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

Carola Stapper†

Last update: 2024-08-13

Abstract

Forced labor remains to be a large issue in today's economy. According to estimates by the International Labour Organization, almost 28 million people were working under some form of coercion in 2021. In this paper, I study the causal effects of facing labor coercion on long-term individual labor market outcomes. I exploit the setting of displaced Dutch civilians who were conscripted by Nazi Germany during WWII based on their birth year and compare individuals born just above the cut-off date to those just below the cut-off date. Using Dutch census data from 1971 and Eurobarometer survey data from 1975 to 1994, I estimate the effects on individuals' later educational attainment, skill level of occupation, and income. I find that there are negative effects on income and skill level of occupation for those individuals who had already started their career in the Netherlands prior to the coercion, while there are no negative effects for those who were drafted right after finishing high school.

Keywords: Economic History, Labor Economic History, Labor Economics, Employment, Particular Labour Markets, Coercive Labor Market, Forced Labor, Labour Economics: General, General - Health, Education, and Welfare

JEL Codes: N34, N44, J24, J47,

*Funded by the Reinhard Selten Institute, Universities of Bonn and Cologne. I gratefully acknowledge support and guidance from Erik Hornung and Anna Bindler throughout this projec. I thank XX for helpful comments and suggestions. This paper has benefited greatly from discussions with participants of seminars at Erasmus University Rotterdam, XX XX.

†University of Cologne, Center for Macroeconomic Research. stapper@wiso.uni-koeln.de

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, factors which contribute to vulnerability of being exposed to forced labor may be correlated with the outcomes we are interested in (ILO, 2022).

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, exploiting exogenous assignment into labor coercion. In particular, I study the case of Dutch civilian forced workers. Following the unexpected loss at the battle of Stalingrad in February 1943, there was a rising labor shortage in Germany resulting from drafting more men for military service and expanding the armaments industry. The German government thus intensified their efforts to fill 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. Exceptions were granted to men who were already working in industries that were strategic to the war effort. 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%. While in Germany, the majority (68%) of the Dutch civilian workers were employed in manufacturing and construction, and assignment into industry was done irrespective of previous skills (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,

I compare later educational attainment, skill level of occupation and income of individuals who were born within the years that were conscripted to that of individuals who were born outside the conscripted years. More formally, I employing a regression discontinuity design at the two cutoffs of 01.01.1922 and 31.12.1924 of the forced conscription policy. Since I cannot differentiate between individuals who were conscripted but avoided transportation to Germany and those who were actually coerced to work in Germany, I estimate an intention to treat effect.

Using Dutch census data from 1971, I find negative effects for the labor market success of the older group born in 1922 compared to those born in 1921 (aged 20–21), most notably in the skill level of their occupation, which is lower by two thirds of a SD. I find no differences in any of the labor market outcomes for the younger group born in 1924 compared to those born in 1925 (aged 18–19 years at conscription). To shed light on contributing factors, I study the effects on physical and psychological health. While I find no differences in the need for assistance in daily life based on 1971 census data for either cutoff, I do find lower life satisfaction for the conscripted cohort born in 1922 using Eurobarometer survey data covering the period from 1975 to 1994.

To further understand the differences in the effects for the younger and older treated individuals and the mechanisms at play, I examine heterogeneities based on the likelihood of compliance and the differences in experiences in Germany by linking archival records on forced workers from the Arolsen Archives to the Dutch census data based on place and date of birth. This allows me to study whether the sector in which the forced workers were employed affects their later occupational choices and whether the persistence was different based on the age at which individuals were allocated into these sectors in Germany.

When interpreting the results, it is important to note that I estimate a conservative lower bound of the effects of forced labor, because there are several factors in this setting that are stacked against me finding any effects: First, the Dutch forced workers were treated relatively better than forced workers of other nationalities, so the effects for civilians of other

occupied countries would probably be larger than what I find. This was also mirrored by German policy makers 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 a lack of discriminatory living conditions (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). So, in settings of forced labor with more stringent conditions, the effect would only be larger. Second, the forced labor affected adult men instead of possibly more vulnerable groups such as children, for whom the trauma of such an experience may be more severe. On the other hand, the disruptive effect of forced work on career trajectories may be larger for the young individuals entering the labor market in the studied setting, compared to older individuals. The forced labor also only lasted for a comparatively short period of time of two years, which may lead to less severe long-run effects. The control group of Dutch men not being subject to conscription to whom I compare the treated cohorts were also affected by living in an occupied country during WWII, which could lead to an underestimation of effect sizes. Additionally, I observe individuals at 15 years after the end of their forced labor experience, so it may be the case that some effects have already subsided or not yet realized. Thus, I am probably estimating a lower bound of what the true consequences of the forced labor conscription might have been.

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 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.

Additionally, I contribute to the literature on consequences of forced workers by analyzing a setting that is similar to the experience of many workers who are coerced into labor today, and 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 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 both endangering current workers subject to coercion as well as data quality. The exogenous conscription into forced labor by year of birth applied by the Germans constitutes a natural experiment which alleviates endogeneity concerns when identifying causal effects of forced labor experiences. These endogeneity concerns arise due to the fact that 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.

My paper also adds to previous literature on individual-level effects of forced combat and military conscription (Angrist et al., 2010; Blattman and Annan, 2010; Hjalmarsson and Lindquist, 2019) by focusing on the effects of being exposed to labor conscription outside of military service and combat, which is a different type of treatment.

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 consequences of facing adverse events such as hunger, war or natural disasters (Braun and Stuhler, 2023; Conti et al., 2021; 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 tracing individuals from their late 40s up to old age, studying labor market effects and differences on mortality in the very long run, and by focusing on the effects of a random assignment into specific industry sectors and how that affects later labor market trajectories.

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 recruitment of civilian workers started with a focus on the unemployed, who faced a cut of their unemployment payment if they refused to accept work in Germany, as well as "combing out" workers from individual factories (Sijes, 1966). This did not meet the increased labor demand in Germany, especially after the failure of the German Blitzkrieg strategy. This strategy relied on short and powerful military attacks, but losses at several fronts including the of Stalingrad in 1943 led to German men having to fight at the front for longer periods than previously anticipated and the armaments production needing to be expanded even further. Therefore the German government intensified their efforts of coercing civilians into forced labor in Germany (Herbert, 1999; Spoerer, 2001).

The German *Hauptabteilung Soziale Verwaltung* (Office of Social Administration, HSV henceforth), which was responsible for recruiting Dutch workers, therefore announced in May 1943 that they would conscript all men of specific age groups for work in Germany (the so-called *Yearclass Action*). This was initially planned to include all men born between 1908 and 1925. 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. 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. For the men born between 1922 and 1924, coercion was executed by withholding food ration cards and forbidding firms

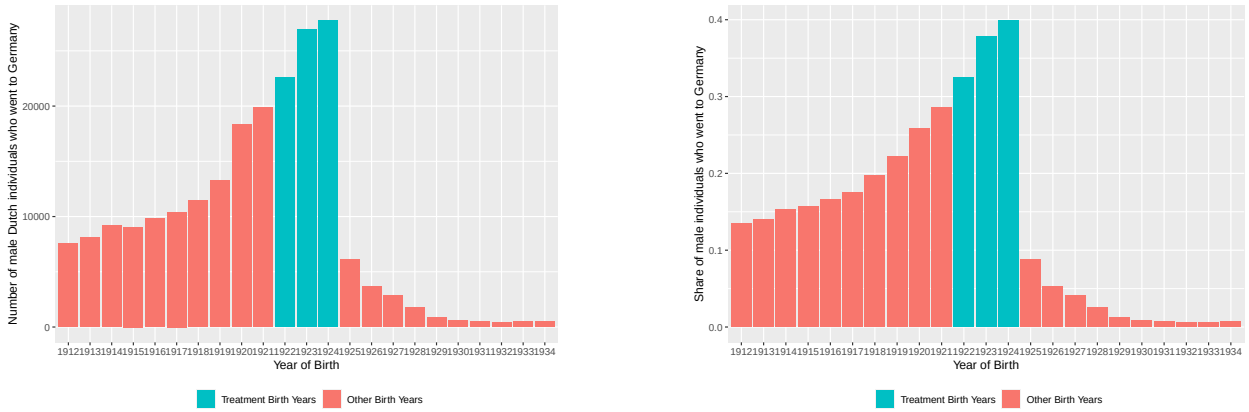
to employ men from these cohorts. Men already working in war-related industries were granted exemptions, and others managed to go into hiding with the help of the resistance, which forged food ration cards and helped with the placement of men of the conscripted age groups into hiding locations using false documents (Sijes, 1966).

Figure 1 shows the absolute number and the percentage of each cohort of male Dutch forced workers present in Germany. These numbers suggest that in total, around 77.000 Dutch forced workers born between 1922 and 1924 were present in Germany, and compliance in these cohorts was between 32.4 and 39.9%, larger than for any other cohorts not conscripted based on their year of birth[1]. The individuals not born in these years who were still present as foreign workers in Germany were recruited through different measures such as the recruitment of unemployed at the beginning of the war or raids. While these raids were mostly targeted at conscripted men who went into hiding, sometimes men were rounded up indiscriminately (Sijes, 1966). I thus have non-compliers in both the treatment and control group: In the control cohorts of 1921 and 1925 (right before and after the conscription window of 1922–1924), 28.5% and 8.8% respectively were forced workers in Germany. From the individuals of the conscripted cohorts who did not go to Germany, around 16% were granted an exemption (Sijes, 1966). The remaining men went into hiding.

The Dutch forced workers who were not granted exceptions and who did not manage to go into hiding were transported to Germany with trains and were distributed over all of Germany. Figure **??** shows the regional distribution of Dutch male forced workers born in the conscripted years 1922–1924 over the German counties. One can see that while there are certain clusters of forced workers around industrial and urban centers, the Dutch men were located over all of Germany and not only close to the Dutch border[2]. The distribution of forced workers was done by the Ministry of Labor, based on the labor shortages that

[1]Note that the numbers of forced workers in Germany are mostly based on forced workers who were still present at the end of the war, so there is bound to be some underestimation. On the other hand, the data includes some refugees and prisoners of war, leading to overestimation. The percentage is based on men still alive in 1971 as per the census data. See 3 for a more detailed discussion.

[2]Figure XX in Appendix shows the map separately for each cohort, and for all Dutch men.



(a) Absolute number of forced workers

(b) Compliance for each cohort

Figure 1: Forced workers across cohorts

Notes. Panel a depicts XXX. The data is based on registration of displaced persons, of whom the majority were forced workers XXXEXPLAIN. This percentage is based on men still alive in 1971 as per the census data.

companies reported to their local *Arbeitsamt* (employment office) Marx, 2019). This was done irrespective of the workers' skills or previous training because the administrative effort of that would have been too costly (Kuck, 2010; Marx, 2019). This means that the men were assigned randomly into the specific industries, based on local demands at the time of their deportation. The majority of the Dutch forced workers were employed in unskilled positions in manufacturing and construction[3], and the pay was lower than that for German workers (Herbert, 1999; Sijes, 1966; Kuck, 2010; Tooze, 2006)[4]. The living conditions varied widely, as firms were responsible for housing and feeding the forced workers (Althausen, 1999). Most Dutch workers were housed in camps formed of large barracks or repurposed schools and theatres. 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 such as sabotage or absenteeism, forced workers were sentenced to temporary stays in *Arbeitserziehungslager* (labor education camps) of several weeks, where conditions were similar to those in concentration

[3]Figure **??** in the appendix shows the number of forced workers in different industries.

[4]In theory, the pay should have been the same as that for German workers, but in reality a lot of firms did not pay the workers their wages (Pfahlmann, 1968).

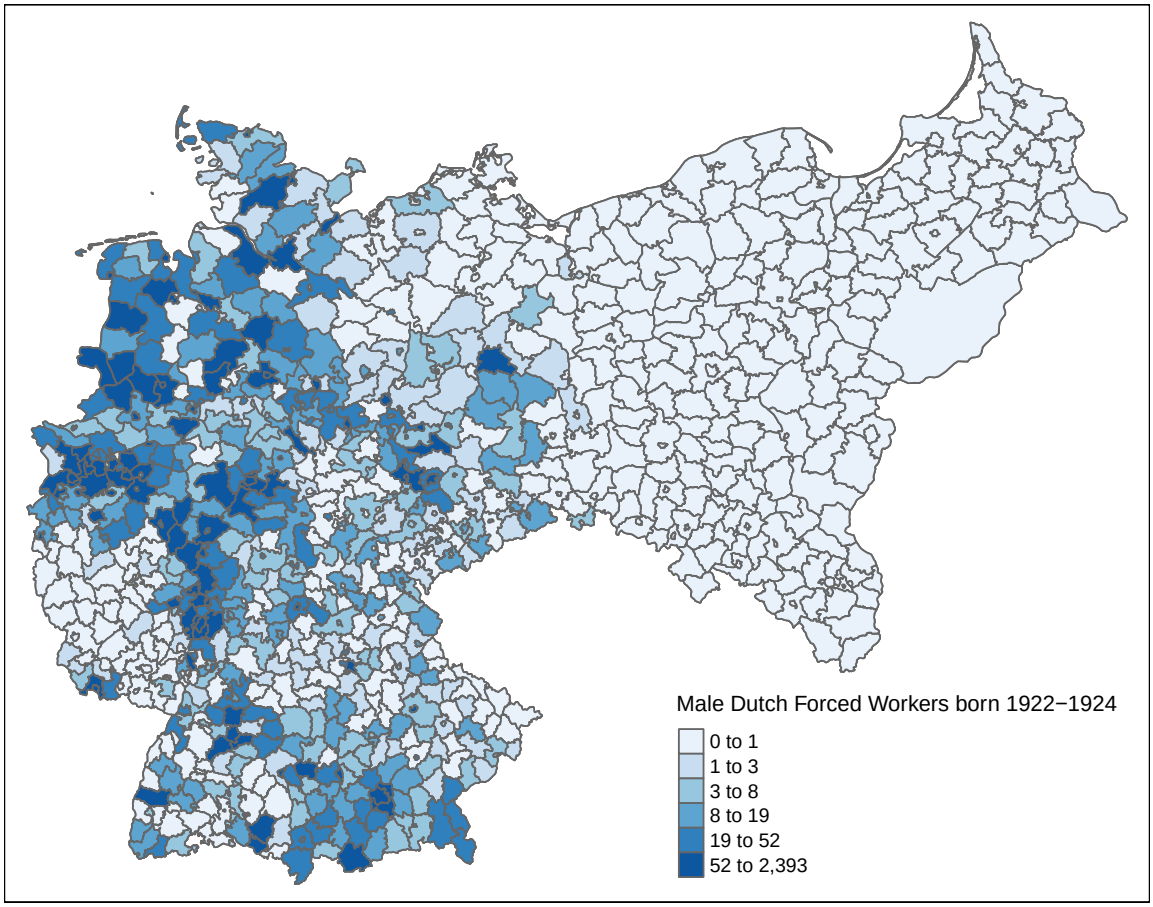


Figure 2: Distribution of forced workers across Germany

Notes. The data is based on registration of displaced persons, of whom the majority were forced workers XXXEXPLAIN.

camps (Lofti, 2000). While the forced workers were mostly promised yearly contracts at deportation (Beening, 2003), the majority of workers were not allowed to leave after that period and had to stay until the end of the war. 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 vary, but most sources put the number between 5,000 and 29,000. Based on the total estimated number of Dutch forced workers of somewhere between 400,000-600,000, at least 93%, maybe up to 99%, survived the war (Warmbrunn, 1972; Beening, 2003). After the successive liberation of Germany in

1945, the Allied Forces started the registration of the so-called displaced persons in order to organize the transport back to their home countries. The majority of the Dutch forced workers were able to return to the Netherlands. 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)).

The control group to which I compare the experiences of the individuals subject to the forced labor system was also not unaffected by the historical circumstances of living through WWII, 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)). Additionally, forced workers also experienced the war while being stationed in Germany, so they share comparable experiences in this regard. Nevertheless, I construct a subsample based on individuals from regions with lowest war exposure based on the number of civilian deaths from war-related causes[5] Secondary education typically started at nineteen years old, so at least the older cohorts should have completed all schooling but university already (Warmbrunn, 1972). In 1940, only 4% percent attended the higher education system (Van Eden, 1946), which had mostly stopped operating from 1943 onwards (Warmbrunn, 1972), so neither the control nor the treatment group would have gotten significant higher education during the period of conscription in 1943 until the end of the war in 1945. There was also no systematic large-scale enlisting of men into the *Wehrmacht* that differed for age-groups[6] One possible 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

[5]See section XX for a detailed description of the construction of this subsample.

[6]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)

this period, around 20,000 people died. The groups that suffered the most were infants and individuals over 55 years old, so the control group in my setting will probably not have been affected severely[7] (de Zwarte, 2020). Since only the Western regions of the Netherlands experienced the hunger winter, I still perform a robustness check excluding individuals from areas affected by the hunger winter to abstract from any possible differences driven by the hunger experience[8].

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. The compensation was paid half by the Gereman companies involved in forced labor and half by the German government. Depending on the severity of the treatment, individuals were paid between 572 and 7,760 € [XXX USD]. Individuals who were deported and had to work in industry were paid 2,560 € 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)). ()

[7]In their recent paper, Ramirez and Haas (2022) find negative effects of the hunger winter on education also for adolescents of up to 14 years old, so born in in 1930 the earliest, which is after the birth date of both my control and treatment group.

[8]See section 5.1.2 for a detailed discussion of the construction of this subsample.

3 Data

3.1 Dutch Census Data

3.1.1 1971 Census

To measure the effects of being conscripted into this forced labor system, I use individual-level admin data supplied by Statistics Netherlands (). Specifically, I use the 1971 census (*14de Algemene Volkstelling*) which is a comprehensive census of the population[9].. To identify treatment and control group, I use the individuals' gender and month and year of birth. The potential treatment group is defined as all men born from January 1922 to December 1924. The potential control group are individuals born outside of the conscription cohort. To ensure that treatment and control group are comparable in terms of their labor market outcomes due to their closeness in age, I restrict the analysis to individuals born in 1921 and 1922 for the older cohorts and to those born in 1924 and 1925 for the younger cohorts[10]. The individuals are thus aged between 45 and 50 years old at the time of the census.

The labor market outcomes reported in the census are the highest degree (reported in 9 different levels), income (reported in 6 different income classes with a range of 4,000 Dutch Guilder or 1,970 USD) and social group, which is a measure that combines information on income class, level of education, occupation type and position in the company and takes on 36 different levels. While the census covers almost all Dutch individuals, the non-response rates for these variables of interest are somewhat higher: For the highest degree, the non-response rate was 19.1%, and for income it was 10.3%, where people from lower socioeconomic classes are more likely to be non-responders (**?**).. The missing data due to non-response may introduce a bias to the effects of the conscription into forced labor, because the treatment and control group likely differ in their socioeconomic class and thus also in their likelihood to be non-responders. The non-response rate for the outcomes of interest is XXhigher?lower?

[9]The non-response rate was 0.2%

[10]I perform robustness checks using different bandwidths, see appendix XX

for the treated group, thus I systematically observe fewer low-income or low-education men in the treatment group than in the control group. I therefore probably underestimate the effect of forced labor on education or income and my findings should be interpreted as a lower bound of the true effect.. Table XX shows the descriptive statistics separately for each of the relevant cohorts. Apart from the labor market outcomes, I also look at further secondary outcomes taken from the census data. These are a persons' marital status and number of children as a proxy for their social situation, the value of their self-owned house if applicable and the number of telephone lines as a proxy for their wealth (following Marie and Zwiers (2023)), and a person's need for assistance in everyday life as a proxy for their health.

The census data reports the municipality of birth only for individuals who still live in the same municipality as the one they were born in. When merging further data sources based on the place of birth, I thus have to restrict my analysis to these individuals. This reduces my sample to XX observations. Discuss Bias introduction (Balancedness table?)

3.1.2 1960 Census

XX

3.2 Eurobarometer

To shed further light on the effects of experiencing conscription into forced labor on other secondary outcomes apart from an individuals' labor market success, I also use Eurobarometer survey data. 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 and includes questions on individuals' health, subjective well-being and political views. 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. This amounts

to 50 waves in total[11]. Since I only know an individuals' age and not their exact date of birth, there is a subset of individuals for whom it is unclear whether they were born in the potential treatment years (1922-1924) or whether they are part of the potential control birth cohorts[12]. I will exclude these individuals in my analysis[13]. Not all Eurobarometer waves include the same questions of interest, and the way in which the answers to the questions are coded varies by Eurobarometer wave. I therefore created harmonized variables which make the answers comparable over the different years[14]. I also employ wave fixed effects to control for these differences. Table XX shows the descriptive statistics of the questions of interest separately for control and treatment group. Life satisfaction is the answer to the question how happy a person is overall[15], and I harmonized the answers to a scale from zero to three. The question for left-leaning voting is harmonized from a question on party affiliation to a scale from zero to two, where zero is right-leaning and 2 is left-leaning voting[16].

3.3 Individual Archival Records

In order to quantify how many individuals actually complied with the conscription of forced labor, and to do a heterogeneity analysis based on the type of industry they worked in while in Germany, I use archival data on Dutch persons present in Germany during WWII supplied 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. 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

[11]This includes waves 3 through 42.

[12]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 3rd Eurobarometer survey of June 1975. They were thus born between June 1921 and June 1922. It is therefore impossible to determine whether this person is part of the treatment cohort (born 1922) or the control cohort (born 1921).

[13]In a robustness check, I will include these individuals in either the control or the treatment group.

[14]See section 5.1.4 for a detailed description of this harmonization process

[15]The exact wording of the question 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?"

[16]The wording of the question on party affiliation asks: "If there were a general election tomorrow, which party would you support?"

prisoners of war and former inmates of concentration camps, the majority of them are forced workers[17]. The number of unique Dutch individuals in the archival data of around 473,000 also closely matches the historical estimates of forced workers of somewhere between 450,000 and 530,000 (Spoerer, 2001; CBS, 1947). I will therefore assume that all individuals in this dataset are forced workers. Most 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.

The data includes information on the name, date and location of birth and the location where the person stayed in Germany. Some individuals may show up multiple times in the data because multiple sources have been aggregated for the data. I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another and not double-count individuals. I follow the Abramitzky, boustan, and Eriksson (ABE) Algorithm (Abramitzky et al., 2021), and adjust their method according to my data availability[18]. To reduce computational requirements, I only compare individuals born on the same date of birth and who share the same first letter of their first and last name. Using Jaro-Winkler (JW) string distance, I calculate the string distance of the first name, last name and, where available, the place of birth[19]. Following Abramitzky et al. (2021), I restrict links to individuals for whom all available string distances are less than or equal to 0.1. Of the originally 594,967 observations, my algorithm links 121,561 observations to another entry, leaving 473,406 observations of probably unique individuals.[20]

The archival data does not include any information on the gender of the individuals. Since only male civilians were forced to work in Germany based on their date of birth, I want to be able to restrict my analysis to male individuals from the conscripted cohorts

[17]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)

[18]See section [link appendix] for a comparison between their proposed linking method and my approach

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

[20]Allowing for string distances of up to 0.12 (which translates to partial agreement according to [cite Winkler 2000]), would increase the number of links to 128,332. However, the resulting links are less reliable upon inspection, which is why I stick to the dataset using 0.1 as the cutoff for linking two entries together.

when inspecting compliance and type of industry exposure in Germany. For this, I use the first names available in the archival records and combine them with information from the Corpus of First Names in the Netherlands published by the Meertens Institut [XX cite]. The data comes from civil registration data from 2017 and 19th cenury marriage certificates and indicates how often a given name is used for female and male persons[21][22]. 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[23]. 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[24] 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[25]. In the following analysis, I restrict the archival data to male individuals.

To investigate heterogeneous effects based on compliance and experience in Germany, especially the exposure of forced wokers to different industries, I link the archival records to the census data by aggregating the archival data at the level at which I have identifying information in the census data: Place of birth, year and month of birth as well as the

[21]Since the data is not readily available for download, but published on the web with a consistent URL pattern, I used webscraping to access the amount of times that a name is given to female and to male individuals.

[22]Of the 34,831 unique first names in my dataset, 11,802 (so 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, in accordance to what is done in the ABE method). This allows me to add a gender probability for additional 17,417 names. In total, I can assign a gender probability to 29,219 or 83.9% of all unique first names

[23]78.9% of all unique first names can be classified as either male of female using the cutoff of 70%.

[24]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.

[25]Figure 10 shows the gender composition over the different cohorts. In total, the dataset includes 75.7% male, and 18.2% percent are female individuals

gender[26]. These variables are available for 39.8% of the relevant cohorts of 1922 and 1924[27]. For my analysis, I thus have to assume that both the share of individuals complying as well as the locations in Germany where the forced workers were staying is the same for those individuals for whom I have these identifying variables, compared to those for whom at least on of these variables is missing. Since I cannot directly test this assumption, I instead report a balance test in table XX of available characteristics for individuals with and without certain information. One can see that the two groups with and without either of these variables are similar in terms of their other characteristics.

3.4 Industry Structure

Germany To measure in which industry forced workers were employed, I use the industry structure in German counties which the forced workers faced while working there. Specifically, I use data from the 1925 occupational census provided by **?** which covers all German counties, I use it to calculate employment shares for agriculture, industry and manufacturing, trade and transportation, administration, the medical sector, and domestic services[28]. The underlying assumption is that the industry structure of 1925 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. For each month-year and municipality of birth cell, I then create a measure of in which industries these individuals were most likely to be employed by calculating a weighted mean of the industry share in the destination counties for all counties in which individuals from this cell were present. This is done in the following way:

I then merge the collapsed archival data to the census data of 1971 collapsed at the same level of place of birth, month and year of birth. Because of spelling inconsistencies in the

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

[27] Figure 11 shows the availability of a place of birth for the different cohorts.

[28] I only use the industry shares of male workers as it is more likely to match the employment that the (mostly) male forced workers faced in their destinations

place of birth in the archival data, I employ a fuzzy merge based on the JW-string distance between the birth place recorded in the archival data and the municipality in the census data[29].

Netherlands To investigate whether the effects on labor market success and occupational choice differ depending on how different the sectoral 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 XX to measure the industry 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 sectoral composition in their municipality of birth.

4 Empirical Analysis and Results

4.1 Labor Market Outcomes

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 concerns, as I can exploit the exogenous assignment into forced labor based on an individuals' year of birth. While all years of the cohorts of 1908–1925 were potential draftees for working in Germany, only the cohorts of 1922–1924 were actually drafted. Thus, the other birth

[29] Explain in more detail and how many places I end up being able to merge!

years 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 will hence employ a fuzzy Regression Discontinuity Design (RDD) and compare individuals born just within the conscription period to those born just outside of the conscription period. By doing so, I ensure that the age difference between treatment and control group is minimal and therefore negligible in its' effect on labor market outcomes.[30] Since the conscription policy yields two cutoffs, one on either side of the birth dates of the conscribed cohort (01.01.1922 and 31.12.1924), I estimate two separate treatment effects. At the cutoff at the beginning of 1922, the control group is older than the treatment group, while at the cutoff at the end of 1924, the control group is younger than the treatment group. This should alleviate concerns that any differences are driven only by differences in age as the effects should operate in opposite directions in the two settings.

The central identifying assumption for RDD to estimate a causal effect of the treatment is that nothing else changes at the cutoff of forced labor recruitment which could potentially affect labor market success. To the best of my knowledge, there were no other policies changing discontinuously at the cutoff dates of the conscription policy. The cutoff for school enrollment was mid-year (Richardson, 2000) and XXX. Unfortunately, there are no pre-war census microdata available, so I cannot check for continuity at the cutoff of labor market outcomes prior to the treatment. To give some evidence for no discontinuous jumps at the cutoff, I estimate the RDD with religious denomination as the outcome variable. Religion is often determined at birth and unlikely to change later in life or to be affected by the forced labor conscription. Figure [xx] shows the results for this.

The second identifying assumption of RDD is that individuals cannot manipulate the running variable (month of birth in my setting). While a persons' date of birth is generally

[30]In the census data, I only know the month of birth instead of the actual date of birth. One assumption in RDD is that one can extrapolate the running variable infinitely close to the cutoff, which is not possible with the running variable being the month of birth. However, XXX

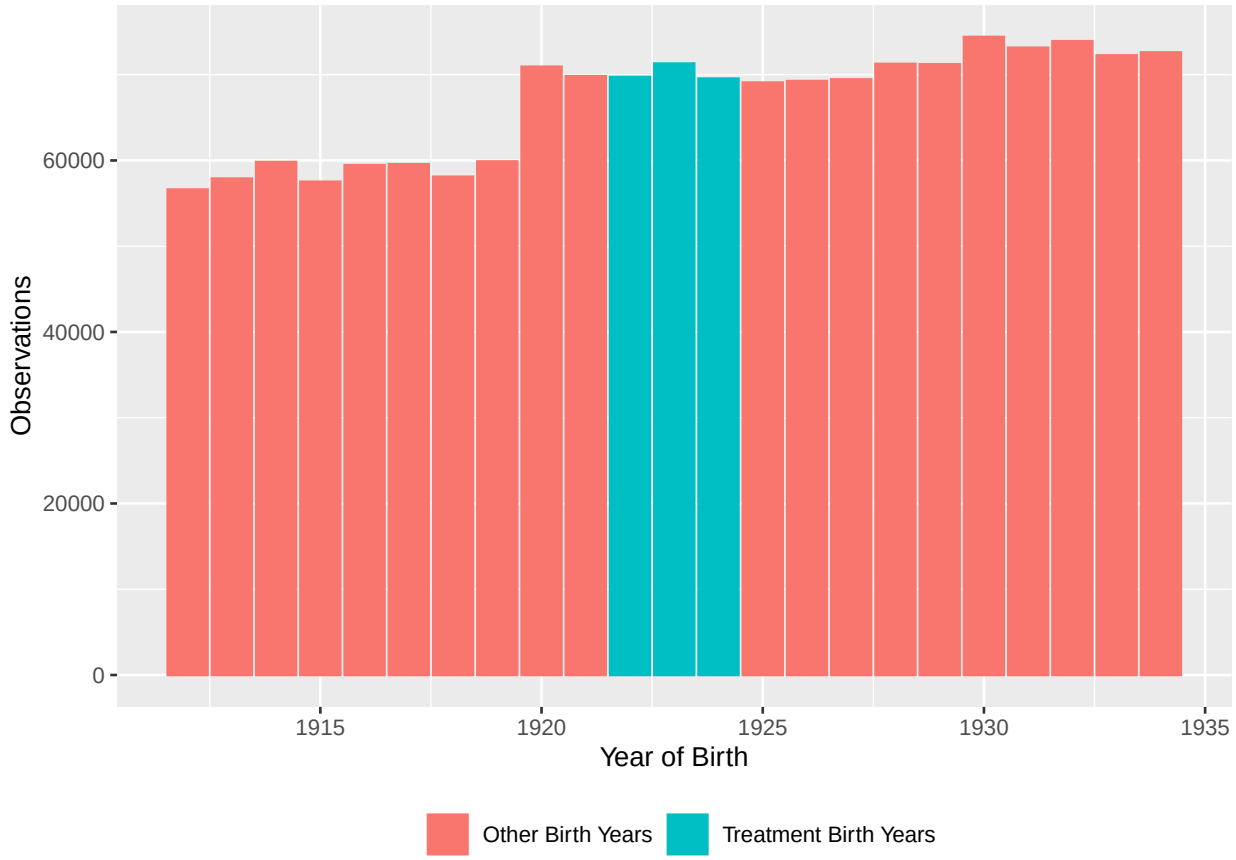


Figure 3: Density of Running Variable

Notes. The data is based on XXXEXPLAIN.

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, they 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 **??** depicts the density of birth dates in the 1971 census. As one can see, 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 differences at the cutoffs can be attributed to the treatment effect. In my setting, the treatment of forced labor conscription is a bundle

of different experiences: For compliers of the conscription 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 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 having either no or no freely chosen employment (see section 2 for a detailed discussion).

Some individuals who were 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[31]. So there are non-compliers with the treatment assignment in both the control and the treatment group. Since I cannot identify on an individual level who these non-compliers are, I estimate a reduced form of a Fuzzy RDD, where I exploit that the probability of treatment discontinuously changes at the cutoff of conscription. The estimation equation takes the following form:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 MonthofBirth_i + \beta_3 Treat_i * MonthofBirth_i \epsilon_i \tag{1}$$

Y_i are labor market outcomes, specifically educational attainment, income class, and social group, which is a compound measure combining skill level of occupation, type of position in a company, and income and education. The coefficient β_1 identifies 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 being treated. 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.

[31] See figure 1b for the share of each cohort that went to Germany

Figure 4 shows the average outcomes for each month of birth and the corresponding linear functions fitted to either side of the cutoffs, with a bandwidth of 12 months. The resulting intention to treat effect β_1 is displayed in each corresponding graph, and the underlying regression results are shown in table XX. For the treated group born in 1922, the highest education attainment (measured on a scale from 0 to 8) is lower by 0.0473 points, which translates to 59% of one standard deviation. They also have lower income class (measured on a scale from 0 to 5) by 0.0268, which corresponds to 67% of one SD. As the the income classes are measured in steps of 4,000 Dutch Guilders yearly income (1,970 USD), this translates to an income loss of 52 USD per year. The social group indicator, which is a proxy for socioeconomic status on a scale from 0 to 35, is lower by 0.2845 or 72% of a standard deviation. At the cutoff at the end of 1924, there is no significant negative effect of being conscribed to forced labor. If anything, the social group indicator is slightly larger for the treatment group by 0.1782, which translates to 45% of one standard deviation. One factor which may explain these results is the difference in compliance with the conscription between the two groups: As the recruitment started with individuals born in 1924, there was a larger share of men from the older cohort who were able to go into hiding[32]. The men who went to Germany did have constant employment, mostly in the manufacturing sector, while those who avoided conscription often hid in rural areas and were often only employed in the agricultural sector with non-economically-meaningful tasks to keep them busy. These differences in skill acquisition may then translate into lower earnings also later in life. Another reason may be that the younger individuals born in 1924 had not yet chosen a specific field of work when being deported at age 18-19. Those born in 1922, aged 20-21 at time of conscription had already started working in the Netherlands prior to the forced labor experience and may therefore have faced a larger disruption in their employment history by the forced labor. Since the majority of Dutch forced workers were employed in manufacturing in Germany, especially the younger group may have stayed in this field of employment, while

[32]Figure 1b shows the compliance by year of birth

the older group may have been less likely to switch careers to the one they gained experience in while in Germany after returning to the Netherlands. Since especially manufacturing was a well-paying industry in the 1970s in the Netherlands, this difference in occupational choice may explain some of these differences. I will explore these possibilities empirically in section 4.1.2 by studying differences in effects by compliance, by sector of employment in Germany, and by later industry of employment in the Netherlands. Using data on the sectoral composition of the municipality of birth of the forced workers, I will study whether the effects are more pronounced for individuals who gained experience in a different and possibly higher-paying sector in Germany compared to their location of origin.

Robustness Checks Table XX shows robustness checks with different bandwidths and non-parametric estimation to relax the assumption of linearity. Add results for restricted control group

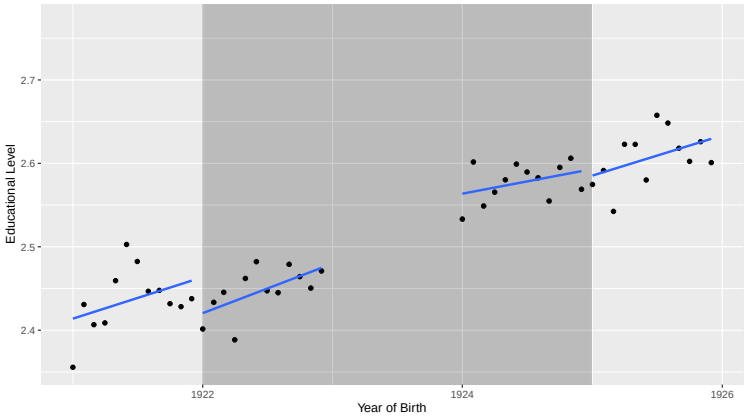
4.1.1 Further Data sources

1960 Census The results using the 1971 census data show the consequences at one point in time. To add to these results and examine how the effects may change over the lifetime of the affected individuals, I use two additional data sources from different points in time. First, I examine possible consequences by using the 1960 census, thus 15 years after the end of the forced labor experience. Since I only know the year of birth, I can only estimate simple differences without controlling for the month of birth as the runnin variable. The estimation equation takes the following form:

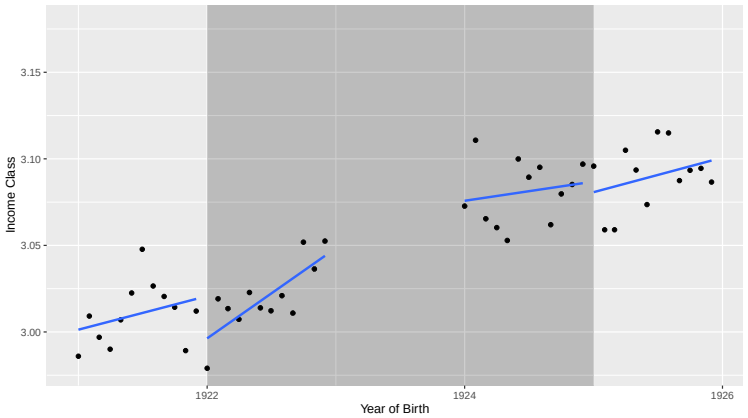
$$Y_i = \beta_0 + \beta_1 Treat_i + \epsilon_i \tag{2}$$

I find XX

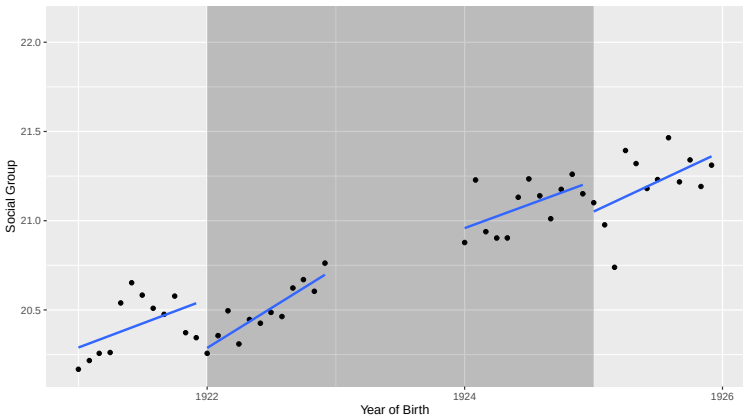
Eurobarometer To uncover possible effects later in life, I use 50 waves of the Eurobarometer survey from 1975 to 1994. I infer the year of birth based on the individuals'



(a) RDD Results: Educational Attainment
Discontinuity at 01.01.1922: β_1 = -0.0473 (0.0193)**
Discontinuity at 31.12.1924: β_1 = 0.0091 (0.0191)



(b) RDD Results: Income Class
Discontinuity at 01.01.1922: β_1 = -0.0268 (0.0134)**
Discontinuity at 31.12.1924: β_1 = 0.0061 (0.0134)



(c) RDD Results: Social Group
Discontinuity at 01.01.1922: β_1 = -0.2845 (0.0985)***
Discontinuity at 31.12.1924: β_1 = 0.1782 (0.0957)*

Figure 4: RDD Results

Notes. Panel a depicts XXX.

reported age at the time of the survey. I exclude individuals for whom I do not know whether they were born within the period of 1922 to 1924 or outside of it.[33][34]. Due to the lack of birth date beyond an individuals' age, I have to resort to simple differences instead of exploiting the discontinuity at the cutoff of conscription in a RDD. I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \tag{3}$$

where λ_t are wave fixed effects to control for differences in survey design. The outcomes of interest Y_{it} are years of education, income class and skill level of occupation. Figure 5a shows the results. There seem to be no significant differences in either of the labor market outcomes. When estimating equation (3) separately for the older and the younger cohorts, mirroring the two RDD settings done for the 1971 census, I do find significantly lower skill level of occupation (see figure 5b) for the older cohort, by 0.325[35] (see figure 5b). This amounts to 16% of a standard deviation.

To study how the effect changes over the lifetime of treated individuals, I exploit the time-variation of the Eurobarometer survey waves and estimate a flexible setup where I allow the effect of treatment to vary over time:

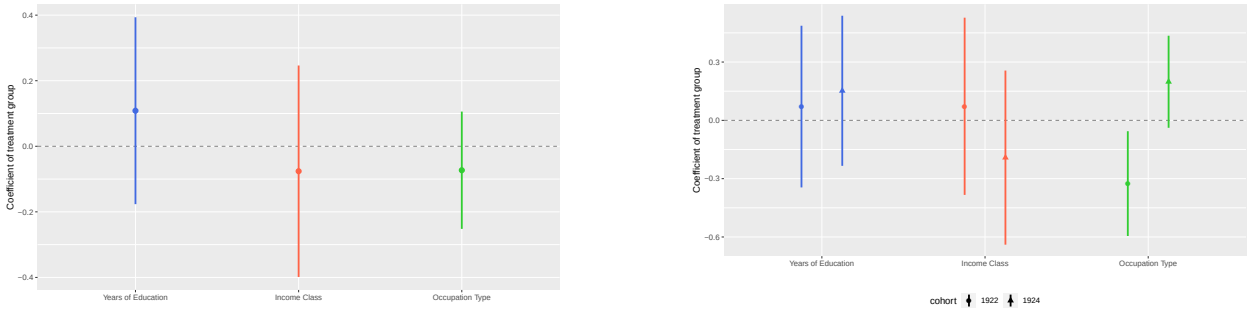
$$Y_{it} = \beta_0 + \Sigma_{t=1}^{T} \beta_{1t} Treat_{it} + \lambda_t + \epsilon_{it} \tag{4}$$

I define t as four segments of waves from five years each to ensure sufficient power:

[33]For example, in the 3rd Eurobarometer wave done in June 1975, an individual who is 53 years old at that time is born sometime between June 1921 and April 1922. It is therefore unclear whether they are part of the treatment or the control group. I define the following treatment and control groups, 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, the younger treatment group are born in 1923 or 1924, and the younger control group are men born in 1925 or 1926. In the example of the Eurobarometer from June 1975, these groups would be 54, 52, 51 and 49 years old respectively. Individuals aged 53 and 50 in June 1975 are excluded from the sample.

[34]Figure XX shows the number of observations for these for groups.

[35]The outcome variable is measured on a level from 0 to 7.



(a) XX (b) XX

Figure 5

Notes. Panel a depicts XXX.

$t \in 1975-1979, , ..., 1990-1994$[36]. Figure XX shows the results, XX

4.1.2 Heterogeneities

Compliance Show map of compliance by Dutch municipality, scale estimates by compliance, discuss results

Occupational choice XX Refer back to discussion in introduction/results about diff in in disruption, compare for different

4.2 Secondary Outcomes

So far, I have looked at the effects of forced labor conscription on a persons' labor market sucess. However, it is also insightful to understand whether other areas of life were also affected by this drastic experience. I therefore look at indicators for wealth, physical health, psychological well-being, family formation and political attitudes. I cannot disentangle the way in which causality runs, as all of these factors may be mechanisms which explain my findings of lower labor market outcomes, but in turn the lower labor market outcomes may be affecting these outcomes. 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

[36]Figure 12 depicts the number of observations per wave.

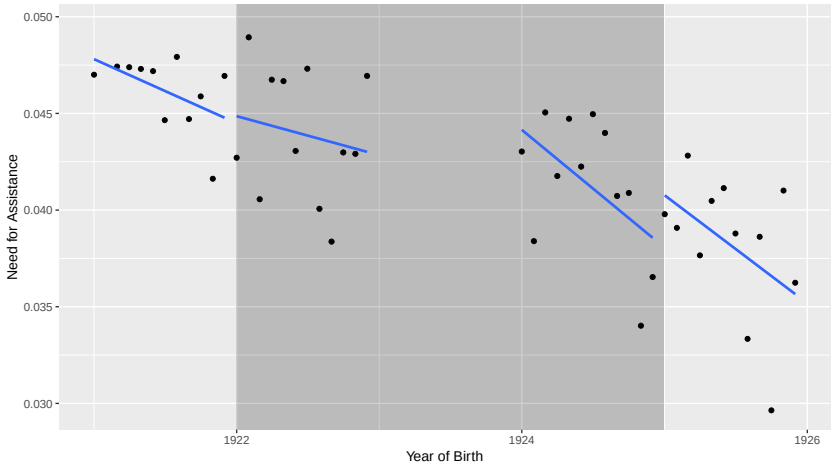


Figure 6: RDD Results: Need for Assistance

Discontinuity at 01.01.1922: β_1 = -0.0005 (0.0026)
Discontinuity at 31.12.1924: β_1 = -0.0027 (0.0025)

consequences and thereby pinpoint policy implications for bettering the situation of former forced workers.

Wealth To measure wealth, I use the worth of self-owned housing and the number of telephone lines as reported in the 1971 census. Figure XX ...

Physical Health 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 6 shows the average likelihood of declaring a need for assistance for each month of birth of Dutch men around the two cutoffs as well as the estimated discontinuities using equation 1 and a 12 months bandwidth. There is no significant difference at either cutoff, indicating no difference in physical health. 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

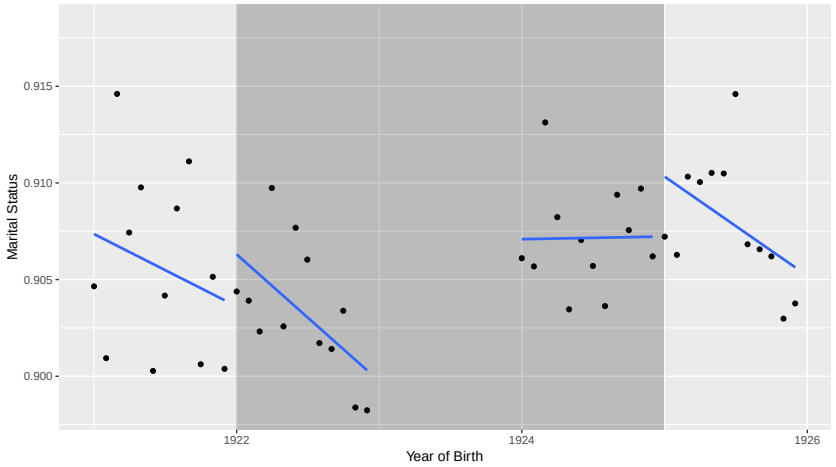
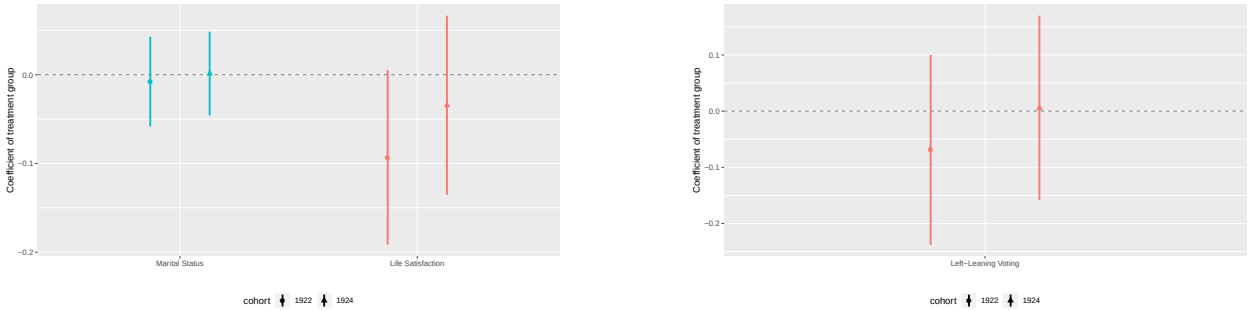


Figure 7: RDD Results: Marriage Probability

Discontinuity at 01.01.1922: β_1 = -0.0028 (0.0032)
Discontinuity at 31.12.1924: β_1 = -0.0034 (0.0031)

for assistance in daily life do not (yet) show up in the results. Besides, the measurement error because of individuals who needed assistance but refused to answer the question may bias the estimated discontinuity towards zero.

Psychological and Social Outcomes To understand what consequences the conscription into forced labor had on the individuals' social life outside of their labor market experience, I look at marital status, number of children, and self-reported well-being. Figure 7 shows the results for marital status as reported in the 1971 census using equation 1 and a 12 months bandwidth, where the dependent variable is a dummy that takes the value of one for ever being married (including widowed, living separately and divorced), and zero otherwise. There are no discontinuous differences between the treatment and control groups at either cutoff. Figure XX shows the effect of forced labor on the total number of children the subjects have by the time of the census of 1971 (so aged 45 to 50 years old). XX Panel a of figure 8 confirms the non-effect of forced labor on ever being married using the Eurobarometer survey data and estimating equation 3 with a dummy of ever being married as the dependent variable. So there seems to be no effect of being conscripted into forced labor on the likelihood of ever



(a) XX (b) XX

Figure 8

Notes. Panel a depicts XXX.

getting married.

To further investigate whether there may be consequences on individuals' well-being, I repeat the same estimation with a measure of life satisfaction as the dependent variable, which is answered in the Eurobarometer survey and which I harmonized to a scale from zero to three. Figure 8a shows that while the differences are not statistically significant, the effect is possibly a bit more negative for the older cohort. This is in line with my findings for labor market outcomes, which were also worse for the older treatment group.

Political Attitudes In Panel b of figure 8, I test whether individuals who were subject to forced labor conscription differ in their political attitudes by using the indicated voting preference from the Eurobarometer survey, which distinguishes between left, central and right leaning voting. I again estimate equation 3 where left-leaning voting is coded as two, central as one, and right-leaning voting as zero. There are no differences for either cohort, suggesting that the forced labor experience did not affect individuals' political attitudes, at least not on this left-to-right voting scale.

5 Conclusion

References

(1934). Uitkomsten van de Beroepstelling 1930. Algemene Landsdrukkerij, 's-Gravenhage.

Abramitzky, R., Boustan, L., and Eriksson, K. (2019). TO THE NEW WORLD AND BACK AGAIN: RETURN MIGRANTS IN THE AGE OF MASS MIGRATION. *Industrial & Labor Relations Review*, 72(2):300–322.

Abramitzky, R., Boustan, L., Eriksson, K., Feigenbaum, J., and Pérez, S. (2021). Automated Linking of Historical Data. *Journal of Economic Literature*, 59(3):865–918.

Althausen, M. (1999). Niederländische Zwangsarbeiter in Berlin. In Spanjer, R., Oudesluijs, D. M., and Meijer, J., editors, *Zur Arbeit gezwungen: Zwangsarbeit in Deutschland 1940-1945*, pages 173–180. Edition Temmen, Bremen.

Angrist, J., Chen, S. H., and Frandsen, B. R. (2010). Did Vietnam veterans get sicker in the 1990s? The complicated effects of military service on self-reported health. *Journal of Public Economics*, 94(11-12):824–837. Publisher: Elsevier.

Bauer, T. K., Braun, S., and Kvasnicka, M. (2013). The Economic Integration of Forced Migrants: Evidence for Post-War Germany. *The Economic Journal*, 123(571):998–1024. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/ecoj.12023.

Bauer, T. K., Giesecke, M., and Janisch, L. M. (2019). The Impact of Forced Migration on Mortality: Evidence From German Pension Insurance Records. *Demography*, 56(1):25–47.

Becker, S. O. (2022). Forced displacement in history: Some recent research. *Australian Economic History Review*, 62(1):2–25. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/aehr.12237.

Becker, S. O., Grosfeld, I., Grosjean, P., Voigtländer, N., and Zhuravskaya, E. (2020). Forced Migration and Human Capital: Evidence from Post-WWII Population Transfers. *American Economic Review*, 110(5):1430–1463.

Beening, A. (2003). Der Kampf um Anerkennung: Ehemalige Zwangsarbeiter aus den Niederlanden. *Jahrbuch / Zentrum für Niederlande-Studien*, 14:105–119.

Bertocchi, G. and Dimico, A. (2014). Slavery, education, and inequality. *European Economic Review*, 70(C):197–209.

Blattman, C. and Annan, J. (2010). The Consequences of Child Soldiering. *The Review of Economics and Statistics*, 92(4):882–898.

Braun, S. T. and Stuhler, J. (2023). Exposure to War and its Labor Market Consequences Over the Life Cycle. *SSRN Electronic Journal*.

Buggle, J. C. and Nafziger, S. (2021). The Slow Road from Serfdom: Labor Coercion and Long-Run Development in the Former Russian Empire. *The Review of Economics and Statistics*, 103(1):1–17.

Buonanno, P. and Vargas, J. F. (2019). Inequality, crime, and the long run legacy of slavery. *Journal of Economic Behavior & Organization*, 159(C):539–552.

CBS (1947). *Economische en sociale Kroniek der oorlogsjaren 1940 - 1945*. De Haan, Utrecht.

Cinnirella, F. and Hornung, E. (2016). Landownership concentration and the expansion of education. *Journal of Development Economics*, 121(C):135–152.

Conti, G., Poupakis, S., Ekamper, P., Bijwaard, G., and Lumey, L. H. (2021). Severe Prenatal Shocks and Adolescent Health: Evidence from the Dutch Hunger Winter. Technical Report 2021-056, Human Capital and Economic Opportunity Working Group. Publication Title: Working Papers.

Davis, R. G. (2010). *Bombing the European Axis Powers: A Historical Digest of the Combined Bomber Offensive, 1939 -1945*. Air University Press.

de Zwarte, I. (2020). *The Hunger Winter: Fighting Famine in the Occupied Netherlands, 1944–1945*. Studies in the Social and Cultural History of Modern Warfare. Cambridge University Press, Cambridge.

Dell, M. (2010). The persistent effects of peru's mining mita. *Econometrica*, 78(6):1863–1903.

Deryugina, T., Kawano, L., and Levitt, S. (2018). The Economic Impact of Hurricane Katrina on Its Victims: Evidence from Individual Tax Returns. *American Economic Journal: Applied Economics*, 10(2):202–233.

Fujiwaray, T., Laudaresz, H., and Caicedo, F. V. (2017). Tordesillas, slavery and the origins of brazilian inequality.

Grüter, R. and Mourik, A. v. (2020). Dutch Repatriation from the Former Third Reich and the Soviet Union: Political and Organizational Encounters and the Role of the Netherlands Red Cross. *Historical Social Research*, 45(4):151–172.

Herbert, U. (1999). *Fremdarbeiter: Politik und Praxis des Ausländer-Einsatzes in der Kriegswirtschaft des Dritten Reiches*. Dietz, Bonn, neuaufl. edition.

Hjalmarsson, R. and Lindquist, M. J. (2019). The Causal Effect of Military Conscription on Crime*. *The Economic Journal*, 129(622):2522–2562.

Homze, E. L. (1967). *Foreign Labor in Nazi Germany*. Princeton University Press.

Höschler, C. and Panek, I., editors (2019). *Zweierlei Suche: Fundstücke zu Displaced Persons in Arolsen nach 1945*. Arolsen Archives, Bad Arolsen.

ILO (2022). *Global estimates of modern slavery forced labour and forced marriage*. Geneva.

Jongbloed, A. (1996). *Voor koning(in) en vaderland: Dienstplicht door de jaren heen*. ALPHA, Zutphen.

Kesternich, I., Siflinger, B., Smith, J. P., and Winter, J. K. (2014). The Effects of World War II on Economic and Health Outcomes across Europe. *The Review of Economics and Statistics*, 96(1):103–118.

Kuck, C. (2010). NiederlandeNet – Geschichte - Niederländische Zwangsarbeiter - Wirtschaft und Arbeitslosigkeit in den Niederlanden vor Beginn der Besatzungszeit.

Lagrou, P. (1999). *The Legacy of Nazi Occupation: Patriotic Memory and National Recovery in Western Europe, 1945–1965*. Studies in the Social and Cultural History of Modern Warfare. Cambridge University Press, Cambridge.

Lak, M. (2016). Trading with the Enemy? In *Paying for Hitler's War: The Consequences of Nazi Hegemony for Europe*, pages 140–163. Cambridge University Press.

LeBaron, G., editor (2018). *Researching forced labour in the global economy: methodological challenges and advances*. Number 220 in Proceedings of the British Academy. The British Academy by Oxford University Press, Oxford, first edition edition. OCLC: on1079791481.

Lofti, G. (2000). Niederländische Zwangsarbeiter in Arbeitserziehungslagern der Gestapo. In Fasse, N., Houwink ten Cate, J. T. M., and Lademacher, H., editors, *Nationalsozialistische Herrschaft und Besatzungszeit: historische Erfahrung und Verarbeitung aus niederländischer und deutscher Sicht*, number Bd. 1 in Studien zur Geschichte und Kultur

Nordwesteuropas, pages 257–264. Waxmann, Münster ; New York. OCLC: ocm47898877.
Lumey, L. H., Ekamper, P., Bijwaard, G., Conti, G., and van Poppel, F. (2021). Overweight and obesity at age 19 after pre-natal famine exposure. *International Journal of Obesity*, 45(8):1668–1676. Publisher: Nature Publishing Group.
Marie, O. and Zwiers, E. (2023). Religious Barriers to Birth Control Access. *IZA Discussion Paper Series*, 16051.
Markevich, A. and Zhuravskaya, E. (2018). The economic effects of the abolition of serfdom: Evidence from the russian empire. *American Economic Review*, 108(4-5):1074–1117.
Marx, H. (2019). *Die Verwaltung des Ausnahmezustands: Wissensgenerierung und Arbeitskräftelenkung im Nationalsozialismus*. Geschichte des Reichsarbeitsministeriums imNationalsozialismus. Wallstein Verlag, Göttingen.
Mitchener, K. J. and McLean, I. W. (2003). The productivity of us states since 1880. *Journal of Economic Growth*, 8(1):73–114.
Nunn, N. (2008). *Slavery, Inequality, and Economic Development in the Americas: An Examination of the Engerman-Sokoloff Hypothesis*, pages 148–180. Harvard University Press, Cambridge.
Oreopoulos, P., von Wachter, T., and Heisz, A. (2012). The Short- and Long-Term Career Effects of Graduating in a Recession. *American Economic Journal: Applied Economics*, 4(1):1–29.
Pfahlmann, H. (1968). *Fremdarbeiter und Kriegsgefangene in der deutschen Kriegswirtschaft 1939-1945*. Beiträge zur Wehrforschung. Wehr und Wissen Verlagsgesellschaft, Darmstadt.
Proudfoot, M. J. (1957). *European Refugees: 1939-52 : A study in forced population movement*. Faber and Faber, London.
Ramirez, D. and Haas, S. A. (2022). Windows of Vulnerability: Consequences of Exposure Timing during the Dutch Hunger Winter. *Population and Development Review*, 48(4):959–989. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/padr.12513.
Richardson, A. (2000). Children, Youth and Schooling Disruption in the Netherlands During World War II. *Groniek*, (148). Number: 148.
Sarvimäki, M., Uusitalo, R., and Jäntti, M. (2022). Habit Formation and the Misallocation of Labor: Evidence from Forced Migrations. *Journal of the European Economic Association*, 20(6):2497–2539.
Schwandt, H. and von Wachter, T. (2020). Socio-Economic Decline and Death: The Life-Cycle Impacts of Recessions for Labor Market Entrants. Technical Report w26638, National Bureau of Economic Research, Cambridge, MA.
Sijes, B. A. (1966). *De arbeidsinzet: De gedwongen arbeid van Nederlanders in Duitsland, 1940-1945*. Martinus Nijoff, Den Haag. Google-Books-ID: mrNCAQAAIAAJ.
Soares, R., Assunção, J. J., and Goulart, T. F. (2012). A note on slavery and the roots of inequality. *Journal of Comparative Economics*, 40(4):565–580.
Spoerer, M. (2001). *Zwangsarbeit unter dem Hakenkreuz. Ausländische Zivilarbeiter, Kriegsgefangene und Häftlinge im Dritten Reich und im besetzten Europa 1939-1945*.
Spoerer, M. and Fleischhacker, J. (2002). Forced Laborers in Nazi Germany: Categories, Numbers, and Survivors. *The Journal of Interdisciplinary History*, 33(2):169–204.
Stiftung Erinnerung, Verantwortung und Zukunft (2017). *The German compensation program for forced labor: practice and experiences*. Stiftung Erinnerung, Verantwortung und Zukunft, Berlin.

Tooze, J. A. (2006). *The wages of destruction: the making and breaking of the Nazi economy*. Allen Lane, London ; New York. OCLC: ocm64313370.

Van Eden, C. (1946). The Education of the Youth. *The ANNALS of the American Academy of Political and Social Science*, 245(1):129–143. Publisher: SAGE Publications Inc.

von Wachter, T. (2020). The Persistent Effects of Initial Labor Market Conditions for Young Adults and Their Sources. *Journal of Economic Perspectives*, 34(4):168–194.

Warmbrunn, W. (1972). *The Dutch under German occupation: 1940-1945*. Stanford Univ. Pr, Stanford, Calif, repr edition.

Tables

Table 1: Labor Market Effects – Eurobarometer

	Dependent variable:		
	Years of Education	Occupation Type	Income
	(1)	(2)	(3)
treatmentGroup	0.108	−0.073	−0.076
	(0.173)	(0.109)	(0.196)
Wave FE	YES	YES	YES
Dependent Variable Range	14-22	0-7	1-12
Mean Dependent Variable	16.47	0.92	6.32
Observations	1,249	1,182	1,044
R^2	0.039	0.349	0.127
Adjusted R^2	0.0002	0.322	0.087
Note:	*p<0.1; **p<0.05; ***p<0.01		
	Standard Errors are clustered at wave level.		

Table 2: Labor Market Effects – Eurobarometer – separately by Cohort

	Dependent variable:					
	Years of Education	Occupation Type	Income	Years of Education	Occupation Type	Income
	(1)	(2)	(3)	(4)	(5)	(6)
treatmentGroup	0.071	−0.325**	0.072	0.152	0.199	−0.192
	(0.252)	(0.164)	(0.277)	(0.234)	(0.144)	(0.272)
Wave FE	YES	YES	YES	YES	YES	YES
Dependent Variable Range	14-22	0-7	1-12	14-22	0-7	1-12
Mean Dependent Variable	16.47	0.92	6.32	16.47	0.92	6.32
Observations	616	591	505	633	591	539
R^2	0.082	0.357	0.187	0.075	0.410	0.145
Adjusted R^2	0.005	0.301	0.108	−0.001	0.359	0.069
Note:	*p<0.1; **p<0.05; ***p<0.01					
	Standard Errors are clustered at wave level.					

Table 3: Secondary Outcomes – Eurobarometer – separately by Cohort

	Dependent variable:			
	Marital Status	Life Satisfaction	Marital Status	Life Satisfaction
	(1)	(2)	(3)	(4)
treatmentGroup	−0.008	−0.093	0.001	−0.034
	(0.031)	(0.060)	(0.029)	(0.061)
Wave FE	YES	YES	YES	YES
Dependent Variable Range	0-1	0-3	0-1	0-3
Mean Dependent Variable	0.73	2.29	0.73	2.29
Observations	612	492	631	520
R^2	0.092	0.137	0.077	0.120
Adjusted R^2	0.014	0.063	0.001	0.049
Note:	*p<0.1; **p<0.05; ***p<0.01			
	Standard Errors are clustered at wave level.			

Table 4: Secondary Outcomes – Eurobarometer – separately by Cohort

	Dependent variable:	
	Left-Leaning Voting	
	(1)	(2)
treatmentGroup	−0.069	0.006
	(0.102)	(0.099)
Wave FE	YES	YES
Dependent Variable Range	0-2	0-2
Mean Dependent Variable	0.88	0.88
Observations	309	324
R^2	0.057	0.063
Adjusted R^2	−0.037	−0.025
Note:	*p<0.1; **p<0.05; ***p<0.01	
	Standard Errors are clustered at wave level.	

Figures

Appendix A.

5.1 Data

5.1.1 Cleaning Archival Records

REWRIte!!! The method suggested by Abramitzky et al. (2019) using string comparators (ABE-JW henceforth) links individuals using 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.[37] This alleviatest 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. This means that XX observations, for whom either the date of birth (XX), the first name (XX) or the last name (XX) is missing, remain unlinked and are treated as unique individuals[38]. 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 possible matches to those that have a JW string distance up to 0.1 in either of these variables (where available)[39]. 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. In a robustness check, I relax the requirement on the string distances and allow them to be less than or equal to 0.12 (which translates

[37] For XX of these observations, I only know their exact year of birth, and for XX of these, I only know their exact month of birth. I code them as being born in June/on the 15ths XXX add reasons and say that I exlude month uncertain in my final analysis anyways.

[38] To get an estimate on the size of double-counting unique individuals due to this decision, I also linked individuals using only their name and, where available, their place of birth. Using the same cutoff for the string distances [XXCHECK], this suggests that up to XX individuals may be duplicates that I will treat as unique individuals going forward.

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

to partial agreement according to [cite Winkler]). I then treat all linked individuals as only one observation going forward. The remaining dataset then contains XX observations (XX observations less than in the original data). Because some linked individuals stayed in different German counties, and I want to keep the information on their stay in all of their locations in Germany for some parts of the analysis, I create a separate datafile which only treats all linked individuals who were reported to have been in the same German county as a unique individual, and counts them as two separate observations if they appear in two different locations in Germany. The difference in observations is XX, so an overcounting of XX percent compared to the dataset where all links are treated as a unique observation, irrespective of their location in Germany.

Since, in contrast to Abramitzky et al. (2021), I do know the exact date of birth instead of just an individuals' age, I then also restrict links to individuals who have the same date of birth

5.1.2 Subsample: Most Unaffected Control Group

To exclude that the effects are driven by the control group also being exposed to adverse events connected to WWII, I conduct a robustness check where I limit the sample to individuals from areas most unaffected by the war. To measure exposure to war, I use two different measures: First, following Conti et al. (2021), I digitized the number of civilian deaths due to warfare in each region from June 1943 (when the first cohort of the yearclass-action was called upon) until June 1944 (cbs, 1934). Because most allied bombings did not lead to civilian deaths, but may have still affected the control group in ways which impact their later labor market success, I add data from Davis (2010) on the number of planes of the Allied Forces bombings. I then classify all regions in the upper 25% of either measure as being heavily exposed to the war. Following the literature on the Dutch hunger winter, I classify the cities in the western regions as affected by the hunger winter[40]. When excluding individuals from both the war-affected regions and the hunger-winter regions, this leaves me with XX observations (XX% of the original sample).

5.1.3 Employment Structure

Explain data on empl structure of German counties and of Netherlands.

[40]While there are slight discrepancies in which cities should be included between different studies, the majority includes Amsterdam, Delft, The Hague, Haarlem, Leiden, Rotterdam and Utrecht, which is what I follow in my classification (Conti et al., 2021; Ramirez and Haas, 2022; Lumey et al., 2021)

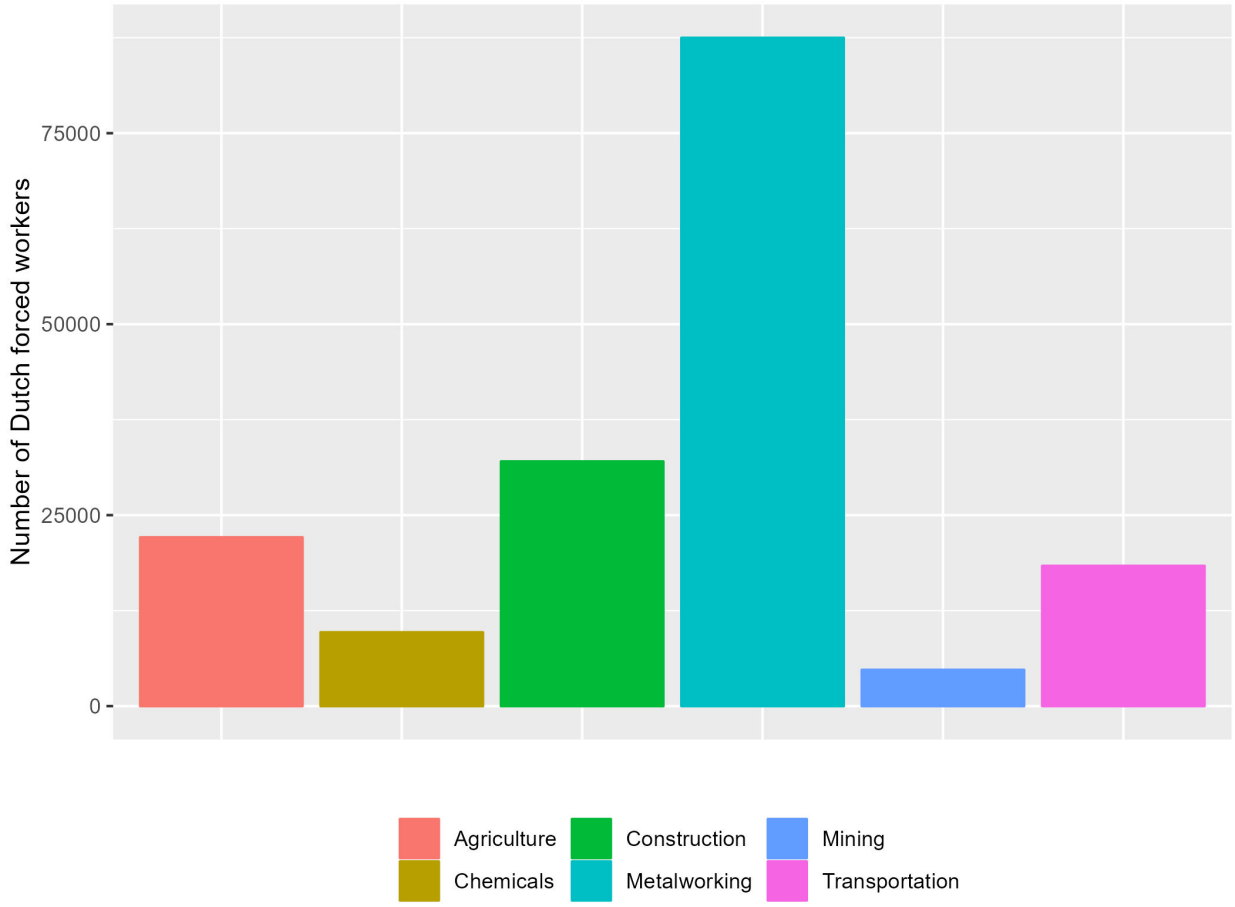


Figure 9: Allocation of Dutch forced workers across industries

Notes. The data is based on XXXEXPLAIN Herbert, 1999.

5.1.4 Eurobarometer

Describe how I harmonized the variables over time

5.2 Descriptives

Map of all male forced workers irrespective of birth year Map of male forced workers by birth year Map of all Dutch forced workers (men and women, irrespective of birth year)

5.3 Robustness Checks

Other Bandwidth Quadratic RDD Simple differences for 1971 to compare with 1960 and Eurobarometer

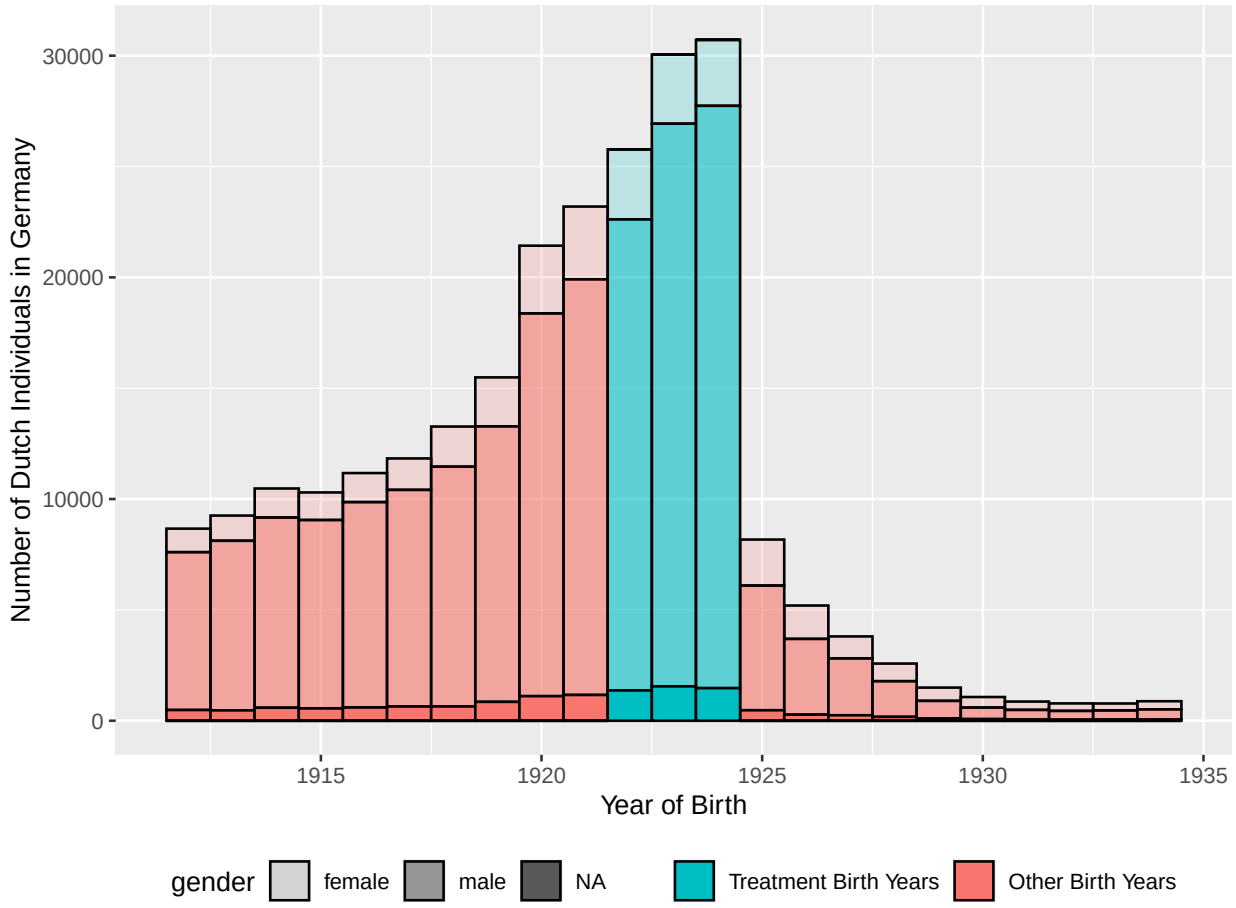


Figure 10: Gender composition of cohorts

Notes. The data is based on XXXEXPLAIN.

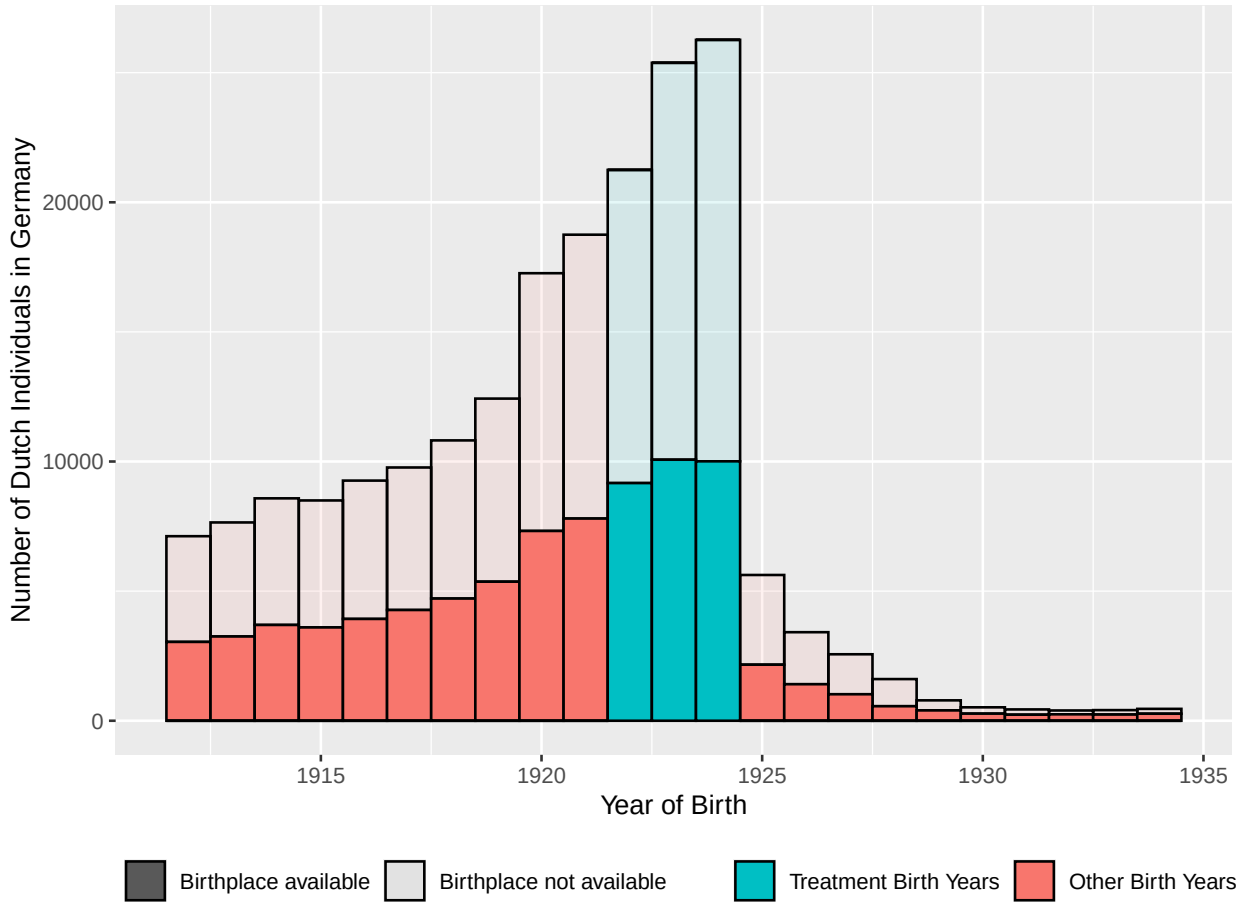


Figure 11: Availability of place of birth of cohorts

Notes. The data is based on XXXEXPLAIN.

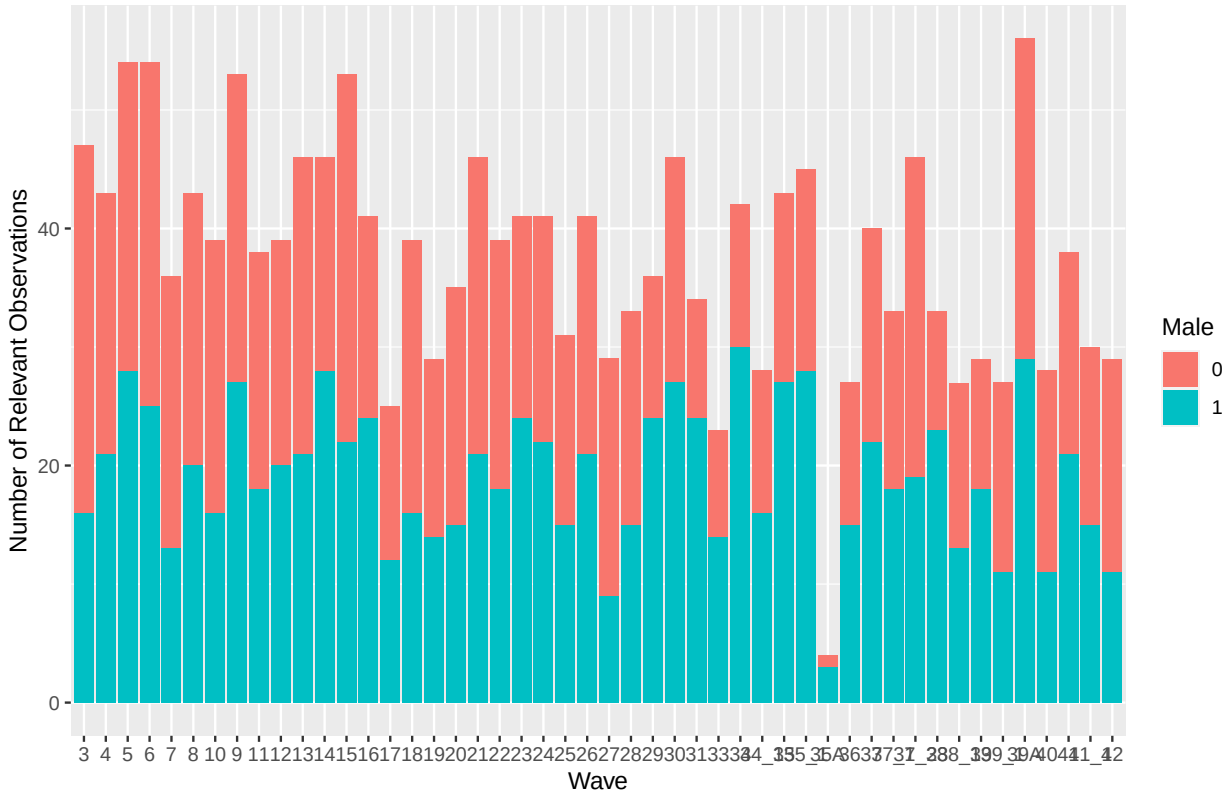


Figure 12: Eurobarometer: Number of observations per wave

Notes. The data is based on XXXEXPLAIN.