Consequences of Forced Labor Conscription: Evidence from Dutch Civilians*

Carola Stapper†

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Abstract

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

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

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

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1 Introduction

We know from previous literature that interruptions of a persons' labor market career can have lasting effects on their later economic success. One type of interruption that has not been studied is the exposure to forced labor policies. In today's economy, forced labor remains to be a large issue: According to estimates by the International Labor Office (ILO), around 27.6 million people worked in some type of forced labor in 2021 (ILO, 2022)[1]. Given the high prevalence of forced labor, even today, it is important to understand the consequences such a disruption can have. This is inherently difficult in the contemporaneous setting for several reasons: First, data is often absent, and data collection could endanger affected workers (LeBaron, 2018). Additionally, the factors that contribute to the vulnerability of being exposed to forced labor may be correlated with the outcomes we are interested in (ILO, 2022). Moreover, current forced labor systems do not allow for studying long-term consequences.

I 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). I exploit quasi-experimental variation in the assignment into forced labor for Dutch civilians to avoid endogeneity concerns. By linking archival data with micro-level census data, I can circumvent the issue of data absence.

During WWII, the German government conscripted civilians of 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, the occupational regime decided to conscript all men born between 1922 and 1924 (aged 18–21 at the time) for labor in Germany[2]. The coercion was enforced through the withholding of food ration cards and forbidding businesses to employ men born in these years. At least 37% of these cohorts went to Germany for forced work. Anyone who was not granted an exemption

[1] 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).

[2] This decision was made in May 1943

had to go into hiding[3]. Assignment into sectors in Germany was done irrespective of previous skills. Most of the Dutch workers survived the forced labor experience and returned to the Netherlands after the end of the war (Tooze, 2006).

Exploiting the exogenous variation in being subject to conscription by the German forces based on the birth date in the Dutch context, I compare later educational attainment, income, and likelihood of employment of individuals who were born within the years that were conscripted to that of individuals who were born before. More formally, I employ a regression discontinuity design at the cutoff of January 1, 1922 of the forced conscription policy[4]. I estimate an intention to treat effect, where the treatment of conscription into forced labor includes both the forced labor experience as well as the need to go into hiding to avoid transportation to Germany.

Using Dutch census data from 1971, when treated individuals were around 49 years old, I find that individuals born in the conscripted cohort have lower labor market success compared to those born before. They are 0.64%p. less likely to have finished secondary education, their yearly income is lower by 1% compared to the average income (or 2.1% of one standard deviation), and their likelihood of being employed is 0.68%p. lower, which translates to 2.9% of one standard deviation[5]. I find no differences in family formation, both for marital status as well as the probability of having children.

To be able to differentiate between the two types of treatment, 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[6]. For each Dutch municipality, I construct

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

[4]I focus on the lower cutoff date r of the conscription period, because individuals born after the cutoff date of December 31, 1924 were the first cohort drafted for the Indonesian War of Independence in 1946, with around 30% of the cohort being drafted (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022), thereby violating the assumption of nothing else changing at the cutoff.

[5]The point estimate for income is -0.0289, where income is measured as yearly income in brackets of 4,000 Dutch guilder (2,916 EUR in 2024), meaning that on average the conscripted individuals' income is lower by 115 Dutch guilder (83 EUR in 2024).

[6]The data from the Arolsen Archives includes hand-transcribed records on the workers' name, place, and date of birth, and their location in Germany. I cleaned the hand-transcribed places of birth to match the Dutch municipalities in the census data, and classify an individual's gender using their first name and

a measure of the share of conscripted individuals who went to Germany to differentiate between which type of treatment was more likely, forced labor or hiding. I then allow for heterogeneous effects and find that the effects are similar for both groups, 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 on forced workers' location in Germany from the archival records, I show that the negative consequences on later labor market success are driven by individuals who had a higher exposure to severe living conditions. I proxy the exposure to severe 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 severe conditions to each Dutch municipality, based on where in Germany forced workers from that municipality where located. I find that the lower probability of being employed is driven solely by conscripted individuals who were exposed to more severe conditions in Germany, suggesting that the traumatic experiences have a lasting negative effect on labor market participation, possibly due to adverse health effects. I then directly study the effects of the treatment on physical and mental health to corroborate this finding. Indeed, the likelihood of needing assistance in daily life in 1971 is higher for conscripted individuals from places with higher exposure to severe conditions. 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.

I find that the loss in relevant labor market experience due to forced labor is also 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 being forced to work in sectors similar to the ones in a persons' place of origin. Specifically, I allow for heterogeneous effects based on the similarity in employment shares in the Dutch municipality of birth compared to the average employment share in German counties where

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.

people from that municipality were located while in Germany.. This finding could be driven by a lower loss of relevant labor market experience for individuals who had the possibility to continue working in the sectors that they were forced to work in while in Germany.

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

My results show important consequences of the disruption due to forced labor conscription. Yet, they still only constitute the lower bound of the social costs of forced labor conscription in a more generalized sense because the Dutch civilians were treated relatively better than forced workers of other nationalities. This was also mirrored by German policymakers when deciding on compensation for affected former forced workers in the early 2000s, as they excluded former forced workers from Western countries, including the Netherlands, due to a lack of discriminatory living conditions (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). My findings contradict this assessment, showing that especially those Dutch forced workers who faced more severe conditions in Germany did suffer from long-lasting effects on their labor market success and their health.

My paper contributes the literature on the consequences of different types of disruptions of a person's labor market career. Most related are studies looking at conscription into the army (Angrist, 1990; Blattman and Annan, 2010; Imbens and Klaauw, 1995). I contribute to this literature by studying conscription into a different type of forced work, not just focusing on the military sector. The exogenous assignment of forced workers into sectors and locations in Germany allows me to identify which parts of the forced labor experience drives the effects. 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

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

the labor market affects an individuals' labor market success, related to previous papers on work displacement due to plant closures (Huttunen et al., 2011; Ichino et al., 2017).

This paper also contributes to the literature on forced migration by studying a setting where forced migrants were able to return to their home country, which gives insights the effects of temporary displacement, and excludes that effects are driven by the ongoing exposure to conditions in the destination location (Bauer et al., 2013; Bauer et al., 2019; Becker et al., 2020; Becker, 2022; Sarvimäki et al., 2022).

More broadly, this paper is also related to the literature on consequences for labor market outcomes of facing other adverse events, such as natural disasters (Deryugina et al., 2018; Schwank, 2024), war (Braun and Stuhler, 2023; Kesternich et al., 2014) and hunger (Ramirez and Haas, 2022).

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). Persistence is thus often driven by the institutions that were shaped by the forced labor systems. In the case of Dutch civilians being coerced into labor in Germany, both the group intended for the treatment and the control group lived under the same institutions in the Netherlands after the end of WWII, but only differ in their exposure to forced labor conscription. I can therefore separate the effects of forced labor conscription for individuals from the effect of forced labor systems on institutions

As the conscripted individuals were around 21 years old at the onset of the forced labor policy, my study also contributes to the literature on the consequences of labor market conditions at early stages of a persons' labor market career (Oreopoulos et al., 2012; Schwandt and von Wachter, 2020; von Wachter, 2020). I contribute to this literature by showing that both an involuntary absence from the labor market due to hiding, as well as labor market experience in sectors that a person did not freely chose themselves and that is not relevant for their later labor market career, have long-lasting consequences for later labor market success.

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. The Yearclass Action was initially scheduled to include all men born between 1908 and 1925, but due to concerns of turmoil in the Dutch population because of the unpopularity of the conscription of age groups, the other birth cohorts were ultimately not called upon for forced labor. Coercion was executed by withholding food ration cards and forbidding firms to employ men from these cohorts.

In total, at least 77,200 Dutch forced workers born between 1922 and 1924 were recorded in Germany during WWII in the data by the Arolsen Archives. Comparing that to a total of around 210,500 men born in these years according to the 1971 census, compliance was at least around 37%. 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 (hsv, 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[8] and only men from the conscripted cohort were forced to systematically go into hiding until the end of the war to avoid forced labor[9][10].

The conscripted individuals who were taken to Germany were distributed irrespective of their skills or previous training and over large parts of Germany. Figure **??** shows the regional distribution of Dutch male forced workers born in the conscripted years 1922–1924 over the German counties. The men were assigned quasi-randomly into the specific locations and industries, based on local labor shortages at the time of their deportation that

[8],

[9]The numbers 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 1971. The share of men who went into hiding is derived by subtracting the the share of men who were granted an exemption, which is taken from German records from 1943 and available separately for each cohort.

[10]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).

companies reported to their local *Arbeitsamt* (employment office). The administrative effort of recording previous skills and training and assigning workers based that was deemed too costly (Kuck, 2010; Marx, 2019). The majority of the Dutch forced workers were employed in 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 was often of low quality, the access to medical care was scarce or non-existent, and both deteriorated as the bombing of the allied forces intensified. 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 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 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)[11]. After the successive liberation of Germany in 1945, the Allied Forces organized the transports of former forced workers back to their home countries. By September 1945, 98% of all Dutch persons present in Germany at the end of the war had returned to the Netherlands (Grüter and Mourik, 2020; Proudfoot, 1957). Figure 3 also shows that the number of men born before and after the cutoff of conscription is not

[11]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%.

different in 1971, confirming no significant differences in survival probability and no gap of conscripted men who did not return. At their return, the forced workers faced stigma because their labor for Germany was seen as collaboration with the enemy. Therefore, most of them stayed silent about what happened to them during the war. Only in the 1980s, a public debate about the experiences of the forced labors began in the Netherlands (Kuck (2010))[12].

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 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[13].

In 2000, the German government set up a fund to pay compensation to former forced workers of occupied countries. Depending on the severity of the treatment, individuals were paid between 572 EUR and 7,760 EUR depending on the severity 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

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

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

and discriminating living conditions", except from 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 a 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[14]. To identify 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 the years of 1922, 1923 and 1924. The control group are 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 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)[15]. The sample then includes 356,681 observations.

To measure labor market success, I use a dummy variable for finishing secondary education, yearly labor income (reported in 6 different income brackets with a range of 4,000 Dutch guilder (2,916 EUR in 2024)), and a dummy variable of whether a person is employed. I also use the marital status and a dummy for whether an individual has children as a proxy for their social situation, and a person's need for assistance in everyday life as a proxy for their health. Table 1 shows the descriptive statistics of these variables[16][17].

[14]The non-response rate was 0.2%

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

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

[17]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.

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.

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. The archive evolved from the International Tracing Service (ITS) established by the Allied forces, and its aim is to document and trace victims of the Nazi regime. The majority of the data originates from registration efforts by the Allied forces after WWII to organize the transport of the displaced persons back to their country of origin. I use data on so-called displaced persons, who are defined as individuals who had been deported by the Nazi regime (Höschler and Panek, 2019). While these do include prisoners of war and former inmates of concentration camps, the vast majority of them are forced workers[18]. 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

[18] 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)

of individuals. I follow Abramitzky et al. (2021) and adjust their algorithm slightly to exploit the data structure of the archival records[19]. This reduces my sample to 473,406 individuals.

To restrict the archival records to male individuals, I exploit the information on a persons' 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 construct a measure of how likely a name belongs to either gender (Meertens Instituut, nd)[20]. For the relevant cohorts of 1922–1924, there are 84.2% male and 10.7% female individuals[21]. 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[22]. 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 municipalities[23]. I am able to link the place of birth for 82.6% of all 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 by comparing the sample size of linked individuals from the archival data to the sample size of conscripted individuals in the census. Figure 7 shows the regional distribution of the likelihood to have a high share of conscripted workers to be deported to Germany[24]. There appears to be no regional pattern, meaning that the probability to avoid conscription by going into hiding does not seem to be driven by regional characteristics.

3.3 Eurobarometer

To investigate consequences on mental health, I use Eurobarometer survey data which includes a question on self-reported life satisfaction. The Eurobarometer is a survey conducted

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

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

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

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

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

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

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)[25]. Since I only know an individuals' 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[26]. The variable for mental health is the subjective life satisfaction (ranging from 0 to 3)[27]. I also repeat the analysis on labor market success to ensure the validity of the Eurobarometer survey data, using years of education (ranging from 7 to 15), labor income (reported in 12 different income brackets with a range of 250 Dutch guilder (182 EUR in 2024) and a dummy for whether a person is employed. Table 4 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 severe conditions in Germany, and data on the industry structure in Germany and the Netherlands.

3.4.1 Exposure to severe conditions

I proxy severe 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

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

[26]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.

[27]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?"

forced to clean up after the bombings. Second, I use the distance to so-called labor education camps, to which forced labor were sentenced in case of disobedience. The idea is that forced workers located close to a labor education camp faced a higher probability to be 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 4a 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 4b 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 persons' 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 persons' 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 likelihood with which forced workers located in these counties worked in the respective sector. The data distinguishes between 28 different sectors[28]. Figure 5 shows the distribution of the average employment share in agriculture, industry and services over

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

the German counties. My analysis uses variation over all 28 sectors. 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 the forced workers were appointed to sectors in a similar way to German workers. 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).

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 28 different sectors present in the German data. Figure 6 shows the regional distribution of the employment share in agriculture, industry and services[29]. 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

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

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 individuals' date of birth by using a fuzzy Regression Discontinuity Design (RDD) 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)[30].

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)[31]. 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

[30] 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 persons' age is related to a 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.

[31] 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 also have been affected by forced labor conscription and thus are not suitable for checking smoothness at the cutoff.

was not based on age (Sijes, 1966)[32]. The oldest cohort conscripted for the Indonesian War of Independence was the one of 1925 (NIOD Inst. v. Oorlogs-, Holocaust- en Genocidestudies et al., 2022)[33]. Both 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[34].

The second identifying assumption of RDD is that individuals cannot manipulate the running variable and thereby induce endogenous sorting around the cutoff. A persons' 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 chose, 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

[32]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)

[33]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).

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

without contact to their usual social environment, living in fear of being found, and often having no formal employment (Warmbrunn, 1972)[35].

Some individuals born within the conscripted years were granted an exemption and had to endure neither forced labor nor going into hiding[36], 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 conscription, using the 1971 census. The estimation equation takes the following form:

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

Y_i are labor market outcomes, specifically educational attainment, income bin and employment status. $MonthofBirth_i$ 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 going into hiding. I include a linear and a quadratic term of the running variable $MonthofBirth_i$ following Gelman and Imbens (2019)[37]. I use a bandwidth of 15 months, which is the me-

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

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

[37] 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 12.

dian 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)[38].

4.1.2 Results

Figure 10 shows the average outcomes for each month of birth and the corresponding function estimated using equation (1), with a bandwidth of 15 months[39]. I find that the probability to finish secondary education is lower by 0.64%p., which corresponds to 1.6% of one standard deviation. The yearly income is lower by 0.0289 levels. As income is classified into bins of 4,000 Dutch guilder (2,916 EUR in 2024), this implies that forced labor conscription lowers the yearly labor income by 115 Dutch guilder (83 EUR in 2024), 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. The probability to be employed is lower by 0.69%p., which is 2.9% of the standard deviation. So individuals subject to the labor conscription policy are performing significantly worse on the labor market in terms of education, their probability to be employed, and their income. In terms of education, 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. Comparing the effects to studies on conscription into the military, which find a reduction in earnings of 5% (**?**) and 15% (**?**), the reduction in income of 1% of the average income appears to be quite small. However, 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 probably poses a larger disruption compared to the additional hardship due to hiding and forced labor while living through an ongoing war. **?** study the effect of war experience of conscripted men in Germany during WWII (born 1919-1921), which is a more comparable setting, and find that war captivity and displacement do not affect employment probability, and only war injuries lower employment probability. The effect becomes negative

[38] I perform robustness checks using different bandwidths and a uniform kernel, see figure 12.
[39] The underlying regression results are shown in table 2

only once people reach their early 50s, and is around 3%p. at age 50. Compared to that, my finding that individuals faced with forced labor conscription are 0.69%p. less likely to be employed at age 49 is in a similar range. Moreover, 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 the ones that i find for Dutch civilians. Moreover, 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 12 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[40], and including individuals from the Hunger Winter regions[41].

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 13 shows the results of this placebo analysis[42]. I find insignificant results with estimates close to zero for all placebo specifications for income and employment probability. For finishing secondary education, two of the four placebo estimates become positive and significant, suggesting that maybe something else changes at the cutoff

[40] I run Logit for the dummy dependent variables of finishing secondary education and being employed, and Poisson for income, which is a count variable of 6 different income bins.

[41] Only for education, the effect using the specification with half of the optimal bandwidth becomes zero. All other estimates remain negative, the majority also retaining significance.

[42] I estimate this placebo exercise for four years prior to conscription where on both sides, there are three cohorts which only belong to the control group to mirror the three years of conscription period. Based on these three-year-windows, I again calculated the median optimal bandwidth.

of a new year that may affect education.

4.2 Family Formation

So far, I have looked at the consequences of the disruption of forced labor conscription on a persons' labor market success. To understand what consequences this disruption has on an individuals' social life outside of their labor market experience, I look at marital status and parental status, i.e. the probability to have children.

Figure 11 shows the average likelihood to be married and to have children as reported in the 1971 census in a 15 month bandwidth around the cutoff, and the results from equation (1)[43]. There is no discontinuous difference between the treatment and control group at the cutoff for both marital status and the probability to have children, meaning that in 1971, when treated individuals were aged around 49 years old, forced labor conscription does not seem to affect family formation[44].

4.3 Mechanisms

4.3.1 Forced Labor or Hiding

Until now, I estimated the intention to treat effect, where the treatment is a bundle of being forced to work in Germany and being forced to go into hiding. To disentangle these two factors, I conduct a heterogeneity analysis allowing for different effects based on which type of treatment was more likely, deportation to Germany or hiding. I proxy the likelihood 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. Since the ability to go into hiding may be in part driven by factors that could also affect

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

[44] Table 3 shows the full regression results.

labor market outcomes, the results are suggestive evidence[45].

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.

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. Figure 7 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[46].

I then split the sample by municipalities with a deportation share above and below median[47]. Individuals in the sub-sample with a above median deportation share thus have a higher likelihood 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 likelihood for going into hiding compared to individuals from the second sub-sample. I repeat the analysis for labor market outcomes by estimating equation (1), first for the new baseline of observations with a place of birth (thus those who still live in their municipality of birth), and then separately for the two sub-samples. The results are displayed in figure 14.

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 effects on education and income become insignificant and close to zero, which could be due to

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

[46] I checked the likelihood to have a place of birth based on date of birth and find that the share is overall flat, so no indication of a sample selection (see figure 21 in the appendix). I thus assume that the likelihood to be linked is random.

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

the endogenous sample selection: While the treatment itself does not change the likelihood of a person to still live in their municipality of birth (see figure 22 in the appendix), 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 education and 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.

4.3.2 Exposure to severe conditions

For the disruption in the form of forced work in Germany, I am able to open the black box of what people experienced by exploiting the archival data on locations of former forced workers. I conduct a heterogeneity analysis based on severity of the forced labor experience to understand whether being exposed to harsher circumstances during the forced labor period may be a contributing factor for 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 severity of the forced labor experience is thus exogenous.

I proxy severity of the forced labor experience 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:

$$severity_m = \frac{\sum_{c=0}^{C} severity_c fw_{cm}}{\sum_{c=0}^{C} fw_{cm}} \quad (2)$$

where m is the Dutch municipality, c is the German county, fw_{cm} is the number of forced workers in county c who are born in municipality m[48], and $severity_c$ 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 of this measure is that it captures the average exposure of forced workers from each municipality to severe conditions in Germany. Figure 8 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 Germany.

I then split the sample based on whether an individual is from a municipality with an above or below median exposure to severe conditions as measured by $severity_m$ and estimate equation 1 separately for each sample[49].

Figure 15 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[50]. In both cases, the effects are more negative for individuals from Dutch places where forced workers were more exposed to severe 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 severe conditions in Germany (both for the share of houses damaged and the distance to labor education camps)[51]. This suggests that the severe living conditions while in Germany

[48] The number of forced workers is restricted to men born in the conscripted period.

[49] 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.

[50] 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 restricted to individuals with information on their municipality of birth.

[51] 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 actually positive.

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 likelihood of employment, I repeat the heterogeneity analysis with a direct measure of an individuals' 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[52]. I find that for the sub-sample with higher exposure to severe conditions in Germany, the treatment increases the likelihood to need assistance, which is in line with the interpretation that a severe forced labor experience led to worse health (see figure 16). Physical health is also worse for the individuals from the sub-sample with a higher likelihood of forced work instead of hiding (see panel a of figure 16), 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 at mental health as a possible contributing factor, I use Eurobarometer data to study the effect of forced labor conscription on life satisfaction. I estimate a simple difference equation of the intention to treat effect as this data only contains individuals' age:[53]

$$lifesat_{it} = \beta_0 + \Sigma_{t=1}^{T} \beta_{1t} Treat_{it} + \lambda_t + \epsilon_{it} \tag{3}$$

where $lifesat_{it}$ is a measure of life satisfaction as the dependent variable[54] and $labmda_t$ are wave fixed effects to control for slightly different ways in which the question was formulated. To ensure that this approach is valid, I replicated the results on labor market success and family formation using the same estimation setup. Figures 19 and 20 in the appendix show

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

[53]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).

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

that the results are overall comparable[55][56].

Figure 17 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 likelihood of PTSD is positively correlated with the likelihood of having been a forced worker during the war (Bramsen, 1998)[57]. 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.

4.3.3 Loss in labor market experience

In a 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 in which these young forced workers were allocated into 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

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

[56] As the data lacks information on a persons' place of birth, I cannot link it to the archival records to estimate heterogeneous effects by the severity of the forced labor experience.

[57] The likelihood of PTSD was 4% for former forced workers vs. 1.5% for other individuals. Note that the survey did not rely on exogenous variation in having been a forced worker.

calculating the weighted average employment share for each industry, based on where forced workers from these municipalities were located in Germany:

$$empshare_{mj}^{DE} = \frac{\sum_{c=0}^{C} empshare_{cj} fw_{cm}}{\sum_{c=0}^{C} fw_{cm}} \tag{4}$$

where m is the Dutch municipality, c is the German county, j is the sector, fw_{cm} is the number of forced workers in county c who are born in municipality m[58], and $empshare_{cj}$ is the employment share of sector j in county c. Figure 9 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 6 shows the distribution of the employment share in agriculture, industry and services in the Dutch municipalities.

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, $empshare_{mj}^{DE}$, and the employment shares of the respective municipality itself, $empshare_{mj}^{NL}$.

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 an below or above median value of the correlation between the German and the Dutch industries[59][60]. Figure 18 shows that the negative effect of forced labor conscription on the likelihood to be 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.

[58] The number of forced workers is again restricted to men born in the conscripted period.

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

[60] The median value is 0.55.

5 Conclusion

In this project, 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 work, 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.

Specifically, I find that individuals who were born after the cutoff of conscription have lower education, lower income and a lower likelihood to be employed. 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 who were exposed to harsher living conditions while in Germany. For this group, the forced labor conscription is also associated with worse physical health. Suggestive evidence points towards lower psychological well-being of conscripted individuals as well. Taken together, this implies that the forced labor conscription had negative consequences for labor market success due to adverse effects on an individuals' health and well-being. I also 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.

When applying my findings to contexts beyond the forced labor regime of Nazi Germany, a key policy implication is the need to provide adequate support to former forced workers upon their return to heir home countries to avoid that the possibly traumatic experiences lead to long-term disadvantages in the labor market. In the Netherlands, the former forced workers faced suspicion of collaboration with the enemy, leading them to remain silent about their experiences. This may have prevented affected individuals from seeking help, thus exacerbating and perpetuating their losses on the labor market. This is also reflected in a positive correlation between forced labor experience and PTSD in 1995 (Bramsen, 1998). 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

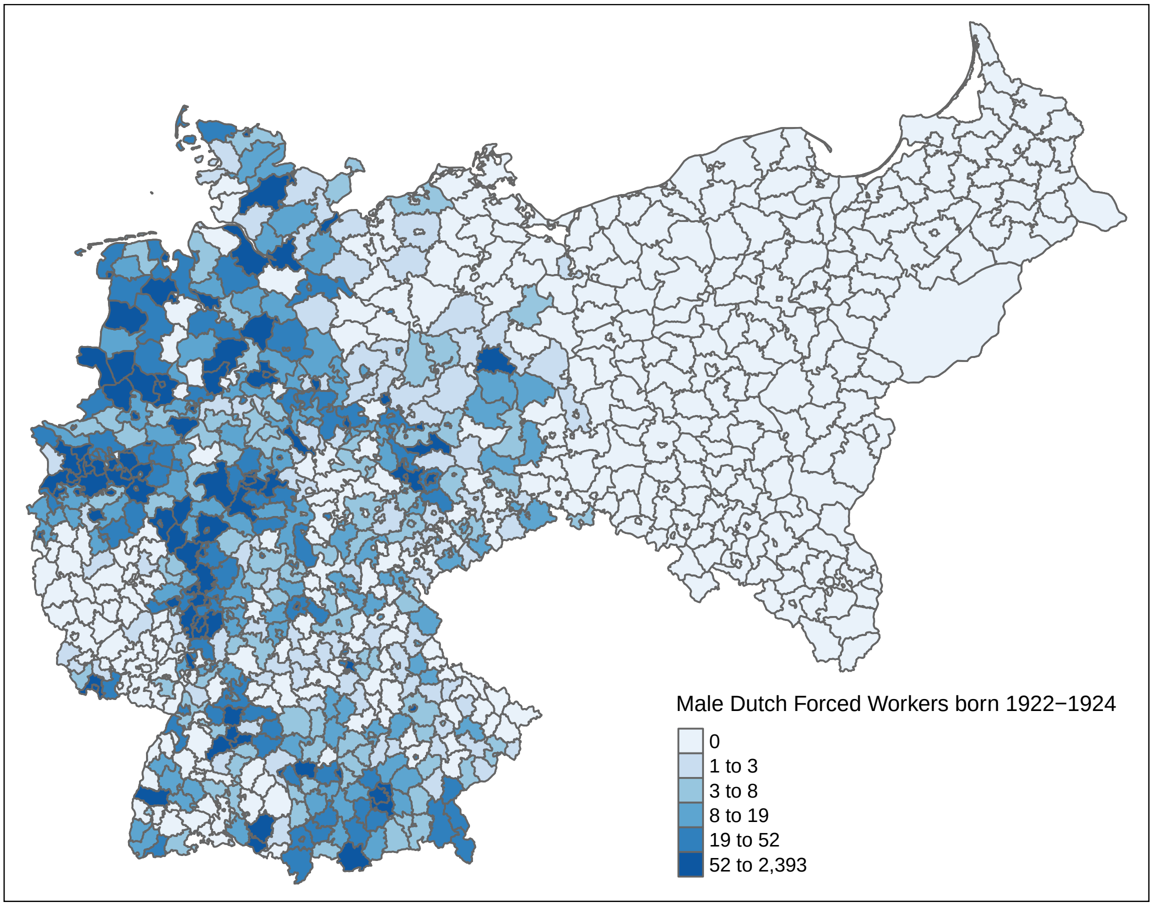


Figure 1: Regional Distribution of Male Dutch Forced Workers born 1922-1924 across German counties

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

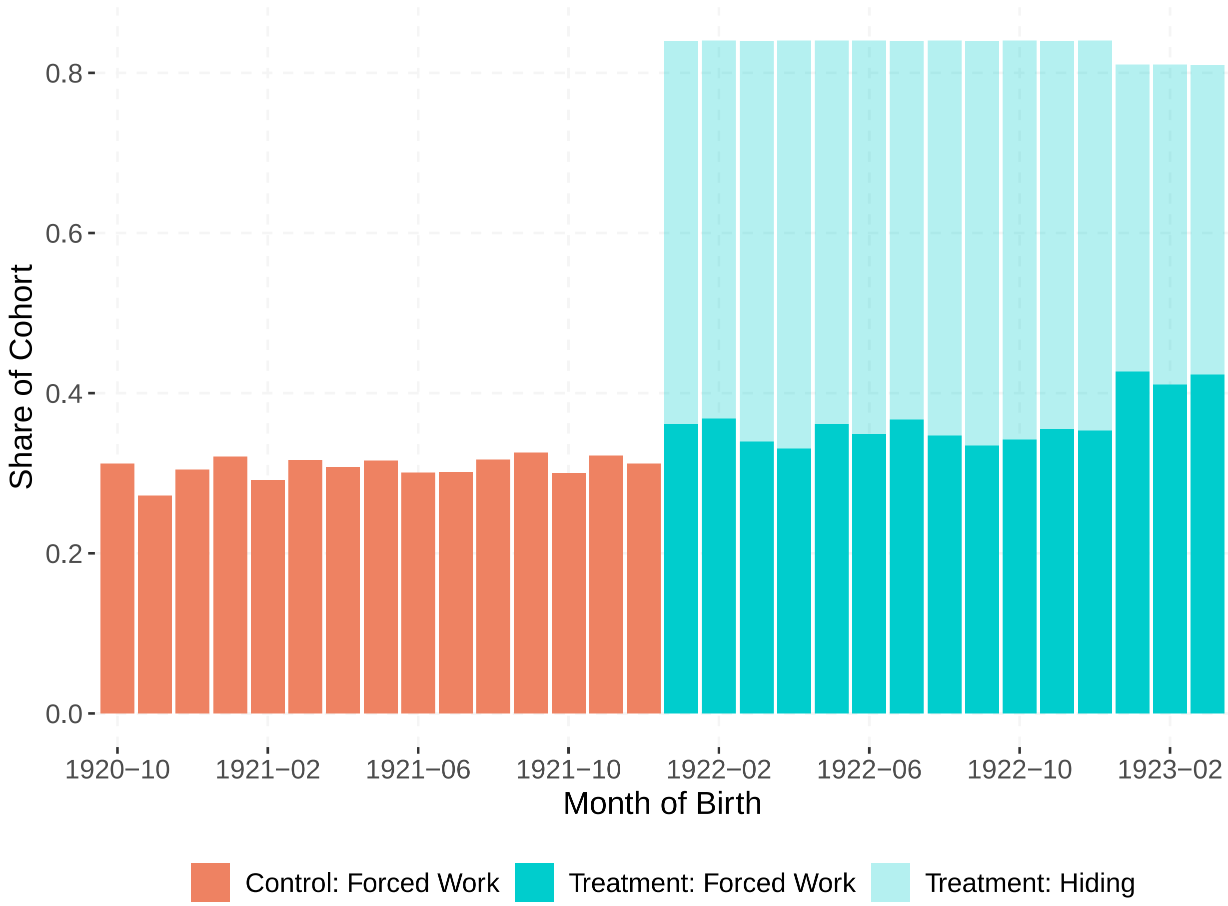


Figure 2: Number of treated individuals

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 numbers 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 number on the share of men who were granted an exemption taken from German records from 1943, which is available separately for each cohort.

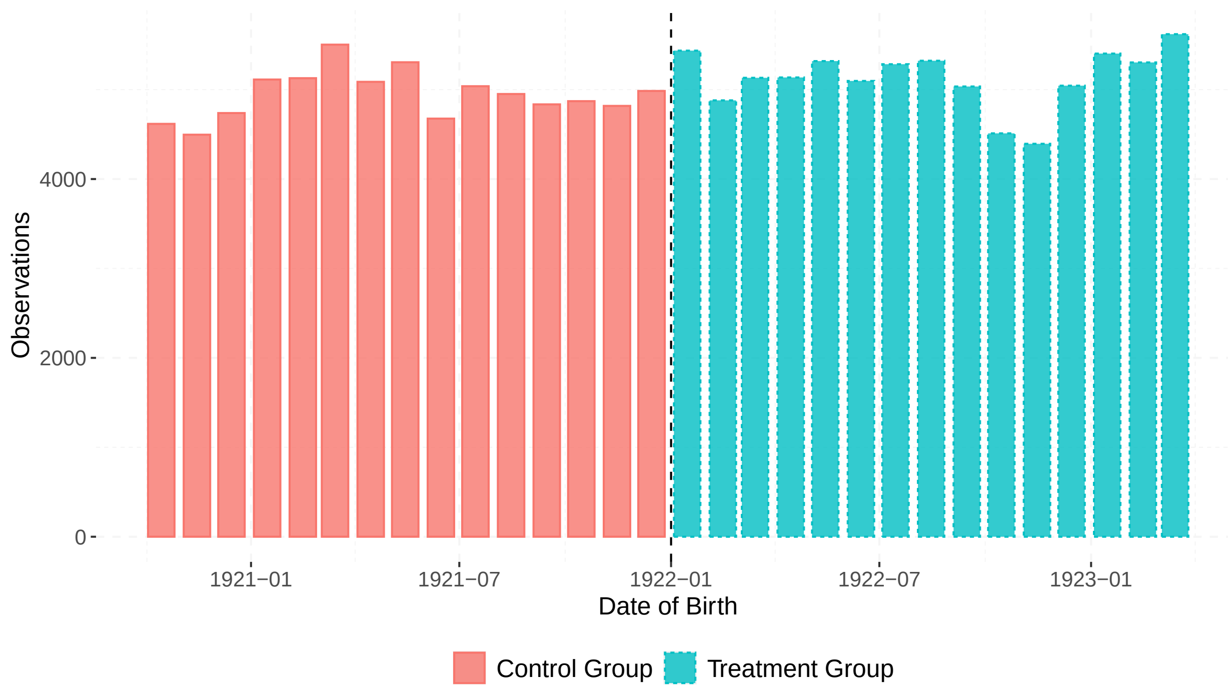
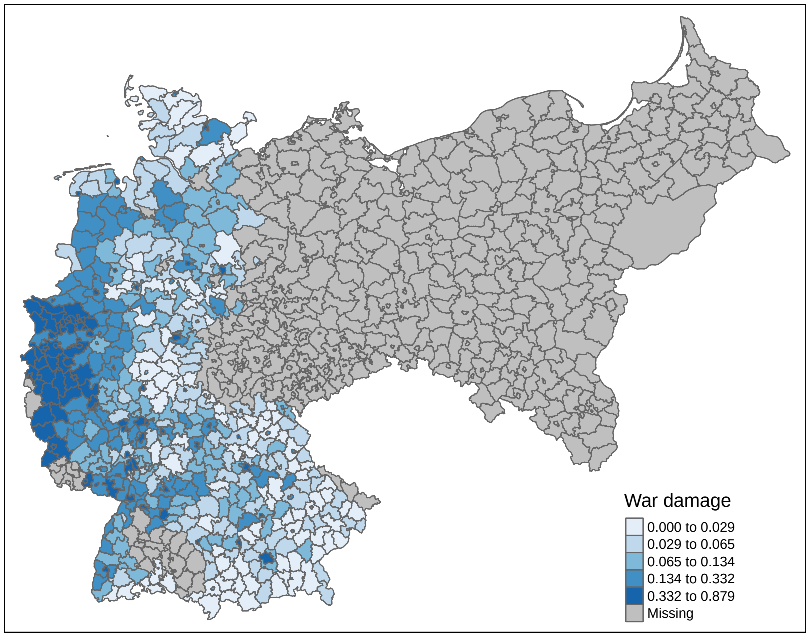
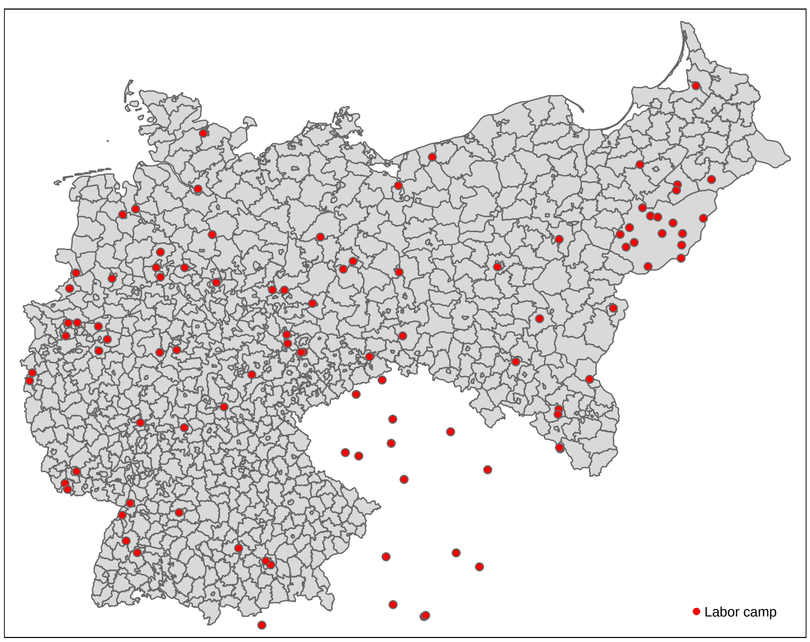


Figure 3: Number of male observations per month and year of birth based on the 1971 Census

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.



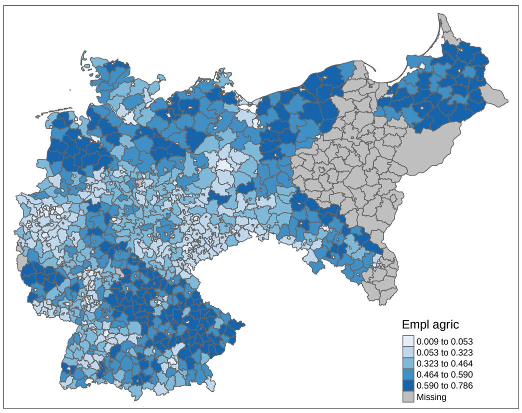
(a) Share of damaged houses across German counties



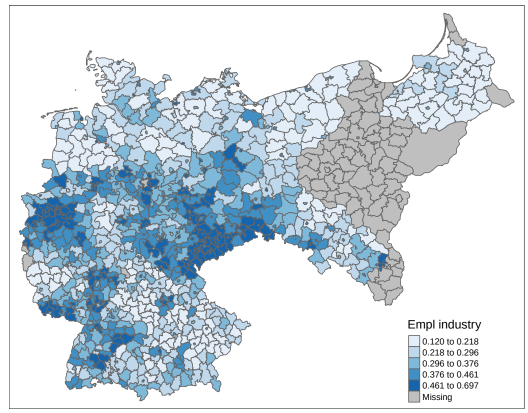
(b) Location of labor education camps

Figure 4: Regional distribution of severe forced labor conditions over German counties

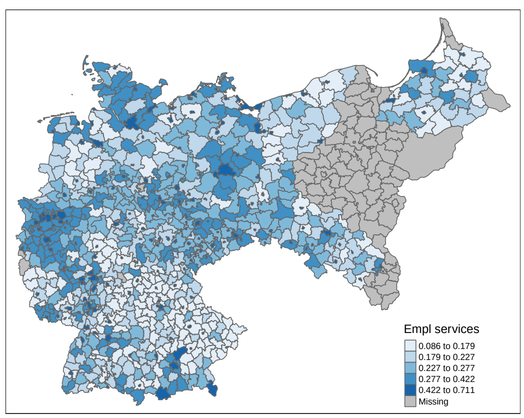
Notes. This figure shows the regional distribution of two measures for the severity of forced labor in Germany: Panel a shows the share of housing stock that was damaged during the war in German counties. Each color represents a quantile of the distribution of the respective variable. The data comes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



(a) Employment share: Agriculture



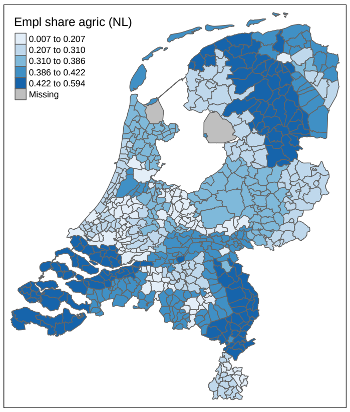
(b) Employment share: Industry



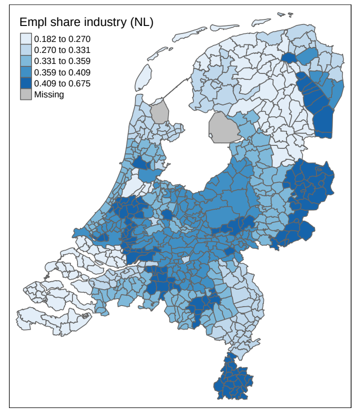
(c) Employment share: Services

Figure 5: Sectoral composition across German counties

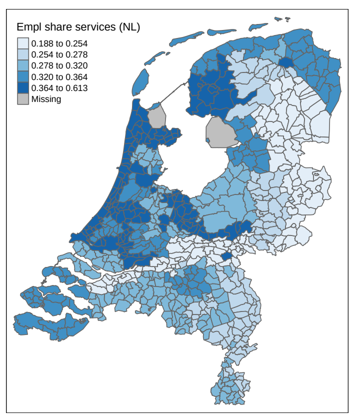
Notes. The figures show the employment share for agriculture, industry and services in 1939 over German counties. Each color represents a quantile of the distribution of the respective variable. The data comes from Braun and Franke (2021).



(a) Employment share: Agriculture



(b) Employment share: Industry



(c) Employment share: Services

Figure 6: 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 which did not yet exist in 1930.

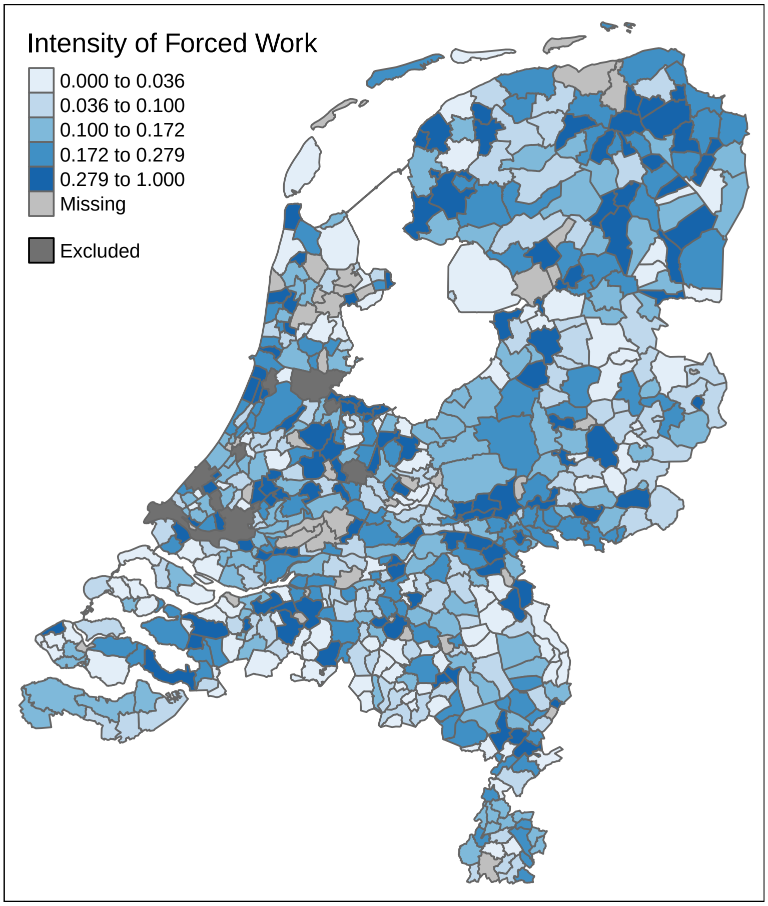
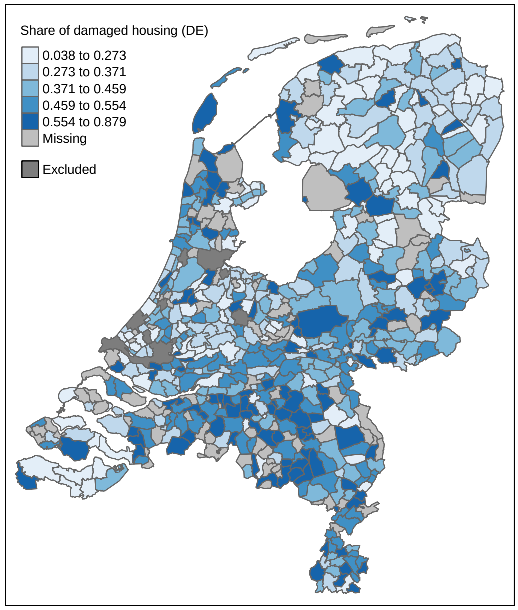
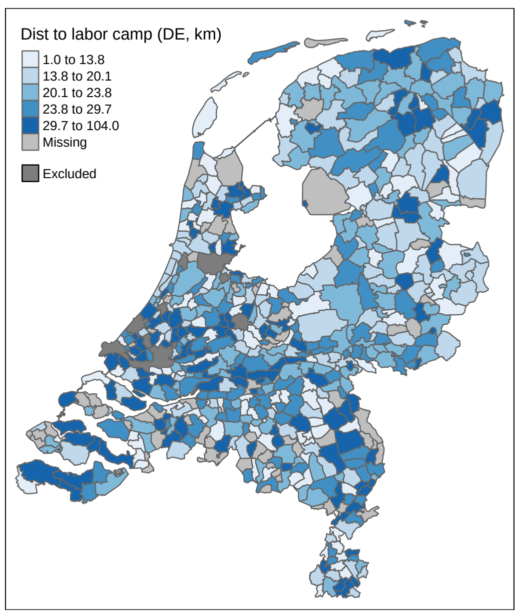


Figure 7: Regional Distribution of Intensity of Forced Work over Dutch municipalities

Notes. This figure shows the intensity of forced work 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 work 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.



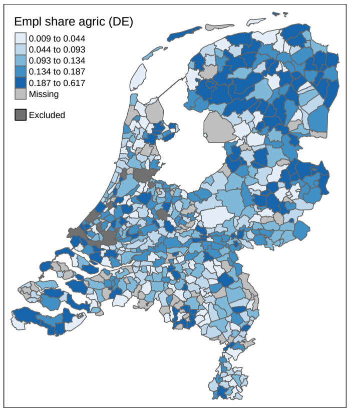
(a) Regional Distribution of average share of damaged housing in German locations over Dutch municipalities



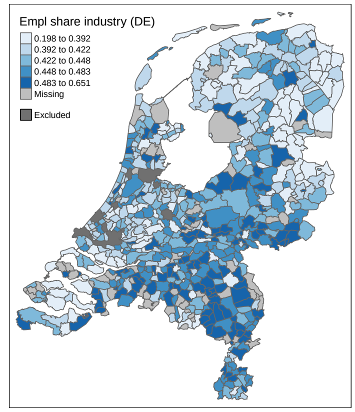
(b) Regional Distribution of average distance to Labor Education Camps from German locations over Dutch municipalities

Figure 8: Regional Distribution of Exposure to Severe Conditions over Dutch municipalities

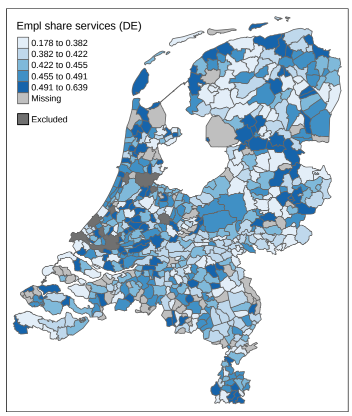
Notes. This figure shows the average exposure of conscripted forced workers from each Dutch municipality to severe conditions during their stay in Germany, measured as the average share of destroyed housing stock in German counties where forced workers from each municipality were stationed in panel a 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 work 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.



(a) Employment share: Agriculture



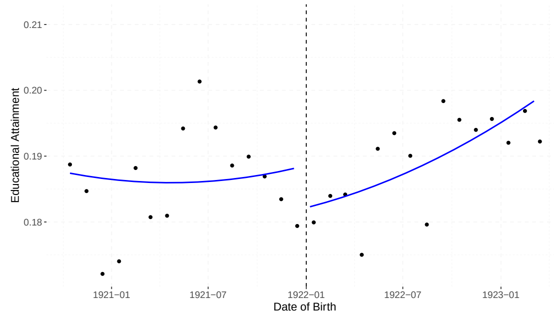
(b) Employment share: Industry



(c) Employment share: Services

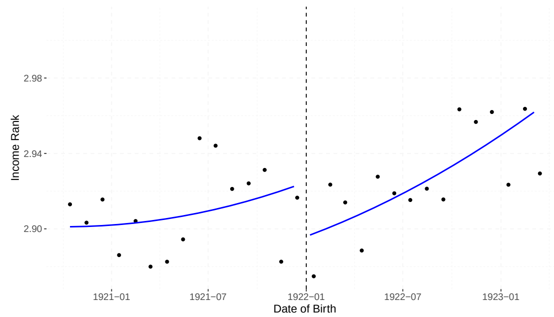
Figure 9: Average exposure to sectoral composition across Dutch municipalities

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.



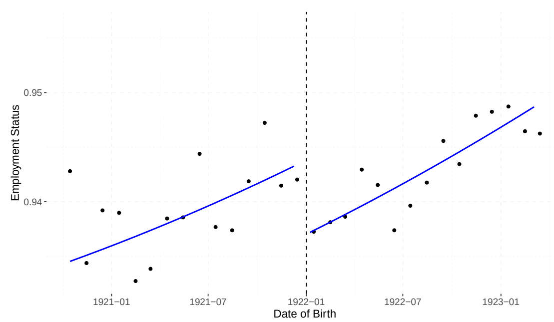
(a) Educational Attainment

Estimate: β_1 = -0.0064 (0.0038)*



(b) Income Class

Estimate: β_1 = -0.0289 (0.0126)**

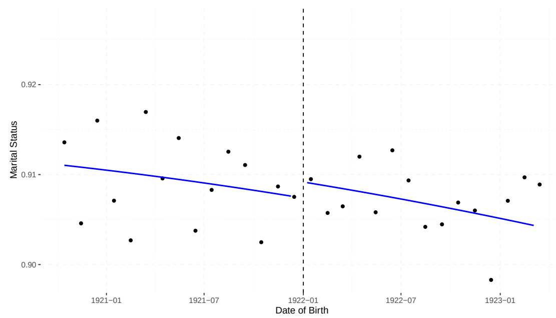


(c) Employment Status

Estimate: β_1 = -0.0068 (0.0021)***

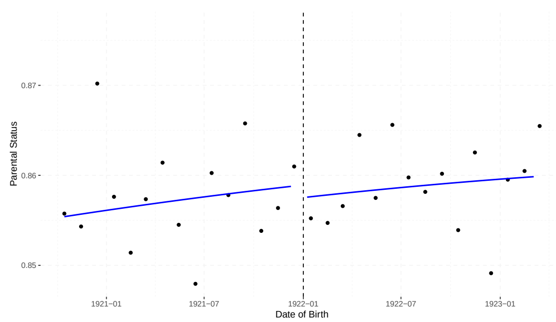
Figure 10: RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

Notes. The figures show the average of labor market outcomes based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the underlying individual-level data. Panel a shows effect on the probability of finishing secondary education, panel b shows the income class measured from 0 to 5 in steps of 4,000 Dutch Guilder, and panel c shows the employment status taking a value of zero or one. The y-axis is normalized to 10% of a standard deviation for each respective outcome.



(a) Marital Status

Estimate: β_1 = 0.0018 (0.0026)

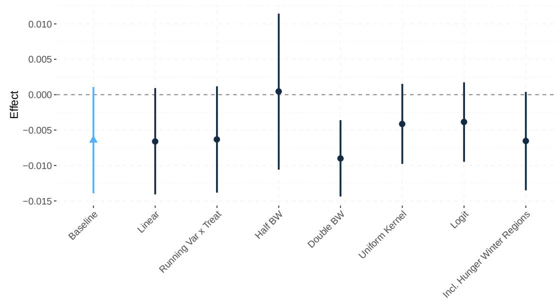


(b) Parental Status

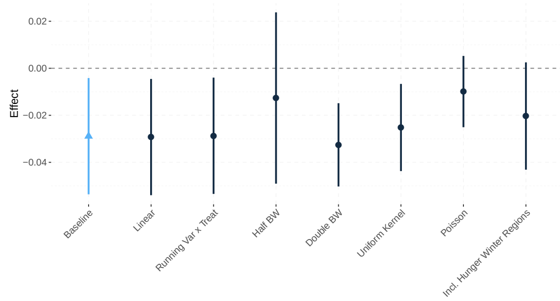
Estimate: β_1 = -0.0014 (0.0031)

Figure 11: RDD Effects of Forced Labor Conscription on Family Formation based on 1971 Census

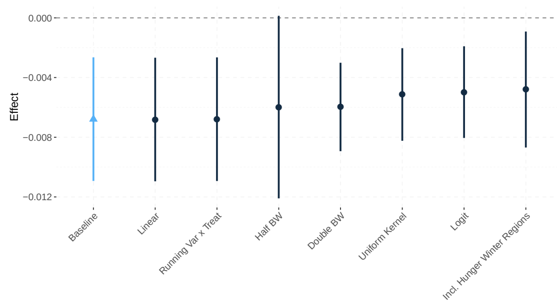
Notes. The figures show the average family formation outcomes based on the 1971 Census for each month and year of birth, and the regression line based on an RDD estimation using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two using the underlying individual-level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



(a) Educational Attainment



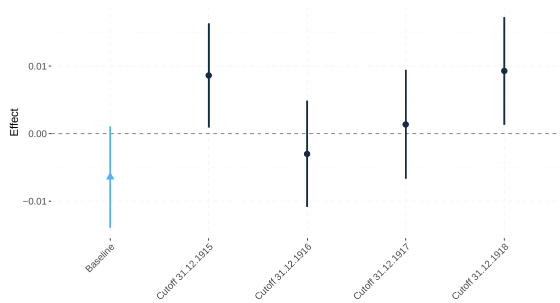
(b) Income Class



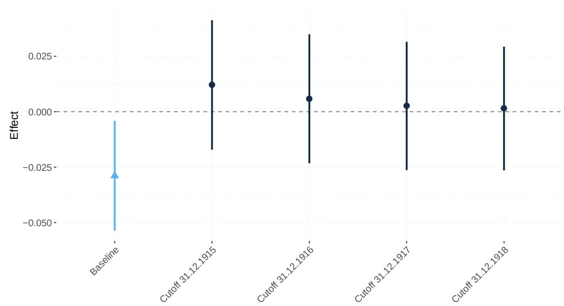
(c) Employment Status

Figure 12: Robustness of RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

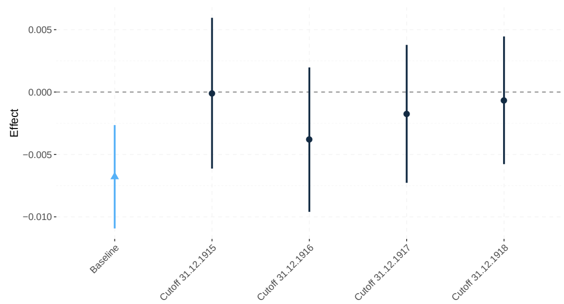
Notes. This figure shows RDD regressions using the 1971 Census with different specifications. The bars show the 95% confidence interval.



(a) Educational Level



(b) Income Class



(c) Employment Status

Figure 13: Placebo RDD Effects of Forced Labor Conscription on Labor Market Outcomes based on 1971 Census

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

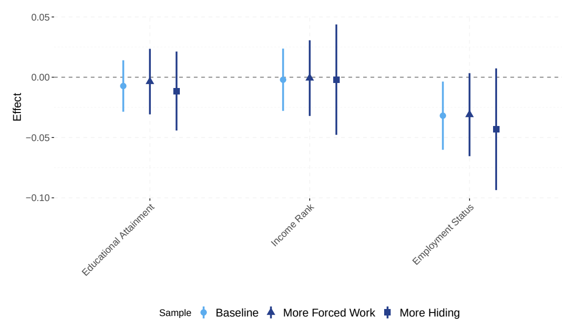
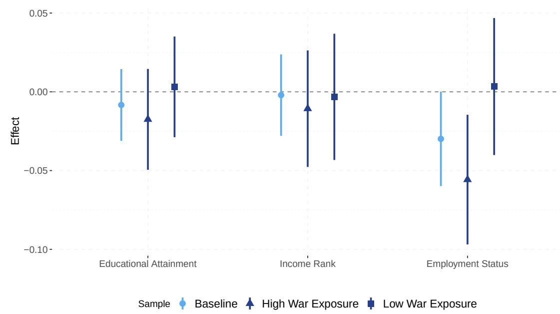
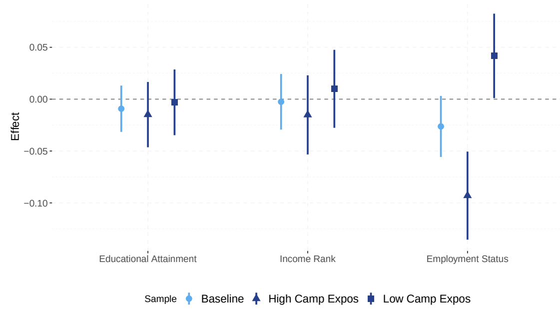


Figure 14: Heterogeneous RDD Effects of Forced Labor Conscription on Labor Market Outcomes by share of forced workers

Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for economic outcomes using the 1971 Census. 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. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable.



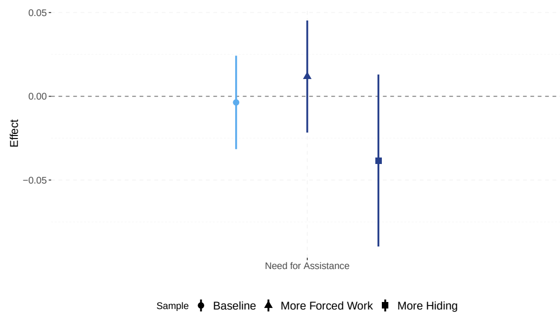
(a) Heterogeneity by share of damaged housing in DE



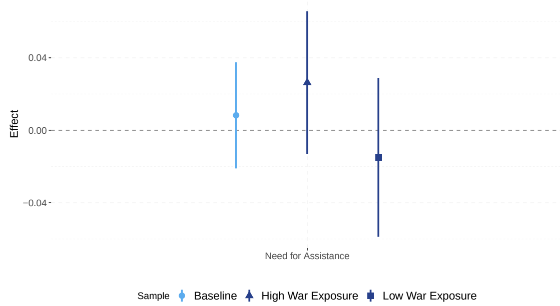
(b) Heterogeneity by distance to labor education camps in DE

Figure 15: Heterogeneous RDD Effects of Forced Labor Conscription on Labor Market Outcomes by Severity of Forced Labor Experience

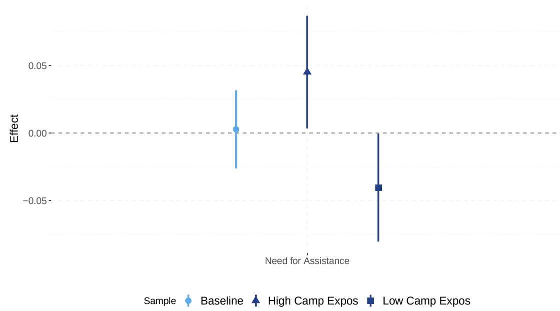
Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for economic outcomes using the 1971 Census with subsamples. In panel a, the sample is split by the median of the average weighted exposure of forced workers 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. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable.



(a) Heterogeneity by Compliance



(b) Heterogeneity by War Exposure in DE



(c) Heterogeneity by Camp Exposure in DE

Figure 16: Heterogeneous RDD Effects of Forced Labor Conscription on Need for Assistance based on 1971 Census

Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for the need of assistance in daily life using the 1971 Census with subsamples. In panel a, the sample is split by the median share of conscripted individuals from a Dutch municipality who can be found in the data provided by the Arolsen Archives. In panel b, the sample is split by the median of the average weighted exposure of forced workers from a Dutch municipality to houses damaged in West Germany. In panel 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.

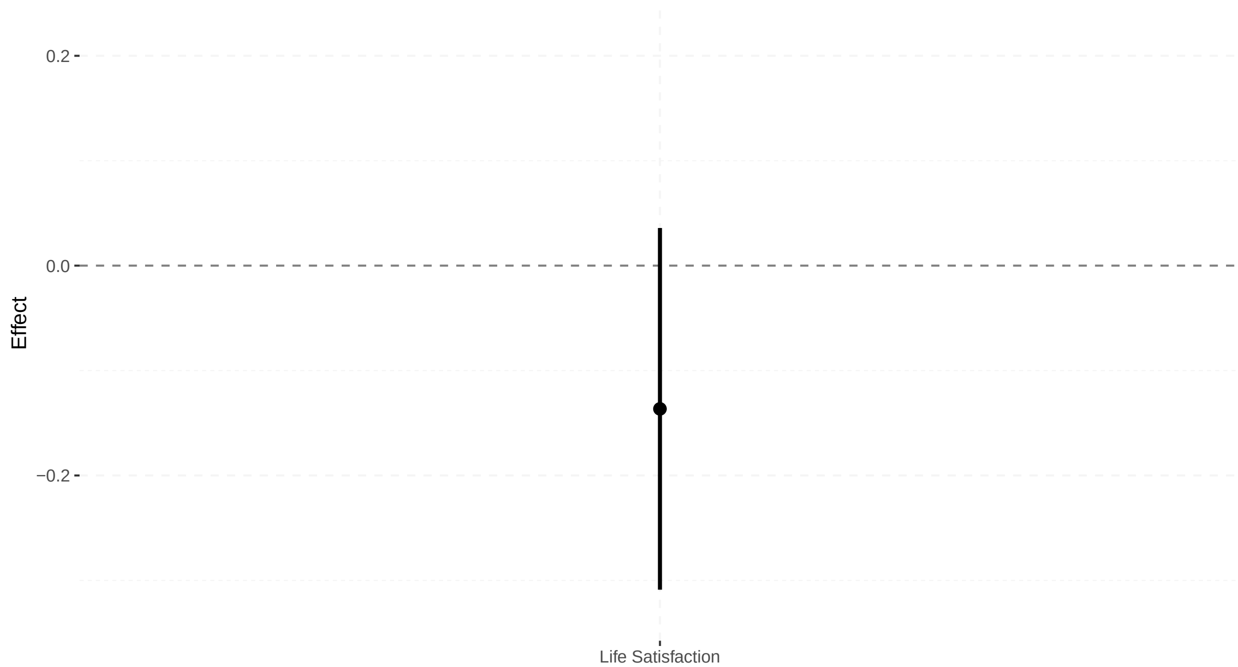


Figure 17: Effects of Forced Labor Conscription on Life Satisfaction using 1975-1944 Eurobarometer

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.

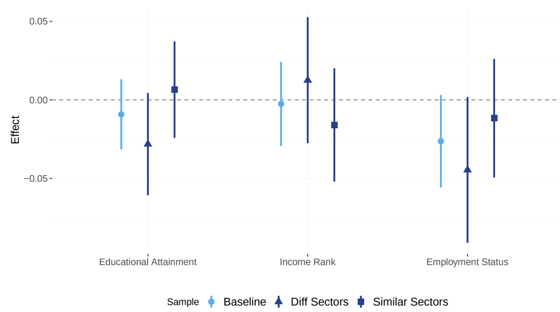


Figure 18: Heterogeneous RDD Effects of Forced Labor Conscription on Labor Market Outcomes by Similarity of Sectoral Composition in DE and NL

Notes. This figure shows RDD regressions using a 15 months bandwidth, a triangular kernel, and polynomial of the running variable of degree two for economic outcomes using the 1971 Census. The sample is split by the median of the difference in the employment share in Dutch municipalities and the average weighted employment share in German counties that forced workers from each Dutch municipality were exposed to. The bars show the 95% confidence interval. The coefficients and confidence intervals are normalized by the standard deviation of the respective dependent variable.

Tables

Variable	Levels	Mean Overall	Std. Dev. Overall
Educational Attainment	Finished Secondary Education: Yes/No	0.188	0.39
Income Rank	0-5 (4,000 Dutch guilder range)	2.918	1.362
Employment Status	Employed: Yes/No	0.941	0.235
Marital Status	Married: Yes/No	0.908	0.289
Parental Status	Has a Child: Yes/No	0.858	0.349
Need for Assistance	Yes/No	0.038	0.191
Observations		151080	151080

Table 1: Descriptive Statistics

Table 2: Labor Market Effects

	Dependent variable:		
	Educational Attainment	Income Rank	Employment Status
	(1)	(2)	(3)
RDD Estimate	−0.006*	−0.029**	−0.007***
	(0.004)	(0.013)	(0.002)
Observations	124490	141109	151080
Bandwidth	15 months	15 months	15 months
Dependent Variable Range	Finished Secondary Education: Yes/No	0-5 (4,000 Dutch guilder range)	Employed: Yes/No
Mean Dependent Variable	0.188	2.918	0.941
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3: Social Effects

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

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

Table 4: Descriptive Statistics

Appendix

5.1 Data

5.1.1 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 which are unlikely to change over time, namely a person's place of birth, name and age. To reduce computational requirements, only individuals with the same first letters of the first and last name, the same place of birth and an age difference of up to 5 years are compared (so-called blocking). 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' age, such as rounding of reported age and differences in age at different points in time of reporting. I therefore block on the date of birth and on the first letters of the first and last name instead[61]. 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[62][63]. The ABE-JW method links two datasets where every individual only shows up once in each dataset, so a possible match is only linked if it is unique, and there are not multiple entries which are close to the original. In my case however, I am linking observations to other entries from the same dataset, and links to multiple entries are plausible because a person may show up more than twice in the archival records. I therefore do not restrict links to only those entries which have only one plausible match. I then treat all linked individuals as only one observation going forward. Of the originally 594,967 observations, my algorithm links 121,561 observations to

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

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

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

another entry, leaving 473,406 observations of probably unique individuals.

Since the archival data does not include the gender of the individuals, I use the data on first names and combine this with information on name frequency based on gender from the Corpus of First Names in the Netherlands published by Meertens Instituut (nd). Of the 34,831 unique first names in my dataset, 11,802 (33.9%) are part of the Corpus of First Names. To include names with slightly different spellings, I calculate the JW-distance between first names and assign the same gender to a name with a sufficiently similar name that is part of the Corpus of First Names (a JW-string distance of up to 0.1, as suggested by Abramitzky et al. (2021)). This yields an addition of 17,417 names. In total, I can assign a gender probability to 29,219 or 83.9% of all unique first names. Based on this, I calculate the probability of a given name to be male or female and classify names for which at least 70% of individuals with that name are either male of female respectively. All other names are classified as uncertain. 78.9% of all unique first names can be classified as either male of female using the cutoff of 70%. Since some persons have multiple first names (either because they have a middle name, or because two observations with differently spelled names were linked to the same individual), I use the mode of each persons' first names' genders to assign their gender[64] 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.

5.2 Eurobarometer

I define the following treatment and control group, consisting of one age group respectively: The older control group consists of men born in 1920 or 1921, the older treatment group are men born in 1922 or 1923.

I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \tag{5}$$

where λ_t are wave fixed effects to control for differences in survey design between different waves. The outcomes of interest Y_{it} are years of education, income class, and employment status. Figure 19 shows the results. While I find no significant differences in years of education or income, I again find that individuals of the treatment group are 5.1% less likely to participate in the labor market which is 10.4% of one standard deviation. Figure 20

[64] 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.

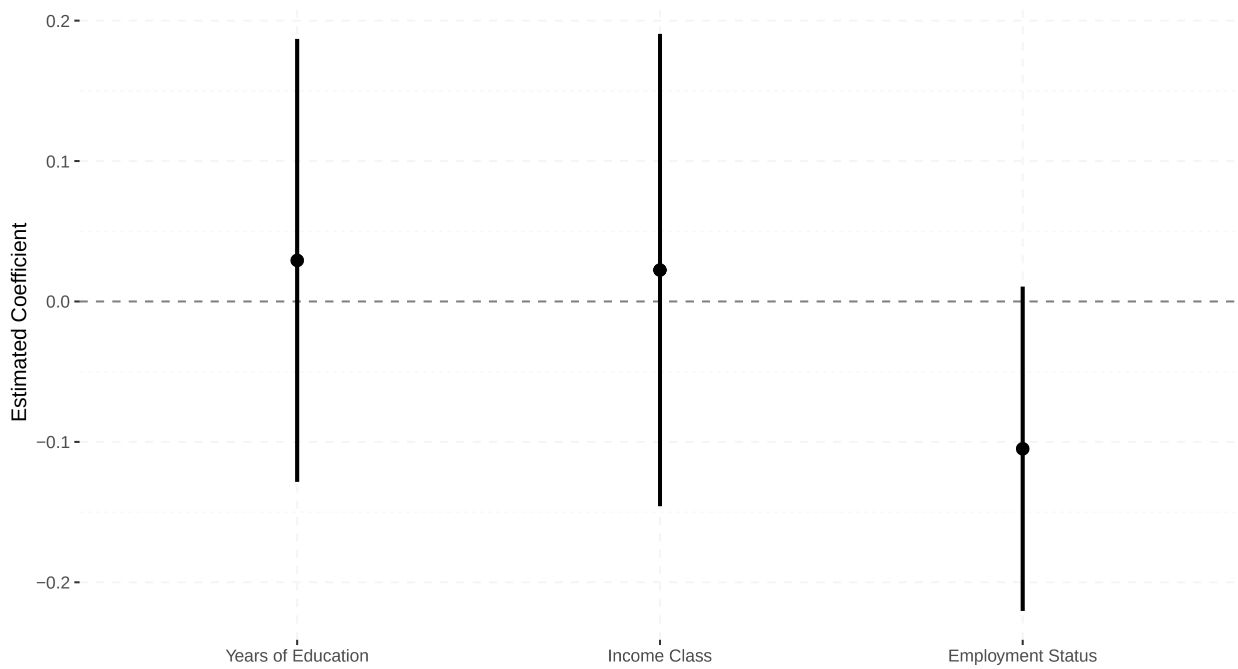


Figure 19: Effects of Forced Labor Conscription on Labor Market Outcomes using 1975-1944 Eurobarometer

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

shows the results for family formation (a dummy for being married and a dummy for having children), where I find no significant differences..

5.3 Figures

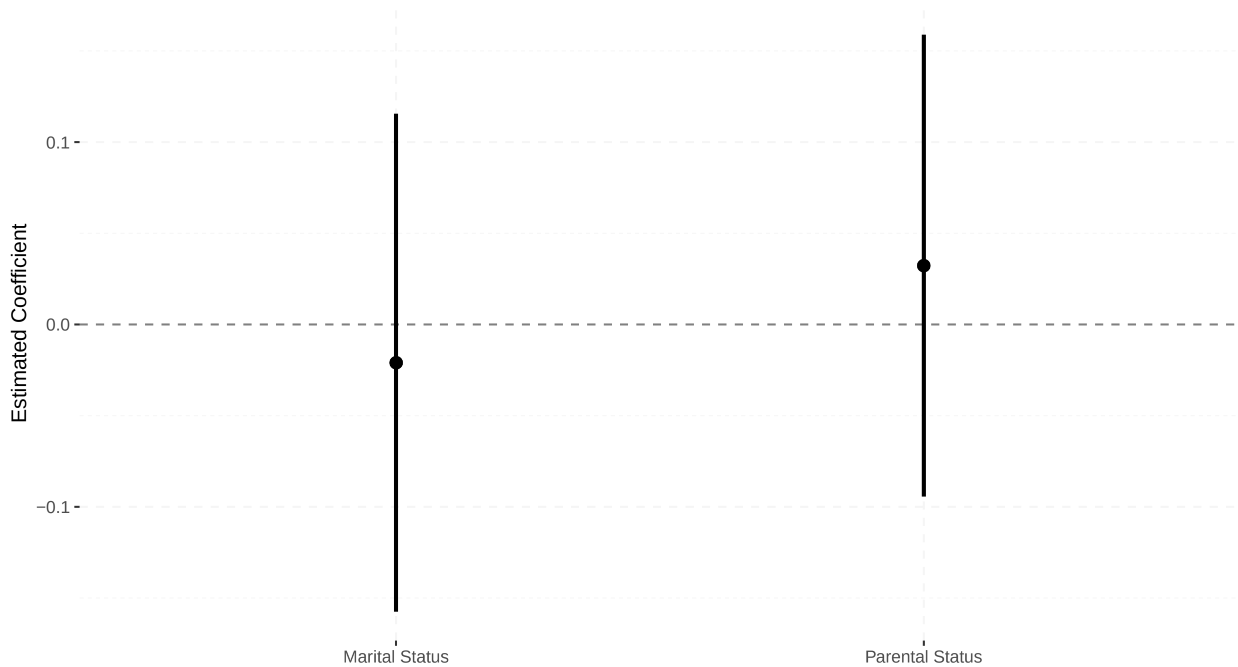


Figure 20: Effects of Forced Labor Conscription on Family Formation using 1975-1944 Eurobarometer

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.

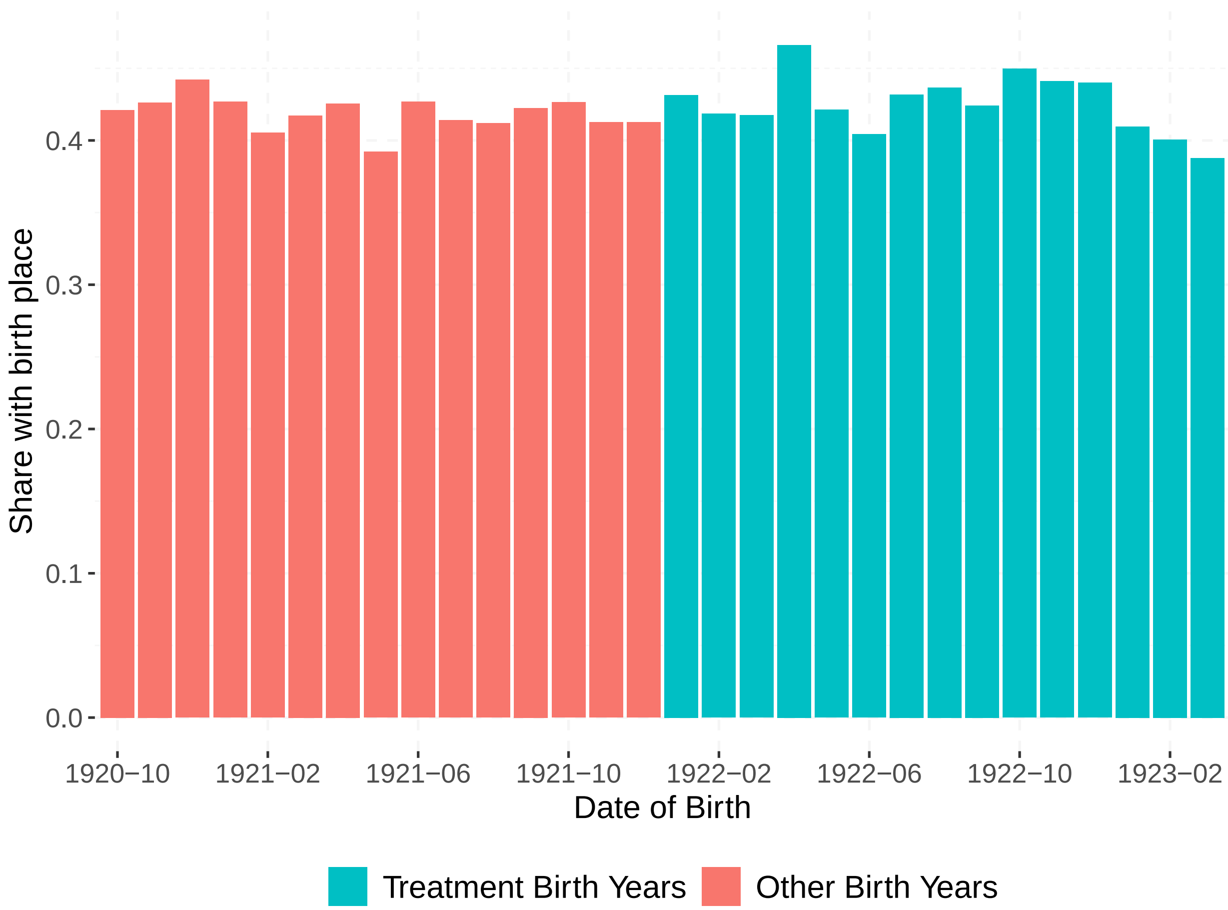


Figure 21: Share of Conscripted Men with Information on Place of Birth

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.

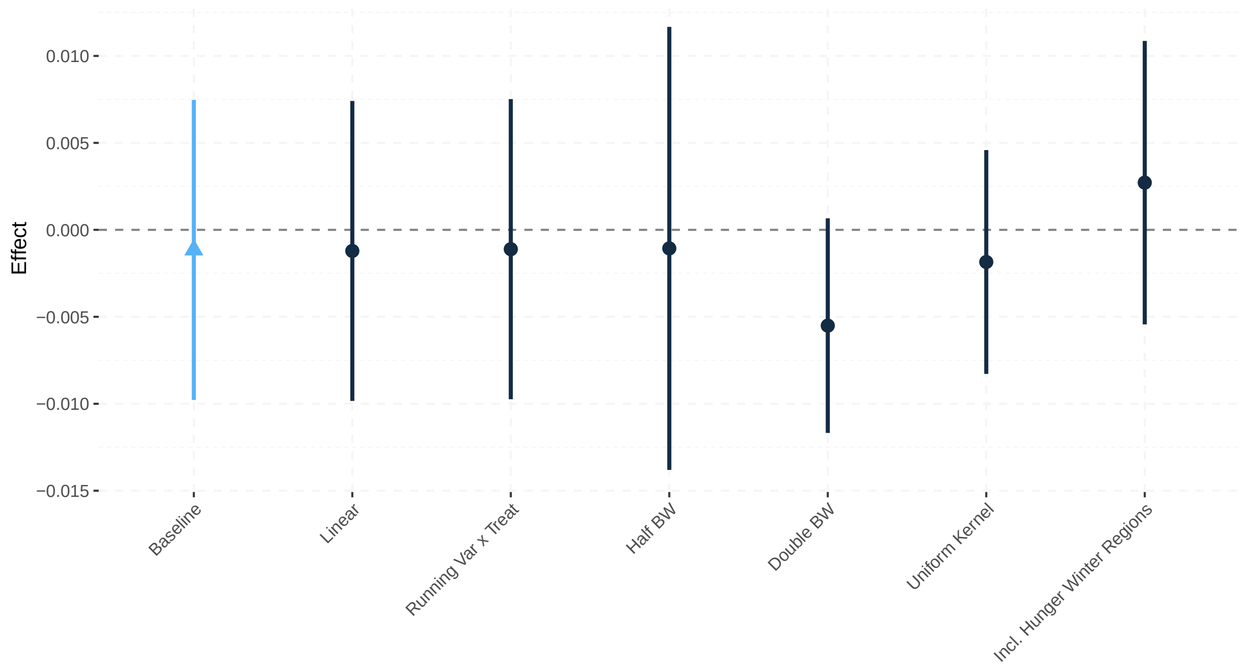


Figure 22: RDD Effects of Forced Labor Conscription on Moving based on 1971 Census

Notes. This figure shows RDD regressions using the 1971 Census with different specifications. The dependent variable is the probability to not live in the municipality of birth. The bars show the 95% confidence interval.