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## The effect of compulsory service on life satisfaction and its channels

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# IAB-Discussion Paper

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Articles on labour market issues

## The Effect of Compulsory Service on Life Satisfaction and its Channels

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# The Effect of Compulsory Service on Life Satisfaction and its Channels

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

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## Abstract

Compulsory military service is still a prominent feature of young people's careers in many countries. We use the abolition of compulsory military and civil service for males in 2011 in Germany as a natural experiment to identify effects of institutionalized career disruptions on life satisfaction. Drawing on data from the SOEP, we apply a difference-in-differences design (comparing young males and females) to assess the causal effect of this reform on individual life satisfaction. Our results show a significant and robust positive effect of the abolition of compulsory service on young males' life satisfaction. Furthermore, we provide empirical evidence that reductions in career disruptions, forgone earnings, uncertainty regarding the future, and forced labor contribute to this effect.

## Zusammenfassung

In vielen Ländern ist die Wehrpflicht nach wie vor zentraler Bestandteil in den Erwerbskarrieren junger Menschen. Wir nutzen die Abschaffung der Wehrpflicht und des Wehersatzdienstes für Männer in Deutschland 2011 als natürliches Experiment, um Effekte institutionalisierter Karriereunterbrechungen auf die Lebenszufriedenheit zu untersuchen. In difference-in-difference Analysen der Daten des SOEP vergleichen wir junge Männer und Frauen, um den kausalen Effekt der Reform auf die individuelle Lebenszufriedenheit zu messen. Unsere Ergebnisse zeigen einen signifikanten und robusten positiven Effekt der Abschaffung der Wehrpflicht auf die Lebenszufriedenheit junger Männer. Außerdem weisen die Ergebnisse darauf hin, dass der Effekt durch seltenere Karriereunterbrechungen, Einkommensausfälle, Unsicherheit gegenüber der Zukunft und Zwang zur Arbeit vermittelt wird.

**JEL-Klassifikation:** I31, I38, J18

**Keywords:** policy evaluation, military service, happiness, well-being, life satisfaction

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# 1 Introduction

Compulsory military service is an important event in the lives of young men worldwide. Countries such as Israel, Austria, Cyprus, Denmark, Finland, Greece, South Korea, Switzerland, and Norway require young men (women, too, in Israel and Norway) to serve their time in the military (or a civilian alternative in most countries), typically after the completion of schooling. Sweden, Lithuania and the Ukraine have even reinstated compulsory military service in recent years, and German politicians are discussing whether to do the same (Handelsblatt, 2018). Compulsory service can be seen as a lengthy interruption in the early careers of millions of young people, interruptions that could affect various contemporary and subsequent aspects of individual lives such as individual health, family formation, and career success. However, there is surprisingly little evidence on the impact of these services on the individuals concerned. While there are some studies on the labor market consequences of military service (e.g. Angrist, 1990; Bauer, Bender, Paloyo, and Schmidt, 2012; Bauer, Bender, Paloyo, and Schmidt, 2014; Paloyo, 2010; Sampson and Laub, 1996; Teachman and Call, 1996), there is no empirical evidence showing whether and how such policies affect individual well-being.<sup>1</sup> This paper uses the abolition<sup>2</sup> of compulsory military service in Germany as a natural experiment to evaluate the causal impact of such policies on individual life satisfaction. Furthermore, we use specifics of the German institutional setting to discuss the effects of career disruptions and uncertainty on individuals' future life satisfaction more generally.

We argue that compulsory service (both military and civilian) negatively affects life satisfaction mainly (but not exclusively) for two reasons: uncertainty regarding the future as well as career disruptions. Compulsory service, as implemented in Germany, was mandatory for men only and was mostly conducted directly after the end of educational episodes at age 18 or 19, either between two educational episodes or before the transition into the labor market, depending on the educational level (Scherer, 2001, p. 139). Compulsory services have been characterized by uncertainty because the individual's military draft was arbitrary to some extent, and the determination of who had to serve was often considered unfair (see *Wehrgerechtigkeit* in the institutional setting). Additionally, compulsory services cause deviations from the standard transition from school to (vocational) training and into stable employment (Brzinsky-Fay and Solga, 2016; Scherer, 2005, p. 432), i.e., career disruptions. Such disruptions may result in short- and long-term disadvantages, especially when they occur during a stage in the life course that is accompanied by major changes, such as the transition from school to work, moving out of parental homes or starting a family (Brzinsky-Fay

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<sup>1</sup> We use the terms *happiness*, *well-being* and *life satisfaction* synonymously. We define happiness as the subjective satisfaction with one's life.

<sup>2</sup> Technically, compulsory military service in Germany is only suspended and can be reactivated during military conflicts. However, because this practically equals abolition during peace times and thus within our period of observation, we use the term *abolition*.

and Solga, 2016; Manzoni and Mooi-Reci, 2011). Thus, we expect compulsory services to lower life satisfaction of young men before and after the draft. Due to their imminent compulsory service, young men awaiting the draft supposedly perceive uncertainty concerning their futures, and young men who have already been drafted have experienced career disruptions. Consequently, the abolition of compulsory services should lead to an increase in young men's life satisfaction in general.

We draw on the German Socio-Economic Panel (SOEP) to analyze the causal effect of compulsory services on life satisfaction. Compulsory military service (and the corresponding civil services for individuals who refused to serve in the military) in Germany was abolished in 2011. We use a difference-in-differences (DD) approach to estimate the causal effect of this reform on young males' (aged 18-23) life satisfaction. Our results reveal that the abolition of compulsory services leads to an increase in the life satisfaction of young men. Various robustness checks, including triple differences (DDD) estimation and data from other countries (UK, Australia), support our findings. The effect still exists 4 years after the abolition. Furthermore, our results indicate that both a reduction in uncertainty concerning the future (for those who have not already served) as well as smoother labor market entries without disruptions or uncertainty likely drive the positive treatment effect. However, we acknowledge that there are other potential channels that we cannot identify, such as social comparison between males and females or simply a distaste for forced labor that could also contribute to the treatment effects.

Nevertheless, even without knowledge of the exact channels, our causal estimation of the overall effect of compulsory service on life satisfaction provides important information for the debate on the pros and cons of (re)introducing compulsory services that is currently being held in various countries, including Germany, assuming that re-introducing compulsory services affects life satisfaction symmetrically to the abolition. This paper is organized as follows: the first section provides the institutional background, which explains compulsory military service in Germany in more detail. The next section explains theoretical mechanisms through which compulsory military service affects life satisfaction. The "Data" section describes the data and the econometric approaches, and the "Empirical Results" section presents the results and includes different robustness checks with regard to the analysis. The final section concludes.

## 2 Institutional Background

This section describes the institutional setting of German compulsory national service prior to its abolition. In Germany, military service (*Grundwehrdienst*) or civilian service (*Zivildienst*) were formerly mandatory for young men between the ages of 18 and 23. The service was introduced in 1956 and was in place until its abolition in 2011. During the 1960s, legislators introduced civilian service as an alternative to military service for those who refused to serve in the military for ethical reasons. Various reforms changed the duration of compulsory services, which ranged between 18 months (in the 1960s and 1970s) to 6 months (prior to abolition).



In the years before the abolition, compulsory service did not affect all young men. There were predictable exceptions for individuals such as married men, fathers, and men with two siblings who had already provided military service. These men were not expected to provide compulsory national service. Additionally, men in vocational or civil servant training could postpone compulsory services to finish their training/education. For all others, the process of recruitment for compulsory service entailed some uncertainties, such as the medical examination.

For those men who potentially had to serve military service or civilian service, the first step in the procedure was registration. For registration, the Residents' Registration Offices transferred the data of every male who reached the age of 18 to the German Armed Forces on a quarterly basis. Men who were not registered at the Residents' Registration Office were therefore not reachable.<sup>3</sup> However, not being registered constitutes an administrative offence, and only a few persons were not reachable.<sup>4</sup>

Within the selected age group of 18 to 23 years, men were obliged to undergo a medical examination at the German Armed Forces; non-compliance was a legal offense. Based on the examination, the men were classified into six different tiers: T1, T2, T3, T4, T5, and T6. Only the first two levels (T1, T2) were qualified for military service. A person sorted into T1 was fully qualified to perform military service, while individuals rated T2 were qualified for military service but with some restrictions. For example, men with moderate allergies were classified as T2.<sup>5</sup> Men sorted into T3 had some physical restrictions and thus were only qualified for certain tasks. However, T3 was only used until October 1, 2004. Afterwards, individuals usually sorted into T3 were no longer liable for military service and were thus rated either T4 or T5.

Tiers T4 and T5 included men who were not required to provide compulsory service. Men classified in T4 were temporarily not able to participate in military service. For example, people with fractured bones were classified as T4, and they had an additional medical check after some time. By contrast, men who were classified as T5 were not able to perform any military service for medical reasons. For example, men with cardiovascular diseases were classified as T5. T6 included a small group of men who were able to perform military service only as reservists; for conscripts, this tier was only used for reservists after the abolition of T3. For an overview of how many men from the birth cohorts 1988-1991 were sorted into different categories, see Table 1 (unfortunately, the data contain no information on the number of males classified into T6).

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<sup>3</sup> For example, people are not registered at the Resident's Registration Office if they have recently moved houses or are homeless.

<sup>4</sup> In the 1987 cohort, only 0.003 percent (1,272) of all males were not reachable (Bundestag, 2011).

<sup>5</sup> Young men who were classified in T4 or T5 were also unqualified for civilian services.

**Table 1**  
**Classification of young men into military service tiers by birth cohort.**

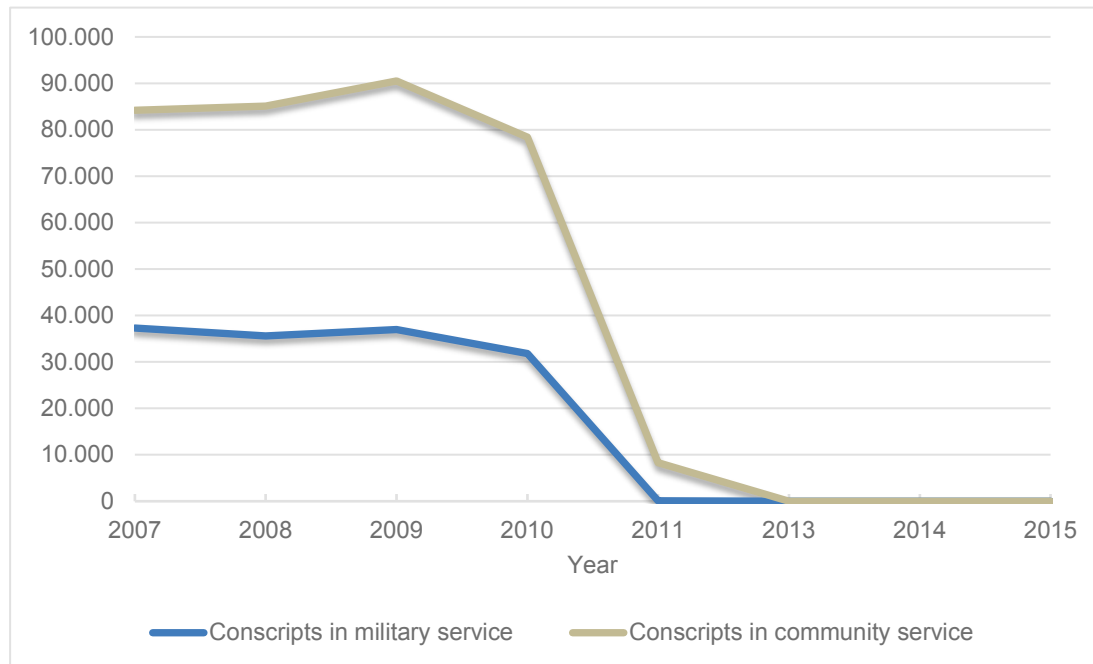
Birth cohort	Able to provide military service (T1, T2)	Able to provide military service but with some restrictions (T3)	Temporarily not able to do military service (T4)	Not able to do military service (T5)	Percent Unfit to Serve
1983	254,201	26,432	2,360	115,894	29.65
1984	237,138	17,493	3,485	124,945	33.53
1985	222,038	8,436	2,983	142,069	38.63
1986	234,649	1,975	2,317	184,532	44.12
1987	230,263	84	2,084	197,152	46.38
1988	230,697	0	2,583	209,078	47.85
1989	205,899	0	4,085	192,903	48.89
1990	179,025	0	5,405	165,364	48.82
1991	118,798	0	6,297	111,173	49.72
1992	55,079	0	4,950	66,656	56.52
1993	6,392	0	955	19,140	75.87

Notes: T3 was abolished in 2004; T6 is not shown because it was just a relabeling of already examined conscripts in T3.

Source: Bundestag (2010)

As previously mentioned, only men sorted into T1 or T2 had to serve. The default for these individuals was military service. However, it was possible to provide civilian service (e.g., in retirement homes) instead of military service for ethical reasons. In practice, after the cold war ended, civilian service was far more popular than military service, and the requirements for refusal of military service were relatively low, mainly driven by two factors: (i) a declining demand for armed forces overall in reunited, post-cold war Germany and (ii) the increased demand for (cheap) labor in the social service sectors. The total number of individuals who were conscripted to compulsory military service or civil service decreased over time (see Figure 1). The decrease in conscription over time also reflects the number of men required to complete the examination at the German Armed Forces (see Figure 1).

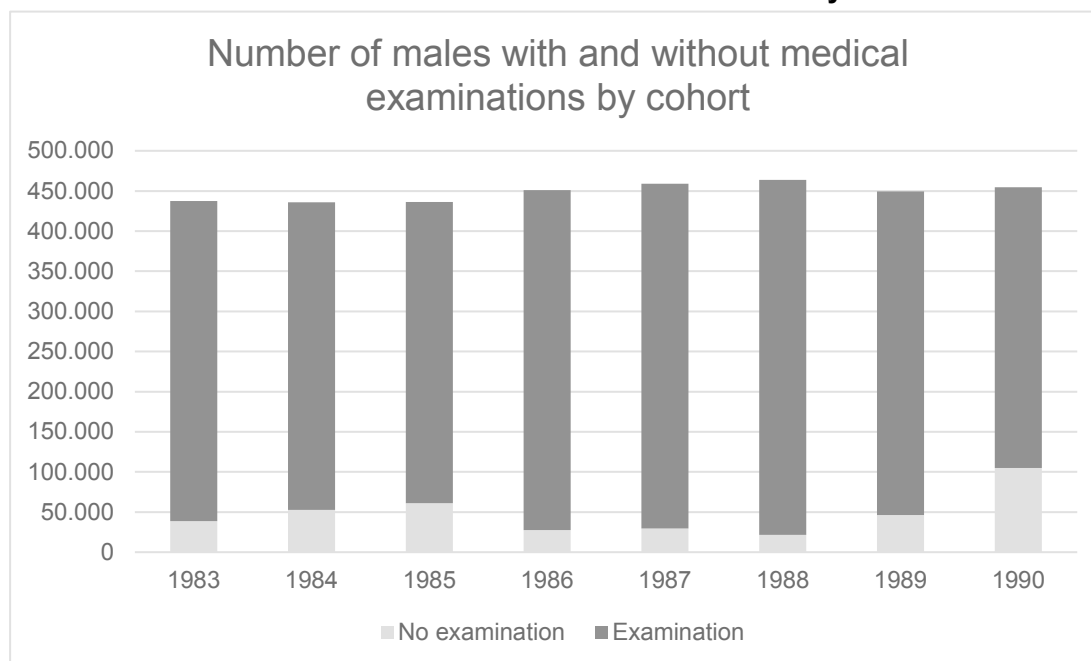
**Figure 1**  
**Number of military conscripts and men in community service by year.**



Source: Statista.de and Bundesamt für Familie und zivilgesellschaftliche Aufgaben, own diagram.

Compulsory services in Germany were accompanied by various sources of uncertainty for young men. First, because the German Armed Forces did not register everyone, as seen in Figure 2, there was uncertainty regarding whether men were required to perform compulsory service. Unintentional and arbitrary disregard was possible at the step of the medical examination and at the step of the draft itself. Second, decisions by the German Armed Forces regarding who was fit to serve were not precisely governed by law. Thus, due to the decreasing demand of the German armed forces for conscripts, the requirements for fitness to serve seem to have increased over the years, which is reflected in a drastic increase in the share of young men classified as unfit to serve (Table 1). Third, additional uncertainty arose because not everyone who was able to do compulsory service (T1 or T2) and did not refuse to provide military service actually had to provide military service. As another symptom of the decreasing demand of the German Armed Forces for conscripts, only approximately two-thirds of those fit to serve were eventually drafted. Using the term *Wehrgerechtigkeit*, this was publicly discussed and claimed to be a lack of justice in Germany. This shows that providing these compulsory services was accompanied by a large amount of dissatisfaction and uncertainty for young men.

**Figure 2**  
**Number of males with and without medical examinations by cohort.**



Notes: The numbers show some inaccuracies between the years (Bundestag, 2011). Thus, descriptive graphs and tables only provide a rough description.

Source: Own figure.

Due to the lack of demand for conscripts as well as the debates concerning *Wehrgerechtigkeit*, a commission to reform the armed forces in June 2010 proposed the abolition of compulsory military service. In November 2010, the governing party (CDU) vowed to suspend compulsory services, and this proposal was adopted by German legislators on December 15, 2010. The compulsory military service and thus the accompanying civilian service were finally suspended on March 1, 2011. The last conscription to compulsory military service was on January 3, 2011 (Table 2 shows the milestones of the reform).

**Table 2**  
**Classification of young men into military service tiers by birth cohort.**

Event	Date
Proposal that compulsory military service should be suspended	June 6 <sup>th</sup> and 7 <sup>th</sup> , 2010
CDU party conference: suspension of compulsory military service	November 15 <sup>th</sup> , 2010
Resolution by the Federal Cabinet	December 15 <sup>th</sup> , 2010
Last conscription to compulsory military service	January 3 <sup>rd</sup> , 2011.
Suspension of compulsory military service	March 1 <sup>st</sup> , 2011

Notes: T3 was abolished in 2004; T6 is not shown because it was just a relabeling of already examined conscripts in T3.

Source: Bundesministerium der Verteidigung (2013) and Strukturkommission der Bundeswehr (2010).

### 3 Theoretical Concepts and Related Literature

#### 3.1 Career Disruptions and Foregone Earnings

As described above, compulsory service entailed uncertainty and career disruptions for young men in Germany. While a smooth transition into one's first employment is

achievable (Brzinsky-Fay and Solga, 2016), disruptions still cause disadvantages in the short and long run (Brzinsky-Fay, 2007; Brzinsky-Fay and Solga, 2016; Manzioni and Mooi-Reci, 2011). Scherer (2005, p. 432) states that a “stable labor market entry is not possible before military service has been completed” (also see: Hogan, 1978). Compulsory service postponed young men’s transitions into training or into the labor market. The service itself and the accompanying organizational procedure caused interruptions due to delays and waiting periods. These interruptions are thought to have mainly affected young men after their compulsory military service when they applied for a subsequent labor market activity.

While all young men faced some sort of disruption as a result of compulsory military service, the impact was greater for some young men than for others. Most young men were drafted at age 18 or 19 (Scherer, 2001, p. 139). At that age, they found themselves in different stages of transition depending on their educational background: they could be at either the first or second threshold. The first threshold between school and vocational training or university influences the occupational choice and unifies complex aspects of identity-building and disentanglement from one’s social origin (Blossfeld, 1988; Pätzold, 2004, p. 569 ff.). This challenging time is often eased by apprenticeships, marginal employment or gap years, which provide insight into occupational areas or function as a moratorium (Gutman and Schoon, 2012; Mortimer, Zimmer-Gembeck, Holmes, and Shanahan, 2002). Compulsory military service might have worked similarly and freed young people at the first threshold to consider their career aspirations.

By contrast, the second threshold in the German system is the more stringent transition from vocational training or university studies into employment. For the first time, young people position themselves in the labor market with their own scope of action (Hurrelmann, 2010, p. 36 f.). This step implies the leap into financial independence. Consequently, disruptions during the second threshold seem to be more severe than those during the first threshold. While grammar school (the German *Gymnasium*) graduates, on average, have just finished their a-levels and find themselves on the first threshold when they are drafted, those who are (lower) secondary school (the German *Hauptschule* and *Realschule*) graduates at the age of conscription have typically already started or even finished vocational training and are approaching the second threshold (Brzinsky-Fay and Solga, 2016). In their case, compulsory service could potentially create a gap in employment with their current employer between vocational training and regular employment, in contrast to graduates from grammar school who have just finished their schooling. This is also likely to raise the perceived opportunity costs of compulsory services: while graduates who plan to attend university suffer no direct impact from foregone income, individuals who have just finished their training and whose alternative to compulsory services would be a regular full-time job experience a more direct effect of foregone income. We therefore assume

that conscription causes more consequential disruptions for (lower) secondary school graduates than for grammar school graduates.<sup>6</sup>

### 3.2 Uncertainty and Forced Labor

In addition to its disruptive character, compulsory military service represented an element of uncertainty in young men's career aspirations. Until its abolition in 2011, it caused uncertainty because, despite its compulsive label, not all young men underwent physical examination, and a declining number were considered fit for military service, as described in the previous section. There are even cases where young men expected a call for compulsory military service but were never drafted. Consequently, young men faced difficulties when planning for their careers before a draft. This unpredictability increased the disruptiveness of compulsory military service in the life courses and early career paths of young men (Beicht and Ulrich, 2008; Heinz, 2002; Hogan, 1978; Scherer, 2005).

With the draft, young men were forced into compulsory military service. If they refused, they required to complete civilian service instead (see chapter Institutional Setting). Refusals of both military service and civilian service (*Totalverweigerung*) were prosecuted. Consequently, compulsory military service resembled forced labor. The prospect of imminent forced labor supposedly reduced young men's well-being because autonomy is a crucial factor in individual well-being (Weidema, 2006).

Since the abolition of compulsory military service in Germany, the share of young participants in gap years, which are usually taken to do voluntary work in the social sector (*Freiwilliges Soziales Jahr FSJ*), has increased. While the opportunity to do voluntary work existed before 2011, a new form called *Bundesfreiwilligendienst BFD* was established as a consequence of the abolition of compulsory military service. Two factors indicate that the BFD does not interfere with our empirical design. First, young men were able to participate in voluntary work prior to the abolition of compulsory service. The introduction of BFD only extended the existing opportunity. Consequently, the life satisfaction of young men should not change considerably as a result of the opportunity for voluntary work. Second, all forms of voluntary work were accessible to men and women equally. If BFD as a new option for voluntary work had any

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<sup>6</sup> While compulsory military service caused disruptions for all young conscripts to some degree, perceptions of that disruption might have differed. For young men who were determined about their vocational aspirations, the disruptions might have caused delays and therefore dissatisfaction. In contrast, undecided young men might have evaluated conscription as a welcome moratorium. Previous studies show that low educated people in particular face problems finding an apprenticeship or vocational training. They often take part in transition measures offered by the Bundesagentur für Arbeit to help them find suitable jobs (Beicht & Ulrich, 2008). Unfortunately, our data do not provide information about the young men's vocational aspirations. The respective group of low educated young men who dropped out of secondary school are also underrepresented in our data. Therefore, we cannot examine this phenomenon, but it should also not influence our results.

effect on young men's life satisfaction, young women should experience the same effect.

If the introduction of the BFD had any effect on young men's life satisfaction, that effect was supposedly caused by a reinterpretation of civilian services on the part of young men themselves. While the actual work tasks remained identical in compulsory civilian services before 2011 and in voluntary work in the social sector after 2011, the only difference was in a changed understanding of the task from compulsory to voluntary. Young men's life satisfaction would increase because they enjoyed voluntary work more than forced labor, strengthening our reasoning regarding the effect of forced labor on life satisfaction. As history shows, the two systems of civil work strongly interlock: the introduction or abolition of one system affects participation in the other. Young draftees' life satisfaction has thus far remained under-researched. This gap is surprising because life satisfaction is arguably both a means and an end to individual actions and has been broadly analyzed from economic (Frey and Stutzer, 2002a), psychological and sociological standpoints (Diener and Diener, 2009; Walsh, Boehm, and Lyubomirsky, 2018; Wüst, 2016). Apart from positive correlations with career success (Walsh et al., 2018; Wüst, 2016) and income (Wolbring, Keuschnigg, and Negele, 2011), life satisfaction is also associated with better health (Faragher, Cass, and Cooper, 2005). One can even argue (Frey and Stutzer, 2002b) that life satisfaction is the ultimate goal of all individual actions. Therefore, it is necessary to learn more about the relationship between life satisfaction and compulsory service, and we aim to address this topic with our empirical analysis. The recent changes in the institutional setting of compulsory military service in Germany in 2011 offer an ideal opportunity to do so, with the abolition of the compulsory military service resembling the conditions of a natural experimental. Given the state of literature and the theoretical considerations, we derive the following hypothesis:

**H1: The abolition of compulsory service increases young males' life satisfaction.**

Furthermore, we assume that the positive effect of the abolition of compulsory services on life satisfaction is mainly driven by uncertainty and career disruptions.

**H2: Reduced uncertainty regarding the future partly explains the positive effect of the abolition.**

**H3: Reduced career disruptions partly explain the positive effect of the abolition.**

## **4 Empirical Strategy and Identification**

### **4.1 Empirical Strategy**

We employ a difference-in-differences (DD) approach to determine the causal effect of the abolition of compulsory military service on individual life satisfaction. We compare the life satisfaction of 18- to-23-year-old males (the treatment group) before and after the abolition of compulsory military service to females in the same age group (our sample includes birth cohorts from 1984 to 1997). As previously described (in



Section 2), males aged 18 to 23 were subject to compulsory service by law, while females were exempt from this obligation. In our treatment group, however, the exact treatment status is unclear (this is further discussed in the results section). Table 3 shows the different possibilities for males and females aged 18-23 before and after the reform. As seen in the table, prior to 2011, males could be treated in several ways. They could already have provided service, they could still be awaiting their medical examination, they could be declared unable to perform service or they could be exempt from service due to, e.g., being fathers. From 2011 onward, compulsory services were abolished. We also provide (in italics) the corresponding channel through which we suspect the treatment might affect life satisfaction, in line with the theoretical considerations. It is also possible that some (older) respondents in our treatment group had already served prior to the abolition of compulsory services. We keep them in the data set to estimate our treatment effects. However, this should bias the overall effect downward. Thus, our baseline specification provides a conservative estimate of the treatment effect.

**Table 3**  
**Treatment status before and after the reform by gender.**

	Males aged 18-23 (Treatment group)	Females aged 18-23 (control group)
Prior to 2011	<p>Already provided service/ in service (<i>Channel: career disruptions</i>)</p> <p>Able to provide service (after examination), before actual service (<i>Channel: Uncertainty</i>)</p> <p>Awaiting examination (<i>Channel: Uncertainty</i>)</p> <p>Not able to do service (after examination)</p> <p>Exempt from service</p>	Not required to do any service
2011 and onward	Not required to do any service	Not required to do any service

Source: Own table.

In the baseline model, we look at the overall effect of these various treatment stages in combination. Furthermore, we also try to isolate two potential channels, career disruptions and uncertainty, through which the treatment could work. Our baseline empirical model, estimated by OLS, can be written as:

$$lifesat_{it} = \beta_0 + \beta_1 male_i + \lambda male_i \times post_t + \gamma Z'_t + \delta X'_{it} + \epsilon_{it}$$

where  $lifesat_{it}$  is individual  $i$ 's life satisfaction in year  $t$ ,  $male$  is an indicator variable that indicates individual  $i$ 's gender (and thus the treatment and control group),  $post$  is an indicator variable that is 0 before the reform (2010) and 1 when the reform comes into effect (2011).  $Z'_t$  is a set of year dummies to capture time trends, and  $X'_{it}$  is a set of individual-level controls. Note that we do not estimate the main effect of  $post$  because it is perfectly collinear with one variable in the set of year dummies.  $\lambda$  is the coefficient of interest and gives the difference in life satisfaction between the treatment



and control group after subtracting the difference in life satisfaction between the treatment and control groups before the reform. Perhaps more intuitively than in the regression equation,  $\lambda$  (not controlling for any covariates included) can be expressed as:

$$\lambda = (\overline{lifesat_m}|post = 1) - (\overline{lifesat_m}|post = 0) - (\overline{lifesat_f}|post = 1) - (\overline{lifesat_f}|post = 0)$$

where the subscript f denotes females and m denotes males, and  $\overline{lifesat}$  is the mean life satisfaction.

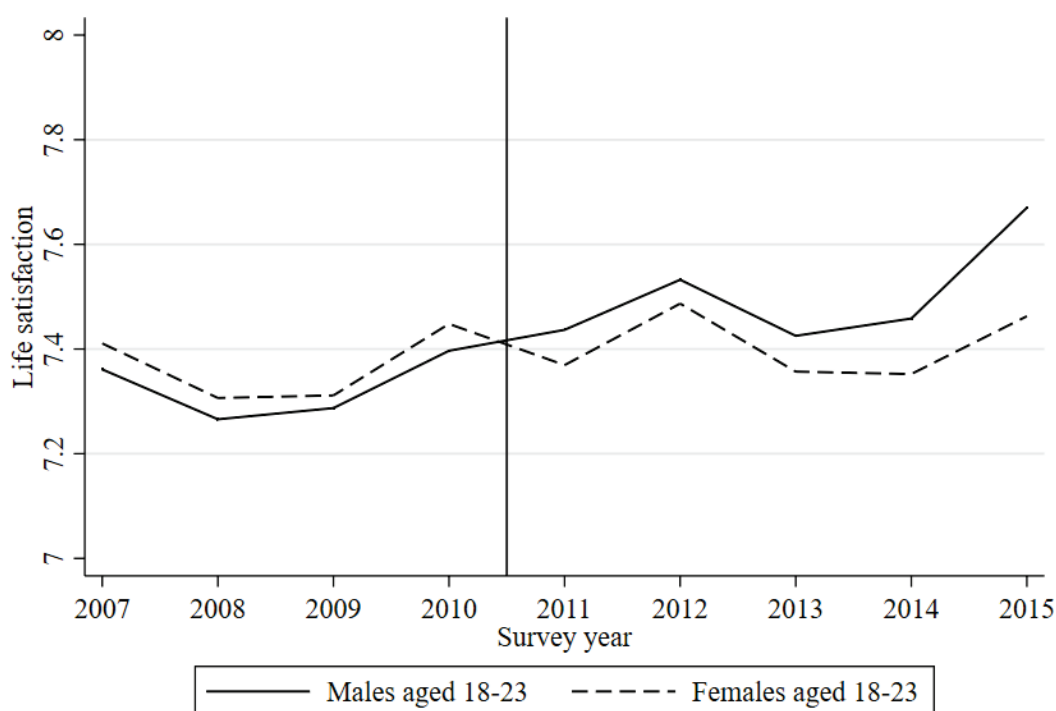
Theoretically, the data would also allow for fixed effects estimations. However, because our empirical design mostly rests on differences between birth cohorts, using fixed effects is not suitable to identify the effect of interest because it mostly rests on between-individual as opposed to within-individual variation. Unsurprisingly, when introducing fixed effects in our regression, the treatment effect is practically zero.

## 4.2 Threats to Identification

To isolate the causal effect of the treatment, one key assumption in this setting is that there would be common trends in life satisfaction between the treatment and control groups in the absence of the treatment. Figure 3 shows the mean life satisfaction by year for the treatment and control groups. Overall, the trends seem to move in parallel. We will further investigate the robustness of the parallel trends assumption after the main analysis using a triple differences approach as well as placebo treatments.

Another central assumption is that there are no contemporaneous events whose effects are captured by the coefficient of interest in our DD design. In our case, these events would only be a problem if they only affected either men or women adversely. To the best of our knowledge, there were no events that occurred contemporaneously with the treatment and only affected young males or females and not both. One could think of the recovery from the great recession as one potential confounding event that affected men more than women due to selection into certain occupations that were especially affected by the crisis. However, the recovery began in 2010 in Germany. Nevertheless, we will also use a triple differences strategy using older males and females (aged 24 to 28) as additional control groups and will investigate trends in life satisfaction in other countries to validate this assumption. In the triple differences setting, a contemporaneous event would pose a problem for our identification only if it solely affected young men or women but not older males or females. As we will show, the results remain stable in the triple differences approach, which is why we conclude that there are likely no confounding events that bias our treatment effect.

**Figure 3**  
**Gender-specific trends in life satisfaction by survey year.**



Jan 2011: Abolition of compulsory military service for males. Males aged 18 to 23 are treated.

Source: SOEP v32, waves 2007-2015, own calculations.

After 2001, several Western German states introduced high school reforms that shortened the duration of higher secondary education from 9 to 8 years. In two states (Bavaria and Lower Saxony), the graduation of the first cohort effect coincided with the abolition of compulsory services. However, these reforms seem to have led to an increase in agreeableness and a decrease in emotional stability, especially among males (Dahmann and Anger, 2014). We suspect that this would be associated with a decrease in life satisfaction. In this case, the expected positive treatment effect is a conservative estimate of the overall effect.

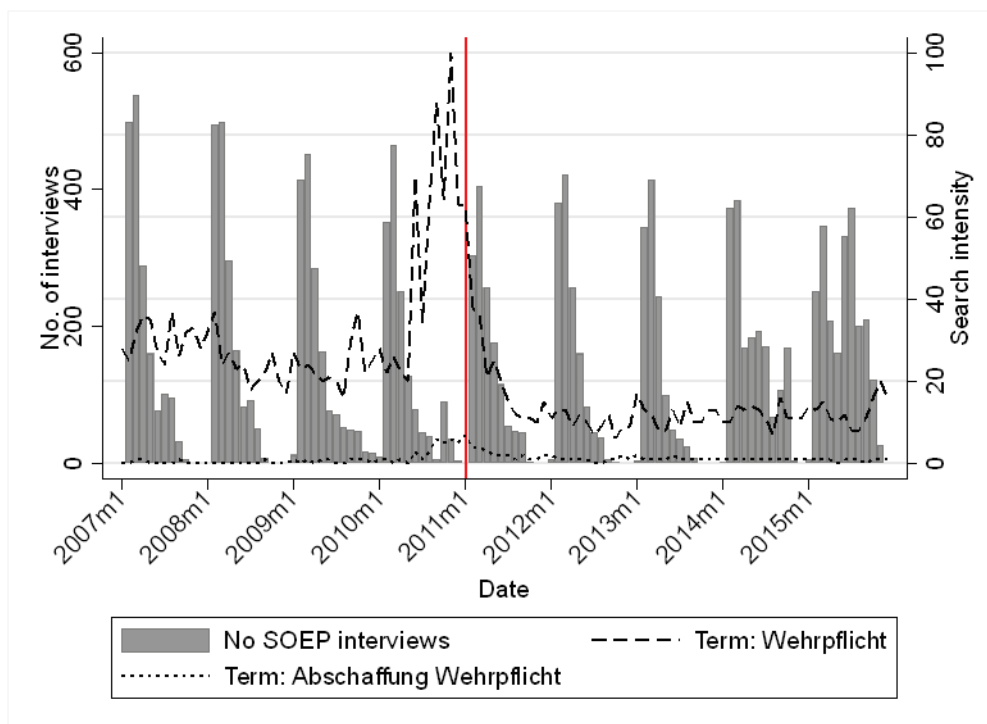
Furthermore, the stable unit treatment value assumption (SUTVA) could be violated if there are spillover effects due to the treatment that affect the control group. For example, the abolition of compulsory services could lead to a shortage of young men in the nursing and care sectors in which a large share of employees are female. Potentially, the abolition of civil service required females to perform tasks that were typically fulfilled by males, such as heavy lifting. If such changes in job characteristics lead to a decline in the life satisfaction of female employees, the SUTVA would be violated. We further investigate this assumption in the robustness section.

Additionally, anticipation effects are a threat to our identification strategy. For example, young males could have anticipated the abolition of compulsory military service and thus could have changed their behavior prior to the treatment. To investigate the potential magnitude of anticipation effects and whether these pose a problem, we merged search intensity (from 0 to 100) data from Google trends for the terms

*Wehrpflicht* (compulsory military service) and *Abschaffung Wehrpflicht* (abolition of compulsory military service) with data on the dates of the SOEP interviews for our estimation sample. Anticipation effects should matter particularly if the interview dates prior to the reform coincide with high intensity for the search terms. Figure 4 plots the number of SOEP interviews and the search intensity over time. Additionally, the vertical line shows the effective date of abolition in the reform process.

Overall, most interviews in 2010 occurred prior to the discussion of the abolition of compulsory military service. The interviews in 2011 began in February, shortly after the treatment occurred. The spike in the search intensity occurs between the interview periods of the SOEP. Thus, anticipation affects should not bias our estimations.

**Figure 4**  
**Number of SOEP interviews and Google search intensity for terms related to compulsory services by month.**



Notes: The vertical line corresponds to the abolition of compulsory services.

Source: SOEP v32, waves 2007-2015 and Google trends; own figure.

## 5 Data

We use the German Socio-Economic Panel (Wagner, Frick, and Schupp, 2007) v32 in our analysis. We restrict our sample to the survey years 2007 to 2015 (the treatment year, four years after and four years prior to the reform enactment) and to males and females aged 18 to 23, in line with the DD approach. The dependent variable of interest is self-assessed life satisfaction from 0 to 10.

**Table 4**  
**Sample descriptive statistics by gender.**

	Males			Females		
	Pre	Post	Difference	Pre	Post	Difference
Life satisfaction	7.322	7.512	0.189***	7.371	7.407	0.036
Age	20.363	20.352	-0.012	20.450	20.343	-0.107**
<i>Education</i>						
lower secondary school (German <i>Hauptschule</i> )	0.158	0.140	-0.018*	0.097	0.085	-0.011
secondary school (Ger- man <i>Realschule</i> )	0.241	0.239	-0.002	0.253	0.229	-0.024*
vocational diploma	0.038	0.044	0.007	0.052	0.042	-0.011*
secondary school (A- levels) (German <i>Abitur</i> )	0.164	0.213	0.048***	0.206	0.258	0.052***
Other	0.011	0.013	0.002	0.018	0.013	-0.005
Left without degree	0.013	0.014	0.002	0.007	0.011	0.004
In school	0.206	0.172	-0.034***	0.198	0.190	-0.008
No information	0.171	0.165	-0.005	0.169	0.171	0.003
<i>Labor market experi- ence (years)</i>						
Full-time	0.302	0.271	-0.030	0.281	0.212	-0.070***
Part-time	0.310	0.345	0.036*	0.439	0.437	-0.002
Unemployed	0.188	0.137	-0.051***	0.191	0.115	-0.075***
Parent: secondary school (German <i>Abitur</i> ) (0/1)	0.264	0.259	-0.006	0.275	0.274	-0.001
ln(monthly household income)	7.872	7.944	0.072***	7.795	7.876	0.081***
East Germany (0/1)	0.223	0.139	-0.084***	0.181	0.141	-0.040***
Share missing values in 1 or more of the above	0.081	0.083	0.002	0.074	0.089	0.014
Observations	3,388	4,123		3,344	4,181	

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: SOEP v32, waves 2007-2015, own calculations.

We use a number of variables to check the robustness of our results and to ensure that any relevant group characteristics did not change between the SOEP waves. We

use age<sup>7</sup> and survey year as control variables in our analysis. Any other control variables such as education or labor market experiences can be considered bad controls (Angrist and Pischke, 2009) because they could themselves be outcomes of the treatment determined contemporaneously with life satisfaction. However, in Table 4, we still show labor market experience, educational categories, monthly household income and parental education for the treatment and the control groups before and after the treatment to ensure that the sample composition did not change drastically during the time of interest. The table shows that balancing does not seem to pose a problem. We also checked whether SOEP sampling (e.g., refreshment samples) varies between the groups; this does not seem to be the case. Overall, our sample consists of 15,036 observations. There are some missing values in some of these variables (most notably household income with 1000 missing values), but we do not exclude these cases from the analysis because the share of missing values does not vary systematically between treatment and control group and the pre- and post-treatment periods.

One concern in the data might be that males prior to 2011 could have married their partners or had children to gain exemption from military service. This is hardly a problem in our data: 99 percent of males prior to 2011 are unmarried, and only 1 percent report having a child. However, we retain these cases in the sample because it is unclear whether they already had their medical examination prior to marriage or the birth of a child.<sup>8</sup>

## 6 Empirical Results

### 6.1 Main Results

Table 5, panel A presents the results of the DD estimations. The first row shows the effect of the abolition of compulsory services on life satisfaction without additional controls. We use clustered standard errors at the individual level to account for the panel structure of our data.<sup>9</sup> This result suggests that the reform led to a statistically significant increase in young males' life satisfaction of 0.153 points on the 0-to-11 scale, thus supporting hypothesis H1. Additionally, controlling for year fixed effects and age hardly changes the results. Thus, our findings support the expectation that the abolition of compulsory services did increase young men's life satisfaction. The

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<sup>7</sup> We compute age as survey year – year of birth. Unfortunately, we have missing information on the month of birth for 21 percent of our sample, and using month of birth to calculate age would thus lead to a large loss of observations. Solely using year of birth and survey years thus leads to an overestimation of individual age in our data. Both treatment and control group are equally subject to this overestimation, and overestimating respondents' age leads to the assignment of some males to the treatment group who would otherwise be exempt from the sample. However, this should downward bias our results because it introduces noise. Additionally, we re-estimated our results using month of birth to calculate age, and we present the results in our robustness section.

<sup>8</sup> This potentially downward biases our results. However, even excluding these (few) cases does not alter our results significantly, as we will show in the robustness section.

<sup>9</sup> We also tried clustering standard errors by survey year as an additional check for the robustness of our results. Individual-level clustering provides larger and thus more conservative standard errors. Thus, we use individual-level clustering.

effect is also economically meaningful: according to the relative pay literature, the effect size of the abolition of compulsory services is comparable to moving from the bottom to the top of the wage distribution for males (e.g. Collischon, 2017).

**Table 5**  
**Results of the DD analysis.**

	(1)	(2)	(3)
	Estimate	SE	N
(A) Main Results			
No controls	0.153**	(0.064)	15,036
Year FE + Age	0.158**	(0.063)	15,036
(B) Age groups (Channel: Uncertainty)			
Age 18 only, year FE + age	0.265**	(0.124)	2,777
Age 18, higher secondary education, year FE + age	0.344**	(0.173)	1,067
Age 18, no secondary education, year FE + age	0.220	(0.171)	1,710
Age 22 only, year FE + age	0.041	(0.137)	2,324
(C) By education (Channel: career disruptions)			
( <i>Fach-</i> ) <i>Abitur</i> , not in education, age > 20, year FE + age	-0.163	(0.124)	2,746
No ( <i>Fach-</i> ) <i>Abitur</i> , not in education, age > 20, year FE + age	0.377***	(0.146)	2,948

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: SOEP v32, waves 2007-2015, own calculations.

In the next step, we exclude individuals aged 23 because we do not know with certainty if they are subject to the treatment. However, the results do not differ significantly from our baseline findings. Overall, we find that the abolition of mandatory services for young males leads to a significant increase in life satisfaction in these groups.

## 6.2 Channels

We want to disentangle the channels through which the abolition of compulsory services could affect life satisfaction. We identify the role of uncertainty (proposed in hypothesis H2) by investigating the treatment effect for 18- and 22-year-olds separately. We do not directly observe whether individuals already had their examination to determine fitness for military or civil service. After the examination, a large part of the uncertainty should disappear. However, we argue that the uncertainty is largest for younger individuals overall because they are more likely to have not yet undergone the medical examination. Thus, if uncertainty is indeed a channel through which compulsory services affect life satisfaction, the treatment effect should be larger for 18-year-olds compared to 22-year-olds. Table 5, panel B shows the results. The treat-

ment effect is largest for 18-year-olds and statistically insignificant for 22-year-old individuals.<sup>10</sup> Additionally, the effect of uncertainty, in contrast to the effect of career disruptions, should be persistent independent of the educational track. Thus, we additionally investigate the effect for 18-year-olds by education, contrasting individuals in higher secondary education with individuals in other tracks. Although the effect for the latter sample is statistically insignificant, we still see economically large effects for both educational groups. Thus, reduced uncertainty seems to be an important channel of the effect of the abolition of compulsory services, in line with H2.

Another channel through which compulsory services could affect life satisfaction is career disruptions, i.e., exiting the labor market during the time of the service (as proposed in hypothesis H3). Unfortunately, using the SOEP, we cannot directly identify who had already participated in either form of mandatory service. The data set includes a question of whether the individual provided any service during the previous year, but this variable contains a large number of missing values – only 19 percent of males aged 18 to 23 up until 2011 report ever having served, a number that does not compare well to the figures presented in the section on the institutional background. Additionally, the SOEP does not sample military barracks; thus, there are likely no conscripts in our sample. Although knowing with certainty who served and who did not would be helpful in identifying the effect of career disruptions, we can also approximate this effect indirectly.

To identify the effect of career disruptions, we compare the treatment effects by educational level. Individuals who completed their A-levels (the German Abitur) likely did not experience a severe career disruption through compulsory services because they mostly served directly after school, when they were 18 or 19 years old. In contrast, individuals with lower levels of schooling, i.e., individuals on the second threshold, according to the theoretical considerations, likely experienced career disruptions through compulsory service because they had already worked or participated in vocational training prior to serving. Thus, if career disruptions are a channel through which compulsory services affect life satisfaction, the treatment effects should be larger for respondents with lower educational degrees. The lower two rows of Table 5, panel (C) show the results. This analysis excludes individuals still in school and below the age of 21 (to avoid capturing effects of uncertainty if they have not been to examination) or for which we do not know the educational degree. The treatment effect is positive and statistically significant only for those without Abitur. We take this finding as evidence supporting the argument that career disruptions are one channel by which compulsory service affects life satisfaction, consistent with H3.

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<sup>10</sup> As an additional check for the robustness of our argument, we used the survey years 2008–2011 of the Pairfam-survey (Brüderl et al., 2015) and re-estimated our DD design with 16 and 17-year-olds. The results are presented in Appendix A and show a positive treatment effect even in this sample. This is further evidence that uncertainty decreases life satisfaction.



### 6.3 Robustness

As previously described, we calculate age as survey year minus birth year in our main analysis. However, this could introduce noise. Thus, we test the robustness of our main findings by calculating age as the interview month in the survey year minus birth month in the birth year. We lose 21 percent of our sample due to missing values in the month-of-birth variable. Panel A in Table 6 shows the results. Although we lose a large share of the sample, the results remain statistically significant and are almost identical to the baseline. Additionally, we exclude male individuals in this age group who are married or report the birth of a child, even though we do not know whether they have already served. Again, this does not change the results significantly and even leads to an increase in the coefficient. The same holds true when combining exclusion reasons and exact birth dates. We also exclude individuals who are 23 years old because they have likely served already or are exempt from service; this is based on exact birth dates and the exclusion reasons discussed above. Again, the results remain significant and of the same magnitude. This confirms the robustness of our main findings.

The key identifying assumptions in a DD approach are common trends between the treatment and control groups in the absence of the treatment. In addition to the graphical evaluation of this assumption, we use several checks to ensure the robustness of our results.

First, we employ a difference-in-differences-in-differences (DDD, also known as triple differences) approach and add an additional control group that is not treated by the reform. In addition to 18-23-year-old females, we use 24-28-year-old males and females born until 1988 (thus, the males included in this group are subject to compulsory service laws) as an additional control group to check the robustness of the common trends assumption. As 24-28-year-old males and females should not be treated, their inclusion in our estimation should not alter the results significantly if there are indeed common trends between the genders in the absence of the treatment. The empirical model for the DDD approach, estimated by OLS, can be written as:

$$lifesat_{it} = \beta_0 + \beta_1 male_i + \beta_2 young_{it} + \beta_3 male_i \times post_t + \beta_4 young_{it} \times post_t + \beta_5 male_i \times young_{it} + \lambda_2 male_i \times post_t \times young_{it} + \gamma Z'_t + \delta X'_{it} + v_{it}$$

In addition to the DD estimation previously employed, young is an indicator variable that takes the value 1 if the individual is aged 18-23 and 0 if the individual is aged 24-28. The coefficient of interest in this case is  $\lambda_2$  and should not differ significantly from  $\lambda$  from the main specification if the treatment only affects young males. Additionally, to account for the broader variation in age and for the additional control group, we add a squared term for age as well as interaction terms between young and survey year fixed effects.



Table 6, panel (B) shows the results. Overall, the DDD estimates even increase in magnitude compared to the baseline results. The results in this specification are statistically insignificant due to the additional noise introduced through the placebo control groups; however, the p-value of the model using year FEs and age as control variables is still 0.104. We also estimate the DDD specification solely using 18- and 22-year-olds to check the robustness of our effect for age groups. As in the baseline estimation, we expect the effect for 18-year-olds to be larger compared to that for 22-year-olds due to the higher degree of uncertainty. The DDD findings confirm our baseline results: the effect for 18-year-olds is approximately three times larger than the treatment effect for 22-year-olds, which is also statistically insignificant. Thus, uncertainty seems to be a key driver of the effect. Additionally, we re-estimate the treatment effect by educational levels using DDD. The findings mirror the results of the DD approach and provide further evidence for the argument that career disruptions are a channel of the effect. Furthermore, the treatment effect on male respondents aged 24-28 is statistically insignificant and negative (last row of panel (B), Table 6). This also shows that events that asymmetrically could have affected males compared to females, such as the recovery from the great recession discussed previously, seem to pose no problem for our identification strategy because then we should see an effect for 24-28 year olds. Overall, the DDD approach supports the robustness of our findings.

Second, the SOEP also contains a question regarding attitude towards the future; this question was asked in 2009 as well as 2014, both within the time frame of our analysis. Respondents were asked to answer the question “If you think about the future: Are you...” with a four-item scale from “optimistic” to “pessimistic”. We create a binary indicator variable for being optimistic about the future and use this variable as a control variable in our DD and DDD approaches to see whether the reform leads to an increase in optimism about the future, as is implied by our considerations concerning uncertainty about the future. A decrease in uncertainty should lead to increased optimism. Closing this channel via controlling for it, we can investigate through changes in the treatment effect whether uncertainty is a plausible channel. Table 6, panel (C) shows the results of our estimations. We show both the DD and DDD with and without controlling for optimism about the future. The results without controlling for optimism reproduce our baseline results with the whole 2007 to 2015 period but are statistically insignificant due to the small sample size. Controlling for optimism leads to a substantial decrease in the treatment effect in both cases. Unfortunately, the sample size used to estimate our subsample estimations within this setting is too small to make reliable statements. Nevertheless, we take our findings concerning optimism about the future as evidence that a decrease in uncertainty is an important channel of our effect.

**Table 6**  
**Robustness tests.**

	(1)	(2)	(3)
	Estimate	SE	N
Baseline results			
Year FE + Age	0.158**	(0.063)	15,036
(A) Noise in the age-variable (Year FE + controls)			
Birth date using month of birth	0.173**	(0.079)	10,973
Exclusion of respondents married / with children	0.161**	(0.064)	14,542
Birth date using month + exclusion of respondents married / with children	0.171**	(0.080)	10,599
Birth date using month + exclusion of respondents married / with children, without age 23	0.148*	(0.086)	9,483
(B) DDD (re-estimations of DD results)			
No controls	0.178	(0.111)	24,527
Year FE + age	0.181	(0.111)	24,527
2010/2011, year FE + age	0.184	(0.155)	5,119
2009-2012, year FE + age	0.198	(0.125)	10,663
Age 18 as treated, year FE + age	0.290*	(0.150)	12,268
Age 22 as treated, year FE + age	0.065	(0.168)	11,815
( <i>Fach-</i> ) <i>Abitur</i> , not in education, age >20	-0.166	(0.178)	6,631
No ( <i>Fach-</i> ) <i>Abitur</i> , not in education, age >20	0.372*	(0.206)	7,388
Ages 24-28 (placebo groups), year FE + age	-0.024	(0.086)	9,491
(C) Controlling for optimism about the future (only 2009 and 2014)			
DD, age w/o controlling for optimism	0.134	(0.108)	3,587
DD, age, controlling for optimism	0.028	(0.103)	3,587
DDD, age w/o controlling for optimism	0.180	(0.187)	5,645
DDD, age, controlling for optimism	0.083	(0.176)	5,645
(D) Placebo Treatments			
(Only 2007-2010)			
Placebo 2008	0.015	(0.082)	6,732
Placebo 2009	0.011	(0.075)	6,732
Placebo 2010	-0.015	(0.086)	6,732

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: SOEP v32, waves 2007-2015, own calculations.

Third, we use placebo treatments pretending that the reform occurred in 2008, 2009 or 2010. In this specification, we drop the treated period (2011-2015) because we are only interested in the common trends prior to the reform date. If the time trends between young males and females differed prior to the reform, the placebo reform effect should be significantly different from zero. Table 6, panel (D) shows the results of the

placebo treatments. As expected, all treatment effects of the placebos are practically 0 and statistically insignificant.

Fourth, we investigate whether the violation of the SUTVA poses a problem. As previously discussed, the abolition of civil service could have led to a shortage of males in the nursing and care sector, and women's tasks in these fields could have shifted towards relatively unpleasant work such as heavy lifting, which could have affected their life satisfaction. To see if these effects matter, we exclude from our analysis those employees in the sectors culture and sports, health and social services and education and teaching. The treatment effect is still of the same magnitude (0.134) and statistically significant ( $p$ -value = 0.04) in this specification. Thus, spillover effects in the labor market should pose no problem.

Fifth, we tested whether there are differences in the treatment effect between East and West Germany. One could, for example, argue that regional differences in labor market opportunities lead to variation in the perceived costs of compulsory services. Arguably, in East Germany, where unemployment rates are higher, the costs of compulsory services should be lower because the outside options are not as good as in West Germany. However, the treatment effects are very similar (0.135 in East and 0.162 in West Germany).

Sixth, one concern might be that global economic phenomena such as an economic upturn after the Great Recession might especially affect young males. In this case, the increase in life satisfaction in 2011 should not be exclusive to Germany. To account for this potential problem, we repeated our analysis with data from the UK (with the UKHLS) and Australia (with the HILDA). We find no comparable effect in these countries, and the results are shown in Appendix B. This further supports the robustness of our findings.

## **7 Discussion and Conclusion**

There is an ongoing debate about reintroducing compulsory military service in Germany, and other European countries have already reintroduced such military service. We assume that compulsory military service caused disruptions and uncertainty in the careers of young German men. Therefore, the abolition should lead to increased life satisfaction.

Applying a difference-in-differences design, we compare young males and females to assess the causal effect of this reform on individual life satisfaction. Our results show statistically significant positive effects of the abolition of conscription on the life satisfaction of young men in Germany. In contrast to the control group of young females, the life satisfaction of young men increases after the suspension of compulsory services. The size of the effect is meaningful compared to other studies investigating life satisfaction. Our analysis provides evidence that disruptions, forgone earnings, uncertainty and forced labor are channels that contribute to this effect: uncertainty and forced labor for young men before the draft, disruptions and foregone earnings for

young men after providing military or civil service. Because of the abolition of compulsory services, young men gained the power to plan their futures and were unburdened by career disruptions. The results show a larger increase in satisfaction for (lower) secondary school graduates than for graduates with an Abitur. This finding suggests that the disruption of compulsory military service hit lower educated youths harder because they were more advanced in the labor market at conscription age.

One could argue that gains in later life, such as higher employability, offset the immediate negative effects of compulsory military service. However, studies find no career advantage for men who had served in compulsory military service compared to other men (Bauer et al., 2012, 2014; Paloyo, 2010).

Apart from these testable mechanisms, other channels might explain our findings. Independence and social comparison appear to be the most important ones: The compulsory nature of the military service used to cause a twofold situation of dependence for young men in Germany. On the one hand, they were dependent on the invitation of the conscription's recruiting board in terms of deadlines. Subsequent vocational trainings, academic studies or employment had to be postponed until the end of compulsory services. On the other hand, the military service itself strongly insisted on obedience and caused a traditionalization of young men's attitudes by forced compliance.<sup>11</sup> Although the alternative of civilian service was possible and caused less compliance, obligations in terms of timing and deadlines were still enforced. Consequently, compulsory services forced young men into a situation of dependence and lack of autonomy. Psychological theories show the importance of independence and autonomy in the life satisfaction of youth (Caprara, Steca, Gerbino, Paciello, and Vecchio, 2006; Crocco and Costigan, 2007; Ryan and Deci, 2000; Sen, 1995) and among adolescents (Ferguson, Kasser, and Jahng, 2011; Wüst, 2016). Potentially, a distaste for compulsory service due to fear of these obligations and the temporary loss of independence can also partly explain our findings.

Though it was compulsory, military or civilian service was obligatory only for young men, while young women faced no equivalent obligation. This gender-specific duty might have been perceived as unfair. Easterlin's psychological model states that happiness and life satisfaction are strongly affected by social comparison rather than by the actual gain of money, freedom, time or other assets (Easterlin, 1995). Against this background, young males – in contrast to their female counterparts – experienced a time-consuming disadvantage as a result of compulsory military service. Compared to their same-aged females, young men are enabled by the abolition of compulsory

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<sup>11</sup> As a study by Mummendey, Schiebel and Sturm shows, compulsory military service caused a shift in attitudes towards traditionalization (according to the measure of conservatism *Bevorzugung autoritärer Erziehung*), which the authors of the study claim to be an adaptation of young men's attitudes to their actions following the theory of forced compliance. This means that the discrepancy between young men's attitudes and the actions imposed on them in military service was resolved via a change in attitude (Mummendey, Schiebel, & Sturm, 1985).

military service to reach a state of equality, which supposedly leads to increased life satisfaction due to the elimination of this particular disadvantage. One drawback of our study, however, is that we cannot identify all the channels that cause an increase in life satisfaction among young men.

Our results provide information about the implications of the abolition of compulsory military service in Germany on young males' life satisfaction. With regard to current debates in various countries on reintroducing compulsory military service, our study suggests that such policies decrease the life satisfaction of affected individuals if the effect is symmetrical to the abolition.

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## Appendix A: Additional analyses using the Pairfam

As an additional check for the robustness of our results concerning uncertainty as a potential channel through which the reform could affect life satisfaction, we draw on data from the Pairfam (Panel Analysis of Intimate Relationships and Family Dynamics) using the survey years 2008 - 2011 and the birth cohorts 1991-1993. We retain in our data only males and females aged 16 and 17, leaving us with 6,186 observations. Males in this age group are certainly not subject to compulsory services; thus, if we find a treatment effect in this group, uncertainty concerning the future is the only plausible channel. We only use survey year as a control variable; life satisfaction is surveyed from 0 to 10 as in our main analysis.

**Table A1**  
**DD estimation using the Pairfam.**

	(1)	(2)	(3)
	Estimate	SE	N
(A) DD estimation	0.242*	(0.147)	6,186
(B) Placebo Treatments			
(Only 2008-2010)			
Placebo 2009	-0.067	(0.084)	5,726
Placebo 2010	0.072	(0.092)	5,726

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: Pairfam Release 6, own calculations.

Table A1 presents the results. As displayed in panel A, there is a positive and statistically significant treatment effect for 16- and 17-year-old males. This suggests that uncertainty seems to be an important channel for the treatment effect, supporting our main findings. Additionally, we ran placebo treatments (Table A1, panel B) to ensure that a violation of the parallel trends assumption is not a problem in this sample. The placebo effects are insignificant and small in magnitude, thus indicating that the parallel trends assumption seems to hold.

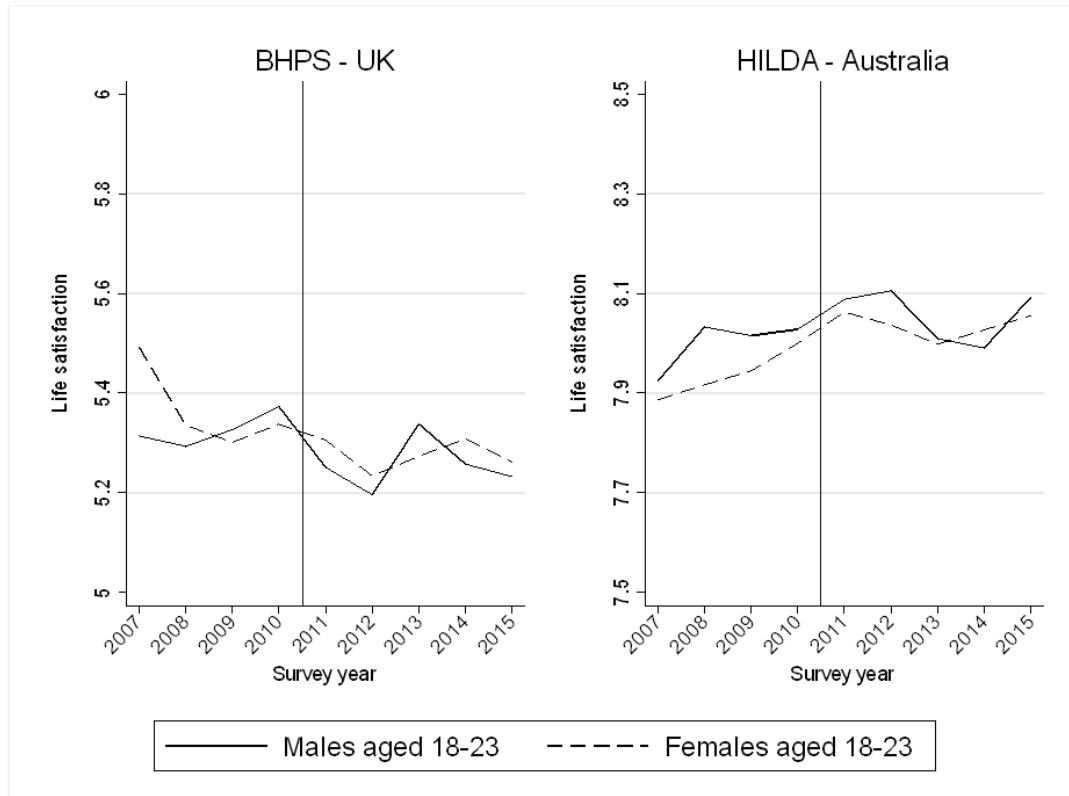
## Appendix B: Additional analyses using the HILDA and UKHLS

One concern might be that there are other confounding factors that could bias our results, e.g., recovery from the Great Recession. For example, if the Great Recession especially hit young men compared to women, the upturn after the crash could also especially target young males and would thus be a confounding factor. To test the robustness of our results to global phenomena, we replicate our analysis for the UK, using the British Household Panel Study (BHPS) and its follow-up, the UK Household Longitudinal Study (UKHLS, also known as Understanding Society), and for Australia using the Household and Income Dynamics in Australia (HILDA) Survey.

Both surveys ask respondents to rate their overall life satisfaction on a 1-to-7 scale in the UKHLS and on a 0-to-10 scale in the HILDA. We restrict our samples to the period

from 2007 to 2015 and to the age group of 18 to 23, resulting in 7,499 observations in the UKHLS and 16,174 observations in the HILDA. Figure B1 plots the trends in life satisfaction by gender over time. As seen, there seems to be no jump for males in 2011, unlike in Germany.

**Figure B1**  
**Gender-specific trends in life satisfaction by survey year for UK and Australia.**



Source: BHPS and HILDA, waves 2007-2015, own calculations.

Additionally, we perform placebo analyses with these countries, pretending that there was a reform in 2011. We control for survey year fixed effects as well as a linear age trend. Table B1 presents the results. The placebo treatments are statistically insignificant and practically zero in both cases. We thus conclude that global economic trends do not drive our results.

**Table B1**  
**DD estimation using the UKHLS and HILDA**

	(1)	(2)	(3)
	Estimate	SE	N
UKHLS – Placebo 2011	-0.006	(0.108)	7,499
HILDA – Placebo 2011	-0.040	(0.055)	16, 174

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: UKHLS and HILDA, waves 2007-2015, own calculations.

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