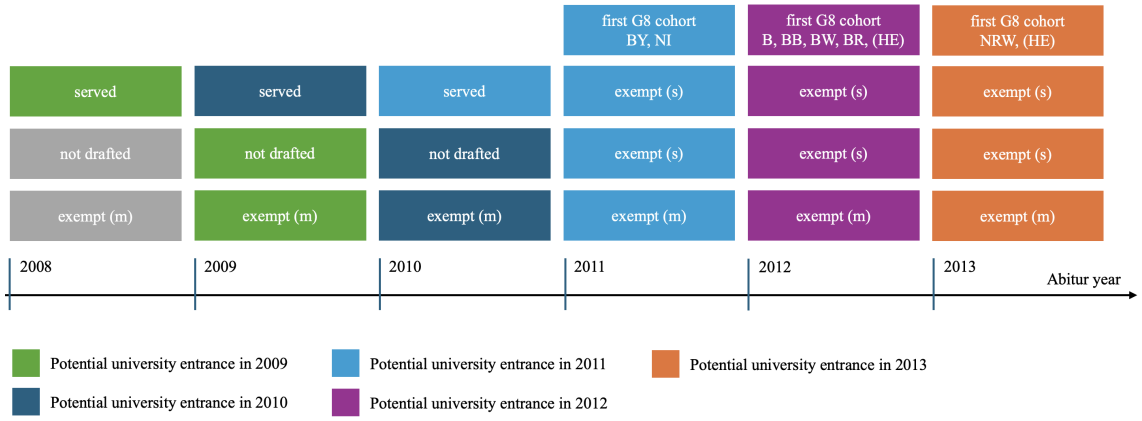
The suspension of conscription may affect wage through general equilibrium effects. Figure B1 shows the year of potential university entrance by Abitur graduation year. Men who would have served in 2011 under conscription but were exempt because of the suspension additionally start university in 2011. Additionally, there were large double Abitur cohorts in Bavaria and Lower Saxony because of a reduction in schooling duration.

Some students might have decided for other university majors in order to avoid crowded programs. The German federal and state governments created additional capacities for universities between 2007 and 2020 (*Hochschulpakt*), addressing double cohorts in connection with reductions in schooling duration on the federal state level. Additional study places were generated especially targeting faculties of natural sciences, but not in popular majors like medicine or humanities (Aerztezeitung, 2010). University majors like have restricted capacities in Germany also because they are subject to state examinations. In line with this, Figure A1 shows a peak in the number of first-year university enrollments in Bachelor and Master programs in 2011, but not in the state examination majors.

Men and women could have reacted differently to the anticipation of crowded universities for popular majors, the universities' increasing capacities in STEM fields, or the corresponding attempts of nudging more graduates into STEM. My result could be generated by gender-heterogeneous selection into STEM in response to double cohort effects. However, Table 3 shows that the ITT effect is not driven by selection into STEM fields or particular sectoral areas.

I test whether more general double cohort effects drive my result by exploiting variation on the federal state level. Double cohort effects induced by the suspension

Figure B1: Potential university entrance cohorts by Abitur year



Notes: Men who are exempt by mustering result are denoted by "exempt (m)"; men who would have been fit for service but are exempt by the suspension of conscription are denoted by "exempt (s)". Men who are fit for service may have served or were "not drafted" because of low demand for conscripts in the military. Some federal states switched the duration of secondary school leading to Abitur from 9 years to 8 years ("G8"), leading to a double cohort when the first G8 pupils graduate. The three double cohorts occur in this stylized model because they lead to larger between-cohort differences, which were of comparable extent population wise (see Table A1, Panel A).

of conscription are reinforced by a reduction of schooling duration leading to Abitur in 2 federal states. In Lower Saxony and Bavaria, the first 8-year-Abitur cohorts of men and women born in/after July 1992 enter universities at the same time as the last 9-year-Abitur cohort of men born in July 1991 (see Table A1). Therefore I expect any general equilibrium effects to be stronger in Lower Saxony and Bavaria.

If my results were driven by double cohort effects, I would expect them to decrease when excluding men with residence in Lower Saxony and Bavaria[5]. Table B1 shows that the ITT effect does not decrease strongly when excluding men with residence in Bavaria and Lower Saxony. For comparison, I also exclude men with residence in North Rhine-Westphalia, the most populous federal state, where a double cohorts occurred only in 2013. Taken together, the results suggest that the point estimate decreases because of lower sample size rather than being driven by particular federal states.

[5]The federal state of residence is only a proxy for the federal state in which men visited school/university. Residence in the pension data is observed only as a time-constant recent report.

Table B1: Testing for mechanism: general equilibrium effects

	all	excl. NI	excl. BY	excl. NRW	excl. NI,BY
	(1)	(2)	(3)	(4)	(5)
born June	9.537*	9.169*	8.542+	6.256	7.889
	(4.277)	(4.500)	(4.636)	(4.959)	(4.921)
Constant	115.2***	115.3***	112.4***	118.6***	112.2***
	(2.976)	(3.132)	(3.271)	(3.427)	(3.481)
N (total)	649	600	543	518	494
N (served)	103	94	89	91	80

Notes: the dependent variable is average daily wage in 2020, the calendar year men in my sample turn 29. NI is Lower Saxony, BY Bavaria, NRW North Rhine-Westphalia, where I expect general equilibrium effects stronger in NI and BY; NRW is excluded in Column 4 as a placebo. Included are German men born June/July 1991 with the highest schooling degree. Wage at a given age is average daily wage in the calendar year this age is reached. Coefficients are estimated using censored linear regression models. Average daily wage is capped at 4 euros below the social security ceiling. Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

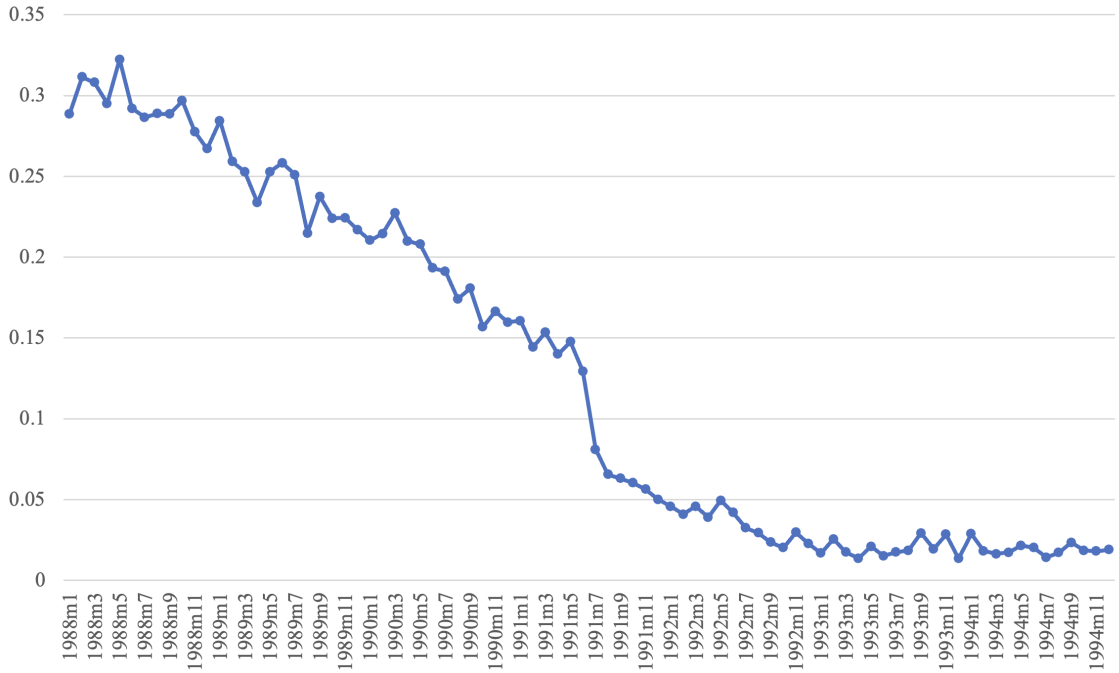
C Additional Figures and Tables

Table C1: Sample construction

men born June/July 1991 ...	**5697**
... with German nationality	3459
exclude missing schooling degree	-945
exclude non-Abitur schooling degree	
none	-42
lower	-424
intermediate	-621
unknown	-549
... with Abitur	878

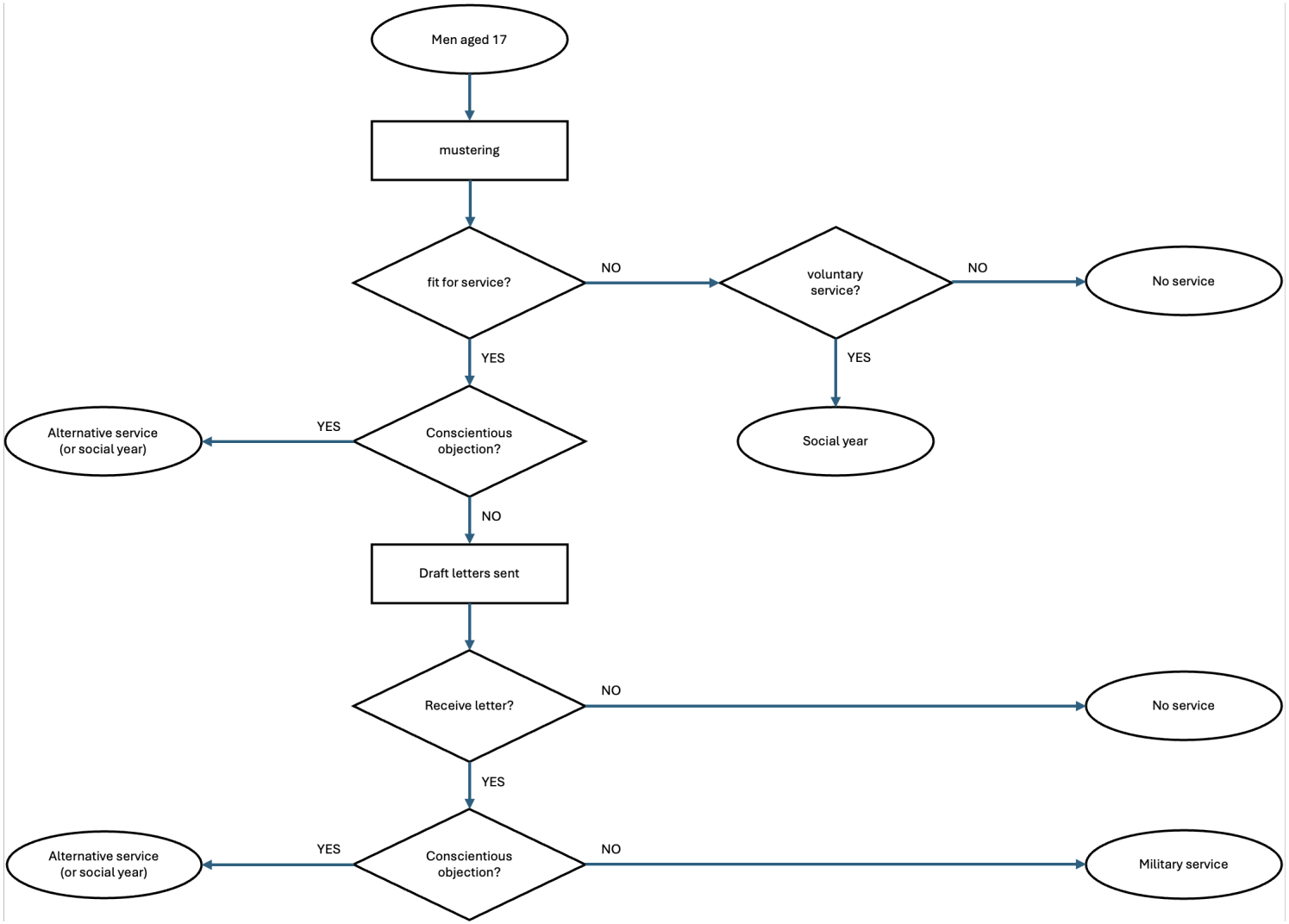
Notes: The table shows how the main sample is obtained, starting from all men born in a 1-month window around the cutoff.

Figure C1: Having served by month of birth (all men)



Notes: Included are all men with German nationality, including men with all types of schooling degrees or missing data on schooling degree. On average there are 1,694 men per birth month. Service includes compulsory military/alternative service, and voluntary military service.

Figure C2: A stylized model of compulsory and voluntary service in Germany in the years before the suspension of conscription



Notes: The flowchart shows a stylized model of the conscription process in Germany for the years preceding the suspension. The "social year" is shorthand for the voluntary social (or ecological) year that was open to all men and women independently of conscription. Since August 2002, these voluntary services counted as surrogates to alternative service.

Table C2: Descriptive statistics of main sample

	N	mean	SD	min	5%	median	95%	max
service								
served (at least one day)	892	0.164	0.370	0	0	0	1	1
duration of service (total, in months)	146	7.90	3.93	0.07	2.33	6.49	12	29
duration of service (first uninterrupted period)	146	7.70	3.94	0.07	2.04	6.05	12	29
age at service start (year of service start – birth year)	146	19.14	0.94	18	18	19	20	25
pre-service variables								
mini job at age 17	892	0.223	0.417	0	0	0	1	1
mini job at age 18	892	0.307	0.462	0	0	0	1	1
tertiary education								
vocational training started until/in 2011	892	0.254	0.436	0	0	0	1	1
university degree	892	0.553	0.497	0	0	1	1	1
labor market outcomes								
average daily wage at age 26 (capped)	638	84.52	47.72	4.06	18.19	83.49	173.73	207.52
average daily wage at age 29 (capped)	649	118.85	51.92	12.20	27.94	118.97	212.23	214.33
capped average daily wage at age 26	638	0.014	0.118	0	0	0	0	1
capped average daily wage at age 29	649	0.060	0.238	0	0	0	1	1
work experience at age 26 (in months)	892	24.72	21.36	0	0	19.41	66.08	99.65
work experience at age 28 (in months)	892	42.47	26.67	0	1.38	40.47	89.26	129.21
unemployment at age 28 (dummy)	892	0.040	0.197	0	0	0	0	1
occupational information								
leadership position	892	0.056	0.230	0	0	0	1	1
O*NET measure for team work	329	80.38	10.28	26	69	80	92	100
part time dummy	892	0.225	0.418	0	0	0	1	1
STEM occupation	892	0.359	0.480	0	0	0	1	1

Notes: The table shows summary statistics for German men born in June/July 1991. Variables observed at given age (like 28) refer to the calendar year in which men reach this age.

Table C3: Placebo ITT effects

DV:	wage at 26	wage at 27	wage at 28	wage at 29
	(1)	(2)	(3)	(4)
Panel A. women born 1991, with Abitur				
born June	-0.263	1.302	0.509	1.032
	(2.725)	(2.771)	(2.959)	(3.426)
Constant	80.63***	88.31***	97.01***	102.1***
	(1.746)	(1.809)	(1.864)	(2.195)
N (total)	736	785	806	680
N (served)	0	0	0	0
Panel B. men born 1991, without Abitur				
born June	-0.703	-0.396	0.406	1.312
	(1.736)	(1.812)	(1.838)	(2.038)
Constant	84.23***	88.74***	92.80***	93.65***
	(1.219)	(1.270)	(1.283)	(1.426)
N (total)	1462	1506	1550	1243
N (served)	76	79	81	72
Panel C. men born 1990, with Abitur				
born June	7.079*	1.548	-1.632	-0.914
	(3.398)	(3.458)	(3.326)	(3.351)
constant	79.80***	91.91***	104.7***	112.8***
	(2.335)	(2.263)	(2.251)	(2.249)
N (total)	668	746	820	867
N (served)	203	232	264	277

Notes: The tables shows placebo ITT effects, in contrast to the main results for men born June/July 1991 with Abitur in Table 2. Included are people born in June/July of a given birth year. Wage at a given age is average daily wage in the calendar year this age is reached. Coefficients are estimated using censored linear regression models. Average daily wage is capped at 4 euros below the social security ceiling. Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D Additional Robustness

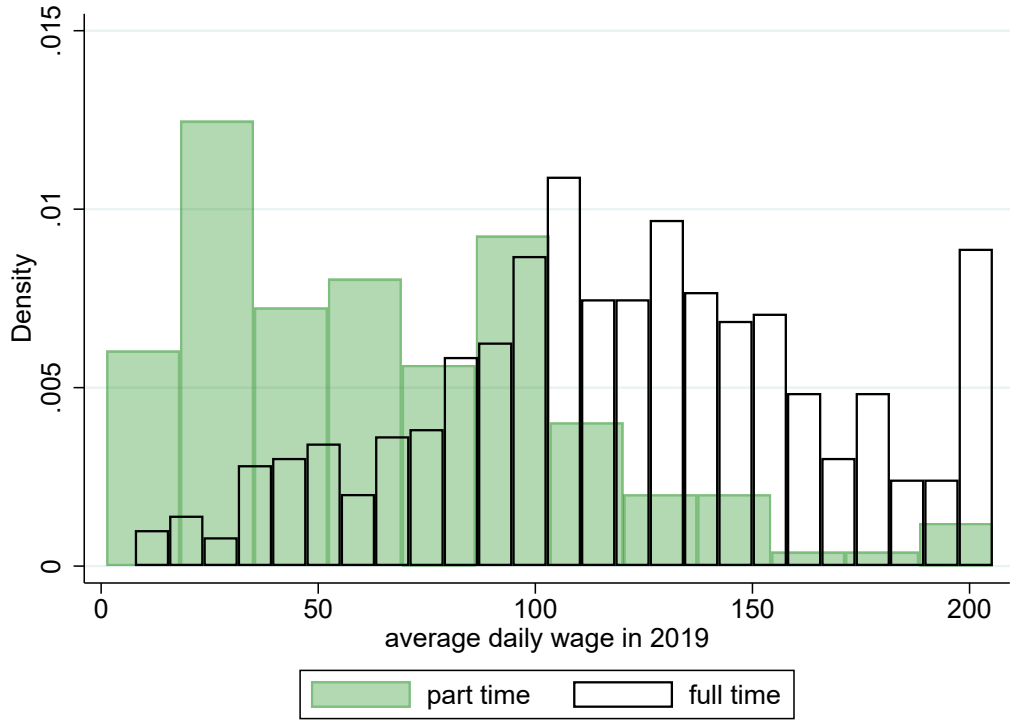
D.1 Part time work

The federal insurance data does not include information on working hours. The data include, for each date-to-date spell, gross wage subject to mandatory social insurance capped at the social security ceiling. With this information I calculate average daily wage in a given year. The federal insurance data also include part time work as a time-constant, one-off report. This part time variable is a recent firm report, which however may not be accurate or actually up to date.

Even though firms are legally required to report information on full/part time work along with other variables which do not affect the calculation of pension claims, they are not incentivized or monitored for reporting these variables accurately. In particular, firms may fail to update individual information over time once the variable has been entered into their reporting system. Consequently, the variable is accurate with higher probability for more mobile workers who switch employers more often. The general problem of measurement error in German administrative data has been addressed concerning the part time variable (Schmidt, 2020), the leadership position variable (Collischon, 2023), and educational degrees (Fitzenberger et al., 2006).

The distributions of daily wage suggest that there is likely some measurement error in the part time variable. For example, Figure D1 shows that the upper part of the distribution of daily wage in "part time" resembles the distribution for full time work, with a peak at the social security ceiling and a mode just below the one for full time work. This indicates that some workers might have switched from part time to full time work, but the firm report still classifies them as part time workers. Workers who start in part time—possibly while still in university education—and transition to full time work staying in the same firm may be of lower ability. Therefore the part time variable would likely introduce endogeneity in the model. Similarly, some workers might be falsely classified as full time workers.

Figure D1: Average daily work by part time work



Notes: The histogram shows average daily wage in 2019 for men with Abitur born in June/July 1991 (in 2015 euros).

D.2 Work experience

Table D1: ITT effect of conscription, controlling for work experience

DV:	wage at 26	wage at 27	wage at 28	wage at 29	wage at 30
	(1)	(2)	(3)	(4)	(5)
born June	-3.296	3.527	6.356+	9.596*	10.19+
	(3.606)	(3.517)	(3.594)	(3.951)	(5.391)
experience	0.912***	0.995***	0.787***	0.856***	0.850***
	(0.140)	(0.0915)	(0.0822)	(0.0797)	(0.110)
experience squared	-0.00540	-0.0107**	-0.00842**	-0.0114***	-0.00823**
	(0.00393)	(0.00289)	(0.00245)	(0.00211)	(0.00247)
constant	82.17***	94.84***	109.4***	118.7***	116.7***
	(2.691)	(2.587)	(2.683)	(2.993)	(4.193)
N (total)	638	726	772	649	352
N (served)	99	113	120	103	60

Notes: experience is total work experience in full or part time employment, measured in months and demeaned, up until (and including) the calendar year in which the corresponding average daily wage is observed. Included are German men born June/July 1991 with the highest schooling degree. Wage at a given age is average daily wage in the calendar year this age is reached. Coefficients are estimated using censored linear regression models. Average daily wage is capped at 4 euros below the social security ceiling. Robust standard errors in parentheses, + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$