Worker Displacement and Labor Market Success: Evidence from Forced Labor Conscription during WWII*

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Abstract

Disruptions of labor market trajectories have lasting effects on later economic success. Displacement due to forced labor conscription is a disruption that remains understudied despite its continued prevalence in contemporary contexts. I investigate the consequences of exposure to forced labor conscription for individuals' long-term labor market outcomes. I exploit the fact that cohorts of Dutch civilians faced a differential probability of displacement due to temporary labor coercion in Nazi Germany during WWII in a Regression Discontinuity Design. Using Dutch census data from 1971, I find that conscripted individuals have a lower probability of employment and lower income. I exploit the quasi-exogenous distribution of forced workers in Germany to uncover two contributing factors. First, exposure to harsher conditions in Germany is associated with reduced labor force participation and poorer health. Second, my findings suggest that the negative impact on labor force participation is mitigated when individuals are conscripted to work in sectors that are also present in the Netherlands, which enhances their ability to reintegrate into the workforce.

Keywords: Forced labor; Forced migration; Labor market displacement; Health; Human capital

JEL Codes: J61, J24, J47 J24, J47, N34, N44

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1 Introduction

Disruptions of labor market trajectories can have lasting effects on later economic success.[1] One type of interruption that has received little attention is the displacement due to forced labor conscription, despite it continuing to be a relevant issue in today's economy: According to the International Labor Office (ILO), around 27.6 million people worked in some type of forced labor relationship in 2021 (ILO, 2022).[2] In addition to the violation of human rights, forced labor is problematic from an economic perspective for two main reasons: First, individuals are subject to coercion, threats of violence, and punishments, which may lead to psychological and physical trauma. Second, because of the involuntary nature of work, individuals may have lower incentives for skill acquisition and losses in relevant labor market experience, which in turn can result in lower productivity even after the forced labor experience. Given these issues and the high prevalence of forced labor, it is important to understand the potential consequences of these disruptions. This is inherently difficult for several reasons: First, data is often unavailable and data collection could endanger affected workers (LeBaron, 2018). Second, 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). Third, studying recent experiences of forced labor does not allow for examining long-term consequences.

I study the long-run consequences of facing labor coercion on individual labor market success, based on the historical system of forced labor set up by Germany during World War II (WWII). To avoid endogeneity concerns, I exploit quasi-experimental variation in the assignment into forced labor for Dutch civilians across cohorts in a Regression Discontinuity Design (RDD). I link archival data with micro-level census data for detailed information on the forced labor experience, overcoming issues of data unavailability. My key result is that

[1] See e.g. Angrist (1990); Becker (2022); Braun and Stuhler (2023); Huttunen et al. (2011); Schwank (2024).

[2] Forced labor 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).

by interrupting the labor market trajectories of Dutch adolescents, the German system of forced labor conscription resulted in lower labor market success in the long run.

During WWII, the German government conscripted civilians from occupied countries to mitigate the rising labor shortage caused by the mass conscription of men for military service and the expansion of the armaments industry (Spoerer, 2001). In the Netherlands, in May 1943 the German occupational regime decided to conscript all men born in 1922, 1923, and 1924 (aged 18–21 at the time) for labor in Germany. The coercion was enforced by withholding food ration cards and by prohibiting businesses from employing men born in these years. At least 37% of the men in these cohorts were taken to Germany for forced labor. Anyone who was not granted an exemption was forced to go into hiding.[3] In Germany, forced workers were assigned into sectors irrespective of their previous skills, only based on local labor shortages at the time of their deportation. They worked alongside Germans and were often housed in larger barracks, and the living situation in Germany varied widely. Although most Dutch forced workers were permitted to move about within the local community, they faced harsh punishment in case of disobedience, and experienced worsening access to food and healthcare as Allied bombings intensified. Around 96% of the Dutch workers survived the forced labor experience and returned to the Netherlands after the end of the war (Tooze, 2006).

Using exogenous variation in conscription rates across birth dates, I compare the later income and employment status of individuals in conscripted cohorts to those born before the conscripted years of birth. More formally, I employ a Regression Discontinuity Design using the cutoff of the forced conscription policy at January 1, 1922. I estimate an intention-to-treat effect, in which the treatment of forced labor conscription includes forced migration to Germany for labor as well as being forced to go into hiding to avoid transportation to Germany.[4]

[3] Exemptions were granted for men who were already working in war-related industries, which were around 16% of the cohorts.

[4] I estimate the intention-to-treat effect as there is non-compliance in the control group due to some men born before the conscription cutoff still being forced to work in Germany through e.g. razzias, and some

Using Dutch census data from 1971, when treated individuals were around 49 years old, I find that individuals born in the conscripted cohort show significantly diminished labor market success compared to those born before. Their probability of being employed is 0.68 percentage points lower (2.9% of one standard deviation), and their yearly income is lower by 1% compared to the average income (2.1% of one standard deviation). Compared to papers on other disruptions, the effects are on the lower end of effect sizes: Papers on natural disasters have found employment losses of 0.5 to 4.2 percentage points (Barattieri et al., 2023; Deryugina et al., 2018); the effects of conscription and military combat range from no effects (Bauer et al., 2012; Braun and Stuhler, 2023) to income losses of up to 15% (Angrist, 1990). The long-run income loss of forced labor is of the same magnitude as the long-run effect of a 1 percentage point higher unemployment rate at the time of graduation (Schwandt and von Wachter, 2020). I find marginally significant losses in education, and no differences in family formation, both for marital status as well as the probability of having children. When applying these losses to all conscripted men, there are around 1.430 men missing from the workforce, and the total income loss in 1971 is around 40 million EUR (measured in 2024).[5]

To be able to differentiate between the two types of displacement, either being forced to work in Germany or being forced to go into hiding, I am the first to link archival data on forced workers in Germany to the Dutch census data. The data comes from the Arolsen Archives (Arolsen Archives, 2023) and includes hand-transcribed records on the workers' name, place of birth, date of birth, and their location in Germany. I cleaned the places of birth and merged them with the Dutch municipalities in the census data. I classify an individual's gender using their first name and name frequencies from Meertens Instituut (nd). I link the data by the municipality of birth, restricting the archival records to Dutch male forced workers born in the conscripted cohorts. For each Dutch municipality, I construct a

men born after the conscription cutoff being granted an exception.

[5]The cohort of men born between 1922–1924 consists of 210,565 individuals, and the GDP per capita in 1971 was around 19,000 EUR in 2024 values (van Sonsbeek et al., 2023).

measure of the share of conscripted individuals who went to Germany to identify municipalities with more intensive forced labor (and thus less hiding). I then allow for heterogeneous effects based on this share and find that the effects are similar, implying that the treatment effect is driven by the disruption of the labor market career, independent of whether that constituted being forced to work or being forced to go into hiding.

Exploiting information from the archival records on forced workers' locations in Germany, I show that the negative consequences on later labor market success are driven by individuals who had higher exposure to adverse living conditions while in Germany. I proxy the exposure to adverse living conditions in Germany by the share of houses damaged due to Allied bombings and the distance to so-called labor re-education camps which served as punishment for forced workers. I then assign a measure of average exposure to adverse conditions to each Dutch municipality, based on the German locations where forced workers from each municipality were sent.[6] The results show that the lower probability of being employed is driven solely by conscripted individuals who were exposed to more adverse conditions in Germany, suggesting that the traumatic experiences and the reduced access to medical and nutritional care have a lasting negative and significant effect on labor market participation. To directly test whether this is due to adverse health effects, I then study the consequences of the treatment on physical and mental health. Indeed, the probability of needing assistance in daily life in 1971 is higher for conscripted individuals from places with higher exposure to adverse conditions compared to non-conscripted individuals. I also 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.

In addition, I find that the loss of relevant labor market experience due to forced labor is a relevant factor in explaining the negative consequences of forced labor conscription: I show that the negative effect on labor force participation is mitigated for individuals who were forced to work in sectors similar to the ones in a person's place of origin. Specifically, I allow

[6] As I can only link the archival records to the 1971 census by the place of birth, the analysis uses heterogeneity on the municipality level.

for heterogeneous effects based on the similarity in the sectoral composition of the Dutch municipality of birth compared to the average sectoral composition of German counties where people from that municipality were located while in Germany and find no negative effects for individuals presumably forced to work in similar sectors. This finding suggests that individuals who after the war could continue working in the sectors that they were forced to work in while in Germany had a lower loss of relevant labor market experience.

The results are robust to several checks, including different specifications of the RDD equation, the use of various bandwidths, and different sample restrictions.[7] Additionally, I estimate placebo effects with the cutoff at different years, showing that there are no significant differences in labor market success at these placebo cutoffs.

My paper contributes to the literature on the economic consequences of disruptions of individuals' lives and labor market trajectories. Closely related are studies on the effects of exposure to warfare on health and labor market success (Akbulut-Yuksel et al., 2022; Braun and Stuhler, 2023; Kesternich et al., 2014). I contribute to this literature in several ways: First, I contribute by studying displacement due to forced labor during wartime, which has not been investigated before, despite it affecting around 10–15 million people during WWII alone (Spoerer and Fleischhacker, 2002). Second, I exploit exogenous assignment into displacement, while previous studies have often relied on between-country or within-country variation in exposure to warfare. Third, by exploiting the quasi-random distribution of forced workers into locations and sectors in Germany, I can explore which aspects of the forced labor experience affect labor market success.

This paper also contributes to the literature on forced migration by studying a setting where the displacement was only temporary and the majority of the forced migrants returned to their home country. This mitigates concerns about selection of return migration, and gives insights into the effects of temporary displacement by excluding that effects are driven by the ongoing exposure to conditions in the destination location (Bauer et al., 2013; Bauer

[7] In particular, the results are robust to including individuals from the Dutch Hunger Winter regions, which is an experience that only the control group endured.

et al., 2019; Becker et al., 2020; Becker, 2022; Sarvimäki et al., 2022). While Arellano-Bover (2022) also investigates temporary displacement, specifically that of Japanese Americans during World War II, this paper contributes by studying a setting that combines forced displacement with forced labor. In addition, the age-based conscription in my setting allows for a control group from the same national and ethnic population.

I contribute to the literature on conscription into the military sector by studying how being forced to work in different sectors changes individuals' labor market trajectories (Angrist, 1990; Blattman and Annan, 2010; Bauer et al., 2012; Imbens and Klaauw, 1995). Moreover, by combining archival records with micro-level census data, I can construct estimates for the extent to which conscripted individuals went into hiding, which is usually hard to measure. This allows me to study how an involuntary absence from the labor market affects an individual's labor market success, which relates to previous papers on work displacement (Huttunen et al., 2011; Ichino et al., 2017; Jacobson et al., 1993; Lachowska et al., 2020).

More broadly, this paper is related to the literature on how facing other adverse events affect labor market outcomes, such as natural disasters (Barattieri et al., 2023; Deryugina et al., 2018; Schwank, 2024) and hunger (Ramirez and Haas, 2022).

By showing that missing out on relevant labor market experience in young age has lasting effects on labor market success, my study also contributes to the literature on the consequences of graduating in a recession (Grenet et al., 2024; Kahn, 2010; Oreopoulos et al., 2012; Schwandt and von Wachter, 2020; von Wachter, 2020) and on skills mismatch and career choice more broadly (Brunello and Wruuck, 2021; Gathmann and Schönberg, 2010; Kambourov and Manovskii, 2009). I find that individuals who are employed in sectors that they did not freely choose themselves and that are not relevant for their later career face long-term reductions in their later productivity and labor market outcomes.

By studying the consequences of forced labor on an individual level, my paper also directly contributes to the literature on forced labor. Previous studies have compared regions with more or less intensive use of labor coercion and show persistent negative effects (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). In these settings, it is impossible to distinguish whether the negative effects persist due to forced labor systems shaping local institutions or due to forced labor experience altering individuals' labor market trajectories. In the case of Dutch civilians being coerced into labor in Germany, both the treatment and control groups lived under the same institutions in the Netherlands after the end of WWII and only differed in their exposure to forced labor conscription. I can therefore separate the effects of forced labor conscription on individuals from the effect of forced labor systems on local institutions.

Finally, my paper also adds to the literature on the forced labor regime by Germany during WWII by being the first study to empirically evaluate the long-term consequences for former forced workers. While there is previous research on this forced labor system from a historical perspective (Herbert, 1999; Pfahlmann, 1968; Homze, 1967; Sijes, 1966; Spoerer, 2001; Spoerer and Fleischhacker, 2002), its economic aspects, and specifically the consequences of forced labor for individuals' labor market success, have not been studied before.

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). Due to organizational considerations and since it was more efficient to produce in Germany than in the occupied countries, most of these civilians were transported to Germany to work there (Tooze, 2006).

In the Netherlands, the occupying regime announced in May 1943 that they would conscript all men of specific age groups for work in Germany (the so-called *Yearclass Action*). In June 1943, the cohort of men born in 1924 was the first to be transported to Germany, and in August the cohorts of 1923 and 1922 followed (see figure 26 in the appendix for the posters calling up the cohorts). The Yearclass Action was initially scheduled to include all men born between 1908 and 1925, but due to concerns of turmoil in the Dutch population because of the unpopularity of the conscription of age groups, the other birth cohorts were ultimately not called upon for forced labor. Coercion was executed by withholding food ration cards and forbidding firms from employing men from these cohorts.

In total, at least 77,200 Dutch forced workers born between 1922 and 1924 were recorded in Germany during WWII in the data by the Arolsen Archives. Comparing that to a total of around 210,500 men born in these years according to the 1971 census, compliance was at least around 37%. Men already working in war-related industries were granted exemptions, which applied to 16% for those born in 1922, 19% for those born in 1923, and 17% for those born in 1924 (Hauptabteilung Soziale Verwaltung, 1943; Sijes, 1966). The remaining men went into hiding, often with the help of the resistance, who forged food ration cards and helped with the placement of men of the conscripted age groups into hiding locations (Sijes, 1966; Warmbrunn, 1972). The situations in hiding varied, but often men hid in locations away from their homes, with limited contact to their social networks to avoid being found (Warmbrunn, 1972). Figure 2 shows the exogenous variation that I use in this study: Men from the conscripted cohort were more likely to be forced to work in Germany than men born before the cutoff, and only men from the conscripted cohort were forced to systematically go into hiding until the end of the war to avoid forced labor. The share of forced workers is derived by comparing the sample size of men in the Arolsen archival records to the sample size of men in the census data from 1971. The share of men who went into hiding is derived by subtracting the share of men who were granted an exemption, which is taken from German records from 1943 and available separately for each cohort. This approach results in similar shares of forced workers and men in hiding in the conscripted cohorts, which is supported by historical sources (Klemann, 2001; Warmbrunn, 1972).[8]

The conscripted individuals who were taken to Germany were distributed irrespective of their skills or previous training, and over large parts of Germany. Figure 1 shows the regional distribution of Dutch male forced workers born in the conscripted years 1922–1924 over the German counties. The men were assigned quasi-randomly to the specific locations and industries, based on local labor shortages at the time of their deportation that companies reported to their local *Arbeitsamt* (employment office). The administrative effort of recording previous skills and training and assigning workers based on that was deemed too costly (Kuck, 2010; Marx, 2019). The majority of the Dutch forced workers were employed in manufacturing and construction, and the pay was lower than that for German workers (Herbert, 1999; Sijes, 1966; Tooze, 2006).

The living conditions varied widely, as firms were responsible for housing and feeding the forced workers (Althausen, 1999). Most Dutch workers were housed together in barrack camps or repurposed public buildings. Food supply and nutrition were often of low quality, access to medical care was scarce or non-existent, and both deteriorated as the bombing of the Allied forces intensified. Forced workers were also tasked with clean-up after bombing attacks (Sijes, 1966). In case of any so-called nondisciplinary conduct such as sabotage or absenteeism, forced workers were sentenced to stays in so-called *Arbeitserziehungslager* (labor re-education camps) for several weeks, where conditions were similar to those in concentration camps (Lofti, 2000). While the forced workers were mostly promised yearly contracts at deportation, the majority of workers were not allowed to leave after their contract ended and had to stay until the end of the war (Beening, 2003). When workers tried to flee to return to the Netherlands, they faced a sentence to labor education camps and then being

[8] The individuals who were forced workers in Germany but born before the conscripted cohorts were recruited through different measures such as the recruitment of unemployed or raids. While these raids were mostly targeted at men from the conscripted cohorts who went into hiding, sometimes men were rounded up indiscriminately (Sijes, 1966).

brought back to Germany (Kuck, 2010).

The estimates for the share of Dutch forced workers who died in Germany range between 0.9% and 6.4%, meaning that the majority survived the coercion (Beening, 2003; CBS, 1947; Spoerer, 2001; Warmbrunn, 1972).[9] After the successive liberation of Germany in 1945, the Allied Forces organized the transport of former forced workers back to their home countries. By September 1945, 98% of all Dutch persons present in Germany at the end of the war had returned to the Netherlands (Grüter and Mourik, 2020; Proudfoot, 1957). Figure 3 also shows that the number of men born before and after the cutoff of conscription is not different in 1971, confirming no significant differences in survival probability and no gap of conscripted men who did not return. On 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 laborers began in the Netherlands (Kuck, 2010).[10]

Both the treatment and the control group experienced the war, and the Dutch economy which the control group was exposed to 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). In 1940, only 4% percent attended higher education, and universities mostly stopped operating from 1943 onwards, so neither the control nor the treatment group had access to higher education during the period of conscription, which lasted from May 1943 until the end of the war in 1945 (Van Eden, 1946; Warmbrunn, 1972).

One experience that only the control group faced which may affect later labor market outcomes is the Dutch hunger winter, which took place between November 1944 and May 1945 in urban regions in the West (de Zwarte, 2020). While the most directly affected groups

[9] The reported numbers of Dutch forced workers who died in Germany range from 5,000 to 29,000. Compared to the estimated total number of Dutch forced workers ranging from 450,000 to 530,000, this puts the estimated mortality rate between 0.9% and 6.4%.

[10] The first book interviewing former forced workers about their experiences was published in April of 2024 (Krimp-Schraven, 2024).

were infants and older people, Ramirez and Haas (2022) show that the Hunger Winter had negative effects on education for adolescents of up to 14 years old (which is when their sample ends). I therefore exclude individuals from areas affected by the hunger winter in my baseline sample to abstract from any possible differences driven by the hunger experience.[11]

In 2000, the German government set up a fund to pay compensation to former forced workers of occupied countries. Depending on the adversity of the treatment, individuals were paid between 572 EUR and 7,760 EUR depending on the adversity of their forced labor experience. However, forced workers from Western countries were excluded from this compensation program because of the limited sum of the compensation program and a "lack of deportation and discriminating living conditions", except for individuals who had been working in a concentration camp (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). Thus, only 4,500 of the around 500,000 former Dutch forced workers received compensation through this program.

3 Data

3.1 Dutch Census Data

To estimate the consequences of being conscripted into the forced labor system, I use individual-level admin data from the 1971 census (*14de Algemene Volkstelling*) which is a comprehensive census of the Dutch population (Centraal Bureau voor Statistiek, 2004).[12] To identify the treatment and control group, I use the individuals' gender, month of birth, year of birth, and country of birth. The treatment group is defined as all men born in the Netherlands in the conscription period, so within 1922, 1923, and 1924. The control group is individuals born in the Netherlands within the three years prior to the conscription, so 1921, 1920 and 1919. The individuals are thus between 46 and 52 years old at the time of

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

[12] The non-response rate was 0.2%.

the census. I further restrict the sample to individuals born outside of the municipalities that were affected by the Dutch Hunger Winter following Conti et al. (2024).[13] The sample then includes 356,681 observations.

To measure labor market success, I use a dummy variable of whether a person is employed, and yearly labor income reported in 6 different income bins with a range of 4,000 Dutch Guilder (2,916 EUR in 2024). I also use a dummy variable for finishing secondary education, as well as the marital status and a dummy for whether an individual has children as a proxy for their social situation.[14] Additionally, I use a person's need for assistance in everyday life as a proxy for their health. Table 1 shows the descriptive statistics of these variables.[15]

The 1971 census only includes the municipality of birth for individuals who still live in the same municipality (excluding temporary absences such as war-related reasons). This is the case for 56% of the sample. When linking other data sources based on the place of birth, I further restrict my analysis to these individuals who still live in their municipality of birth. This reduces the sample to 145,286 observations. Note that still living in the place of birth is not affected by the forced labor conscription (see figure 12).

3.2 Individual Archival Records

To disentangle the bundled treatment of being forced to work in Germany and being forced to go into hiding, and to investigate heterogeneities based on the forced labor experience in Germany, I supplement the data with archival records on forced workers during WWII provided by the Arolsen Archives (Arolsen Archives, 2023). The archive evolved from the International Tracing Service (ITS) established by the Allied forces, and its aim is to document and trace victims of the Nazi regime. The majority of the data originates from registration efforts by the Allied forces after WWII to organize the transport of the displaced persons back to their country of origin. I use data on so-called displaced persons, who are defined as

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

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

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

individuals who had been deported by the Nazi regime (Höschler and Panek, 2019). While these do include prisoners of war and former inmates of concentration camps, the vast majority of them are forced workers.[16] The number of unique Dutch individuals in the archival data of around 473,000 also matches the historical estimates of Dutch forced workers of somewhere between 450,000 and 530,000 (CBS, 1947; Spoerer, 2001). I therefore will assume that all individuals in this dataset are forced workers.

The data includes information on the full name, date of birth, location of birth, and the county where the person was located while in Germany. The original sample consists of 594,967 observations. Some individuals show up more than once in the data because multiple sources have been aggregated for the archival records. I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another and exclude the double-counting of individuals. I follow Abramitzky et al. (2021) and adjust their algorithm slightly to exploit the data structure of the archival records.[17] This reduces my sample to 473,406 individuals.

To restrict the archival records to male individuals, I exploit the information on a person's first name to classify their gender. I use information on name frequency by gender from the Corpus of First Names in the Netherlands published by the Meertens Institut to construct a measure of how likely a name belongs to either gender (Meertens Instituut, nd).[18] For the relevant cohorts of 1922–1924, there are 84.2% male and 10.7% female individuals.[19] I restrict the sample to unique male individuals from the cohorts of 1922, 1923, and 1924, which reduces the number of observations to 72,898 observations.

I link the archival records to the census data using the place of birth.[20] This information is available for 40.1% of the sample. I use a fuzzy merge and complement it with a list of over 3,500 hand-coded places of birth.[21] I am able to link the place of birth for 82.6% of all

[16] One statistic on Dutch individuals returning from Germany at the end of the war puts the share of forced workers of all Dutch displaced persons at 92.5% (Lagrou, 1999).

[17] See section 5.1 for a detailed description of my approach.

[18] See section 5.1 for a detailed description of my approach.

[19] For the remaining 5.1%, the first name was not unambigiuously male or female.

[20] Linking on an individual level is not possible, since the 1971 Census does not include information on the name and exact date of birth.

[21] See section 5.1 for a detailed description of my method.

individuals with that information, and the final sample consists of 24,151 observations.

I calculate the average share of conscripted individuals who were deported to Germany for each Dutch municipality using the following equation:

$$sharefw_m = \frac{forcedworkers_m^{DE}}{conscriptedmen_m^{NL}} \tag{1}$$

which compares the sample size of linked individuals from the archival data to the sample size of conscripted individuals in the census for municipality m. Figure 4 shows the regional distribution of the share of conscripted workers who were deported to Germany. As I was only able to link a subset of the individuals from the archival records, the share constitutes a lower bound.[22]

3.3 Eurobarometer

To investigate the consequences on mental health, I use Eurobarometer survey data which includes a question on self-reported life satisfaction. The Eurobarometer is a survey conducted in all member countries of the European Union and samples 1,000 random individuals per country in every survey round. I use all Eurobarometer survey waves since 1975, when age was first recorded, until 1994, when the youngest individuals in the potential treatment group would be 70 years old (Kommission Der Europäischen Gemeinschaften, 2012).[23] Since I only know an individual's age and not their exact date of birth, I restrict the analysis to individuals for whom I know for certain that they are in the control or treatment cohort.[24] The variable for mental health is the subjective life satisfaction (ranging from 0 to 3).[25] I

[22]Municipalities with fewer than 10 conscripted men are coded as missing in this map because of data protection concerns.

[23]This includes waves three through 42 and amounts to 50 waves in total.

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

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

also repeat the analysis on labor market success to ensure the validity of the Eurobarometer survey data, using a dummy for whether a person is employed, and their labor income (reported in 12 different income bins with a range of 250 Dutch Guilder (182 EUR in 2024). Table 2 shows the descriptive statistics of the variables of interest.

3.4 Forced Labor Experience

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

3.4.1 Exposure to adverse conditions

I proxy adverse conditions in Germany by two measures: First, I use exposure to Allied bombings. The intuition is that forced workers suffered most in areas with lots of bombings, as shelters were often reserved for German citizens. Additionally, forced workers were often forced to clean up after the bombings. Second, I use the distance to so-called labor education camps, to which forced laborers were sentenced in case of disobedience. The idea is that forced workers located close to a labor education camp faced a higher probability of being sentenced to a stay in such a camp, as these stays were temporary and the forced workers usually returned to their former occupation after the sentence ended.

The data on war exposure comes from Peters (2022) and measures the share of houses damaged during the war by Allied bombings. This data is available for West Germany, where also the majority of Dutch forced workers were located (see figure 1). Figure 20a in the appendix shows the share of houses damaged in the German counties. The locations of labor education camps come from a map by Lofti (2000) which I geocoded. Figure 20b in the appendix shows the locations of these labor education camps.

3.4.2 Loss in labor market experience

To understand whether a loss in labor market experience affects the consequences of the forced labor experience on labor market success, I need a measure for the difference in the industry structure in Germany compared to that in a person's place of origin. The idea is that labor market experience, albeit involuntary, in a sector that a person could potentially keep working in after their return to the Netherlands may be somewhat useful, while experience in a sector that is not present in a person's place of origin is not transferable and therefore results in a larger loss in useful labor market experience.

I thus first need information on the type of occupation that forced workers were coerced into while in Germany. I use data on the local industry structure in German counties in 1939 taken from Braun and Franke (2021) and use the employment share of each sector as a proxy for the probability with which forced workers located in these counties worked in the respective sector. The data distinguishes between 28 different sectors.[26] The two underlying assumptions are that the industry structure of 1939 is similar to that of 1943–1945, when the Dutch forced workers were present in Germany, and that forced workers were appointed to sectors according to the local employment shares. As the German economy was directed towards wartime preparation already in 1936, and forced workers were used to substitute for German men who were missing from the local economy, both assumptions are reasonable (Treue, 1955). Figure 21 in the appendix shows the distribution of the average employment share in agriculture, industry, and services over the German counties.

Secondly, I use data on the industry structure in Dutch municipalities taken from the occupational census of 1930, which distinguishes between over 400 different sectors (CBS, 1934). I re-classify these sectors to match the sectors present in the German data. I end up with 27 different sectors that can be identified in both data. Figure 23 in the appendix shows the regional distribution of the employment share in agriculture, industry, and services.[27]

[26] I thank Sebastian Braun and Richard Franke for giving me access to data containing this more fine-grained sectoral variation.

[27] The data is available separately for 42 Dutch regions.

In the analysis, I restrict the sample to individuals still living in the municipality they were born in, thus ensuring that the industry structure in that municipality is relevant for former forced workers upon their return after WWII. The underlying assumption is that the industry structure after WWII was similar to that in 1930.

4 Empirical Strategy and Results

4.1 Labor Market Outcomes

4.1.1 Empirical Strategy

One challenge when identifying causal effects of disruptions on later labor market outcomes is to find a suitable control group, which could have also been subject to the disruption, but, for reasons exogenous to their labor market performance, did not share this disruption in their labor market career. Especially when focusing on forced labor conscription as a disruption, certain particularly vulnerable groups of people are faced with coercion to enter such a forced labor "employment" (ILO, 2022), and this vulnerability could possibly translate into different labor market outcomes, regardless of the forced labor experience. Using the historical setting of the forced labor regime in WWII as a natural experiment allows me to avoid this endogeneity concern. While all years of the cohorts of 1908–1925 were considered for conscription through the Yearclass action, only the cohorts of 1922–1924 were actually conscripted. Thus, individuals born before the cutoff of January 1, 1922, pose a suitable control group: They were deemed as suitable for forced labor as the actually conscripted cohorts, and the reason that they were not conscripted was due to political considerations not due to differences in any underlying characteristics of the cohorts themselves that may also affect labor market outcomes.

I exploit the exogenous assignment into forced labor based on an individual's date of birth by using a fuzzy Regression Discontinuity Design with year and month of birth as the running variable and compare individuals born just within the conscription period (in or after January of 1922) to those born just outside of the conscription period (before January of 1922).[28]

The main identifying assumption is that individuals born after the cutoff are similar to those born before and that labor market success would be smooth at the cutoff in the absence of treatment. This is a reasonable assumption if there are no other discrete changes at the cutoff that could potentially affect labor market success (Cattaneo et al., 2019).[29] To the best of my knowledge, there were no other policies that changed discontinuously at the cutoff date of the conscription policy (January 1, 1922). The cutoff for school enrollment was mid-year (Richardson, 2000), and the limited conscription into the military during WWII was not based on age (Sijes, 1966).[30] The oldest cohort conscripted for the Indonesian War of Independence in 1946 was the one of 1925 (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022).[31] Both the treatment and control group were subject to the war. The control group was more likely to experience the Dutch Hunger Winter in 1944–1945, which is why I exclude men from municipalities affected by the Hunger Winter in my baseline specification.[32]

The second identifying assumption of RDD is that individuals cannot manipulate the

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

[29] I estimate placebo regressions with the cutoff of January 1st of different years to provide suggestive evidence that in the absence of treatment, control and treatment group would not have differed in their labor market success. Lacking pre-treatment individual-level data, I cannot check for continuity of labor market success at the cutoff prior to the treatment. Also, any covariates included in the 1971 census may have been affected by forced labor conscription and thus are not suitable for checking smoothness at the cutoff.

[30] 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 the *Wehrmacht* to build coastal defense constructions in 1944, but not based on their date of birth (Sijes, 1966).

[31] Around half of this cohort were drafted. Individuals born after the upper cutoff of the forced labor conscription period during WWII (December 31, 1924) are thus affected differentially by this conscription into the Indonesian War of Independence, which is why I focus on the lower cutoff (January 1, 1922).

[32] I perform a robustness check where I include these men and find that this decision does not drive the results.

running variable and thereby induce endogenous sorting around the cutoff. A person's date of birth is generally exogenous, and even if some conscripted men forged their documents to avoid conscription by changing their date of birth, it is unlikely that the false date would still be reported in their administrative records in 1971. Figure 3 shows the density of date of birth in the 1971 census. The distribution is flat and there seems to be no discontinuous bunching left and right of the cutoff of the conscription period. This also alleviates concerns on differential survival probability of treatment and control group and thus sample selection issues, as at least when treated individuals were aged around 49 years old, there are no differences in the number of individuals in either group.

If these assumptions are fulfilled, then any difference in outcomes at the cutoff can be attributed to the treatment effect. In my setting, the treatment of forced labor conscription is a bundle of different experiences: For individuals who were deported to Germany it entails being forcibly moved to another country, then being forced to work in an occupation that they did not freely choose, being subject to harsh living conditions and punishments, and having to hide this traumatic experience due to the associated stigma. In the case of those who went into hiding, the treatment consists of having to leave their known environment without contact with their usual social environment, living in fear of being found, and often having no formal employment (Warmbrunn, 1972).[33]

Some individuals born within the conscripted years were granted an exemption and had to endure neither forced labor nor going into hiding[34], and some individuals born outside of these years still faced forced labor because they were coerced through other measures than the conscription of birth cohorts. So there are non-compliers with the treatment assignment in both the control and the treatment group. I estimate the reduced form of a Fuzzy RDD, where I exploit that the probability of treatment discontinuously changes at the cutoff of

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

[34]This applied to men working in war-related industries before the conscription.

conscription, using the 1971 census. The estimation equation takes the following form:

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

Y_i are labor market outcomes, specifically employment status and income. $MonthofBirth_i$ is the running variable and c is the cutoff (January 1, 1922). $1\{MonthofBirth_i \geq c\}$ is the indicator for treatment, which is one for treated individuals and zero for the control group. The coefficient β_1 is the intention-to-treat (ITT) effect, which is the effect of being subject to conscription into forced labor, irrespective of actual compliance, compared to individuals who were born outside the conscripted years and were thus less likely to face forced labor and did not have to go into hiding to avoid forced labor. The estimated 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 went into hiding. I include a linear and a quadratic term of the running variable $MonthofBirth_i$ following Gelman and Imbens (2019).[35] I use a bandwidth of 15 months, which is the median of the optimal bandwidths from all labor market outcomes, based on the MSE-optimal bandwidth selection and a triangular kernel as suggested by Cattaneo et al. (2019).[36]

4.1.2 Results

Figure 7 shows the average outcomes for each month of birth and the corresponding function estimated using equation (2), with a bandwidth of 15 months.[37] I find that the probability of being employed is lower by 0.69 percentage points, which is 2.9% of the standard deviation. The yearly income is lower by 0.0289 levels, which corresponds to 2.1% of one standard deviation. Compared to the average income in 1971, this is a reduction of 1% of the mean.

[35] I perform robustness checks using only a linear term of the running variable, and including an interaction term of the running variable and the treatment indicator $1\{MonthofBirth_i \geq c\}$, see figure 8.

[36] I perform robustness checks using different bandwidths and a uniform kernel, see figure 8.

[37] The underlying regression results are shown in table 3.

Both effects are significant at the 5% level. So individuals subject to the labor conscription policy are performing significantly worse on the labor market in terms of their probability of being employed and their income. These effects also translate to significant productivity losses for the Dutch economy on a whole: The cohorts of conscripted men born between 1922 and 1924 consist of 210.565 men. When aggregating the individual losses, there are around 1.430 men missing from the workforce, and the total income loss for the Dutch economy in 1971 is around 40 million EUR (measured in 2024).[38]

How do these effects compare to other disruptions of individuals' working lives? The estimates for conscription into the military vary widely, with some studies finding no effect on earnings (Bauer et al., 2012), while others reporting income reductions ranging from 5% (Imbens and Klaauw, 1995) to 15% (Angrist, 1990). Against this backdrop, my estimated income reduction of approximately 1% of average annual earnings appears moderate. However, note that these papers study conscription in settings where men were drafted while living in peaceful times in their home country. In such cases, conscription into the military may pose a larger disruption compared to the additional hardship due to the forced labor conscription while living through an ongoing war. Braun and Stuhler (2023) study the effect of war experience of conscripted men in Germany during WWII (born 1919-1921), which is more comparable to my setting. They find that war captivity and displacement do not affect employment probability, and only war injuries lower employment probability. The effect becomes negative only once people reach their early 50s, and the magnitude is then around three percentage points. Compared to that, my finding that individuals faced with forced labor conscription are 0.69 percentage points less likely to be employed at age 49 is in a similar range.

Compared to other types of disruptions such as natural disasters or graduating in a recession, my estimates are within a similar range: For instance, studies on natural disasters have found employment losses between 0.5 and 4.2 percentage points (Barattieri et al., 2023;

[38]The cohort of men born between 1922–1924 consists of 210,565 individuals, and the GDP per capita in 1971 was around 19,000 EUR in 2024 values (van Sonsbeek et al., 2023).

Deryugina et al., 2018). Schwandt and von Wachter (2020) find that earnings are lower by 1% when graduating in a labor market with a 1 percentage point higher unemployment rate, which would correspond to the effect of labor conscription.

Finally, my findings probably constitute a lower bound of the costs of conscription, as Dutch forced workers were treated relatively better than forced workers of other nationalities. The consequences for forced workers of other occupied countries are probably larger than the ones that I find for Dutch civilians. As there is non-compliance on both sides of the cutoff, the local average treatment effect for the treated is probably larger than the intention-to-treat effect that I estimate.

The results are robust to a number of different specifications. Figure 8 shows the estimates for regressions with different specifications of the RDD equation: Using only a linear term of the running variable $MonthofBirth_i$, including an interaction term of the running variable and the treatment indicator $1\{MonthofBirth_i \geq c\}$ to allow for different slopes on both sides of the cutoff, using different bandwidths (half and two times of the optimal bandwidth), using a uniform kernel, running nonlinear regressions[39], and including individuals from the Hunger Winter regions. Throughout all regressions, the estimates remain negative, the majority also retaining significance.

To provide further evidence for the main identifying assumption that labor market success would have been smooth at the cutoff in the absence of treatment, I run a placebo exercise where I shift the cutoff of January 1st to different years. In these years, I should not find any significant differences as nothing should have changed discontinuously at the cutoff that affects labor market success. Figure 9 shows the results of this placebo analysis.[40] I find insignificant results with estimates close to zero for all placebo specifications.

One concern for identification is that because men were missing from the workforce in the 1940s due to the conscription, the remaining men of the control group may have benefited,

[39] I run Logit for the dummy dependent variables of being employed.

[40] I estimate this placebo exercise for four years prior to conscription where on both sides of the cutoff, there are three cohorts that only belong to the control group to mirror the three years of conscription period.

which could also explain the negative effects that I find. Note however that by the end of 1940, there was almost no unemployment in the Netherlands (Klemann, 2001). Also in 1971, the unemployment rate was as low as 2% (van Sonsbeek et al., 2023). This means that both at the time of the forced labor conscription (when men were missing from the workforce because of forced labor deportation and going into hiding), as well as at the time when I measure the outcome (when men were missing from the workforce because of the negative treatment effect on employment), this absence of men did not open up large opportunities for an otherwise unemployed control group.

4.2 Further outcomes

So far, I have looked at the consequences of the disruption of forced labor conscription on a person's labor market success. To understand what consequences this disruption has on an individual's education and social life outside of their labor market experience, I look at finishing secondary education as well as the marital status and parental status, i.e. the probability of having children.

Education. Figure 10 shows the average share of individuals who finished secondary education as reported in the 1971 census in a 15-month bandwidth around the cutoff, and the results from equation (2). I find that the probability of finishing secondary education is lower by 0.64 percentage points, which corresponds to 1.6% of one standard deviation. This effect is only significant at the 10% level. The effect is probably driven by treated individuals being less likely to go back to finish their secondary education after WWII, as the conscription started when treated individuals were around 19 years old and had thus already left school.

Family Formation. Figure 11 shows the average share of individuals who are married and have children as reported in the 1971 census in a 15-month bandwidth around the cutoff, and the results from equation (2).[41] There is no discontinuous difference between the treatment and control group at the cutoff for both marital status and the probability of having children, meaning that in 1971, when treated individuals were aged around 49 years old, forced labor conscription does not seem to affect family formation.[42]

4.3 Contributing factors

Until now, I estimated the intention-to-treat effect, where the treatment is a bundle of being forced to work in Germany and being forced to go into hiding. Both types of treatment consist of a variety of different experiences, making it challenging to clearly separate single mechanisms, especially given the long time frame between treatment and outcome of more than 25 years. With these caveat in mind, I still attempt to provide suggestive evidence for contributing factors by combining the census data with archival records on the location and origin of forced workers.

4.3.1 Forced Labor or Hiding

First, I set out to disentangle the two types of treatment, deportation to work Germany or hiding, by conducting a heterogeneity analysis allowing for different effects based on which type of treatment was more likely. I proxy the probability of being deported by calculating the share of conscripted individuals from each Dutch municipality that can be found in the archival records on forced workers in Germany.

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

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

[42] Table 9 shows the full regression results.

the treatment itself affects the probability of a person still living in their municipality of birth, but I do not find any differences (see figure 12).

For each Dutch municipality, I then compare the sample size of linked men from the archival records to the sample size of conscripted men born in the respective municipality to calculate a share of individuals who were forced workers in Germany (equation (3)). Figure 4 shows the regional distribution of the share of forced workers for each Dutch municipality. Because the linking is restricted to observations of the archival data that report a place of birth, this share is a lower bound of the actual share of men who were deported to Germany from each municipality.[43]

There are no apparent no regional patterns in the share of deported men. To test more formally whether the probability of avoiding conscription by going into hiding is related to regional characteristics, I plot the share of deported individuals in each municipality against the employment share in agriculture, industry, and services in figure 25 in the appendix. I find no significant correlation, indicating that it was not the case that men in more industrial or more agricultural areas had an easier time avoiding conscription. Still, since the ability to go into hiding may be in part driven by factors that could also affect labor market outcomes, the results should be interpreted with caution.[44]

I now split the sample by municipalities with a deportation share above and below the median.[45] Individuals in the sub-sample with an above-median deportation share thus have a higher probability for forced labor compared to individuals in the sub-sample with a below-median deportation share. Accordingly, individuals from the first sub-sample have a lower probability of going into hiding compared to individuals from the second sub-sample. I repeat the analysis for labor market outcomes by estimating equation (2), first for the new baseline of observations with a place of birth (thus those who still live in their municipality

[43]I check the probability of having a place of birth based on the date of birth and find that the share is overall flat, not indicating sample selection (see figure 24 in the appendix). I thus assume that the probability of being linked is random.

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

[45]The median share of deported conscripted men is 0.11.

of birth), and then separately for the two sub-samples. The results are displayed in figure 13.

In the new baseline using only individuals who still live in their municipality of birth in 1971, the effect on employment probability is again negative and significant. The effect on income becomes insignificant and close to zero, which could be due to the endogenous sample selection: While the treatment itself does not change the probability of a person of still living in their municipality of birth (see figure 12), it is possible that individuals who return to and stay in their place of birth have stronger social ties, which may alleviate some of the negative effects of the disruption of forced labor conscription, thereby rendering the treatment effects for income small and insignificant.

The effects for the two sub-samples are similar in size and significance to both the baseline effect and to each other, suggesting that the negative effect of the treatment on employment probability comes from both types of disruptions, going into hiding and being forced to work in Germany. In light of the historical background, this is not surprising: Both disruptions led individuals to leave their home and their former workplace. While the deported individuals faced the hardship of forced labor in Germany, the individuals in hiding were potentially even more socially isolated and mostly missed out on any labor market experience during this time.

4.3.2 Exposure to adverse conditions

For the individuals who were forced to go into hiding, I lack additional information on their experiences during the period of hiding to explore the contributing factors. For the disruption in the form of forced labor in Germany, however, I am able to open the black box of what people experienced by exploiting that the archival records include the location of the former forced workers in Germany. I conduct a heterogeneity analysis based on exposure to adverse conditions during the forced labor experience to understand whether exposure to harsher circumstances during the forced labor period may be a contributing factor to the negative effects on labor market success. Allocation to locations in Germany was as good as random since it was decided on in response to ad-hoc demand for labor at the time of deportation. The variation in adversity of the forced labor experience can thus be considered exogenous.

I proxy exposure to adverse conditions by two measures: First, the share of houses damaged due to Allied bombings, as forced workers often lacked access to shelters and had to clean up after bombings. Second, I use the distance to so-called labor education camps as the punishment for workers who disobeyed orders was a temporary stay in such a camp, which had similar conditions to concentration camps. I aggregate each variable to the level of Dutch municipalities by the following equation:

$$adversity_m = \frac{\sum_{c=0}^{C} adversity_c forcedworkers_{c,m}}{\sum_{c=0}^{C} forcedworkers_{c,m}} \tag{3}$$

where m is the Dutch municipality, c is the German county, $forcedworkers_{c,m}$ is the number of forced workers in county c who are born in municipality m,[46] and $adversity_c$ is either the share of damaged houses due to bombings or the distance to the nearest labor education camp for each German county c. The intuition is that this measure captures the average exposure of forced workers from each municipality to adverse conditions in Germany. Figure 5 shows the regional distribution of both measures, with exposure to Allied bombings in panel a and exposure to labor education camps in panel b. The variation does not display any regional patterns, which is in line with the quasi-random distribution of forced workers over German locations.

I then split the sample based on whether an individual is from a municipality with an above or below median exposure to adverse conditions as measured by $adversity_m$ and estimate equation (2) separately for each sample.[47]

[46]The number of forced workers is based on men born in the 15 month after the cutoff, which is the optimal bandwidth.

[47]The median average weighted share of houses damaged in Germany is 0.4, and the median average weighted distance to a labor education camp is 21.8 km.

Figure 14 shows the results, with panel a showing the results for the sample split by exposure to Allied bombings, and panel b showing the results for the sample split by exposure to labor education camps.[48] In both cases, the effects are more negative for individuals from Dutch places where forced workers were more exposed to adverse conditions in Germany. The negative effect of forced labor disruption on the probability of being employed is completely driven by individuals from Dutch municipalities where forced workers were more exposed to more adverse conditions in Germany (both for the share of houses damaged and the distance to labor education camps).[49] This suggests that the adverse living conditions while in Germany are a reason for the continued lower labor market success of former forced workers, possibly because this negatively affected their health.

To test if indeed poorer health due to harsher forced labor conditions is a contributing factor for the lower probability of employment, I repeat the heterogeneity analysis with a direct measure of an individual's health. I proxy physical health by a question in the 1971 census on 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.[50] I find that for the sub-sample with higher exposure to adverse conditions in Germany, the treatment increases the probability of needing assistance, which is in line with the interpretation that an adverse forced labor experience led to worse health (see figure 15). Physical health is also worse for the individuals from the sub-sample with a higher probability of forced labor instead of hiding (see panel a of figure 15), which again suggests that for forced workers, it was their negatively impacted health that drives the lower labor market success.

To not only look at physical health but also mental health as a possible contributing factor, I use Eurobarometer data to study the effect of forced labor conscription on life

[48]As this analysis again relies on individuals who I was able to link to the archival records using their place of birth, the sample is again restricted to individuals with information on their municipality of birth.

[49]The effect for individuals from places with forced workers who were less exposed to bombings is a precise zero. For individuals from places with forced workers who were less exposed to labor education camps, the effect is positive.

[50]Note that not needing help and not answering the question is both coded the same, so the results have to be interpreted with caution.

satisfaction. I estimate a simple difference equation of the intention-to-treat effect as this data only contains individuals' age:[51]

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

where $lifesat_{it}$ is a measure of life satisfaction as the dependent variable[52] and $labmda_t$ are wave fixed effects to control for slightly different ways in which the question was formulated. To ensure the validity of using the Eurobarometer data with this approach, I replicate the results on labor market success and family formation using the same estimation setup. Figures 18 and 19 in the appendix show that the results are overall comparable.[53] As the data lacks information on a person's place of birth, I cannot link it to the archival records to estimate heterogeneous effects by the adversity of the forced labor experience.

Figure 16 suggests that while the effect does not reach significance, there may be a negative effect on life satisfaction for treated individuals, further underlying the finding that poorer health, both physical and mental, may be one reason for the negative effects on labor market success. This interpretation is also supported by a survey of Dutch men born between 1920 to 1929 conducted in 1995, which found that the probability of PTSD (Post Traumatic Stress Disorder) is positively correlated with the probability of having been a forced worker during the war (Bramsen, 1998).[54] These effects were probably exacerbated by the stigmatization of forced workers, as work in Germany during WWII was perceived as collaboration with the enemy by the Dutch public. As a result, most forced workers did not speak about their experiences, possibly intensifying the negative mental health consequences.

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

[52] Life satisfaction is measured from 0 to 3, ranging from not at all satisfied to very satisfied.

[53] See section 5.2 for a detailed discussion of the approach.

[54] The share of individuals with PTSD was 4% for former forced workers vs. 1.5% for other individuals. Note that the survey did not rely on exogenous variation of who became a forced worker.

4.3.3 Loss in labor market experience

In the next step, I want to understand whether the coercion into a job that a person would not have chosen for themselves led to a loss in relevant labor market experience, which in turn may explain the lower labor market success. To do so, I construct a measure of how different the jobs were that forced workers were employed in while in Germany compared to what they would have done otherwise. The idea is that a greater divergence resulted in a larger loss of relevant work experience: If the sector into which these young forced workers were allocated does not exist in their place of origin, this probably made the transfer of any human capital that they acquired during their time in Germany a lot harder.

To measure the difference in occupations in Germany compared to occupations in the Netherlands, I use data on the sectoral composition of both locations. For Germany, I use county-level data from 1939. I link this data to the 1971 census on the municipality level by calculating the weighted average employment share for each industry, based on where forced workers from these municipalities were located in Germany:

$$empshare^{DE}_{m,j} = \frac{\sum_{c=0}^{C} empshare_{c,j} forcedworkers_{c,m}}{\sum_{c=0}^{C} forcedworkers_{c,m}} \tag{5}$$

where m is the Dutch municipality, c is the German county, j is the sector (with the data distinguishing between 27 sectors), $forcedworkers_{c,m}$ is the number of forced workers in county c who are born in municipality m[55], and $empshare_{cj}$ is the employment share of sector j in county c. Figure 22 in the appendix shows the distribution of the average employment share in agriculture, industry, and services over the Dutch municipalities that forced workers were exposed to in Germany. For the type of occupations in the Netherlands, I use data on the sectoral structure of Dutch municipalities in 1930. Figure 23 in the appendix shows the distribution of the employment share in agriculture, industry, and services in the Dutch municipalities.

[55] The number of forced workers is again based on men born in the 15 months after the cutoff, which is the optimal bandwidth.

For each municipality m, I then calculate the correlation between the employment shares that forced workers from that municipality were exposed to while in Germany, $empshare^{DE}_{mj}$, and the employment shares of the respective municipality itself, $empshare^{NL}_{mj}$. Figure 6 shows the regional distribution of the resulting measure of industry similarity. Again, there are no clear regional patterns, as expected due to the quasi-random distribution of forced workers into sectors in Germany.

To allow for heterogeneous effects based on whether forced workers had a lower or higher loss in relevant labor market experience, I split the sample by whether a person is from a municipality with a below-median or above-median value of the correlation between the German and the Dutch industries.[56][57] Figure 17 shows that the negative effect of forced labor conscription on the probability of being employed is more pronounced for persons who were coerced into sectors different from the ones in their place of origin. In other words, being coerced into a sector in which a person could continue working in after their return to the Netherlands alleviated some of the negative consequences of forced labor conscription, probably because of a lower loss in relevant skills and labor market experience.

5 Conclusion

In this paper, I study how a disruption of a labor market career due to forced labor conscription affects later labor market success. I exploit exogenous variation in being exposed to forced labor conscription by studying the case of Dutch civilians during WWII, who were conscripted to work in Germany based on their date of birth. Conscripted individuals had to either go to Germany for forced labor or were forced to go into hiding. Using a Regression Discontinuity Design, I find that individuals who were conscripted into forced labor have lower labor market success when they are around 49 years old, more than 25 years after the conscription.

[56] I again restrict the sample to individuals with information on their place of birth.

[57] The median value is 0.55.

Specifically, I find that individuals who were born after the cutoff of conscription have a lower probability of being employed and lower income, and I show that the negative consequences arise for both individuals who were deported to Germany for forced work, and for individuals who were forced to go into hiding. The negative effects are mostly driven by individuals exposed to harsher living conditions while in Germany. For this group, forced labor conscription is also associated with worse physical health. Suggestive evidence also points towards lower psychological well-being of conscripted individuals. Taken together, this implies that the forced labor conscription had negative consequences for labor market success due to adverse effects on an individual's health and well-being. In addition, I present evidence that being coerced into sectors that a person could continue working in after the war mitigates some of the negative consequences on labor market success, probably due to a lower loss in relevant labor market experience. I find no effect of being conscripted into forced labor on family formation.

In the early 2000s, Germany set up a compensation program for former forced workers. People from Western countries, including the Netherlands, were excluded from receiving compensation, due to a supposed 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 adverse conditions in Germany did suffer from long-lasting effects on their labor market success and their health. The consequences that I find constitute only a lower bound of the social costs of the forced labor conscription by Germany during World War II in a more generalized sense because compared to forced workers of other nationalities, Dutch forced workers were still treated relatively better.

When applying my findings to contexts beyond the forced labor regime of Nazi Germany, a key policy implication is a need to provide adequate support to former forced workers upon their return to their home countries to avoid that the possibly traumatic experiences lead to long-term disadvantages in the labor market. In the Netherlands, the former forced workers faced suspicion of collaboration with the enemy, leading them to remain silent about their experiences. This may have prevented affected individuals from seeking help, thus exacerbating and perpetuating their losses in the labor market. Additionally, it may be beneficial to understand which skills former forced workers might have acquired during their forced labor experience, as I show that the negative consequences are less pronounced for people who gained more relevant labor market experience as forced workers. Encouraging the application of these skills in future work settings may help lessen the negative consequences of forced labor in the medium and long run.

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Figures

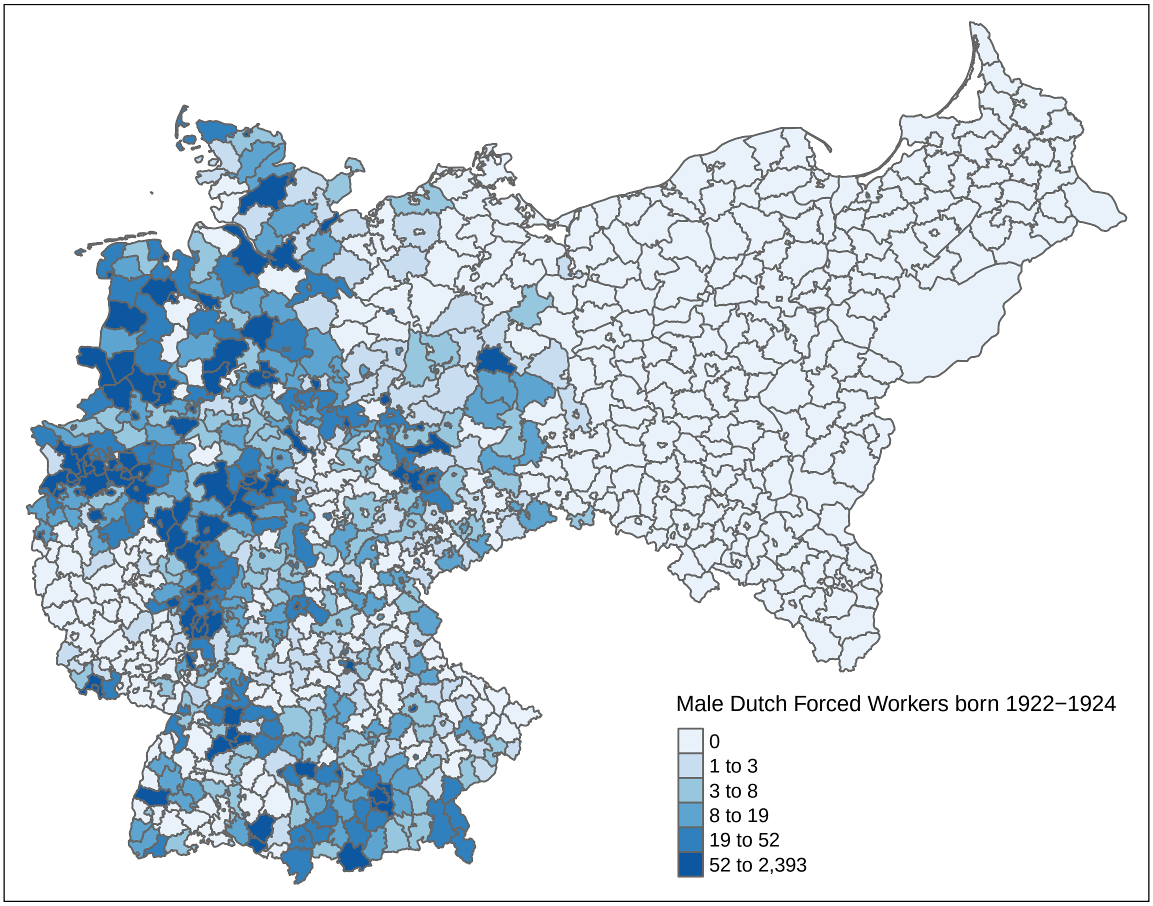


Figure 1: Regional distribution of male Dutch forced workers born 1922-1924 across German counties

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

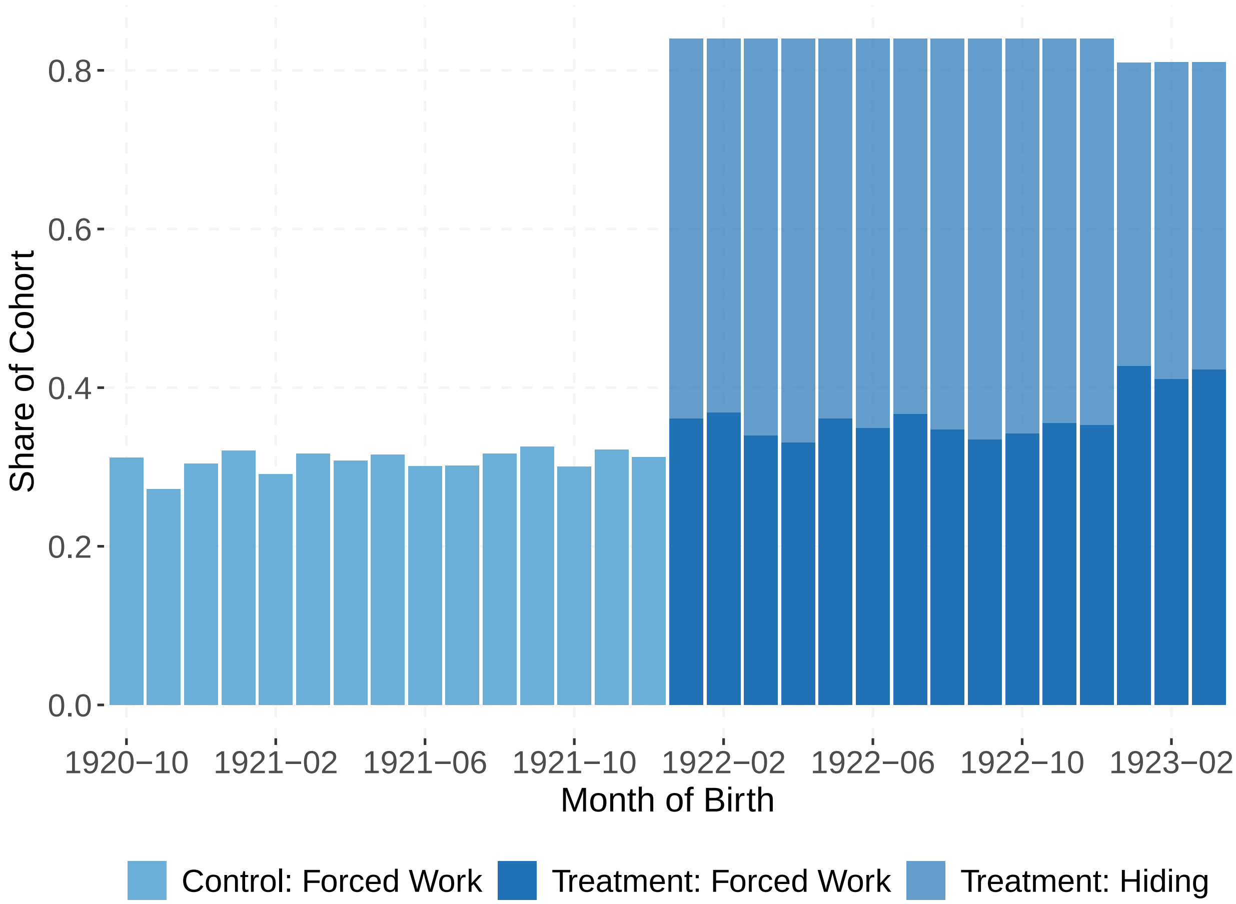


Figure 2: Number of treated individuals

Notes. This figure shows the number of male treated individuals for each month and year of birth in the 1971 Census in a 15-month bandwidth around the cutoff of treatment, January 1, 1922. The number of forced workers is derived by comparing the sample size in the Arolsen archival records to the sample size in the census data 1971. The share of men who went into hiding is derived by subtracting the share of men who were granted an exemption taken from German records from 1943, which is available separately for each cohort.

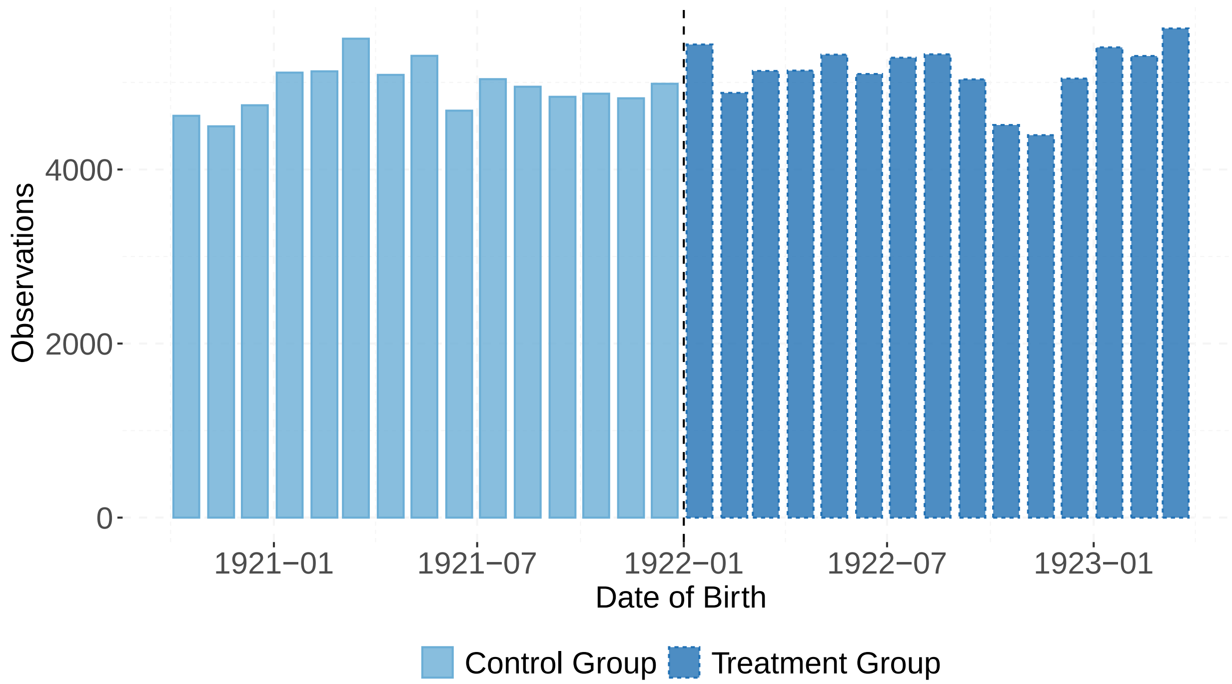


Figure 3: Number of male observations per month and year of birth based on the 1971 Census

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

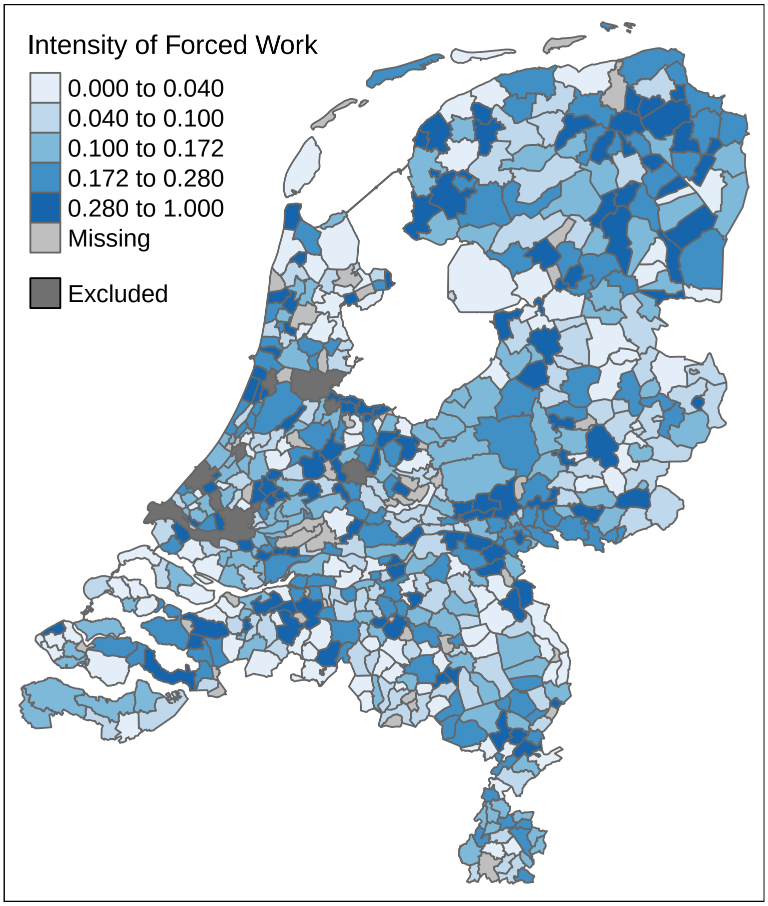
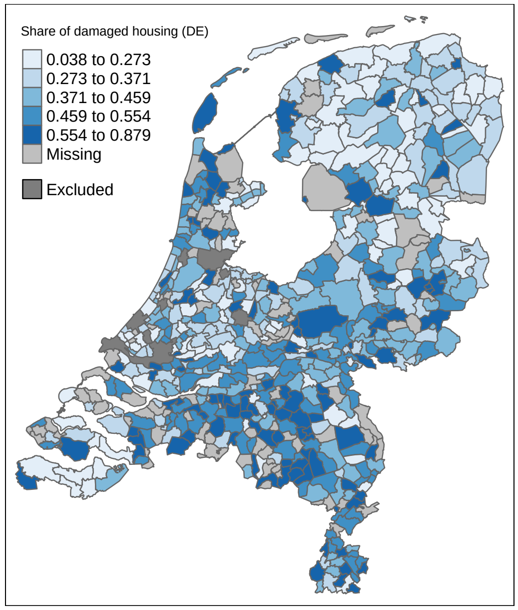
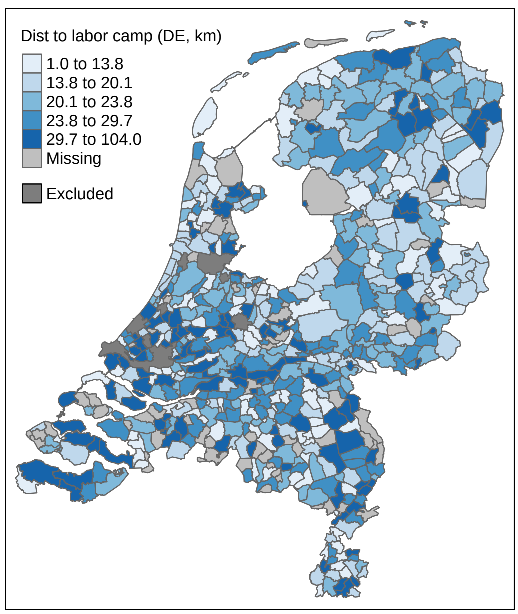


Figure 4: Regional distribution of intensity of forced labor over Dutch municipalities

Notes. This figure shows the intensity of forced labor for each Dutch municipality, measured as the number of male forced workers born within a 15-month bandwidth after the conscription cutoff of January 1, 1922 from Arolsen Archives whose place of birth can be linked to a Dutch municipality, divided by the number of men born in the same period from the 1971 census. Each color represents a quantile of the distribution of the forced labor intensity. The data is missing for municipalities for which the number of observations in the 1971 census is below 10 due to data protection regulations. The municipalities affected by the Dutch Hunger Winter are excluded.



(a) Regional distribution of the average share of damaged housing in German locations over Dutch municipalities



(b) Regional distribution of the average distance to labor education camps from German locations over Dutch municipalities

Figure 5: Regional distribution of exposure to adverse conditions over Dutch municipalities

Notes. This figure shows the average exposure of conscripted forced workers from each Dutch municipality to adverse conditions during their stay in Germany, measured as the average share of destroyed housing stock in German counties where forced workers from each municipality were stationed in panel (a) and the average distance to labor education camps from those locations in Germany in panel (b). Each color represents a quantile of the distribution of the forced labor intensity. The data is missing for municipalities for which no conscripted men could be linked to the archival records by Arolsen Archives, from which I take the location of conscripted forced workers in Germany. The municipalities affected by the Dutch Hunger Winter are excluded.

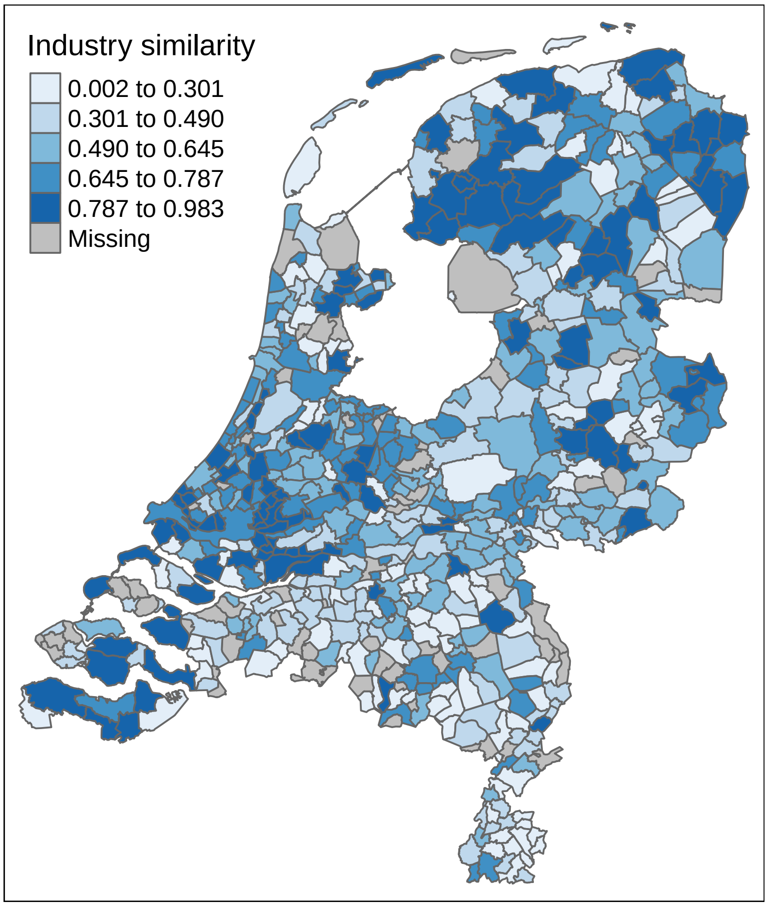
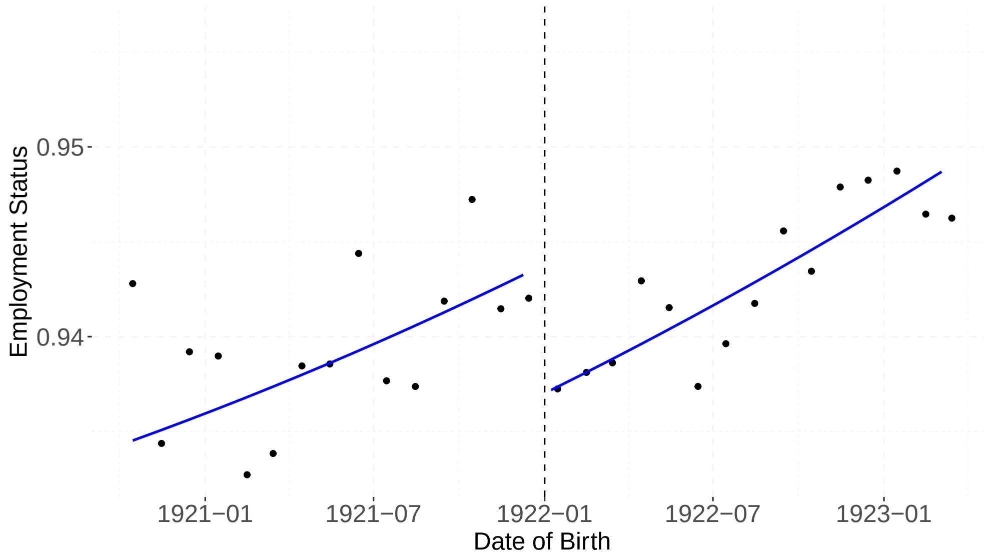
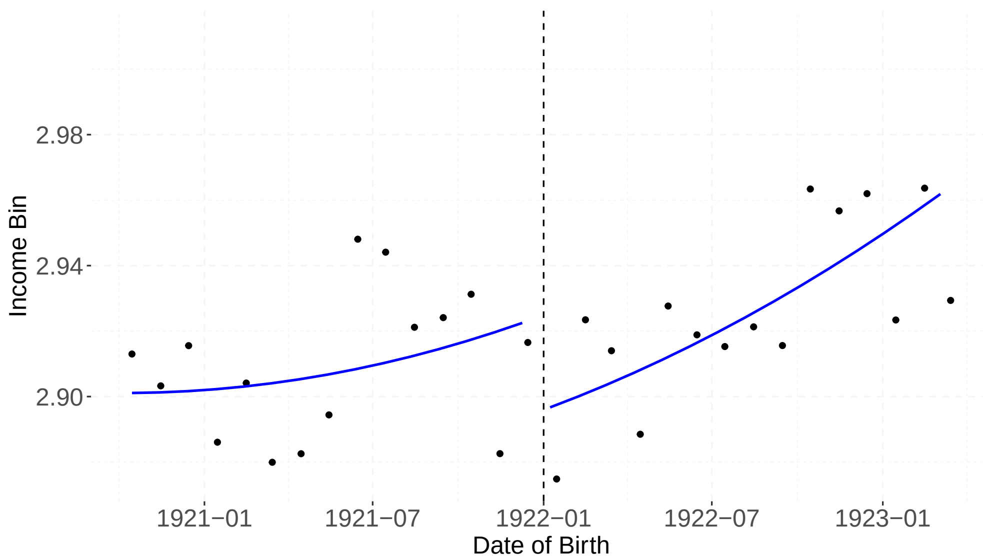


Figure 6: Regional Distribution of Correlation between industries

Notes. This figure shows the correlation between the employment share of industries in the Dutch municipality in 1930 and the average exposure to employment shares of industries in Germany in 1939 (calculated according to equation (5)), differentiating between 27 industries, across Dutch municipalities. Each color represents a quantile of the distribution of industry similarity. The data is missing for municipalities for which the number of observations in the 1971 census is below 10 due to data protection regulations. The municipalities affected by the Dutch Hunger Winter are excluded.



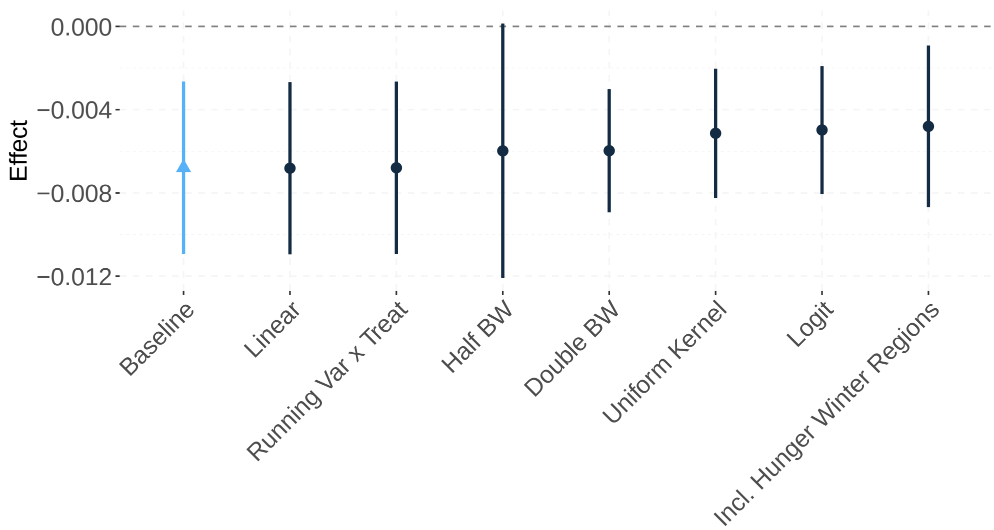
(a) Employment status
Estimate: β_1 = -0.0068 (0.0021)***



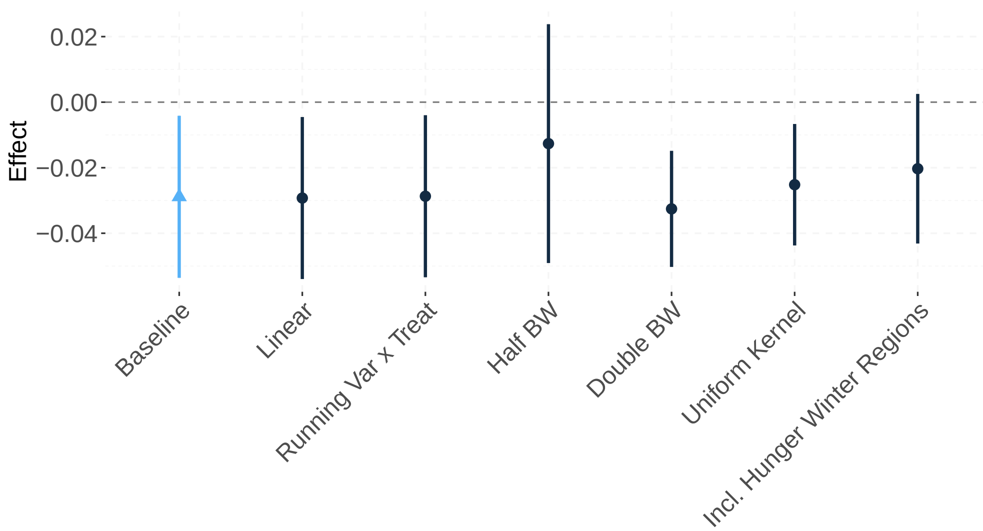
(b) Income bin
Estimate: β_1 = -0.0289 (0.0126)**

Figure 7: RDD effects of forced labor conscription on labor market outcomes based on 1971 Census

Notes. *p<0.1; **p<0.05; ***p<0.01. The figures show the average of labor market outcomes based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. Panel (a) shows the employment status taking a value of zero or one, and panel (b) shows the income bin measured from 0 to 5 in steps of 4,000 Dutch Guilder. The y-axis is normalized to 10% of a standard deviation for each respective outcome. Table 3 shows the underlying regression results in table form.



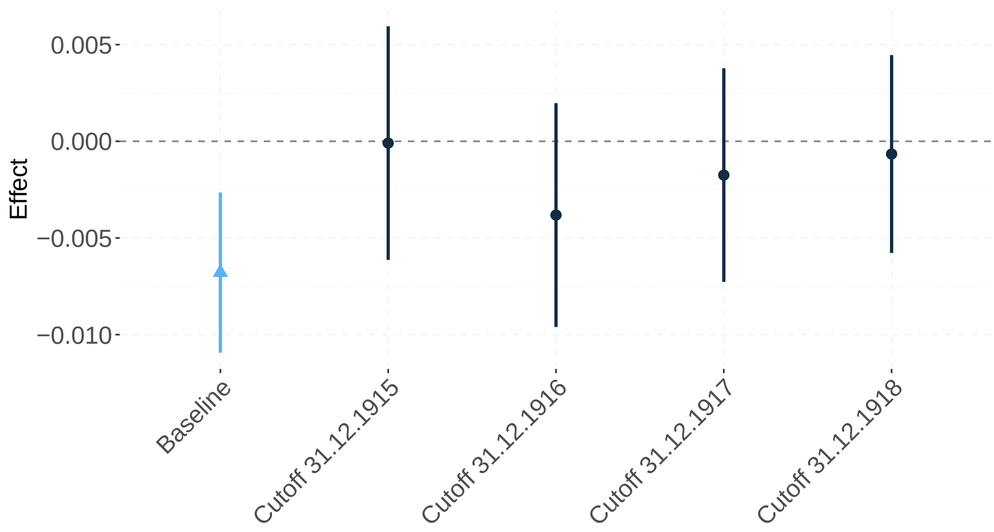
(a) Employment status



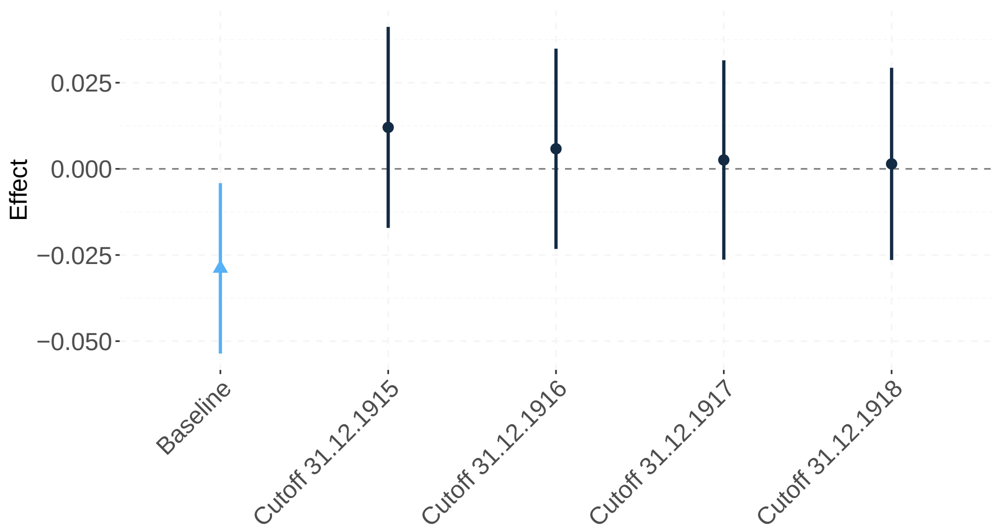
(b) Income bin

Figure 8: Robustness of RDD effects of forced labor conscription on labor market outcomes based on 1971 census

Notes. This figure shows RDD regressions using the 1971 Census with different specifications. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. The bars show the 95% confidence interval. Tables 4 and 5 show the underlying regression results in table form.



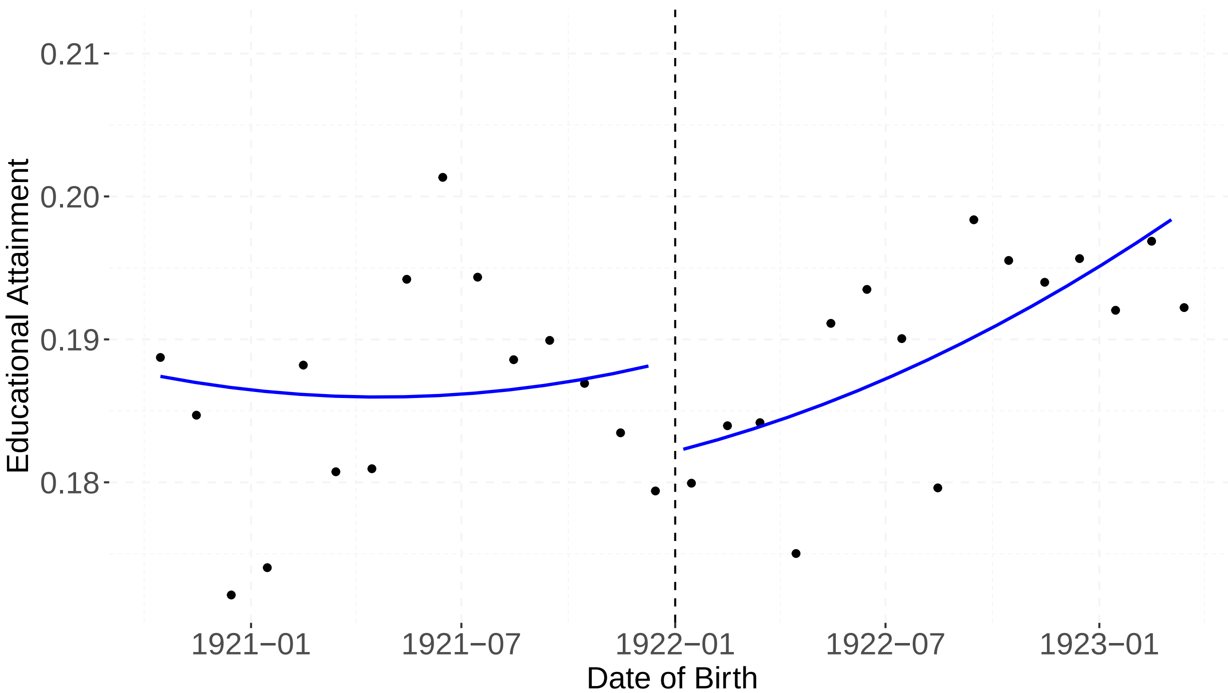
(a) Employment status



(b) Income bin

Figure 9: Placebo RDD effects of forced labor conscription on labor market outcomes based on 1971 Census

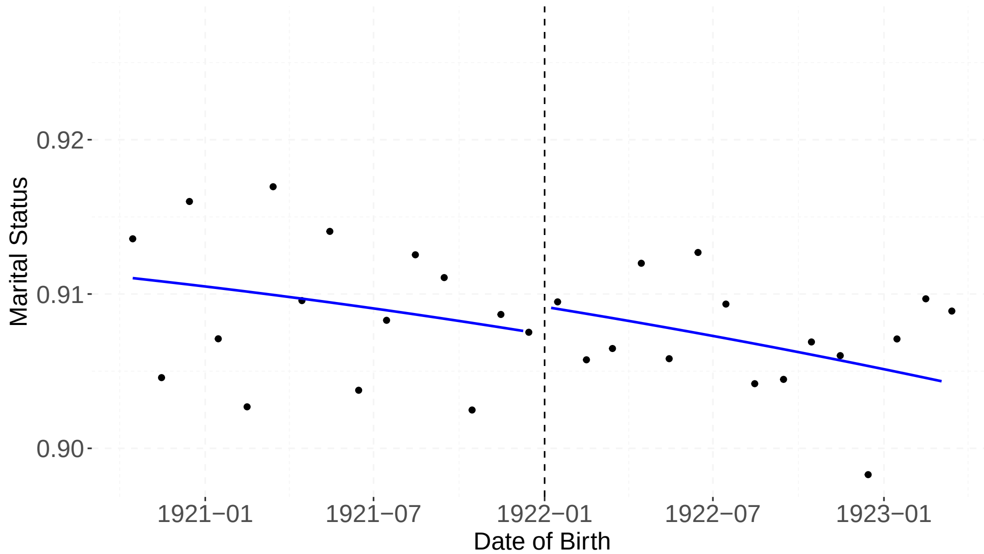
Notes. This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the 1971 Census with different placebo cutoffs. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. The bars show the 95% confidence interval. Tables 6 and 7 show the underlying regression results in table form.



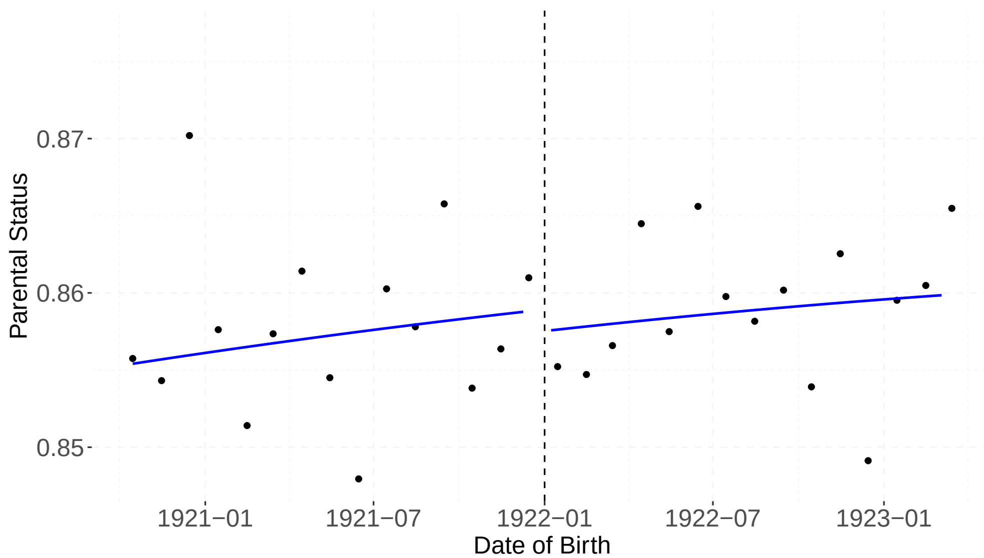
Estimate: β_1 = -0.0064 (0.0038)*

Figure 10: RDD effects of forced labor conscription on education based on 1971 Census

Notes. *p<0.1; **p<0.05; ***p<0.01. The figure shows the average of education based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. The dependent variable a dummy for whether a person finished secondary education. The y-axis is normalized to 10% of a standard deviation for the outcome. Table 8 shows the underlying regression results in table form.



(a) Marital status
Estimate: β_1 = 0.0018 (0.0026)



(b) Parental status
Estimate: β_1 = -0.0014 (0.0031)

Figure 11: RDD effects of forced labor conscription on family formation based on 1971 Census

Notes. *p<0.1; **p<0.05; ***p<0.01. The figures show the average share of individuals who are married and who have at least one child based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. Panel (a) shows a dummy that takes the value of one if married, and zero otherwise. Panel (b) shows a dummy that takes the value of one if an individual has a child, and zero otherwise. The y-axis is normalized to 10% of a standard deviation for each respective outcome. Table 9 shows the underlying regression results in table form.

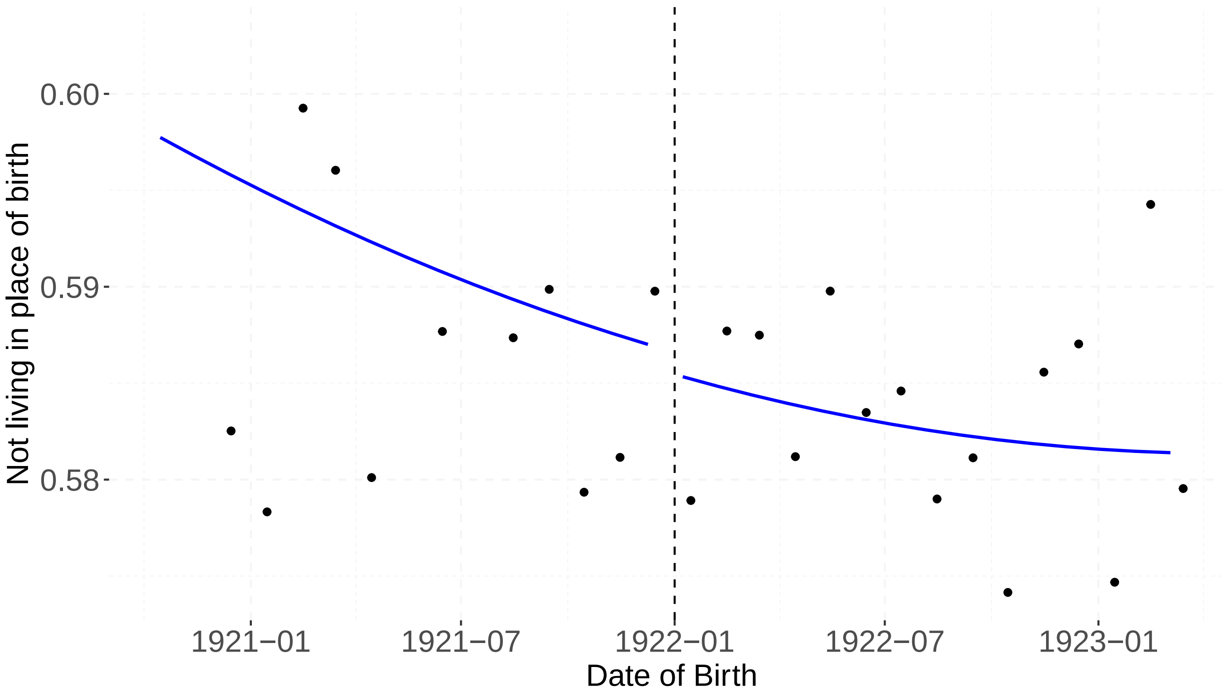


Figure 12: RDD Effects of forced labor conscription on moving based on 1971 Census
Estimate: β_1 = -0.0012 (0.0044)

Notes. *p<0.1; **p<0.05; ***p<0.01. The figure shows the average share of individuals who are not living in the municipality of birth based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. The dependent variable is a dummy for whether a person does not live in their place of birth. The y-axis is normalized to 10% of a standard deviation for each respective outcome. Table 10 shows the underlying regression results in table form.

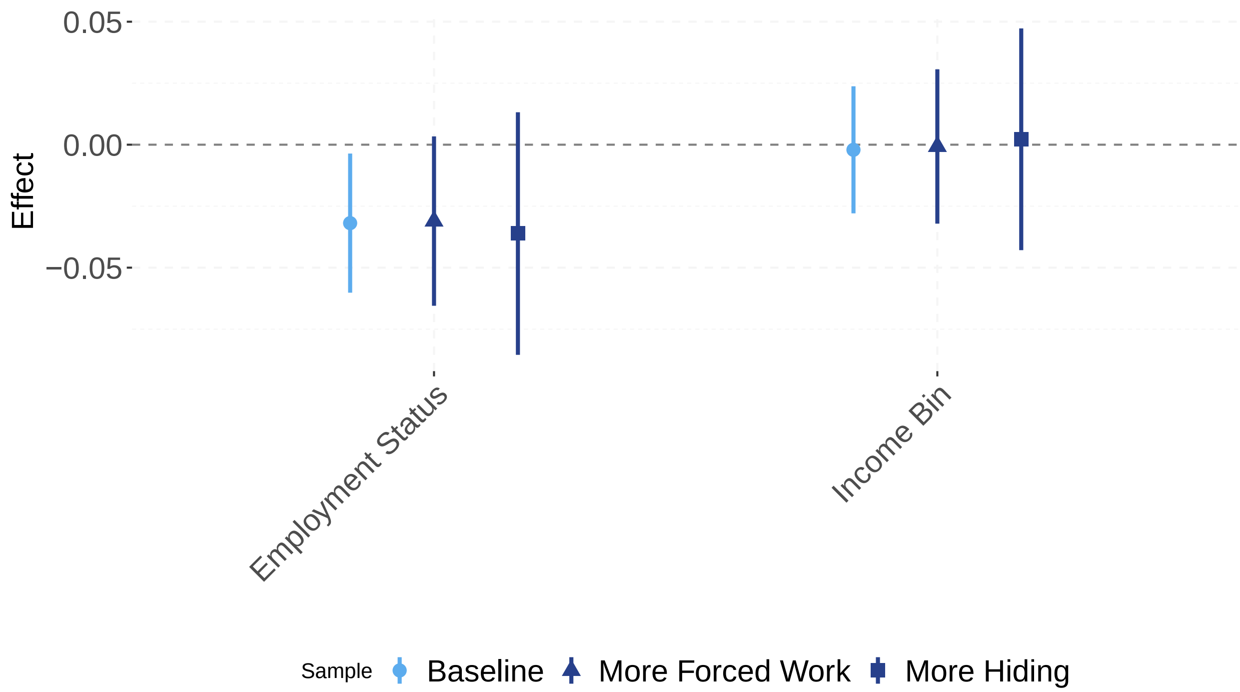
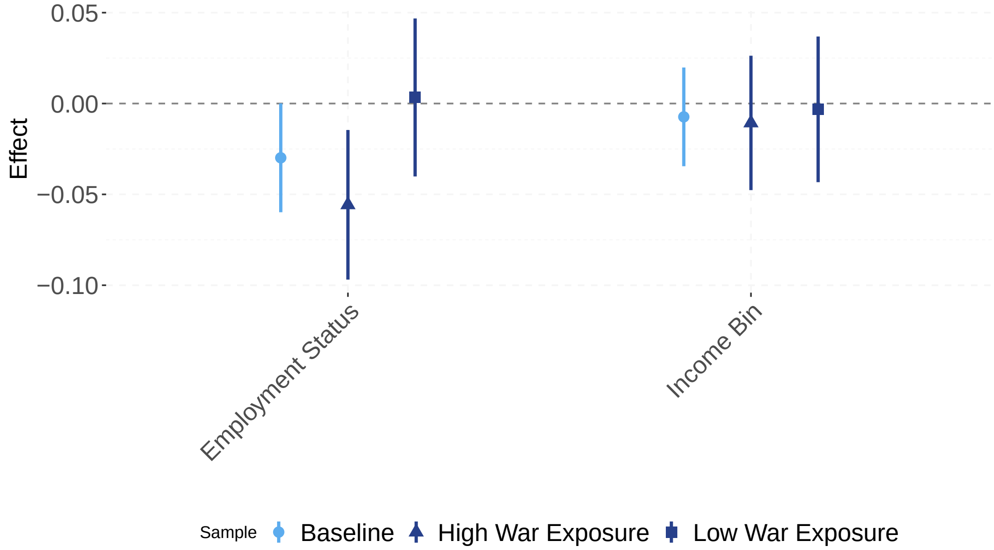
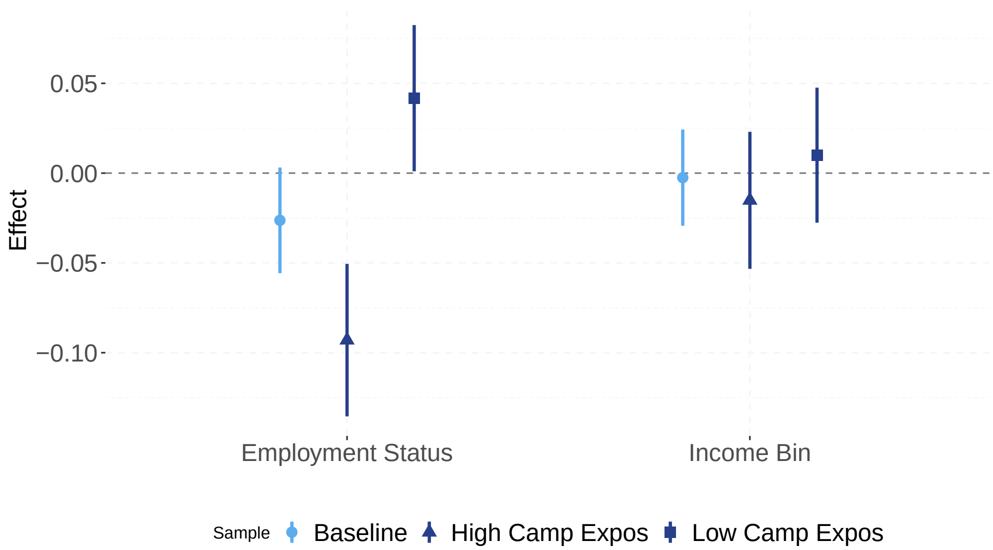


Figure 13: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by share of forced workers

Notes. This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is restricted to individuals who still live in their place of birth and is then split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable. Table 11 shows the underlying regression results in table form.



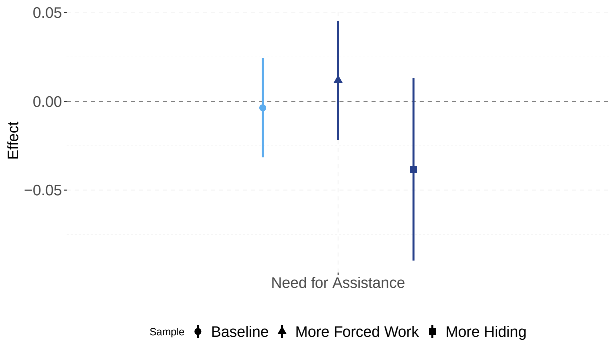
(a) Heterogeneity by share of damaged housing in DE



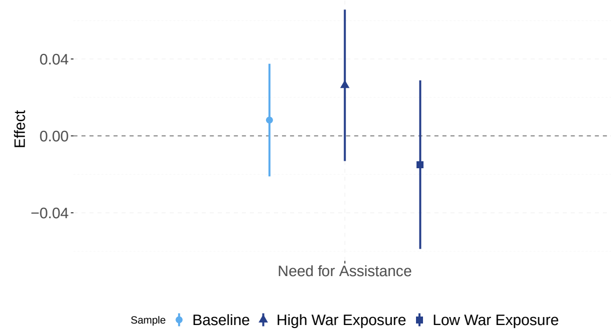
(b) Heterogeneity by distance to labor education camps in DE

Figure 14: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience

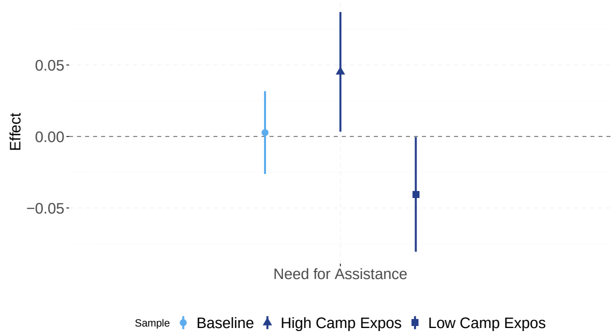
Notes. This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census with subsamples. The sample is restricted to individuals who still live in their place of birth. In panel (a), the sample is then split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. In panel a), the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable. Tables 12 and 13 show the underlying regression results in table form.



(a) Heterogeneity by compliance



(b) Heterogeneity by war exposure in DE



(c) Heterogeneity by camp exposure in DE

Figure 15: Heterogeneous RDD effects of forced labor conscription on need for assistance based on 1971 Census

Notes. This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two 1971 Census. The dependent variable is a dummy variable of whether a person needs assistance in their daily life. The sample is restricted to individuals who still live in their place of birth. In panel (a), the sample is split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. In panel (b), the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. In panel (c), the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the need for assistance. Table 14 shows the underlying regression results in table form.

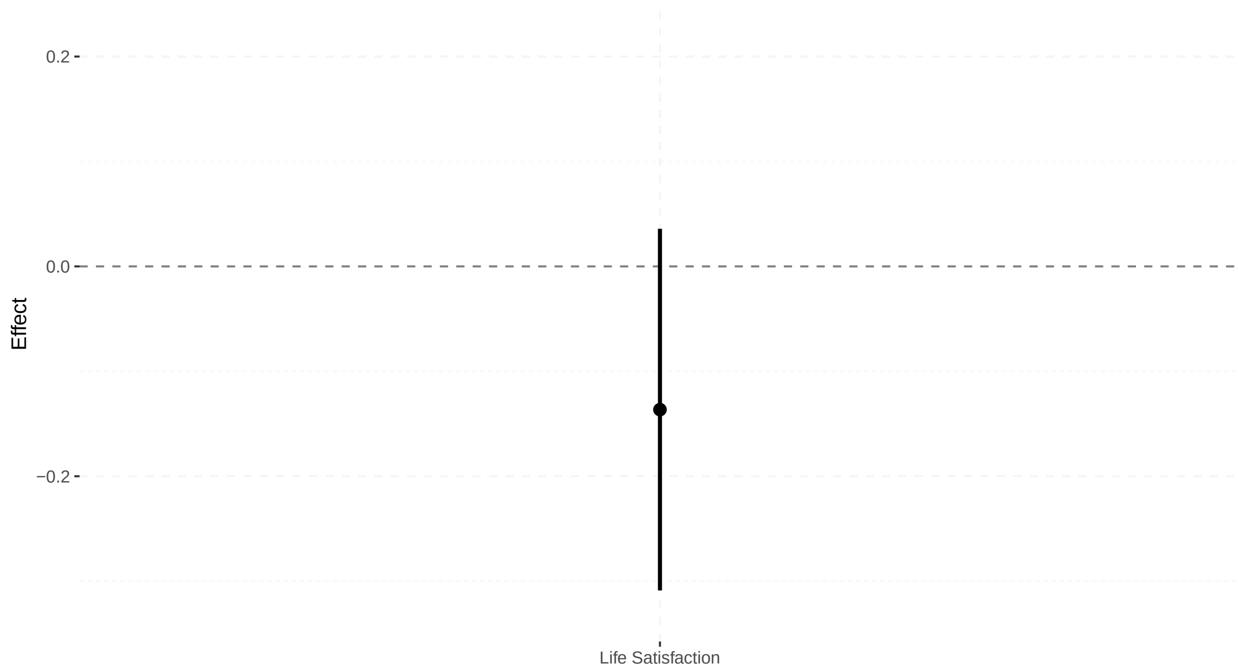


Figure 16: Effects of forced labor conscription on life satisfaction using 1975-1944 Eurobarometer

Notes. This figure shows the estimated coefficient of a simple differences estimation using Eurobarometer data from 1975 to 1994. Life satisfaction is measured from 0 to 3. The 95% confidence intervals and the estimate are standardized by the standard deviation of the respective dependent variable. Table 15 shows the underlying regression results in table form.

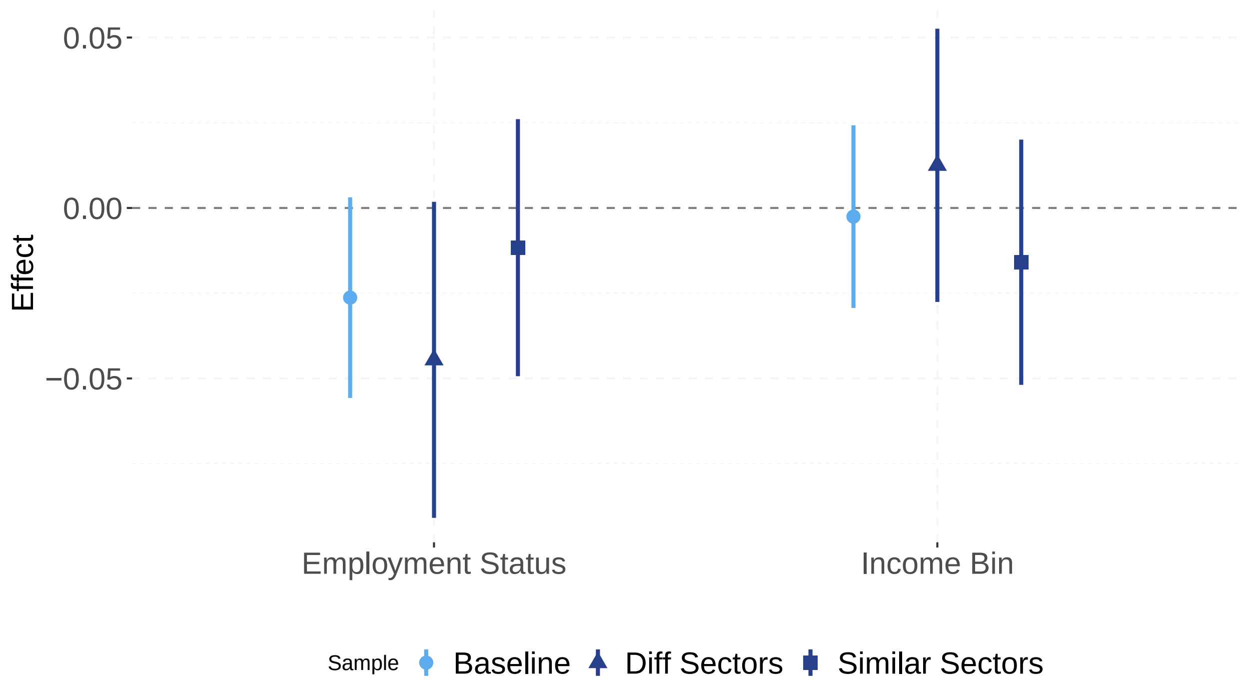


Figure 17: Heterogeneous RDD effects of forced labor Conscription on labor market outcomes by similarity of sectoral composition in DE and NL

Notes. This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. The sample is restricted to individuals who still live in their place of birth. The sample is then split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable. Table 16 shows the underlying regression results in table form.

Tables

Table 1: Descriptive Statistics

Variable	Levels	Mean	Std. Dev.	Mean Treat	Mean Control	Nonresp. Rate (T)	Nonresp. Rate (C)
Employment Status	Employed: Yes/No	0.941	0.235	0.943	0.939	0	0
Income Bin	0-5 (4,000 Dutch guilder range)	2.918	1.362	2.926	2.909	0.067	0.066
Marital Status	Married: Yes/No	0.908	0.289	0.907	0.909	0	0
Parental Status	Has a Child: Yes/No	0.858	0.349	0.859	0.858	0	0
Educational Attainment	Secondary Education: Yes/No	0.188	0.39	0.189	0.186	0.169	0.176
Need for Assistance	Yes/No	0.038	0.191	0.037	0.039	0	0
Observations		151080	151080	76918	74162	76918	74162

Notes. The table shows the descriptive statistics of the outcome variables from the 1971 census for individuals within a 15-month around the cutoff of January 1st, 1922. It reports the mean, standard deviation, mean values separately for treatment and control group, and the non-response rate separately for treatment and control group.

Table 2: Descriptive Statistics: Eurobarometer

Variable	Levels	Mean Overall	Std. Dev. Overall	Mean Treatment	Mean Control
Employment Status	0-1	0.38	0.49	0.37	0.40
Income Class	1-12	7.13	3.23	7.23	7.04
Marital Status	0-1	0.84	0.37	0.84	0.84
Parental Status	0-1	0.10	0.31	0.12	0.09
Life Satisfaction	0-3	2.23	0.68	2.19	2.27

Notes. The table shows the descriptive statistics of the outcome variables from the Eurobarometer waves three to 42 for individuals who are born within a maximum of 2 years around the cutoff of January 1st, 1922 and born certainly before the cutoff (control) or certainly after (treatment). It reports the mean, standard deviation, and mean values separately for treatment and control group.

Online Appendix

5.1 Cleaning Arolsen archival data

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

I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another to be able to exclude double-counts of individuals. I follow the Abramitzky, Boustan, and Eriksson (ABE) Algorithm (Abramitzky et al., 2021), and adjust their method according to my data availability. The ABE method uses variables that 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). Only ca. 40% 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 the observations instead of only their self-reported age as in the census data for which the ABE-JW method was derived. This alleviates the issues connected to only knowing individuals' ages, such as rounding of reported age and differences in age at different points in time of reporting. Therefore, I block on the date of birth and on the first letters of the first and last name instead. Following ABE-JW, for each of the possible matches within a block, I then calculate the string distance of the first name, last name, and place of birth where available using the Jaro-Winkler string distance and restrict links to individuals for whom all available JW distances are less than or equal to 0.1.[58][59] 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 that are close to the original. In my case, however, I am linking observations to other entries from the same dataset, and links to multiple entries are plausible because a person may show up more than twice in the archival records. I therefore do not restrict links to only those entries which have only one plausible match. I then treat all linked individuals as only one observation going forward. Of the originally 594,967 observations, my algorithm links 121,561 observations to another entry, leaving 473,406 observations of probably unique individuals.

Since the archival data does not include the gender of the individuals, I use the data on first names and combine this with information on name frequency based on gender from

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

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

the Corpus of First Names in the Netherlands published by Meertens Instituut (nd). Of the 34,831 unique first names in my dataset, 11,802 (33.9%) are part of the Corpus of First Names. To include names with slightly different spellings, I calculate the JW-distance between first names and assign the same gender to a name with a sufficiently similar name that is part of the Corpus of First Names (a JW-string distance of up to 0.1, as suggested by Abramitzky et al. (2021)). This yields an addition of 17,417 names. In total, I can assign a gender probability to 29,219 or 83.9% of all unique first names. Based on this, I calculate the probability of a given name to be male or female and classify names for which at least 70% of individuals with that name are either male or female respectively. All other names are classified as uncertain. 78.9% of all unique first names can be classified as either male of female using the cutoff of 70%. Since some persons have multiple first names (either because they have a middle name, or because two observations with differently spelled names were linked to the same individual), I use the mode of each person's first names' genders to assign their gender.[60] 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.

To link the places of birth recorded in the archival records to the municipalities of the 1971 census, I use a fuzzy merge and complement it with a list of over 3,500 hand-coded places of birth. The recorded places of birth differ from the municipalities because of spelling issues and changes in municipal borders from time of reporting until 1971. In a first step, I apply a fuzzy merge and classify all entries with a JW-string distance of below 0.12 as in agreement. I then hand-check all municipalities with a JW-string distance of greater or equal to 0.12 and below 0.2. By doing so, I add another 3,559 places of birth that I am able to link. In total, I am able to link the place of birth for 82.6% of all individuals.

5.2 Replication of main results using Eurobarometer data

In section 4.3.1, I use Eurobarometer survey data to test for effects of forced labor conscription on mental health. To ensure that this approach is valid, I replicate the main findings with this dataset. I define the following treatment and control group, consisting of one age group respectively: The older control group consists of men born in 1920 or 1921, the older treatment group are men born in 1922 or 1923.

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

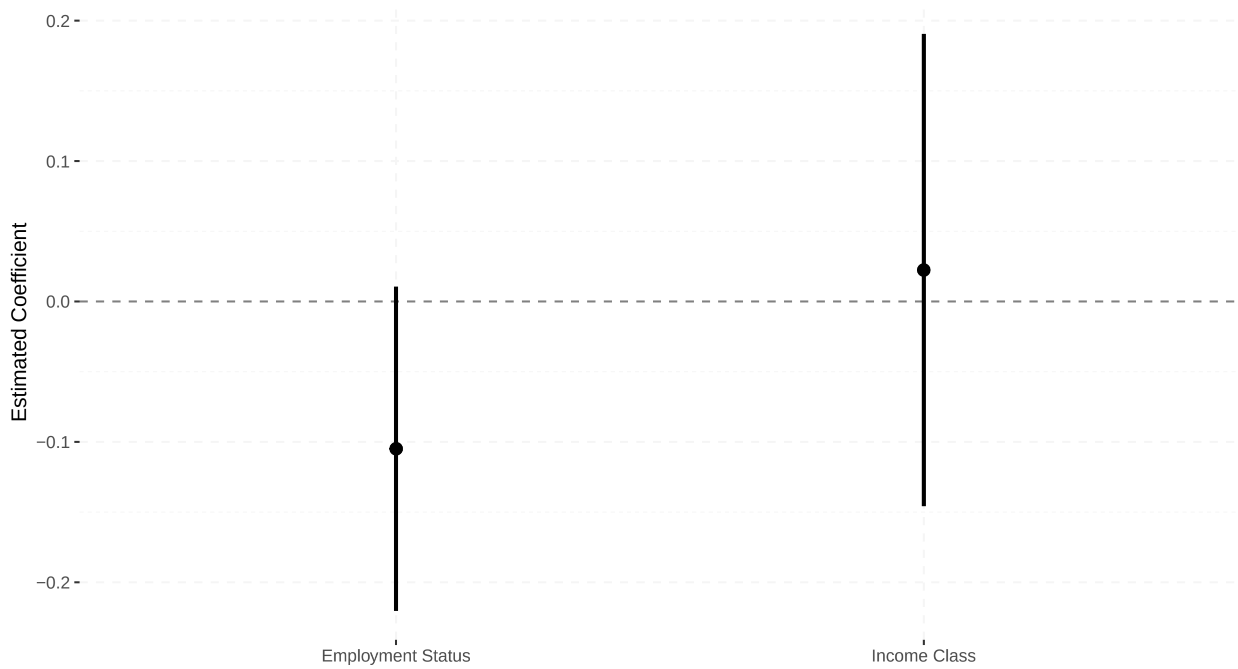


Figure 18: Effects of forced labor conscription on labor market outcomes using 1975-1944 Eurobarometer

Notes. This figure shows the estimated coefficients of a simple differences estimation using Eurobarometer data from 1975 to 1994. Employment status is a dummy variable for whether a person is employed, and income class is measured from 1 to 12. Bars indicate the 95% confidence intervals. Estimates are standardized by the standard deviation of the respective dependent variable. Table 17 shows the underlying regression results in table form.

I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \tag{6}$$

where λ_t are wave fixed effects to control for differences in survey design between different waves. The outcomes of interest Y_{it} are employment status and income measured in 12 bins. Figure 18 shows the results. While I find no significant differences in income, I again find that individuals in the treatment group are 5.1% less likely to participate in the labor market which is 10.4% of one standard deviation. Figure 19 shows the results for family formation (a dummy for being married and a dummy for having children), where I find no significant differences..

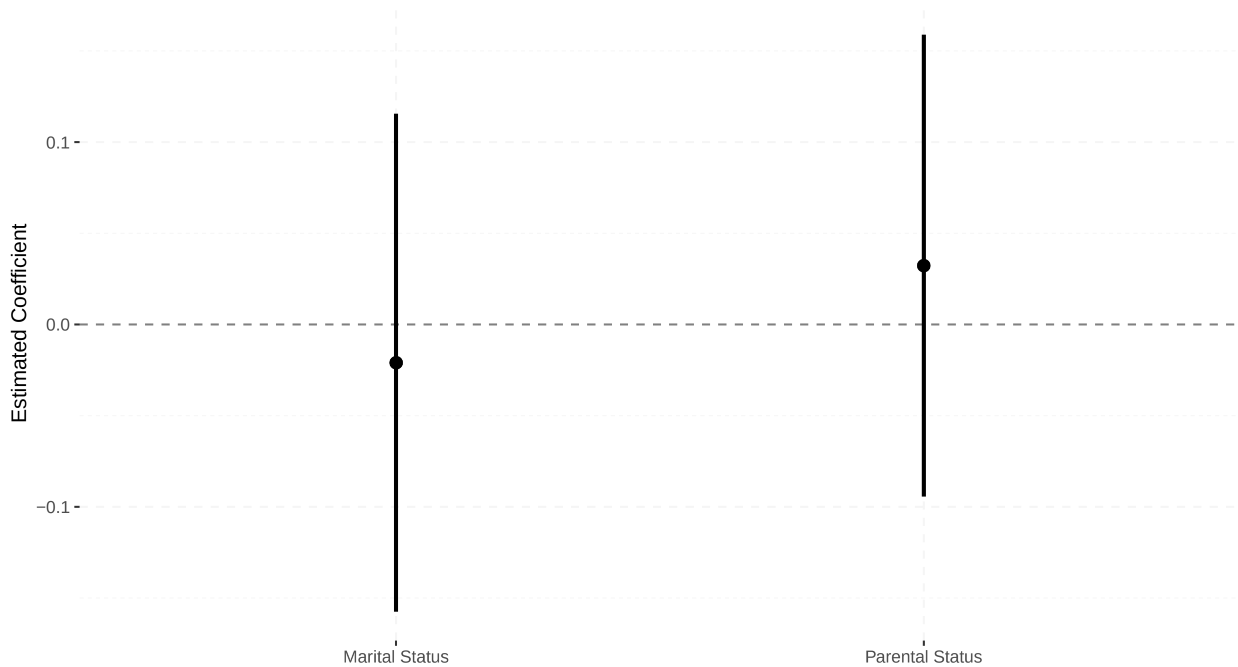
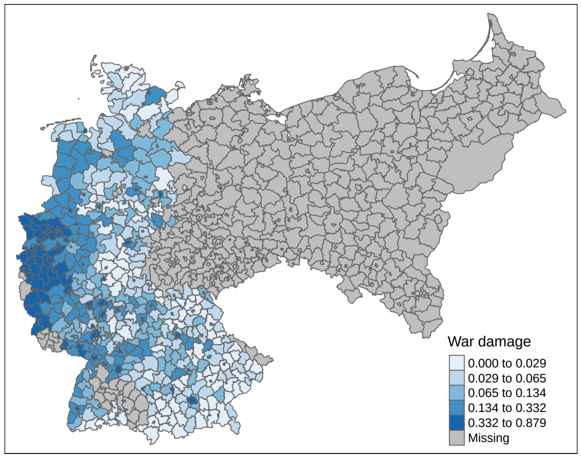


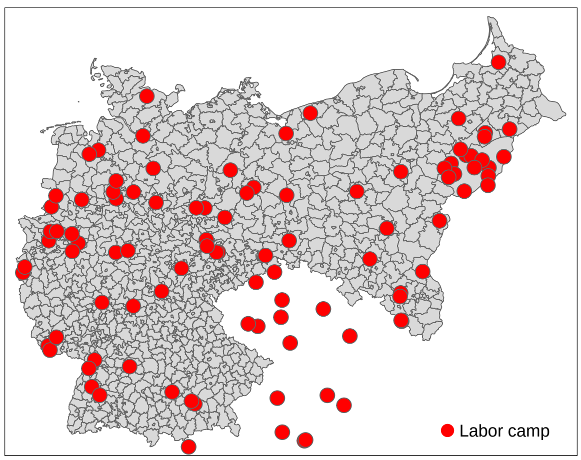
Figure 19: Effects of forced labor conscription on family formation using 1975-1944 Eurobarometer

Notes. This figure shows the estimated coefficients of a simple differences estimation using Eurobarometer data from 1975 to 1994. Marital status is a dummy which takes the value of one if married, and zero otherwise, and parental status is a dummy which takes the value of one if an individual has a child, and zero otherwise. The 95% confidence intervals and the estimates are standardized by the standard deviation of the respective dependent variable. Table 18 shows the underlying regression results in table form.

5.3 Supplementary Figures



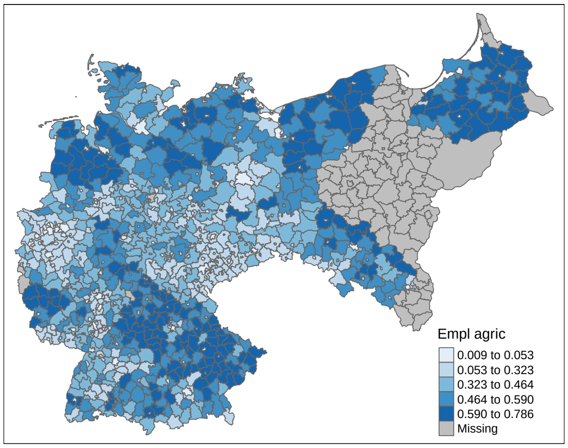
(a) Share of damaged houses across German counties



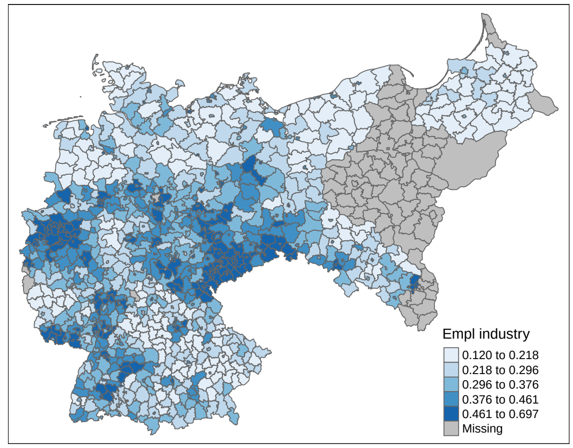
(b) Location of labor education camps

Figure 20: Regional distribution of adverse forced labor conditions over German counties

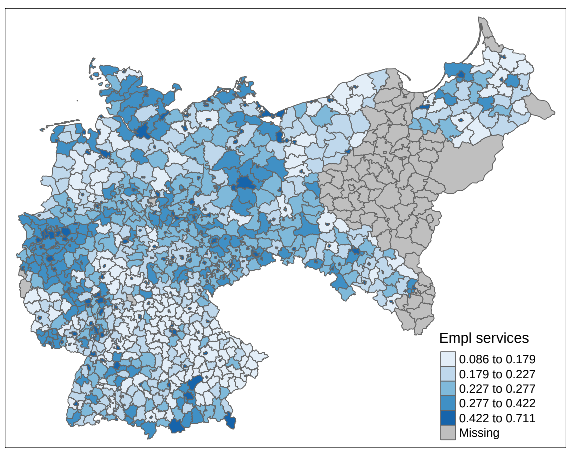
Notes. This figure shows the regional distribution of two measures for the adversity of forced labor in Germany: Panel (a) shows the share of housing stock that was damaged during the war in German counties. Each color represents a quantile of the distribution of the respective variable. The data comes from Peters (2022). Panel (b) shows the location of labor education camps. The data comes from Lofti (2000).



(a) Employment share: Agriculture



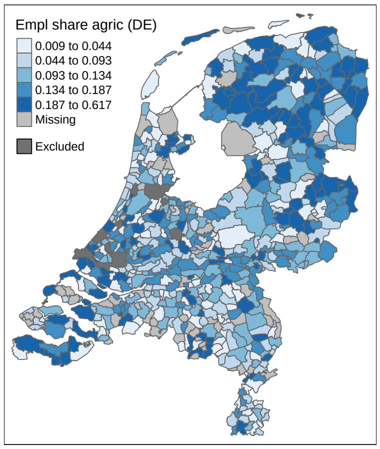
(b) Employment share: Industry



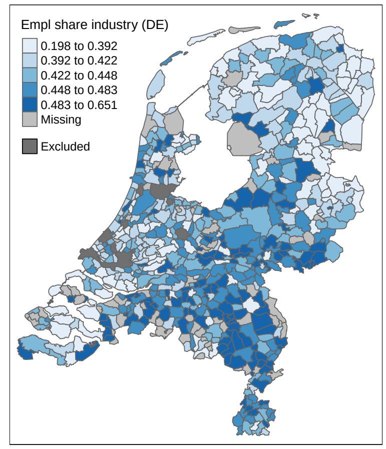
(c) Employment share: Services

Figure 21: Sectoral composition across German counties

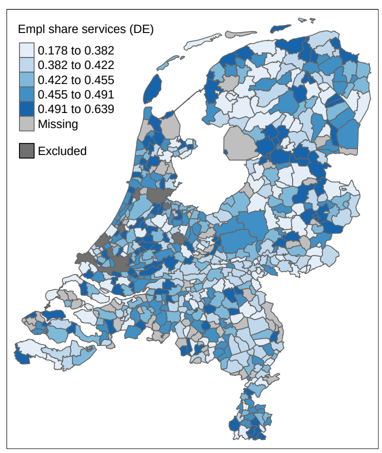
Notes. The figures show the employment share for agriculture, industry, and services in 1939 over German counties. Each color represents a quantile of the distribution of the respective variable. The data comes from Braun and Franke (2021).



(a) Employment share: Agriculture



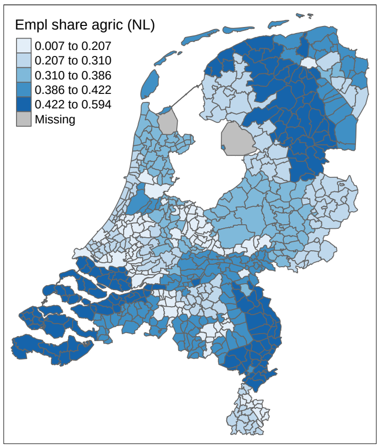
(b) Employment share: Industry



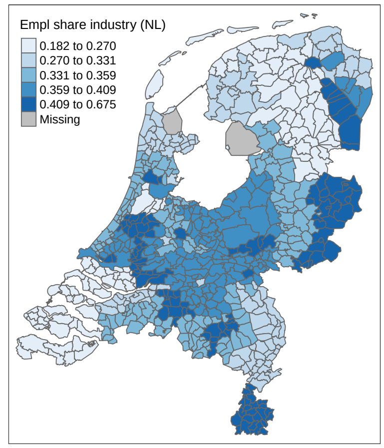
(c) Employment share: Services

Figure 22: Average exposure to sectoral composition across Dutch municipalities

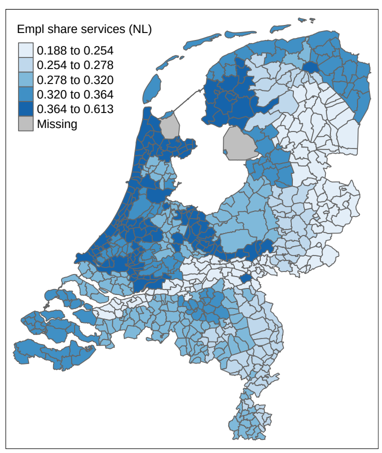
Notes. The figures show the average employment share for agriculture, industry, and services that forced workers from each Dutch municipality were exposed to in their location in Germany during their forced labor stay. Each color represents a quantile of the distribution of the respective variable. The data comes from Braun and Franke (2021). The data is missing for municipalities for which no conscripted men could be linked to the archival records by Arolsen Archives, from which I take the location of conscripted forced workers in Germany. The municipalities affected by the Dutch Hunger Winter are excluded.



(a) Employment share: Agriculture



(b) Employment share: Industry



(c) Employment share: Services

Figure 23: Sectoral composition across Dutch regions

Notes. The figures show the employment share for agriculture, industry, and services in 1930 over 42 Dutch regions. Each color represents a quantile of the distribution of the respective variable. The data comes from CBS, 1934. The data is missing for municipalities that did not yet exist in 1930.

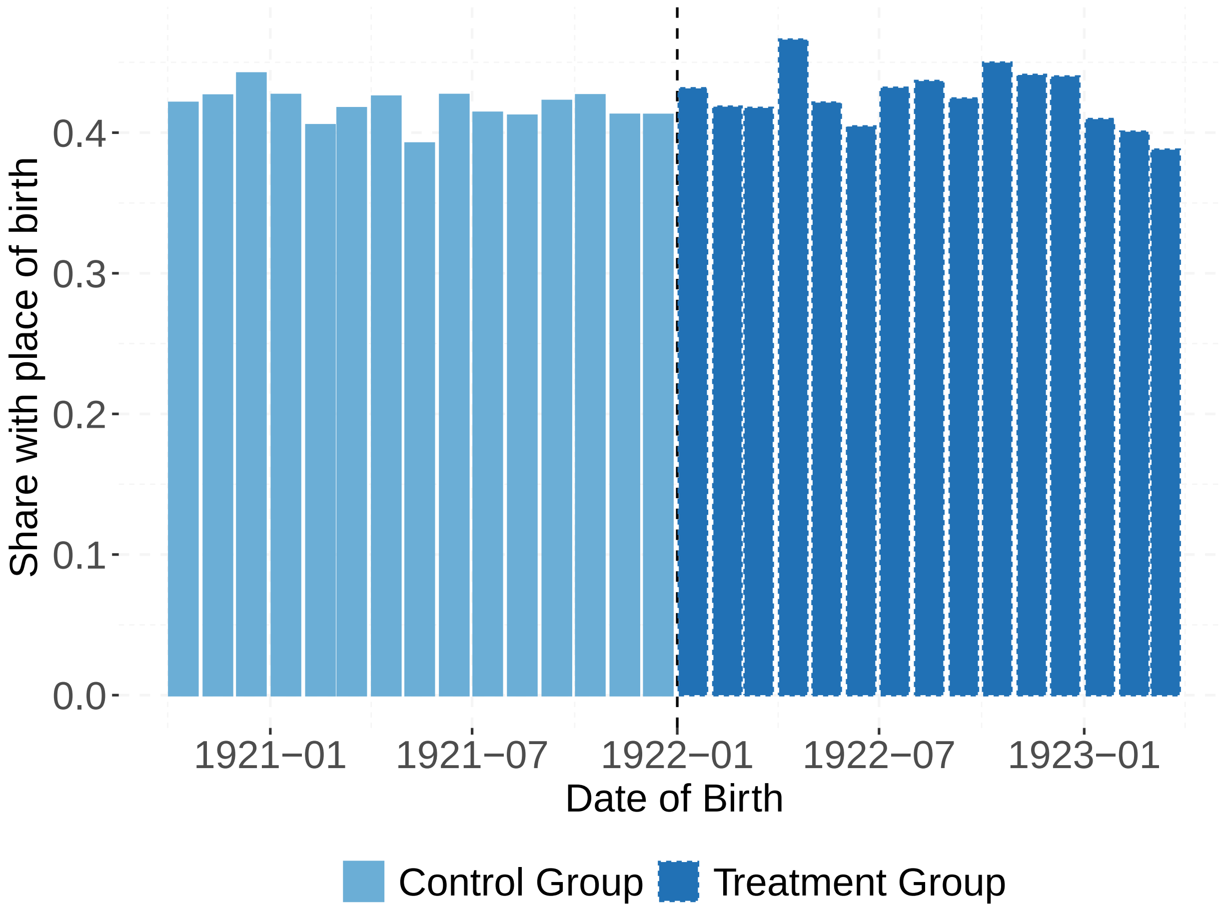
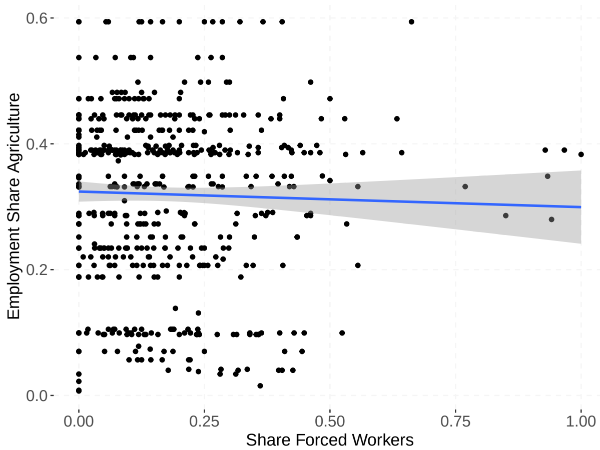
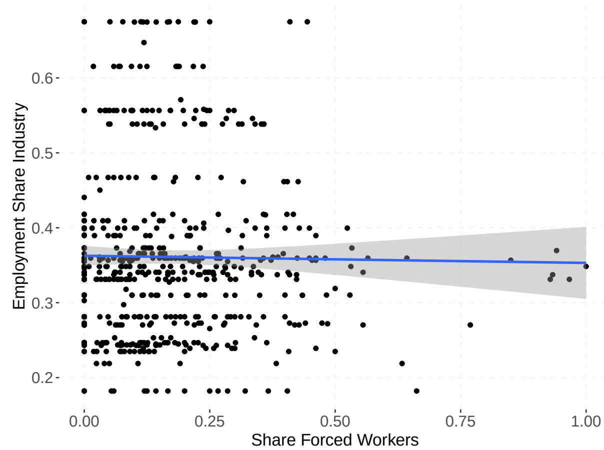


Figure 24: Share of conscripted men with information on place of birth

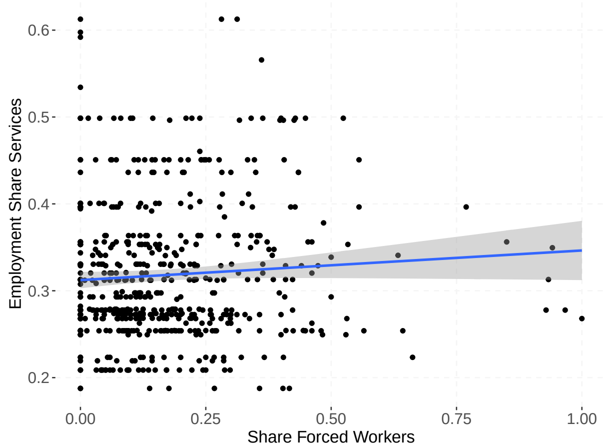
Notes. The data shows the share of conscripted men from the Arolsen Archives born within the optimal bandwidth of 15 months who have information on their place of birth.



(a) Correlation of forced labor intensity and agriculture employment share



(b) Correlation of forced labor intensity and industry employment share



(c) Correlation of forced labor intensity and service employment share

Figure 25: Correlation of forced labor intensity and industry composition

Notes. This figure shows the correlation of a municipality's forced labor intensity, measured as the number of male forced workers born within a 15-month bandwidth after the conscription cutoff of January 1, 1922 from Arolsen Archives whose place of birth can be linked to a Dutch municipality, divided by the number of men born in the same period from the 1971 census, with the employment share in agriculture (panel (a), industry (panel (b), and service (panel (c), all measured at the regional level in 1930 taken from CBS, 1934.

(a) Call for men born in 1922

(b) Call for men born in 1923 and 1924

Figure 26: Call for men born in 1922-1924

Notes. This figure shows the posters calling for men born in 1922, 1923, and 1924 for work in Germany. Source: NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies (2025).

5.4 Supplementary Tables

This section reports the regression tables for the regressions underlying the RDD plots and coefficient plots.

Table 3: RDD effects of forced labor conscription on labor market outcomes

	Dependent variable:	
	Employment Status	Income Bin
	(1)	(2)
RDD Estimate	-0.007^{***}	-0.029^{**}
	(0.002)	(0.013)
Observations	151056	141063
Bandwidth	15 months	15 months
Dependent Variable Range	Employed: Yes/No	0-5 (4,000 Dutch guilder range)
Mean Dependent Variable	0.941	2.918

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. Employment status is a dummy variable for a person being employed, and income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 7 shows the results in an RDD plot.

Table 4: Robustness of RDD effects of forced labor conscription on employment

	Dependent variable:							
	Employment Status							
	Baseline	Linear	Running Var x Treat	Half BW	Double BW	Uniform Kernel	Logit	Incl. Hunger Winter Regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RDD Estimate	-0.007^{***}	-0.007^{***}	-0.007^{***}	-0.006^{*}	-0.006^{***}	-0.005^{***}	-0.090^{***}	-0.005^{**}
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.028)	(0.002)
Observations	151056	151056	151056	70448	294141	356617	356617	174388
Bandwidth	15 months	15 months	15 months	30 months	7.5 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD with different specifications. Employment status is a dummy variable for a person being employed. Figure 8 shows the results in a coefficient plot.

Table 5: Robustness of RDD effects of forced labor conscription on income

	Dependent variable:						
	Income Bin						
	Baseline	Linear	Running Var x Treat	Half BW	Double BW	Uniform Kernel	Incl. Hunger Winter Regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RDD Estimate	-0.029^{**}	-0.029^{**}	-0.029^{**}	-0.013	-0.033^{***}	-0.025^{***}	-0.020^{*}
	(0.013)	(0.013)	(0.013)	(0.019)	(0.009)	(0.009)	(0.012)
Observations	141063	141063	141063	65727	274723	333179	161859
Bandwidth	15 months	15 months	15 months	30 months	7.5 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD with different specifications. Income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 8 shows the results in a coefficient plot.

Table 6: Placebo RDD effects of forced labor conscription on employment

	Dependent variable:				
	Employment Status				
	Baseline	Cutoff 31.12.1915	Cutoff 31.12.1916	Cutoff 31.12.1917	Cutoff 31.12.1918
	(1)	(2)	(3)	(4)	(5)
RDD Estimate	-0.007^{***}	-0.0001	-0.004	-0.002	-0.001
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	151056	124776	126116	125539	130965
Bandwidth	15 months	15 months	15 months	15 months	15 months
Note:					*p<0.1; **p<0.05; ***p<0.01

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two with different placebo cutoffs. Employment status is a dummy variable for a person being employed. Figure 9 shows the results in a coefficient plot.

Table 7: Placebo RDD effects of forced labor conscription on income

	Dependent variable:				
	Income Bin				
	Baseline	Cutoff 31.12.1915	Cutoff 31.12.1916	Cutoff 31.12.1917	Cutoff 31.12.1918
	(1)	(2)	(3)	(4)	(5)
RDD Estimate	-0.029^{**}	0.012	0.006	0.003	0.001
	(0.013)	(0.015)	(0.015)	(0.015)	(0.014)
Observations	141063	115812	117198	116847	122176
Bandwidth	15 months	15 months	15 months	15 months	15 months
Note:					*p<0.1; **p<0.05; ***p<0.01

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two with different placebo cutoffs. Income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 9 shows the results in a coefficient plot.

Table 8: RDD effects of forced labor conscription on education

	Dependent variable:
	Educational Attainment
RDD Estimate	-0.006^{*}
	(0.004)
Observations	125025
Bandwidth	15 months
Dependent Variable Range	Finished Secondary Education: Yes/No
Mean Dependent Variable	0.188

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. The dependent variable a dummy for whether a person finished secondary education. Figure 10 shows the results in an RDD plot.

Table 9: RDD effects of forced labor conscription on family formation

	Dependent variable:	
	Marital Status	Parental Status
	(1)	(2)
RDD Estimate	0.002	-0.001
	(0.003)	(0.003)
Observations	151080	151080
Bandwidth	15 months	15 months
Dependent Variable Range	Married: Yes/No	Has a Child: Yes/No
Mean Dependent Variable	0.908	0.858

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. Marital status is a dummy variable which takes the value of one if a person is married and zero otherwise (i.e. never married, widowed, divorced, living separately). Parental status is a dummy variable indicating whether a person has at least one child. Figure 11 shows the results in an RDD plot.

Table 10: RDD effects of forced labor conscription on not living in place of birth

	Dependent variable:
	Not living in place of birth
RDD Estimate	−0.001
	(0.004)
Observations	151080
Bandwidth	15 months
Dependent Variable Range	Yes/No
Mean Dependent Variable	0.038

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows regression results based on the 1971 Census estimating a RDD using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two using the underlying individual-level data. The dependent variable is a dummy for whether a person does not live in their place of birth. Figure 12 shows the results in an RDD plot.

Table 11: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by share of forced workers

	Dependent variable:					
	Employment Status			Income bin		
	Baseline	More Hiding	More Forced Work	Baseline	More Hiding	More Forced Work
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	−0.007**	−0.008	−0.007*	−0.003	0.003	−0.001
	(0.003)	(0.006)	(0.004)	(0.018)	(0.032)	(0.022)
Observations	62311	18874	43437	58123	17786	40346
Bandwidth	15 months	15 months	15 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is restricted to individuals who still live in their place of birth and is then split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 13 shows the results in a coefficient plot.

Table 12: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience: Share of damaged housing

	Dependent variable:					
	Employment Status			Income bin		
	Baseline	Low War Exposure	High War Exposure	Baseline	Low War Exposure	High War Exposure
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	-0.007^{*}	0.001	-0.013^{***}	-0.010	-0.004	-0.015
	(0.004)	(0.005)	(0.005)	(0.019)	(0.028)	(0.026)
Observations	56775	25144	31631	52923	23426	29497
Bandwidth	15 months	15 months	15 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is restricted to individuals who still live in their place of birth and is then split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 14 shows the results in a coefficient plot.

Table 13: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience: Distance to labor education camps

	Dependent variable:					
	Employment Status			Income bin		
	Baseline	Low Camp Exposure	High Camp Exposure	Baseline	Low Camp Exposure	High Camp Exposure
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	-0.006^{*}	0.010^{**}	-0.022^{***}	-0.003	0.014	-0.021
	(0.004)	(0.005)	(0.005)	(0.019)	(0.026)	(0.027)
Observations	58363	29111	29252	54409	27053	27356
Bandwidth	15 months	15 months	15 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is restricted to individuals who still live in their place of birth and is then split by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 14 shows the results in a coefficient plot.

Table 14: Heterogeneous RDD effects of forced labor conscription on need for assistance

	Dependent variable:						
	Need for Assistance						
	Baseline	More Hiding	More Forced Work	Low War Exposure	High War Exposure	Low Camp Exposure	High Camp Exposure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RDD Estimate	-0.001	-0.007	0.002	-0.003	0.005	-0.008^{**}	0.009^{**}
	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	62319	18876	43443	25145	31636	29115	29255
Bandwidth	15 months	15 months	15 months	15 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree using the 1971 Census. The dependent variable is a dummy variable of whether a person needs assistance in their daily life. The sample is restricted to individuals who still live in their place of birth. The sample is then split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives, by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany, and by the median of the average weighted exposure of forced workers from a Dutch municipality to labor education camps in Germany. Figure 15 shows the results in a coefficient plot.

Table 15: Effects of forced labor conscription on life satisfaction using 1975-1944 Eurobarometer

	Dependent variable:
	Life Satisfaction
	(1)
treatmentGroup	−0.093
	(0.060)
Wave FE	YES
Dependent Variable Range	0-3
Mean Dependent Variable	2.23
Observations	492
R^2	0.137
Adjusted R^2	0.063

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows the estimated coefficient of a simple differences estimation using Eurobarometer data from 1975 to 1994 including wave fixed effects. Life satisfaction is measured from 0 to 3. Figure 16 shows the results in a coefficient plot.

Table 16: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by similarity of sectoral composition

	Dependent variable:					
	Employment Status			Income bin		
	Baseline	Different Sectors	Similar Sectors	Baseline	Different Sectors	Similar Sectors
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	−0.006*	−0.010*	−0.003	−0.003	0.017	−0.022
	(0.004)	(0.006)	(0.005)	(0.019)	(0.028)	(0.025)
Observations	58363	26170	32193	54409	24556	29853
Bandwidth	15 months	15 months	15 months	15 months	15 months	15 months

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is restricted to individuals who still live in their place of birth and is then split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to. Employment status is a dummy variable for a person being employed, income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder. Figure 17 shows the results in a coefficient plot.

Table 17: Effects of forced labor conscription on labor market outcomes using 1975-1944 Eurobarometer

	Dependent variable:	
	Employment Probability	Income
	(2)	(3)
treatmentGroup	-0.051^{*}	0.072
	(0.029)	(0.277)
Wave FE	YES	YES
Dependent Variable Range	0-1	1-12
Mean Dependent Variable	0.38	7.13
Observations	620	505
R^2	0.482	0.187
Adjusted R^2	0.438	0.108

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows the estimated coefficient of a simple differences estimation using Eurobarometer data from 1975 to 1994 including wave fixed effects. Employment status is a dummy variable for whether a person is employed, and income class is measured from 1 to 12. Figure 18 shows the results in a coefficient plot.

Table 18: Effects of forced labor conscription on family formation using 1975-1944 Eurobarometer

	Dependent variable:	
	Marital Status	Parental Status
	(1)	(2)
treatmentGroup	−0.008	0.010
	(0.031)	(0.024)
Wave FE	YES	YES
Dependent Variable Range	0-1	0-1
Mean Dependent Variable	0.84	0.1
Observations	612	620
R^2	0.092	0.218
Adjusted R^2	0.014	0.152

Notes. *p<0.1; **p<0.05; ***p<0.01. The table shows the estimated coefficient of a simple differences estimation using Eurobarometer data from 1975 to 1994 including wave fixed effects. Marital status is a dummy which takes the value of one if married, and zero otherwise, and parental status is a dummy which takes the value of one if an individual has a child, and zero otherwise. Figure 19 shows the results in a coefficient plot.