Consequences of Forced Labor Conscription: Evidence from Dutch Civilians after WWII*

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Abstract

There is limited evidence on the long-term consequences of forced labor, despite it being a frequent event in the previous century and still happening nowadays. I study the consequences of labor coercion for individual labor market outcomes. Cohorts of Dutch civilians faced a differential probability of labor coercion in Nazi Germany during WWII. Using Dutch census data from 1971 and Eurobarometer survey data from 1975 to 1994, I exploit the discontinuity in conscription at the date of birth to study effects of conscription on long-term labor market success in a Regression Discontinuity Design. I find that conscripted individuals have lower education, income, and likelihood of employment. Studying heterogeneous effects, I find that facing harsher conditions in Germany is associated with lower labor force participation and worse health. My findings suggest that the negative effect on labor force participation is mitigated by being forced to work in sectors that pay better than the sectors present in the Netherlands.

Keywords: Labor Economic History, Coercive Labor Market, Forced Labor

JEL Codes: N34, N44, J24, J47

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1 Introduction

In today's economy, forced labor remains to be a large issue. According to estimates by the International Labor Office (ILO), around 27.6 million people worked in some type of forced labor in 2021, which is defined as any work or service that is being extracted from a person under a threat of penalty, and for which the person has not offered themselves voluntarily (ILO, 2022). Studying the effects of contemporaneous systems of forced labor is challenging because of data limitations and safety concerns for affected workers (LeBaron, 2018). Additionally, the factors that contribute to the vulnerability of being exposed to forced labor may be correlated with the outcomes we are interested in (ILO, 2022). Moreover, current forced labor systems do not allow for studying long-term consequences.

I, therefore, turn to the historical system of forced labor set up by Germany during World War II (WWII) to study the consequences of facing labor coercion for individuals' later labor market success During the war, there was a rising labor shortage in Germany which resulted from drafting men for military service and expanding the armaments industry. The German government thus filled this gap with civilians of occupied countries (Spoerer, 2001). I exploit quasi-experimental variation in the assignment into forced labor in the Netherlands, where in May 1943 the occupational regime decided to conscript all men born between 1922 and 1924 (aged 18–21 at the time) for labor in Germany[1]. The coercion was enforced through the withholding of food ration cards and forbidding businesses to employ men born in these years. Archival records point to a compliance of at least 37%. Assignment into industry was done irrespective of previous skills. The majority (68%) of the Dutch civilian workers were employed in manufacturing and construction (Herbert, 1999). Most of the Dutch workers survived the forced labor experience and returned to the Netherlands after the end of the war (Tooze, 2006).

Exploiting the exogenous variation in being subject to conscription by the German forces

[1]Exceptions were granted to men who were already working in industries that were strategic to the war effort.

based on birth date in the Dutch context, I compare later educational attainment, income, likelihood of employment, and skill level of occupation of individuals who were born within the years that were conscripted to that of individuals who were born before the conscripted years. More formally, I employ a regression discontinuity design at the cutoff of January 1, 1922 of the forced conscription policy[2]. I estimate an intention to treat effect, where the treatment of conscription into forced labor encompasses both the forced labor experience as well as the need to go into hiding to avoid transportation to Germany.

Using Dutch census data from 1971, I find that individuals born in the conscripted cohort have lower labor market success compared to those born before. Their highest educational attainment is lower by 2.4% of one standard deviation, their income is lower by 2.1% of one standard deviation, and their likelihood of being employed is 0.68%p. lower, which translates to 2.9% of one standard deviation[3]. I find no differences in an individual's occupational level (ranging from unskilled workers to executives and professionals) if employed. To shed light on contributing factors, I study the effects on physical and psychological health. While I find no differences in the average need for assistance in daily life based on the 1971 Census, I do find suggestive evidence for lower subjective life satisfaction for individuals from the conscripted cohort using Eurobarometer survey data covering the period from 1975 to 1994. I find no differences in family formation, both for marital status as well as the probability of having children.

Since the treatment consists of either being forced to work in Germany or being forced to go into hiding, I construct a measure of the share of conscripted individuals who went to Germany to differentiate between the two treatment types. I then allow for heterogeneous effects based on which type of treatment was more likely, forced labor or hiding. The effects

[2] I focus on the lower cutoff date because the control group on the upper cutoff of 31.12.1924, individuals born in 1925, were the oldest cohort conscripted for the Indonesian War of Independence in 1946 (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022), thereby violating the assumption of nothing else changing at the cutoff

[3] The point estimate for education is -0.041, where educational attainment goes from 0 (only basic education) to 8 (finished university). The point estimate for income is -0.0289, where income is measured as yearly income in brackets of 4,000 Gulden (1,815 EUR or 2000 USD), meaning that on average the conscripted individuals' income is lower by 115 Gulden (52 EUR or 57 USD).

are similar for both, implying that the treatment effect is driven by both being forced to work and being forced to go into hiding. To construct this share of conscripted individuals who went to Germany, I link the census data with archival records on forced workers during WWII provided by the Arolsen Archives[4]. Though insignificant, the results suggest that individuals from municipalities that went to Germany at a higher rate tend to hold slightly more skilled occupations, while the effect is negative for individuals who went into hiding at a higher rate, pointing to some skill acquisition during the forced labor experience.

Based on the location of forced workers in Germany, I show that the negative effects on later labor market success are most pronounced for individuals who had a higher exposure to severe living conditions. I proxy the severity of forced labor in Germany by the share of houses damaged due to bombings and the distance to so-called labor re-education camps which served as punishment for forced workers and calculate the average weighted exposure to severe conditions for forced workers from each Dutch municipality. I find that the lower probability of being employed is driven solely by conscripted individuals who were exposed to more severe conditions in Germany, pointing to adverse health effects as a possible mechanism for the long-lasting lower labor force participation. To corroborate these results, I repeat the heterogeneity analysis for the likelihood of needing assistance in daily life in 1971 as a proxy for health and find that the effects are also larger for conscripted individuals from places with higher exposure to severe conditions.

My findings suggest that the negative effect on labor force participation is mitigated by being forced to work in sectors that pay better than the sectors present in the Netherlands.

I also present some suggestive evidence that the negative effect on labor force participation is mitigated by being forced to work in sectors that pay better than the sectors present in an individual's place of origin in the Netherlands. Specifically, I derive the difference in employment share in industries that pay above the median in 1971 in the German counties

[4]The data from the Arolsen Archives includes the name, place, and date of birth, and the location in Germany. I classify an individual's gender using their first name and name frequencies from Meertens Instituut (nd). Since the census data lacks information on names, I link the data based on gender, date of birth, and place of birth.

and the Dutch municipalities and split the sample based on these differences. The idea is that individuals from Dutch municipalities who went to German counties that on average had a higher share of high-paying industries than their home municipality had a higher likelihood to gain skills in high-paying industries than they would have had at home. Individuals from Dutch municipalities that went to counties with on average less high-paying industries would have been more likely to acquire skills in industries that were less well-paying in the 1970s than if they had not been conscripted into forced labor. The negative effects of forced labor on labor force participation are driven completely by individuals who had a downgrade in their type of industry exposure, which could be due to their less valuable skills.

The results are robust to several robustness checks, including different specifications of the RDD equation, the use of different bandwidths, and the inclusion of individuals of the Dutch Hunger Winter regions. Additionally, I estimate placebo estimates with the cutoff at different years, showing that there are no significant differences in labor market success at these cutoffs.

The effects which I estimate are likely a conservative lower bound of the persistent effects of forced labor. This is because the control group to whom I compare the treated cohorts were also affected by living in an occupied country during WWII. While I exclude individuals from regions affected by the Dutch Hunger Winter in my baseline estimation to ensure that this is not biasing my results, other war-related factors that only the control group experienced could still lead to an underestimation of effect sizes[5]. Since there is no perfect compliance with forced labor conscription, the intention to treat effect is also likely lower than the local average treatment effect. Because the Dutch forced workers were treated relatively better than forced workers of other nationalities, the effects for forced workers of other occupied countries would probably be larger than for those of Dutch civilians. This was also mirrored by German policymakers when deciding on compensation for affected former forced workers in the early 2000s, as they excluded Western forced workers, including the Dutch ones, due to

[5]The Dutch Hunger Winter took place in the winter of 1944-1945 when the forced workers were still in Germany and affected only the urban regions in the west

a lack of discriminatory living conditions (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). My findings contradict this assessment, showing that especially those Dutch forced workers who faced more severe conditions in Germany did suffer from long-lasting effects on their labor market success and possibly their health.

My paper contributes to four main strands of literature. First, my paper contributes to the literature on forced labor. I add to this literature by studying the consequences that forced labor has on individuals, separating the effects for individuals from the effect of forced labor systems on institutions. Previous studies have compared regions with more or less intensive use of labor coercion (Bertocchi and Dimico, 2014; Buggle and Nafziger, 2021; Buonanno and Vargas, 2019; Cinnirella and Hornung, 2016; Dell, 2010; Fujiwaray et al., 2017; Mitchener and McLean, 2003; Nunn, 2008; Soares et al., 2012; Markevich and Zhuravskaya, 2018). While these studies show that forced labor has persistent negative consequences, they all compare different regions. In these cases, persistence is often driven by the institutions that were shaped by the forced labor systems. In the case of Dutch civilians being coerced into labor in Germany, both the group intended for the treatment and the control group live under the same institutions in the Netherlands after the end of WWII, but only differ in their exposure to forced labor conscription[6].

Additionally, I contribute to the literature on the consequences of forced workers by analyzing a setting that is similar to the experience of many workers who are coerced into labor today, thereby giving important insights into the consequences of such an experience of forced labor: According to a report by the ILO, forced workers are more likely to be migrants, to be male, and more likely to work in manufacturing and construction. Additionally, the setting of being transported to a foreign country for a limited amount of time is a form of forced labor that is still very prevalent today (ILO, 2022). By studying Dutch male individuals who were transported to Germany against their will, who were predominantly

[6]There exists previous literature on individual-level effects of forced combat and military conscription (Angrist et al., 2010; Blattman and Annan, 2010; Hjalmarsson and Lindquist, 2019), but by focusing on the effects of being exposed to labor conscription outside of military service and combat, which is a treatment in and of itself, my paper can further the understanding of forced labor in a civilian context.

employed in manufacturing and construction, and who returned to their home country, my paper can give important insights into what policy may be necessary to support former forced workers who have had a similar experience of forced labor and have since returned to their home country.

Using a historical example allows me to circumvent concerns of endangering current workers subject to coercion and of data quality. The exogenous conscription into forced labor by year of birth applied by the Germans constitutes a natural experiment that alleviates endogeneity concerns when identifying causal effects of forced labor experiences. These endogeneity concerns arise because especially vulnerable groups of people are faced with coercion of some kind to enter such a forced labor "employment" (ILO, 2022), and the underlying factors for their vulnerability may also affect individuals' labor market outcomes.

Second, this paper also contributes to the literature on forced migration by studying a setting in which the forced migrants were able to return to their home country after about two years, in contrast to previously studied settings, where the forced migration is permanent and a return to the migrants' locations of origin is impossible in the long-run. The existing literature found both positive and negative effects depending on the specific economic situations of migrants before their forced migration, as well as their receiving location (Bauer et al., 2013; Bauer et al., 2019; Becker et al., 2020; Becker, 2022; Sarvimäki et al., 2022). More broadly, my paper adds to the literature on the consequences of facing adverse events such as hunger, war, or natural disasters (Braun and Stuhler, 2023; Conti et al., 2024; Deryugina et al., 2018; Kesternich et al., 2014).

Third, this paper also speaks to the literature on the consequences of the type of labor market conditions individuals face when entering the labor market after graduating, by studying a setting where young individuals are forced into a certain type of initial employment in contrast to situations where young workers face lower paying and lower quality jobs due to recessions (Oreopoulos et al., 2012; Schwandt and von Wachter, 2020; von Wachter, 2020). I contribute to this literature by studying a case of young workers who at the beginning of their career are forced to work in industries they did not choose or go into hiding. My results show that such early experiences can have a long-lasting effect on later labor market decisions.

Finally, my paper also adds to the literature on the forced labor regime by Germany during WWII by being the first one to empirically evaluate the long-term consequences for workers after WWII had ended and the forced labor regime was abolished. While there is ample research on it from a historical perspective (Herbert, 1999; Pfahlmann, 1968; Homze, 1967; Sijes, 1966; Spoerer, 2001; Spoerer and Fleischhacker, 2002), neither its economic aspects nor the consequences of forced labor after the war and have been studied thoroughly.

2 Historical Background

During WWII, the German economy faced an intense labor shortage due to the expansion of the armaments industry and the drafting of men for fighting at the front. Replacing the missing men with women was an unpopular policy because it went again the Nazi ideology of women's roles as housewives and mothers. The *Reichsarbeitsministerium* (Ministry of Labor) therefore set out to recruit civilians of occupied countries, first by advertising to unemployed workers, and later by using coercion (Spoerer, 2001). Since it was more efficient to produce in Germany than in the occupied countries, most of these civilians were transported to Germany to work there (Tooze, 2006).

In the Netherlands, the occupying regime announced in May 1943 that they would conscript all men of specific age groups for work in Germany (the so-called *Yearclass Action*). In June 1943, the cohort of men born in 1924 was the first to be transported to Germany, and in August the cohorts of 1923 and 1922 followed. The Yearclass Action was initially scheduled to include all men born between 1908 and 1925, but due to concerns of turmoil in the Dutch population because of the unpopularity of the conscription of age groups, the other birth cohorts were ultimately not called upon for forced labor. Coercion was executed by withholding food ration cards and forbidding firms to employ men from these cohorts.

In total, at least 77,200 Dutch forced workers born between 1922 and 1924 were recorded in Germany during WWII in the data by the Arolsen Archives. Comparing that to a total of around 210,500 men born in these years according to the 1971 census, compliance was at least around 37%[7] Men already working in war-related industries were granted exemptions, which applied to around 16% (Sijes, 1966). The remaining men went into hiding with the help of the resistance, who forged food ration cards and helped with the placement of men of the conscripted age groups into hiding locations using false documents (Sijes, 1966; Warmbrunn, 1972). The situations in hiding varied, but in general men lived away from their homes, with limited contact to their social networks to avoid being found, and were encouraged to take up keep up a regular schedule or take up a hobby to avoid boredom (Warmbrunn, 1972).

The conscripted individuals who were taken to Germany were distributed irrespective of their skills or previous training and over large parts of Germany. Figure **??** shows the regional distribution of Dutch male forced workers born in the conscripted years 1922–1924 over the German counties. The men were assigned quasi-randomly into the specific locations and industries, based on local labor shortages at the time of their deportation that companies reported to their local *Arbeitsamt* (employment office). The administrative effort of recording and assignment based on previous skills and training was deemed too costly (Kuck, 2010; Marx, 2019). The majority of the Dutch forced workers were employed in in manufacturing and construction[8], and the pay was lower than that for German workers (Herbert, 1999; Sijes, 1966; Tooze, 2006).

The living conditions varied widely, as firms were responsible for housing and feeding the forced workers (Althausen, 1999). Most Dutch workers were housed together in barrack camps or repurposed public buildings. Food supply and nutrition was often of low quality, the access to medical care was scarce or non-existent, and both deteriorated as the bombing of the allied forces intensified (Sijes, 1966). In case of any so-called nondisciplinary conduct

[7]On average, the share of individuals who were present in Germany at that time was ADD NUMBER.
[8]Figure 14 in the appendix shows the number of forced workers in different industries.

such as sabotage or absenteeism, forced workers were sentenced to stays in so-called *Arbeitserziehungslager* (labor education camps) for several weeks, where conditions were similar to those in concentration camps (Lofti, 2000). While the forced workers were mostly promised yearly contracts at deportation, the majority of workers were not allowed to leave after that period and had to stay until the end of the war (Beening, 2003). When workers tried to flee to return to the Netherlands, they faced being sent to labor education camps and then being brought back to Germany upon being captured (Kuck, 2010).

The estimates on deaths of Dutch forced workers in Germany range from 5,000 to 29,000, meaning that based on the total estimated number of Dutch forced workers of somewhere between 450,000 and 530,000, between 93% and 99% survived the war (Beening, 2003; CBS, 1947; Spoerer, 2001; Warmbrunn, 1972). After the successive liberation of Germany in 1945, the Allied Forces organized the transports of former forced workers back to their home countries. By September 1945, 98% of all Dutch persons present in Germany at the end of the war had returned to the Netherlands (Grüter and Mourik, 2020; Proudfoot, 1957). At their return, the forced workers faced stigma because their labor for Germany was seen as collaboration with the enemy. Therefore, most of them stayed silent about what happened to them during the war. Only in the 1980s, a public debate about the experiences of the forced labors began in the Netherlands (Kuck (2010))[9].

The control group to which I compare the experiences of the individuals subject to the forced labor conscription was also not unaffected by the experience of living through WWII in an occupied country, and this may have affected their labor market prospects as well. However, the Dutch economy was doing comparatively well: In 1945, the Dutch GDP was 86% of that of 1938, and industrial capacity in 1945 was larger than before the war (Lak (2016)). Both the treatment and the control group were exposed to the war. Secondary education typically finished at nineteen years old, so the individuals around the cutoff of January 1, 1922 should have completed all schooling but university already by the start of

[9]The first book interviewing former forced workers about their experiences was published in April of 2024 (?)

the Yearclass action in May 1943 (Warmbrunn, 1972). In 1940, only 4% percent attended higher education, and universities mostly stopped operating from 1943 onwards, so neither the control nor the treatment group would have had significant higher education during the period of conscription until the end of the war in 1945 (Van Eden, 1946; Warmbrunn, 1972).

One experience that only the control group faced which may affect later labor market outcomes is the Dutch hunger winter, which took place between November 1944 and May 1945 in urban regions in the West (de Zwarte, 2020). While the groups that suffered the most directly were infants and older people, Ramirez and Haas (2022) show that there are negative effects on education for adolescents of up to 14 years old (which is when their sample ends). I therefore exclude individuals from areas affected by the hunger winter in my baseline sample to abstract from any possible differences driven by the hunger experience[10].

There was no systematic large-scale enlisting of men into the *Wehrmacht* that differed for age-groups[11]. For the Indonesian War of Independence (1949–1949), the cohorts of 1925, 1926 and 1927 were conscripted (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022)[12]. Thus, at the upper cutoff of the forced labor conscription cohort window (31.12.1924), the conscription for the Indonesian War of Independence is a factor that only affected the possible control group, making it impossible to identify the effects of forced labor conscription during WWII as conscription for military service also affects labor market success (Angrist et al., 2010; Blattman and Annan, 2010; Hjalmarsson and Lindquist, 2019). The cutoff for school enrollment was in summer (Richardson, 2000).

In 2000, the German government set up a fund to pay symbolic amounts of compensation to former forced workers following pressure due to impeding law suits of former forced workers against German companies located in the United States. Depending on the severity

[10]Also men who went into hiding were probably less affected by the hunger winter, as most hiding locations were rural areas (Warmbrunn, 1972)

[11]Military conscription for the Dutch armed forces came to a halt with the capitulation of the Netherlands in May 1940 (Jongbloed, 1996). Around 40,000 men were conscripted into building coastal defense constructions in 1944, but not based on their date of birth (Sijes, 1966)

[12]In total, around 100,000 conscripted soliders were drafted (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022). The cohort size of these three years of birth is around 200,000, so a substantial part of these men were drafted.

of the treatment, individuals were paid between 572 EUR and 7,760 EUR (629 USD to 8,546 USD). Individuals who were deported and had to work in manufacturing were paid 2,560 EUR (2819 USD) as a recognition of the the harsher conditions compared to those working in the agricultural sector. However, because of the limited sum of the compensation program and because of the lower severity of their experience and a "lack of deportation and discriminating living conditions", forced workers from Western countries were excluded from this compensation program unless they had been working in a concentration camp or other comparable places of detainment (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). Thus, only 4,500 Dutch individuals received a compensation through this program, despite the fact that the majority of the around 500,000 Dutch civilian workers were involuntarily deported from the Netherlands, and the majority of them worked in the industry sector (77% according to Herbert (1999)).

3 Data

3.1 Dutch Census Data

To measure the effects of being conscripted into this forced labor system, I individual-level admin data from the 1971 census (*14de Algemene Volkstelling*) which is a comprehensive census of the Dutch population[13].. To identify treatment and control group, I use the individuals' gender, month of birth, and year of birth. The potential treatment group is defined as all men born in 1922, 1923 and 1924. The potential control group are individuals born in the three years prior to the conscription, so 1921, 1920 and 1919. The individuals are thus between 46 and 52 years old at the time of the census.

The outcomes to measure labor market success that I use are the highest educational attainment (ranging from 0 for only basic education to 8 for a university degree), labor income class (reported in 6 different income brackets of 4,000 Dutch Gulden (1,815 EUR

[13] The non-response rate was 0.2%

or 2000 USD)), a dummy variable of whether a person is employed, and occupational rank (ranging from 0 to 8 and measuring the type of position a person has if employed, from unskilled support personnel to executives and professionals). I also use the marital status and a dummy for whether an individual has children as a proxy for their social situation, and a person's need for assistance in everyday life as a proxy for their health. Table 1 shows the descriptive statistics of these variables[14].[15]

The 1971 Census does not include the municipality of birth directly, but only the current municipality and an indicator for whether an individual still lives in their municipality of birth (excluding temporary absences such as war-related reasons). I therefore only know the place of birth for non-movers (199,797 from the total 411,192 of the three-years sample). I exclude individuals born in the municipalities that were affected by the Dutch Hunger Winter following Conti et al. (2024)[16]. The sample then includes 356,681 observations. When merging further data sources based on the place of birth, I further restrict my analysis to individuals who did not move. This reduces my sample to 145,286 observations.

3.2 Eurobarometer

To investigate effects on psychological well-being, and to study effects after 1971, I use Eurobarometer survey data which includes a question on self-reported life satisfaction. The Eurobarometer is a survey done in all member countries of the European Union and samples 1,000 random individuals per country in every survey round. I use all Eurobarometer survey waves since 1975, when age was first recorded, until 1994, when the youngest individuals in the potential treatment group would be 70 years old (Kommission Der Europäischen Gemeinschaften, 2012). This amounts to 50 waves in total[17]. Since I only know an individuals' age and not their exact date of birth, there is a subset of individuals for whom I do not know

[14] The sample is based on the median optimal bandwidth of 15 months.

[15] The non-response rates are around 17% for educational attainment and 7% for income, but these numbers are similar for treatment and control group, see table 1

[16] These municipalities are Amsterdam, Delft, The Hague, Haarlem, Leiden, Rotterdam and Utrecht.

[17] This includes waves three through 42.

whether they were born in the potential treatment years (1922-1924)[18]. I therefore exclude these individuals in my analysis.

The variables of interest are years of education (ranging from 7 to 15), income class (ranging from 1 to 12 with brackets of typically 250 Gulden (114 EUR or 125 USD)), a dummy for whether a person is employed, occupational rank (ranging from 1 to 7, again measuring the type of position a person has if employed, from unskilled support personnel to executives and professionals), marital status, parental status, and subjective life satisfaction (ranging from 0 to 3)[19]. Table 5 shows the descriptive statistics of the variables of interest.

3.3 Individual Archival Records

To disentangle the bundled treatment of being forced to work in Germany and being forced to go into hiding, and to investigate heterogeneities based on the forced labor experience in Germany, I supplement the data with archival records on forced workers during WWII provided by the Arolsen Archives. The archive evolved from the International Tracing Service (ITS) established by the Allied forces, and its aim is to document and trace victims of the Nazi regime. The majority of the data originates from registration efforts by the Allied forces after WWII to organize the transport of the displaced persons back to their country of origin. I use data on so-called displaced persons, who are defined as individuals who had been deported by the Nazi regime (Höschler and Panek, 2019). While these do include prisoners of war and former inmates of concentration camps, the vast majority of them are forced workers[20]. The number of total unique Dutch individuals in the archival data of around 473,000 also matches the historical estimates of Dutch forced workers of somewhere

[18] To give an example of an individual with an uncertain treatment status, imagine a person who reports to be 53 years old at the time of the third Eurobarometer survey of June 1975. They were thus born between June 1921 and June 1922 and could be part of the treatment cohort (born 1922) or the control cohort (born 1921).

[19] The exact wording of the question for life satisfaction is "Taking all things together, how would you say things are these days - would you say you're very happy, fairly happy, or not too happy these days?"

[20] One estimate taken from a statistic on Dutch individuals returning from Germany at the end of the war put the share of forced workers of all Dutch individuals who returned after WWII at 92.5%, while the prisoners of war make up another 3.6%, and inmates of concentration camps make up 3.9% (Lagrou, 1999)

between 450,000 and 530,000 (CBS, 1947; Spoerer, 2001). I therefore will assume that all individuals in this dataset are forced workers. Considering that some forced workers fled or were allowed to leave before the end of the war and are therefore missing from the records, this should not bias my results significantly.

The data includes information on the full name, date of birth, location of birth, and the location where the person stayed in Germany. The original sample consists of 594,967 observations. Some individuals may show up more than once in the data because multiple sources have been aggregated for the archival records. I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another and exclude the double-counting of individuals. I follow Abramitzky et al. (2021) and adjust their algorithm slightly to exploit the data structure of the archival records[21]. This reduces my sample to 473,406 individuals.

Since the archival data does not include the gender of the individuals, I use the data on first names and combine this with information on name frequency based on gender from the Corpus of First Names in the Netherlands published by the Meertens Institut to classify individuals as male or female (Meertens Instituut, nd)[22]. For the relevant cohorts of 1922–1924, there are 84.2% male and 10.7% female individuals[23]. I restrict the sample to unique male individuals from the cohorts of 1922, 1923 and 1924, which reduces the number of observations to 72,898 observations.

I link the archival records to the census data using the place of birth[24]. This information is available for 40.1% of the sample. I employ a fuzzy merge complemented with manually coded municipalities[25]. The final sample consists of 24,151 observations that I am able to link to the census data[26].

Comparing the sample size of each municipality and each month of birth, I calculate the

[21]See section 5.1.1 for a detailed description of my approach

[22]See section 5.1.1 for a detailed description of my approach.

[23]For the remaining 5.1%, I was unable to assign a gender based on their first name.

[24]A linking on an individual level is not possible, since the 1971 Census does not include information on the name and exact date of birth

[25]See section 5.1.1 for a detailed description of my method.

[26]I was thus able to link 82.6% of all observations which have information on their place of birth

average share of conscripted individuals who were deported to Germany[27].

3.4 Forced Labor Experience

To study heterogeneities based on the type of forced labor experience that individuals faced, I add data on exposure to severe conditions in Germany, and data on the industry structure in Germany and the Netherlands to the data.

3.4.1 Exposure to severe conditions

I proxy severe conditions in Germany by two measures, exposure to war and distance to so-called labor education camps, to which forced labor were sentenced in case of disobedience. The intuition is that forced workers suffered most in areas with lots of bombings, as shelters were often reserved for German citizens and they were often forced to clean up after the bombings. It is also plausible that forced workers faced a higher probability to be sent to a labor education camp if one was close-by, as these stays were temporary and the forced workers were supposed to return to their former occupation after the sentence ended. The data on war exposure comes from Peters (2022) and measures the share of houses damaged during the war by Allied bombings. This data is available for West Germany, where also the majority of Dutch forced workers were located (see figure 1). The locations of labor education camps come from a map by Lofti (2000) which I geocoded. Both variables are then aggregated to the level of Dutch municipalities by calculating the weighted average over all German counties, using the number of male conscripted forced workers present in these counties as the weights.

3.4.2 Industry Structure

I proxy the industry in which forced workers were employed by the employment shares in German counties in which the forced workers were present using the 1939 occupational census

[27]In cases where the number of forced workers in the archival records is larger than the number in the census, I restrict the compliance measure to one.

(Braun and Franke, 2021). The underlying assumptions are that the industry structure of 1939 is similar to that of 1943–1945, when the Dutch forced workers were present in Germany, and that the forced workers were distributed across industries according to the industry composition of the county to which they were deported to. I then link this data to the 1971 Census by again calculating the weighted average based on where forced workers from these municipalities were located in Germany.

To investigate whether the effects on labor market success and occupational choice differ depending on how different the industry composition was at the German county during the forced labor stay compared to the setting from where individuals originally come from, I use data from the Beroepstelling 1930 (CBS Central bureau of Statistics, 1934) to measure the employment shares at the location of origin. Due to lack of individual-level data of occupation before the war, I again have to assume that the likelihood of an individual working in a specific industry is according to the industry composition in their municipality of birth.

4 Empirical Strategy and Results

4.1 Labor Market Outcomes

4.1.1 Empirical Strategy

The challenge when identifying causal effects of forced labor experiences on later labor market outcomes is to find a suitable control group, which could have also been subject to the forced labor, but, for reasons exogenous to their labor market performance, did not share this experience of being forced to work in an employment that they did not chose for themselves. Typically, especially vulnerable groups of people are faced with coercion of some kind to enter such a forced labor "employment" (ILO, 2022), and this vulnerability could possibly translate into different labor market outcomes compared to people who were able to evade the coercion, regardless of the forced labor experience. Using the historical setting of the forced labor regime in WWII as a natural experiment allows me to avoid this endogeneity concern. While all years of the cohorts of 1908–1925 were considered for conscription through the Yearclass action, only the cohorts of 1922–1924 were actually drafted. Thus, individuals born before the cutoff of January 1, 1922 pose a suitable control group since the reason that they were not drafted was not due to differences in any underlying characteristics that may also affect labor market outcomes, as they were deemed as suitable for forced labor as the actually drafted cohorts.

I exploit the exogenous assignment into forced labor based on an individuals' year of birth by using a fuzzy Regression Discontinuity Design (RDD) and compare individuals born just within the conscription period to those born just outside of the conscription period. The identifying assumption is that individuals born after January 1st are similar to those born before, and labor market success would be smooth in the absence of treatment[28]

The identifying assumption of RDD relies on the fact that there are no other discrete changes at the cutoff of forced labor recruitment which could potentially affect labor market success (Cattaneo et al., 2019). To the best of my knowledge, there were no other policies that changed discontinuously at the cutoff dates of the conscription policy (January 1, 1922). The cutoff for school enrollment was mid-year (Richardson, 2000) and the limited conscription into military during WWII was not based on age (Sijes, 1966). The oldest cohort conscripted for the Indonesian War of Independence was 1925 (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022)[29]. Both treatment and control group were subject to the war, but only the control group would have experienced the Dutch Hunger Winter in 1944–1945. I therefore exclude men from municipalities affected by the Hunger Winter.

[28]I thus rely on a continuity based identification The alternative of Local Randomization relies on the assumption that potential outcomes are unrelated to the running variable, which is violated because of age effects, since age is related to a persons labor market success. Using a discrete running variable such as month of birth in the continuity-based RDD is appropriate if the number of mass points is sufficiently large(Cattaneo et al., 2024). Since my potential treatment window is three years, this assumption is satisfied.

[29]Lacking pre-treatment individual level data, I cannot check for continuity at the cutoff of labor market outcomes prior to the treatment. outcomes available in the census might also have been affected by labor conscription.

Another identifying assumption of RDD is that individuals cannot manipulate the running variable. While a persons' date of birth is generally exogenous, it is possible that individuals may have forged their birth certificates to evade treatment and thereby sort into the control group, and the motivation for this manipulation may also be correlated with underlying characteristics that affect an individuals' later labor market trajectory. For this to bias my results, individuals would have to still use the documents with their incorrect date of birth in 1971. As the incentive to lie about their date of birth ceased after the end of WWII, this is unlikely. Figure 2 depicts the density of month of birth in the 1971 census. The distribution is flat and there seems to be no discontinuous bunching left and right of the conscription period.

If these assumptions are fulfilled, then any difference at the cutoff can be attributed to the treatment effect. In my setting, the treatment of forced labor conscription is a bundle of different experiences: For individuals who were deported to Germany it entails being forcibly moved to another country, then being forced to work in an occupation that is not freely chosen, possibly being subject to harsh living conditions and punishments, and having to hide this traumatic experience due to the associated stigma. In the case of those who went into hiding, the treatment consists of having to leave their known environment, living in fear of being found, and often having no formal employment (Warmbrunn, 1972)[30].

Some individuals born within the conscribed years were granted an exemption and had to endure neither forced labor nor going into hiding, and some individuals born outside of these years still faced forced labor because they were coerced through other measures than the Yearclass action. So there are non-compliers with the treatment assignment in both the control and the treatment group. I estimate a reduced form of a Fuzzy RDD, where I exploit that the probability of treatment discontinuously changes at the cutoff of conscription, using the Dutch census of 1971. The estimation equation takes the following form:

[30]See section 2 for a detailed discussion of both experiences.

$$Y_i = \beta_0 + \beta_1 1 MonthofBirth_i \geq c + \beta_2 MonthofBirth_i + \beta_3 MonthofBirth_i^2 + \epsilon_i \qquad (1)$$

Y_i are labor market outcomes, specifically educational attainment, income class, employment status, and occupational rank, which measures the type of position a person has if employed. $MonthofBirth_i$ is the running variable and c is the cutoff (January 1, 1922). $1\{MonthofBirth_i \geq c\}$ is the indicator for treatment, which is one for treated individuals and zero for the control group. The coefficient β_1 is the intention to treat (ITT) effect, which is the effect of being subject to conscription into forced labor, irrespective of actual compliance, compared to individuals who were born outside the conscribed years and were thus less likely to face forced labor or going into hiding. This effect is thus a lower bound of the true effect of being subject to forced labor conscription as the control group includes individuals also affected by forced labor, and the treatment group includes individuals who were able to avoid forced labor and going into hiding. I include a linear and a quadratic term of the running variable $MonthofBirth_i$ following Gelman and Imbens (2019)[31]. I a bandwidth of 15 months, which is the median of the optimal bandwidths from all labor market outcomes, based on the MSE-optimal bandwidth selection and a triangular kernel as suggested by Cattaneo et al. (2019)[32].

4.1.2 Results

Figure 3 shows the average outcomes for each month of birth and the corresponding function estimated using equation (1), with a bandwidth of 15 months[33]. I find that the highest educational attainment (measured from 0 to 8) is lower by 0.041 levels, which corresponds to 2.4% of one standard deviation. The income class is lower by 0.0289 levels, which corresponds

[31] I perform robustness checks using only a polynomial of one of the running variable and including an interaction term of the running variable and the treatment indicator $1\{MonthofBirth_i \geq c\}$, see figure 9.

[32] I perform robustness checks using different bandwidths and uniform kernel, see figure 9

[33] The underlying regression results are shown in table 2

to 2.1% of one standard deviation. Since the income is classified into levels by brackets of 4000 Gulden (1,815 EUR or 2000 USD) and measured as yearly labor income, this implies the forced labor conscription lowers the yearly labor income by 115 Gulden (52 EUR or 57 USD). Compared to the average income class (2.92), this is a reduction of 1% of the mean. The effect on the likelihood to be employed is -0.68%p, which is 2.9% of the standard deviation. I find no differences in an individual's occupational rank if employed, implying that the type of position that individuals hold once they do enter the labor force do not differ significantly.

The results are robust to a number of different specifications. Figure 9 shows the estimates for regressions different specifications of the RDD equation: Using only a polynomial of one for the running variable $MonthofBirth_i$, including an interaction term of the running variable and the treatment indicator $1\{MonthofBirth_i \geq c\}$ to allow for different slopes on both sides of the cutoff, using different bandwidths (half and double of the optimal bandwidth), using a uniform kernel, running a Poisson regression for outcomes that are count variables, and including individuals from the Hunger Winter regions.

I also estimate placebo specifications, shifting cutoff to years where nothing should have changed at January 1st that affects labor market success. Figure 10 shows the results for four different placebo cutoffs, which are all insignificant for educational attainment, income class and employment status[34].

4.1.3 Effects at older age: Eurobarometer

At the time of the census in 1971, the individuals around the cutoff were around 49 years old. To check whether the effects are similar for later stages in life, I use 50 waves of the Eurobarometer survey from 1975 to 1994. I infer the year of birth based on the individuals' reported age at the time of the survey[35] Since I only know an individuals age, I estimate

[34] I used cutoffs where the three years on both sides of the cutoff only incldue the control group and again took to median optimal bandwidth

[35] I exclude individuals for whom I cannot tell whether they were part of the control or the treatment group.

simple differences instead of an RDD[36]. I define the following treatment and control group, consisting of one age group respectively: The older control group consists of men born in 1920 or 1921, the older treatment group are men born in 1922 or 1923.

I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \tag{2}$$

where λ_t are wave fixed effects to control for differences in survey design between different waves. The outcomes of interest Y_{it} are years of education, income class, employment status and occupational rank (measuring the type of position a person has if employed). Figure 4 shows the results[37]. While I find no significant differences in years of education or income, I again find that individuals of the treatment group are 5.1% less likely to participate in the labor market which is 10.4% of one standard deviation. The occupational rank is lower but insignificant..

4.2 Secondary Outcomes

So far, I have looked at the effects of forced labor conscription on a persons' labor market success. However, it is insightful to understand whether other areas of life were also affected by this drastic experience. I therefore look at indicators for physical health, psychological well-being, and family formation. This can also give rise to suggestive evidence for contributing factors which could explain the mechanisms through which the forced labor conscription hampers labor market success. However, since the lower labor market trajectory may in turn affect an individuals' health and social situation, I cannot disentangle the way in which causality runs. I can however study which spheres of an individuals' life are affected by being conscribed into forced labor and thus give a broader picture of possible consequences.

[36]Using only the age as the running variable would violate the assumption of a sufficiently large number of mass points described by Cattaneo et al. (2024).

[37]Table 6 shows the full regression results

4.2.1 Health and Well-being

As a proxy for physical health, I use the response to the question of the 1971 census whether an individual is in need of assistance by others for their own care, household tasks, or for getting to places outside of their home. In the census data, not needing help and not answering the question is both coded the same, so the results have to be interpreted with caution. Figure 5 shows the average likelihood of declaring a need for assistance for each month of birth of Dutch men around the cutoff and the results from estimating equation(1) using a bandwidth of 15 months bandwidth[38][39]. There is no significant jump at the cutoff, indicating no detrimental effect on physical health for conscripted individuals. However, the need for assistance is a quite severe outcome, and the individuals in the sample are between 45 to 50 years old, so it is possible that less severe differences in health that do not lead to the need for assistance in daily life do not (yet) show up in the results.

To not only look at physical health but also at consequences on individuals' well-being, I turn to the Eurobarometer data and estimate equation (2) with a measure of life satisfaction as the dependent variable which I harmonized to a scale from zero to three. Figure 6 suggests that there may be a negative effect on life satisfaction for treated individuals, even though the difference is just barely so insignificant.

4.2.2 Family Formation

To understand what consequences the conscription into forced labor has on the individuals' social life outside of their labor market experience, I look at marital status and parental status, meaning the probability to have children.

[38] The median of the optimal bandwidth for the secondary outcomes of need for assistance, marital status and parental status is 12 months. To make the results comparable to the analysis on labor market outcomes, I kept the optimal bandwidth of 15 months.

[39] Table 4 shows the full regression results.

Figure 7 shows the average likelihood to be married and to have children as reported in the Dutch census of 1971 in a 15 month bandwidth around the cutoff, and the results from equation (1)[40]. There is no discontinuous difference between the treatment and control group at the cutoff for both marital status and the probability to have children, meaning that at the time of the census, when individuals were aged around 49 years old, forced labor conscription does not seem to affect family formation. [41]. Figure 8 confirms these results also for later stages in life using the Eurobarometer data, estimating equation (2) with marital status and parental status as outcome variables.

4.3 Heterogeneities

4.3.1 Compliance

Until now, I estimated the intention to treat effect, where the treatment is a bundle of being forced to work in Germany and being forced to go into hiding. To disentangle between these two factors, I conduct a heterogeneity analysis based on the share of conscripted individuals from each Dutch municipality that can be found in the archival records. since the ability and probability to go into hiding may be in part driven by factors that could also affect labor market outcomes, the results are suggestive evidence and should not be interpreted causally[42].

The 1971 Census does not include the municipality of birth itself, but only the current municipality and an indicator for whether an individual still lives in their municipality of birth (excluding temporary absences such as war-related reasons). I therefore first restrict the census data to non-movers which leaves me with 62,319 observations in the sample with a 15 month bandwidth.

[40]Marital Status is a dummy that takes the value of one for ever being married (including widowed, living separately and divorced), and zero otherwise

[41]Table 3 shows the full regression results.

[42]For example, men from families that were better connected may have had an easier time to go into hiding, and this network may also be beneficial for landing high-paying jobs.

For each Dutch municipality, I then compared the sample size of men born in the respective municipality and within the treatment group to the numbers of men from that date and place of birth who show up in the cleaned and linked data by the Arolsen Archives on forced workers in Germany. Because I was only able to link a subset of individuals, this number is a lower bound of the actual share of men who were deported to Germany from each municipality. Figure ADD FIGURE shows the regional distribution of the share of deported conscripted men over the Dutch municipalities.

I then split the sample by municipalities with a deportation share above and below median[43] and repeat the analysis for labor market outcomes by estimating equation (1) first for the new baseline of non-movers and then separately for each sample. The results are displayed in figure 11a. In this new baseline, the effects for education and income become insignificant and close to zero, while the effect on employment probability is still negative and significant.

The effects are similar for both groups, suggesting that the negative effect of treatment comes from both going into hiding and from the forced labor experience itself. Though insignificant, the results suggest that individuals from municipalities that went to Germany at a higher rate tend to hold slightly more skilled occupations, while the effect is negative for individuals who went into hiding at a higher rate, pointing to some skill acquisition during the forced labor experience.

4.3.2 Exposure to severe conditions

While the insignificant results for differences in need for assistance points to no adverse health effects for forced workers, the results on subjective well-being suggest that the forced labor experience may have affected conscripted men psychologically. I therefore conduct a heterogeneity analysis based on severity of the forced labor experience in Germany to understand whether being exposed to harsher circumstances during the forced labor period

[43]The median share of deported conscripted men is ADD NUMBER

may be a contributing factor for the negative effects on labor market success.

Using information on the location of forced workers in Germany and data on the share of houses damaged due to bombings and the distance to so-called labor education camps, to which forced workers were sent in case of disobedience, I construct a measure of average weighted severity of the forced labor experience that individuals from for each Dutch municipality faced. As shelters were often reserved for Germans, and living conditions for forced workers deteriorated with more severe Allied bombings (Sijes, 1966), it is likely that forced workers in areas with a high number of Allied bombings suffered more. The likelihood to be sentenced to a (temporary) stay at a labor education camp was probably also higher for individuals in counties close to the next such camp. Figure ADD FIGURE shows the regional distribution of both measures of severe conditions over the Dutch municipalities.

I again split the sample based on whether an individual is from a municipality with an above or below median exposure to severe conditions[44]. Figure 11b shows the results for the sample split by the averge weighted share of houses damaged, and figure 11c shwos the results for the split by average weighted distance to the nearest labor education camp. Since the regional distribution of forced workers in Germany was arguably exogenous (see section 2 for a detailed discussion), the differences in exposure to severe conditions should also be as good as randomly assigned.

The lower probability of being employed is driven solely by individuals from Dutch municipalities where forced workers were more exposed to more severe conditions in Germany (both for the share of houses damaged and the distance to labor education camps). This points to adverse health effects as a possible contributing factor for the long-lasting lower labor force participation. To corroborate these results, I repeat heterogeneity analysis for the likelihood to need assistance in daily life in 1971 as a proxy for health. I find that individuals exposed to more severe conditions have a higher likelihood to need assistance, which is in line with the interpretation that a severe forced labor experience is one explanation for the

[44]The median average weighted share of houses damaged in Germany is ADD NUMBER, and the median average weighted distance to a labor education camp is ADD NUMBER.

lower labor force participation of treated individuals in the 1971.

4.3.3 Industry Structure

Because the assignment into occupations in Germany was done irrespective of prior skills and training, but only based on local labor shortages at the time of deportation, the forced labor experience can also be thought of as a random assignment into a specific industry. I therefore want to understand whether the human capital acquired in certain industries during the forced labor experience may have plaid a role in shaping later labor market success.

I conduct a heterogeneity analysis based on whether the type of industries to which forced workers were exposed to in Germany differed from the industries present in their home municipalities. Specifically, I identify the industry sectors which pay above the median of all sectors in 1971, and then calculate the employment share in these high-paying industries for both the German counties and the Dutch municipalities based on employment censuses from the 1930s. I then assign each Dutch municipality the average employment share in high-paying industries to which the forced workers were exposed to in Germany, again weighted by the number of conscripted forced workers who went to each German county. The underlying assumption here is that individuals were more likely to be employed in a specific sector if it was more prevalent in their respective location (both in the Netherlands and in Germany).

I split the sample based on differences in the employment share of high-paying industries in the home municipality and the German counties: Individuals from Dutch municipalities who on average went to German counties that had a higher share of high-paying industries than their home municipality had a higher likelihood to gain skills in high-paying industries than they would have had at home. On the other hand, individuals from Dutch municipalities who went to counties with less high-paying industries would have been more likely to acquire skills in industries which were less well-paying in the 1970s than if they had not been conscripted into forced labor. Figure 11d shows that the negative effects of forced labor on employment are driven completely by the latter group, so individuals who had a downgrade in their type of industry exposure.

One possible explanation for this finding could be that forced workers who were more likely to gain experience in high-paying industries in Germany have higher earning opportunities in the Netherlands, thus not negatively impacting their labor force participation. Workers who acquired work experience in lower-paying industries than in the absence of conscription on the other hand have a disadvantage in their possible earnings in 1971, thereby lowering their expected earnings if employed and thus lowering their labor force participation. To more formally test for this, I repeated the heterogeneity analysis with an indicator variable for whether an individual is employed in such a high-paying industry in 1971 (excluding unemployed individuals). Figure 13 shows that neither group of forced workers differ significantly in their probability to be employed in a high-paying industry, but the directions of the effect would be in line with the notion that exposure to higher high-paying industries does indeed increase the probability to later be employed in such an industry, while the opposite effect has a negative sign.

5 Conclusion

In this project, I study how being conscripted into forced labor affects later labor market success. I exploit exogenous variation in the probability to be experience forced labor by studying the setting of Dutch male civilians who were conscripted to work in Germany during WWII based on their date of birth. Using a Regression Discontinuity Design, I find that individuals who were conscripted into forced labor (were either deported to Germany or were forced to go into hiding) have lower labor market success in 1971, more than 25 years after the former forced workers returned to the Netherlands in 1945.

Specifically, I find that individuals who were born within the conscripted cohorts have lower education, lower income and a lower likelihood to be employed. These negative effects are mostly driven by individuals who were exposed to harsher living conditions while in

Germany. For this group, the forced labor conscription is also associated with worse physical health. Suggestive evidence also points towards lower psychological well-being of conscripted individuals. Taken together, this implies that the forced labor conscription led to persistent lower labor market success due to adverse effects on an individuals' health and well-being. I also present evidence that being exposed to higher-paying industries in Germany than in an individuals' place of origin mitigates some of the negative effects, possibly due to forced workers being exposed and thus shifting their occupation to higher-paying sectors during their forced labor experience. I find no effect of being conscripted into forced labor on family formation.

When applying my findings to contexts beyond the forced labor regime of Nazi Germany, a key policy implication is the need to provide adequate support to former forced workers upon their return to heir home countries to avoid that the possibly traumatic experiences lead to long-term disadvantages in the labor market. In the Netherlands, the former forced workers faced suspicion of collaboration with the enemy, leading them to remain silent about their experiences. This may have prevented affected individuals from seeking help, thus exacerbating and perpetuating their losses on the labor market.

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Figures

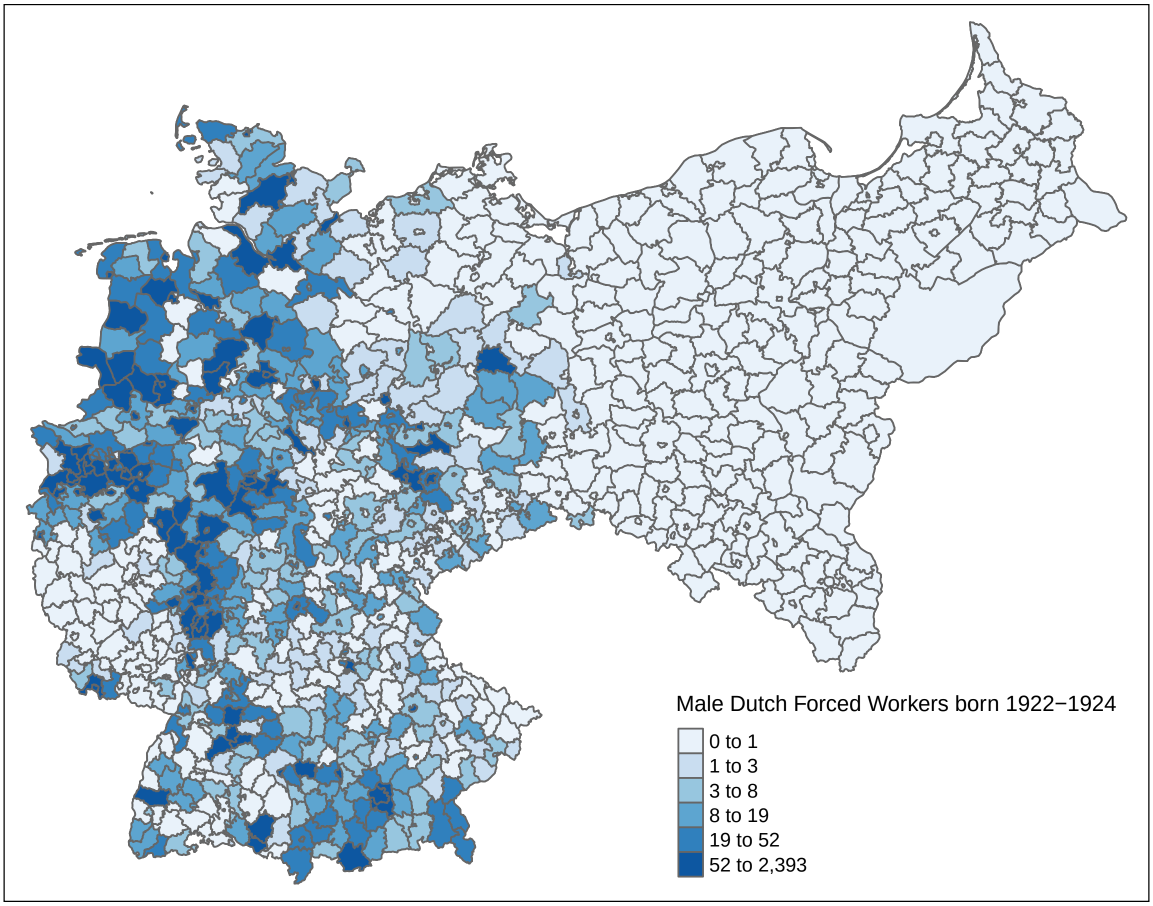


Figure 1: Regional Distribution of Male Dutch Forced Workers born 1922-1924 across German counties

Notes. This figure shows the number of male Dutch forced workers born between 1922 and 1924 based on data by the Arolsen Archives, excluding double-entries of identical individuals. Gender was assigned using first names and data from Meertens Instituut (nd).

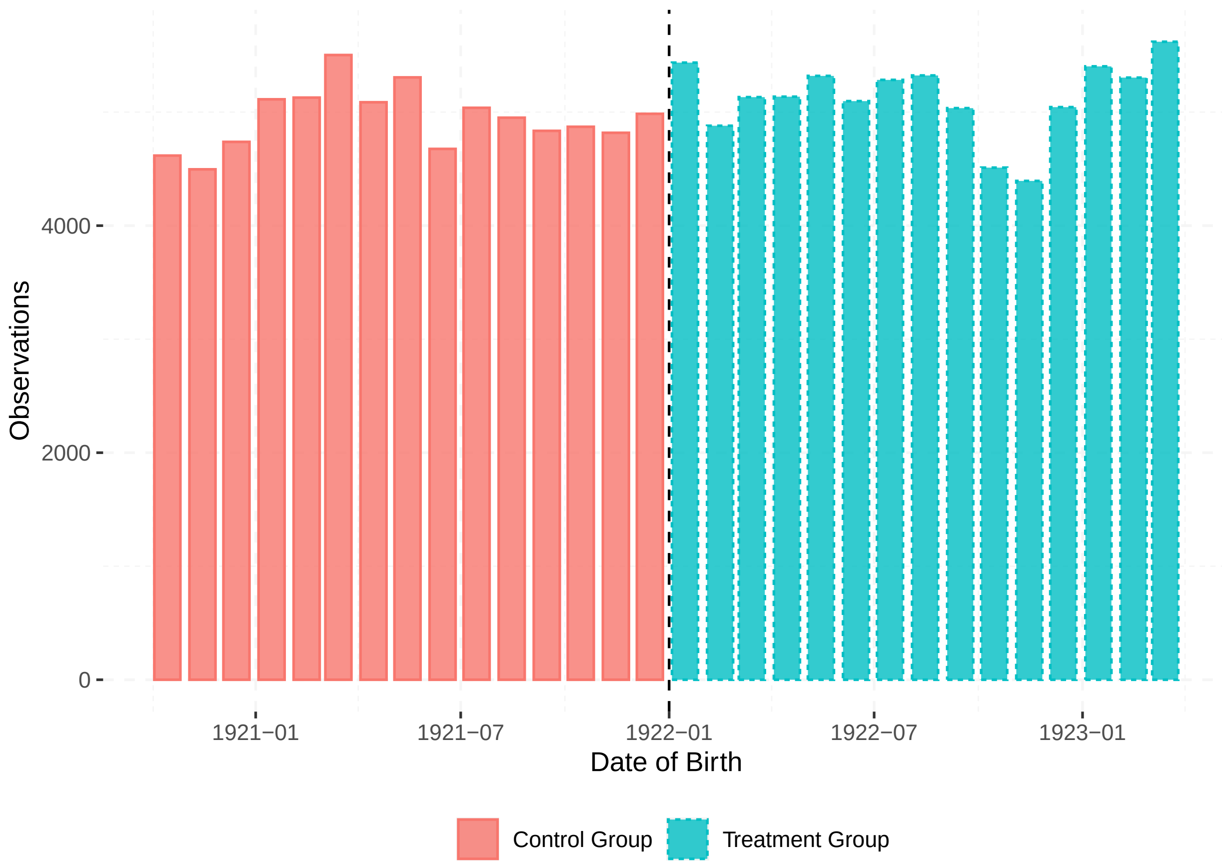
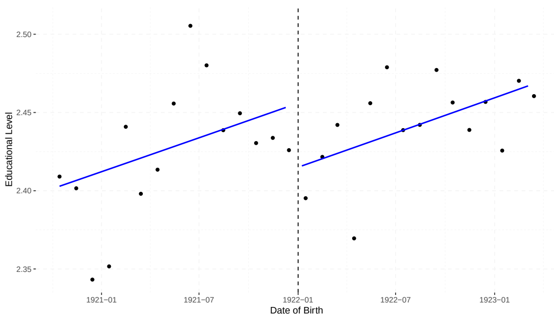


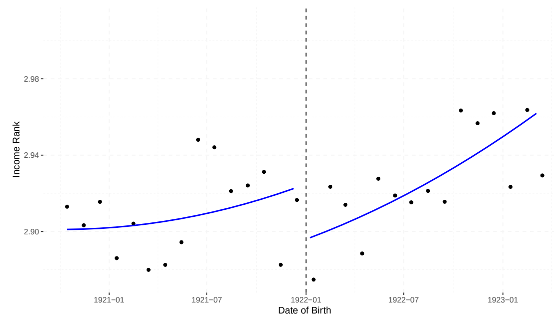
Figure 2: Number of male observations per month and year of birth based on the 1971 Census

Notes. This figure shows the number of male individuals for each month and year of birth in the 1971 Census in a 15 month bandwidth around the cutoff of treatment, January 1, 1922.



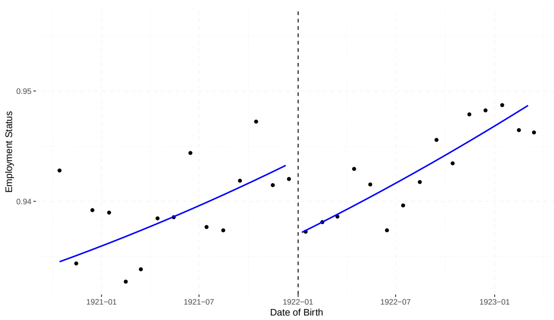
(a) Educational Attainment

Estimate: β_1 = -0.041 (0.0163)**



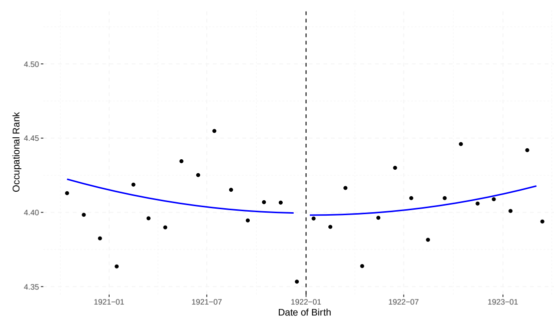
(b) Income Class

Estimate: β_1 = -0.0289 (0.0126)**



(c) Employment Status

Estimate: β_1 = -0.0068 (0.0021)***



(d) Occupational Rank

Estimate: β_1 = -0.0013 (0.018)

Figure 3: RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

Notes. The figures show the average of labor market outcomes based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the underlying individual-level data. Panel a shows the highest educational attainment measured from 0 to 8, panel b shows the income class measured from 0 to 5, panel c shows the employment status taking a value of zero or one, and panel d shows the occupational rank measured from 0 to 8. The y-axis is normalized to 10% of a standard deviation for each respective outcome.

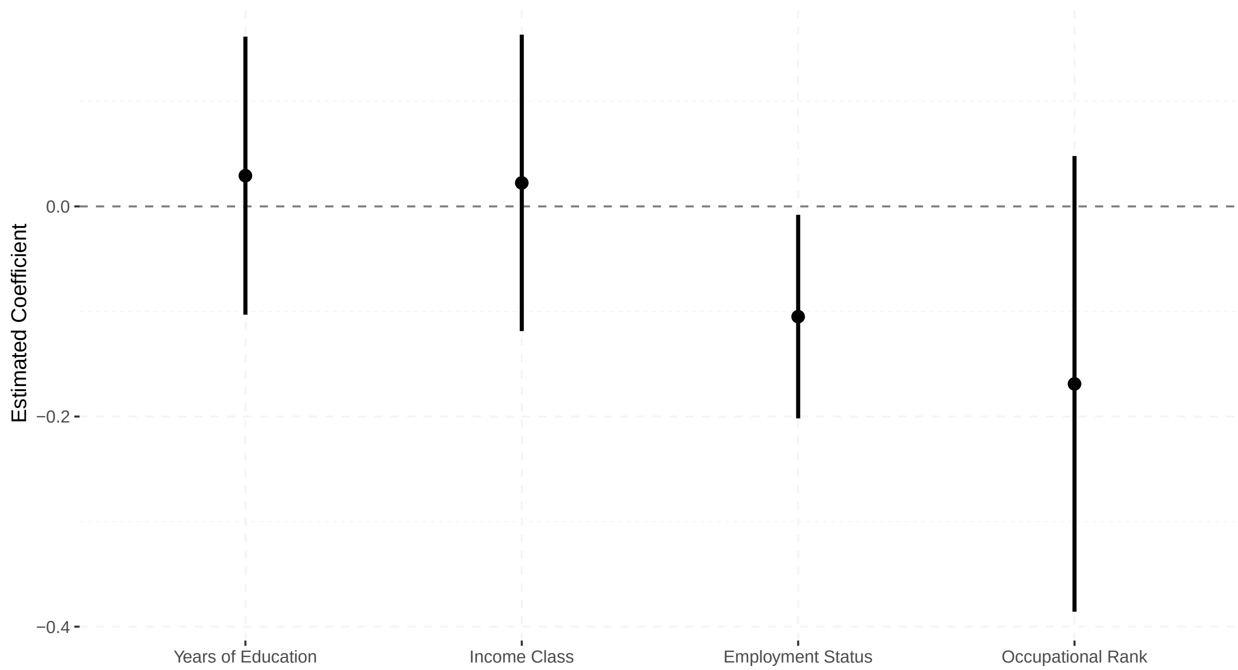


Figure 4: Effects of Forced Labor Conscription on Labor Market Outcomes using 1975-1944 Eurobarometer

Notes. This figure shows the estimated coefficients of a simple differences estimation using Eurobarometer data from 1975 to 1994. Years of education is measured from 7 to 15, income class is measured from 1 to 12, employment status takes a value of zero or one, and occupational rank is measured from 1 to 7. The 95% confidence intervals and the estimates are standardized by the standard deviation of the respective dependent variable.

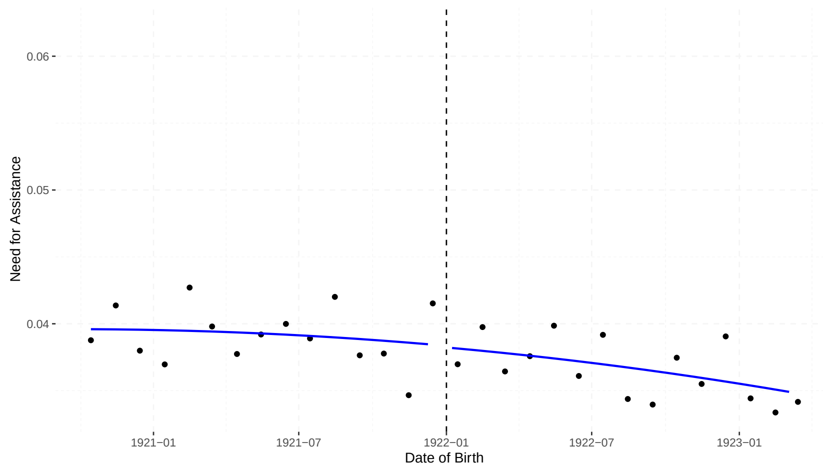


Figure 5: RDD Effects of Forced Labor Conscription on Need for Assistance based on 1971 Census

Estimate: β_1 = -0.0001 (0.0017).
Notes. The figures show the average of a dummy variable for the need for assistance in daily life based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the underlying individual-level data. The y-axis is normalized to 10% of a standard deviation of the need for assistance.

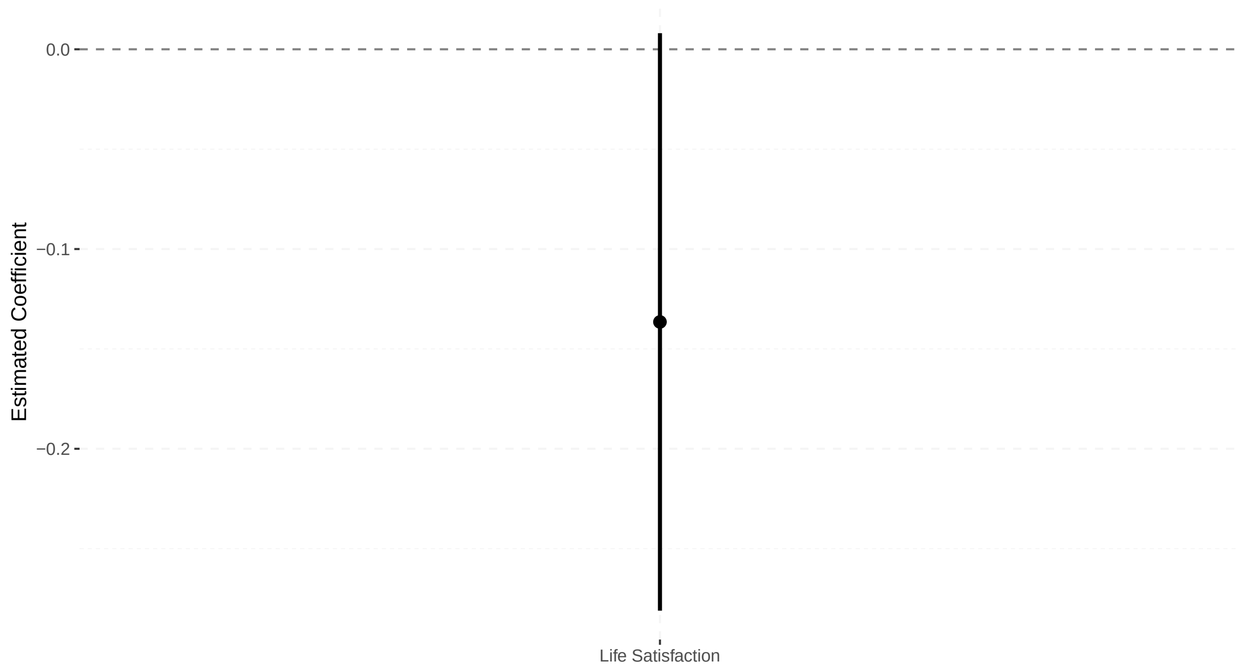
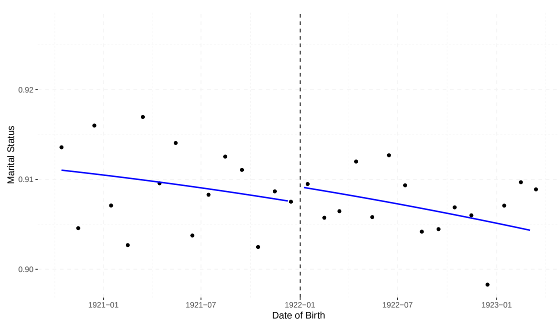


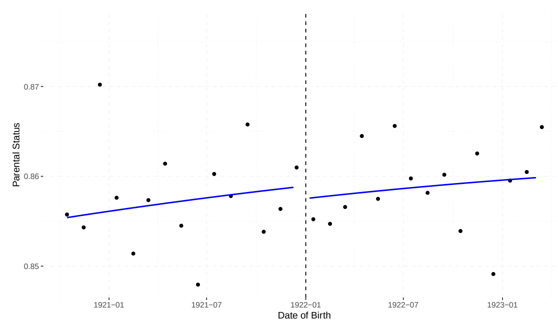
Figure 6: Effects of Forced Labor Conscription on Life Satisfaction using 1975-1944 Eurobarometer

Notes. This figure shows the estimated coefficient of a simple differences estimation using Eurobarometer data from 1975 to 1994. Life satisfaction is measured from 0 to 3. The 95% confidence intervals and the estimate are standardized by the standard deviation of the respective dependent variable.



(a) Marital Status

Estimate: β_1 = 0.0018 (0.0026)



(b) Parental Status

Estimate: β_1 = -0.0014 (0.0031)

Figure 7: RDD Effects of Forced Labor Conscription on Family Formation based on 1971 Census

Notes. The figures show the average family formation outcomes based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the underlying individual-level data. Panel a shows a dummy which takes the value of one if married, and zero otherwise. Panel b shows a dummy which takes the value of one if an individual has a child, and zero otherwise. The y-axis is normalized to 10% of a standard deviation for each respective outcome.

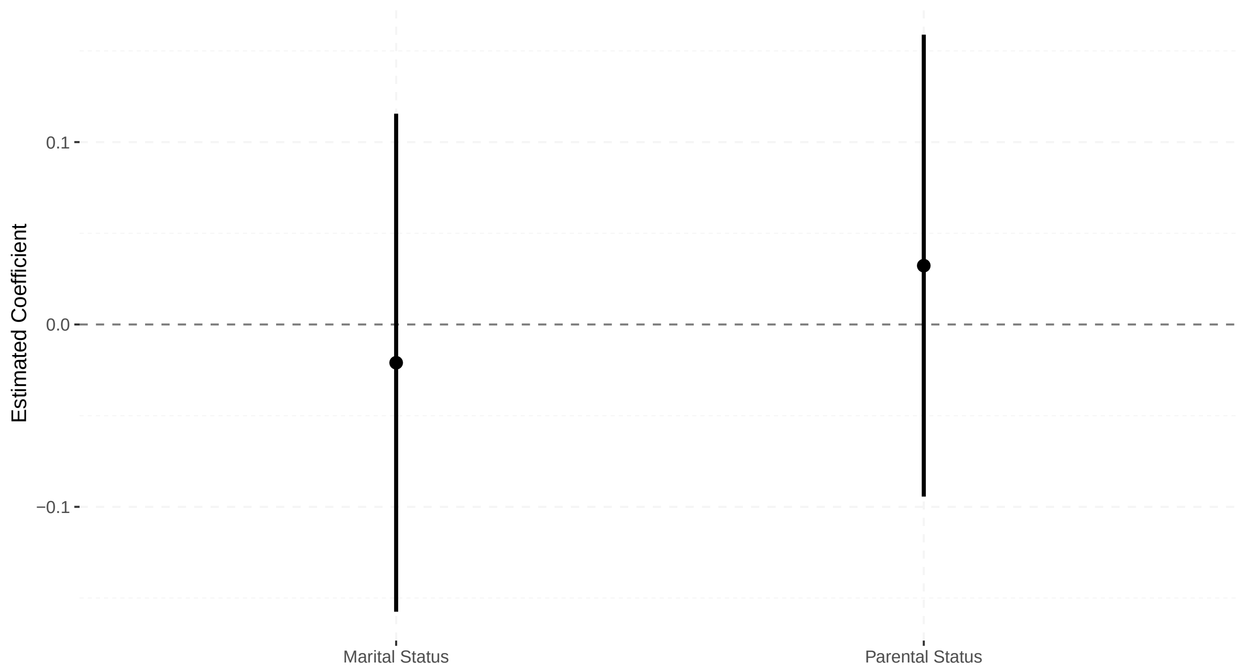
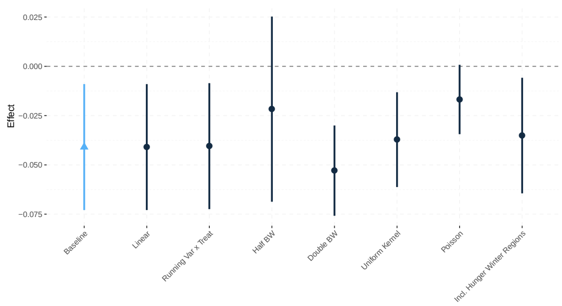
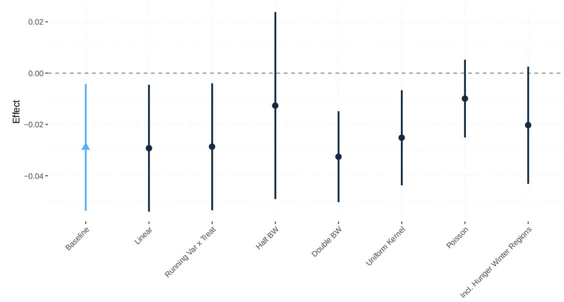


Figure 8: Effects of Forced Labor Conscription on Family Formation using 1975-1944 Eurobarometer

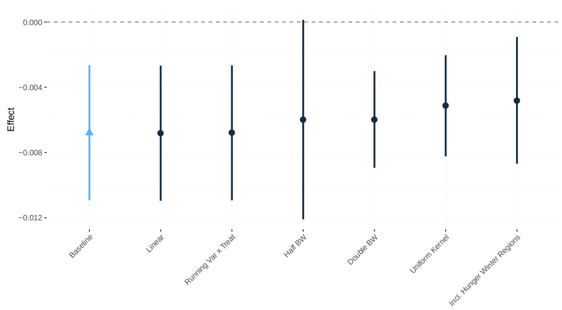
Notes. This figure shows the estimated coefficients of a simple differences estimation using Eurobarometer data from 1975 to 1994. Marital status is a dummy which takes the value of one if married, and zero otherwise, and parental status is a dummy which takes the value of one if an individual has a child, and zero otherwise. The 95% confidence intervals and the estimates are standardized by the standard deviation of the respective dependent variable.



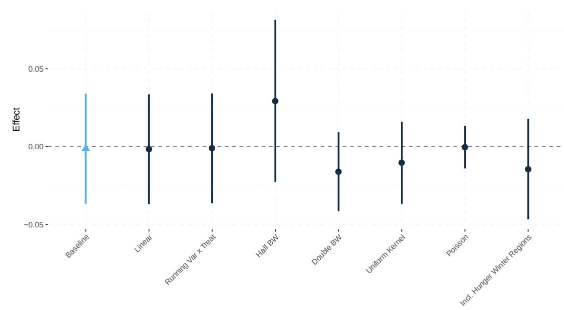
(a) Educational Level



(b) Income Class



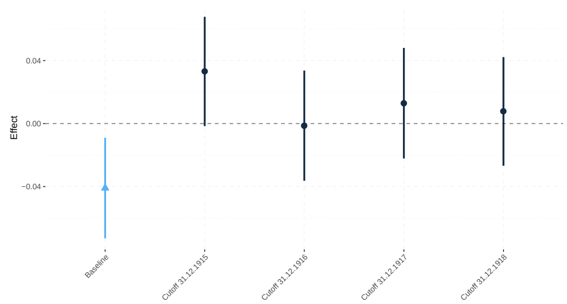
(c) Employment Status



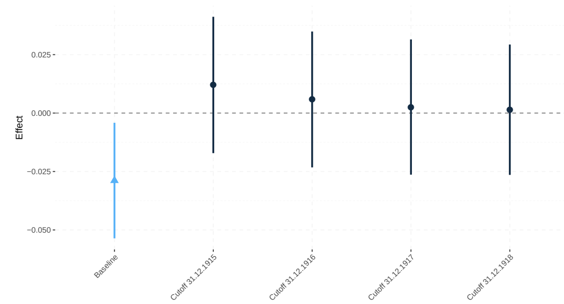
(d) Occupational Rank

Figure 9: Robustness of RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

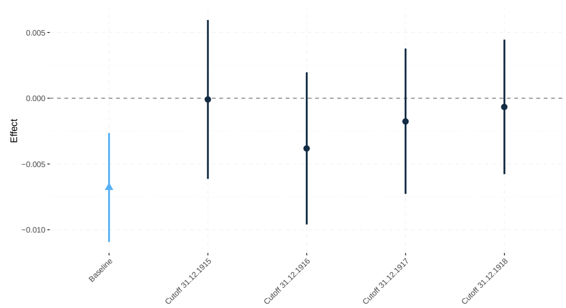
Notes. This figure shows RDD regressions using the 1971 Census with different specifications. The bars show the 95% confidence interval.



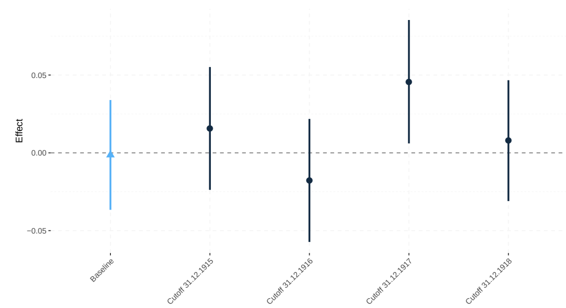
(a) Educational Level



(b) Income Class



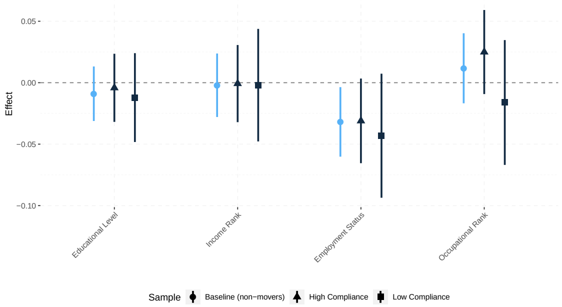
(c) Employment Status



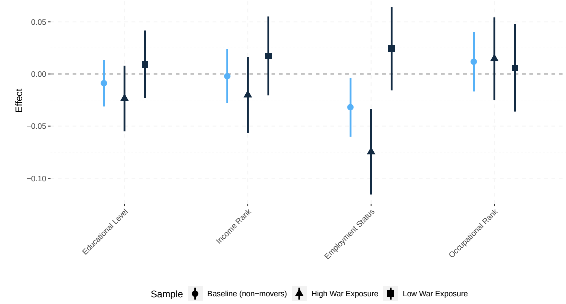
(d) Occupational Rank

Figure 10: Placebo RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

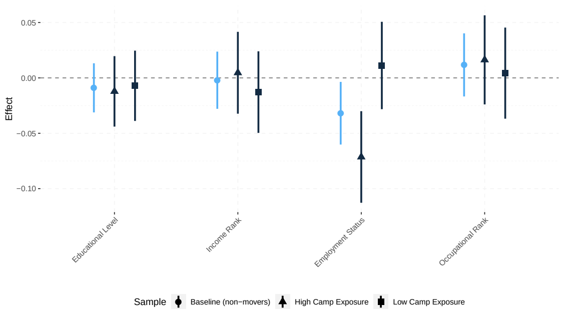
Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the 1971 Census with different placebo cutoffs. The bars show the 95% confidence interval.



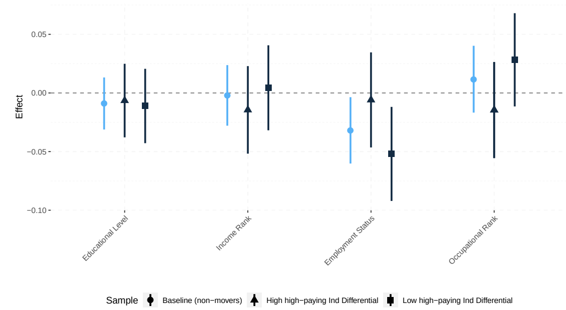
(a) Heterogeneity by Compliance



(b) Heterogeneity by War Exposure in DE



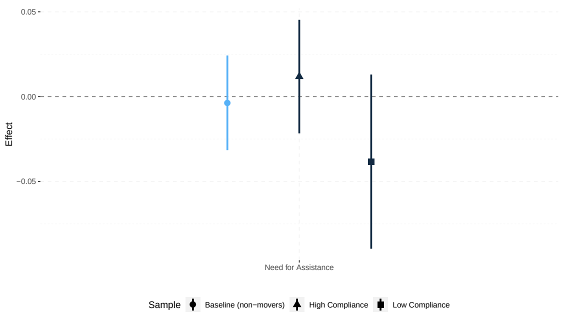
(c) Heterogeneity by Camp Exposure in DE



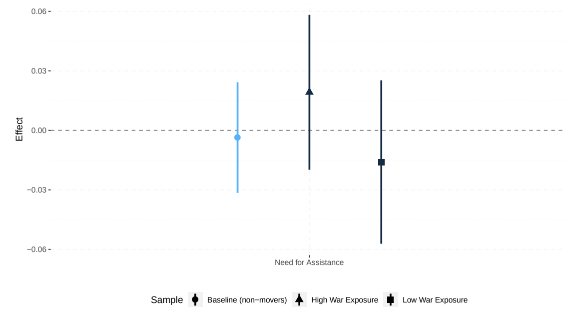
(d) Heterogeneity by Difference in Industry Structure

Figure 11: Heterogeneous RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

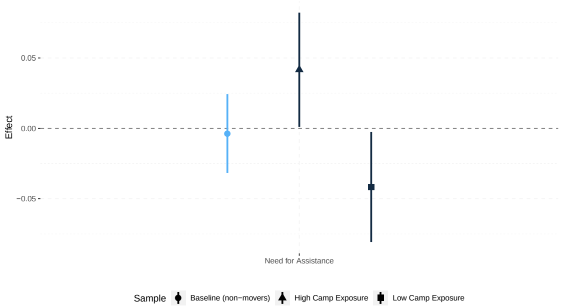
Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for economic outcomes using the 1971 Census with subsamples. In panel a, the sample is split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. In panel b, the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. In panel b, the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. In panel c, the sample is split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to. The bars show the 95% confidence interval. The coefficients and confidence intervals are by the standard deviation of the respective dependent variable.



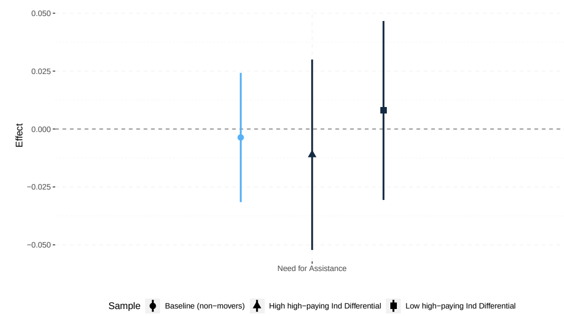
(a) Heterogeneity by Compliance



(b) Heterogeneity by War Exposure in DE



(c) Heterogeneity by Camp Exposure in DE



(d) Heterogeneity by Difference in Industry Structure

Figure 12: Heterogeneous RDD Effects of Forced Labor Conscription on Need for Assistance based on 1971 Census

Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for the need of assistance in daily life using the 1971 Census with subsamples. In panel a, the sample is split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. In panel b, the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. In panel b, the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. In panel c, the sample is split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to. The bars show the 95% confidence interval.

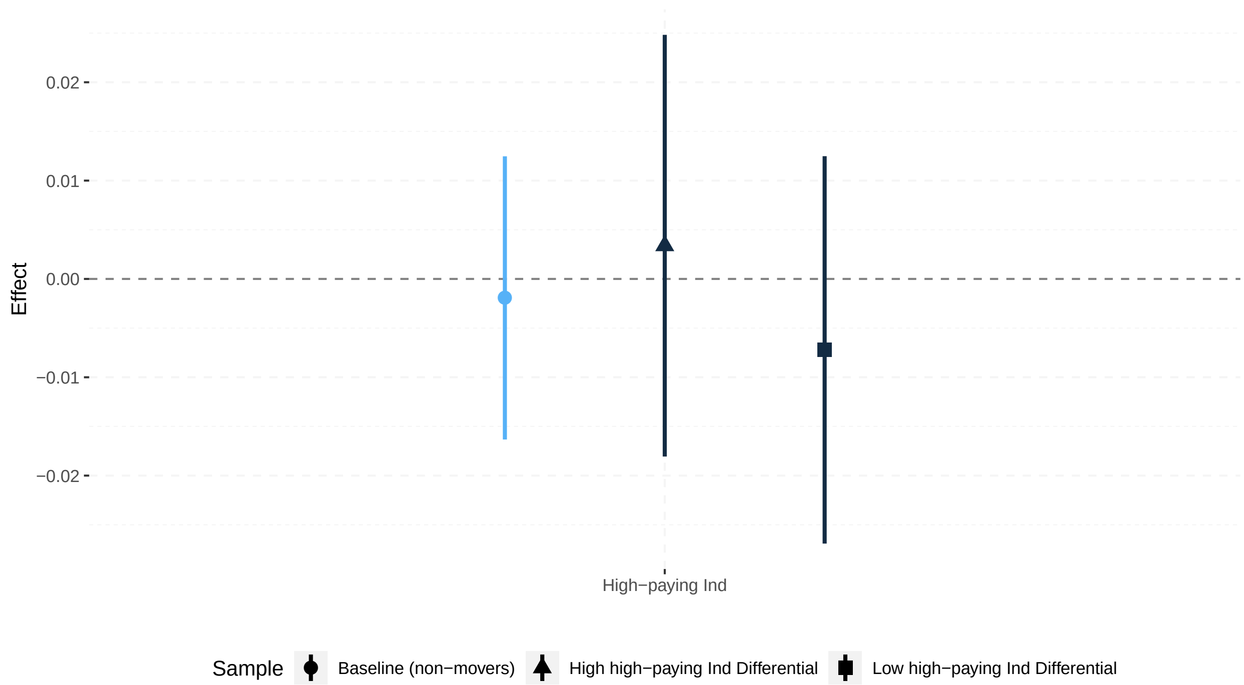


Figure 13: Heterogeneous RDD Effects of Forced Labor Conscription on Probability to work in High-paying Industry based on 1971 Census

Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for the probability to be employed in above-median paying sectors using the 1971 Census with different subsamples. The sample is split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to.

Tables

Variable	Levels	Mean Overall	Std. Dev. Overall	Mean Treatment	Mean Control	Nonresponse Rate (Treatment)	Nonresponse Rate (Control)
Educational Level	0-8	2.434	1.662	2.442	2.425	0.169	0.176
Income Rank	0-5	2.918	1.362	2.926	2.909	0.067	0.066
Employment Status	0-1	0.941	0.235	0.943	0.939	0	0
Occupational Rank	0-8	4.405	1.746	4.406	4.404	0	0
Marital Status	0-1	0.908	0.289	0.907	0.909	0	0
Parental Status	0-1	0.858	0.349	0.859	0.858	0	0
Need for Assistance	0-1	0.038	0.191	0.037	0.039	0	0
Observations		151080	151080	76918	74162	76918	74162

Table 1: Descriptive Statistics

Table 2: Labor Market Effects

	Dependent variable:			
	Educational Level	Income Rank	Employment Status	Occupational Rank
	(1)	(2)	(3)	(4)
RDD Estimate	-0.041^{**}	-0.029^{**}	-0.007^{***}	-0.001
	(0.016)	(0.013)	(0.002)	(0.018)
Observations	124490	141109	151080	151080
Bandwidth	15 months	15 months	15 months	15 months
Dependent Variable Range	0-8	0-5	0-1	0-8
Mean Dependent Variable	2.434	2.918	0.941	4.405
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 3: Social Effects

	Dependent variable:	
	Marital Status	Parental Status
	(1)	(2)
RDD Estimate	0.002	-0.001
	(0.003)	(0.003)
Observations	151080	151080
Bandwidth	15 months	15 months
Dependent Variable Range	0-1	0-1
Mean Dependent Variable	0.908	0.858
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 4: Health Effects

	Dependent variable:
	Need for Assistance
RDD Estimate	−0.0001
	(0.002)
Observations	151080
Bandwidth	15 months
Dependent Variable Range	0-1
Mean Dependent Variable	0.038
Note:	*p<0.1; **p<0.05; ***p<0.01

Variable	Levels	Mean Overall	Std. Dev. Overall	Mean Treatment	Mean Control
Years of Education	7-15	10.01	3.14	10.01	10.01
Income Class	1-12	7.13	3.23	7.23	7.04
Employment Status	0-1	0.38	0.49	0.37	0.40
Occupational Rank	1-7	4.17	1.94	4.02	4.31
Marital Status	0-1	0.84	0.37	0.84	0.84
Parental Status	0-1	0.10	0.31	0.12	0.09
Life Satisfaction	0-3	2.23	0.68	2.19	2.27

Table 5: Descriptive Statistics

Table 6: Labor Market Effects – Eurobarometer

	Dependent variable:			
	Years of Education	Income	Employment Probability	Occupation Type
	(1)	(2)	(3)	(4)
treatmentGroup	0.092	0.072	−0.051*	−0.327
	(0.252)	(0.277)	(0.029)	(0.254)
Wave FE	YES	YES	YES	YES
Dependent Variable Range	14-22	1-12	0-1	
Mean Dependent Variable	16.46	6.32	0.21	
Observations	615	505	620	237
R^2	0.084	0.187	0.482	0.146
Adjusted R^2	0.006	0.108	0.438	−0.023
Note:	*p<0.1; **p<0.05; ***p<0.01 Standard Errors are clustered at wave level.			

Table 7: Labor Market Effects – Eurobarometer

	Dependent variable:		
	Marital Status	Children	Life Satisfaction
	(1)	(2)	(3)
treatmentGroup	−0.008	0.010	−0.093
	(0.031)	(0.024)	(0.060)
Wave FE	YES	YES	YES
Dependent Variable Range	0-1	0-1	
Mean Dependent Variable	0.73	0.08	
Observations	612	620	492
R^2	0.092	0.218	0.137
Adjusted R^2	0.014	0.152	0.063

Note: *p<0.1; **p<0.05; ***p<0.01
Standard Errors are clustered at wave level.

Appendix A.

5.1 Data

5.1.1 Removing duplicate entries in Arolsen Archive data

The Arolsen Archive on forced workers in Germany includes information on the full name, date of birth, location of birth, and the location where the person stayed in Germany. The original sample consists of 594,967 observations. Some individuals show up more than once in the data because multiple sources have been aggregated for the archival records.

I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another to be able to exclude double-counts of individuals. I follow the Abramitzky, Boustan, and Eriksson (ABE) Algorithm (Abramitzky et al., 2021), and adjust their method according to my data availability. The ABE method uses variables which are unlikely to change over time, namely a person's place of birth, name and age. To reduce computational requirements, only individuals with the same first letters of the first and last name, the same place of birth and an age difference of up to 5 years are compared (so-called blocking). Of the total XX observations of the archival data on forced workers, only XX have information on their place of birth. Because of this, I cannot reasonably block on the place of birth without not linking a majority of the observations. In contrast, I do know the exact date of birth for XX (XX percent) of the observations instead of only their self-reported age as in the census data for which the ABE-JW method was derived. This alleviates the issues connected to only knowing individuals' age, such as rounding of reported age and differences in age at different points in time of reporting. I therefore block on the date of birth and on the first letters of the first and last name instead[45]. Following ABE-JW, for each of the possible matches within a block, I then calculate the string distance of the first name, last name and place of birth where available using the Jaro-Winkler string distance and restrict links to individuals for whom all available JW distances are less than or equal to 0.1[46][47]. The ABE-JW method links two datasets where every individual only shows up once in each dataset, so a possible match is only linked if it is unique, and there are not multiple entries which are close to the original. In my case however, I am linking observations to other entries from the same dataset, and links to multiple entries are plausible because a person may show up more than twice in the archival records. I therefore do not restrict links to only those entries which have only one plausible match. I then treat all linked individuals as only one observation going

[45]This means that ADD NUMBER observations, for whom either the date of birth, the first name or the last name is missing, remain unlinked and are treated as unique individuals.

[46]The place of birth is reported for 30.5% of all entries.

[47]Following ABE-JW, I use a weight of 0.1, which puts more weight on the first character of a string.

forward. Of the originally 594,967 observations, my algorithm links 121,561 observations to another entry, leaving 473,406 observations of probably unique individuals.

Since the archival data does not include the gender of the individuals, I use the data on first names and combine this with information on name frequency based on gender from the Corpus of First Names in the Netherlands published by Meertens Instituut (nd). Of the 34,831 unique first names in my dataset, 11,802 (33.9%) are part of the Corpus of First Names. To include names with slightly different spellings, I calculate the JW-distance between first names and assign the same gender to a name with a sufficiently similar name that is part of the Corpus of First Names (a JW-string distance of up to 0.1, as suggested by Abramitzky et al. (2021)). This yields an addition of 17,417 names. In total, I can assign a gender probability to 29,219 or 83.9% of all unique first names. Based on this, I calculate the probability of a given name to be male or female and classify names for which at least 70% of individuals with that name are either male of female respectively. All other names are classified as uncertain. 78.9% of all unique first names can be classified as either male of female using the cutoff of 70%. Since some persons have multiple first names (either because they have a middle name, or because two observations with differently spelled names were linked to the same individual), I use the mode of each persons' first names' genders to assign their gender[48] In total, I can assign a gender to 93.7% of individuals in the archival records. For the relevant cohorts of 1922 and 1924, there are 10.7% female and 84.2% male individuals.

ADD description of linking birth places of Arolsen Archives to 1971 Census.

5.1.2 Employment Structure

ADD detailed description of employment structure and industry classification of German counties and of Dutch municipalities.

5.1.3 Eurobarometer

ADD detailed description of how I harmonized the Eurobarometer data.

5.2 Descriptives

[48]So if a person has two names, where one is classified as male and one is classified as uncertain, I assign this person a male gender. If a person has two male names and one female name, I assign a male gender as well. If a person has the same number of names being classified as male and female, I do not assign them a gender.

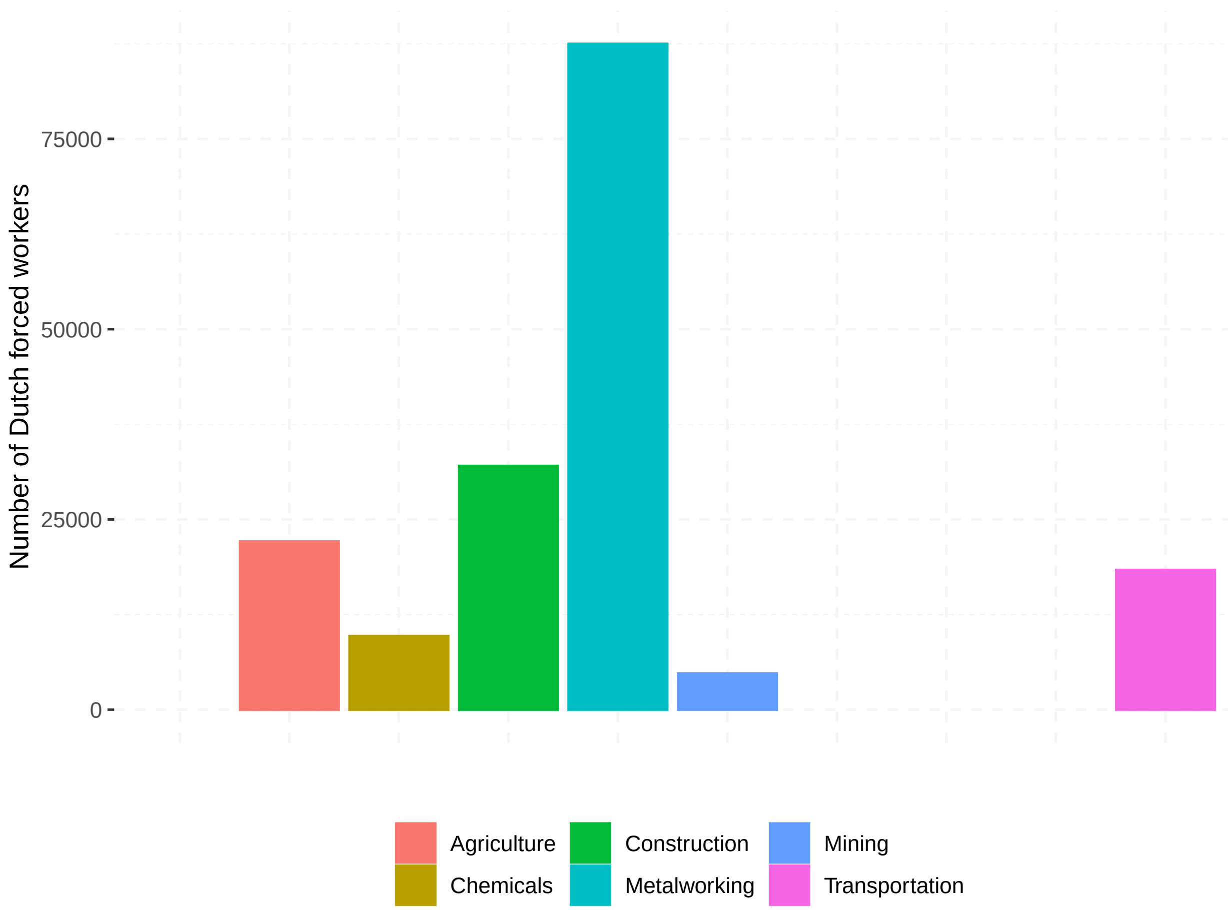


Figure 14: Allocation of Dutch forced workers across industries in Germany

Notes. The data is based on Herbert (1999) and shows the number of Dutch forced workers by industry in Germany.