

# 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 on labor market outcomes, 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 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 also 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:** Economic History, Labor Economic History, Labor Economics, Employment, Particular Labour Markets, Coercive Labor Market, Forced Labor, Labour Economics: General, General - Health, Education, and Welfare

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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).

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. 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. 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

based on birth date, 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 01.01.1922 of the forced conscription policy<sup>1</sup>. 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 0.68%p. lower, which translates to 2.9% of one standard deviation<sup>2</sup>. 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 1971 census data, 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.

I find no differences in the effect by the share of conscripted individuals who went to Germany, implying that the treatment effect comes from both being forced to work in Germany and being forced to go into hiding. To show this, I link the census data with archival

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<sup>1</sup>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

<sup>2</sup>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).

records on forced workers during WWII provided by the Arolsen Archives<sup>3</sup>. For each Dutch municipality, I derive the share of conscripted workers who can be found in the census and run separate regressions for individuals from places with a share above or below the median. 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.

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 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.

I also present some suggestive evidence that skills acquired during the forced labor experience may have been beneficial for later labor market success if the industries that individuals were working in while in Germany were better in terms of average pay than the industries that they would have most likely been working in had they stayed in the Netherlands. Specif-

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<sup>3</sup>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 XXcite. Since the census data lacks information on names, I link the data based on gender, date of birth, and place of birth.

ically, I derive the difference in employment share in industries that pay above the median in 1971 in the German counties 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 employment 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 probably a conservative lower bound of the persistent effects of forced labor, because the control group to whom I compare the treated cohorts, Dutch men born before the conscription period, 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<sup>4</sup>. Since there is no perfect compliance with forced labor conscription, the intention to treat effect is also probably lower than the local average treatment effect. Additionally, I observe individuals 15 years after the end of their forced labor experience,

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<sup>4</sup>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

so some effects may have already subsided or not yet realized<sup>5</sup>. 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 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<sup>6</sup>.

Additionally, I contribute to the literature on the consequences of forced workers by

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<sup>5</sup>For example, Braun and Stuhler (2023) find that labor market prospects of war veterans are diminished mostly by earlier retirement.

<sup>6</sup>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.

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 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., 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 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 against 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<sup>7</sup>. 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<sup>8</sup>. The distribution of forced workers was done by the Ministry of Labor, based on the labor shortages that 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<sup>9</sup>, and the pay was lower than that for German workers (Herbert, 1999; Sijes, 1966; Kuck, 2010; Tooze, 2006)<sup>10</sup>. The living conditions varied widely,

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<sup>7</sup>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.

<sup>8</sup>Figure XX in Appendix shows the map separately for each cohort, and for all Dutch men.

<sup>9</sup>Figure ?? in the appendix shows the number of forced workers in different industries.

<sup>10</sup>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).

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 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<sup>11</sup> 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<sup>12</sup> 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 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<sup>13</sup> (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<sup>14</sup>.

In 2000, the German government set up a fund to pay symbolic amounts of compensation to former forced workers following pressure due to impending law suits of former forced workers

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<sup>11</sup>See section XX for a detailed description of the construction of this subsample.

<sup>12</sup>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)

<sup>13</sup>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.

<sup>14</sup>See section 5.1.2 for a detailed discussion of the construction of this subsample.

against German companies located in the United States. The compensation was paid half by the German 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)). ()

## 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<sup>15</sup>. 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

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<sup>15</sup>The non-response rate was 0.2%

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<sup>16</sup>. 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? 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

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<sup>16</sup>I perform robustness checks using different bandwidths, see appendix XX

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?)

### 3.2 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<sup>17</sup>. 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<sup>18</sup>. I will exclude these individuals in my analysis<sup>19</sup>. 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<sup>20</sup>. 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<sup>21</sup>, 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

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<sup>17</sup>This includes waves 3 through 42.

<sup>18</sup>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).

<sup>19</sup>In a robustness check, I will include these individuals in either the control or the treatment group.

<sup>20</sup>See section 5.1.4 for a detailed description of this harmonization process

<sup>21</sup>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?"



scale from zero to two, where zero is right-leaning and 2 is left-leaning voting<sup>22</sup>.

### 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 prisoners of war and former inmates of concentration camps, the majority of them are forced workers<sup>23</sup>. 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<sup>24</sup>. To reduce computational requirements, I only compare individuals born on the same date of birth and

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<sup>22</sup>The wording of the question on party affiliation asks: “If there were a general election tomorrow, which party would you support?”

<sup>23</sup>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)

<sup>24</sup>See section [link appendix] for a comparison between their proposed linking method and my approach

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<sup>25</sup>. 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.<sup>26</sup>

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 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 Instituut [XX cite]. The data comes from civil registration data from 2017 and 19th century marriage certificates and indicates how often a given name is used for female and male persons<sup>27,28</sup>. 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 or female respectively. All other names are classified as uncertain<sup>29</sup>. 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

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<sup>25</sup>The place of birth is reported for 30.5% of all entries.

<sup>26</sup>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.

<sup>27</sup>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.

<sup>28</sup>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

<sup>29</sup>78.9% of all unique first names can be classified as either male or female using the cutoff of 70%.

assign their gender<sup>30</sup>. 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<sup>31</sup>. 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 workers 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 gender<sup>32</sup>. These variables are available for 39.8% of the relevant cohorts of 1922 and 1924<sup>33</sup>. 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 one 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 manufactur-

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<sup>30</sup>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.

<sup>31</sup>Figure 16 shows the gender composition over the different cohorts. In total, the dataset includes 75.7% male, and 18.2% percent are female individuals

<sup>32</sup>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

<sup>33</sup>Figure 17 shows the availability of a place of birth for the different cohorts.

ing, trade and transportation, administration, the medical sector, and domestic services<sup>34</sup>. 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 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<sup>35</sup>.

**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.

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<sup>34</sup>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

<sup>35</sup>Explain in more detail and how many places I end up being able to merge!

## 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 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.<sup>36</sup> 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

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<sup>36</sup>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

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 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<sup>37</sup>. 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:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 MonthofBirth_i + \beta_3 MonthofBirth_i^2 \epsilon_i \quad (1)$$

$Y_i$  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  $\beta_1$  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.

Figure 5 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

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<sup>37</sup>See figure 1b for the share of each cohort that went to Germany

intention to treat effect  $\beta_1$  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 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<sup>38</sup>. 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 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

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<sup>38</sup>Figure 1b shows the compliance by year of birth



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 4.3 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.

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 that may bias the effects downwards: 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 for those of Dutch civilians. 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). Second, the forced labor also only lasted for a comparatively short period of time of two years, which may be associated with less severe long-run effects. Third, the control group to whom I compare the treated cohorts, Dutch men born before the conscription period, 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 could still lead to an underestimation of effect sizes. Fourth, treatment probability increases at the cutoff, but there is no perfect compliance because some individuals from the control group were also forced to work in Germany through measures of coercion, and some individuals from the conscripted cohorts were granted an exception. So the intention to treat effect is probably lower than the local average treatment effect. Additionally, I observe individuals at 15 years after the end of their

forced labor experience, so some effects may have already subsided or not yet realized<sup>39</sup>.

**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 The results are robust to different specifications of the RDD equation: Excluding the quadratic term of the running variable, a non-parametric estimation, and allowing for different slopes on both sides of the cutoff. My results are also robust to using different bandwidths, different kernel choice, poisson regression and including individuals from the Hunger Winter regions.

#### 4.1.1 Further Data sources

**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' 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.<sup>4041</sup>. 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:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \quad (2)$$

where  $\lambda_t$  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 ?? shows the results. There seem to be no significant differences in either of the labor market

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<sup>39</sup>For example, Braun and Stuhler (2023) find that labor market prospects of war veterans are diminished mostly by earlier retirement.

<sup>40</sup>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.

<sup>41</sup>Figure XX shows the number of observations for these for groups.

outcomes. When estimating equation (2) 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 ??) for the older cohort, by 0.325<sup>42</sup> (see figure ??). 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:

$$Y_{it} = \beta_0 + \sum_{t=1}^T \beta_{1t} Treat_{it} + \lambda_t + \epsilon_{it} \quad (3)$$

I define  $t$  as four segments of waves from five years each to ensure sufficient power:  $t \in 1975 - 1979, \dots, 1990 - 1994$ <sup>43</sup>. Figure XX shows the results, XX

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

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<sup>42</sup>The outcome variable is measured on a level from 0 to 7.

<sup>43</sup>Figure 18 depicts the number of observations per wave.

**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 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 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.

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 ?? 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.

**Marital and Parental Status** 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 and parental status, meaning the probability to have children.

Figure 8a shows the results for marital status as reported in the 1971 census using equa-

tion 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 ?? of figure ?? confirms the non-effect of forced labor on ever being married using the Eurobarometer survey data and estimating equation 2 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 getting married.

### 4.3 Heterogeneities

XXShow maps! **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

The 1971 census data 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 sample to non-movers, which leaves me with XX observations. 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.

From introduction, write differently and expand: To differentiate between the bundled treatment of being forced to work in Germany and being forced to go into hiding, I supplement the data with archival records on forced workers during WWII provided by the Arolsen Archives, see data description, ... I then conducted a heterogeneity analysis based on the share of conscripted individuals from each Dutch municipality that can be found in

the archival records. 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. I differentiate between individuals from Dutch municipalities with an above or below median share of conscripted individuals that can be found in the archival records.

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 conditions. I measure the severity of forced labor in German counties by 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. For each Dutch municipality, I then calculate the average exposure to severe conditions weighted by the number of conscripted forced workers who went to each German county. Especially 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, pointing to adverse health effects as a possible mechanism for the long-lasting lower labor force participation. To corroborate these results, I repeat the analysis for likelihood to need 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.

To further investigate whether the skills acquired during the forced labor experience may be beneficial under some circumstances, 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 identified the industry sectors which pay above the median of all sectors in 1971, and then calculated 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 then 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 that 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, while individuals from Dutch municipalities that went to counties with less high-paying industries would have been more likely to acquired skills in industries which were less well-paying in the 1970s than if they had not been conscripted into forced labor. Figure 13 shows that the negative effects of forced labor on employment are driven completely by 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 14 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

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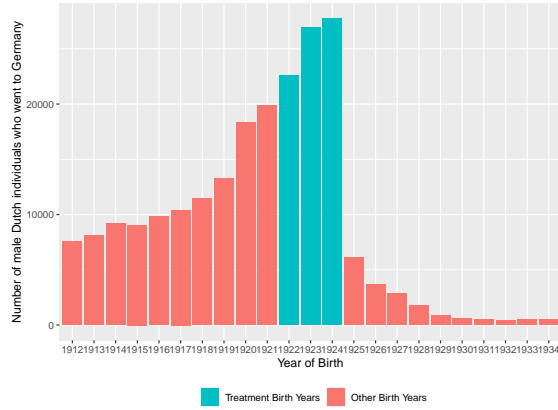
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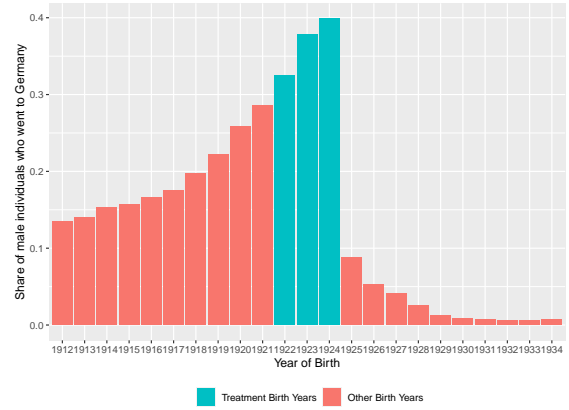
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(a) Absolute number of forced workers



(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.

## Figures

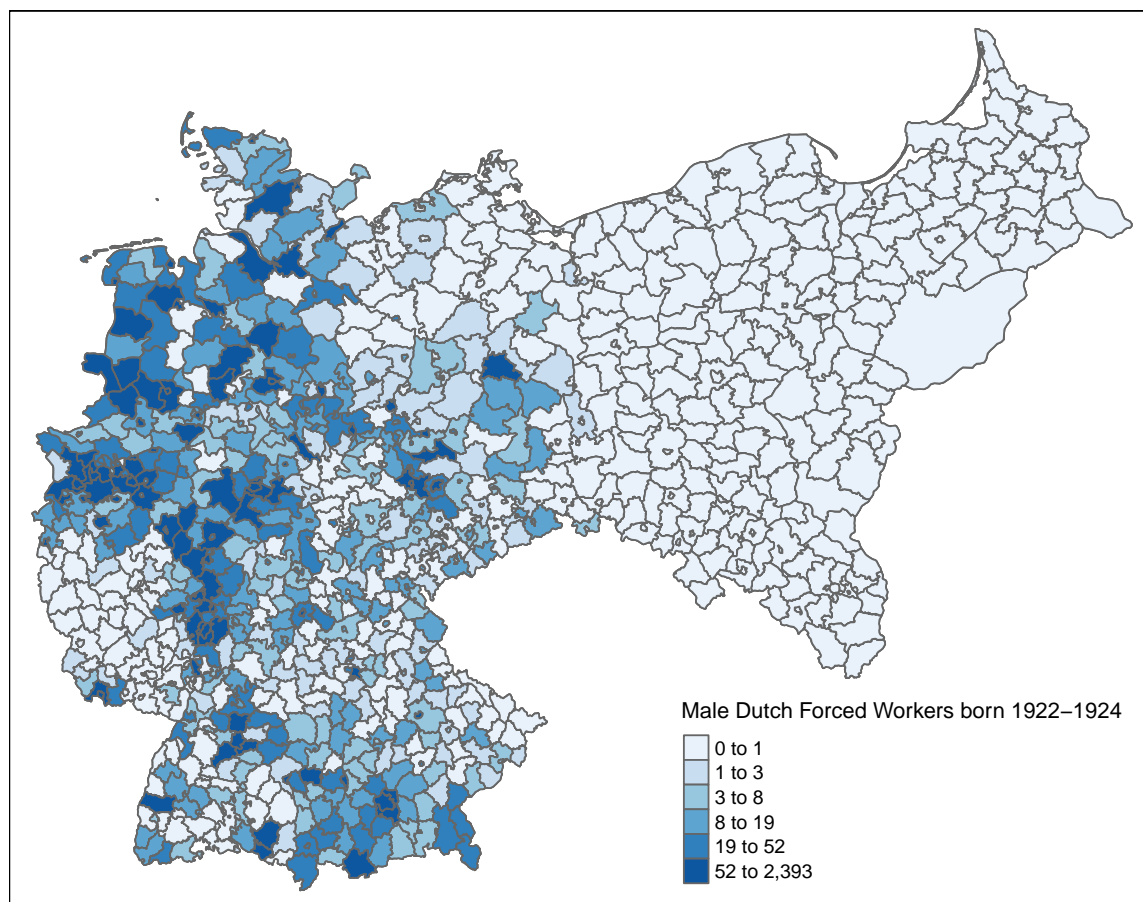


Figure 2: Distribution of forced workers across Germany

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



Figure 3: Density of Running Variable

*Notes.* The data is based on XXXEXPLAIN.

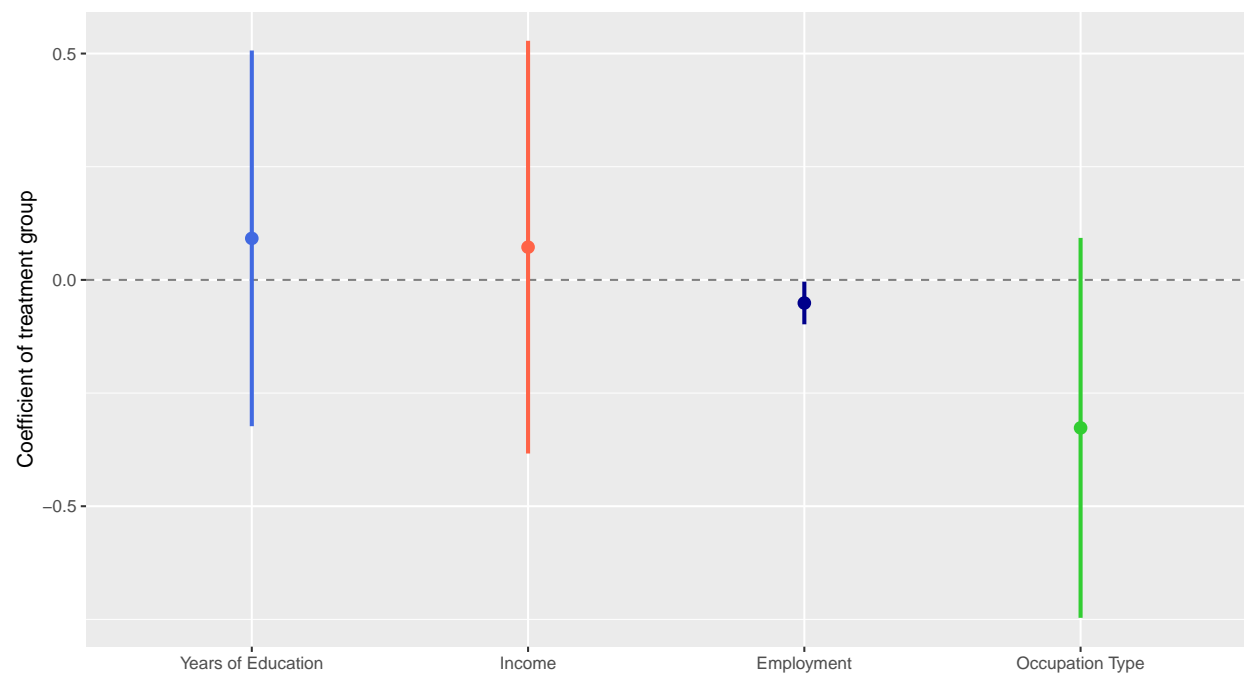
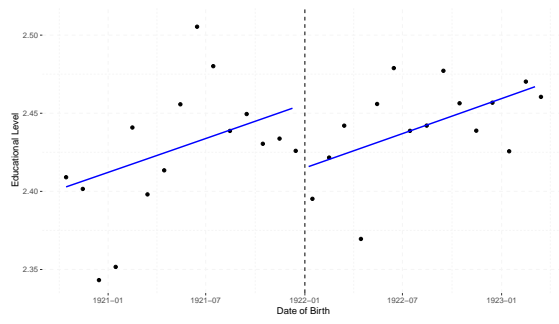
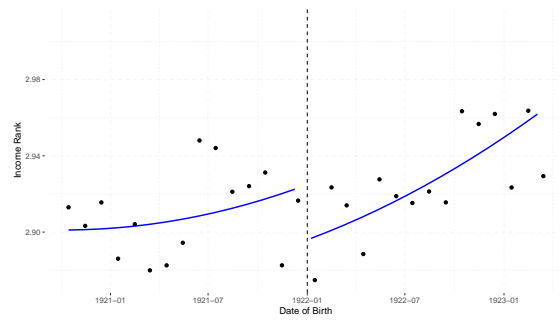


Figure 4: XX

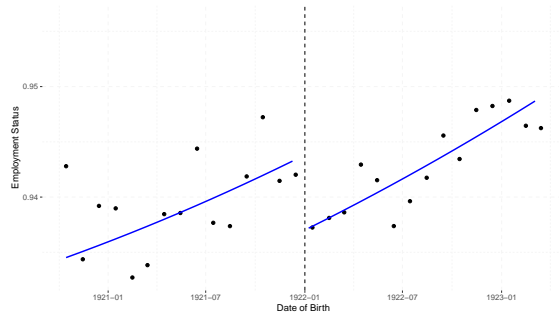
*Notes.* Figure XX depicts XXX.



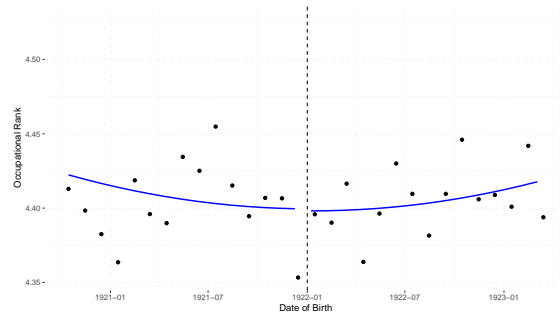
(a) Educational Attainment  
Estimate:  $\beta_1 = -0.041$  (0.0163)\*\*



(b) Income Class  
Estimate:  $\beta_1 = -0.0289$  (0.0126)\*\*



(c) Employment Status  
Estimate:  $\beta_1 = -0.0068$  (0.0021)\*\*\*



(d) Occupational Rank  
Estimate:  $\beta_1 = -0.0013$  (0.018)

Figure 5: Labor Market Outcomes

*Notes.* Panel a depicts XXX.

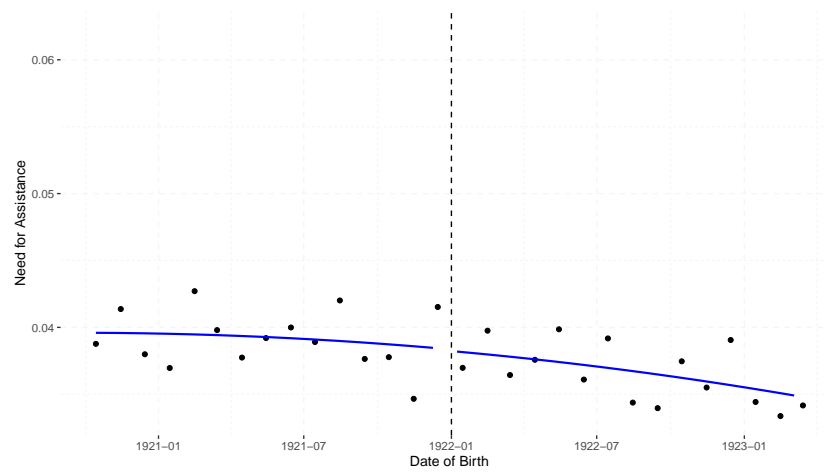


Figure 6: RDD Results: Need for Assistance

Estimate:  $\beta_1 = -0.0001$  (0.0017)



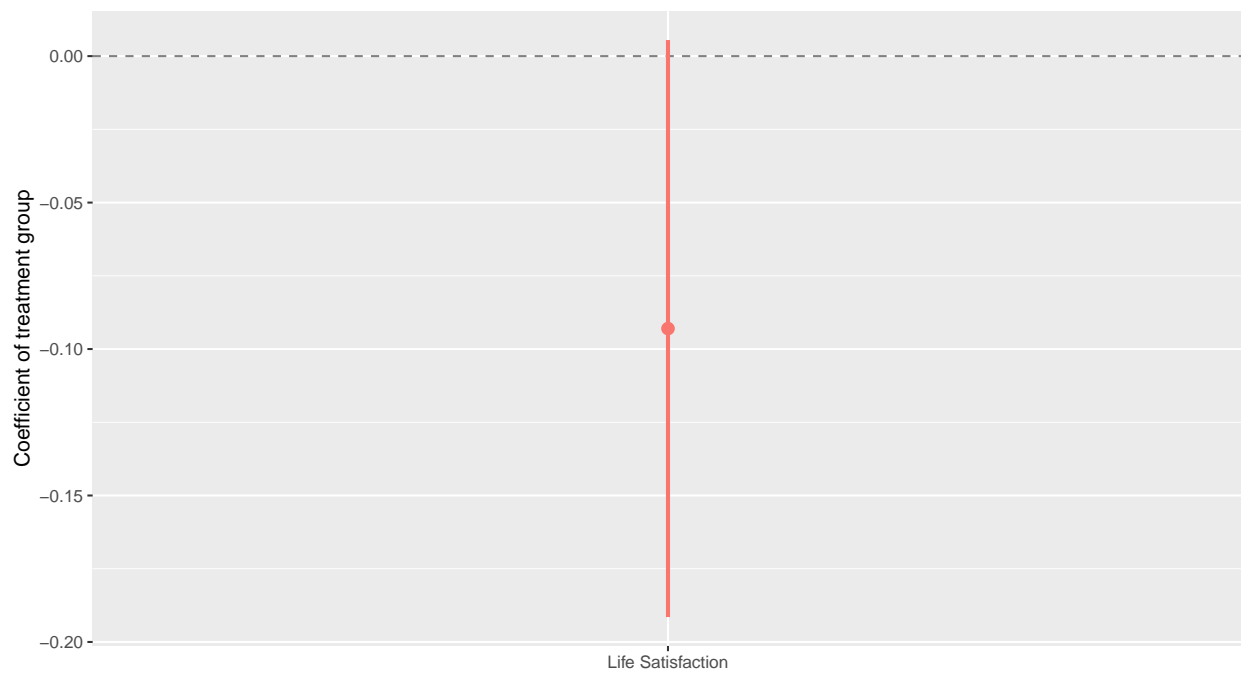


Figure 7: XX

*Notes.* Figure depicts XXX.

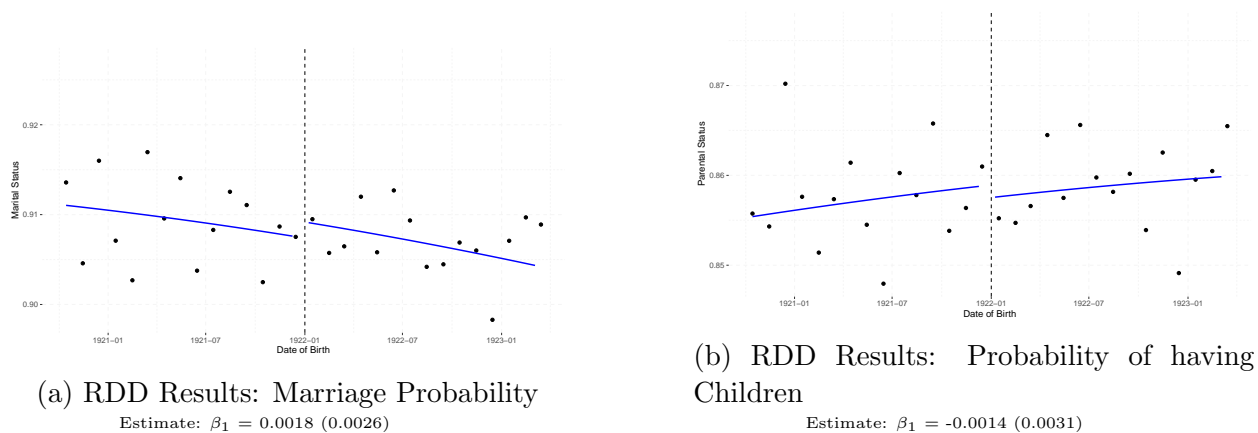


Figure 8: RDD Results: Marital and Parental Status

*Notes.* Panel a depicts XXX.

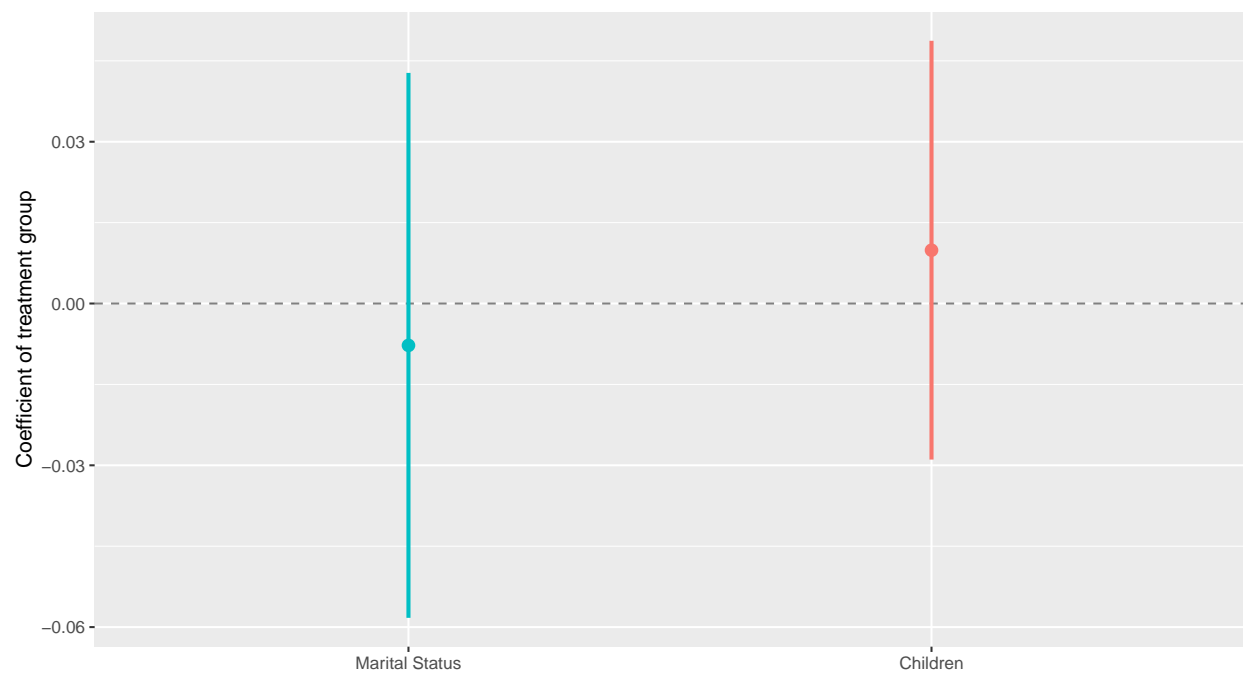
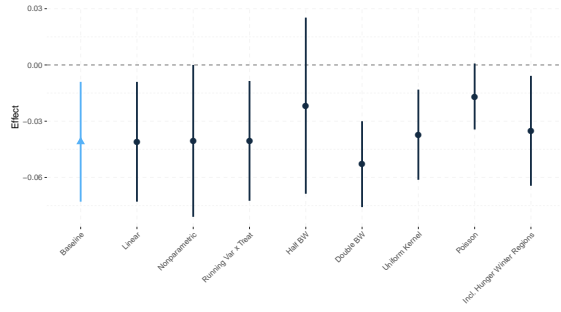
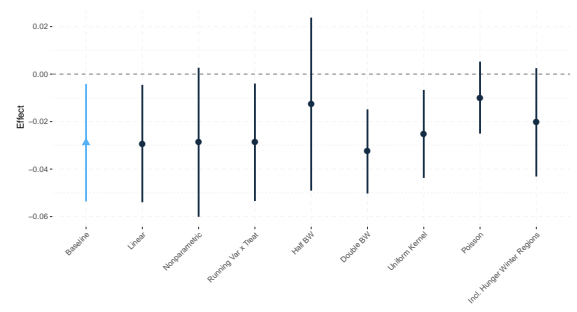


Figure 9: XX

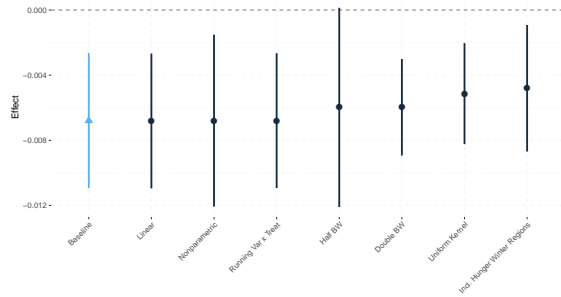
*Notes.* Figure depicts XXX.



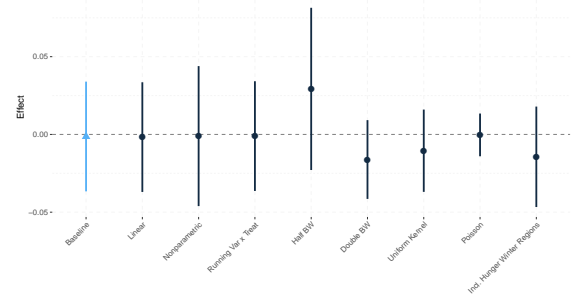
(a) Educational Level



(b) Income Class



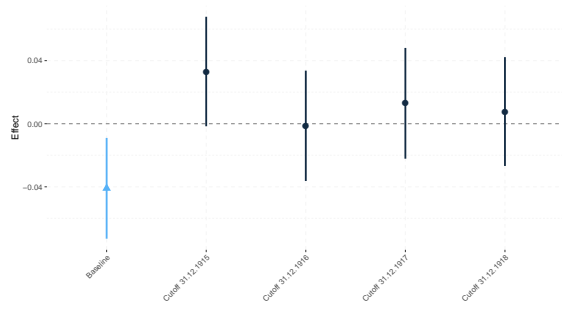
(c) Employment Status



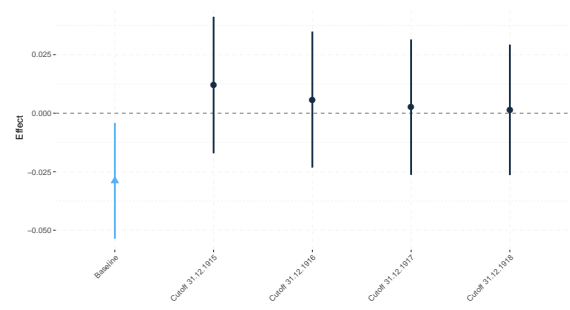
(d) Occupational Rank

Figure 10: Labor Market Outcomes: Robustness

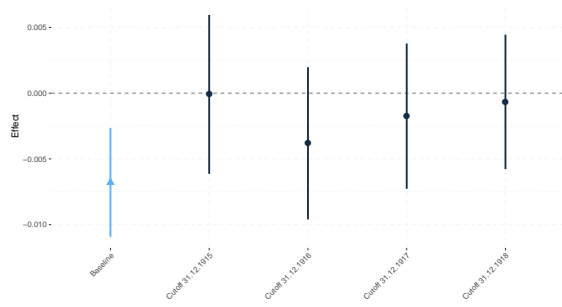
*Notes.* Panel a depicts XXX.



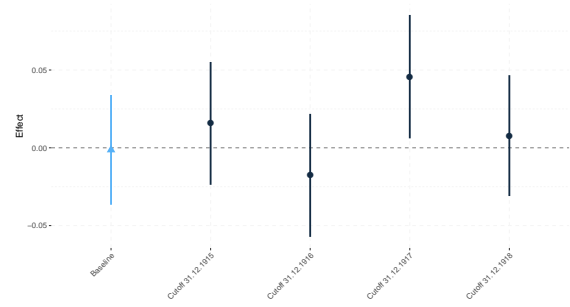
(a) Educational Level



(b) Income Class



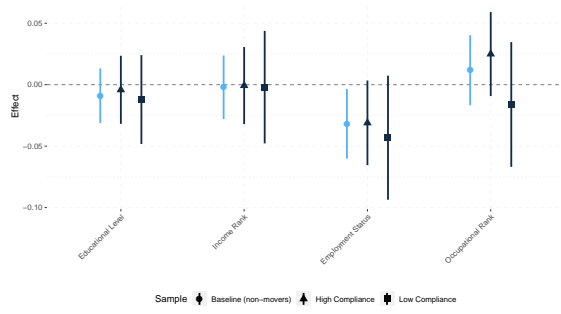
(c) Employment Status



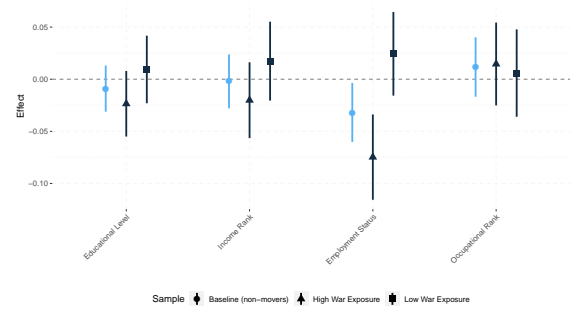
(d) Occupational Rank

Figure 11: Labor Market Outcomes: Placebo

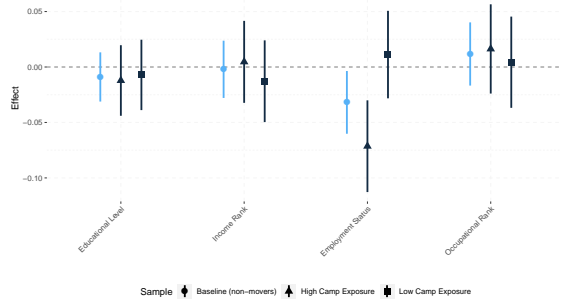
*Notes.* Panel a depicts XXX.



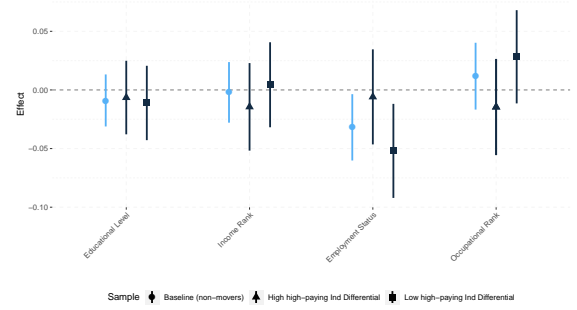
(a) Heterogeneity by Compliance



(b) Heterogeneity by War Exposure in DE



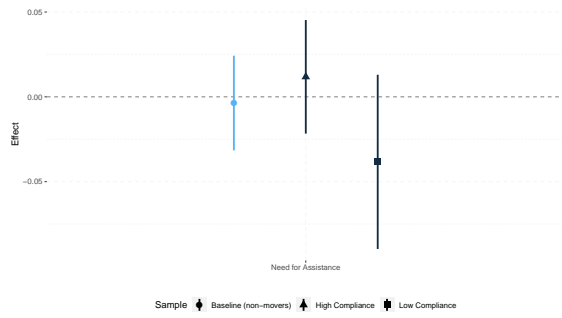
(c) Heterogeneity by Camp Exposure in DE



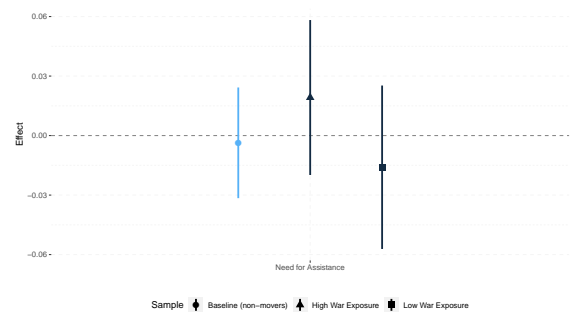
(d) Heterogeneity by Difference in Industry Structure

Figure 12: Labor Market Outcomes: Heterogeneities

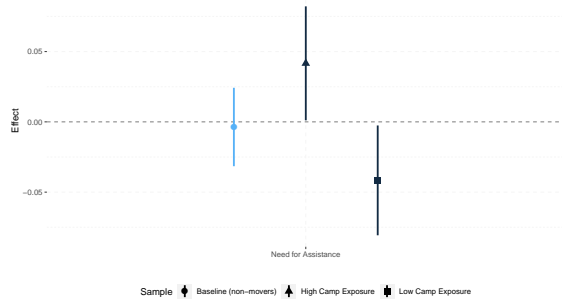
*Notes.* Panel a depicts XXX.



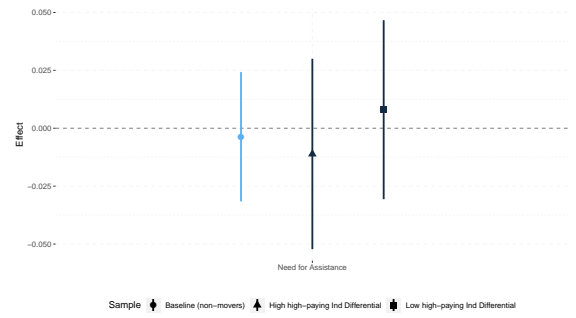
(a) Heterogeneity by Compliance



(b) Heterogeneity by War Exposure in DE



(c) Heterogeneity by Camp Exposure in DE



(d) Heterogeneity by Difference in Industry Structure

Figure 13: Labor Market Outcomes: Heterogeneities

*Notes.* Panel a depicts XXX.

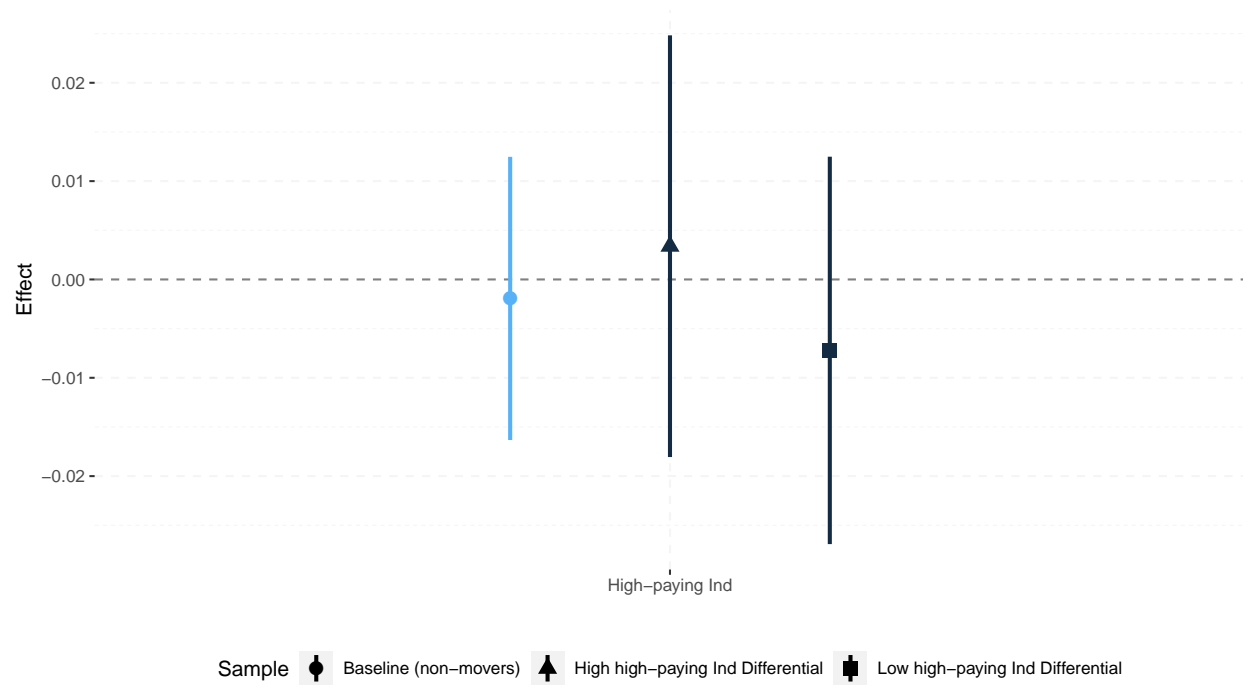


Figure 14: Heterogeneity by Difference in Industry Structure

Notes. XXX depicts XXX.

# 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

	<i>Dependent variable:</i>			
	Educational Level	Income Rank	Employment Status	Occupational Rank
	(1)	(2)	(3)	(4)
RDD Estimate	−0.041** (0.016)	−0.029** (0.013)	−0.007*** (0.002)	−0.001 (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

	<i>Dependent variable:</i>	
	Marital Status	Parental Status
	(1)	(2)
RDD Estimate	0.002 (0.003)	−0.001 (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

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

Variable	Values	Mean	Std
Years of Education	14-22	17.01	3.14
Income	1-12	7.13	3.23
Employment Probability	0-1	0.38	0.49
Occupation Type	1-7	4.17	1.94
Marital Status	0-1	0.84	0.37
Children (Dummy)	0-1	0.10	0.31
Life Satisfaction	0-3	2.23	0.68
Left-Leaning Voting	0-2	0.92	0.88

Table 5: Descriptive Statistics

Table 6: Labor Market Effects – Eurobarometer

	<i>Dependent variable:</i>			
	Years of Education	Income	Employment Probability	Occupation Type
	(1)	(2)	(3)	(4)
treatmentGroup	0.092 (0.252)	0.072 (0.277)	−0.051* (0.029)	−0.327 (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 <sup>2</sup>	0.084	0.187	0.482	0.146
Adjusted R <sup>2</sup>	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

	<i>Dependent variable:</i>		
	Marital Status	Children	Life Satisfaction
	(1)	(2)	(3)
treatmentGroup	−0.008 (0.031)	0.010 (0.024)	−0.093 (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 <sup>2</sup>	0.092	0.218	0.137
Adjusted R <sup>2</sup>	0.014	0.152	0.063
<i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 Standard Errors are clustered at wave level.			

# 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.<sup>44</sup> 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. 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<sup>45</sup>. 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)<sup>46</sup>. 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

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<sup>44</sup>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 exclude month uncertain in my final analysis anyways.

<sup>45</sup>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.

<sup>46</sup>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<sup>47</sup>. 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.

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<sup>47</sup>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)

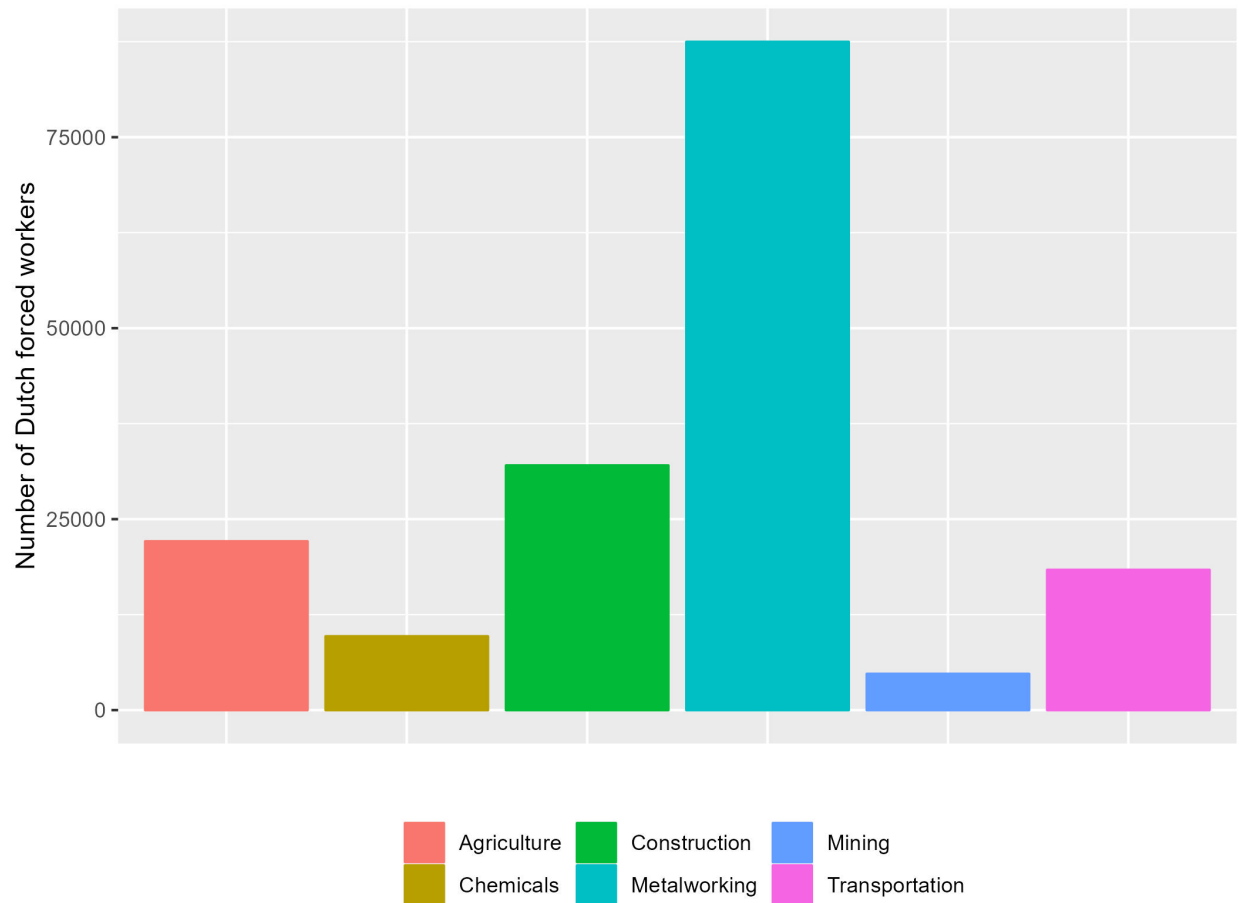


Figure 15: Allocation of Dutch forced workers across industries

*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

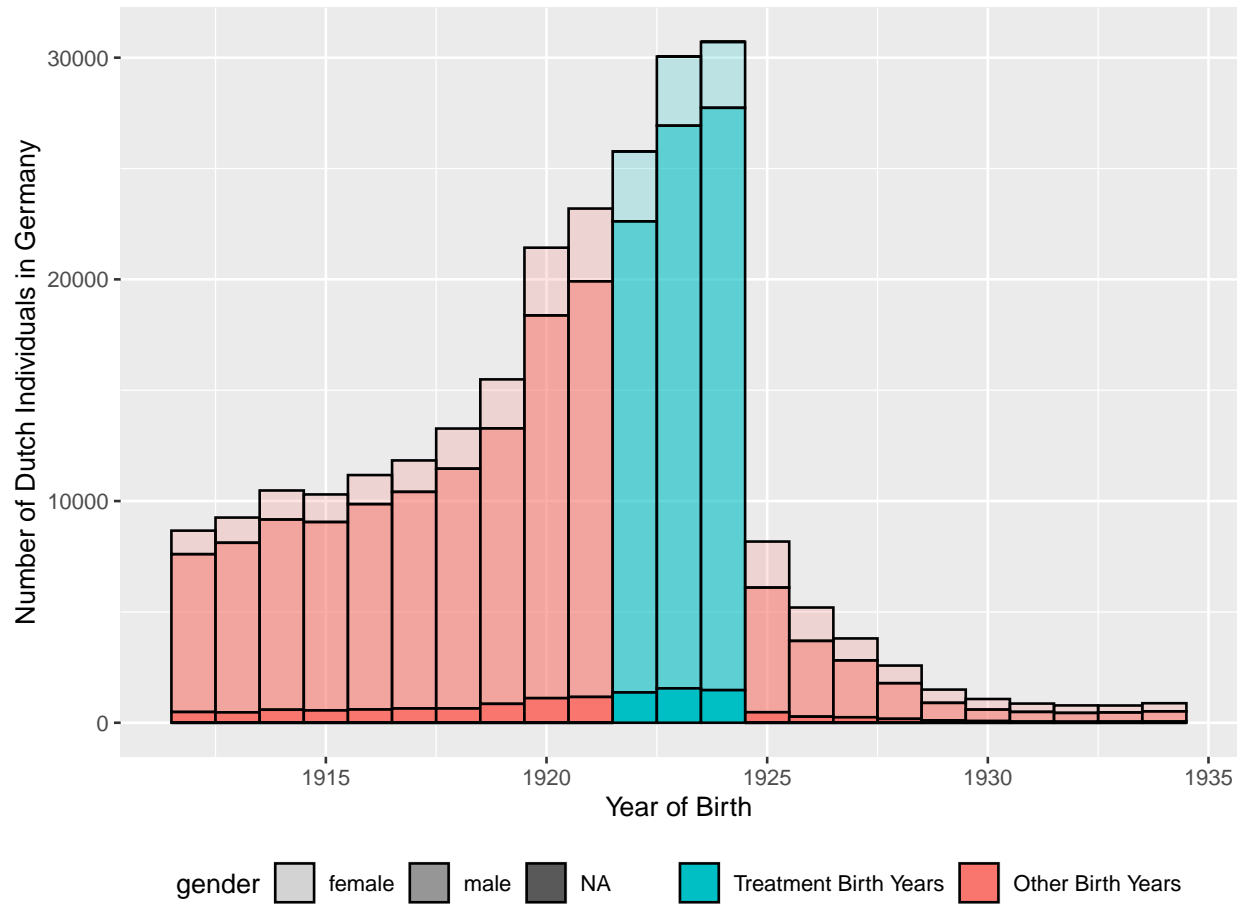


Figure 16: Gender composition of cohorts

*Notes.* The data is based on XXXEXPLAIN.

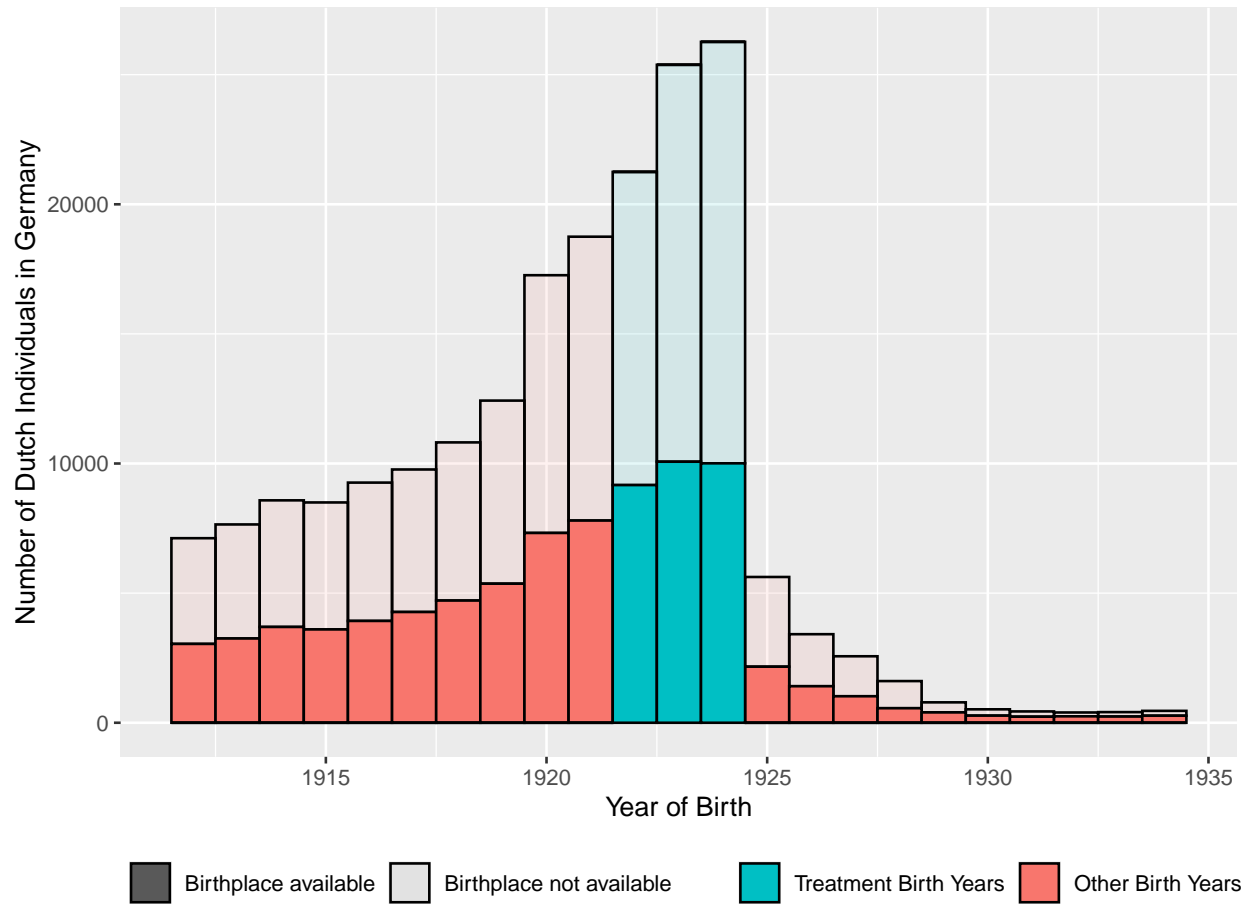


Figure 17: Availability of place of birth of cohorts

*Notes.* The data is based on XXXEXPLAIN.

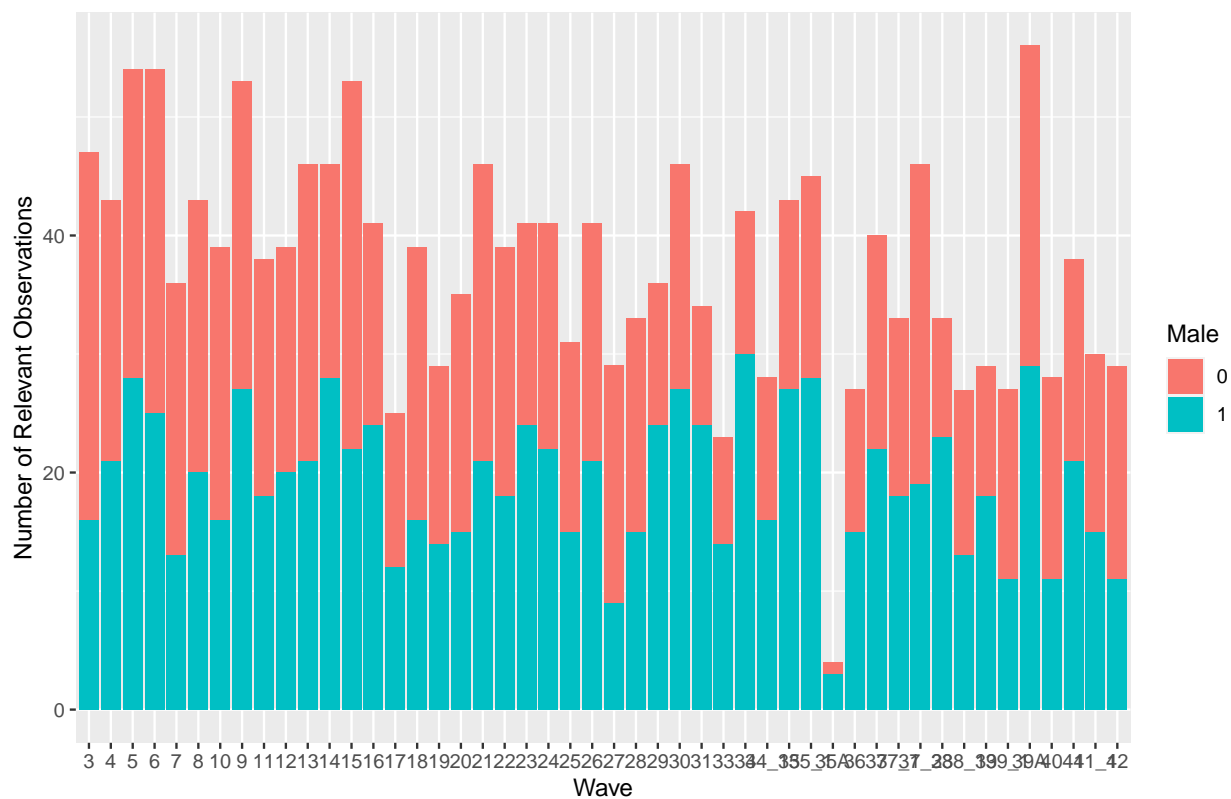


Figure 18: Eurobarometer: Number of observations per wave

*Notes.* The data is based on XXXEXPLAIN.