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**Tag der Disputation**:

*For Alex.*
*Thank you for cheering me up when regressions were not significant,*
*and cheering me on when they were.*

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# Introduction

This dissertation consists of three essays in the field of Economic History. The chapters study questions from political economy and labor economics from a historical perspective. In chapters 1 and 2 of this dissertation, I study processes of societal transformations to understand how disruptions impact the political economy. Chapter 1 focuses on the supply of the franchise by elites, while chapter 2 studies the demand for political participation. In chapter 3, I investigate how disruptions in a person's working life affect their long-term success in the labor market. In each chapter, I analyze extensive historical data with microeconometric methods.

In Chapter 1, which is co-authored with Erik Hornung and Noam Yuchtman, we study the disruption of social conflict in the form of widespread labor unrest. Specifically, we investigate how labor unrest influenced autocratic elite's willingness to share resources and power in early 20th century Prussia. During this period of rapid industrialization, the social contract was highly contested. Despite high levels of social conflict, with the labor movement mobilizing more than 300,000 workers in 1905 alone, the voting system in Prussia remained unequal until after WWI. Using detailed strike data, we show that in regions with more intense labor unrest elites became more inclined to support policies aimed at appeasing workers: First, we show that in cities with higher strike activity, the expenditure share for education and health increased. Second, we show that strike intensity induced members of the bourgeoisie to support democratic reform, more so for those who were ideologically aligned with the working class. Lastly, we find that where strike intensity and support by the bourgeois coincide, members of parliament were more likely to vote in favor of franchise reform. We control for a host of geographic and development factors that could influence both strike activity and elite's decisions, and use changes in expenditures to capture unobserved factors. In addition, we use a shift-share instrumental variable approach that maps industry-specific international commodity price changes to Prussian regions based their exposure to the respective industry. This chapter contributes to the literature by studying the effect of protest on policy in autocratic

settings (as opposed to democratic settings, see Ellman and Wantchekon (2000) and Archibong, Moerenhout, and Osabuohien (2022)), by focusing on a setting with high levels of mobilization, and by studying a broader set of policies as part of the social contract compared to previous studies (Aidt and Franck, 2015; Aidt and Jensen, 2014; Castañeda Dower et al., 2018; Lehmann-Hasemeyer, Hauber, and Opitz, 2014; Seghezza and Morelli, 2019; Ziblatt and Dasgupta, 2015).

Moving the focus from the supply of public goods and political participation by the elites, Chapter 2 now turns towards the demand for political representation by marginalized groups, focusing on women's voting rights. Women remain underrepresented in leadership positions in business and politics around the world. Recent literature has highlighted demand-side constraints as a contributing factor to this under-representation: Women are less likely to apply to leadership positions (Haegele, 2025) and to run in political elections (Gonzalez-Eiras and Sanz, 2021). This chapter, which is co-authored with Barbara Boelmann, contributes to this literature by studying the role of agency as a factor that shapes women's demand for political representation. We focus on the German suffragette movement in the early 20th century, and use the disruption of WWI which brought about an absence of missing men and thereby an increase in women's agency. We argue that, conditional on local industry and age structure, the variation in drafting of men was quasi-random. To make demand for representation measurable, we digitized panel data on the presence of local suffragette clubs. Using a DiD setup, we show that women in counties with more missing men were more likely to maintain a local suffragette club throughout the war. We argue that this is partly driven by reduced social costs for women who fight for the vote, brought along by women's large-scale organization of the war relief. In addition to an increase in demand for political participation, we show that the disruption of missing men also increased women's political participation once female voting rights were introduced. By focusing on the demand for political representation by a disenfranchised group, this chapter contributes to the literature that has previously studied the reasons why men extended voting rights to women (Arnsbarger, 2024; Bertocchi, 2011; Braun and Kvasnicka, 2013; Doepke and Tertilt, 2009; Doepke, Tertilt, and Voena, 2012; Teele, 2018; Wong, Clark, and Hall, 2018).

In chapter 3, I study how the disruptions caused by forced labor conscription during World War II (WWII) impacted the long-term labor market success of affected civilians. When men were again absent from the local labor market during the war, the Nazi regime wanted to avoid the renewed empowerment of women: The place of a woman was supposed to be at home, and Hitler himself was concerned that female employment would do great moral harm to women (Homze, 1967). Instead, the regime relied on forced labor from

occupied countries to fill the labor shortage in Germany. I exploit exogenous variation in conscription rates across birth cohorts of Dutch civilians by employing a regression discontinuity design. Combining archival records with micro-level census data, I show that Dutch adolescents who were conscripted for forced labor have a lower probability of being employed and lower income more than 25 years after WWII. Investigating underlying factors, I find that both being deported to Germany for forced work as well as being forced into hiding to avoid deportation have long-lasting negative effects. For individuals deported to Germany, exposure to harsher conditions and the associated health issues are contributing factors. Forced employment in sectors similar to those prevalent in a person's home municipality on the other hand alleviates negative effects. This chapter contributes to the literature on disruptions and labor market success (see e.g. Angrist, 1990; Becker, 2022; Braun and Stuhler, 2023; Huttunen, Møen, and Salvanes, 2011; Schwank, 2024) by focusing on the disruption of forced labor conscription, to the literature on forced migration (see e.g. Bauer, Braun, and Kvasnicka, 2013; Bauer, Giesecke, and Janisch, 2019; Becker, Grosfeld, et al., 2020; Becker, 2022; Sarvimäki, Uusitalo, and Jäntti, 2022) by studying a setting with return migration, and to the literature on forced labor (see e.g. Bertocchi and Dimico, 2014; Buggle and Nafziger, 2021; Buonanno and Vargas, 2019; Cinnirella and Hornung, 2016; Dell, 2010; Fujiwaray, Laudaresz, and Caicedo, 2017; Mitchener and McLean, 2003; Nunn, 2008; Soares, Assunção, and Goulart, 2012; Markevich and Zhuravskaya, 2018) by isolating the individual-level consequences of forced labor conscription from broader institutional changes. By examining a setting where the relevance of labor market experience at a young age is exogenously determined, I also contribute to the literature studying career trajectories depending on initial conditions (see e.g. Grenet et al., 2024; Oreopoulos, Wachter, and Heisz, 2012; Schwandt and Wachter, 2020; Wachter, 2020).

## Chapter overview

### Chapter 1

**Hornung, E., Stapper, C., and Yuchtman, N.** (2025). "Rewriting the Social Contract: Elite Response to Labor Unrest", Mimeo.

This Chapter is co-authored with Erik Hornung and Noam Yuchtman. Erik Hornung developed the initial idea of the paper, which was jointly refined by all co-authors. I digitized and analyzed the data. Noam Yuchtman contributed to framing the paper. The draft was jointly written by Erik Hornung and myself.

### Chapter 2

**Boelmann, B. and Stapper, C.** (2025). "Missing Men and Women's Fight for the Vote", Mimeo.

This Chapter is co-authored with Barbara Boelmann. Both authors contributed equally to the project. We jointly developed the research idea, digitized and analyzed the data, and wrote the draft together.

### Chapter 3

**Stapper, C.** (2025). "Worker Displacement and Labor Market Success: Evidence from Forced Labor Conscription during WWII".

This Chapter is single-authored and contains the current version of the working paper of the same title by Stapper (2024), ECONtribute Discussion Paper, (338).

# Chapter 1

# Rewriting the Social Contract: Elite Response to Labor Unrest

This chapter is co-authored with Erik Hornung, Professor of Economics at the University of Cologne, and Noam Yuchtman, Professor of Political Economy at the University of Oxford and All Souls College.

## Abstract

We study how autocratic elites modify the social contract in response to social unrest. In early 20th-century Prussia, elites were legitimized through unequal voting rights but faced mounting threats of mass mobilization. Using labor strikes as proxy for mobilization, we find that locations under greater revolutionary pressure increased spending on redistributive public goods. However, parliamentary support for franchise reform increased only when unrest coincided with public support from the enlightened elite. This correlational evidence is bolstered with placebo checks and a shift-share instrumental variable approach that relates industry-specific international commodity price changes to Prussian regions based on industry exposure.

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and Public Policy" (Bochum), "3rd Arne Ryde Workshop on Culture, Institutions, and Development" (Lund), the V. Congress for Economic and Social History 2023, the Economic History Society Annual Conference 2023, and the German Economic Association Annual Conference 2023 for comments.

## Declarations

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## 1.1 Introduction

Episodes of structural transformation and technological change produce significant improvements in the standard of living. They also produce intense social conflict as the size and distribution of the economic pie changes. Historically, such social conflict generated political movements demanding changes in the social contract: around the turn of the 20th century, socialists, communists, progressives, and the labor movement (along with civil rights activists and women's rights movements) emerged and mobilized the masses to demand social and political change.

Few settings were as dramatically contested as Prussia. Following the legalization of the socialist party, a mounting wave of labor strikes emerged to articulate the demands of the people. Despite this strong labor movement with levels of mobilization above those of any other European country at the time (see Figure A.1.1 in the Appendix), Prussia did not democratize until after World War I. The so-called three-class-franchise that weighed votes according to tax contributions and thereby granted disproportionately high political influence to the top income earners remained in place until 1918 (Becker and Hornung, 2020).

In this paper, we study the impact of labor unrest on the social contract in Prussia. While the elite faced a range of options in responding to unrest, we focus on two aimed at appeasing the workers. We argue that they could either increase redistribution and retain power, or extend the franchise, allowing the new median voter to decide on redistribution. The choice between these options may partly depend on intra-elite conflict. Conservative elites, who felt threatened by mass mobilization or who would have materially benefit from improving the conditions of the workers, may have preferred redistribution as a means of preserving the status quo. Liberal elites, motivated by ideological convictions, may have been more favorable to expanding the franchise, even at the cost of ceding political power.

Our quantitative findings support these expectations. We show that higher regional strike intensity led to increased local spending on education and health, which were redistributive priorities of the Social Democratic Workers' Party. At the same time, our evidence suggests that labor unrest alone was insufficient to bring about voting reforms. Only when unrest was accompanied by support from ideologically motivated segments of the bourgeois elite, the likelihood of reform increased.

We use detailed data on the intensity of all labor strikes across Prussian localities during the heyday of the labor movement in 1899–1905. While strikes ostensibly protested labor issues, political elites associated them with revolutionary tendencies. Rightly so,

as the Social Democratic Party, in particular, viewed labor strikes as a political tool for achieving broader objectives, including expanding the franchise, as advocated by Rosa Luxemburg.

First, we study whether strike intensity is associated with redistribution using detailed data on expenditures across 100 Prussian cities for 1898 and 1908. Public goods that could be classified as redistributive were largely provided in cities, where local councils, elected via the three-class-franchise, commanded over taxes and expenditures. We find that cities with higher strike intensity had stronger increases in the expenditure share for redistributive purposes but not for other categories.

Second, we study whether bourgeois support for franchise extension was stronger in regions with higher strike intensity. In 1909, the liberal newspaper 'Berliner Tagesblatt' issued a proclamation to abolish the three-class franchise. This plea received empire-wide support and was signed by 813 well regarded individuals from 159 cities, largely consisting of industrialists and members of the intelligentsia. Our findings indicate that bourgeois support for franchise extension is significantly higher in regions with higher strike intensity. Further evidence suggests that while fear of a revolutionary threat may have played a role, ideological alignment was likely the more important motivation for the bourgeoisie.

Third, we study political support for franchise extension in parliament. While voting rights were not extended until after WWI, there were several attempts to abolish the three-class-franchise through the political process. We use a roll-call vote from 1912 to estimate the effect of mass mobilization on support for the reform. Different than support for franchise extension expressed in the newspaper, this vote constitutes binding support for voting reform. We find that members of the Prussian parliament responded to higher strike intensity. However, only in regions with higher level of unrest *and* bourgeois support, the likelihood of voting in favor of franchise reforms increased significantly.

To identify exogenous variation in strike intensity, we generate a shift-share instrumental variable that maps industry specific changes in international commodity prices to Prussian regions, depending on their exposure to this industry. We use commodity price changes in global markets, which are arguably exogenous to local economic conditions in Prussia and map these price shocks into Prussian regions by the relative employment share of each industry in the respective Prussian locality.

Our findings contribute to our understanding of the role of social movements in shaping policy outcomes. Most closely related work by Ellman and Wantchekon (2000) and Archibong, Moerenhout, and Osabuohien (2022) study how protests affect policy outcomes in democratic settings, contexts that sharply deviates from ours. In this paper we show that protests shape policies and the views of the powerful even in autocratic

settings, where protesters are not fully enfranchised. We also deviate from literature focusing on protest-induced political and institutional change that do not focus on policies such as Madestam et al. (2013), Aidt and Franck (2015), and Mazumder (2018).

By showing that protest induce redistribution without altering the franchise we move beyond standard models in the spirit of Meltzer and Richard (1981) and relate more closely models in the spirit of Acemoglu and Robinson (2000). In such models, threatened elites extend the franchise only if their commitment to future redistribution is not credible, i.e., if the threat of mass mobilization is not permanent. Our paper thus relates to a literature studying how franchise systems are adjusted in response to revolutionary threats (Aidt and Franck, 2015; Aidt and Jensen, 2014; Castañeda Dower et al., 2018; Lehmann-Hasemeyer, Hauber, and Opitz, 2014; Seghezza and Morelli, 2019; Ziblatt and Dasgupta, 2015).[1] We add to this literature by examining a setting with high mobilization capacity and study the elite's response for a much broader set of outcomes that constitute the social contract.

Finally, we relate to literature arguing that elites relinquish power and political representation because they find it beneficial. According to these, franchise extension occurs when elite and mass interests partially align, e.g., when capitalist elites benefit from mass investment in human capital and health of workers or when they can channel public goods to swing voters (Doepke and Zilibotti, 2005; Galor and Moav, 2006; Galor, Moav, and Vollrath, 2009; Hollenbach, 2021; Krieger, 2024; Lizzeri and Persico, 2004). In our context, demands for voting reforms gained support from intellectuals who had no direct material stake in these changes, suggesting that political alignment between the workers and the middle class could have played a crucial role in mass enfranchisement.

## 1.2 Historical background and data

### 1.2.1 German labor movement and strike activity

Following the repeal of 'Socialist Laws' that constituted an effective ban of unions in the German Empire in 1890, the labor movement gained momentum improving the conditions of the working class (Bartels, Kersting, and Wolf, 2023; Kittner, 2005). This period saw a tenfold increase in strike activity between 1891 and 1899, peaking in 1905 with half a million active strikers in more than 2,500 individual strikes, despite legal restrictions such as penalties for inciting strikes and the informal blacklisting of agitators

[1] Castañeda Dower et al. (2018) find that the relationship between redistribution and unrest is weaker in districts without representation, which contradicts the idea that redistribution and democratic participation are substitutes.

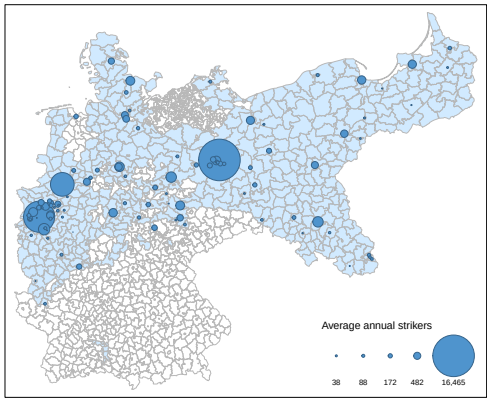
by employers (Boll, 1992; Tenfelde, 1984). Notably, there were 1.9 arrests per 1,000 strikers during 1899–1904, underscoring the state's efforts to suppress this surge in labor unrest (Volkmann, 1978).

While the strikes officially focused on improving wages and working conditions, they were strategically exploited for political purposes by the Social Democrats. Highlighting the labor movement's strategic use of strikes, August Bebel (founder and chairman of the Social Democrats) remarked, "The political mass strike [...] should and must be used if necessary," while Rosa Luxemburg pointedly added, "This shameful franchise can only give way to a franchise storm by the great masses," together advocating for strikes as a means to challenge and potentially overthrow the Prussian electoral system.[2] The elite's fear of revolution was apparent, with Robert von Puttkamer, the Prussian Secretary of the Interior, interpreting labor disputes as precursors to "preparing the sentiment for a violent revolution" (Saul, 1981). This concern led Wilhelm II to authorize extreme measures, ordering the shooting of strikers who resisted government forces (Kittner, 2005).

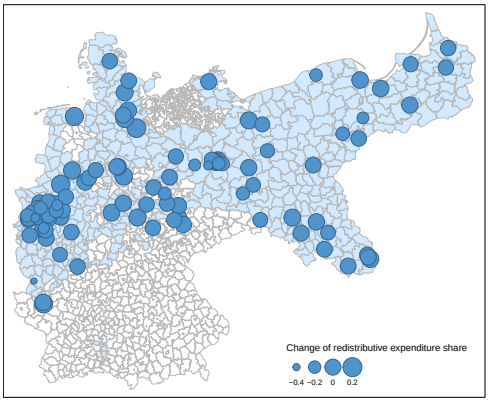
To capture the publicly perceived threat to elite rule and the franchise system, we use newly digitized administrative data on all 10,714 strikes in the German Empire from 1899 to 1905 (Kaiserliches Statistisches Amt, 1900). We derive regional variation in threat intensity, our primary variable of interest, by geo-locating each strike and calculating the average annual number of participants in a given location.[3] Figure 1.1 panel (a) depicts strike intensity across 100 Prussian cities with more than 25,000 inhabitants, while panel (c) depicts strike intensity at the constituency-level, illustrating the geographical distribution of labor unrest.

[2]Rosa Luxemburg's views were highly influenced by the Belgian and Swedish general strikes that demanded universal and equal suffrage in 1902 and especially the 1905 Russian Revolution where spontaneous mass strikes led to the adoption of the constitutional monarchy. Note that during the mass strike debate (*Massenstreikdebatte*), the Social Democrats and unions debated the use of general strikes as a means for revolution. The unions' view to focus on economic and direct political threats for workers prevailed in an agreement in Mannheim in 1906. This underlines the threat of mass mobilization of strikes until at least 1906.

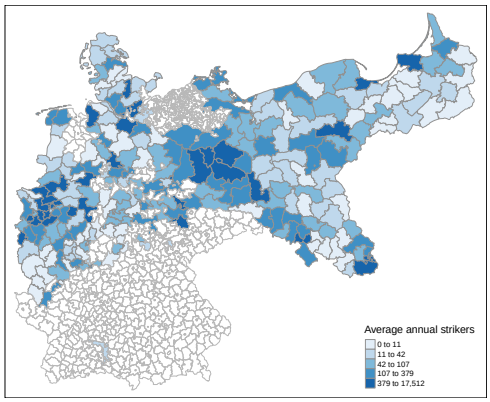
[3]The data not only provides detailed insights into the date, duration, and demands of each strike but also enables the creation of various alternative measures of intensity such as frequency and duration of strikes, distinctions between offensive/defensive, successful/unsuccessful strikes for robustness checks. Additionally, we distinguish between strikes organized by the unions with a larger focus on economic goals (non-political strikes) and wildcat strikes without union support which focus on political goals (political strikes). For robustness checks using these measures, see the Appendix.



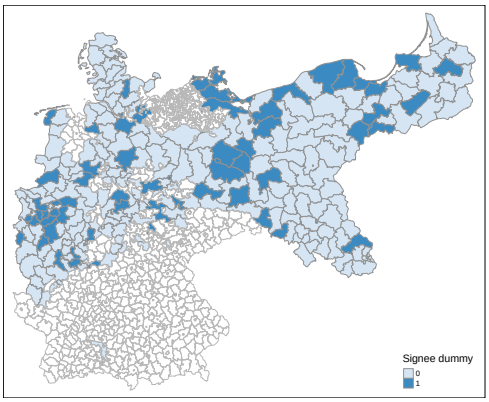

(a) Strike intensity 1899–1905, city level



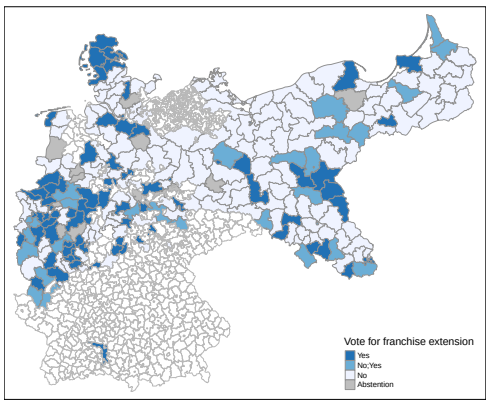

(b) Δ redistribution 1898–1908




(c) Strike intensity 1899–1905, constituency level




(d) Bourgeois support 1908




(e) Roll call vote on franchise extension 1912

Figure 1.1: Regional Variation in Labor Unrest and Elite Reaction

*Notes*. Panels a and c depict city/constituency-level strike intensity, i.e., the average annual number of workers participating in strikes in a location (1899–1905). Panel b depicts city-level changes in redistribution, i.e., the difference in the expenditure share of public funds spent on education and health between 1908 and 1898. Panel d depicts constituency-level bourgeois support, i.e., an indicator that is equal to one if a newspaper petition to abolish the franchise was signed by a resident of this constituency. Panel e depicts results from a roll call vote on franchise extension in the Prussian parliament in 1912 (for more details see main text).

### 1.2.2 Prussian franchise system and elite response

Prussia's highly unequal three-class franchise limited political participation by weighting votes according to tax contributions, resulting in the top 4% of income earners holding as much voting power as the bottom 82%—a suppression of the working class political voice by design (Peter, 2000; M. Schneider, 1989). Due to this system, the SPD only broke into the Prussian Parliament in 1908, contrasting sharply with their success in the German Empire's parliament where an equal voting system allowed them to secure the largest vote share already by 1890. Franchise reform was a recurring topic in Prussia's public debate and a major objective of the SPD. Yet, attempts to reform were repeatedly blocked by the landowning elites in parliament (Ziblatt, 2008). The resulting political landscape left the working class with minimal influence over politics, leaving unrest as their primary recourse to convince the elite to devise a more equitable social contract.

**Redistribution** To study changes in redistribution prior to franchise reforms in 1918, we focus on shifts in city budgets. Domestic policy was the responsibility of the German states and public goods including health, education, and poor relief were determined at the municipality level. City councils, elected via the three-class franchise had the autonomy to set taxes and allocate funds (Hollenbach, 2021; Marcinowski and Hoffmann, 1890; Spoerer, 2002, 2004; Wehler, 2007).

We use newly digitized budget data from 100 Prussian cities with more than 25,000 inhabitants from Silbergleit (1908). We argue that, among ten predefined expenditure categories, budgets for education (incl. institutes for fine arts) and health (including poor relief and orphan care) specifically benefit the working class, thus classifying them as redistributive following the literature (Craw, 2006; Minkoff, 2009). The other categories—treasury, general administration, tax administration, public safety, utilities, construction, public debts, and miscellaneous—are labeled non-redistributive. The dataset spans 1888, 1898, and 1908, allowing us to compute changes in the share of expenditures by category $c$ relative to changes in total spending cross cities $i$ as follows: $\Delta ExpendShare_{c,i,1898-1908} = (\frac{Expend_{c,i,1908}}{ExpendTotal_{i,1908}} - \frac{Expendc,i,1898}{ExpendTotal_{i,1898}})$ x 100. The use of fractions allows us to compare relative spending shifts, neutralizing the effect of absolute expenditure differences across cities, and to exploit changes in spending potentially linked to labor unrest occurring during the period of observation. Figure 1.1 panel (b) shows changes in expenditures used for redistributive public goods between 1898 and 1908 across Prussian cities.

**Bourgeois support** The bourgeoisie in turn-of-the-century Prussia was not a monolithic group. It comprised industrialists, merchants, bureaucrats, professionals, and

intellectuals, each with diverging interests. Broadly speaking, heavy industry favored minimal state intervention, while socially minded liberals supported progressive reforms aimed at improving the conditions of the working class. Indeed, German socialist Bernstein (1899) suggested that workers should form an alliance with the bourgeoisie to push for voting reforms, much like in Britain.[4]

On December 7 and 11, 1909, the liberal newspaper 'Berliner Tageblatt' published a proclamation to abolish the three-class franchise signed by the German elite in science, industry, and culture (see Figure A.1.2 in the Appendix). This plea aimed at "breaking the agrarian-conservative domination of Prussia" and showing that there was indeed a "wish to reform the franchise system among parts of the elite". It was endorsed by 813 signees, including prominent figures such as Georg Simmel and Max Weber.

We digitized name, location, and occupation of signees and geolocated 504 of them in Prussia, 302 in other German regions, and 7 outside of the Empire. In regression analysis, we use an indicator for Prussian constituencies with at least one signee. Results will, however, be robust to using a count variable. We further distinguish signees as 'capitalists' (123 signees) and 'intelligentsia' (370 signees) based on occupation and use the corresponding indicator in the analyses.[5] Figure 1.1 panel (d) indicates constituencies with signee presence, depicting the reform's bourgeois support base in Prussia.

**Franchise reform voting** On May 20, 1912, a vote in the Prussian House of Representatives on replacing the three-class franchise with general, direct, and secret elections fell short by 30 votes: 158 in favor, 188 against, and 91 abstentions, including 47 unexcused (see Figure A.1.3 in the Appendix). While all yea and nay votes were in party-line, unexcused abstentions were arguably used strategically by members of parliament belonging to parties voting in favor of the reform to prevent the new legislation from passing. If the 47 unexcused absentees had participated and voted according to party line, the vote could have led to the abolition of the three-class franchise.

We code parliamentary support for franchise reform as an indicator that assumes the value one, if delegate $j$ in constituency $i$ voted in favor of the reform and against as zero. Abstentions and excused absences are excluded from this coding, but unexcused absences

[4]"It was not when the Chartist movement was at its most revolutionary that the English workers gained the vote, but when the revolutionary slogans had died down and they allied themselves with the radical bourgeoisie to fight for reforms." (Bernstein, 1899, p. 167, Translation: Thomas Dunlap)

[5]We categorize a signee as 'intelligentsia' if their occupation is (university) teacher, artist, physician, legal practitioner, cleric, or public administrator. Signees are categorized as 'capitalist' if their occupation is firm owner, executives, or trade and commerce representatives. We drop five signees who indicated landowner as occupation in regressions distinguishing capitalist and intelligentsia.

are considered opposition to the party line, following Ziblatt (2008).[6] Figure 1.1 panel (e) shows the regional distribution of votes. Due to multi-member constituencies, the map includes an additional category for constituencies with opposing votes.

### 1.2.3 Control variables

The relationship between labor unrest and various aspects of the social contract will be conditioned on a host of controls for local geographical conditions and socio-economic development derived from raster data and historical censuses. Driven by the dependent variable's level of observation, right-hand side variables may reflect characteristics of the county surrounding a city or are aggregated from the county level to the constituency level, where a Prussia constituency contains two counties on average. For details on the construction of controls, see Appendix A.2.

**Geographical controls** Latitude, longitude, elevation, as well as temperature and precipitation levels from Fick and Hijmans (2017) aim to capture spatial variation in Prussia's economic development. Distance to nearest navigable river based on a historical map by IEG (2010) and distance to nearest carboniferous rock strata based on a shapefile by Asch (2005) aim to capture spatial variation in the location of trade and industry.

**Development controls** In our preferred specifications, we condition on a number of characteristics that potentially confound the relationship between strike intensity and the outcomes variables. These variables were obtained from Thome (2006a) and include the population share of Protestants in 1900, the urban population share in 1900, landownership concentration in 1895, and the population share of non-German speakers in 1900. Additionally, we control for the population share of workers employed in the industrial sector in 1895 taken from Kaiserliches Statistisches Amt (1897). See Table A.2.1 in the Appendix for summary statistics of the dependent, explanatory, and control variables.

## 1.3 Empirical strategy

To assess whether labor affected various aspects of the social contract, we estimate versions of the following equation:

[6] Unexcused absentees were present in parliament on the day of the vote but left the parliamentary chamber just before the vote on the franchise reform took place.

$$Y_i = \beta_0 + \beta_1 Strike\ intensity_{i,1899-1905} + \gamma X_i' + \epsilon_i \tag{1.1}$$

where $Y_i$ are the various outcomes described in Section 1.2.2. Depending on the outcome, subscript $i$ reflects variables measured at the city- (redistribution), constituency- (bourgeois support), or MP-level (franchise reform voting). $Strike\ intensity_{i,1899-1905} = \frac{1}{7}\sum_{t=1899}^{1905} Strikers_{i,t}$ is the average annual number of workers participating in strikes between 1899 and 1905. We sum over all strikers in a city when the dependent variable is measured at the city level and over all strikers in a constituency when the dependent variable is measured at the constituency or MP level.[7] $X_i'$ is a vector of geographical and development controls described in Section 1.2.3, defined at the county-level if the dependent variable is measured at the city level or at the constituency level when the dependent variable is measured at the constituency or MP level.

### 1.3.1 Identification

Any of our results may be subject to omitted variable bias since labor unrest is not exogenous to local socio-economic conditions that correlate with our outcomes. While the control variables aim to capture such heterogeneity, there likely remains unobserved heterogeneity. When inspecting a city's expenditures, we partly address this issue by using the first difference in expenditures, thus holding total expenditure levels and other unobserved time-invariant city level confounders fixed. Furthermore, we use expenditure change data from 1888 to 1898, thus before the observed strike period, in a placebo check. However, since this is not possible for other outcomes, we resort to an instrumental variable approach to test the robustness of our results. This approach resembles a shift-share or Bartik IV instrumental setup that combines time-series variation in global commodity prices with spatial variation in the exposure of Prussian locations to industries using the respective commodities as inputs. In this setting, the exogeneity of the shift-share instrument is driven by the shocks as described in Borusyak, Hull, and Jaravel (2022).

To map price changes across Prussian regions, we combine employment data from the occupational census of 1895, covering all Prussian counties and combine it with real annual spot prices for 40 commodities between 1899 and 1905 from Jacks (2019).

[7] Because $Strike\ intensity_{i,1899-1905}$ is a skewed count variable that has a number of zeros (2 at the city, level, 20 at the constituency level), we always transform it using the inverse hyperbolic sine (arcsinh). We are not overly concerned with the implicit weighting of the extensive and intensive margin effect, as the share of observations with zero is below one third, see Bellemare and Wichman (2020). We show the robustness of our results to excluding the observations with zero, to using the optimal scale factor for the average number of strikers following Aihounton and Henningsen (2021), and to using $log(Strikers + 1)$ in the Appendix.

Following Autor, Dorn, and Hanson (2013), we use the concordance of industries and product classes by Pierce and Schott (2012) as a baseline for matching prices into industry categories, adjusting for differences in industry categorization in the historical employment data. Out of the 101 available industry categories, we are able to match the price data of 38 commodities to 44 industries, which employ 69.7% of all workers in manufacturing and services in Prussia.[8] The instrumental variable used to predict strike participation is defined as:

$$Price\ shock_i = \sum_k \frac{emp_{i,k}}{\sum_k emp_{i,k}} \frac{1}{7} \sum_{t=1899}^{1905} \frac{1}{M} \sum_m |\Delta, Price_{m,k,t}| \tag{1.2}$$

where $|\Delta Price_{m,k,t}|$ is the absolute value of the annual price change of commodity $m$, reflecting that price changes in either direction may change worker's participation in strikes. For industries using multiple commodities as inputs, we take the average of these absolute price changes. To match the cross-sectional setup of our estimations, we average price changes over the period for which we have strike data. The employment shares $\frac{emp_{i,k}}{\sum_k emp_{i,k}}$ measure the relative importance of industry $k$ in location $i$, and map the price shocks into Prussian locations.[9] Price shocks are arguably exogenous to location-specific economic and political characteristics, since prices are determined on the global market and Prussian locations are price-takers. We confirm that our instrument fulfills the assumptions described in Borusyak, Hull, and Jaravel (2022) in Table A.3.1 in the Appendix. Employment data are available only at the county level. Hence, we construct the instrument at the county level when the dependent variable is measured at the city level and aggregate to the constituency level when the dependent variable is measured at the constituency or MP level.

The first stage takes the following form:

$$Strike\ intensity_{i,1899-1905} = \pi_0 + \pi_1 Price\ shock_i + \sum_k \frac{emp_{i,k}}{\sum_k emp_{i,k}} + \delta X_i' + u_i \tag{1.3}$$

where $i$ reflects either cities or constituencies and $X_i'$ is the vector of controls included in equation (1.1).[10] Since the total sum of employment shares of all industries that we were able to match with at least one commodity does not sum up to one, we control for the

[8] See Appendix A.3 for details.

[9] Following Borusyak, Hull, and Jaravel (2022), we define employment shares as the employment in industry $k$ relative to total employment in location $i$. Our results are robust to defining the shares as employment in industry $k$ relative to total employment in that industry across all locations.

[10] $Price\ shock_i$ is transformed using the inverse hyperbolic sine transformation.

summed up shares defined as $\sum_k \frac{emp_{i,k}}{\sum_k emp_{i,k}}$ in all IV specifications following Borusyak, Hull, and Jaravel (2022).

## 1.4 Results

### 1.4.1 Results: Redistribution

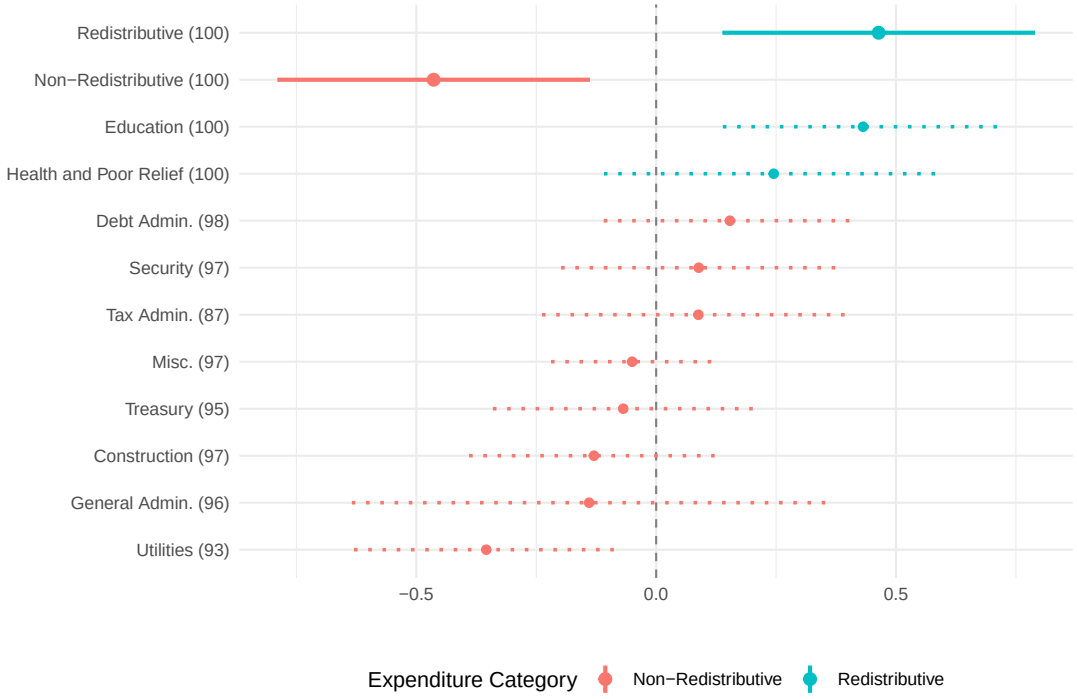



Figure 1.2: Labor unrest and city-level redistribution

*Notes:* Figure plots $\beta_1$ coefficients from estimating equation 1.1 at the city level using OLS. Each row replaces the outcome $Y_i$ for one of the ten expenditure categories. 'Redistributive' expenditures comprise 'education' and 'health and poor relief'. 'Non-redistributive' comprise all others. Dependent variables and main explanatory variable (strike intensity) are standardized with zero mean and standard deviation of one. Strike intensity is transformed using the inverse hyperbolic sine (arcsinh). All regressions are conditional on geographical (longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal) and development controls (industrial employment share, Protestant share, urbanization rate, landownership inequality, non-German share). Standard errors are clustered at the county level. Bars indicate 95% confidence intervals. Numbers in parentheses indicate the number of observations.

Figure 1.2 plots $\beta_1$ coefficients with 95% confidence intervals showing how strike intensity affects changes in a city's expenditure categories. We highlight categories that are considered as redistributive in blue and those that are non-redistributive in orange.

The distinct pattern in expenditures is apparent. A one standard deviation in strike intensity increases the expenditure share by 0.46 standard deviations for the combined redistribution outcome, by 0.43 for education, and by 0.25 for health and poor relief. On the other hand, cities reduced their non-redistributive expenditure share by 0.46 standard deviations for every standard deviation increase in strike intensity. These results indicate that the city council apparently decided to shift funds towards public goods more beneficial to workers in response to labor unrest. Using alternative definitions of strike intensity generates qualitatively similar results (see Figure A.4.1 in the Appendix.) For additional robustness checks, including controls for latent mobilization capacity (proxied by earlier strikes) or for 16 different industry shares, see Tables A.4.1–A.4.2 in the Appendix.

An alternative margin of response could have been to confront the threat with force and to increase spending on policing and public security. However, Figure 1.2 shows a relative precisely estimated lack of change in security spending.

To ensure that our findings are not merely reflecting trends in redistributive expenditure shifts that might have occurred in the absence of labor unrest, we conduct placebo regressions using expenditure share changes between 1888 and 1898 as the dependent variables (see Figure A.4.2 in the Appendix). Given that labor unrest from 1899 to 1905 should not affect spending in this earlier period, the absence of significant coefficients in these placebo tests supports our hypothesis by showing common trends in spending for redistributive purposes before 1898.

We further demonstrate the robustness of our findings using the instrumental variable approach outlined previously in Figure A.4.3.[11] Table A.4.2 confirms the positive association between labor unrest and redistributive spending in cities, with the OLS and IV results displaying similar magnitudes.[12] The first stage presented in Table A.4.2 suggests that larger shocks increase strike intensity.[13]

**Aside: Wages.** It may be interesting to check whether labor unrest affected aggregate wages, representing another potential margin of response for the (capitalist) elite. However,

[11]The employment shares for the construction of the instrumental variable which map the international price changes into Prussian regions is defined at the county level. For 54 out of the 100 cities, the city and county coincide. For the other cities, the county is larger than the city, thus the instrument is less precise. We therefore weight the instrumental variable with the share of the county's population that lives in the respective city.

[12]Figure A.4.3 replicates Figure 1.2 using the IV approach for all expenditure categories, again displaying similar patterns.

[13]The Kleibergen-Paap first stage F-Statistic suggests that the instrument is sufficiently strong, however, the wide Anderson-Rubin confidence interval prevents outright dismissal of the null hypothesis that strike intensity has no effect (see Andrews, Stock, and Sun, 2019).

it is important to note that strikes could both demand wage increases and oppose wage cuts, which complicates the interpretation of changes in aggregate wages resulting from strikes. Furthermore, the only available wage data pertains to day laborers, i.e., unskilled workers who typically do not engage in strike activity (Zentralblatt, 1897-1910). Bearing these caveats in mind, our examination of average day-laborer wage changes across cities from 1897 to 1910 reveals no significant wage increases in response to higher strike intensity (see Table A.5.1 in the Appendix.)

### 1.4.2 Results: Bourgeois support

In this section, we examine the alignment between the workers' cause and the bourgeoisie as a potential ally in the push for franchise extension, as well as the reasons behind this alignment.

Table 1.1: Labor unrest and bourgeois support for franchise reform

| | Dependent variable: signee indicator | | | | |
|---|---|---|---|---|---|
| | Prussia | | | | Non-Prussia |
| | All | | Capitalists | Intelligentsia | All |
| | OLS | IV | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) |
| Strike intensity | 0.045*** | 0.082* | 0.033*** | 0.042*** | 0.044*** |
| | (0.012) | (0.045) | (0.008) | (0.012) | (0.008) |
| Geographic Controls | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES |
| Kleibergen Paap F-Statistic | | 22.13 | | | |
| Anderson Rubin CI | | -0.01–0.19 | | | |
| Observations | 264 | 264 | 264 | 264 | 501 |
| Adjusted $R^2$ | 0.304 | 0.288 | 0.311 | 0.312 | 0.185 |

*Notes:* Table shows results from estimating equation 1.1 at the constituency level. Dependent variable is one if constituency has at least one signee. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Column (5) is estimated at the county level for counties outside of Prussia, excluding states with a three-class franchise (Hamburg, Lippe, and Saxony). Geographical controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal. Development controls: industrial employment share, Protestant share, landownership inequality, urbanization rate, non-German language share. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.1 shows higher levels of bourgeois support for franchise reform in constituencies with higher strike intensity. The positive and significant *beta*-coefficient suggests that increasing the strike intensity in a constituency by one standard deviation is associated with a 11 percentage point increase in the probability of local reform support.[14] The instrumental variable estimates presented in Column 2 suggest that the OLS estimates may understate the true effect: specifically, the same increase in strike intensity increases the probability by 19.7 percentage points.[15] We perform various robustness checks, including

[14]The standard deviation of strike intensity for the sample of the baseline specification is 2.4.

[15]Given that motives for bourgeois support may be directly related economic fluctuations the exclusion restriction may be violated. We are less concerned about this as the tests of the instrument following Borusyak, Hull, and Jaravel (2022) confirm that the shocks are as good as randomly assigned, see Appendix Table A.3.1.

measuring strike intensity in various ways, and to controlling for latent mobilization capacity (see Figure A.6.1 and Tables A.6.1–A.6.4 in the Appendix.)

There may be various reasons underlying the bourgeois support of workers' causes, including a perceived threat, personal benefits from franchise extension, or a desire to signal virtue and convictions. We aim to better discern the motivations of signees by analyzing their occupations, which potentially provide insights into their incentives. We argue that the 'capitalist bourgeoisie,' consisting of firm owners, might have supported reforms to benefit from enhanced worker health and human capital, protect their assets from revolutionary threats, or express solidarity with workers' causes, reflecting enlightened ideals. The 'intelligentsia bourgeoisie,' largely consisting of professors, artists, and writers arguably supported reforms due to their progressive and liberal ideals, rather than immediate benefits or threats to their economic status.

Columns 3–4 reveal that the intelligentsia exhibit a higher magnitude of support in response to labor unrest, though the difference from the capitalist bourgeoisie is not statistically significant ($p = 0.57$). By controlling for the industrial employment share, we determine that capitalists are not primarily motivated by potential improvements in workers' conditions. However, it is not possible to distinguish their motives further between fear and ideological alignment. In contrast, as material interests of the intelligentsia are less likely to be threatened by labor unrest, we conclude that their support for workers' causes in areas of high strike intensity reflects ideological alignment.

The newspaper call garnered support from the bourgeoisie across Germany, not just within Prussia, where most states had adopted more progressive franchise systems.[16] Outside Prussia, bourgeois support for franchise reform is less likely driven by local threats but rather by a desire to support the workers' cause in Prussia, signaling their own convictions, which are partly shaped by their experience with broader democracy. Column 5 indicates that bourgeois support for reform in response to labor unrest is similar in regions outside Prussia. This finding suggests that ideological alignment, rather than the fear of revolution, is the primary driver of their support.

### 1.4.3 Results: Franchise reform voting

Having shown that labor unrest influences both spending for redistribution and bourgeois support for democratic reform, this section will analyze whether labor unrest also

[16]Prussia had the most regressive voting system among the German states (Ziblatt, 2017, p. 221). In our analysis of franchise support outside of Prussia, we exclude Hamburg, Lippe, and Saxony, which also had a three-class franchise at the time of the newspaper call.

translated into political support for democratic reform among members of the Prussian parliament in an important roll-call vote in 1912.

Table 1.2: Labor unrest and roll-call voting on franchise reform

| | *Dependent variable:* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | voting (yea/nay+unexcused) | | | | | | | | |
| | OLS | IV | OLS | OLS | IV | OLS | IV | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Strike intensity | 0.032** | 0.058 | | 0.025* | 0.044 | 0.008 | 0.053 | 0.024 | 0.010 |
| | (0.015) | (0.048) | | (0.015) | (0.049) | (0.015) | (0.049) | (0.015) | (0.015) |
| Strike intensity x Bourgeois support | | | | | | 0.432*** | 0.660*** | | |
| | | | | | | (0.084) | (0.162) | | |
| Strike intensity x Capitalists support | | | | | | | | 0.243*** | |
| | | | | | | | | (0.074) | |
| Strike intensity x Intelligentsia support | | | | | | | | | 0.411*** |
| | | | | | | | | | (0.086) |
| Bourgeois support | | | 0.171*** | 0.153*** | 0.138** | −0.578*** | −1.001*** | | |
| | | | (0.057) | (0.058) | (0.063) | (0.144) | (0.283) | | |
| Capitalists support | | | | | | | | −0.459*** | |
| | | | | | | | | (0.166) | |
| Intelligentsia support | | | | | | | | | −0.561*** |
| | | | | | | | | | (0.157) |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Kleibergen Paap F-Statistic | | 32.76 | | | 29.44 | | 29.44;109.11 | | |
| Anderson Rubin CI | | -0.03–0.16 | | | -0.05–0.15 | | | | |
| Observations | 375 | 375 | 375 | 375 | 375 | 375 | 375 | 375 | 375 |
| Adjusted $R^2$ | 0.241 | 0.231 | 0.249 | 0.253 | 0.247 | 0.295 | 0.251 | 0.254 | 0.291 |

*Notes:* Table shows results from estimating equation 1.1 at the MP level. Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted nay or is absent without excuse during vote. MPs who abstained or were absent with excuse are excluded. *Strike intensity* is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). *Bourgeois support* is one if constituency has at least one signee. The interaction term in columns (6) through (9) is standardized for easier comparison. Geographical controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal. Development controls: industrial employment share, Protestant share, landownership inequality, urbanization rate, non-German language share. Kleibergen Paap F-Statistic in column (7) for instrumenting *Strike intensity* and *Strike intensityxBourgeoissupport* respectively. Standard errors, clustered at the constituency level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.2 shows how labor unrest, bourgeois support and their combination affected roll-call votes in parliament. OLS results presented in column 1 suggest that Prussian MPs were more likely to vote for reform if their constituency experienced higher strike intensity: Increasing the strike intensity by one standard deviation increases the likelihood of a yea vote by 7.4 percentage points.[17] IV results in column 2 confirm the positive effect, though not precisely estimated. In column 3, we show that MP's voting behavior also responded to support signals by the local bourgeoisie. When we include both variables at the same time in columns 4 and 5, the effect of labor unrest becomes smaller.

[17]The standard deviation of strike intensity for the sample of the baseline specification is 2.3.

Columns 6 and 7 report results of a regression adding an interaction term between strike intensity and bourgeois support. In column 6, the baseline effect of strike intensity in the absence of bourgeois support is insignificant and close to zero. Conversely, the interaction term is positive and significantly different from zero, suggesting that labor unrest influenced MPs' reform voting behavior only when supported by the bourgeoisie.[18] We conclude that labor unrest alone was insufficient to alter the voting behavior of MPs.[19] In constituencies where workers and the bourgeoisie united in their demand for franchise reform, MPs either followed their demands or had already been replaced by delegates more favorable to reform in earlier elections.

In columns 8 and 9, we separately estimate interaction effects of strike intensity and 'capitalist support' and 'intelligentsia support,' respectively. Our findings suggest that an alliance between workers and the intelligentsia has a stronger effect on voting for reform than a coalition between workers and capitalists, though the difference is not statistically significant ($p = 0.14$). While this finding can be interpreted in multiple ways, we prefer the explanation that MPs were more comfortable voting for reform when persuaded by arguments from intellectuals rather than from workers alone.

In this analysis, MPs who were absent without excuse during the vote are classified as delegates supporting franchise reform but unwilling to vote against their party line. The robustness of our results, when relaxing this assumption and applying various other coding schemes, is demonstrated in Figures A.7.1a–A.7.1b in the Appendix. Other robustness checks are presented in Tables A.7.1–A.7.4.

## 1.5 Conclusion

At the turn of the 20th century, Prussia was the European country with the highest mobilization capacity. In 1905 alone, there were 2,656 strikers per 100,000 workers. This labor unrest posed an inherent revolutionary threat to the autocratic elite in Prussia,

[18] Note that the coefficient on the baseline effect of bourgeois support is estimated for locations with any bourgeois support but zero strike intensity—a rare situation.

[19] The existence of both a positive interaction effect (where labor unrest influences votes differently with bourgeois support) and a positive mediation effect (labor unrest enhancing bourgeois support, which in turn affects voting behavior) complicates the interpretation of our findings. Specifically, the interaction effect presented in column 6 may be underestimated because it does not account for how increased labor unrest directly boosts bourgeoisie support, which then further influences voting due to the positive interaction term (see Vanderweele, 2015). Accurately isolating these effects would require imposing strict assumptions about the exogeneity of both labor unrest and bourgeoisie support, beyond the mediated variation by exogenous labor unrest. Since we do not have a separate instrument to independently identify bourgeois support, we refrain from separately estimating the mediation and interaction effects.

who were elected through an unequal voting system that heavily favored wealthy voters. Despite this pressure, franchise reforms were only enacted after World War I in 1918.

In this paper, we study how the elite reacted to increased labor unrest. Our findings suggest that elites reacted to mass mobilization by increasing spending on redistributive public goods, such as healthcare, poor relief, and education. While workers may have successfully threatened the elite into channeling more public funds in their direction, their threat alone seems insufficient to gain more political power. MPs were indeed more likely to vote in favor of franchise extension when faced with a stronger revolutionary threat in their electoral district. However, this effect was conditional and only held where the bourgeois elite aligned with workers in demanding franchise reform. Using information from a public newspaper call for franchise reform, which was signed by over 800 individuals who can be described as the bourgeoisie elite of the German Empire, including professors, writers, artists, and capitalists, we find that these elites reacted to mass mobilization with an increase in their demand for franchise extension. Notably, this reaction appears to have been driven by ideological alignment rather than by fear, as those with less personal economic stake in avoiding revolution were more responsive to labor unrest.

Our findings highlight a crucial aspect of democratization processes: while redistribution can serve as an instrument to contain mass discontent, lasting political reforms often require broad coalitions. The case of Prussia suggests that democratic breakthroughs are more likely when ideological shifts among elites align with mass mobilization from below.

# Chapter 2

# Missing Men and Women's Fight for the Vote

This chapter is co-authored with Barbara Boelmann, Postdoctoral Researcher at the University of Cologne and Researcher at the Research Data Centre Ruhr of the RWI – Leibniz Institute for Economic Research.


## Abstract

In this paper, we study the role of agency in shaping demand for political representation of under-represented groups. Specifically, we examine the increase in women's agency during World War I in Germany, proxied by the share of missing men. We make demand for political representation measurable by a newly digitized panel of local suffragette clubs. We exploit exogenous variation in the drafting probability arising from regional differences in recruitment responsibility. Our results suggest that women were more likely to keep open local suffragette clubs in regions with higher male absences during the war. We show suggestive evidence that a normalization of women in leadership roles is a contributing factor. We further show that agency increased political participation among women once female voting rights were introduced.



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## 2.1 Introduction

Women are under-represented in leadership positions in business and politics around the world. In 2024, women held 31.7% of leadership positions in industry, 33% of seats in parliament, and were head of state in only 24 countries (World Economic Forum, 2024). This is despite considerable societal costs as female politicians cause better provision of public goods and higher institutional quality (Hessami and da Fonseca, 2020), and female leaders mitigate toxic culture in the workplace (Alan et al., 2023). Recently, demand-side factors have increasingly been shown to explain part of the remaining under-representation in developed countries, such as women's lower likelihood to apply for leadership positions (Haegele, 2025) and lower willingness of women to run in political elections (Gonzalez-Eiras and Sanz, 2021). However, little is known about factors strengthening and alleviating demand-side constraints for representation.

In this paper, we investigate the role of agency in shaping women's demand for political representation in Germany in the early 20th century. This setting is ideally suited for this for two reasons. First, building on a newly collected dataset, it allows us to make demand for political representation measurable by looking at local chapters of the suffragette movement. Second, the large number of men serving as soldiers during World War I induced a considerable shock to women's agency, women suddenly being the head of the family, taking over new, previously male jobs, and leading the huge charitable effort of war relief.

We define agency not as "the intentions people have in doing things but [as] their capability of doing those things in the first place (which is why agency implies power [...])" (Giddens, 1984, p. 48). Agency thus crucially depends on the individual's environment,[1] such as power structures and cultural norms, which limit or widen an individual's capability of doing things. Pre-WWI Germany would limit women's such capabilities compared to men's both through legal institutions and cultural norms.[2] However, when a large share of men went to the front to fight in WWI, this created–among a great deal of suffering–opportunities for women, increasing their agency: Women became the primary decision-maker in the family when their husbands left home as soldiers, took over jobs previously reserved for men, and organised the charitable effort for the home front. We proxy women's increase in agency by changes in the local sex ratio from prior to during WWI based on German census data from the time.

[1] This would be referred to as "structure" in the sociological literature where the concept of agency is borrowed from.

[2] For example, husbands could decide on the family's place of residence, the wife's job and the family's finances (*Letztentscheidungsrecht des Ehemanns*, see §1354 of the BGB (Reichsamt des Innern, 1896)

In addition, we create a novel dataset to make demand for political representation measurable. To that end, we digitise and geocode the universe of suffragette clubs in Germany between 1912 and 1918. The German suffragette movement was a movement of middle-class women who demanded women's right to vote. A key strategic goal of the movement was the foundation of local chapters—which we refer to as suffragette clubs. Suffragette clubs brought attention to the cause to the local area and strengthened the mobilisation between women fighting for the franchise. We measure demand for women's political representation by the presence of a local suffragette club. This measure has two key advantages: First, it allows us to obtain measures for demand for women's representation on a yearly basis and on a small regional scale–something that is not typically feasible for historic time periods. Second, we view local suffragette clubs as revealed preferences rather than stated preferences as opening and maintaining a suffragette club is considerably more costly than, say, signing a petition in support of the female franchise. As such, local suffragette clubs are more indicative of actual demand for political representation even in light of costs associated with this.

These costs were particularly high during WWI. Right at its beginning, the German Emperor called a truce of parties (*Burgfrieden*), prohibiting any political arguments lest they divide the national front. Instead, national unity was the foremost political goal. The middle-class women's movement followed suit and in the first month after the beginning of the war, the movement's journal predominantly calls for a union of the entire women's movement to build the home front. This resulted in an overall decrease of suffragette clubs during the war by 21% (from 108 to 85 counties).

Did women's increased agency shape their demand for political representation in this political climate? To answer this, we link our measures on demand for political representation—the presence of a local suffragette club—with our measure for women's increased agency—changes in the local sex ratio—on the county (*Kreis*) level, a small German administrative unit (1049 in total). Building on that database, we apply a difference-in-differences (DiD) strategy. In the baseline specification, we define two types of counties. First, treatment counties were in the top 25% of the sex ratio change distribution, thus experiencing the largest decrease in local men (0.365 on average). Second, control counties were in the bottom 25% of that distribution, experiencing the lowest changes (0.061 on average). We estimate a two-way fixed effects (TWFE) regression, exploiting regional idiosyncratic variation in the probability to be drafted. The draft was based on the age and occupation of men; thus, holding these constant via the county fixed effectss (FEs), any remaining variation should be as good as random. Note that this also holds constant initial differences in women's demand for the vote and empowerment

more generally. We further include a range of pre-war county characteristics, including the presence of a club prior to the war, the local age and industry structure, and the share of urbanisation, allowing these to have differential effects in each year. We further probe the robustness of our results in a battery of robustness tests, including a border design, comparing neighbouring counties on different sides of a recruitment area border.

Our main findings indicate that women in treatment counties are 3.8 percentage points (37% of the baseline) more likely to keep suffragette clubs open during the war compared to control counties, suggesting that women's agency helped keep women's demand for the franchise alive despite the considerable costs at the time. We further demonstrate that this is not a general collective-action effect. To that end, we also digitise the universe of local chapters of two other women's clubs—one charitable and one professional organisation[3]—and find no relation of club presence to the degree of missing men locally.

Why would anyone, including women, demand political representation? Political representation is a way to advocate for one's interests and implement policies in this direction–something that will be desirable to people as soon as preferences and interests are heterogeneous across the population. However, whether or not an individual actually demands political representation depends on their expected benefits of this representation, weighed by how likely they find the fight to be successful, compared to the costs associated with the fight. These include social costs, such as a social stigma associated with not conforming to societal expectations. We hypothesise that agency increases a person's willingness to fight for their political representation despite these costs. Experiencing their own agency, i.e. their capabilities of furthering their own agendas, as well as seeing the agency of other women might lead women to think that success of the movement is more likely. In addition, the associated costs might be reduced as women in public roles are less stigmatised and thus women demanding further political rights are perceived as less nonconforming. Thus, we view obtaining more agency, both privately and publicly as a group of women, as an empowering experience which leads to higher demands of political representation.

We provide empirical evidence supporting such a framework of demand for political representation which also yields insights on the relative importance of costs and benefits in shaping the relationship between agency and demand. To that end, we differentiate two types of war-related experiences of women: women in economic distress who enter

[3]Specifically, the two clubs are the German Protestant Women's Association (DEF)—a conservative charitable organisation explicitly against women's franchise—and the General German Women Teachers' Association (ADLV)—a professional organisation for women in the teaching profession, a key occupation for middle-class women.

the workforce and middle-class women organising the war relief in the National Women's Service [German: *Nationaler Frauendienst*] (NFD). Women's employment increased during WWI, mostly out of necessity as poor women were struggling to feed their families without their husband's income. For middle-class women, a central occupation during the war was building and maintaining the NFD, a huge charitable apparatus of war relief.[4] We first split our sample by the median change in the female employment share and show that the effect of missing men is somewhat larger in high-change areas, although the differences are not large, indicating that economic need was not the main driver behind women's continued demand for political representation. Second, we split our sample by whether or not a local chapter of the NFD existed, finding that the effect of missing men on women's demand for political representation is entirely driven by areas with an NFD chapter. These results highlight the central role of women in public leadership roles for female empowerment, reducing the costs associated with demanding and fighting for political representation.[5]

Last, we investigate how demand for political representation translated to political participation after the franchise was extended to women in November of 1918. First, looking at the passive right to the vote, we create a novel and unique dataset on the universe of politicians running for the first election of the Weimar Republic, namely for the National Assembly in early 1919. We find that women are more likely to be political candidates when more men were missing during the war, and this is driven by constituencies that had a suffragette club during the war. This is suggestive evidence for women's continued demand for political representation. Women are, however, less likely to be elected when there were more missing men, primarily driven by constituencies without a suffragette club during the war. Looking at voting patterns from 1919-1930, i.e. the active right to vote, women's turnout is higher in counties with more missing men during the war, and this is mainly due to areas with a suffragette club during the war, indicating women's longer-term demand for political representation. Women vote, on average, more conservatively than men. However, this gap is smaller in areas with more missing men during the war, providing suggestive evidence that experiencing more agency might have shifted political preferences of women.

[4]The NFD acted as an employer and was responsible for many tasks, for example mending clothes, organising and distributing state and volunteer welfare services, and disseminating information from the local authorities.

[5]Bühler and Sabet (2025) highlight heterogeneity in political views within female voters, which are in line with our results that middle-class women's activity during the war is a greater driver of suffragette support.

**Contribution to the literature.** Our paper makes three main contributions, connecting several strands of the economics literature. First, to the best of our knowledge, our paper is the first to explicitly study the demand for political representation by a disenfranchised group. The literature on the extension of the franchise to women almost exclusively studies reasons why men would or have extended it (for overviews, see Doepke, Tertilt, and Voena, 2012; Hanlon, 2022), including *men's* benefits of women's franchise (Bertocchi, 2011; Doepke and Tertilt, 2009; Teele, 2018; Wong, Clark, and Hall, 2018), lower costs to men when women are scare (Braun and Kvasnicka, 2013) and women's labour force participation during WWI (Arnsbarger, 2024). In fact, Teele and P. A. Grosjean (2024, p. 719) come to the conclusion that "[t]o date there are few high-quality measures of women's political activism for suffrage". We propose a new revealed-preferences measure for the demand for representation, using the suffragette clubs, which takes into account the costs associated with fighting for political representation. Historians argue that the suffragette clubs, by creating political pressure over years, contributed to the eventual extension of the franchise to German women (Schaser, 2009). In their interdisciplinary review, Moehling and Thomasson (2020) make a similar argument for the US. Thus, it is important to study both the supply of and demand for political rights when considering non-violent franchise extensions. By contributing this new perspective, our paper also speaks to papers evaluating other extensions of political rights, such as the US civil rights movements. Existing papers have documented which factors ultimately led to the success of the movement (e.g. recently Calderon, Fouka, and Tabellini, 2023), i.e. the supply of further rights, but little is known about the factors driving demand.

Second, our results highlight the central role of agency for shaping demands for representation. This is also relevant in settings where the formal rights to representation have already been granted. Political systems cannot fully account for women's underrepresentation in politics as women are less willing to become political candidates (Fox and Lawless, 2004; Gonzalez-Eiras and Sanz, 2021). Likewise, women remain underrepresented in business leadership because they do not apply for such roles (Haegele, 2025). Complementing other reasons studied in previous research—including lower levels of leadership ambitions (Fox and Lawless, 2014; Lawless and Fox, 2005, 2010, 2025), more aversion to competitive environments (Preece and Stoddard, 2015; M. C. Schneider et al., 2016) and family obligations (Bernhard, Shames, and Teele, 2021)—, our results highlight another reason for why that is the case: lack of agency in light of costs associated with representation at the top. While women in developed societies certainly have more agency than German women at the beginning of the 20th century, being a leader may still have substantial social costs–such as being perceived as too manly or being seen as a bad

mother.[6] These costs associated with leadership roles can reduce women's demand for representation at the top. Our findings indicate that more women *doing* leadership tasks may raise both their own and other women's willingness to demand formal representation in leadership roles. These findings call for restructuring political processes and workplaces in such a way as to give more autonomy and decision-making power to early-career women encouraging them to aspire to leadership roles.

By shifting the perspective to the determinants of demand for representation in lights of costs, we provide a micro foundation for the literature studying the consequences of women in political offices for the supply of female political candidates. On the individual level, both currently (Baskaran and Hessami, 2022; Bhalotra, Clots-Figueras, and Iyer, 2018; Bhavnani, 2009; O'Connell, 2020; Wasserman, 2023)[7] and historically (Carpenter and Moore, 2014), women's participation on political activities has been found to result in their continuous willingness to act as political leaders. On the aggregate level, studies have found role model effects of women in office for other women entering politics (Brown, Mansour, and O'Connell, 2022; Gilardi, 2015; Ladam, Harden, and Windett, 2018), potentially driven by higher aspirations (Beaman et al., 2012) and self-evaluations (Latu et al., 2013).[8] In sum, the evidence on the supply of female political candidates is in line with our findings of the central role of agency in raising own and other women's demand for representation.[9]

[6]For example, when German Chancellor Angela Merkel wore an evening dress instead of her usual suit, this was heavily debated in both regular and social media (Strohmaier, 2008). Likewise, there were many public debates asking whether and how Ursula von der Leyen, at the time German Minister for Family Affairs, can combine her work with raising seven children (Poelchau, 2006).

[7]Baskaran and Hessami (2022) find that women's probability to recontest is lower than men's, especially in male-dominated environments. This indicates that women's overall increased agency also affects the individual willingness to become a political leader. Part of that literature also looks at the probability to recontest after losing the previous election and generally finds no gender differences (Bernhard and de Benedictis-Kessner, 2021; Nzabonimpa, 2023; Wasserman, 2021) apart from for novice politicians (Wasserman, 2023) but even that difference disappears in offices with higher female representation, again showing how agency of women overall contributes to individual change in behaviour.

[8]Bhalotra, Clots-Figueras, and Iyer (2018); Broockman (2014) find no effects of women in political office on political candidates. But in these settings, this finding may be explained by party bias in nominating women, highlighting again the need for high-quality measures of demand for political representation.

[9]A. Evans (2016) makes a similar argument regarding Zambian women's increased political representation. In her qualitative work, she finds that women's increased labour force participation changed gender norms, making women's political representation more likely—something that was not achieved by granting formal rights only. In that sense, our findings help explain why the empirical findings of the effect of women's employment (Aalen, Kotsadam, and Villanger, 2019; Matland, 1998; Ross, 2008; Schlozman, Burns, and Verba, 1999), economic development more generally (see Duflo, 2012, for an overview) and formal political right in themselves (Fallon, Swiss, and Viterna, 2012) on women's political participation is mixed. If most of the decision-making power still lies with men, i.e. women have little agency, their demand to actively participate in politics might be low.

Third, our paper contributes a new domain on how war affects women, namely through political empowerment. The large existing literature mainly looked on how women's employment was shaped by World War I (Boehnke and Gay, 2022) or World War II (WWII) (Acemoglu, Autor, and Lyle, 2004; Bellou and Cardia, 2016; Fernández, Fogli, and Olivetti, 2004; C. Goldin and Olivetti, 2013; C. D. Goldin, 1991; Jaworski, 2014; Rose, 2018), with Braun and Stuhler (2024) looking at medium-run consequences because of war widowhood, and on the effects of war on family outcomes, such as marriage markets (Abramitzky, Delavande, and Vasconcelos, 2011; Battistin, Becker, and Nunziata, 2022; Knowles and Vandenbroucke, 2019) and fertility (Bethmann and Kvasnicka, 2013; Brainerd, 2017; Brodeur and Kattan, 2021; Doepke, Hazan, and Maoz, 2015; Kesternich, Siflinger, Smith, and Steckenleiter, 2020; Kitchens and Rodgers, 2023; Kraehnert et al., 2019; Ogasawara and Komura, 2022; Vandenbroucke, 2014). Since our findings are driven by increased agency due to missing men, they also contribute this new angle of political empowerment to the literature studying imbalances in the local sex ratio. Typically, studies in this literature look at the long-run consequences[10] of historically skewed sex ratios on gendered employment patterns (Cardoso and Morin, 2018; Gay, 2023), marriage market (Angrist, 2002), gender norms (Alix-Garcia et al., 2022; Baranov, De Haas, and P. Grosjean, 2023; P. Grosjean and Khattar, 2019; Rogall and Zárate-Barrera, 2020; Teso, 2019) and intimate partner violence (Boggiano, 2020). Grant et al. (2018) study the long-term effects of missing men during WWII on women's likelihood to run for political offices in 1990. Our paper provides a short-run foundation for why we would expect these long-lasting effects through generations: Only if the directly-affected women change their behaviour, can we expect these changes to unravel over several generations.

## 2.2 Historical background

### 2.2.1 The women's movement in Germany before WWI

**Early women's movement.** Women began founding associations during the war of the German campaign from 1813 to 1815 for patriotic goals and charitable work. During the first half of the 19th century, women continued their demands for better education and employment prospects, but politicians at the time agreed that women should not participate in the public sphere. For example, the writer Robert Springer noted: "Women, you want to participate in the elections? Reassure us first that you will not vote for those who smile at you the sweetest [...]. I would say you are not ready yet, if I even considered

[10] For contemporaneous effects on female labour force participation see Amuedo-Dorantes and Grossbard (2007), for marriage market effects see Charles and Luoh (2010).

you to be able to get ready eventually."[11] One of the main arguments against women's franchise was their lack of military service. Indeed, male politicians went as far as to prohibit women's participation in political associations and assemblies, first in Prussia in 1850 with most states following suit shortly after. This law was in effect until 1908. As a consequence, women's political participation was greatly hindered (Schaser, 2020, Chapter II).

To unite the German women's movement and also to have an international representation, several associations joined in the foundation of an umbrella organisation, the *Bund Deutscher Frauenvereine* (BDF) in 1894. Membership in the BDF became very popular with 70,000 members in 1900, 200,000 in 1908, and 328,000 in 1908 (Schaser, 2020, p. 58). However, as the BDF grew with the goal of uniting the entirety of the German women's movement, so did the tensions within its member associations. In particular, conservative members such as the DEF [German: *Deutsch-Evangelischer Frauenbund*] were openly against women's emancipation and franchise. Even among those in favour of women's emancipation, there was no consensus on how to reach this. While some argued that women first had to earn their place in society through contributing to the country's welfare, others demanded instant equality of men and women (Schaser, 2020, Chapter IV).[12]

**Demand for the franchise.** As such, the BDF refrained from demanding voting rights for women until 1902 when Anita Augspurg and Lida Gustava Heymann founded the first association for women's franchise in Hamburg, the *Deutscher Verband für Frauenstimmrecht*. However, members had different ideas on the extent of the franchise that should be extended to women. In particular, some argued the existing class-based franchise should be kept but extended to women while others demanded a universal franchise for all men and women. This led to the formal division of the association by the foundation of another women's franchise organisation in 1911, the *Deutsche Vereinigung für Frauenstimmrecht*. In 1916, these different associations re-united in the *Reichsverband für Frauenstimmrecht* due to their similar nationalist and patriotic thinking during WWI.

[11] Own translation of the German: "Ihr Weiber wollt wohl an den Urwahlen teilhaben? Wohl aber versichert uns erst, dass ihr nicht denjenigen bevorzugt, der euch bei den Fensterpromenaden am süßesten zulächelt. ... Ich würde sagen, Ihr seid noch nicht reif, wenn ich Euch überhaupt für fähig hielte, reif zu werden." (Schaser, 2020, p. 30)

[12] Another, somewhat separated movement was the worker women's movement (*proletarische Frauenbewegung*. At the end of the 19th century, working-class women put more importance on women's employment and women's double burden of working inside and outside the home (Schaser, 2020, p. 37). They wanted to achieve equality of the sexes through socialism or communism, and therefore, rather than directly advocating for women's rights, supported by overall changes in society towards socialism/communism (Schaser, 2020, p. 11).

In 1913, a third association emerged, the *Deutscher Bund für Frauentimmrecht*, which remained separated (Wischermann, 2003, 112 ff.). In the analysis, we consider suffragette clubs of all three associations.

After women could become members of political associations in 1908, it was the explicit goal of the franchise associations to open local chapters of the head organisations—which we refer to as women's suffragette clubs—to bring attention to the topic of female voting rights as well as to strengthen the mobilization between women fighting for this goal. The main activism of the clubs consisted of lobbying and propaganda for the introduction of female voting rights, and they held frequent meetings and events. Typically, a local chapter would hold two to four public events a year as well as member events once or twice a month, such as evenings of discussion. In addition, the chapter would organise social events, such as teas, theatre visits, and other outings. Regarding propaganda, the local chapters would campaign before elections, disseminate flyers, and initiate petitions and collaborations with local chapters of other non-political women's clubs (Wischermann, 2003, pp. 109–118).

While the higher-level clubs, such as for an entire province, decided on the broader political agenda, local chapters were key for reaching the goals of the social movement by organising these activities locally (Wischermann, 2003, p. 120). The importance of the local chapters for the success of the women's franchise movement was also recognised at the time as the following contemporary quote by the women's activist Agnes Zahn-Harnack demonstrates:

> "The association in a given small village or city may be small in number; there are no important speakers among its members, no special work is carried out – but through the small association the women's circles of the city maintain contact with the overall movement; through it they receive magazines, visits from speakers, work suggestions, and the opportunity to take part in conferences and meetings on a larger scale. And through the small associations the head association or the overall organization always has a base and always the opportunity to spread an idea or a demand very quickly throughout the whole country."[13]

[13] Own translation the German: "Der Verein in Xhausen oder Ystadt ist vielleicht zahlenmäßig schwach; bedeutende Rednerinnen sind nicht unter seinen Mitgliedern, irgendeine Sonder- aufgabe wird nicht betrieben – aber durch den kleinen Verein erhalten die Frauenkreise der Stadt die Verbindung mit der Gesamtbewegung aufrecht; durch ihn bekommen sie Zeitschriften, Besuch von Vortragenden, Arbeitsvorschläge und die Möglichkeit, an Tagungen und Zusammenkünften in größerem Rahmen teilzunehmen. Und durch die kleinen Vereine hat der Spitzenverein oder die Gesamtorganisation immer einen Fußpunkt und immer

### 2.2.2 Men and women during WWI

**Drafting of men.** All men from the birth cohorts 1896 to 1900 were required to serve in the war (Nash, 1977) and exemptions for unfitness were rigorously checked and re-examined in 1915. The main reason for men not to serve in the war was thus their occupation. In particular, those working in factories and mines were likely to not be drafted or to be called back home to continue their work in a war-related industry. The regional military entity responsible for drafting were the so-called *Landwehr-Bezirke* (henceforth recruitment areas) (Koenig, 2020; Meyer, 1905). Note that volunteers to serve in the army were moderate in size.[14]

**Service and labour of women.** Schaser (2020, p. 113) argues that women's agency increased greatly during the war, in particular beyond the home. Early in the war, the BDF proposed a concept on how to best use women for the war effort, and in 1914 founded the National Women's Service [German: *Nationaler Frauendienst*] (NFD). Local women's clubs were merged to form the NFD chapter and feminist activity was largely abandoned in favour of support for the country's war effort. The tasks of the NFD included, among others, charity and legal advice for families of men who were at the front, mobilizing women for labour force participation to fill vacant positions, maintaining food supply, and organizing medical care. Marie-Elisabeth Lüders, a member of the BDF leadership, was put in charge of the Department of War Office to organize female labour supply. In all of these areas, women gained organizational and financial skills, came into contact with local government offices, and gained leadership experience. As Marie-Elisabeth Lüders wrote, "This national responsibility (...) not only gave us the will, but also the strength to take on work and decisions that for most women were far removed from their previous life and work paths." (Lüders, 1936a, p. 7).[15] Underlying this engagement was not only the feeling of national duty, but also the hope that because of their service to the country, women would be able to gain political rights after the end of the war (R. J. Evans, 1978).

Women often had to make important family decisions on their own due to the lack of male family members. In addition, women took over the workplaces of men (but with lower wages) during the war. Especially working-class women faced serious hardship and

die Möglichkeit, einen Gedanken oder eine Forderung sehr schnell durch das ganze Reich zu verbreiten." (Wischermann, 2003, p. 117)

[14]The number of volunteers was around 185,000 men (Verhey, 2000), while the total number of men serving in the German military was around 13.4 million (Koenig, 2020).

[15]Own translation of the German: "Aus dieser nationalen Verantwortung (...) ist nicht nur der Wille, sondern auch die Kraft erwachsen, Arbeiten und Entscheidungen auf uns zu nehmen, die für die meisten Frauen weit ab von ihrem bisherigen Lebens- und Arbeitswege gestanden hatten."

had to enter the labour market to cover for their husband's missing income (Frevert, 2007, p. 149).

### 2.2.3 The introduction of the women's franchise in Germany

When the Emperor announced comprehensive political reforms in 1917, the BDF demanded that the women's franchise be included. In particular, they referred to women's contributions during the war as well as their importance for the economy and population. They argued that women's greater representation had been initiated during the war, and now needed legal support (Schaser, 2020, p. 75).

In 1918, the Council of People's Deputies (*Rat der Volksbeauftragten*) introduced the general franchise, including for women. Historians have reached no consensus as to why voting rights for women were eventually included. Some historians, such as Ute Rosenbusch, argue that political power considerations were the main reason as it was expected that the women's franchise would predominantly benefit the Social Democratic Party of Germany [German: *Sozialdemokratische Partei Deutschlands*] (SPD). Gisela Bock, on the other hand, views the women's movement as a crucial factor. She argues that the joint promotion of the women's franchise of social democratic women, the BDF and the socialist women's movement in 1917 and 1918 led to support of the idea in liberal as well as socialist circles (Schaser, 2020, p. 76).

On January 19, 1919, the first election of the Weimar Republic took place. The female turnout was 87.7% (vs. 89.4% for men). The introduction of female voting rights included passive suffrage, and 285 women ran for parliament. Of these, 37 were elected, so 8.7% of all members of the first parliament were women (Glück, 2018). With the introduction of female voting rights, the suffragette clubs dissolved in 1919 (Schaser, 2020, p. 71).

## 2.3 Data, measurement and descriptives

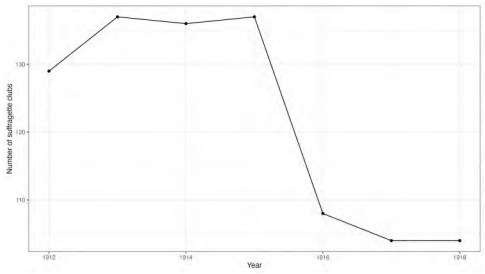
### 2.3.1 Suffragette clubs

**Data source.** To proxy the local demand for voting rights, we create a newly digitized panel dataset of the universe of German suffragette clubs starting in 1912. The data is taken from the yearbook of the *Bund Deutscher Frauenvereine* (BDF), which published a list of all women's associations in Germany with their respective location and head of the club every year from 1912 onwards (Bund Deutscher Frauenvereine, 1912–1918). Figure B.1.1 in the appendix shows a snippet from the original data. Our dataset spans from 1912–1918 and covers all types of suffragette clubs, which were associations dedicated

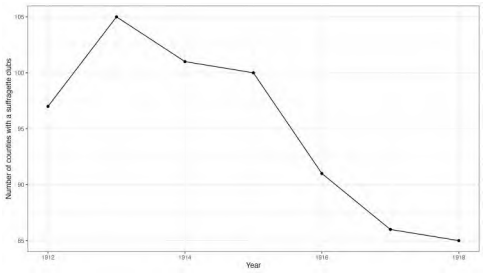
to fighting for female voting rights. We argue that these clubs capture local demand for female voting rights as their goal was to function as local lobby groups to strengthen the mobilization for franchise extension, both by convincing other women and by informing the general public (Wischermann, 2003, p. 120).

**Outcome definition.** The yearbook differentiates between clubs on different regional levels, e.g. the local chapter of the suffragette association for the city of Berlin, and the national club which was also located in Berlin. We define the number of clubs as all types of suffragette clubs, independent of their organizational level. Thus, we count both all local chapters as well as all supra-regional clubs in a location based on three main reasons. First, these clubs are not necessarily located in the capital of the province or region, indicating that local demand governed the decision rather than choosing the state capital by default. For example, the supra-regional club for the province of the Rhineland was located in Aachen instead of Koblenz, which was the capital of the province. Second, some smaller states, such as Lübeck, only have clubs listed as supra-regional clubs for the whole state since the state was too small for additional regional clubs. Thus, a supra-national club in these regions measures local demand. Third, the regional and supra-regional clubs were often managed by different persons even if located in the same city, indicating that more women were seriously involved in the movement when more clubs were present. Thus, our main measure for the demand for the franchise comprises both the regional as well as the supra-regional clubs in each location. In a robustness check, we only use local chapters and find very similar results. No county had more than 5 clubs at any point between 1912 and 1918, and the median number of clubs for counties with at least one club was 1.

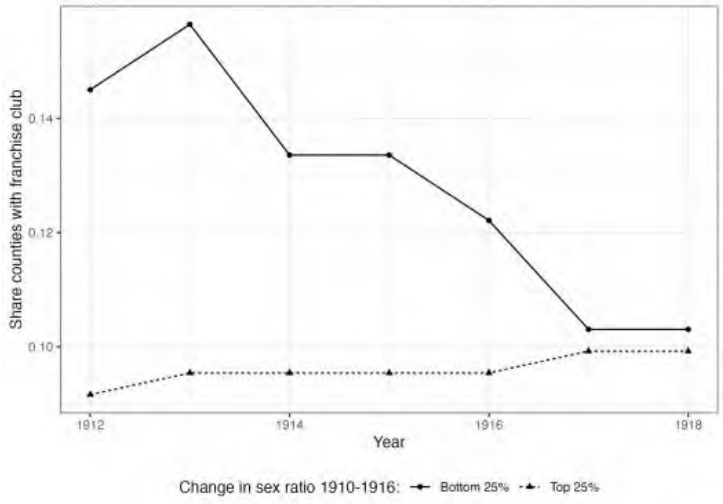
**Evolution over time.** Figure 2.1 shows the evolution in the total number of suffragette clubs over time in Part (a). While there was a slight increase in the number of clubs from 1912 to 1913, the number of suffragette clubs decreased sharply during the war. This is partly due to the merger of associations in 1916 which mechanically reduced the number of suffragette clubs. However, not all the reduction can be explained by this. Looking at the number of counties with a suffragette club in Part (b), a similar pattern can be observed. This indicates a general trend toward less activism concerned with female franchise during the war, consistent with the focus of the women's movement on supporting Germany's war effort. To abstract from the merger of associations, we focus on the binary indicator of the presence of local suffragette club as our main outcome variable. Part (c) shows the share of counties with a suffragette club by this definition for those



(a) Number of suffragette clubs



(b) Counties with a suffragette club

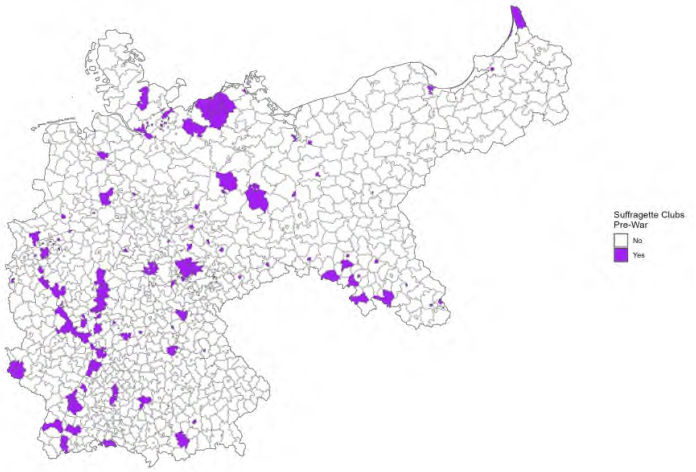



(c) Share of counties with a suffragette club by intensity of missing men

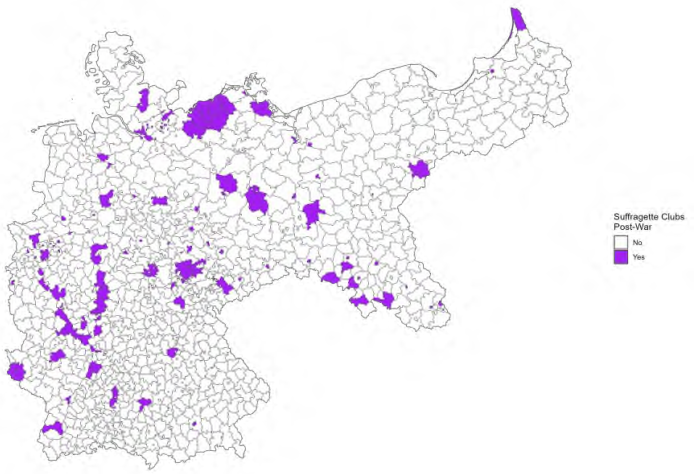
Figure 2.1: The evolution of suffragette clubs over time

*Notes:* The figure shows the total number of (Part (a)) and the number of counties with a suffragette club (Part (b)) for every year between 1912 and 1918, counting clubs on all organizational levels as separate clubs. Part (c) shows the evolution of the share of counties with a suffragette club for counties which experienced the least changes in the sex ratio between 1910 and 1916 (bottom 25% of distribution, solid line) and for those which experienced the most change (top 25%, dashed line).
*Source:* Own illustration. See Table 2.1 for data sources.

which experienced a large decrease in men locally (top 25%, dashed line) and counties which experienced very little change (bottom 25%, solid line). Closures of suffragette clubs exclusively happen in counties with little change in local men while women in counties with a high intensity of missing men kept their clubs open, and even slightly increased the local presence of suffragette clubs.




(a) Before WWI




(b) During WWI

Figure 2.2: Map of the presence of suffragette clubs
*Notes:* The map shows whether a German county had any suffragette club before the war (1912–1913) and during the war (1914–1918) in the borders of 1907.
*Source:* Own illustration based on Bund Deutscher Frauenvereine (1912–1918).

**Distribution across space.** Figure 2.2 shows the spatial distribution of suffragette club before (Part (a)) and during the war (Part (b)). Importantly, suffragette clubs do not seem to be clustered in space but rather seem to be a national movement. Comparing Parts (a) and (b), closures of suffragette clubs can be seen. However, these do not seem to be spatially clustered either but instead occurred across Germany. For example, club closures occurred in Lörrach (south), Danzig (east), Stralsund (north), and Bonn (west). This indicates that there is variation across space to be used in the regional analysis.

### 2.3.2 Missing men

We measure the extent of missing men by the change in the sex ratio during the war compared to before the war in each county, using census data from 1910 and 1916 for the German Empire (Kaiserliches Statistisches Amt, 1915; Kriegsernährungsamt, 1916).[16] The data separately reports the male and female population numbers of individuals who are currently present in each county.[17] Thus, the count of men does not include men who are absent from their home county due to the war, either temporarily or permanently. Our main explanatory variable is the change in the female-to-male ratio before the war (1910) and during the war (1916):[18]

$$\Delta fpm_i = \frac{women_{i,1916}}{men_{i,1916}} - \frac{women_{i,1910}}{men_{i,1910}} \tag{2.1}$$

Figure 2.3 shows the regional distribution of this difference over the German counties.

### 2.3.3 Supplementary data sources

**Controls.** Our main DiD specification includes county FE, which hold fixed any time-invariant differences between counties. To allow for time-varying effects of possible confounders, we interact a host of control variables with year FE in our baseline specification: The presence of a suffragette club before the war allows for different time-trends of counties with pre-war differences in the support of women's franchise. The total share of men employed in the industry sector reflects that men working in the industry sector were more likely to be exempt from drafting because their occupation was considered

[16] For Prussian counties in 1910, we use the already digitized data by Galloway (2007).

[17] The male population includes the military personnel stationed at the respective county. We flexibly control for their share in total population in 1910 and in a robustness check also exclude counties with an increase in the sex ratio.

[18] We conduct a robustness check using the female to male ratio in 1917 taken from Kriegsernährungsamt (1918) instead of 1916.

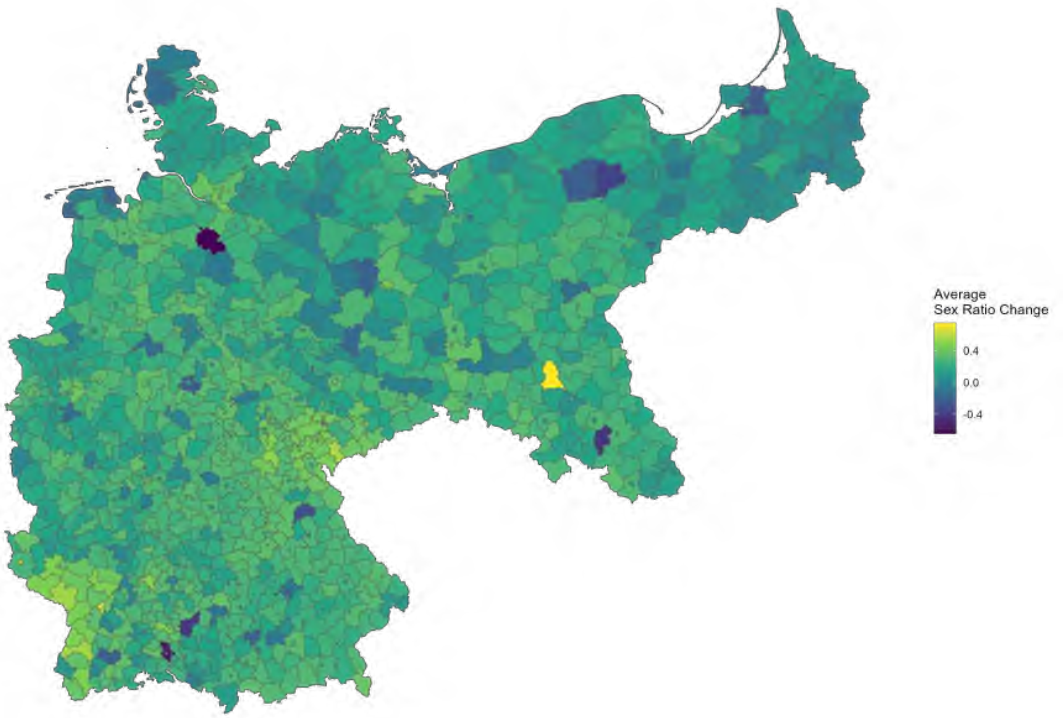



Figure 2.3: Map of the missing men

*Notes:* The map shows the average change of the female-to-male ratio from before the war (1910) to during the war (1916) in German counties in the borders of 1907.
*Source:* Own illustration based on Kaiserliches Statistisches Amt (1915); Kriegsernährungsamt (1916).

important for the war effort.[19] We use the occupational census of 1907 to construct this measure (Kaiserliches Statistisches Amt, 1910). We also control for the share of men already employed in the military taken from the same source. Because younger men were more likely to be drafted, we control for the share of men between 12 and 18 in 1910, thus aged 20 to 26 years in 1918.[20] In addition, we include interactions of year FE and the

[19]The industry sector covers mining, pit and quarry, metal, machines, chemicals, textiles, paper, timber, food, clothing, construction, and print.

[20]Men aged 20 to 27 were part of the standing army (*Gesetz betreffend die Verfassung des Deutschen Reiches* 1871), and the youngest cohort drafted for WWI were 18 years old in 1918 (Nash, 1977).

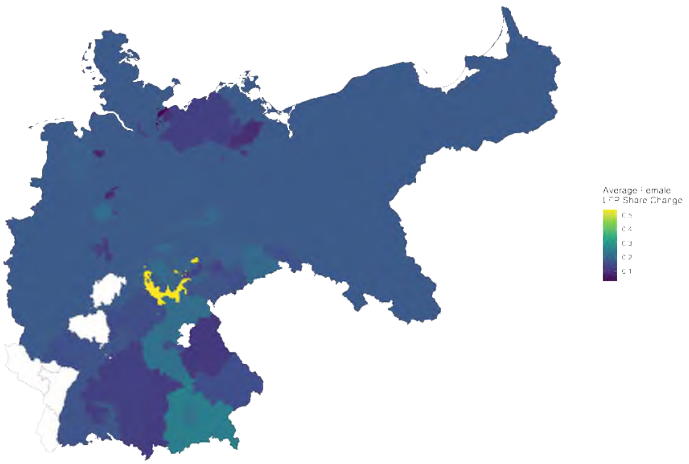
share of urban population, the share of protestants, and the share of foreign population taken from (Thome, 2006b).

**Recruitment areas.** In our analysis, we rely on the fact that conditional on local industry and age structure, differences in the gender ratio were driven by quasi-random variation in the drafting intensity. The regional military entity responsible for drafting were the recruitment areas, which on average consisted of 3.4 counties. To exploit the variation between recruitment areas, we digitized information on these military entities from Reichsamt des Innern (1914[b]). Figure B.1.3 in the appendix shows the recruitment area borders overlaid on the county borders. We will cluster our standard errors at the level of the recruitment areas as this is the level at which the decisions on drafting intensity were made.

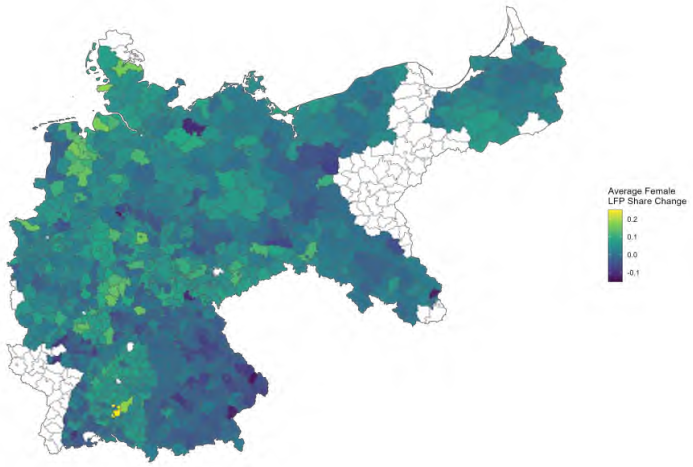
**Labour force participation.** To investigate whether women's economic needs are a contributing factor for demand for political representation, we use data on the increase in female labour force participation which mostly resulted from poor women struggling to feed their families in the absence of their husbands' income. We use two different datasets: First, we construct a newly digitized dataset on employment before the war (1913) and during the war (1918) taken from Reichsamt des Innern (1914[a]) and Reichsamt des Innern (1919[a]).[21] The data differentiates between 40 German regions and includes the number of men and women employed across 16 different sectors. The data covers all firms with at least 10 employees. Since women were often employed in smaller firms or had a quasi self-employment at home (e.g. in the textiles sector), this data underestimates female labour force participation. Figure 2.4a shows the regional distribution of the change in female labour force participation from before to during the war.

As this data only differentiates between 40 regions, with Prussia being one region, we use a second dataset: We compare female employment recorded in the occupational census of 1925 taken from Inter-University Consortium For Political And Social Research (2005) to employment levels in 1907, both measured at the county level. We construct a measure of the change in female full-time employment from before the war to after the war. This measure will also underestimate the increase in female employment as men had already returned to their jobs in 1925. Figure 2.4b shows the respective regional distribution of the change in female labour force participation from before to after the war.

[21]The data for 1918 was collected between 1914 and 1918, with the majority being collected in 1918.



(a) Change in female labour force participation 1913–1918




(b) Change in female labour force participation 1907–1925

Figure 2.4: Change in female labour force participation

*Notes:* The map shows the change in the share of female labor force participation. Part (a) shows the share of female workers employed in the total number of employed persons in firms with at least 10 employees from 1913 to 1918 in German regions, Part (b) shows the change in the share of fulltime female workers from 1907 to 1925 in German counties.

*Source:* Own illustration based on Reichsamt des Innern (1914[a]); Reichsamt des Innern (1919[a]); Kaiserliches Statistisches Amt (1910); Inter-University Consortium For Political And Social Research (2005).

**National women's service.** During WWI, women organized in the NFD to support the German war effort by various tasks, including charity for families, organizing food supply, and mobilizing the (female) labour force. In 1916, the BDF published the locations of local chapters of the NFD, which we digitized and geolocated to proxy for women's leadership experience (Bund Deutscher Frauenvereine, 1912–1918). Figure 2.5 shows the regional distribution of counties with and without a local NFD chapter.

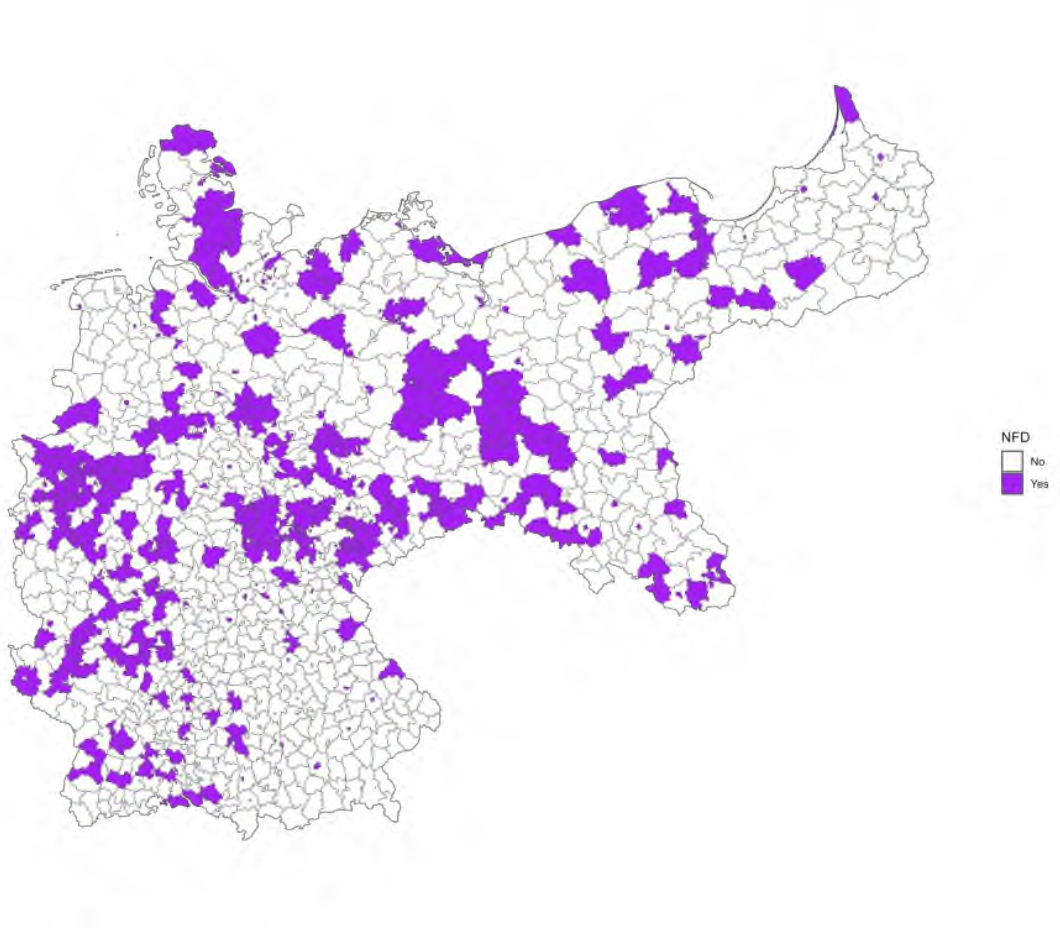



Figure 2.5: Map of the presence of National Women's Service
*Notes:* The map shows whether a German county had any chapter of the National Women's Service in 1916.
*Source:* Own illustration based on Bund Deutscher Frauenvereine (1912–1918).

### 2.3.4 Further outcome variables

**Other women's clubs.** To understand whether missing men affected specifically women's political engagement or also other types of women's organizations, we consider two other women's organizations: The DEF (*Deutsch-Evangelischer Frauenbund*) and the ADLV (*Allgemeiner Deutscher Lehrerinnenverein*). Both clubs were part of the BDF, from which we digitized yearly reports on the locations of these clubs from 1912–1917 (Bund Deutscher Frauenvereine, 1912–1918).[22] The DEF was mostly focused on charitable work, and opposed the introduction of female suffrage. The ADLV represented the interests of female teachers and supported the goal of female education (Schaser, 2020). Figure B.1.4 in the appendix shows the share of counties with either of these clubs separately for those counties which experienced a large decrease in men locally (top 25%, dashed line) and counties which experienced very little change (bottom 25%, solid line). The presence of the DEF stayed relatively stable throughout the war, while the presence of the ADLV increased in both types of counties, irrespective of the change in the sex ratio.

**Election lists.** Beyond women's demand for their voting rights, we want to understand whether the experience of agency due to missing men during WWI also led to women taking up their newly acquired voting rights following their introduction after WWI in 1918. We are the first to digitize the universe of politicians running for parliament in the first election of the Weimar Republic's national assembly in 1919 taken from Reichsamt des Innern (1919[b]).[23] For each of the 37 constituencies, the data lists the politicians running for parliament including information on their name and party affiliation. Based on their first name, we coded the politician's gender. In total, the list covers 37 constituencies with a median of 6 party lists each, of which 88% had at least one female candidate. For 15.4% of the lists, a women on the list was elected into the national assembly. Note that Voters voted for entire lists instead of individual candidates, and the seats were distributed among the lists according to the vote share of the respective list in each constituency. Figure 2.6 shows the regional distribution of the average share of female candidates across the constituencies of the Weimar Republic.

**Voting turnout.** In addition to analysing women's newly gained passive voting rights, we also look at their active voting behaviour. To do so, we newly digitized voting turnout

[22] Note that complete information on the presence of the ADLV is only available for 16 German states, so for 634 counties.

[23] Figure B.1.2 in the appendix shows a snippet from the original data.

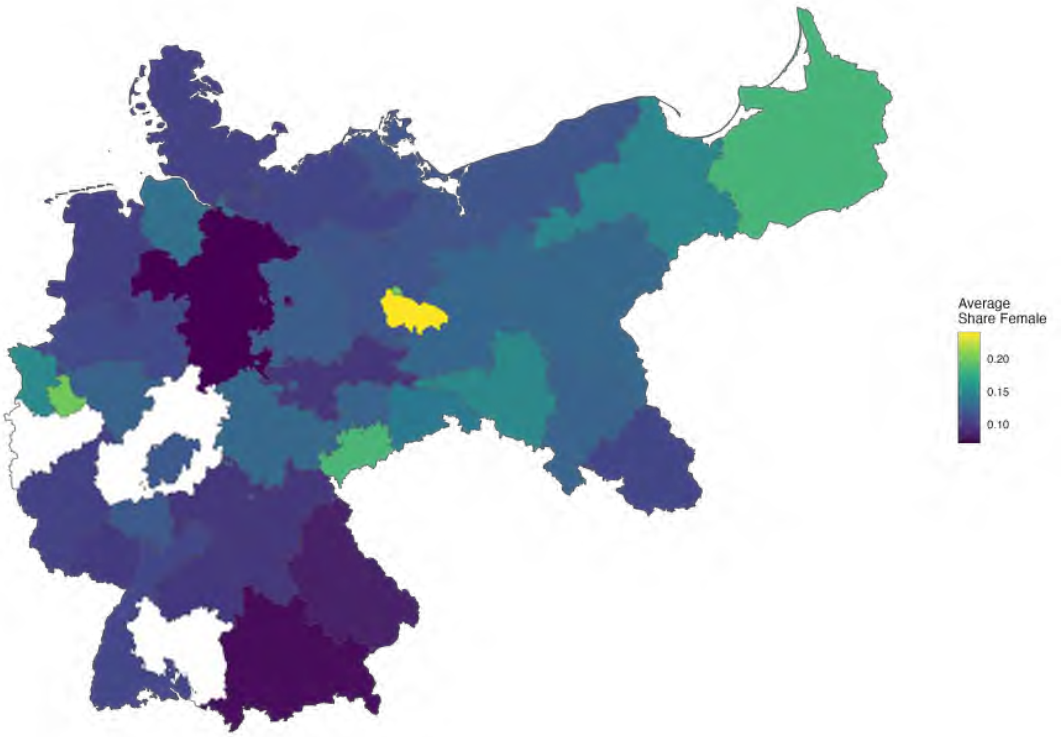



Figure 2.6: Map of the share of women among candidates across constituencies 1919
*Notes:* The map shows the average share on women among candidates on the ballot for each constituency of the 1919 election for the National Assembly. White fillings indicate missing data.
*Source:* Own illustration based on Reichsamt des Innern (1919[b]).

by gender from 1919 to 1930 from Bremme (1956). In the Weimar Republic, it was legal to count votes separately by gender, but only a small subset of locations made use of that possibility. In total, we have voter turnout for 53 counties, covering on average 2.5 elections. Figure 2.7 shows a map of these counties. In 28 counties, the data also reports which party each gender voted for, which we use to inspect not only voter turnout but also voting behaviour.

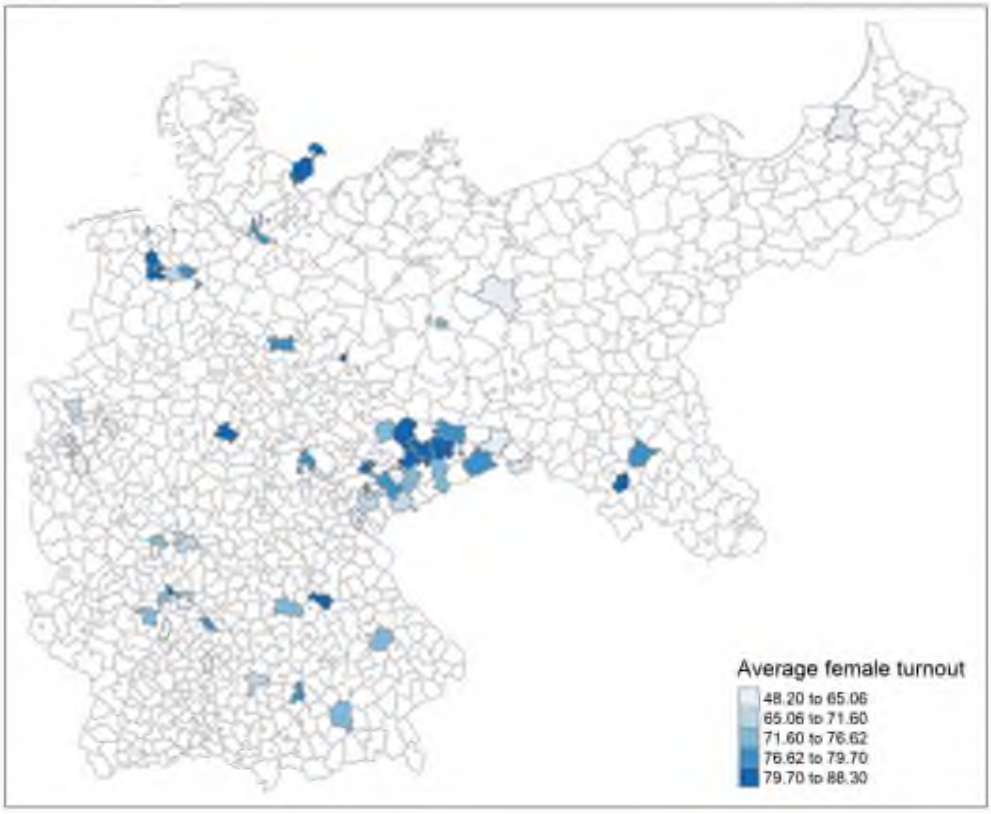



Figure 2.7: Map of the locations with voter turnout by gender
*Notes:* The map shows the average female turnout in elections from 1919–1930 for which the turnout was recorded separately by gender.
*Source:* Own illustration based on Bremme (1956).

### 2.3.5 Descriptive statistics

Table 2.1 shows the descriptive statistics for the main explanatory variable, all outcome variables, the contributing factors, as well as the control variables. In Table **??** we show the average values of pre-war variables separately for counties with high sex ratio changes from 1910 to 1916 (25% of the distribution) and low changes (bottom 25% of the distribution).

## 2.4 Empirical strategy

To estimate the effects of an increase in women's agency on the local demand for political participation, we exploit changes in the local sex ratio induced by the war. To that end, we

Table 2.1: Descriptive Statistics

| | N | Mean | Std. Deviation | Min | Max |
|---|---|---|---|---|---|
| Change in female-to-male ratio | 1048 | 0.215 | 0.137 | -0.645 | 0.755 |
| Any clubs before the war | 1048 | 0.103 | 0.304 | 0.000 | 1.000 |
| Any clubs during the war | 1048 | 0.100 | 0.300 | 0.000 | 1.000 |
| Any DEF clubs before the war | 1048 | 0.121 | 0.326 | 0.000 | 1.000 |
| Any DEF clubs during the war | 1048 | 0.124 | 0.330 | 0.000 | 1.000 |
| Any ADLV clubs before the war | 633 | 0.142 | 0.350 | 0.000 | 1.000 |
| Any ADLV clubs during the war | 633 | 0.158 | 0.365 | 0.000 | 1.000 |
| Change in female LFP 1907-1925 | 944 | 0.025 | 0.045 | -0.151 | 0.253 |
| Change in female LFP 1913-1918 | 1006 | 0.170 | 0.037 | 0.028 | 0.543 |
| Presense of National Women's Service | 1048 | 0.290 | 0.454 | 0.000 | 1.000 |
| Female-to-male ratio before the war | 1048 | 1.026 | 0.068 | 0.650 | 1.437 |
| Share of men employed in industry | 1048 | 0.389 | 0.165 | 0.102 | 0.855 |
| Share of men employed in military | 1048 | 0.026 | 0.059 | 0.000 | 0.448 |
| Male share 12 to 18 | 1048 | 0.129 | 0.013 | 0.063 | 0.179 |
| Urban population share 1900 | 1021 | 0.407 | 0.311 | 0.000 | 1.000 |
| Lutheran population share in 1900 | 1021 | 0.569 | 0.385 | 0.001 | 1.042 |
| Share foreign population in 1900 | 1018 | 0.071 | 0.190 | 0.000 | 0.900 |
| Female candidate | 200 | 0.880 | 0.326 | 0.000 | 1.000 |
| Female elected | 208 | 0.154 | 0.362 | 0.000 | 1.000 |
| Female turnout | 133 | 69.099 | 13.067 | 22.9 | 91.6 |

*Notes:* The table presents mean, standard deviation, minimum and maximum of the change in the sex ratio, the outcome variables, the contributing factors, and control variables at the county level. It shows the share of female candidates and elected women on the ballot level, and the female turnout on the election level. *Source:* Census data for sex ratio changes from Kaiserliches Statistisches Amt (1915); Kriegsernährungsamt (1918, 1916); data for suffragette clubs, protestant clubs, teacher clubs, and national women's service taken from Bund Deutscher Frauenvereine (1912–1918); female employment taken from Inter-University Consortium For Political And Social Research (2005); Kaiserliches Statistisches Amt (1910); Reichsamt des Innern (1914[a], 1919[a]); ballot data based on Reichsamt des Innern (1919[b]); voting data taken from Bremme (1956).

classify counties into high-change counties (treatment) and low-change counties (control). Specifically, we define high-change counties as the top 25% of the distribution of changes in the female-to-male ratio from 1910 to 1916, thus experiencing the largest decrease in local men. Low-change counties are accordingly defined as the bottom 25% of the distribution. We then compare changes in the presence of local suffragette clubs from before to during the war across these groups of countries in a difference-in-differences (DiD) setting,

Table 2.2: Balancedness Table

| | Bottom 25% | Top 25% | Difference | p-value | |
|---|---|---|---|---|---|
| Change in female-to-male ratio | 0.036<br>(0.120) | 0.357<br>(0.069) | 0.321 | 0.000 | *** |
| Any clubs before the war | 0.156<br>(0.364) | 0.103<br>(0.305) | -0.053 | 0.069 | * |
| Any DEF clubs before the war | 0.156<br>(0.364) | 0.111<br>(0.314) | -0.046 | 0.124 | |
| Any ADLV clubs before the war | 0.211<br>(0.409) | 0.172<br>(0.379) | -0.039 | 0.388 | |
| Female-to-male ratio before the war | 1.004<br>(0.076) | 1.041<br>(0.070) | 0.037 | 0.000 | *** |
| Share of men employed in industry | 0.375<br>(0.158) | 0.451<br>(0.164) | 0.076 | 0.000 | *** |
| Share of men employed in military | 0.071<br>(0.091) | 0.015<br>(0.042) | -0.056 | 0.000 | *** |
| Male share 12 to 18 | 0.128<br>(0.015) | 0.126<br>(0.013) | -0.002 | 0.093 | * |
| Urban population share 1900 | 0.526<br>(0.339) | 0.415<br>(0.311) | -0.111 | 0.000 | *** |
| Lutheran population share in 1900 | 0.607<br>(0.378) | 0.619<br>(0.377) | 0.012 | 0.716 | |
| Share foreign population in 1900 | 0.098<br>(0.212) | 0.027<br>(0.110) | -0.071 | 0.000 | *** |
| Group size | 262 | 262 | | | |

*Notes:* The table presents mean and standard deviation (in parentheses) of the change in the sex ratio and the pre-war variables for counties which experienced very little change (Bottom 25%), and for counties which experienced a large increase in the sex ratio from 1910 to 1916 (Top 25%) *Source:* See Table 2.1.

estimated as the following two-way fixed-effects regression:

$$club_{i,t} = \alpha + \delta_i + \delta_t + \beta High\_Change_i \times post_t + \gamma_t X_i + \varepsilon_{it}, \tag{2.2}$$

where $club_{i,t}$ is a binary indicator of whether a suffragette club exists locally in county $i$ in year $t$. $\alpha$ is a constant, $\delta_i$ and $\delta_t$ are county and year fixed effects, respectively. *post* is a dummy variable which is zero for 1912 and 1913 (before the war) and one for 1914 through 1918 (during the war). $\beta$ is the parameter of interest capturing the change in the presence of suffragette clubs in high-change counties relative to low-change counties from before to during the war. $\gamma_t X_i$ is a vector of control variables at the county level

interacted with a time-varying indicator. $\varepsilon_{it}$ is the idiosyncratic error term. We always cluster standard errors at the level at which drafting decisions were made, namely the recruitment area.

The variation in local drafting decisions is not per se random. One possible confounder could be the local industry structure, as men working in industries involved in the war effort were more likely to be exempt from drafting, and places with a larger industry sector may evolve differently in terms of their demand for political participation. Similarly, counties with a higher share of men working in the military face a higher probability of men being drafted, and may also have different trends in demand for female enfranchisement. In addition, young men were predominantly drafted for the military, and younger areas may have different changes in gender norms and demand for female franchise. As these factors are time-invariant, they are captured by the county fixed effects. To bias our results, these factors would need to not only be correlated with our treatment indicator but also be related to differential trends in the local club presence. To capture this concern, our baseline specification of equation 2.2 includes the share of men employed in the industry sector in 1907, the share of men employed in the military sector in 1907, and the share of men at draftable age interacted with a time-varying indicator to allow for differential influences over time.[24] In addition, we also add interactions of a time-varying indicator and the presence of a suffragette before the war, the pre-war sex ratio, and county-level controls (urban share, protestant share, share of foreign population).

The main identifying assumption is that high-change counties, which saw an increase in missing men, would have been on the same trend of club presence as low-change counties. To formally check for pre-trends, we exploit the fact that we have yearly data on the presence of suffragette clubs, including two years before the beginning of the war, by estimating a flexible event-study setup where we allow the effect to vary over time:

$$club_{i,t} = \alpha + \delta_i + \delta_t + \sum_{\substack{t=1912 \\ t \neq 1913}}^{1918} \beta_t High\_Change_i + \gamma_t X_i + \varepsilon_{it}, \tag{2.3}$$

[24]The share of men employed in the industry sector is measured as the share of men working in mining, pit and quarry, metal, machines, chemicals, textiles, paper, timber, food, clothing, construction, and print in the full-time male working population. Since we already implicitly control for initial differences in gender composition by having a change in the gender composition as our main explanatory variable, we take the full-time male working population as the denominator, assuming that all men work full-time, because both come from the same data source of the occupational census in 1907. We proxy the share of draftable men by the share of men who were between 12 and 18 years old in 1910, thus aged 20 to 26 years in 1918. The reason is that men aged 20 to 27 were part of the standing army (*Gesetz betreffend die Verfassung des Deutschen Reiches* 1871), and the youngest cohort drafted for WWI were 18 years old in 1918 (Nash, 1977).

where everything is defined as above and $High\_Change_i$ is a binary indicator equal to 1 for a high-change county, and 0 otherwise. This setting also allows us to investigate heterogeneities in the effect during the war.

**Cross-sectional estimation.** Since we do not have full panel data for all supplementary outcomes, we further estimate a cross-sectional equation where the change in the local sex ratio is the main explanatory variable.

We estimate the following equation:

$$club_{i,s} = \alpha + \beta \Delta fpm_i + X_i' \gamma + \delta_s + \varepsilon_{i,s} \tag{2.4}$$

where the subscript $i$ denotes counties and $s$ states. $club_{i,s}$ is one if a county had a club in any year during WWI (1914–1918). $\Delta fpm_i$ measures changes in the local sex ratio from 1910 to 1916. $X_i$ includes the local pre-war sex ratio, a binary indicator whether a suffragette club existed locally before the war, the local share of men employed in the industry sector and in the military in 1907, and the share of men between 12 and 18 in 1910. $\delta_s$ are state fixed effects, as German states had historically differed in allowing women to organise in formal associations. In a robustness check, we additionally control for the share of urban population, the share of protestants, and the share of foreign population in 1900.

## 2.5 Agency and women's demand for the suffrage

### 2.5.1 Fight for the vote: Suffragette clubs

**DiD estimates.** Table 2.3 shows the results of the DiD specification of equation 2.2, comparing the evolution in local suffragette clubs counties which experienced a very large change in the local female-to-male ratio (top 25%, treatment group) to those which experienced a relatively small change (bottom 25%, control group). Column (1) shows the results without any controls. Counties which experienced the largest change in the local sex ratio, i.e. a lot of men were missing locally, have a 3.5 percentage point higher likelihood to keep open their local suffragette club during the war. In column (2), we add controls for industry structure, age composition, urban share, protestant share, share of foreign population, pre-war presence of clubs and sex ratio interacted with year FE. Both estimates are significant at the 5% level. In column (3), we split the sex ratio change by the median instead of comparing the top to the bottom quartile. As expected, the resulting estimate is lower, but qualitatively similar. Column (4) shows the estimation

of a continuous DiD, replacing the dummy for high vs. low sex ratio change with the continuous change in the sex ratio. The effect is positive and highly significant.

Table 2.3: The effect of missing men on suffragette clubs – DiD results

| | (1) Without controls | (2) Baseline | (3) Median split | (4) Continuous DiD |
|---|---|---|---|---|
| Top 25% sex ratio change × post | 0.035** | 0.038** | | |
| | (0.015) | (0.016) | | |
| Above 50% sex ratio change × post | | | 0.017* | |
| | | | (0.010) | |
| Sex ratio change × post | | | | 0.113*** |
| | | | | (0.041) |
| Controls × year FE | No | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.876 | 0.893 | 0.893 | 0.893 |
| N | 3668 | 3549 | 7112 | 7112 |

*Notes:* The table presents the DiD estimates of $\beta$ of equation 2.2. The estimation compares the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%). Column (1) includes year and county fixed effects. In column (2), we add controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. In column (3), the definition of the treatment and control group is relaxed and counties are now split by median. In column (4), we replace the binary treatment definition with the continuous change in sex ratio. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

In our preferred specification in column (2), we find that after the onset of WWI, counties which saw a large increase in their female to male ratio are 3.8 percentage points more likely to have a suffragette club open, compared to counties which saw a low change in their female to male ratio. Given that only 10% of counties had a suffragette club prior to the war, this is a substantial effect corresponding to 38% evaluated against the baseline.

Figure 2.8 shows the results of the event study specification in equation 2.3, allowing for the estimates to vary by year. While we do not have data on a long pre-period, the results are still reassuring as there is no differential trend between the two groups of counties between 1912 and 1913. This suggests that, just before the war, clubs were not

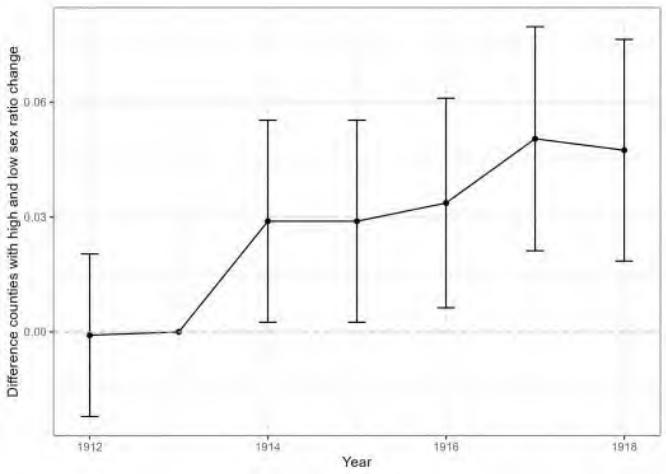



Figure 2.8: Event study of the effect of missing men during WWI

*Notes:* The figure shows the event study estimates on $\beta_t$ of equation 2.3 for the years 1912-1918, 1913 being the omitted base category. The estimation compares the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%). The regression includes controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level.

*Source:* Own illustration. See Table 2.1 for data sources.

systematically opened or closed in regions which later experience a larger change in the local sex ratio. Looking at the evolution of the gap in suffragette clubs during the war, it can be seen that counties with a large change in the sex ratio were more likely to have a suffragette club locally in all war years compared to counties with little changes. These differences are increasing over time, indicating that women in high-change counties keep their local suffragette clubs open while women in other counties increasingly close their club during the war.

**Robustness to sample choice.** In Table 2.4, we test the robustness of our baseline specification to sample choices. In column (2), we exclude counties which experience a relative increase in men, e.g. because of a military base. In column (3), we restrict the sample to counties which were still part of Germany after the Versailles Treaty in 1919. The coefficient is positive and similar in magnitude.

To lend further credibility to the difference-in-differences (DiD) design, in column (4) we additionally apply a border discontinuity design following Dube, Lester, and Reich (2010) and Lichter, Löffler, and Siegloch (2021). Here, we exploit that drafting decisions were made on the level of recruitment areas, which was an administrative unit only relevant for military decisions. We restrict the analysis to counties that share a border but lie on opposite sides of a recruitment area border and replace the county FE with county-pair FE, arguing that neighbouring counties are comparable in unobservables but differ in their recruitment process.[25] Figure B.1.5 in the appendix confirms that neighbouring counties on opposite sides of a recruitment area border are indeed more likely to have a different treatment status compared to neighbouring counties that lie within the same recruitment area. The coefficient remains positive and marginally significant.

In column (5), we restrict the sample to include only the time periods 1912 and 1916 as the treatment definition is derived from changes in the sex ratio from 1910 to 1916 (and the earliest period of measurement for clubs is 1912). In column (5), we instead define treatment status based on changes in the sex ratio from 1910 to 1917. The results remain very similar.

Table 2.4: The effect of missing men on suffragette clubs – Robustness to sample choices

| | (1) Baseline | (2) Excl. increase in men | (3) 1919 borders | (4) Border sample | (5) 1912 and 1916 only | (6) Treatment based on 1917 |
|---|---|---|---|---|---|---|
| Top 25% sex ratio change × post | 0.038** | 0.039** | 0.025 | 0.025* | 0.035** | 0.042** |
| | (0.016) | (0.016) | (0.017) | (0.015) | (0.017) | (0.018) |
| Controls × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County-pair FE | No | No | No | Yes | No | No |
| County FE | Yes | Yes | Yes | No | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.893 | 0.891 | 0.898 | 0.856 | 0.922 | 0.892 |
| N | 3549 | 3421 | 3213 | 4928 | 1014 | 3556 |

*Notes:* The table presents the robustness of the DiD estimates of $\beta$ of equation 2.2 to sample choices. Column (1) repeats our baseline estimate from Table 2.3, column (2), which compares the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%). In column (2), counties which experienced an increase in local men are excluded. Column (3) restricts the analysis to counties which are still part of Germany after the end of WWI. Column (4) restricts the sample to neighbouring counties in opposite recruitment areas, replacing the county fixed effects with county-pair fixed effects. Column (5) only compares 1912 to 1916. In column (6), we use the sex ratio change from 1910 to 1917 (instead of 1916) for the definition of high and low change in the sex ratio. All columns include controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level for all columns, and additionally at the county-pair level for column (4).* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

[25] Following Lichter, Löffler, and Siegloch (2021), we discard county pairs with very short borders below 2km, and, in the case of multiple bordering counties, restrict the analysis to county pairs with the longest border.

**Robustness to choice of outcome.** We further test whether our results are driven by the specific choice of outcome, the probability that any suffragette club is present in a given county. The results are displayed in Table 2.5. We first restrict the clubs to local chapters of clubs in column (2), excluding headquarters of larger regions that might be located in a city.[26] Our results are robust to this. In columns (3) and (4), we use the number of suffragette clubs as the dependent variable. We find very similar results, likely due to the fact that most counties do not have more than one club. We still prefer to use the binary club indicator in our main specification as it can deal with the fact that the two main associations for the women's franchise merged in 1916, mechanically reducing the number of clubs in counties in which both associations were active before their union.

Table 2.5: The effect of missing men on suffragette clubs – Robustness to choice of outcomes

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Baseline | Only local chapters | Number clubs | Number local chapters |
| Top 25% sex ratio change × post | 0.038** | 0.033** | 0.058** | 0.042** |
| | (0.016) | (0.017) | (0.025) | (0.019) |
| Controls × year FE | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.893 | 0.874 | 0.895 | 0.879 |
| N | 3549 | 3549 | 3549 | 3549 |

*Notes:* The table presents the estimates of $\beta$ from equation 2.2, using different outcomes. Column (1) repeats our baseline estimate from Table 2.3, column (2), which compares the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%). In column (2), we restrict the dependent variable to the presence of local chapters of suffragette clubs only, excluding higher-level clubs. Columns (3) and (4) repeat this for the number of clubs (local chapters) instead of a binary presence indicator. All columns include controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
*Source:* See Table 2.1.

[26]For instance, in 1916, Essen hosted both a local chapter and the provincial club representing the Rhine region.

**Cross-sectional estimates.** We compare these results to the cross-sectional estimation of equation 2.4. The results are shown in Table 2.6 where we investigate in how far the change in the local sex ratio affects the probability of having any suffragette club locally. We subsequently build up our set of main controls, starting by controlling for the presence of a suffragette club prior to the war and the pre-war sex ratio as this likely captures pre-existing differences in women's attitudes and local gender norms. We continue by including controls for the industry composition and age structure in column (2) to capture differences in drafting probabilities, which might be correlated with local gender norms. In column (3), we add controls for the urban share, protestant share, and share of foreigners. The coefficient of interest is positive and significant across all of these specifications.

In column (4) of Table 2.6, we restrict the sample to the counties in our DiD sample which slightly increases the size of the point estimates as would be expected when restricting the sample to high-intensity treatment counties. In terms of magnitude, this result is very similar to our DiD results: For counties in the DiD sample, the mean change in the sex ratio in high-change counties is 0.365 compared to 0.061 in low-change counties. Scaling the coefficient of column (4) by this difference would result in a 3.6 percentage point higher probability that a local suffragette club is not closed ((0.365-0.061)*0.120), which is very similar to the effect size of 3.8 percentage points of our baseline specification (column (2) in Table 2.3).

In Table B.2.1 in the appendix, we bound the effect of missing men on the continued presence of a suffragette club, adopting the approach proposed by Oster (2019); see Appendix B.2 for details on the computation of bounds. In column (1), we report the estimate of the restricted model which only includes state FE (0.024). Column (2) reports the estimate of the controlled model which additionally controls for all control variables used in the cross-sectional analysis, including pre-war presence of a suffragette club, sex ratio, and male industry and age structure (cf. column (3) of Table 2.6, estimate of 0.094). The R-squared increases substantially from 0.092 to 0.716 when including these controls, highlighting their importance for explaining the continued presence of a local suffragette club. Under the conservative assumption that non-random unobservable determinants of sex ratio changes are as strong as these observable determinants, we obtain a lower bound of 0.07 (column (3)) and an upper bound of 0.119 (column (4)) if they go in the same or the opposite direction as observable determinants. These bounds lie within the 95% confidence interval of the controlled estimate, are sizeable and clearly different from zero. Overall, this analysis thus supports our earlier findings that missing men lead to women

Table 2.6: The effect of missing men on suffragette clubs – cross-sectional results

| | (1) Pre-war clubs and sex ratio | (2) Industry and age structure | (3) Population controls | (4) DiD sample |
|---|---|---|---|---|
| Change in female-to-male ratio | 0.071* | 0.083** | 0.094** | 0.120** |
| | (0.037) | (0.041) | (0.043) | (0.049) |
| Pre-war sex ratio | 0.116 | 0.253** | 0.162 | 0.080 |
| | (0.094) | (0.111) | (0.103) | (0.122) |
| Any suffragette clubs pre-war | 0.812*** | 0.784*** | 0.769*** | 0.711*** |
| | (0.041) | (0.044) | (0.048) | (0.065) |
| Industry structure | No | Yes | Yes | Yes |
| Age structure | No | Yes | Yes | Yes |
| Population controls | No | No | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| $R^2$ | 0.714 | 0.718 | 0.716 | 0.714 |
| N | 1048 | 1048 | 1016 | 1014 |

*Notes:* The table presents the estimates of $\beta$ from equation 2.4. The dependent variable is 1 when a suffragette club is present during the war locally, and 0 otherwise. The main explanatory variable is the change in the female-to-male ratio from 1910 to 10916. We always control for the presence of suffragette clubs before the war and the pre-war sex ratio. We add subsequently more pre-war control variables in columns (2) to (3): In column (2), we add controls for the share of men employed in industry and military in 1907, and the share of men between 12 and 18 in 1910. In column (3), we additionally control for share of urban population, the share of protestants, and the share of foreign population in 1900. In column In column (4), we restrict the sample to counties used in the DiD analysis presented in Table 2.3. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

keeping local suffragette clubs open more often—even if there were large unobservable and non-random determinants of sex ratio changes during the war.

### 2.5.2 Other women's clubs

After having shown that a higher rate of missing men made it more likely that women kept suffragette clubs open during the war, we now show that our results do not reflect a general collective action effect. We therefore estimate equation 2.2 and replace the presence of a suffragette club with the presence of other women's clubs: The DEF and the ADLV respectively. Table 2.7 shows the results: We find that counties with more missing men do not show a statistically significant difference in their probability of having another women's club during the war. If anything, an increase in missing men reduced

the probability of having a club of the DEF, which opposed the introduction of female suffrage. The effect on the presence of the ADLV is close to zero and insignificant. This indicates that the increase in agency for women during WWI did not lead to higher levels of any type of female organization, but specifically affected clubs which demanded political participation.

Table 2.7: The effect of missing men on other women's clubs – DiD results

| | (1) Suffragette clubs | (2) Protestant clubs | (3) Teacher clubs |
|---|---|---|---|
| Top 25% sex ratio change × post | 0.038** | -0.015 | 0.009 |
| | (0.016) | (0.012) | (0.021) |
| Controls × year FE | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| $R^2$ | 0.893 | 0.963 | 0.959 |
| N | 3549 | 3042 | 1782 |

*Notes:* The table presents the DiD estimates of $\beta$ of equation 2.2 with different club types. The estimation compares the evolution of local women's clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%). Column (1) is our baseline specification. In column (2), the dependent variable is a binary indicator for the presence of a protestant women's club, and for the presence of women's teacher club in column (3). All columns include controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
*Source:* See Table 2.1.

## 2.6 The role of costs and benefits of political representation

The previous results highlight how an increase in agency leads to an increased demand for political representation. Why do people need agency to fight for representation? Political representation is a way to advocate for and implement policies in one's interest– something that will be desirable to people as soon as preferences and interests are heterogeneous across the population. In that sense, it is remarkable that not all members of under-represented groups demand representation. We argue that demand for political representation is shaped by a cost-benefit consideration that weighs these benefits against the costs of the fight, most notably social costs. To guide the following discussion, we

first develop a simple stylized framework formalising this idea. We then present empirical evidence for the role that the costs and benefits played in the German suffragette movement during WWI.

### 2.6.1 Stylized framework of the demand for representation

We develop a stylized framework to characterise women's demand for representation. In that framework, under-represented groups demand representation if the benefits exceed the costs. For individual $i$, we formulate this in the following way:

$$p_i \cdot B \geq e_i + s_i, \tag{2.5}$$

where the left-hand side describes the benefits of higher representation. $B$ are the benefits associated with representation and $B \geq 0$ as it cannot be negative to be politically represented. The actual benefits $B$ are weighted by the expected probability that women will be successful in implementing representation $p_i$. The right-hand of equation 2.5 shows the costs associated with demanding representation. These are comprised of an effort cost $e_i$, such as the organisational effort required in maintaining a suffragette club, and a social cost $s_i$ that arises from behaving differently to societal expectations. During WWI, $e_i$ increased considerably due to the truce of parties (cf. Section 3.2), leading to a downward trend in the demand for political representation.

Increases in agency can affect this individual cost-benefit consideration in several ways, increasing the demand for representation—in line with our empirical findings presented above. We distinguish here between increases in private agency—i.e. women have more autonomy over their own private affairs—and increases in public agency—i.e. women have more power in public decision-making processes, creating public visibility of women's agency. On the individual level, having more private agency may increase women's expected probability of success as they experience their own capabilities in other decision-making processes and thus increase their confidence in women's capabilities to participate politically. Likewise, an increase in public agency of women can equally increase the estimated success probability as women observe other women's capabilities. In addition, public agency decreases the social costs associated with demanding political representation. If women generally have more autonomy and are publicly visible in roles with decision-making power, this makes women advocating for more legal rights to make decisions less "weird". Increases in private agency, however, likely do not, at least in the short run, reduce social costs as they do not shift societal expectations. Thus, in this framework, agency is central for the demand for representation. Without agency,

under-represented groups likely have very low expectations on the success probability of their political representation, decreasing demand for representation. In addition, the social costs associated with demanding representation may be very high, preventing such a demand.

We derive two testable implications of this framework, highlighting how costs and benefits shape the relationship between agency and demand for representation. First, an increase in agency will have stronger effects on the demand for representation when the benefits are higher. To see this, take the net benefits $(p_i \cdot B - e_i - s_i)$ and derive this with respect to $p_i$, yielding $B$, i.e. an increase in the success probability induced by higher agency will increase the net benefits more the higher the actual benefits $B$. Note that potential decreases in the social costs $s_i$ associated with agency increases are the same regardless of the benefit level $B$.

Second, increases in private agency may not be enough to raise demand for representation if the social costs are prohibitively high. In these cases, reducing the social costs, for example through higher public agency, is a necessary prerequisite to increase women's demand for representation through agency. In the following, we provide suggestive empirical evidence supporting these implications.

### 2.6.2 Empirical evidence

**The role of benefits.** We start by investigating the role of benefits in shaping the relationship between agency and the demand for the franchise. We proxy increases in benefits by the increase in women's labour force participation during the war. In particular for poorer women, WWI resulted in economic hardship, causing them to enter the labour force. Poverty and starving children drastically increase the benefits $B$ associated with women's higher representation, as there is a higher necessity to enforce policies to alleviate this economic need. Thus, regions with a higher increase in female labour force participation were likely regions with higher increases in economic need of women and thus in benefits associated with the right to vote.

Figure 2.9 shows the employment share of women across 16 different sectors in 1913 and 1918. We see an increase in female labour force participation across all sectors, with a greater increase in previously male-dominated industries such as chemicals, metals, machinery, and mining, indicating that jobs were taken up because of economic need and not because of their general desirability and prestige.

While middle-class women who founded and maintained the suffragette clubs studied in this paper were unlikely to be directly affected by poverty and most likely did not

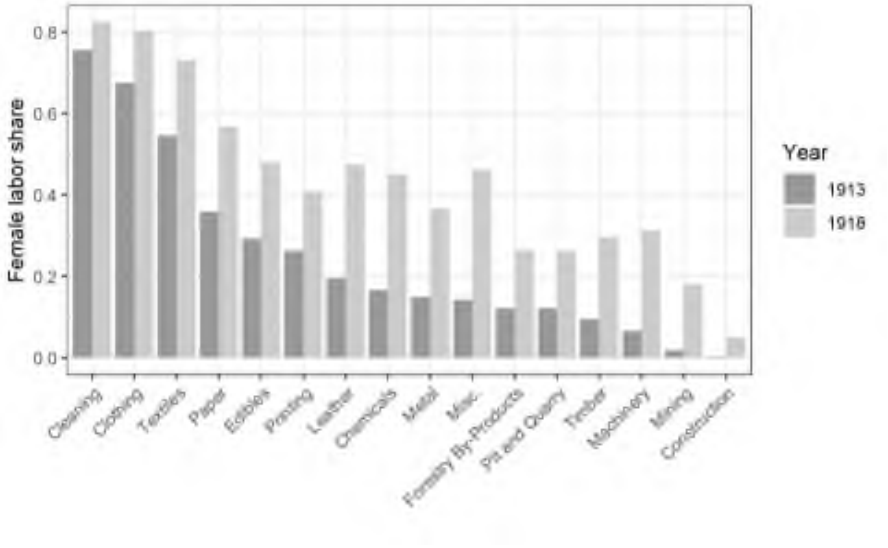



Figure 2.9: The female labour force participation across sectors before and during WWI
*Notes:* The figure shows the share of female workers employed in the total number of employed persons in firms with at least 10 employees in 16 different sectors in 1913 and 1918.
*Source:* Own illustration based on Reichsamt des Innern (1914[a]); Reichsamt des Innern (1919[a]).

enter the labour force as a result, they were, of course, aware of the deprivation of other women (Lüders, 1936b, p. 44). Thus, it is not implausible that middle-class women's decision to fight for the vote would be influenced by the overall increased benefits for other women associated with women's franchise.

To proxy the increase in economic need and thus benefits, we use data on employment by men and women before, during and after WWI from two different data sources. One data source allows us to make regional comparisons between 1913 and 1918 while the other provides more fine-grained regional information on the county level but only covers the years 1907 and 1925 (i.e. after the right to vote was granted).[27] The results are very similar using either data source.

Figure 2.10 presents suggestive evidence that the effect of the missing men on keeping a local suffragette club open is driven by women's economic hardship only to a small extent: We first replicate our baseline DiD results based on equation 2.2 for the sample of counties for which we have employment data confirming the positive effect. In the next step, we split the sample by whether a county had an above or below median change in

[27]As the employment data from 1913 and 1918 only varies across 40 German regions, we assign to each county the change in female labour force participation from 1913 to 1918 from the respective region.

female labour force participation, our proxy for economic hardship and thus the benefits associated with the franchise. From the framework in equation 2.5, we expect an increase in agency, going through an increase of the expected success probability $p$, to have a larger effect when benefits $B$ are higher. Indeed, we find that the positive effect of missing men on suffragette clubs appears somewhat larger in counties which also saw an above median increase in their female labour force participation. However, the effects in areas with high and low changes of female labour force participation are not very different in magnitude nor are they statistically distinguishable, indicating that economic needs are not the main driver of our results.[28]

**The role of social costs.** An alternative explanation for agency lifting demand-side constraints could be reduced costs of demanding political participation. Specifically, the experience of women of the NFD taking on organizational, administrative and leadership tasks in cooperation with local government and administrative bodies could have normalized women holding such positions, thereby alleviating the social costs of women fighting for political representation. Thus, the presence of a local NFD chapter might be indicative of an increase in agency that goes along with joint organization and that is publicly visible—what we call increase in public agency in the framework above—, potentially alleviating social costs which would have been prohibitive to women demanding the franchise otherwise.

To test this, we split the sample by the presence of a local NFD chapter. Specifically, we estimate equation 2.2 separately for counties with and without a local NFD chapter (304 counties vs. 744 counties). Figure 2.11 shows the results. For counties with a local NFD chapter, we estimate a positive and significant effect of $High\_Change_i \times post_t$ that is about three times as large as our baseline estimate. For counties without NFD presence, we find a precisely estimated zero effect, indicating that social costs are so large in these regions that increases in private agency cannot shift demand for political representation. Together, these results highlight the central role of societal costs in shaping women's demand for the franchise during WWI. They further suggest that the new range of tasks that middle-class women held during WWI increased women's public agency, contributing to their continued demand for political representation.

[28] Note that if sex ratio changes only measure economic need due to missing men, i.e. an increase in benefits, and not an increase in agency, we would expect the estimate on missing men to approach zero as we account for economic hardship. However, this is not what we see in the results, indicating that missing men really do proxy agency increases beyond economic hardship.

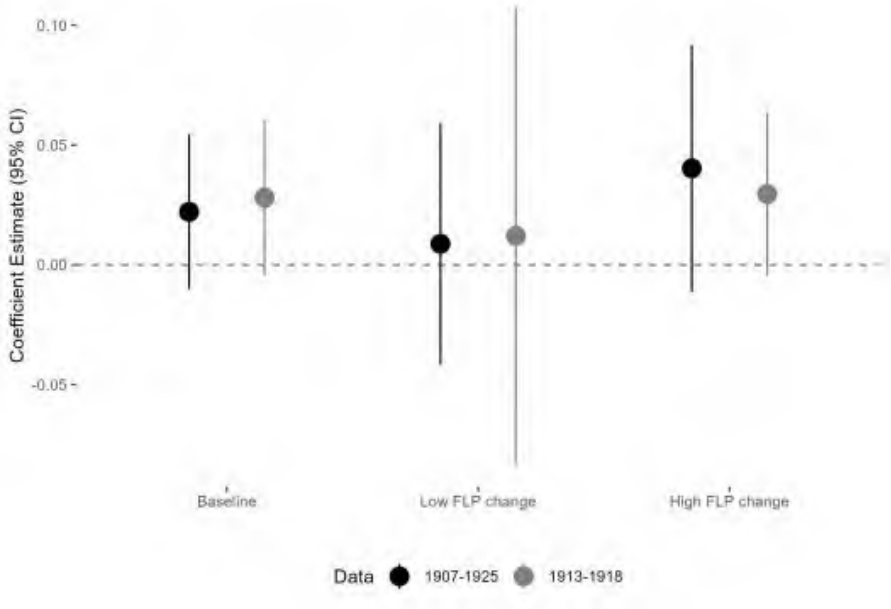



Figure 2.10: The effect of missing men suffragette clubs by female labour force participation
*Notes:* The figure shows the coefficient of comparing the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%), separately for counties with a high increase in female labour force participation (above median) and counties with a low increase in female labour force participation (below median), either between 1913 and 1918 or between 1907 and 1925. All regressions control for year and county fixed effects, the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level, and additionally at the regional level for regressions using the employment data from 1913 to 1918.
*Source:* Own illustration. See Table 2.1 for data sources.

## 2.7 Political participation after the suffrage extension

### 2.7.1 Female candidates

How does experiencing agency and demanding political participation translate to women's realized political participation after the war? To investigate this, we analyse candidates for the election to the National Assembly in 1919. This election was the first after the war and members of the National Assembly were elected with the goal of formulating a constitution for the new state and passing it as legislation. For the first time, women

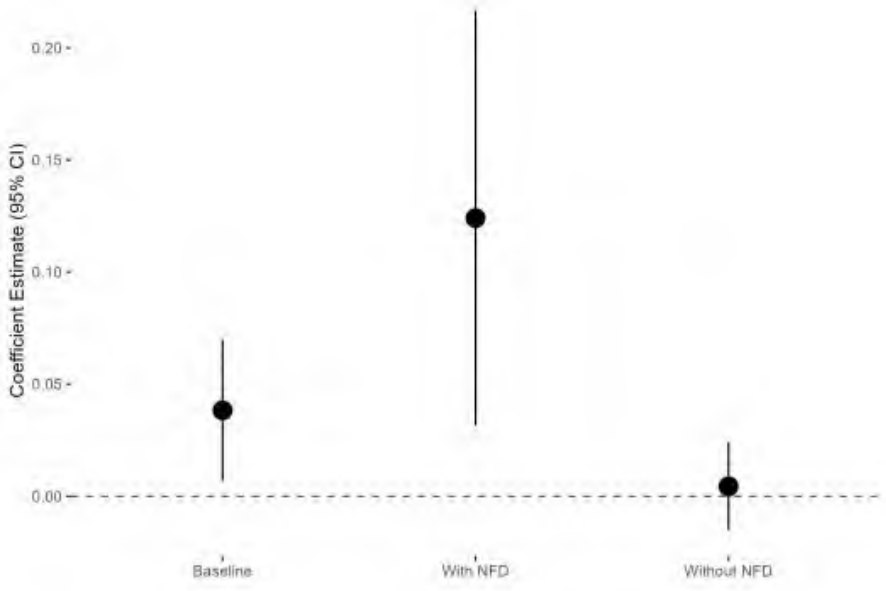



Figure 2.11: The effect of missing men suffragette clubs by National Women's Service participation
*Notes:* The figure shows the coefficient of comparing the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%), separately for counties with a local chapter of the NFD and without one. All regressions control for year and county fixed effects, the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level.
*Source:* Own illustration. See Table 2.1 for data sources.

were allowed to participate, both as voters as well as as candidates. We investigate whether more missing men during the war translate into a larger representation of women among candidates and elected members of the National Assembly, by re-estimating the cross-sectional setup of equation 2.4 at the ballot (election list) level, with a dummy for whether a women was on the ballot and whether a women from the ballot was elected to parliament.[29]

[29]As the data for political participation is only available for after the war, we are only able to estimate the cross-sectional setup. In all regressions, we control for our baseline controls which are industry and age structure, pre-war levels of the sex ratio, and pre-war presence of suffragette clubs. To avoid power issues, we use the continuous variable of the female-to-male ratio instead of the binary definition.

Table 2.8: Missing men and female candidates 1919

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Candidate lists | | | Elected candidates | | |
| | Any woman | No post-war club | Post-war club | Any woman | No post-war club | Post-war club |
| Change in female-to-male ratio | 0.889** | -3.005*** | 1.158** | -1.091** | -5.182*** | -0.781 |
| | (0.362) | (0.163) | (0.460) | (0.522) | (0.183) | (0.602) |
| Liberal party | 0.169*** | 0.484*** | 0.150*** | -0.043 | 0.079 | -0.043 |
| | (0.051) | (0.141) | (0.052) | (0.050) | (0.091) | (0.057) |
| Social-democratic party | 0.016 | 0.164*** | 0.006 | 0.108** | -0.141 | 0.126** |
| | (0.037) | (0.048) | (0.039) | (0.050) | (0.141) | (0.055) |
| Industry structure | Yes | Yes | Yes | Yes | Yes | Yes |
| Age structure | Yes | Yes | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.228 | 0.417 | 0.196 | 0.112 | 0.729 | 0.092 |
| N | 200 | 16 | 184 | 208 | 16 | 192 |

*Notes:* The table presents estimates from equation 2.4 for candidates in the 1919 election for the National Assembly. The estimation is performed on the ballot (election list) level. Columns (1) to (3) show the effect of changes in the female-to-male ratio on whether any woman is among the candidates, for all constituency-list observations and separately for constituencies without and with a suffragette club during WWI (columns (2) and (3)). Columns (4) to (6) refer to whether a women from the list was elected. Social-democratic party is one if the ballot is from either the Independent Social Democrats (*Unabhängige Sozialdemokratische Partei Deutschlands* or the Social Democrats (*Sozialdemokratische Partei Deutschlands*), Liberal party is one if the ballot is from either the German People's Party (*Deutsche Volkspartei*) or the German Democratic Party (*Deutsche Demokratische Partei*), and Conservative party (the ommitted category) is one if the ballot is from the German National People's Party (*Deutschnationale Volkspartei*) or the Centre Party (*Zentrum*). All columns include controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, and state fixed effects. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

The results are displayed in Table 2.8. In all columns, we control for the political orientation of the party in addition to our baseline controls for drafting probability. Compared to conservatives which are the excluded base category, liberal parties have a 16.8 percentage point higher probability of having a woman in an election list. Social-democrats, on the other hand, do not have more women on their election lists.[30] In column (1), we find that a one-unit increase in the sex ratio is associated with a 87.1 percentage point higher probability of having women on the election list. When the estimate is scaled by the mean change in the sex ratio (0.227), this corresponds to an increase in the probability of having of women on the election list by 19.8 percentage points. When we split the sample by whether a constituency managed to keep a suffragette

[30]The Independent Social Democrats (*Unabhängige Sozialdemokratische Partei Deutschlands* and the Social Democrats (*Sozialdemokratische Partei Deutschlands*) are coded as Social-democrats; the German People's Party (*Deutsche Volkspartei*) and the German Democratic Party (*Deutsche Demokratische Partei*) are coded as liberal, and the German National People's Party (*Deutschnationale Volkspartei*) and the Centre Party (*Zentrum*) are coded as conservative.

club open during the war, we find that the positive effect is driven by constituencies with continued demand for political representation.

The result on elected candidates is negative, and is driven by constituencies without continued demand for political representation. Note though that the sample split yields very unequal sample sizes, with only 16 observations for the case of no suffragette club during the war, so these results should be understood as suggestive evidence.

### 2.7.2 Voting

In a next step, we look at the effect of missing men on women's active voting behaviour. We use data on elections from 1919 to 1930 for which voting results were counted separately by gender (53 locations). We estimate the cross-sectional setup of equation 2.4 at the election level, using the female vote share as the outcome variable. All regressions include election fixed effects and our baseline controls for drafting probability. Additionally, we control for the male vote share to keep general levels of political participation and voting behaviour fixed.[31] As the voting data is only available for a small subset of Germany, these results should be interpreted as suggestive evidence.

Table 2.9: Missing men and voting 1919–1930

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Turnout | Turnout: No post-war club | Turnout: Post-war club | Left vote share | Liberal vote share | Conservative vote share |
| Change in female-to-male ratio | 3.866 | 2.074 | 13.546* | 7.361*** | 2.187*** | -5.565** |
| | (3.701) | (4.602) | (7.959) | (1.962) | (0.791) | (2.041) |
| Male voting | 1.195*** | 1.322*** | 1.049*** | 1.270*** | 1.250*** | 1.422*** |
| | (0.069) | (0.071) | (0.110) | (0.074) | (0.059) | (0.019) |
| Industry structure | Yes | Yes | Yes | Yes | Yes | Yes |
| Age structure | Yes | Yes | Yes | Yes | Yes | Yes |
| Election FE | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.966 | 0.972 | 0.972 | 0.981 | 0.986 | 0.996 |
| Num. obs. | 133 | 66 | 67 | 69 | 69 | 69 |

*Notes:* The table presents estimates from equation 2.4 for the female vote share in elections from 1919 to 1930. The estimation is performed on the election level. Columns (1) to (3) show the effect of changes in the female-to-male ratio on female turnout, for all counties and separately for constituencies without and with a suffragette club during WWI (columns (2) and (3)). Columns (4) to (6) show the effect on the female vote share by party orientation. Left parties are the Social Democrats (*Sozialdemokratische Partei Deutschlands*) and the Communist Party of Germany (*Kommunistische Partei Deutschlands*); liberal parties are the German People's Party (*Deutsche Volkspartei*) and the German Democratic Party (*Deutsche Demokratische Partei*); and conservative parties are the German National People's Party (*Deutschnationale Volkspartei*) and the Centre Party (*Zentrum*). All columns include controls for the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, and election fixed effects. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

[31] To avoid power issues in this small sample, we use the continuous variable of the female-to-male ratio instead of the binary treatment definition.

Table 2.9 shows that overall, the change in the female-to-male ratio did not affect female turnout. However, when splitting the sample by whether the county managed to keep a suffragette club open during the war, it seems that in locations with continued demand for political representation, the experience of agency translated into higher female turnout. In column (3), we find that a one unit increase of the sex ratio increased female turnout by 13.2 percentage points, with the effect being marginally significant.

When investigating the female vote share by party orientation, we find that missing men shifted women to vote in higher rates for left, and, to a smaller extent, for liberal parties, at the expense of conservative parties.[32] This indicates that experiencing more agency may have shifted political preferences of women, closing the gender differences in voting as women on average voted more conservative than men.[33]

## 2.8 Conclusion

In this paper, we analyse whether an increase in agency can alleviate constraints on the demand for political representation. We study the setting of the suffragette movement in Germany during WWI and exploit quasi-random variation in drafting intensity, which led to variation in women's agency by changing the local female-to-male ratio. To make demand for political representation measurable, we create a newly digitized dataset on the universe of local suffragette clubs. These clubs reflect revealed preferences of political demand, as maintaining a club was a costly endeavour, especially during wartime.

We show that in counties with more men drafted to the front, women were significantly more likely to keep their suffragette club open during the war, indicating that agency can shape the demand for political representation. Our findings suggest that this effect is primarily driven by regions in which women publicly lead the war relief efforts, decreasing the social costs associated with fighting for the vote. Moreover, we find that the experience of agency and the continued demand for political representation translated into higher female political engagement after the war, when the franchise was extended: Women were more likely to run for parliament, and more likely to participate in elections by voting.

Our paper shifts the focus from reasons for elites to extend the franchise to the role of demand for political participation by disenfranchised groups. By showing that

[32]Left parties are the Social Democrats (*Sozialdemokratische Partei Deutschlands*) and the Communist Party of Germany (*Kommunistische Partei Deutschlands*); liberal parties are the German People's Party (*Deutsche Volkspartei*) and the German Democratic Party (*Deutsche Demokratische Partei*); and conservative parties are the German National People's Party (*Deutschnationale Volkspartei*) and the Centre Party (*Zentrum*).

[33]Figure B.1.6 in the appendix shows that this pattern is also true for the elections in our sample.

agency is an important factor in shaping women's demand for participation, we contribute to the literature highlighting demand-side constraints as a reason for women's under-representation. Our results imply that experiencing agency, and especially women in leadership positions, can induce women to fight for representation, highlighting the importance of female role models and early leadership opportunities as a means for increasing women's participation in decision-making roles.

# Chapter 3

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


## Abstract

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

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## 3.1 Introduction

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

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

During WWII, the German government conscripted civilians from occupied countries to mitigate the rising labor shortage caused by the mass conscription of men for military service and the expansion of the armaments industry (Spoerer, 2001). In the Netherlands, in May 1943 the German occupational regime decided to conscript all men born in 1922, 1923, and 1924 (aged 18–21 at the time) for labor in Germany. The coercion was enforced by withholding food ration cards and by prohibiting businesses from employing men

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

[2] Forced labor is defined as any work or service that is being extracted from a person under a threat of penalty, and for which the person has not offered themselves voluntarily (ILO, 2022).

born in these years. At least 37% of the men in these cohorts were taken to Germany for forced labor. Anyone who was not granted an exemption was forced to go into hiding.[3] In Germany, forced workers were assigned into sectors irrespective of their previous skills, only based on local labor shortages at the time of their deportation. They worked alongside Germans and were often housed in larger barracks. The living situation in Germany varied widely. Although most Dutch forced workers were permitted to move about within the local community, they faced harsh punishment in case of disobedience, and experienced worsening access to food and healthcare as Allied bombings intensified. Around 96% of the Dutch workers survived the forced labor experience and returned to the Netherlands after the end of the war (Tooze, 2006).

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

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

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

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

[5]The effect corresponds to 2.1% of one standard deviation of yearly income.

are around 1.430 men missing from the workforce, and the income loss for the Dutch economy in 1971 is around 40 million EUR (measured in 2024).[6]

To be able to differentiate between the two types of displacement, either being forced to work in Germany or being forced to go into hiding, I am the first to link archival data on forced workers in Germany to the Dutch census data. The data comes from the Arolsen Archives and includes hand-transcribed records on the workers' name, place of birth, date of birth, and their location in Germany. I cleaned the places of birth and merged them with the Dutch municipalities in the census data. I classified an individual's gender using their first name and name frequencies from Meertens Instituut (2025). I link the data by the municipality of birth, restricting the archival records to Dutch male forced workers born in the conscripted cohorts. For each Dutch municipality, I construct a measure of the share of conscripted individuals who went to Germany to identify municipalities with more intensive forced labor (and thus less hiding). I then allow for heterogeneous effects based on this share and find that the effects are similar, implying that the treatment effect is driven by the disruption of the labor market career, independent of whether that constituted being forced to work or being forced to go into hiding.

Exploiting information from the archival records on forced workers' locations in Germany, I show that the negative consequences on later labor market success are driven by individuals who had higher exposure to adverse living conditions while in Germany. I proxy the exposure to adverse living conditions in Germany by the share of houses damaged due to Allied bombings and the distance to so-called labor re-education camps which served as punishment for forced workers. I then assign a measure of average exposure to adverse conditions to each Dutch municipality, based on the German locations where forced workers from each municipality were sent.[7] The results show that the lower probability of being employed is driven solely by conscripted individuals who were exposed to more adverse conditions in Germany, suggesting that the traumatic experiences have a lasting negative and significant effect on labor market participation. To test whether this is due to to adverse health effects, I then directly study the consequences of the treatment on physical and mental health. Indeed, the probability of needing assistance in daily life in 1971 was higher for conscripted individuals from places with higher exposure to adverse conditions compared to non-conscripted individuals. I also find suggestive evidence for lower subjective life satisfaction for individuals from the conscripted cohort using Eurobarometer survey data covering the period from 1975 to 1994.

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

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

I also find that the loss of relevant labor market experience due to forced labor is a relevant factor in explaining the negative consequences of forced labor conscription: I show that the negative effect on labor force participation is mitigated for individuals who were forced to work in sectors similar to the ones in a person's place of origin. Specifically, I allow for heterogeneous effects based on the similarity in the sectoral composition of the Dutch municipality of birth compared to the average sectoral composition of German counties where people from that municipality were located while in Germany and find no negative effects for individuals presumably forced to work in similar sectors. This finding suggests that individuals who after the war could continue working in the sectors that they were forced to work in while in Germany had a lower loss of relevant labor market experience.

The results are robust to several checks, including different specifications of the RDD equation, the use of various bandwidths, and different sample restrictions.[8] Additionally, I estimate placebo effects with the cutoff at different years, showing that there are no significant differences in labor market success at these cutoffs.

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

This paper also contributes to the literature on forced migration by studying a setting where the displacement was only temporary and the majority of the forced migrants returned to their home country. This mitigates concerns about selection of return migration, and gives insights into the effects of temporary displacement by excluding that effects are driven by the ongoing exposure to conditions in the destination location (Bauer, Braun, and Kvasnicka, 2013; Bauer, Giesecke, and Janisch, 2019; Becker, Grosfeld, et al., 2020; Becker, 2022; Sarvimäki, Uusitalo, and Jäntti, 2022). While Arellano-Bover

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

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

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

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

By showing that missing out on relevant labor market experience in young age has lasting effects on labor market success, my study also contributes to the literature on the consequences of graduating in a recession (Grenet et al., 2024; Oreopoulos, Wachter, and Heisz, 2012; Schwandt and Wachter, 2020; Wachter, 2020). I find that if a person is employed in sectors that they did not freely choose themselves and that are not relevant for their later career, they face long-term reductions in their later productivity and labor market outcomes.

By studying the consequences of forced labor on an individual level, my paper also directly contributes to the literature on forced labor. Previous studies have compared regions with more or less intensive use of labor coercion and show persistent negative effects (Bertocchi and Dimico, 2014; Buggle and Nafziger, 2021; Buonanno and Vargas, 2019; Cinnirella and Hornung, 2016; Dell, 2010; Fujiwaray, Laudaresz, and Caicedo, 2017; Mitchener and McLean, 2003; Nunn, 2008; Soares, Assunção, and Goulart, 2012; Markevich and Zhuravskaya, 2018). In these settings, it is impossible to distinguish whether the negative effects persist due to forced labor systems shaping local institutions or due to forced labor experience altering individuals' labor market trajectories. In the case of Dutch civilians being coerced into labor in Germany, both the treatment and control groups lived under the same institutions in the Netherlands after the end of WWII and only differed in their exposure to forced labor conscription. I can therefore

separate the effects of forced labor conscription on individuals from the effect of forced labor systems on local institutions.

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

## 3.2 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 from employing men from these cohorts.

In total, at least 77,200 Dutch forced workers born between 1922 and 1924 were recorded in Germany during WWII in the data by the Arolsen Archives. Comparing that to a total of around 210,500 men born in these years according to the 1971 census, compliance was at least around 37%. Men already working in war-related industries were granted exemptions, which applied to 16% for those born in 1922, 19% for those born in 1923, and 17% for those born in 1924 (Hauptabteilung Soziale Verwaltung, 1943; Sijes,

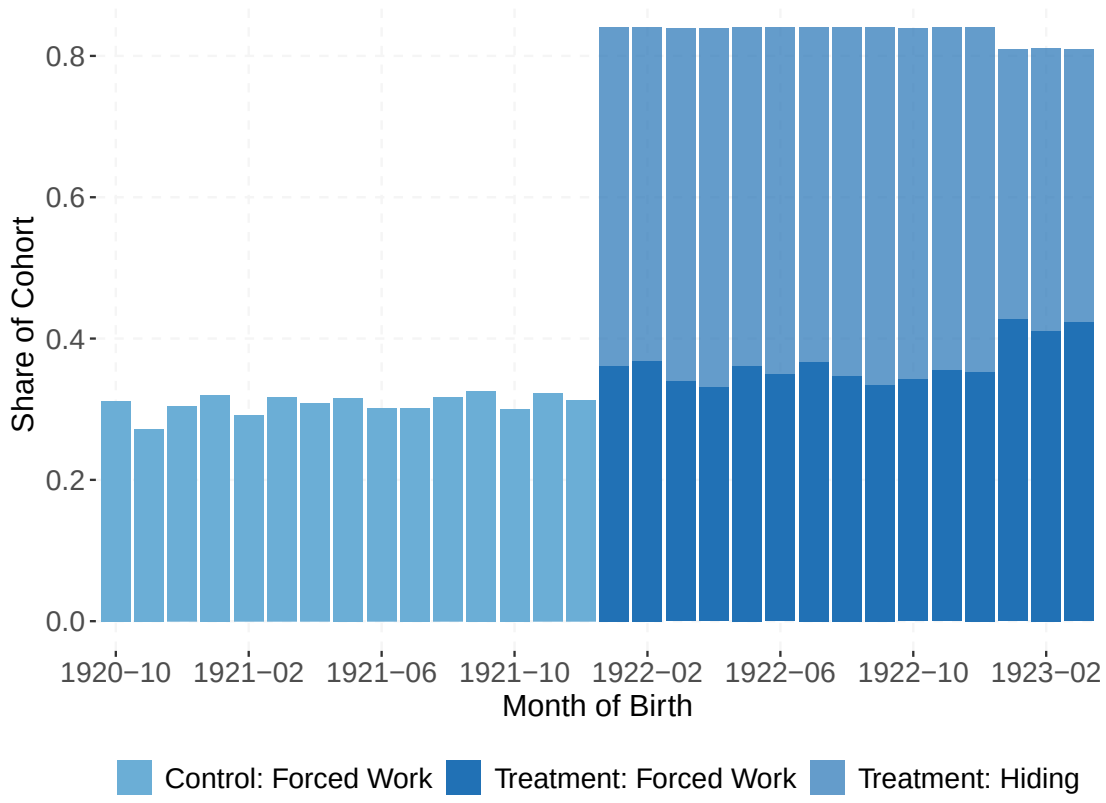



Figure 3.1: Number of treated individuals

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

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 3.1 shows the exogenous variation that I use in this study: Men from the conscripted cohort were more likely to be forced to work in Germany than men born before the cutoff, and only men from the conscripted cohort were forced to systematically go into hiding until the end of the war to avoid forced labor. The share of forced workers is derived by comparing the

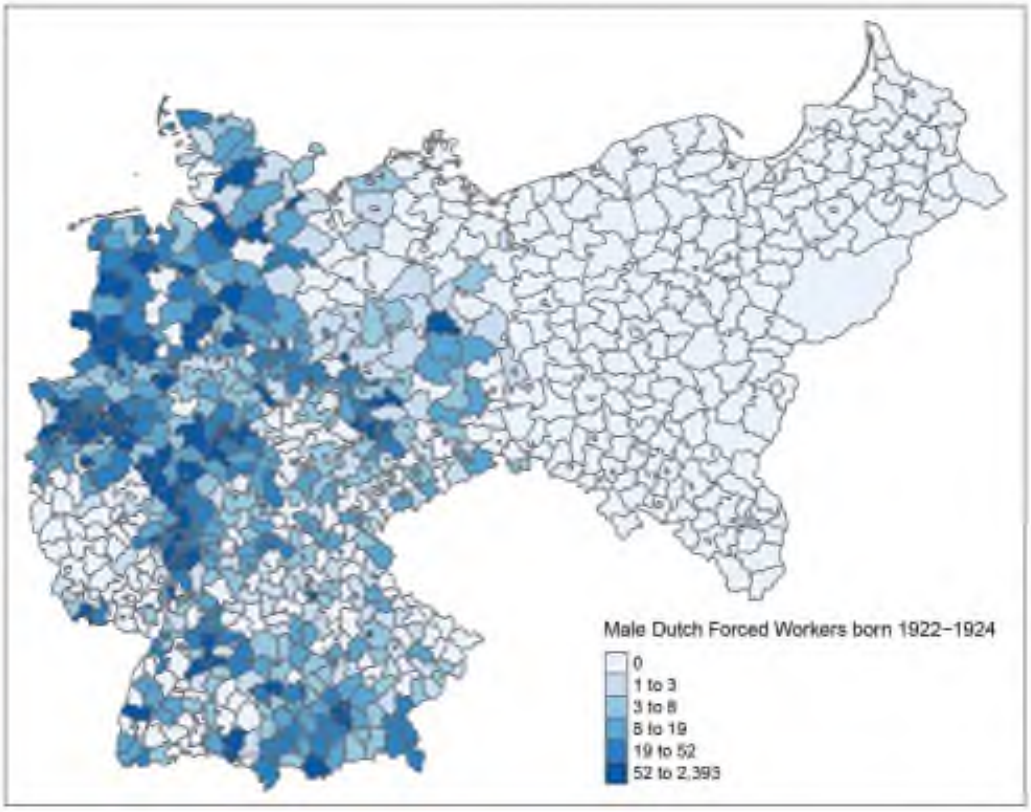



Figure 3.2: Regional distribution of male Dutch forced workers born 1922-1924 across German counties

*Notes.* This figure shows the number of male Dutch forced workers born between 1922 and 1924 based on data from the Arolsen Archives, excluding double-entries of identical individuals. Gender was assigned using first names and data from Meertens Instituut (2025).

sample size of men in the Arolsen archival records to the sample size of men in the census data from 1971. The share of men who went into hiding is derived by subtracting the share of men who were granted an exemption, which is taken from German records from 1943 and available separately for each cohort. This approach results in similar shares of forced workers and men in hiding in the conscripted cohorts, which is supported by historical sources (Klemann, 2001; Warmbrunn, 1972).[9]

[9]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).

The conscripted individuals who were taken to Germany were distributed irrespective of their skills or previous training, and over large parts of Germany. Figure 3.2 shows the regional distribution of Dutch male forced workers born in the conscripted years 1922–1924 over the German counties. The men were assigned quasi-randomly to the specific locations and industries, based on local labor shortages at the time of their deportation that companies reported to their local *Arbeitsamt* (employment office). The administrative effort of recording previous skills and training and assigning workers based on that was deemed too costly (Kuck, 2010; Marx, 2019). The majority of the Dutch forced workers were employed in manufacturing and construction, and the pay was lower than that for German workers (Herbert, 1999; Sijes, 1966; Tooze, 2006).

The living conditions varied widely, as firms were responsible for housing and feeding the forced workers (Althausen, 1999). Most Dutch workers were housed together in barrack camps or repurposed public buildings. Food supply and nutrition were often of low quality, access to medical care was scarce or non-existent, and both deteriorated as the bombing of the Allied forces intensified. Forced workers were also tasked with clean-up after bombing attacks (Sijes, 1966). In case of any so-called nondisciplinary conduct such as sabotage or absenteeism, forced workers were sentenced to stays in so-called *Arbeitserziehungslager* (labor re-education camps) for several weeks, where conditions were similar to those in concentration camps (Lofti, 2000). While the forced workers were mostly promised yearly contracts at deportation, the majority of workers were not allowed to leave after their contract ended and had to stay until the end of the war (Beening, 2003). When workers tried to flee to return to the Netherlands, they faced a sentence to labor education camps and then being 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).[10] After the successive liberation of Germany in 1945, the Allied Forces organized the transport of former forced workers back to their home countries. By September 1945, 98% of all Dutch persons present in Germany at the end of the war had returned to the Netherlands (Grüter and Mourik, 2020; Proudfoot, 1957). Figure 3.3 also shows that the number of men born before and after the cutoff of conscription is not different in 1971, confirming no significant differences in survival probability and no gap of conscripted men who did not return. On their

[10]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%.

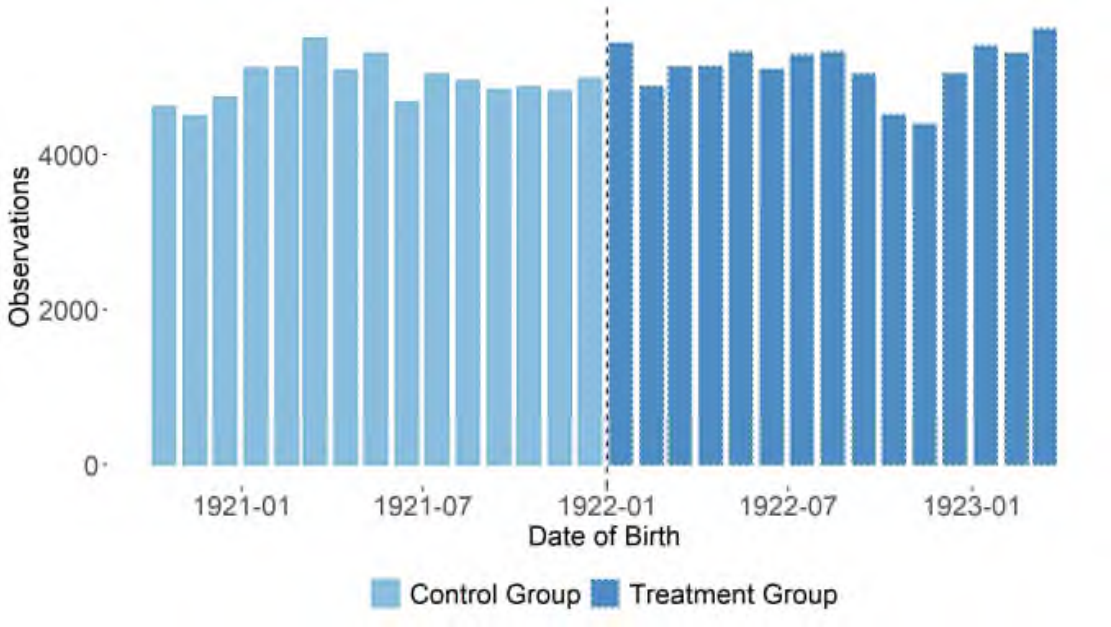



Figure 3.3: Number of male observations per month and year of birth based on the 1971 Census

*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.

return, the forced workers faced stigma because their labor for Germany was seen as collaboration with the enemy. Therefore, most of them stayed silent about what happened to them during the war. Only in the 1980s, a public debate about the experiences of the forced laborers began in the Netherlands (Kuck, 2010).[11]

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

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

May 1945 in urban regions in the West (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.[12]

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

## 3.3 Data

### 3.3.1 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.[13] To identify the treatment and control group, I use the individuals' gender, month of birth, year of birth, and country of birth. The treatment group is defined as all men born in the Netherlands in the conscription period, so within 1922, 1923, and 1924. The control group is individuals born in the Netherlands within the three years prior to the conscription, so 1921, 1920 and 1919. The individuals are thus between 46 and 52 years old at the time of 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).[14] The sample then includes 356,681 observations.

To measure labor market success, I use a dummy variable of whether a person is employed, and yearly labor income reported in 6 different income bins with a range of

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

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

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

Table 3.1: Descriptive Statistics

| Variable | Levels | Mean Overall | Std. Dev. Overall | Mean Treatment | Mean Control | Nonresp. Rate (T) | Nonresp. Rate (C) |
|---|---|---|---|---|---|---|---|
| Employment Status | Employed: Yes/No | 0.94 | 0.23 | 0.94 | 0.94 | 0.00 | 0.00 |
| Income Bin | 0-5 (4,000 Dutch guilder range) | 2.92 | 1.36 | 2.93 | 2.91 | 0.07 | 0.07 |
| Marital Status | Married: Yes/No | 0.91 | 0.29 | 0.91 | 0.91 | 0.00 | 0.00 |
| Parental Status | Has a Child: Yes/No | 0.86 | 0.35 | 0.86 | 0.86 | 0.00 | 0.00 |
| Educational Attainment | Secondary Education: Yes/No | 0.19 | 0.39 | 0.19 | 0.19 | 0.17 | 0.18 |
| Need for Assistance | Yes/No | 0.04 | 0.19 | 0.04 | 0.04 | 0.00 | 0.00 |
| Not living in place of birth | Yes/No | 0.59 | 0.49 | 0.58 | 0.59 | 0.00 | 0.00 |
| Observations | | 151080 | 151080 | 76918 | 74162 | 76918 | 74162 |

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

4,000 Dutch Guilder (2,916 EUR in 2024). I also use a dummy variable for finishing secondary education, as well as the marital status and a dummy for whether an individual has children as a proxy for their social situation.[15] Additionally, I use a person's need for assistance in everyday life as a proxy for their health. Table 3.1 shows the descriptive statistics of these variables.[16]

The 1971 census only includes the municipality of birth for individuals who still live in the same municipality (excluding temporary absences such as war-related reasons). This is the case for 56% of the sample. When linking other data sources based on the place of birth, I further restrict my analysis to these individuals who still live in their municipality of birth. This reduces the sample to 145,286 observations. Note that still living in the place of birth is not affected by the forced labor conscription (see figure C.3.2).

### 3.3.2 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.[17] The number of unique Dutch individuals in the archival data of around 473,000 also matches the historical estimates of Dutch

[15]The non-response rates are around 17% for educational attainment and 7% for income, but these numbers are similar for treatment and control group, see table 3.1.

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

[17]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).

forced workers of somewhere between 450,000 and 530,000 (CBS, 1947; Spoerer, 2001). I therefore will assume that all individuals in this dataset are forced workers.

The data includes information on the full name, date of birth, location of birth, and the county where the person was located while in Germany. The original sample consists of 594,967 observations. Some individuals show up more than once in the data because multiple sources have been aggregated for the archival records. I therefore use a fuzzy linkage method to link duplicate entries of the same person to one another and exclude the double-counting of individuals. I follow Abramitzky, Boustan, et al. (2021) and adjust their algorithm slightly to exploit the data structure of the archival records.[18] This reduces my sample to 473,406 individuals.

To restrict the archival records to male individuals, I exploit the information on a person's first name to classify their gender. I use information on name frequency by gender from the Corpus of First Names in the Netherlands published by the Meertens Institut to construct a measure of how likely a name belongs to either gender (Meertens Instituut, 2025).[19] For the relevant cohorts of 1922–1924, there are 84.2% male and 10.7% female individuals.[20] 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.[21] This information is available for 40.1% of the sample. I use a fuzzy merge and complement it with a list of over 3,500 hand-coded places of birth.[22] 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 using the following equation:

$$sharefw_m = \frac{forcedworkers_m^{DE}}{conscriptedmen_m^{NL}} \tag{3.1}$$

which compares the sample size of linked individuals from the archival data to the sample size of conscripted individuals in the census for municipality $m$. Figure 3.12 shows the regional distribution of the share of conscripted workers who were deported to

[18] See section C.1.1 for a detailed description of my approach.
[19] See section C.1.1 for a detailed description of my approach.
[20] For the remaining 5.1%, the first name was not unambigiuously male or female.
[21] Linking on an individual level is not possible, since the 1971 Census does not include information on the name and exact date of birth.
[22] See section C.1.1 for a detailed description of my method.

Germany. As I was only able to link a subset of the individuals from the archival records, the share constitutes a lower bound.[23]

### 3.3.3 Eurobarometer

To investigate the consequences on mental health, I use Eurobarometer survey data which includes a question on self-reported life satisfaction. The Eurobarometer is a survey conducted in all member countries of the European Union and samples 1,000 random individuals per country in every survey round. I use all Eurobarometer survey waves since 1975, when age was first recorded, until 1994, when the youngest individuals in the potential treatment group would be 70 years old (Kommission Der Europäischen Gemeinschaften, 2012).[24] Since I only know an individual's age and not their exact date of birth, I restrict the analysis to individuals for whom I know for certain that they are in the control or treatment cohort.[25] The variable for mental health is the subjective life satisfaction (ranging from 0 to 3).[26] I also repeat the analysis on labor market success to ensure the validity of the Eurobarometer survey data, using a dummy for whether a person is employed, and their labor income (reported in 12 different income bins with a range of 250 Dutch Guilder (182 EUR in 2024). Table 3.2 shows the descriptive statistics of the variables of interest.

Table 3.2: Descriptive Statistics: Eurobarometer

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

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

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

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

[25] 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.

[26] 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?"

### 3.3.4 Forced labor experience

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

#### 3.3.4.1 Exposure to adverse conditions

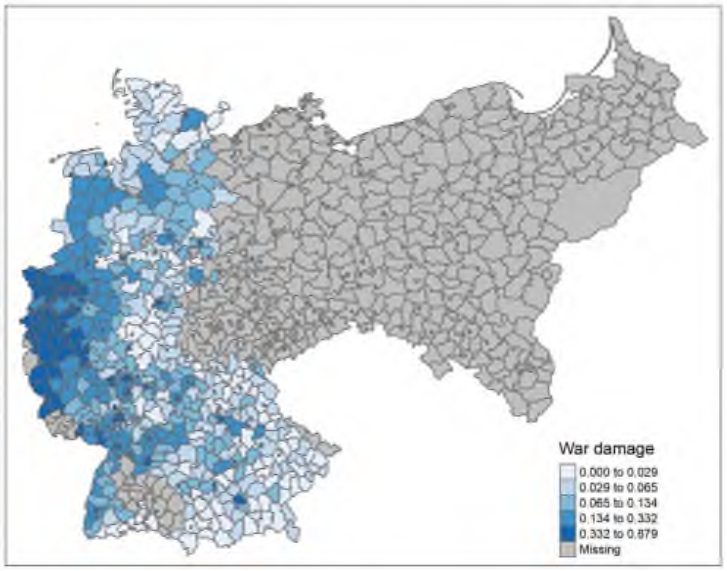
I proxy adverse conditions in Germany by two measures: First, I use exposure to Allied bombings. The intuition is that forced workers suffered most in areas with lots of bombings, as shelters were often reserved for German citizens. Additionally, forced workers were often forced to clean up after the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

The data on war exposure comes from Peters (2022) and measures the share of houses damaged during the war by Allied bombings. This data is available for West Germany, where also the majority of Dutch forced workers were located (see figure 3.2). Figure 3.4a 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 3.4b shows the locations of these labor education camps.

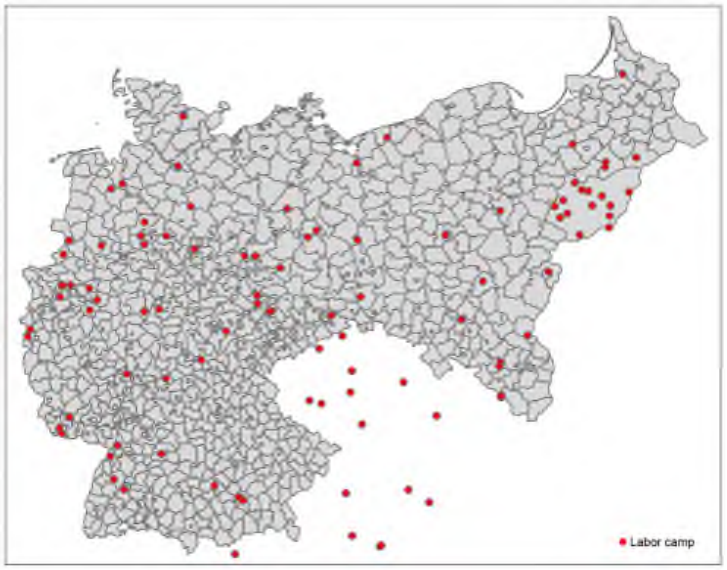
#### 3.3.4.2 Loss in labor market experience

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

I thus first need information on the type of occupation that forced workers were coerced into while in Germany. I use data on the local industry structure in German counties in 1939 taken from Braun and Franke (2021) and use the employment share of each sector as a proxy for the probability with which forced workers located in these counties worked in the respective sector. The data distinguishes between 28 different



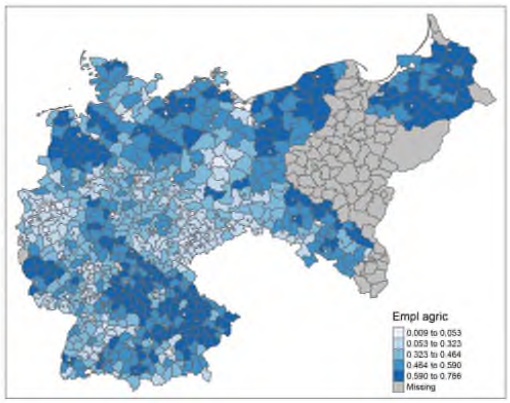

(a) Share of damaged houses across German counties



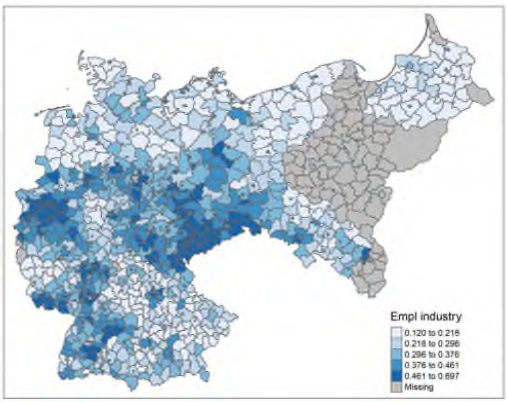

(b) Location of labor education camps

Figure 3.4: Regional distribution of adverse forced labor conditions over German counties

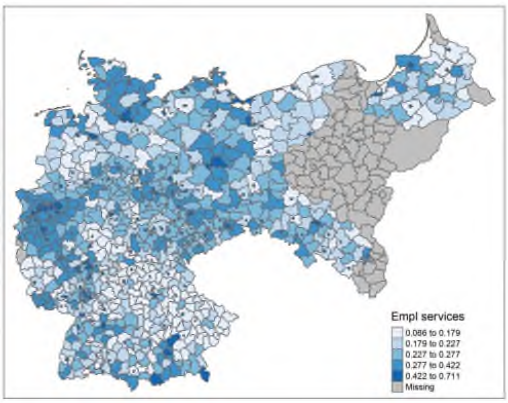
*Notes.* This figure shows the regional distribution of two measures for the adversity of forced labor in Germany: Panel (a) shows the share of housing stock that was damaged during the war in German counties. Each color represents a quantile of the distribution of the respective variable. The data comes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




(a) Employment share: Agriculture




(b) Employment share: Industry



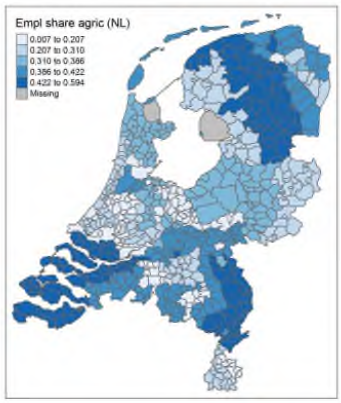

(c) Employment share: Services

Figure 3.5: Sectoral composition across German counties

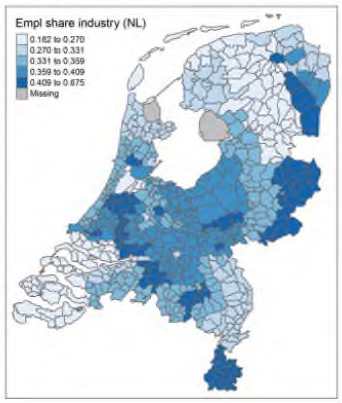
*Notes.* The figures show the employment share for agriculture, industry, and services in 1939 over German counties. Each color represents a quantile of the distribution of the respective variable. The data comes from Braun and Franke (2021).

sectors.[27] Figure 3.5 shows the distribution of the average employment share in agriculture, industry, and services over 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 forced workers were appointed to sectors according to the local employment shares. As

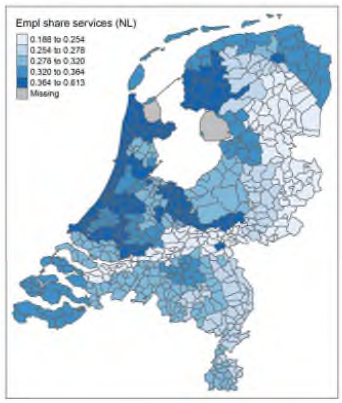
[27] I thank Sebastian Braun and Richard Franke for giving me access to data containing this more fine-grained sectoral variation.




(a) Employment share: Agriculture




(b) Employment share: Industry




(c) Employment share: Services

Figure 3.6: 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 that did not yet exist in 1930.

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 3.6 shows the regional distribution of the employment share in agriculture,

industry, and services.[28] 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.

## 3.4 Empirical strategy and results

### 3.4.1 Labor market outcomes

#### 3.4.1.1 Empirical strategy

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

I exploit the exogenous assignment into forced labor based on an individual's date of birth by using a fuzzy Regression Discontinuity Design (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).[29]

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

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

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, Idrobo, and Titiunik, 2019).[30] To the best of my knowledge, there were no other policies that changed discontinuously at the cutoff date of the conscription policy (January 1, 1922). The cutoff for school enrollment was mid-year (Richardson, 2000), and the limited conscription into the military during WWII was not based on age (Sijes, 1966).[31] The oldest cohort conscripted for the Indonesian War of Independence in 1946 was the one of 1925 (NIOD, NIMH, and KITLV, 2022).[32] Both the treatment and control group were subject to the war. The control group was more likely to experience the Dutch Hunger Winter in 1944–1945, which is why I exclude men from municipalities affected by the Hunger Winter in my baseline specification.[33]

The second identifying assumption of RDD is that individuals cannot manipulate the running variable and thereby induce endogenous sorting around the cutoff. A person's date of birth is generally exogenous, and even if some conscripted men forged their documents to avoid conscription by changing their date of birth, it is unlikely that the false date would still be reported in their administrative records in 1971. Figure 3.3 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

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

[31] 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).

[32] 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).

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

Germany it entails being forcibly moved to another country, then being forced to work in an occupation that they did not freely choose, being subject to harsh living conditions and punishments, and having to hide this traumatic experience due to the associated stigma. In the case of those who went into hiding, the treatment consists of having to leave their known environment without contact with their usual social environment, living in fear of being found, and often having no formal employment (Warmbrunn, 1972).[34]

Some individuals born within the conscripted years were granted an exemption and had to endure neither forced labor nor going into hiding[35], 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:

$$Y_i = \beta_0 + \beta_1 1\{MonthofBirth_i \geq c\} + \beta_2 MonthofBirth_i + \beta_3 MonthofBirth_i^2 + \epsilon_i \quad (3.2)$$

$Y_i$ are labor market outcomes, specifically employment status and income. The running variable is $MonthofBirth_i$ 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 $\beta_1$ is the intention-to-treat (ITT) effect, which is the effect of being subject to conscription into forced labor, irrespective of actual compliance, compared to individuals who were born outside the conscripted years and were thus less likely to face forced labor and did not have to go into hiding to avoid forced labor. The estimated effect is thus a lower bound of the true effect of being subject to forced labor conscription as the control group includes individuals also affected by forced labor, and the treatment group includes individuals who were able to avoid forced labor and went into hiding. I include a linear and a quadratic term of the running variable $MonthofBirth_i$ following Gelman and Imbens (2019).[36] I use a bandwidth of 15 months, which is the median of the optimal bandwidths from all labor market outcomes, based on the MSE-optimal bandwidth selection and a triangular kernel as suggested by Cattaneo, Idrobo, and Titiunik (2019).[37]

[34] See section 3.2 for a detailed discussion of both experiences.

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

[36] 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 3.8.

[37] I perform robustness checks using different bandwidths and a uniform kernel, see figure 3.8.

#### 3.4.1.2 Results

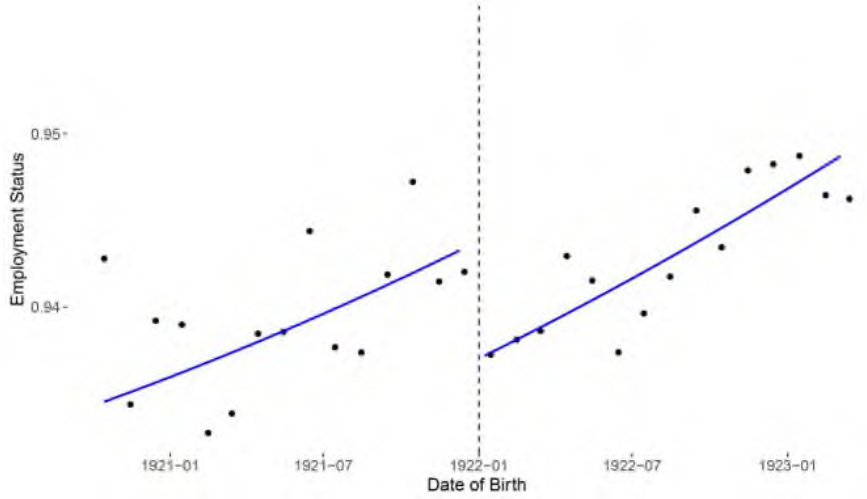
Figure 3.7 shows the average outcomes for each month of birth and the corresponding function estimated using equation (3.2), with a bandwidth of 15 months.[38] I find that the probability of being employed is lower by 0.69 percentage points, which is 2.9% of the standard deviation. The yearly income is lower by 0.0289 levels, which corresponds to 2.1% of one standard deviation. Compared to the average income in 1971, this is a reduction of 1% of the mean. Both effects are significant at the 5% level. So individuals subject to the labor conscription policy are performing significantly worse on the labor market in terms of their probability of being employed and their income. These effects also translate to significant productivity losses for the Dutch economy on a whole: The cohorts of conscripted men born between 1922 and 1924 consist of 210.565 men. When aggregating the individual losses, there are around 1.430 men missing from the workforce, and the total income loss for the Dutch economy in 1971 is around 40 million EUR (measured in 2024).[39]

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

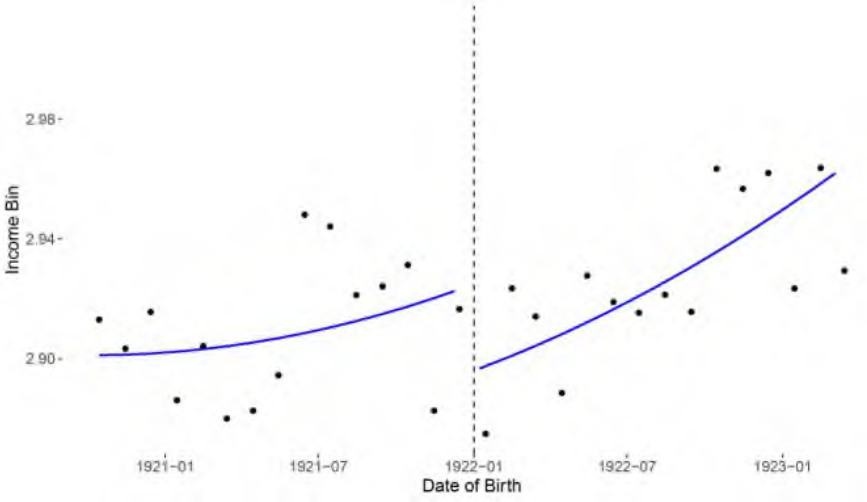
Compared to other types of disruptions such as natural disasters or graduating in a recession, my estimates are within a similar range: For instance, studies on natural disasters have found employment losses between 0.5 and 4.2 percentage points (Barattieri

[38]The underlying regression results are shown in table C.4.1.

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




(a) Employment status
Estimate: $\beta_1$ = -0.0068 (0.0021)***




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

Figure 3.7: RDD effects of forced labor conscription on labor market outcomes based on 1971 Census

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

et al., 2023; Deryugina, Kawano, and Levitt, 2018). Schwandt and Wachter (2020) find that earnings are lower by 1% when graduating in a labor market with a 1 percentage point higher unemployment rate, which would correspond to the effect of labor conscription.

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

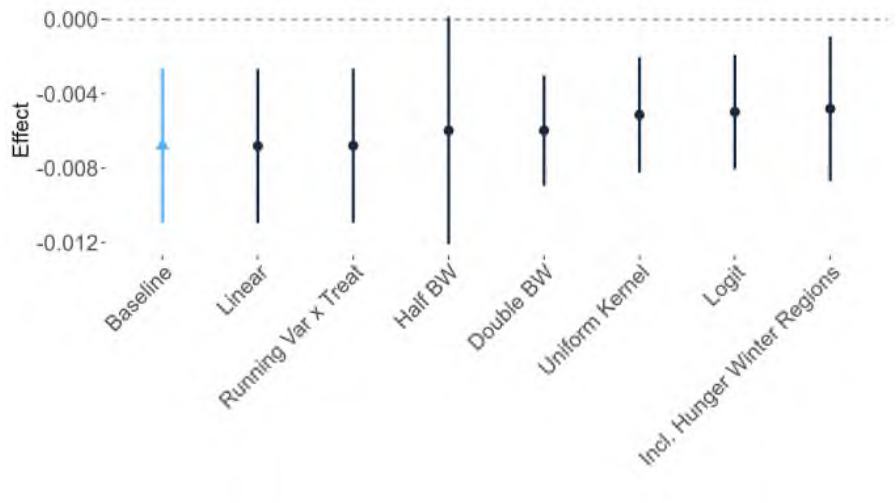
The results are robust to a number of different specifications. Figure 3.8 shows the estimates for regressions with different specifications of the RDD equation: Using only a linear term of the running variable $MonthofBirth_i$, including an interaction term of the running variable and the treatment indicator $1\{MonthofBirth_i \geq c\}$ to allow for different slopes on both sides of the cutoff, using different bandwidths (half and two times of the optimal bandwidth), using a uniform kernel, running nonlinear regressions[40], and including individuals from the Hunger Winter regions. Throughout all regressions, the estimates remain negative, the majority also retaining significance.

To provide further evidence for the main identifying assumption that labor market success would have been smooth at the cutoff in the absence of treatment, I run a placebo exercise where I shift the cutoff of January 1st to different years. In these years, I should not find any significant differences as nothing should have changed discontinuously at the cutoff that affects labor market success. Figure 3.9 shows the results of this placebo analysis.[41] I find insignificant results with estimates close to zero for all placebo specifications.

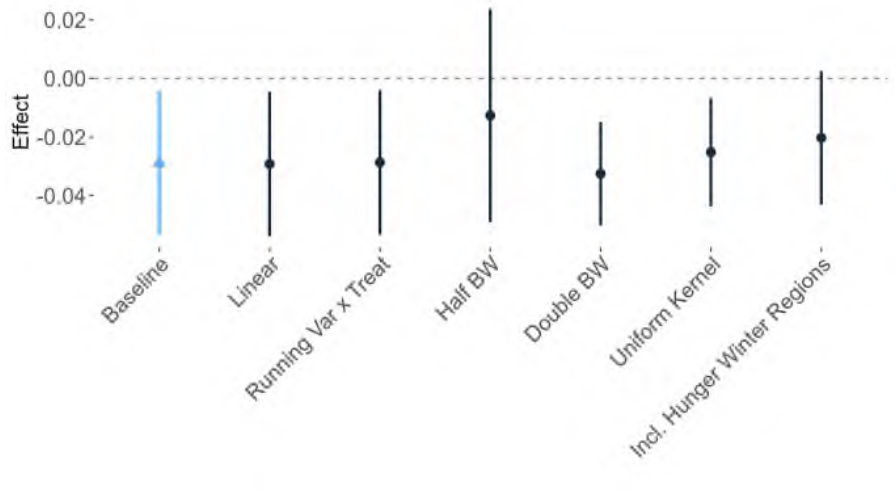
One concern for identification is that because men were missing from the workforce in the 1940s due to the conscription, the remaining men of the control group may have benefited, which could also explain the negative effects that I find. Note however that by the end of 1940, there was almost no unemployment in the Netherlands (Klemann, 2001). Also in 1971, the unemployment rate was as low as 2% (Sonsbeek et al., 2023). This means that both at the time of the forced labor conscription (when men were missing from the workforce because of forced labor deportation and going into hiding), as well as at the time when I measure the outcome (when men were missing from the workforce

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

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



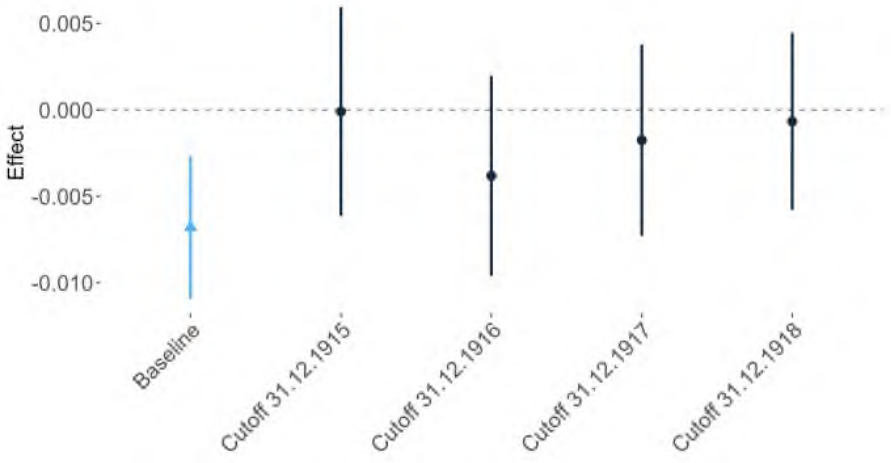

(a) Employment status



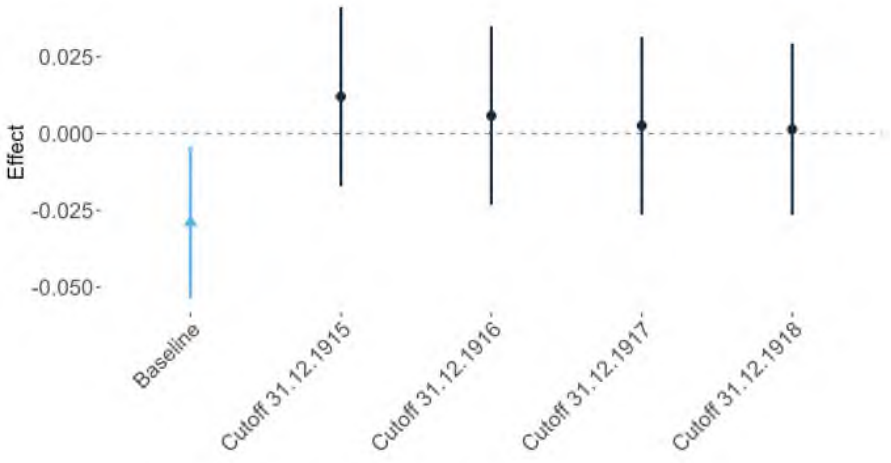

(b) Income bin

Figure 3.8: Robustness of RDD effects of forced labor conscription on labor market outcomes based on 1971 census

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




(a) Employment status




(b) Income bin

Figure 3.9: Placebo RDD effects of forced labor conscription on labor market outcomes based on 1971 Census

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

because of the negative treatment effect on employment), this absence of men did not open up large opportunities for an otherwise unemployed control group.

### 3.4.2 Further outcomes

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

**Education.** Figure 3.10 shows the average share of individuals who finished secondary education as reported in the 1971 census in a 15-month bandwidth around the cutoff, and the results from equation (3.2). I find that the probability of finishing secondary education is lower by 0.64 percentage points, which corresponds to 1.6% of one standard deviation. This effect is only significant at the 10% level. The effect is probably driven by treated individuals being less likely to go back to finish their secondary education after WWII, as the conscription started when treated individuals were around 19 years old and had thus already left school.

**Family formation.** Figure 3.11 shows the average share of individuals who are married and have children as reported in the 1971 census in a 15-month bandwidth around the cutoff, and the results from equation (3.2).[42] There is no discontinuous difference between the treatment and control group at the cutoff for both marital status and the probability of having children, meaning that in 1971, when treated individuals were aged around 49 years old, forced labor conscription does not seem to affect family formation.[43]

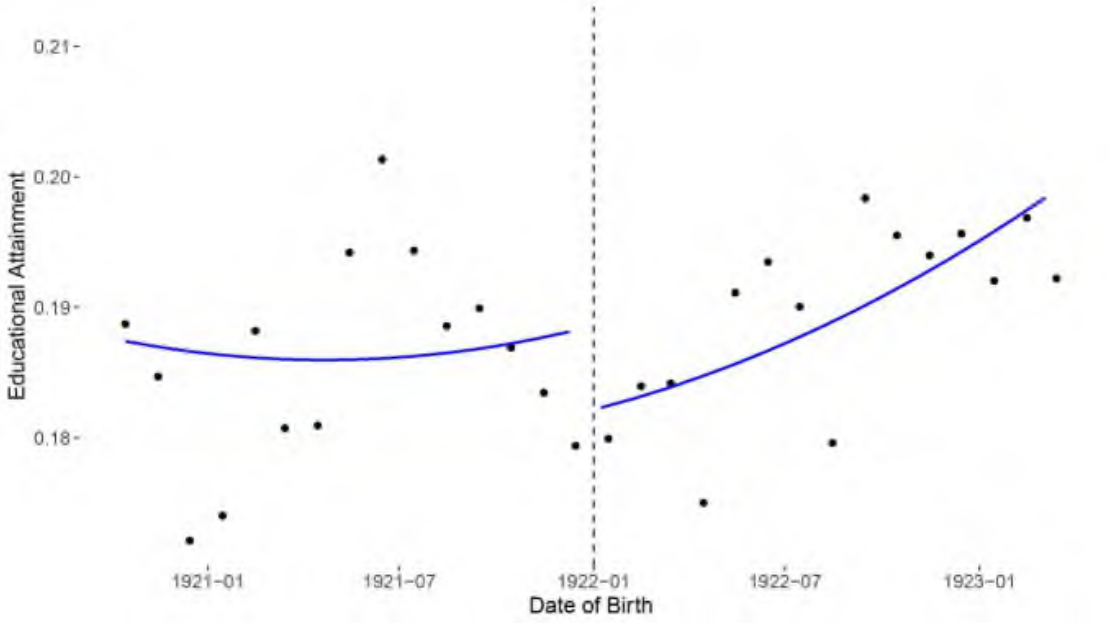
### 3.4.3 Contributing factors

#### 3.4.3.1 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

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

[43] Table C.4.7 shows the full regression results.




Estimate: $\beta_1$ = -0.0064 (0.0038)*

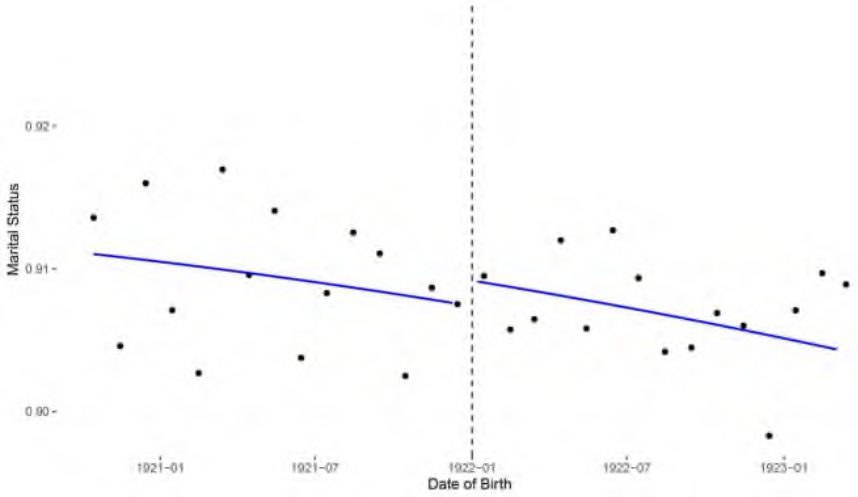
Figure 3.10: RDD effects of forced labor conscription on education based on 1971 Census

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

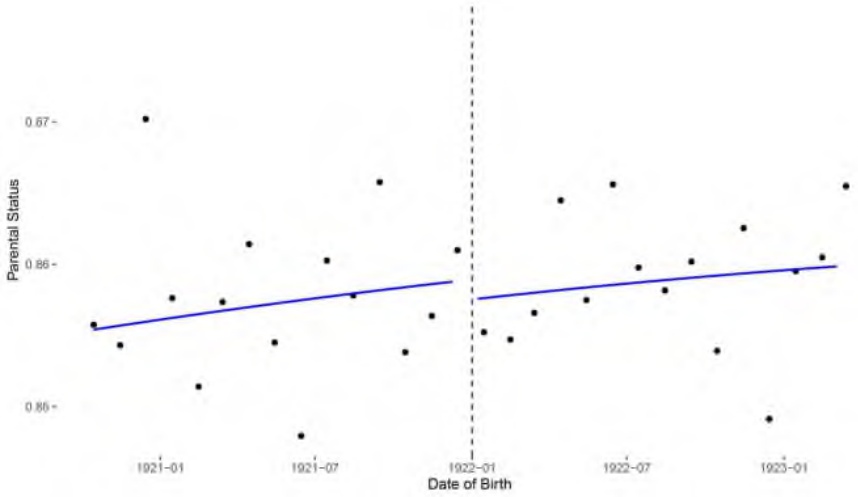
probability of being deported by calculating the share of conscripted individuals from each Dutch municipality that can be found in the archival records on forced workers in Germany.

The 1971 census does not include the municipality of birth itself, but only the current municipality and an indicator for whether an individual still lives in their municipality of birth (excluding temporary absences such as war-related reasons). Since I link the archival records based on place of birth, I therefore first restrict the census data to non-movers which leaves me with 62,319 observations in the sample with a 15-month bandwidth. I test whether the treatment itself affects the probability of a person still living in their municipality of birth, but I do not find any differences (see figure C.3.2 in the appendix).

For each Dutch municipality, I then compare the sample size of linked men from the archival records to the sample size of conscripted men born in the respective municipality to calculate a share of individuals who were forced workers in Germany (equation (3.3)). Figure 3.12 shows the regional distribution of the share of forced workers for each Dutch




(a) Marital status
Estimate: $\beta_1 = 0.0018$ (0.0026)




(b) Parental status
Estimate: $\beta_1$ = -0.0014 (0.0031)

Figure 3.11: RDD effects of forced labor conscription on family formation based on 1971 Census

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

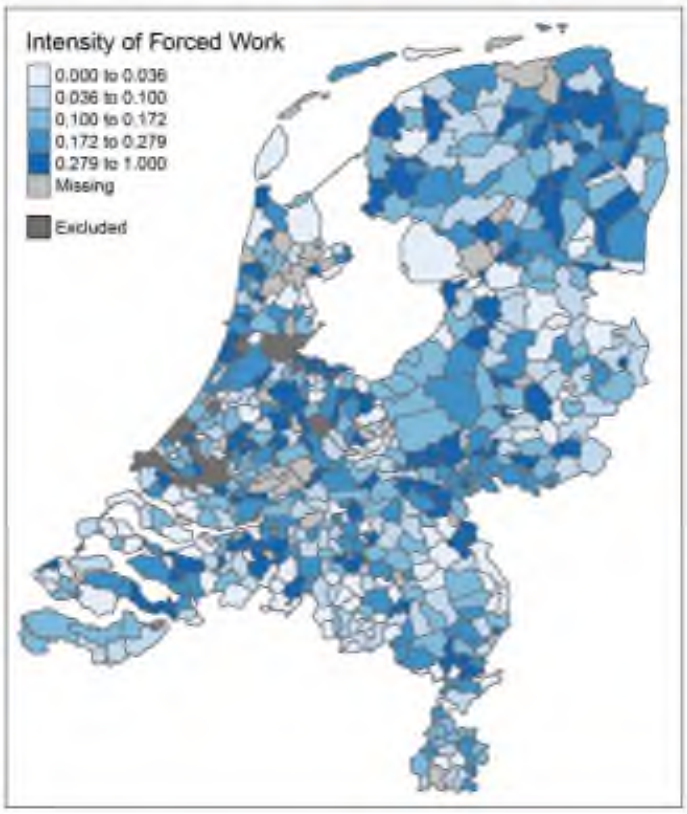



Figure 3.12: Regional distribution of intensity of forced labor over Dutch municipalities

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

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.[44]

There are no apparent no regional patterns in the share of deported men. To test more formally whether the probability of avoiding conscription by going into hiding is related to regional characteristics, I plot the share of deported individuals in each municipality

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

against the employment share in agriculture, industry, and services in figure C.3.1 in the appendix. I find no significant correlation, indicating that it was not the case that men in more industrial or more agricultural areas had an easier time avoiding conscription. Still, since the ability to go into hiding may be in part driven by factors that could also affect labor market outcomes, the results should be interpreted with caution.[45]

I now split the sample by municipalities with a deportation share above and below the median.[46] Individuals in the sub-sample with an above-median deportation share thus have a higher probability for forced labor compared to individuals in the sub-sample with a below-median deportation share. Accordingly, individuals from the first sub-sample have a lower probability of going into hiding compared to individuals from the second sub-sample. I repeat the analysis for labor market outcomes by estimating equation (3.2), 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 3.13.

In the new baseline using only individuals who still live in their municipality of birth in 1971, the effect on employment probability is again negative and significant. The effect on income becomes insignificant and close to zero, which could be due to the endogenous sample selection: While the treatment itself does not change the probability of a person of still living in their municipality of birth (see figure C.3.2 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 income small and insignificant.

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

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

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

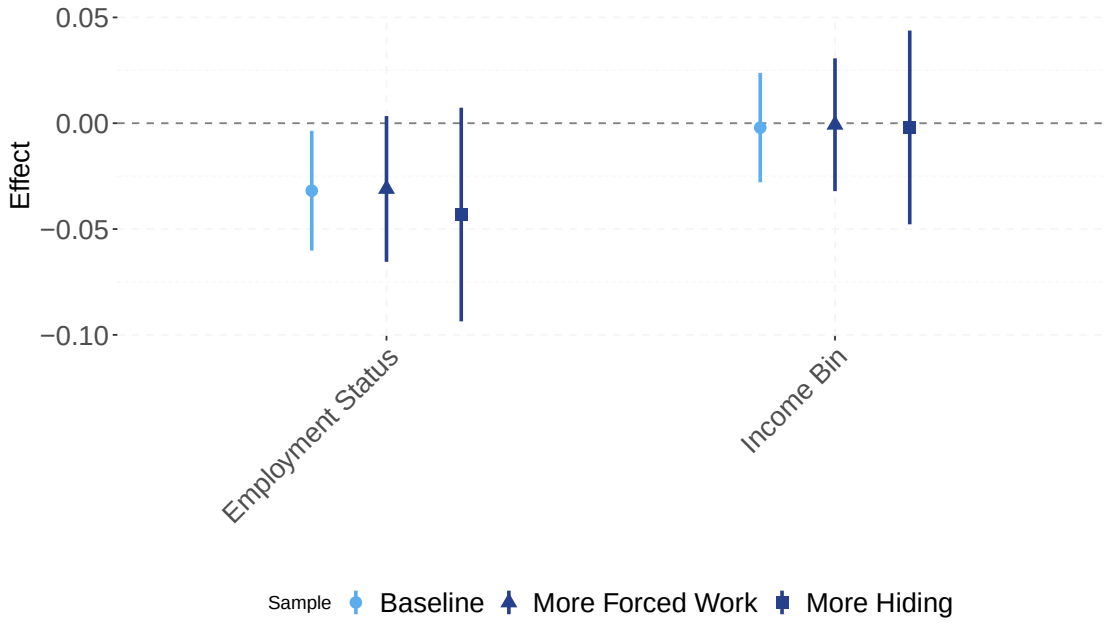



Figure 3.13: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by share of forced workers

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

#### 3.4.3.2 Exposure to adverse conditions

For the disruption in the form of forced labor 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 exposure to adverse conditions during the forced labor experience to understand whether exposure to harsher circumstances during the forced labor period may be a contributing factor to the negative effects on labor market success. Allocation to locations in Germany was as good as random since it was decided on in response to ad-hoc demand for labor at the time of deportation. The variation in adversity of the forced labor experience can thus be considered exogenous.

I proxy exposure to adverse conditions by two measures: First, the share of houses damaged due to Allied bombings, as forced workers often lacked access to shelters and had to clean up after bombings. Second, I use the distance to so-called labor education

camps as the punishment for workers who disobeyed orders was a temporary stay in such a camp, which had similar conditions to concentration camps. I aggregate each variable to the level of Dutch municipalities by the following equation:

$$adversity_m = \frac{\sum_{c=0}^{C} adversity_c forcedworkers_{c,m}}{\sum_{c=0}^{C} forcedworkers_{c,m}} \tag{3.3}$$

where $m$ is the Dutch municipality, $c$ is the German county, $forcedworkers_{c,m}$ is the number of forced workers in county $c$ who are born in municipality $m$,[47] and $adversity_c$ is either the share of damaged houses due to bombings or the distance to the nearest labor education camp for each German county $c$. The intuition is that this measure captures the average exposure of forced workers from each municipality to adverse conditions in Germany. Figure 3.14 shows the regional distribution of both measures, with exposure to Allied bombings in panel a and exposure to labor education camps in panel b. The variation does not display any regional patterns, which is in line with the quasi-random distribution of forced workers over German locations.

I then split the sample based on whether an individual is from a municipality with an above or below median exposure to adverse conditions as measured by $adversity_m$ and estimate equation (3.2) separately for each sample.[48]

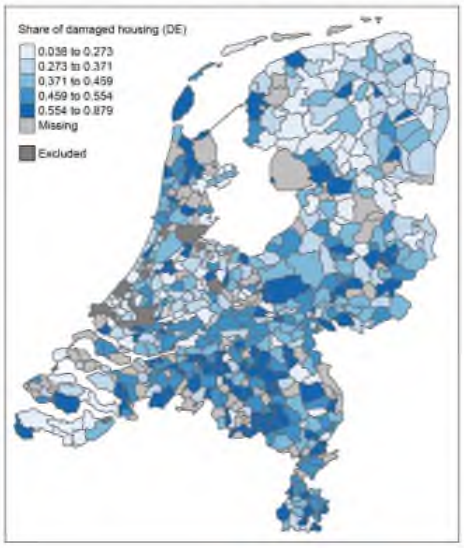
Figure 3.15 shows the results, with panel a showing the results for the sample split by exposure to Allied bombings, and panel b showing the results for the sample split by exposure to labor education camps.[49] In both cases, the effects are more negative for individuals from Dutch places where forced workers were more exposed to adverse conditions in Germany. The negative effect of forced labor disruption on the probability of being employed is completely driven by individuals from Dutch municipalities where forced workers were more exposed to more adverse conditions in Germany (both for the share of houses damaged and the distance to labor education camps).[50] This suggests that the adverse living conditions while in Germany are a reason for the continued lower labor market success of former forced workers, possibly because this negatively affected their health.

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

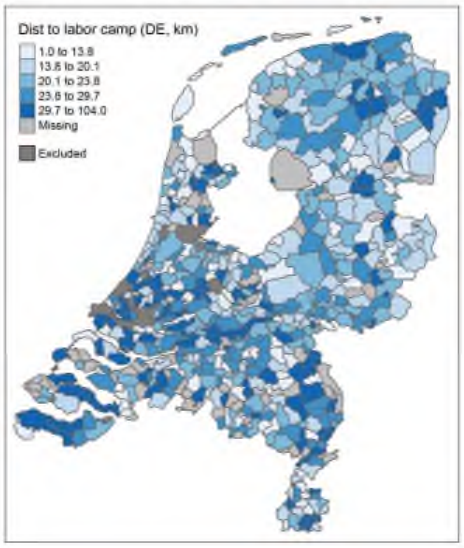
[48]The median average weighted share of houses damaged in Germany is 0.4, and the median average weighted distance to a labor education camp is 21.8 km.

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

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



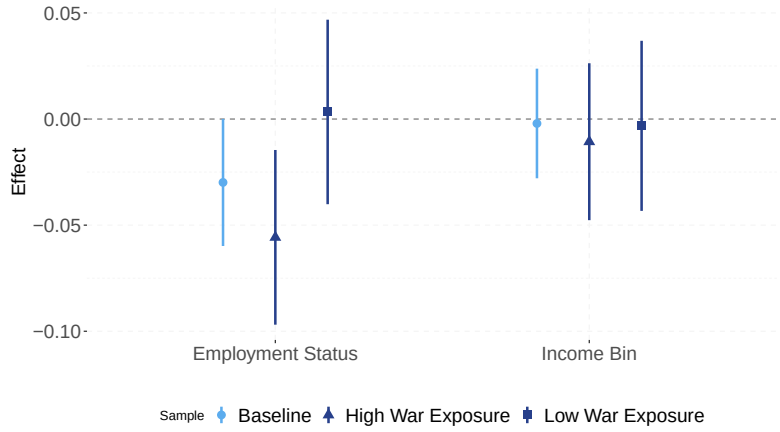

(a) Regional distribution of the average share of damaged housing in German locations over Dutch municipalities



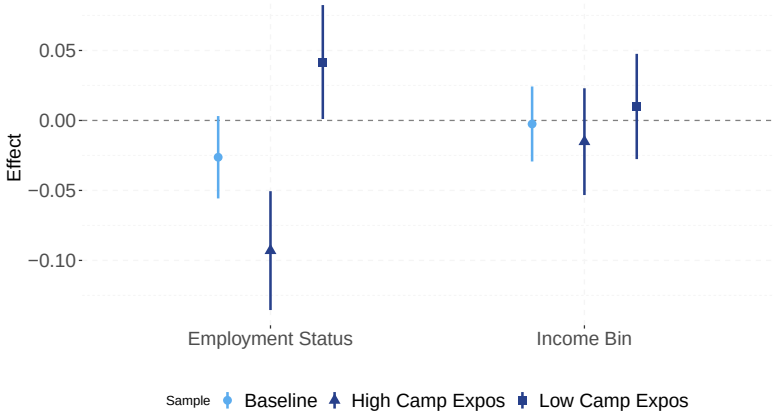

(b) Regional distribution of the average distance to labor education camps from German locations over Dutch municipalities

Figure 3.14: Regional distribution of exposure to adverse conditions over Dutch municipalities

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



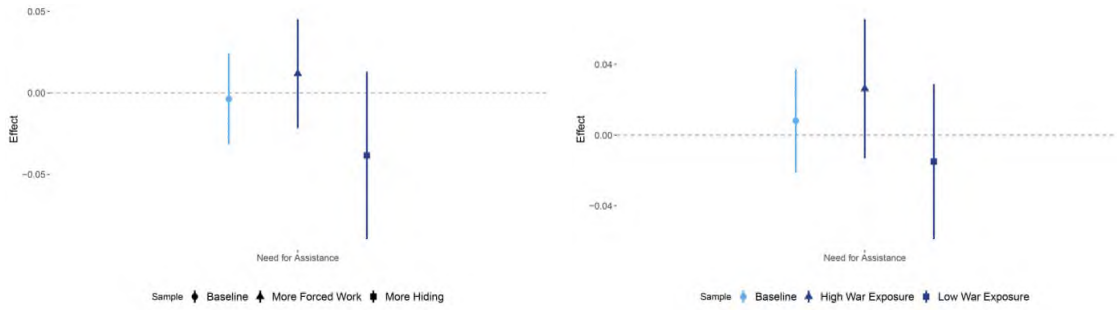

(a) Heterogeneity by share of damaged housing in DE




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

Figure 3.15: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience

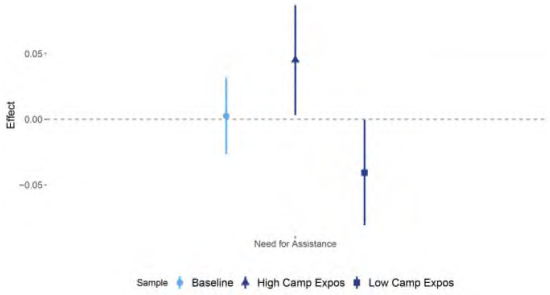
*Notes.* This figure shows RDD regressions using a 15-month bandwidth, a triangular kernel, and a polynomial of the running variable of degree two for economic outcomes using the 1971 Census with subsamples. The sample is restricted to individuals who still live in their place of birth. In panel (a),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




(a) Heterogeneity by compliance




(b) Heterogeneity by war exposure in DE




(c) Heterogeneity by camp exposure in DE

Figure 3.16: Heterogeneous RDD effects of forced labor conscription on need for assistance based on 1971 Census

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

To test if indeed poorer health due to harsher forced labor conditions is a contributing factor for the lower probability of employment, I repeat the heterogeneity analysis with a direct measure of an individual's health. I proxy physical health by a question in the 1971 census on whether an individual is in need of assistance by others for their own care, household tasks, or for getting to places outside of their home.[51] I find that for the sub-sample with higher exposure to adverse conditions in Germany, the treatment

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

increases the probability of needing assistance, which is in line with the interpretation that an adverse forced labor experience led to worse health (see figure 3.16). Physical health is also worse for the individuals from the sub-sample with a higher probability of forced labor instead of hiding (see panel a of figure 3.16), 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 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:[52]

$$lifesat_{i,t} = \beta_0 + \Sigma_{t=1}^{T} \beta_{1,t} Treat_{it} + \lambda_t + \epsilon_{i,t} \tag{3.4}$$

where $lifesat_{it}$ is a measure of life satisfaction as the dependent variable[53] and $labmda_t$ are wave fixed effects to control for slightly different ways in which the question was formulated. To ensure the validity of this approach, I replicated the results on labor market success and family formation using the same estimation setup. Figures C.2.1 and C.2.2 in the appendix show that the results are overall comparable.[54] As the data lacks information on a person's place of birth, I cannot link it to the archival records to estimate heterogeneous effects by the adversity of the forced labor experience.

Figure 3.17 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 probability of PTSD (Post Traumatic Stress Disorder) is positively correlated with the probability of having been a forced worker during the war (Bramsen, 1998).[55] 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.

[52] Using only the age as the running variable would violate the assumption of a sufficiently large number of mass points described by Cattaneo, Idrobo, and Titiunik (2024).

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

[54] See section C.2 for a detailed discussion of the approach.

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

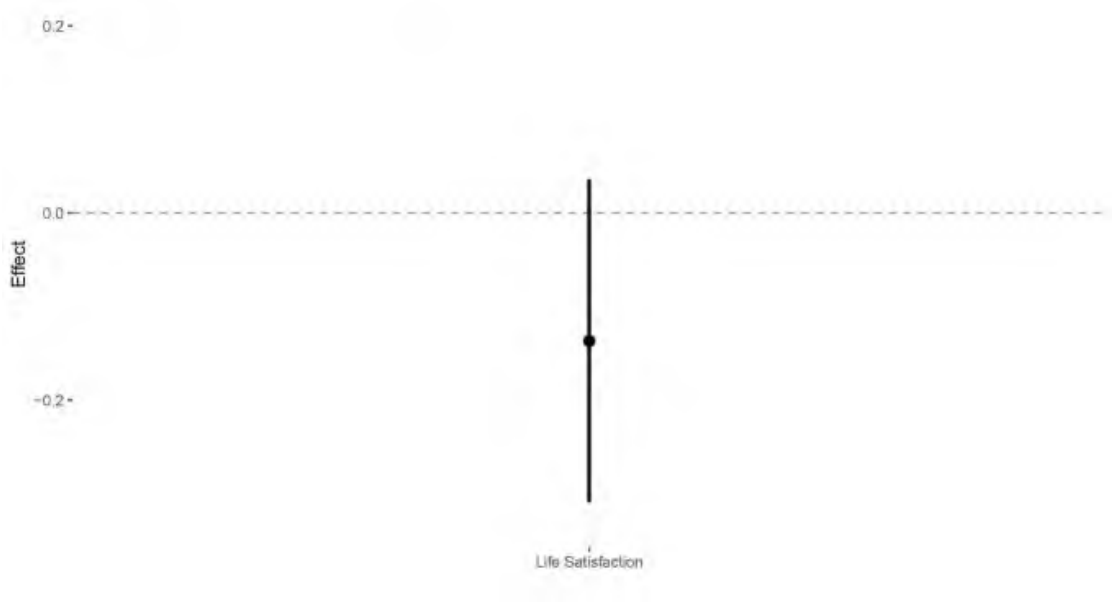


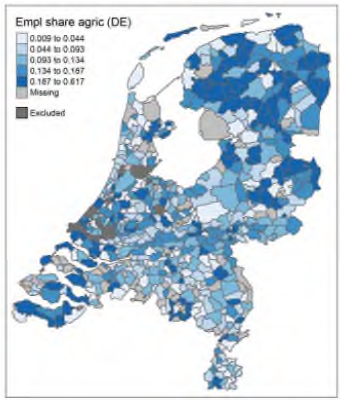

Figure 3.17: Effects of forced labor conscription on life satisfaction using 1975-1944 Eurobarometer

*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.

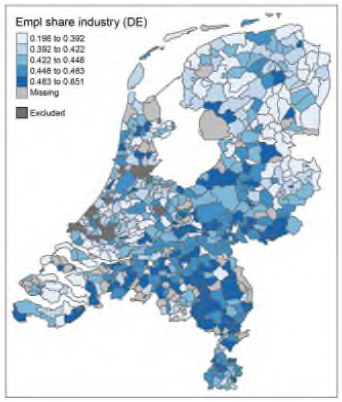
#### 3.4.3.3 Loss in labor market experience

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

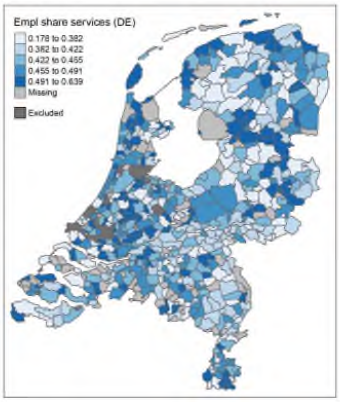
To measure the difference in occupations in Germany compared to occupations in the Netherlands, I use data on the sectoral composition of both locations. For Germany, I use county-level data from 1939. I link this data to the 1971 census on the municipality level by calculating the weighted average employment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




(a) Employment share: Agriculture




(b) Employment share: Industry




(c) Employment share: Services

Figure 3.18: 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.

$$empshare^{DE}_{m,j} = \frac{\sum_{c=0}^{C} empshare_{c,j} forcedworkers_{c,m}}{\sum_{c=0}^{C} forcedworkers_{c,m}} \tag{3.5}$$

where $m$ is the Dutch municipality, $c$ is the German county, $forcedworkers_{cm}$ is the number of forced workers in county $c$ who are born in municipality $m$[56], $j$ is the sector, and $empshare_{cj}$ is the employment share of sector $j$ in county $c$. Figure 3.18 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 3.6 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, $empshare^{DE}_{mj}$, and the employment shares of the respective municipality itself, $empshare^{NL}_{mj}$.

To allow for heterogeneous effects based on whether forced workers had a lower or higher loss in relevant labor market experience, I split the sample by whether a person is from a municipality with a below-median or above-median value of the correlation between the German and the Dutch industries.[57][58] Figure 3.19 shows that the negative effect of forced labor conscription on the probability of being employed is more pronounced for persons who were coerced into sectors different from the ones in their place of origin. In other words, being coerced into a sector in which a person could continue working in after their return to the Netherlands alleviated some of the negative consequences of forced labor conscription, probably because of a lower loss in relevant skills and labor market experience.

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

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

[58]The median value is 0.55.

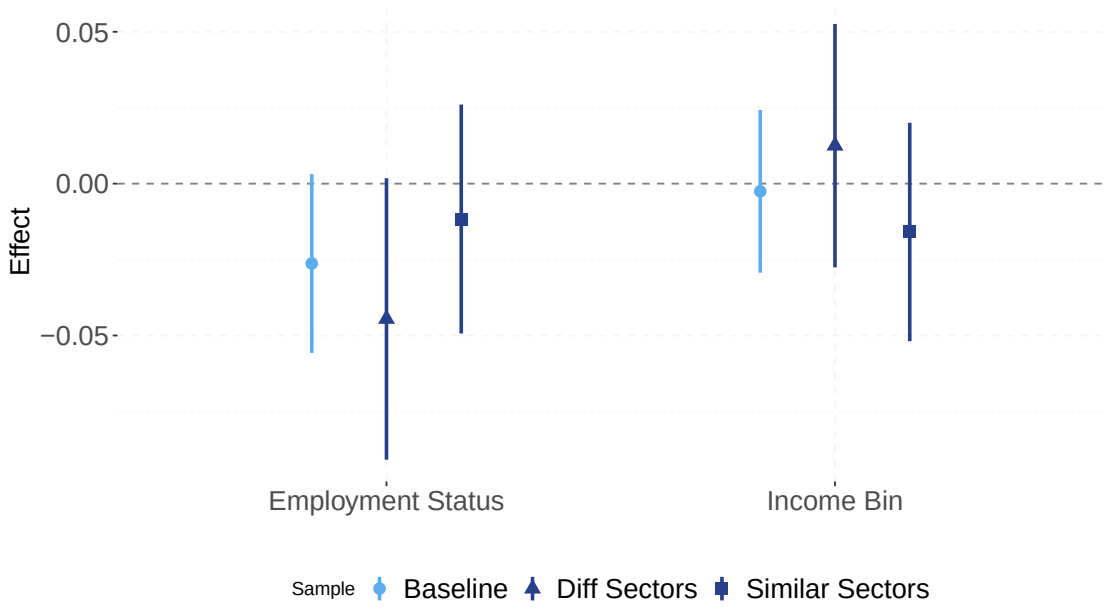



Figure 3.19: Heterogeneous RDD effects of forced labor Conscription on labor market outcomes by similarity of sectoral composition in DE and NL

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

## 3.5 Conclusion

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

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

In the early 2000s, Germany set up a compensation program for former forced workers. People from Western countries, including the Netherlands, were excluded from receiving compensation, due to a supposed lack of discriminatory living conditions (Stiftung Erinnerung, Verantwortung und Zukunft, 2017). My findings contradict this assessment, showing that especially those Dutch forced workers who faced more adverse conditions in Germany did suffer from long-lasting effects on their labor market success and their health. The consequences that I find constitute only a lower bound of the social costs of the forced labor conscription by Germany during World War II in a more generalized sense because compared to forced workers of other nationalities, Dutch forced workers were still treated relatively better.

When applying my findings to contexts beyond the forced labor regime of Nazi Germany, a key policy implication is a need to provide adequate support to former forced workers upon their return to their home countries to avoid that the possibly traumatic

experiences lead to long-term disadvantages in the labor market. In the Netherlands, the former forced workers faced suspicion of collaboration with the enemy, leading them to remain silent about their experiences. This may have prevented affected individuals from seeking help, thus exacerbating and perpetuating their losses in the labor market. Additionally, it may be beneficial to understand which skills former forced workers might have acquired during their forced labor experience, as I show that the negative consequences are less pronounced for people who gained more relevant labor market experience as forced workers. Encouraging the application of these skills in future work settings may help lessen the negative consequences of forced labor in the medium and long run.

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**Note**: All URLs were last accessed 2025-06-14.

# Appendices

# Appendix A

# Appendix to Chapter 1

## A.1 Historical background and data

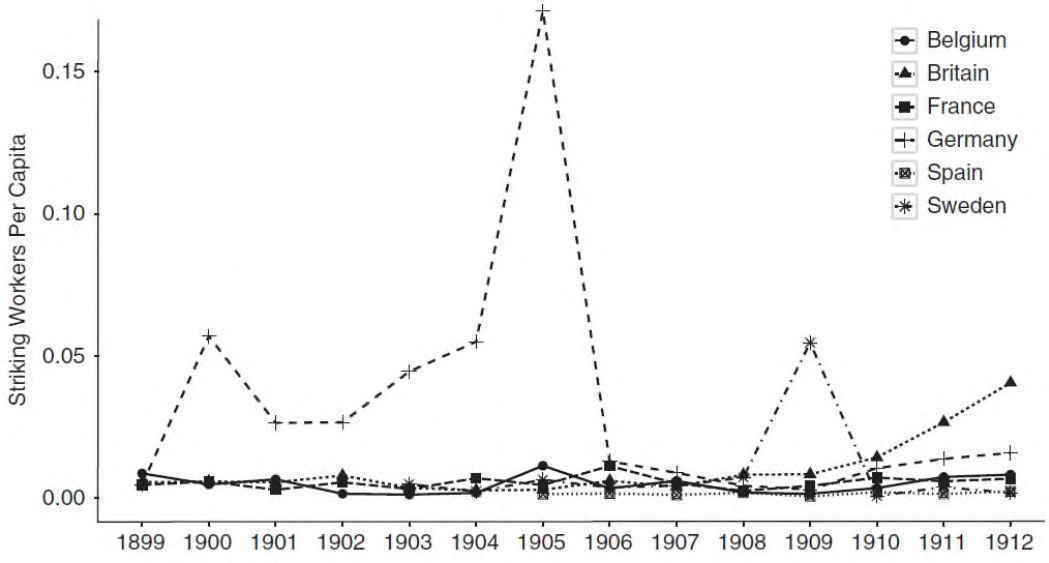



FIGURE 7.1: Annual Number of Striking Workers Per Capita, 1899–1912
Data Source: Mitchell 2003, 3–8, 172–85

Figure A.1.1: Annual number of striking workers per capita, 1899–1912
*Notes.* The Figure compares strike activity across European countries. Source: Ziblatt (2017), p. 228.

Morgen-Ausgabe.

„Berliner Tageblatt"

Bezugs- und Anzeigen-Preis.

# Berliner Tageblatt

und Handels-Zeitung

Nr. 620
38. Jahrgang

Dienstag
7. Dezember 1909

## Für die Preußische Wahlreform.

Die Gegner der preußischen Wahlreform behaupten, daß im deutschen Volke, und besonders in seinen Oberschichten, der Wunsch nach einer Reform des preußischen Wahlrechts nicht bestehe und daß diese Forderung nur von Leuten ohne jede Gefolgschaft erhoben werde. Um den Beweis zu liefern, daß solche Behauptungen unwahr und daß gerade die berufenen Vertreter der deutschen Bildung von der Notwendigkeit der Wahlreform durchdrungen sind, haben wir einer größeren Anzahl bekannter Persönlichkeiten auf den verschiedensten Gebieten des Wissens und des Erwerbslebens die hier folgende Kundgebung vorgelegt und sie um ihre Zustimmung und ihre Unterschrift ersucht. Indem wir heute mit der Kundgebung, die wir bisher zurückgehalten veröffentlichen, glauben wir sagen zu dürfen, daß der Beweis, auf den es uns ankam, schon jetzt voll und ausreichend erbracht ist. Wir begrüßen mit besonderer herzlicher Freude das Erwachen des politischen Geistes an den deutschen Hochschulen, wir konstatieren mit Befriedigung, daß sich dem vom Dreiklassenwahlrecht begünstigten Kreisen des Handels und der Industrie, soweit sie sich ein selbständiges Urteil gestatten, durchaus nicht an freiheitlichem Sinn und Opferfreudigkeit fehlt, und wir sind überzeugt, daß keine Regierung einer so starken und klar geäußerten Volksstimmung dauernd widerstehen kann.

Die Redaktion des „Berliner Tageblatts".

Die Kundgebung, die wir verfaßt haben, lautet:

Der entscheidende Augenblick ist gekommen, wo es sich zeigen muß, ob der Wille des Volkes stark genug ist, Preußen, den führenden Bundesstaat, auf die Bahn des politischen Fortschritts zu drängen. Die Bewegung, die an die überstürzte und egoistische Erledigung der Reichsfinanzreform durch den schwarz-blauen Block anknüpfte, würde ihr Ziel gründlich verfehlen, wenn es ihr nicht gelänge, endlich Bresche zu legen in die agrarkonservative Vorherrschaft über Preußen und damit über Deutschland.

Ein Volk von 60 Millionen, das in Handel und Industrie, in Gewerbe und Technik, in Wissenschaft und Kunst rastlos der Vollkommenheit zustrebt, darf die Richtlinie seiner Politik nicht ausschließlich bestimmen lassen nach den Bedürfnissen jener kleinen Oberschicht, die sich in den östlichen Provinzen Preußens dem Eindringen modernen Geistes erfolgreich entgegenstemmt.

Darum ist es eine Lebensfrage für Preußen nicht nur, sondern auch für das gesamte Deutsche Reich, daß den aufstrebenden Erwerbsständen der Weg freigemacht werde zur preußischen Volksvertretung. Mit vollem Rechte bezeichnete daher auch die Thronrede vom 20. Oktober 1908 die organische Fortentwicklung des Wahlrechts als eine der wichtigsten Aufgaben der Gegenwart.

Es gilt, diesem Gedanken Nachdruck zu verschaffen. Es gilt zu zeigen, daß die preußische Krone sich bei dieser Forderung in Uebereinstimmung befindet mit den berufenen Vertretern deutscher Geistesbildung und deutschen Wirtschaftslebens.

Bis gestern früh hatten uns ihre Zustimmung ausgesprochen und ihre Unterschrift gesandt:

Figure A.1.2: Call for suffrage reform in the "Berliner Tageblatt" 1909

*Notes.* The Figure shows the call for suffrage reform on page 1 of the "Berliner Tageblatt" dating December 7, 1909.

PRUSSIA'S 1912 LEGISLATION ON REFORMING THE SUFFRAGE[a]

| *Party* | *Total Reps* | *Yes* | *No* | *Abstain* |
|---|---|---|---|---|
| Conservative (K) | 152 | 0 | 139 | 12 (0) |
| National Lib (N) | 65 | 45 | 0 | 20 (13) |
| Center (Z) | 102 | 58 | 0 | 44 (33) |
| Reichspartei (R) | 59 | 0 | 49 | 11 (0) |
| Left Liberals (F) | 36 | 33 | 0 | 3 (0) |
| Polish Party (P) | 15 | 14 | 0 | 1 (1) |
| Social Dem (S) | 6 | 6 | 0 | 0 (0) |
| Danes (D) | 2 | 2 | 0 | 0 (0) |
| All | 437 | 158 | 188 | 91 (47) |

[a]Data for this are drawn from the minutes of the Prussian parliament. See Verhandlungen des Hauses der Abgeordneten 77. Sitzung, 21. Legislative Period, May 20, 1912, 6428–6432. In the abstentions column, the figures in parentheses refer to the number of delegates who "abstained without excuse."

Figure A.1.3: Roll call vote on franchise reform in Prussia 1912

*Notes.* The Figure shows results from a roll call vote on franchise reform in the Prussia parliament in 1912 (Source: Ziblatt (2008), p. 228).

## A.2 Variable definitions

**Strike intensity** We use data on strikes from 1899 to 1905 (Kaiserliches Statistisches Amt (1900); Kaiserliches Statistisches Amt (1901); Kaiserliches Statistisches Amt (1902); Kaiserliches Statistisches Amt (1903); Kaiserliches Statistisches Amt (1904); Kaiserliches Statistisches Amt (1905); Kaiserliches Statistisches Amt (1906)) and define strike intensity as the average annual number of participants in a given location. In robustness tests, we also use strike frequency (average annual number of strikes), strike duration (average annual number of working days lost due to strikes), defensive strikers (average annual number of strikers with demands against wage decrease or working time increases), offensive strikers (average annual number of strikers with demands for wage increase or working time reductions), successful strikers (average annual number of strikers whose demands were met), unsuccessful strikers (average annual number of strikers whose demands were not met), political strikers (average annual number of strikers in wildcat strikes), and non-political strikers (average annual number of strikers in union-approved strikes).

**Change in redistributive spending** Using budget data from Prussian cities with at least 25,000 inhabitants Silbergleit (1908), we define changes in the share of redistributive spending (spending for education and health) as $\Delta ExpendShare_{c,i,1898-1908} = (\frac{Expend_{c,i,1908}}{ExpendTotal_{i,1908}} - \frac{Expendc,i,1898}{ExpendTotal_{i,1898}})$ x 100.

**Bourgeois support** We create an indicator variable which takes a value of one if in a given location, at least one person signed the proclamation to abolish the three-class franchise published in the newspaper 'Berliner Tageblatt' on December 7 and 11, 1909.

**Franchise reform voting** Using data on a roll-call vote on replacing the three-class franchise with general, direct, and secret elections in the Prussian House of Representatives on May 20, we code parliamentary support for franchise reform as an indicator that assumes the value one, if delegate $j$ in constituency $i$ voted in favor of the reform, and against and unexcused votes as zero, dropping excused votes and abstentions. In robustness tests, we alternatively code Yes / No excluding any abstentions or absent members of parliament; Yes / Abstention / No coded as 1/0/-1; Yes / Abstention / Unexcused/No coded as 4/3/2/1; Yes / Unexcused / No coded as 1/0/-1; and Yes / Unexcused coded as 1/0.

**Changes in wages** $\Delta wage$ is defined as the change in day-laborer wages, measured in Reichsmark, across cities from 1897 to 1910 taken from Zentralblatt (1897-1910).

### A.2.1 Geographic controls

To control for local geographic conditions, we construct a host of control variables that we include in our regression. These control variables are defined in the following way:

**Latitude and longitude.** When the data is on the city level, latitude and longitude are the coordinates of the city center. For the data on constituency and members of parliament level, latitude and longitude are the centroids of the constituency polygon.

**Elevation.** We use elevation data at a geospatial resolution of 30 arc seconds (i.e. grid cells of 1 kilometer squared), taken from Fick and Hijmans (2017). For the data on city level, we construct the average elevation levels for each county surrounding the city. For the data on constituency level, we construct the average elevation of the respective constituency.

**Temperature.** The data on temperature comes from Fick and Hijmans (2017) and contains monthly temperature measures for 1970–2000 at a geospatial resolution of 30 arc seconds. We calculate average temperature levels (in degrees Celsius) by temporally and spatially aggregating this time series information. The spatial aggregation is done once to the county level for the data on city level, where we take the average temperature of the county surrounding the respective city, and once to the constituency level.

**Precipitation.** The data on precipitation comes from Fick and Hijmans (2017) and contains monthly rainfall measures for 1970–2000 at a geospatial resolution of 30 arc seconds. We calculate average precipitation levels (in millimeters) by temporally and spatially aggregating this time series information. The spatial aggregation is done once to the county level for the data on city level, where we take the average precipitation of the county surrounding the respective city, and once to the constituency level.

**Distance to river.** The distance to nearest navigable river (in kilometers) is based on a historical map by IEG (2010) and is calculated as the shortest distance from the respective city for data on city level and from the centroid of the respective constituency for the data on constituency level.

**Distance to coal.** The distance to coal (in kilometers) is proxied by the distance to the nearest carboniferous rock strata following Michaels (2011) and Fernihough and O'Rourke (2021), as most coal in Germany was formed during the carboniferous area. This geospatial data comes from the 1:5 Million International Geological Map of Europe and Adjacent Areas (Asch (2005)). The distance is calculated as the shortest distance from the respective city for data on city level and from the centroid of the respective constituency for the data on constituency level.

### A.2.2 Development controls

To control for local socio-economic conditions, we construct the following control variables:

**Employment share industry.** The share of workers employed in the industrial sector in 1895 on the county level is taken from Bräuer and Kersting (2024), as reported in the occupational census 1895 (KaiserlichesStatistischesAmt.1897.109). We construct the variable by dividing the number of full-time workers in the industrial sector in each county by the total population of the county in 1895 taken from Thome (2006). For the data on city level, we take the value of the county surrounding the city. For the data on constituency level, we sum up employment and population data to the constituency level. On average, a constituency consists of 2.01 counties.

**Land inequality.** Landownership concentration is measured as the share of farms in county that have a size of at least 100ha in 1895 and is taken from Thome (2006). For the data on city level, we use the share of large farms in the county surrounding the respective city. For the data on constituency level, we calculate the weighted average of the share of large farms by aggregating the county-level shares, weighting each by the proportion of the county's area within the respective constituency. On average, a constituency consists of 2.01 counties.

**Share urban.** The share of population that lives in urban areas is defined as the share of people living in locations with over 2,000 inhabitants in 1900 as reported by Thome (2006). For the data on city level, we use the share of urban population in the county surrounding the respective city. For the data on constituency level, we use the share of urban population in the respective constituency, summing up the population numbers from the county level by Thome (2006) to the constituency level. On average, a constituency consists of 2.01 counties.

**Share protestant.** The share of population that is protestant is measured in 1900 and taken from Thome (2006). For the data on city level, we use the share of protestant population in the county surrounding the respective city. For the data on constituency level, we use the share of protestant population in the respective constituency, summing up the population numbers from the county level by Thome (2006) to the constituency level. On average, a constituency consists of 2.01 counties.

**Share foreign.** The share of the population that has a mother language other than German is measured in 1900 and taken from Thome (2006). For the data on city level, we use the share of foreign population in the county surrounding the respective city. For the data on constituency level, we use the share of foreign population in the respective constituency, summing up the population numbers from the county level by Thome (2006) to the constituency level. On average, a constituency consists of 2.01 counties.

### A.2.3 Further controls

**Strikes 1871–1882.** In our Appendix, we also report a robustness check where we control for local levels of labor unrest that precedes our period of analysis. To do so, we digitized and geocoded data on strikes from Machtan (1984) and Milles (1980) for the period of 1871–1882 which was hand-collected based on various newspaper sources. We then aggregated the number of strikes to the city level and the constituency level.

**Sum of IV shares.** Because we were not able to match all industries to at least one commodity in the instrumental variable setup, we follow Borusyak, Hull, and Jaravel (2022) by controlling for the total sum of employment shares of all matched industries in both the first and second stage of all IV estimations.

## A.2.4 Summary statistics

Table A.2.1: Summary statistics

| Variable | Obs. | Min | Mean | Max | SD |
|---|---|---|---|---|---|
| Δ redistributive expenditure share | 100 | -42.51 | -3.89 | 18.08 | 10.17 |
| Δ non-redistributive expenditure share | 100 | -18.08 | 3.89 | 42.51 | 10.17 |
| Strike intensity | 100 | 0.00 | 535.89 | 16464.57 | 1927.37 |
| arcsinh(Strike intensity) | 100 | 0.00 | 5.47 | 10.40 | 1.72 |
| arcsinh(Price Shock) | 100 | 0.21 | 1.79 | 2.97 | 0.77 |
| Elevation | 100 | 2.39 | 112.24 | 421.92 | 91.70 |
| Temperature | 100 | 7.13 | 9.13 | 10.87 | 0.79 |
| Precipitation | 100 | 42.04 | 59.44 | 97.35 | 13.67 |
| arcsinh(Distance to River) | 100 | 0.03 | 2.46 | 5.47 | 1.41 |
| arcsinh(Distance to Coal) | 100 | 0.00 | 3.96 | 6.47 | 1.79 |
| Employment share industry | 100 | 0.06 | 0.20 | 0.28 | 0.05 |
| Land inequality | 100 | 0.00 | 0.17 | 0.73 | 0.19 |
| Share urban | 100 | 0.29 | 0.86 | 1.00 | 0.22 |
| Share protestant | 100 | 0.04 | 0.66 | 0.98 | 0.31 |
| Share foreign | 100 | 0.00 | 0.06 | 0.78 | 0.15 |
| Strikes 1871-1882 | 100 | 0.00 | 0.27 | 3.01 | 0.42 |
| Sum of IV shares | 100 | 0.19 | 0.51 | 0.81 | 0.15 |
| Δ wage | 100 | -0.40 | 0.73 | 1.30 | 0.30 |
| Signee indicator | 264 | 0.00 | 0.31 | 1.00 | 0.46 |
| Strike intensity | 264 | 0.00 | 1087.23 | 17512.00 | 3798.90 |
| arcsinh(Strike intensity) | 264 | 0.00 | 4.83 | 10.46 | 2.40 |
| arcsinh(Price Shock) | 264 | 0.95 | 1.84 | 2.80 | 0.45 |
| Elevation | 264 | -1.29 | 142.73 | 670.78 | 129.03 |
| Temperature | 264 | 6.38 | 8.65 | 10.83 | 0.80 |
| Precipitation | 264 | 42.13 | 58.38 | 96.15 | 11.99 |
| arcsinh(Distance to River) | 264 | 0.01 | 3.17 | 5.56 | 1.30 |
| arcsinh(Distance to Coal) | 264 | 0.00 | 4.47 | 6.48 | 1.58 |
| Employment share industry | 264 | 0.04 | 0.17 | 2.18 | 0.17 |
| Land inequality | 264 | 0.00 | 0.22 | 0.76 | 0.19 |
| Share urban | 264 | 0.00 | 0.43 | 1.00 | 0.29 |
| Share protestant | 264 | 0.01 | 0.67 | 1.00 | 0.34 |
| Share foreign | 264 | 0.00 | 0.11 | 0.86 | 0.22 |
| Strikes 1871-1882 | 264 | 0.00 | 0.31 | 3.02 | 0.66 |
| Sum of IV shares | 264 | 0.11 | 0.33 | 0.78 | 0.16 |
| voting (yea/nay+unexcused) | 375 | 0.00 | 0.40 | 1.00 | 0.49 |
| Signee indicator | 375 | 0.00 | 0.33 | 1.00 | 0.47 |
| Strike intensity | 375 | 0.00 | 852.36 | 17512.00 | 3135.80 |
| arcsinh(Strike intensity) | 375 | 0.00 | 4.96 | 10.46 | 2.30 |
| arcsinh(Price Shock) | 375 | 0.95 | 1.83 | 2.80 | 0.45 |
| Elevation | 375 | -1.29 | 144.71 | 670.78 | 127.93 |
| Temperature | 375 | 6.38 | 8.61 | 10.83 | 0.84 |
| Precipitation | 375 | 42.13 | 57.13 | 96.15 | 11.61 |
| arcsinh(Distance to River) | 375 | 0.01 | 3.25 | 5.56 | 1.23 |
| arcsinh(Distance to Coal) | 375 | 0.00 | 4.44 | 6.48 | 1.59 |
| Employment share industry | 375 | 0.04 | 0.17 | 2.18 | 0.19 |
| Land inequality | 375 | 0.00 | 0.25 | 0.76 | 0.20 |
| Share urban | 375 | 0.00 | 0.42 | 1.00 | 0.27 |
| Share protestant | 375 | 0.01 | 0.64 | 1.00 | 0.36 |
| Share foreign | 375 | 0.00 | 0.12 | 0.86 | 0.24 |
| Strikes 1871-1882 | 375 | 0.00 | 0.26 | 3.02 | 0.56 |
| Sum of IV shares | 375 | 0.11 | 0.32 | 0.78 | 0.16 |

*Notes:* Table shows summary statistics for main and control variables at the city level (100 obs.), constituency level (264 obs.), and at the MP level (375 obs).

## A.3 Instrument construction and tests

For plausibly exogenous variation in labor unrest, we employ an instrumental setup with a shift-share IV that maps price changes in global commodities to Prussian regions using the relative importance of each location as an employer in the respective industry.

The prices are annual spot prices for 40 commodities between 1899 and 1905 from Jacks (2019). We match these to the 101 industry categories from the occupational census of 1895 in a two-step procedure: First, following Autor, Dorn, and Hanson (2013), we use the concordance of industries and product classes by Pierce and Schott (2012) which are based on US census data as a baseline for matching prices into industry categories. We do so by first assigning each commodity the corresponding four-digit HS code. From the data by Pierce and Schott (2012), we get the respective NAICS code of the industries that use each commodity. We then match these NAICS industry codes to the industry classifications of the occupational census of 1895. This results in 30 commodities matched into 19 industries. In a second step, we add additional matches which reflect the (historical) production processes. We match a commodity to an industry if it was used either as an input or an output commodity, as price changes in both inputs and outputs may induce strike activity. We exclude agriculture in this matching process because there were no strikes in agriculture in our sample period. In the end, out of the 103 available industry categories, we are able to match the price data of 38 commodities to 45 industries, which employ 69.7% of all workers in industry and services in Prussia.

The detailed matching is as follows:[A.3.1]

- ore extraction and processing is matched to *coal*, iron ore, *copper*, *lead*, and *zinc*;
- smelting is matched to *coal*, iron ore, *steel*, *copper*, *lead*, and *zinc*;
- salt industry is matched to *coal* and *potash*;
- coal industry is matched to coal;
- loam and clay goods industry is matched to *coal*;
- glass industry is matched to *potash*, *coal*, and *lead*;
- precious metal industry is matched to gold, silver, and platinum;

[A.3.1] Commodities in italics indicate that they were assigned by us to reflect the (historical) production processes. The remaining commodities are assigned according to the concordance of industries and product classes by Pierce and Schott (2012).

- non-precious metal industry is matched to aluminum, chromium, copper, lead, manganese, nickel, platinum, tin, and zinc;
- iron and steel industry is matched to *coal*, iron ore, and steel;
- machinery industry is matched to *coal*, *iron ore*, *steel*, and *rubber*;
- wagon and ships industry is matched to *steel*, *copper*, and *rubber*;
- guns industry is matched to *steel*;
- electric machinery industry is matched to *coal* and *rubber*;
- chemical industry is matched to coal, petroleum, potash, sulfur, and phosphate;
- color industry is matched to *coal* and *lead*;
- explosives industry is matched to *phosphate* and *sulfur*;
- waste industry is matched to phosphate and potash;
- gas works is matched to coal and petroleum;
- light and soap industry is matched to *potash*;
- oil mills is matched to *cottonseed*, palm oil, and *peanuts*;
- resin, fat and varnish industry is matched to coal and petroleum;
- spun textiles is matched to wool and cotton;
- spinning industry is matched to wool and cotton;
- weaving industry is matched to wool and cotton;
- ropes industry is matched to *wool* and *cotton*;
- leather industry is matched to *hides* and *chromium*;
- wax and rubber products industry is matched to hides and rubber;
- saddle production is matched to *hides*;
- gold plating is matched to *gold*;
- plant-based food industry is matched to *barley*, corn, rice, *rye*, *wheat*, *cocoa*, *coffee*, and sugar;

- animal-based food industry is matched to beef, lamb, and pork;
- tinned food industry is matched to *tin*, beef, lamb, and pork;
- animal feed industry is matched to *barley*, *corn*, *rye*, and *wheat*;
- drinks industry is matched to *barley* and *wheat*;
- tobacco industry is matched to tobacco;
- clothing industry is matched to *hides*, *wool*, and *cotton*;
- shoes industry is matched to *hides*;
- construction is matched to *steel*, *tin*, and *copper*;
- roofing is matched to *tin* and *copper*;
- woodcarving is matched to *tin* and *zinc*;
- letterpress printing is matched to *tin* and *zinc*;
- engraving is matched to *copper* and *zinc*;
- commodity trade is matched to *coal* and *petroleum*;
- other trade is matched to *coal* and *petroleum*;
- land transportation is matched to *coal* and *petroleum*;
- water transportation is matched to *coal*.

In our instrumental variable approach, we employ the interpretation of Borusyak, Hull, and Jaravel (2022), where we think of exogeneity being driven by the shocks (changes in international commodity prices in our setting). For this approach to be valid, it needs to satisfy that the shocks are as-good-as-randomly assigned and that there are a large number of sufficiently independent shocks with sufficiently small average exposure. To test this, we report the shock summary statistics and shock intra-class correlations following Borusyak, Hull, and Jaravel (2022).[A.3.2] Table A.3.1 shows that the price shocks have a regular distribution with a mean of 0.08 and an interquartile range of 0.09. The effective sample size (the inverse of the Herfindahl Index of the industry exposure weights, which are defined as $\sum_i \frac{emp_{i,k}}{\sum_k emp_{i,k}}$) is 132.25 and thus sufficiently large, and the largest

[A.3.2] All tests are conducted on the constituency level.

industry exposure weight is 0.04, so sufficiently small. In addition, Table A.3.1 reports the shock intra-class correlation coefficient (ICC) to test whether the shocks are sufficiently mutually uncorrelated. Specifically, the ICC comes from estimating a random effects model with 17 industry categories. The correlation is sufficiently small with 0.01 and insignificant.

Table A.3.1: Shock Summary Statistics and Shock Intra-Class Correlation

| | Estimate | SE |
|---|---|---|
| Mean | 0.08 | |
| Standard deviation | 0.12 | |
| Interquartile range | 0.09 | |
| Effective sample size (1/HHI of weights) | 132.25 | |
| Largest weight | 0.04 | |
| Number of industries | 44.00 | |
| Shock ICCs (16 sectors) | 0.01 | (0.24) |

*Notes:* Table shows the distribution of price shocks across industries $k$, weighted by the average industry exposure share, as well as the intra-class correlation coefficients for the price shocks, estimated from a random effects model. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## A.4 Robustness: Redistribution

To test the robustness of our result that labor unrest increased spending for redistributive purposes, we do several checks: First, we separately add our baseline control variables in Table A.4.1. Throughout, our coefficient of interest remains similar in magnitude and highly significant. Second, we use different ways to deal with the scaling or our main explanatory variable *Strike intensity*, reported in Table A.4.2 columns (2) through (4). To address the issue of weighting the intensive and extensive margin effect described in Bellemare and Wichman (2020), we restrict the sample to only observations with Strike intensity larger than zero in column (2), thus only capturing the intensive margin. Out of the 100 initial observations, we only lose two observations with zero strikes. As expected, the coefficient of interest remains similar in magnitude and highly significant. In column (3), we employ the optimal scale factor for *Strike intensity* guided by the R-squared as described by Aihounton and Henningsen (2021) and find that if anything, the interpretation of our baseline specification (displayed in column (1)) is a lower bound of the true effect size. In column (4), we use the traditionally employed specification of using the natural logarithm but adding one to avoid zero values, which yields very similar results to our baseline. In a next step, we add additional control variables that may bias our results: In column (5), we add the employment share of each of the 16 industrial sectors separately to control for regional variation driven by industry presence of e.g. the mining sector. The resulting coefficient of interest is even larger than in our baseline results. In column (6), we control for previous levels of labor unrest (from 1871 to 1882) to exploit only realized mobilization during our sample period over and beyond underlying mobilization potential. As expected, the coefficient of interest becomes smaller and less precise, but remains positive and marginally significant. Column (7) reports the first stage results and column (8) reports the second stage results from our instrumental variable setup, which is similar to our baseline in magnitude but loses significance. The Kleibergen Paap F-Statistic is strong, but the wide Anderson-Rubin confidence interval prevents outright dismissal of the null hypothesis that strike intensity has no effect.

Figure A.4.1 shows the robustness of our results to using different measures of *Strike intensity*, namely the average annual number of strikes, the average annual number of working days lost due to strikes. In addition, we only consider the average annual number of strikers who participated in the following subcategories of strikes: Defensive vs. offensive, Successful vs. unsuccessful, and non-political vs. political. The point estimate is always positive and largely similar in magnitude to our baseline.

Figure A.4.3 replicates the results of Figure 1.2 using the instrumental variable approach. Reassuringly, we find that in response to labor unrest, the spending share for redistributive public goods increased, while the spending share for non-redistributive purposes decreased. The results indicate that the increase in redistributive spending is mostly driven by an increase in spending for education. Again, we find a zero effect for the change in security spending. Figure A.4.2 shows the results from a placebo exercise, where we replace the dependent variable with the change in the expenditure share from 1888 to 1898, so before the period of labor unrest from 1899 to 1905 that we observe. We do not find significant changes in spending patterns, and particularly no differential spending for redistributive and non-redistributive purposes.

Table A.4.1: Labor unrest and redistribution: robustness

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Δ redistributive expenditure share | | | | | | | |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike intensity | 1.287** | 2.379*** | 2.393*** | 2.150*** | 2.262*** | 2.686*** | 2.802*** | 2.740*** |
| | (0.636) | (0.697) | (0.712) | (0.707) | (0.695) | (0.794) | (0.793) | (0.797) |
| Latitude | | −2.047 | −2.165 | −2.253 | −2.014 | −1.845 | −0.229 | −0.766 |
| | | (2.951) | (3.010) | (2.993) | (2.928) | (2.813) | (2.786) | (2.781) |
| Longitude | | −0.765 | −0.752 | −0.940* | −1.019** | −1.080** | −1.424** | −1.538*** |
| | | (0.489) | (0.517) | (0.524) | (0.513) | (0.490) | (0.563) | (0.568) |
| Elevation | | −0.012 | −0.014 | −0.021 | −0.017 | −0.018 | −0.007 | −0.014 |
| | | (0.037) | (0.035) | (0.036) | (0.035) | (0.034) | (0.033) | (0.034) |
| Temperature | | −8.433* | −8.515* | −10.273** | −11.099** | −11.323*** | −12.349*** | −12.633*** |
| | | (4.758) | (4.665) | (4.706) | (4.575) | (4.375) | (4.686) | (4.666) |
| Precipitation | | −0.001 | 0.0001 | −0.048 | −0.124 | −0.133 | −0.233* | −0.221* |
| | | (0.090) | (0.101) | (0.113) | (0.133) | (0.129) | (0.133) | (0.132) |
| Distance to River | | | 0.126 | −0.112 | −0.159 | −0.483 | −0.764 | −0.677 |
| | | | (0.794) | (0.806) | (0.795) | (0.761) | (0.754) | (0.761) |
| Distance to Coal | | | −0.025 | 0.116 | −0.137 | −0.158 | −0.341 | −0.221 |
| | | | (0.644) | (0.612) | (0.677) | (0.661) | (0.643) | (0.626) |
| Employment share industry | | | | 38.217 | 27.715 | 60.421 | 74.529* | 73.452* |
| | | | | (30.221) | (28.925) | (37.022) | (38.646) | (38.335) |
| Land inequality | | | | | −10.479* | −10.432* | −10.791* | −10.814* |
| | | | | | (6.348) | (6.248) | (6.071) | (6.112) |
| Share urban | | | | | | −11.645** | −12.913** | −12.378** |
| | | | | | | (5.910) | (6.175) | (5.964) |
| Share protestant | | | | | | | −5.564 | −3.355 |
| | | | | | | | (3.890) | (5.025) |
| Share foreign | | | | | | | | 6.935 |
| | | | | | | | | (7.278) |
| Observations | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 |
| Adjusted $R^2$ | 0.038 | 0.097 | 0.078 | 0.086 | 0.103 | 0.129 | 0.132 | 0.127 |

*Notes:* Table shows results from estimating equation 1.1 at the city level, adding controls separately. Dependent variable is the change in the redistributive expenditure share from 1898 to 1908. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Column (8) is our baseline specification. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4.2: Labor unrest and redistribution: robustness

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Δ redistributive expenditure share | | | | | | Strike intensity | Δ redistributive expenditure share |
| | OLS | OLS | OLS | OLS | OLS | OLS | First Stage | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike intensity | 2.740*** | 3.292*** | | | 3.217*** | 2.208** | | 4.727* |
| | (0.797) | (0.828) | | | (0.848) | (0.930) | | (2.772) |
| Strike intensity*k | | | 3.442*** | | | | | |
| | | | (0.875) | | | | | |
| log(Strikers+1) | | | | 2.937*** | | | | |
| | | | | (0.815) | | | | |
| Strikes 1871-1882 | | | | | | 4.831* | | |
| | | | | | | (2.690) | | |
| Price Shock | | | | | | | 0.761*** | |
| | | | | | | | (0.222) | |
| Sum of IV shares | | | | | | | −3.915 | 45.007** |
| | | | | | | | (2.619) | (21.944) |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES |
| Employment Shares Separately | NO | NO | NO | NO | YES | NO | NO | NO |
| Kleibergen Paap F-Statistic | | | | | | | 11.82 | |
| Anderson Rubin CI | | | | | | | | -1.09–14.31 |
| Observations | 100 | 98 | 100 | 100 | 100 | 100 | 100 | 100 |
| Adjusted $R^2$ | 0.127 | 0.173 | 0.155 | 0.135 | 0.187 | 0.144 | 0.457 | 0.098 |

*Notes:* Table shows results from estimating equation 1.1 at the city level, with different transformations of the explanatory variable, adding additional controls, and using the instrumental variable approach described in section 2.4. Column (1) is our baseline specification. Dependent variable is the change in the redistributive expenditure share from 1898 to 1908 in columns (1) through (6) and column (8), and strike intensity in column (7). Strike intensity is average annual number of strikers in a constituency transformed in different ways: Using the inverse hyperbolic sine (arcsinh) in columns (1), (2), (5), (6), (7) and (8); using the inverse hyperbolic sine after multiplying with the optimal scale factor of 0.06 following Aihounton and Henningsen (2021) in column (3); and using the logarithm after adding 1 in column (4). Column (5) controls for the employment share of 16 different industrial sectors separately. All columns control for longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, and non-German share. Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

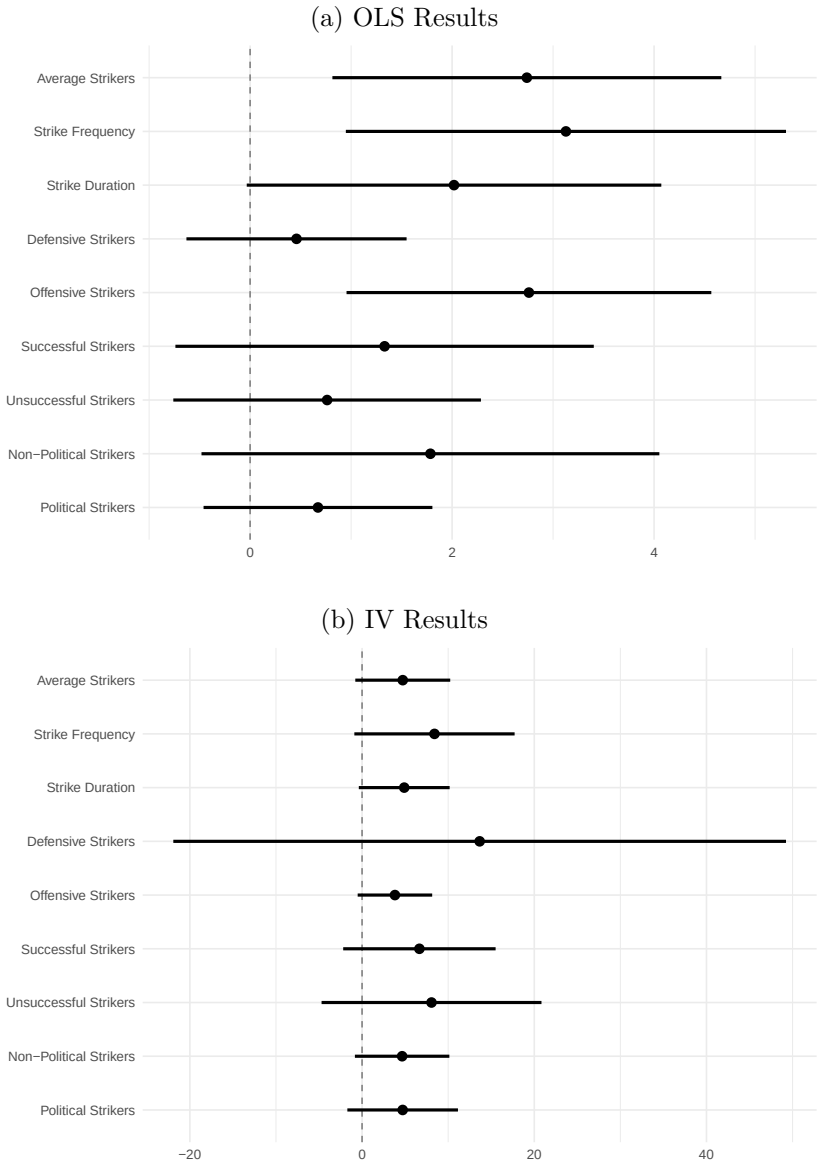



Figure A.4.1: Labor unrest and redistribution, measuring strike intensity in various ways

*Notes:* Dependent variable is change in city-level share of expenditures for redistributive public goods between 1898 and 1908. Explanatory variables are various constituency-level measures of strike intensity 1899–1905 (arcsinh transformed): average strikers (average annual number of strikers), strike frequency (average annual number of strikes), strike duration (average annual number of working days lost due to strikes), defensive strikers (average annual number of strikers with demands against wage decrease or working time increases), offensive strikers (average annual number of strikers with demands for wage increase or working time reductions), successful strikers (average annual number of strikers whose demands were met), unsuccessful strikers (average annual number of strikers whose demands were not met), political strikers (average annual number of strikers in wildcat strikes), non-political strikers (average annual number of strikers in union-approved strikes). Controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, non-German share. Standard errors are clustered at the county level. Bars indicate 95% confidence intervals.

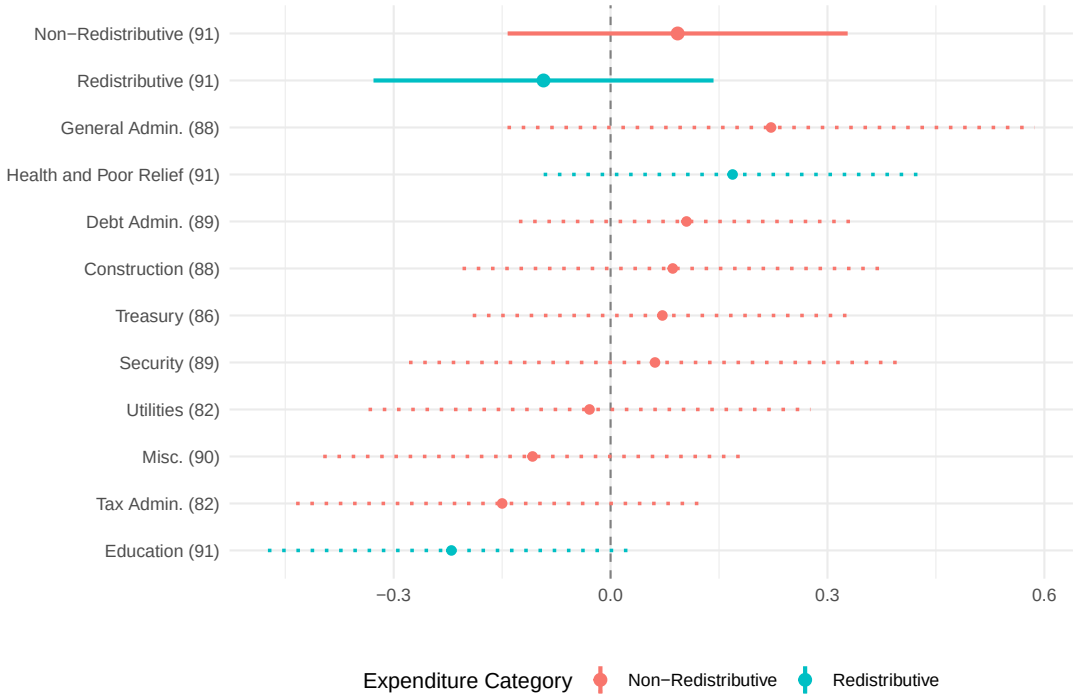



Figure A.4.2: Labor unrest and city-level redistribution: placebo results (1888-1898)

*Notes:* Figure plots $\beta_1$ coefficients from estimating equation 1.1 at the city level using OLS, where the dependent variable is measured as the difference in expenditures between 1888 and 1898. Each row replaces the outcome $Y_i$ for one of the ten expenditure categories. 'Redistributive' expenditures comprise 'education' and 'health and poor relief'. 'Non-redistributive' comprise all others. Dependent variables and main explanatory variable (strike intensity) are standardized with zero mean and standard deviation of one. Strike intensity is transformed using the inverse hyperbolic sine (arcsinh). All regressions are conditional on geographical (longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal) and development controls (industrial employment share, Protestant share, urbanization rate, landownership inequality, non-German share). Standard errors are clustered at the county level. Bars indicate 95% confidence intervals. Numbers in parentheses indicate the number of observations.

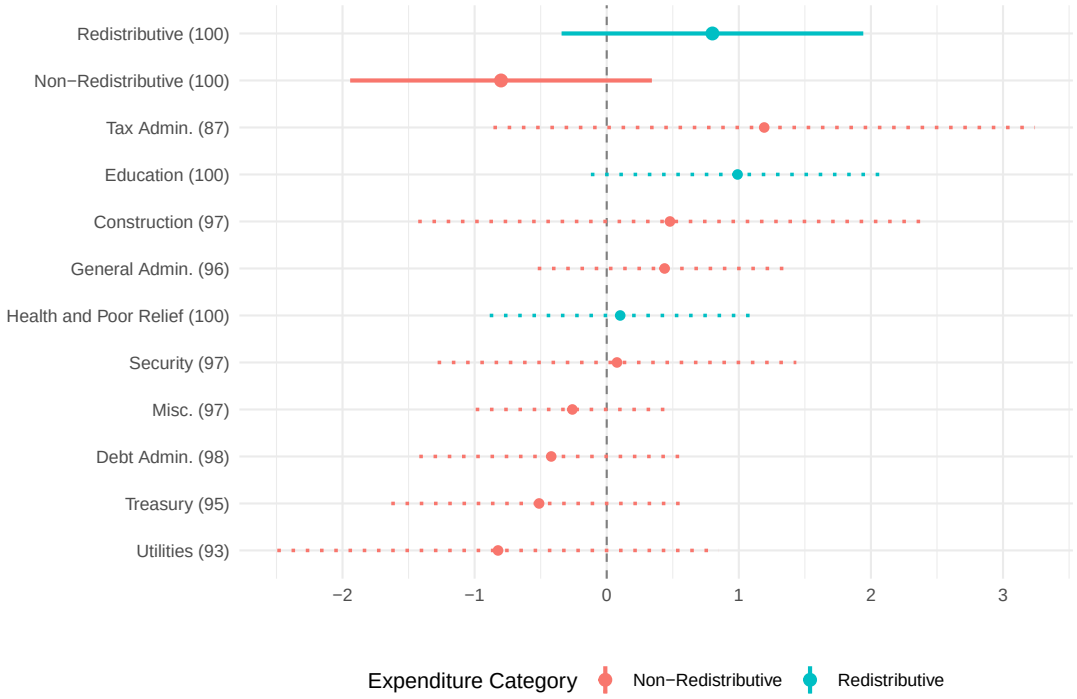



Figure A.4.3: Labor unrest and city-level redistribution: IV results

*Notes:* Figure plots $\beta_1$ coefficients from estimating equation 1.1 at the city level using the price shock from equation 1.2 as an instrumental variable. Each row replaces the outcome $Y_i$ for one of the ten expenditure categories. 'Redistributive' expenditures comprise 'education' and 'health and poor relief'. 'Non-redistributive' comprise all others. Dependent variables and main explanatory variable (strike intensity) are standardized with zero mean and standard deviation of one. Strike intensity is transformed using the inverse hyperbolic sine (arcsinh). All regressions are conditional on geographical (longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal) and development controls (industrial employment share, Protestant share, urbanization rate, landownership inequality, non-German share). Standard errors are clustered at the county level. Bars indicate 95% confidence intervals. Numbers in parentheses indicate the number of observations.

## A.5 Aside: Wages

Table A.5.1: Labor unrest and redistribution: robustness

| | *Dependent variable:* | | |
|---|---|---|---|
| | Δ wage OLS | Strike intensity First Stage | Δ wage IV |
| | (1) | (2) | (3) |
| Strike intensty | 0.021 | | 0.009 |
| | (0.019) | | (0.123) |
| Price Shock | | 0.761*** | |
| | | (0.222) | |
| Geographic Controls | YES | YES | YES |
| Development Controls | YES | YES | YES |
| Kleibergen Paap F-statistic | | 11.82 | |
| Anderson Rubin CI | | | -0.22–0.22 |
| Observations | 100 | 100 | 100 |
| Adjusted $R^2$ | 0.101 | 0.457 | 0.090 |

*Notes:* Table shows results from estimating equation 1.1 at the city level, replacing the dependent variable with the change in wages from 1897 to 1910. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Standard errors are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## A.6 Robustness: Bourgeois support

Table A.6.1 shows the robustness of our result that bourgeois support increased in response to labor unrest to adding our baseline control variables separately. The coefficient remains positive, highly significant and similar in magnitude. Table A.6.2 shows the robustness to several specification choices: Column (1) reports the baseline. In columns (2) through (4), we use different ways to address the issue of using an explanatory variable with some zero values (see section A.4 for a detailed discussion).[A.6.1] The point estimate remains positive and significant and, if anything, is larger than our baseline estimate. In columns (5) we control for the employment shares in industrial sectors separately, and in column (6) we control for initial levels of labor unrest. In column (7), we exclude the largest strike in 1905, which was a general strike in mining in Arnsberg, which alone had over 116,000 strikers, accounting for 34.4% of all strikers in 1905. Columns (8) and (9) report the first and second stage of our instrumental variable approach, which finds larger but less precisely estimated effects. In column (10), we use the number of signees instead of an indicator variable as the dependent variable, with the estimate finding positive and significant effects.[A.6.2]

Figure A.6.1 shows the robustness of our results to using different measures of *Strike intensity*, namely the average annual number of strikes, the average annual number of working days lost due to strikes. In addition, we only consider the average annual number of strikers who participated in the following subcategories of strikes: Defensive vs. offensive, Successful vs. unsuccessful, and non-political vs. political. The point estimate is always positive and largely similar in magnitude to our baseline.

Tables A.6.3 and A.6.4 follow the same structure, this time using the sample of counties outside of Prussia, excluding states with a three-class franchise (Saxony, Hamburg, and Lippe). In all OLS specifications, the point estimate remains positive and highly significant, confirming the finding that also outside of Prussia, the bourgeois elite responded to labor unrest by supporting democratic reform. The effect of price shocks in the first stage of the instrumental variable approach is insignificant, rendering the Anderson Rubin confidence intervals of the strike intensity estimate in the second stage unbounded.

[A.6.1] Out of the 264 initial observations, we only have 20 observations with zero strikes.
[A.6.2] The number of signees is transformed using the inverse hyperbolic sine to account for observations with zero signees and the skewness of the count variable.

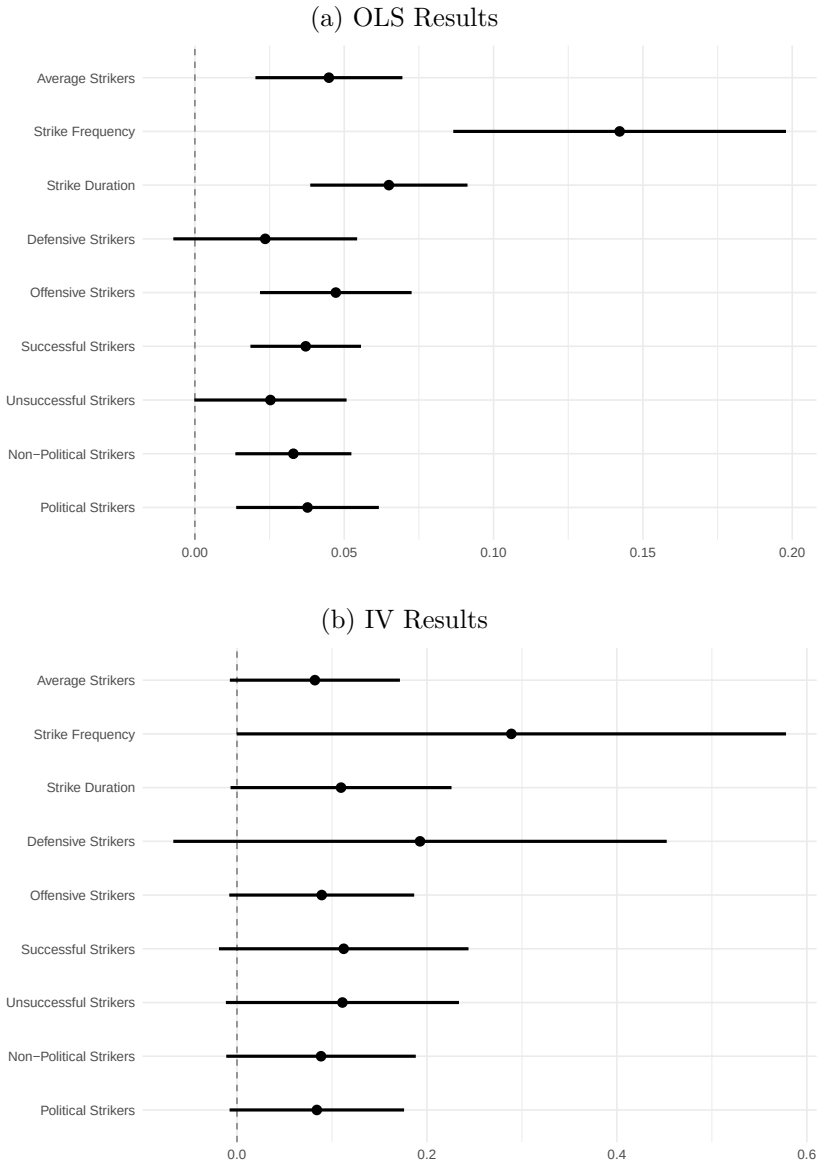



Figure A.6.1: Labor unrest and bourgeois support, measuring strike intensity in various ways

*Notes:* Dependent variable is one if constituency has at least one signee in 1909. Explanatory variables are various constituency-level measures of strike intensity 1899–1905 (arcsinh transformed): average strikers (average annual number of strikers), strike frequency (average annual number of strikes), strike duration (average annual number of working days lost due to strikes), defensive strikers (average annual number of strikers with demands against wage decrease or working time increases), offensive strikers (average annual number of strikers with demands for wage increase or working time reductions), successful strikers (average annual number of strikers whose demands were met), unsuccessful strikers (average annual number of strikers whose demands were not met), political strikers (average annual number of strikers in wildcat strikes), non-political strikers (average annual number of strikers in union-approved strikes). Controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, non-German share. Bars indicate 95% confidence intervals.

Table A.6.1: Labor unrest and bourgeois support for franchise reform

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | signee indicator | | | | | | | |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike intensity | 0.096*** | 0.074*** | 0.072*** | 0.070*** | 0.070*** | 0.045*** | 0.044*** | 0.045*** |
| | (0.008) | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) | (0.012) | (0.012) |
| Latitude | | 0.169*** | 0.153** | 0.151** | 0.149** | 0.098 | 0.077 | 0.081 |
| | | (0.059) | (0.064) | (0.063) | (0.064) | (0.062) | (0.067) | (0.068) |
| Longitude | | 0.032** | 0.029** | 0.028** | 0.026* | 0.007 | 0.010 | 0.016 |
| | | (0.013) | (0.013) | (0.014) | (0.015) | (0.015) | (0.015) | (0.015) |
| Elevation | | 0.002*** | 0.002** | 0.002** | 0.002** | 0.001 | 0.001 | 0.001 |
| | | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Temperature | | 0.362*** | 0.320*** | 0.305** | 0.303** | 0.129 | 0.130 | 0.135 |
| | | (0.113) | (0.120) | (0.122) | (0.122) | (0.124) | (0.125) | (0.126) |
| Precipitation | | 0.001 | 0.001 | 0.001 | 0.001 | −0.002 | −0.001 | −0.001 |
| | | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Distance to River | | | −0.029 | −0.028 | −0.029 | −0.014 | −0.010 | −0.008 |
| | | | (0.022) | (0.022) | (0.022) | (0.021) | (0.022) | (0.022) |
| Distance to Coal | | | 0.006 | 0.007 | 0.007 | 0.023 | 0.029 | 0.029 |
| | | | (0.024) | (0.023) | (0.023) | (0.024) | (0.025) | (0.025) |
| Employment share industry | | | | 0.137 | 0.133 | 0.133 | 0.138 | 0.126 |
| | | | | (0.121) | (0.122) | (0.112) | (0.108) | (0.105) |
| Land inequality | | | | | 0.084 | 0.122 | 0.114 | 0.116 |
| | | | | | (0.233) | (0.220) | (0.220) | (0.217) |
| Share urban | | | | | | 0.540*** | 0.555*** | 0.531*** |
| | | | | | | (0.132) | (0.133) | (0.133) |
| Share protestant | | | | | | | 0.086 | 0.025 |
| | | | | | | | (0.091) | (0.098) |
| Share foreign | | | | | | | | −0.198* |
| | | | | | | | | (0.112) |
| Observations | 264 | 264 | 264 | 264 | 264 | 264 | 264 | 264 |
| Adjusted $R^2$ | 0.247 | 0.260 | 0.260 | 0.259 | 0.256 | 0.302 | 0.302 | 0.304 |

*Notes:* Table shows results from estimating equation 1.1 at the constituency level, adding controls separately. Dependent variable is one if constituency has at least one signee. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Column (8) is our baseline specification. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6.2: Labor unrest and bourgeois support for franchise reform

| | *Dependent variable:* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | signee indicator | | | | | | | Strike intensity | signee indicator | number signees |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | First Stage | IV | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Strike intensity | 0.045*** | 0.051*** | | | 0.038*** | 0.039*** | 0.048*** | | 0.082* | 0.136*** |
| | (0.012) | (0.017) | | | (0.012) | (0.013) | (0.013) | | (0.045) | (0.030) |
| Strike intensity*k | | | 0.055*** | | | | | | | |
| | | | (0.016) | | | | | | | |
| log(Strikers+1) | | | | 0.049*** | | | | | | |
| | | | | (0.014) | | | | | | |
| Strikes 1871-1882 | | | | | | 0.081* | | | | |
| | | | | | | (0.049) | | | | |
| Price Shock | | | | | | | | 5.478*** | | |
| | | | | | | | | (1.165) | | |
| Sum of IV shares | | | | | | | | −7.441** | −0.424 | |
| | | | | | | | | (3.466) | (0.523) | |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Employment Shares Separately | NO | NO | NO | NO | YES | NO | NO | NO | NO | |
| Kleibergen Paap F-Statistic | | | | | | | | 22.13 | | |
| Anderson Rubin CI | | | | | | | | | -0.01–0.19 | |
| Observations | 264 | 244 | 264 | 264 | 264 | 264 | 264 | 264 | 264 | 264 |
| Adjusted $R^2$ | 0.304 | 0.283 | 0.306 | 0.305 | 0.351 | 0.306 | 0.307 | 0.615 | 0.288 | 0.609 |

*Notes:* Table shows results from estimating equation 1.1 at the constituency level, with different transformations of the explanatory variable, adding additional controls, using the instrumental variable approach described in section 2.4, and using the number of signees instead of an indicator variable. Column (1) is our baseline specification. Dependent variable is one if constituency has at least one signee in columns (1) through (7) and column (9), strike intensity in column (8), and the arcsinh transformed number of signees in column (10). Strike intensity is average annual number of strikers in a constituency transformed in different ways: Using the inverse hyperbolic sine (arcsinh) in columns (1), (2), and (5) through (10); using the inverse hyperbolic sine after multiplying with the optimal scale factor of 0.167 following Aihounton and Henningsen (2021) in column (3); and using the logarithm after adding 1 in column (4). Column (5) controls for the employment share of 16 different industrial sectors separately. Column (7) excludes the largest strike in 1905. All columns control for longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, and non-German share. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6.3: Labor unrest and bourgeois support for franchise reform outside Prussia

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | signee indicator | | | | | | | |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike intensity | 0.053*** | 0.050*** | 0.052*** | 0.048*** | 0.048*** | 0.045*** | 0.045*** | 0.044*** |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) |
| Latitude | | 0.047* | 0.045 | 0.039 | 0.059* | 0.051 | 0.032 | 0.032 |
| | | (0.027) | (0.028) | (0.029) | (0.032) | (0.032) | (0.035) | (0.035) |
| Longitude | | 0.009 | 0.006 | 0.005 | 0.010 | 0.009 | 0.013 | 0.013 |
| | | (0.011) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.013) |
| Elevation | | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** |
| | | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0004) |
| Temperature | | 0.127** | 0.115* | 0.103 | 0.117* | 0.105 | 0.092 | 0.092 |
| | | (0.060) | (0.062) | (0.063) | (0.063) | (0.064) | (0.066) | (0.066) |
| Precipitation | | −0.003*** | −0.003*** | −0.003*** | −0.003*** | −0.003*** | −0.003** | −0.003** |
| | | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Distance to River | | | −0.007 | −0.006 | −0.007 | −0.002 | −0.006 | −0.006 |
| | | | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) | (0.013) |
| Distance to Coal | | | 0.011 | 0.012 | 0.015 | 0.008 | 0.009 | 0.009 |
| | | | (0.011) | (0.011) | (0.012) | (0.012) | (0.012) | (0.012) |
| Employment share industry | | | | 0.322 | 0.358 | −0.154 | −0.329 | −0.329 |
| | | | | (0.228) | (0.233) | (0.269) | (0.257) | (0.257) |
| Land inequality | | | | | −0.163* | −0.128 | −0.126 | −0.128 |
| | | | | | (0.095) | (0.094) | (0.093) | (0.099) |
| Share urban | | | | | | 0.178*** | 0.187*** | 0.187*** |
| | | | | | | (0.060) | (0.059) | (0.059) |
| Share protestant | | | | | | | 0.100** | 0.101** |
| | | | | | | | (0.044) | (0.044) |
| Share foreign | | | | | | | | 0.024 |
| | | | | | | | | (0.187) |
| Observations | 501 | 501 | 501 | 501 | 501 | 501 | 501 | 501 |
| Adjusted $R^2$ | 0.148 | 0.161 | 0.161 | 0.162 | 0.165 | 0.180 | 0.187 | 0.185 |

*Notes:* Table shows results from estimating equation 1.1 at the county level for counties outside of Prussia, excluding states with a three-class franchise (Saxony, Hamburg, and Lippe), adding controls separately. Dependent variable is one if constituency has at least one signee. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6.4: Labor unrest and bourgeois support for franchise reform outside Prussia

| | *Dependent variable:* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | signee indicator | | | | | | Strike intensity | signee indicator | number signees |
| | OLS | OLS | OLS | OLS | OLS | OLS | First Stage | IV | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Strike intensity | 0.044*** | 0.075*** | | | 0.044*** | 0.028*** | | 0.118 | 0.107*** |
| | (0.008) | (0.015) | | | (0.008) | (0.008) | | (0.281) | (0.020) |
| Strike intensity*k | | | 0.180*** | | | | | | |
| | | | (0.033) | | | | | | |
| log(Strikers+1) | | | | 0.054*** | | | | | |
| | | | | (0.010) | | | | | |
| Strikes 1871-1882 | | | | | | 0.436*** | | | |
| | | | | | | (0.110) | | | |
| Price Shock | | | | | | | 0.513 | | |
| | | | | | | | (0.869) | | |
| Sum of IV shares | | | | | | | 4.406 | −0.374 | |
| | | | | | | | (2.986) | (1.693) | |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Employment Shares Separately | NO | NO | NO | NO | YES | NO | NO | NO | NO |
| Kleibergen Paap F-Statistic | | | | | | | 0.35 | | |
| Anderson Rubin CI | | | | | | | | unbounded | |
| Observations | 501 | 278 | 501 | 501 | 501 | 501 | 501 | 501 | 501 |
| Adjusted $R^2$ | 0.185 | 0.204 | 0.234 | 0.192 | 0.192 | 0.235 | 0.346 | −0.010 | 0.212 |

*Notes:* Table shows results from estimating equation 1.1 at the county level for counties outside of Prussia, excluding states with a three-class franchise (Saxony, Hamburg, and Lippe), with different transformations of the explanatory variable, adding additional controls, using the instrumental variable approach described in section 2.4, and using the number of signees instead of an indicator variable. Column (1) is our baseline specification. Dependent variable is one if constituency has at least one signee in columns (1) through (6) and column (8), strike intensity in column (7), and the arcsinh transformed number of signees in column (9). Strike intensity is average annual number of strikers in a constituency transformed in different ways: Using the inverse hyperbolic sine (arcsinh) in columns (1), (2), and (5) through (9); using the inverse hyperbolic sine after multiplying with the optimal scale factor of 0.017 following Aihounton and Henningsen (2021) in column (3); and using the logarithm after adding 1 in column (4). Column (5) controls for the employment share of 16 different industrial sectors separately. All columns control for longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, and non-German share. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## A.7 Robustness: Franchise reform voting

In this section, we first report the robustness of our result that an increase in labor unrest in a constituency increased the likelihood of a member of parliament to vote for democratic reform in 1912. Table A.7.1 shows that this result is robust to adding controls separately. Table A.7.2 shows the robustness to different specifications (see section A.6 for a detailed description).[A.7.1] Throughout all specifications, the point estimate remains positive, and in most instances also significant. When including previous strike levels and when using the instrumental variable approach, the coefficient becomes insignificant but remains positive.

Figure A.7.1 shows the results from different ways of coding the dependent variable: Yes / Unexcused+No is our baseline and codes unexcused abstentions as No; Yes / No excludes any abstentions or absent members of parliament; Yes / Abstention / No codes 1/0/-1; Yes / Abstention / Unexcused / No codes 4/3/2/1; ; Yes / Unexcused / No codes 1/0/-1; Yes / Unexcused codes 1/0. The point estimate is always positive. However, the point estimate when considering only yea votes and unexcused votes, thus not using any variation coming from MPs who voted against the reform outright, the coefficient becomes insignificant in the OLS and the IV specification and is close to zero.

Figure A.7.2 shows the results from defining *Strike intensity* in different ways. For most specifications, the point estimate remains positive and, using OLS, also significant. The evidence suggests that strikers participating in non-political strikes were less effective in changing the MPs voting behavior.

In our main results, we showed that the positive effect of labor unrest on voting for democratic reform is driven by regions where also bourgeois support is present. Table A.7.3 shows the robustness of this result to different specifications (see section A.6 for a detailed description). In all columns, the coefficient of the interaction effect is positive and highly significant, while the coefficient of *Strike intensity* alone is mostly insignificant and close to zero.

Lastly, Table A.7.4 shows the result of estimating the effect of labor unrest of voting for franchise reform for the subsample of constituencies in which a city from our city sample is located. The idea is to understand whether the increased likelihood of voting for franchise reform is done even in regions where we were able to show that redistributive spending on the city level was increased. Since we do not observe any constituencies in this subsample without signees, we cannot perform the interaction of labor unrest and bourgeois support. The results show that the coefficient of strike intensity is positive and

[A.7.1] Out of the 375 initial observations, we only have 25 observations with zero strikes.

significant, and about four times in magnitude compared to our baseline, suggesting that redistributive spending and voting for franchise reform were not substitutes, but that labor unrest shifted elite's responses on both dimensions.

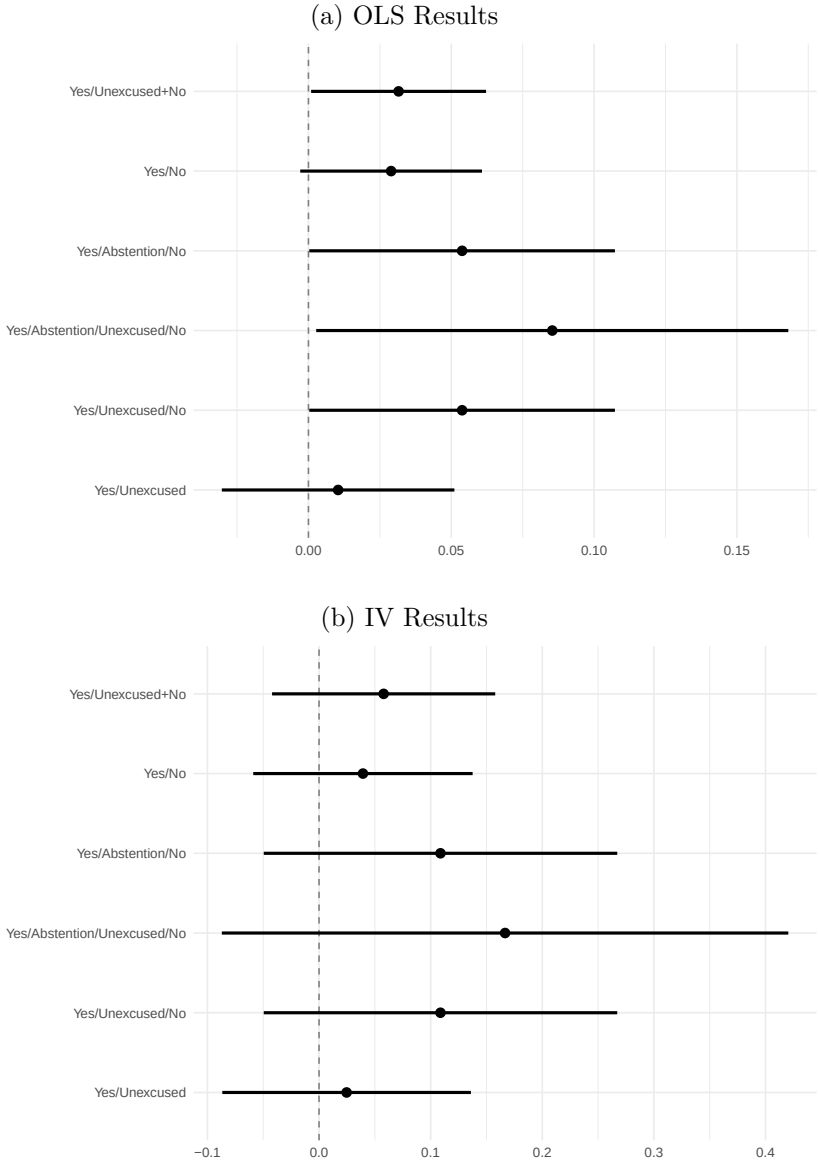



Figure A.7.1: Labor unrest and franchise reform voting, coding votes in various ways

*Notes:* Dependent variables are variations on voting on the reform bill. Yes/No excludes any abstentions or absent MPs; Yes/Abstention/No codes 1/0/-1; Yes/Abstention/Unexcused/No codes 4/3/2/1; Yes/Unexcused+No codes unexcused abstentions as No; Yes/Unexcused/No codes 1/0/-1; Yes/Unexcused codes 1/0. Explanatory variable is strike intensity, i.e., the (arcsinh transformed) average annual number of strikers in a constituency. Controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, landownership inequality, urbanization rate, non-German language share. Bars indicate 95% confidence intervals.

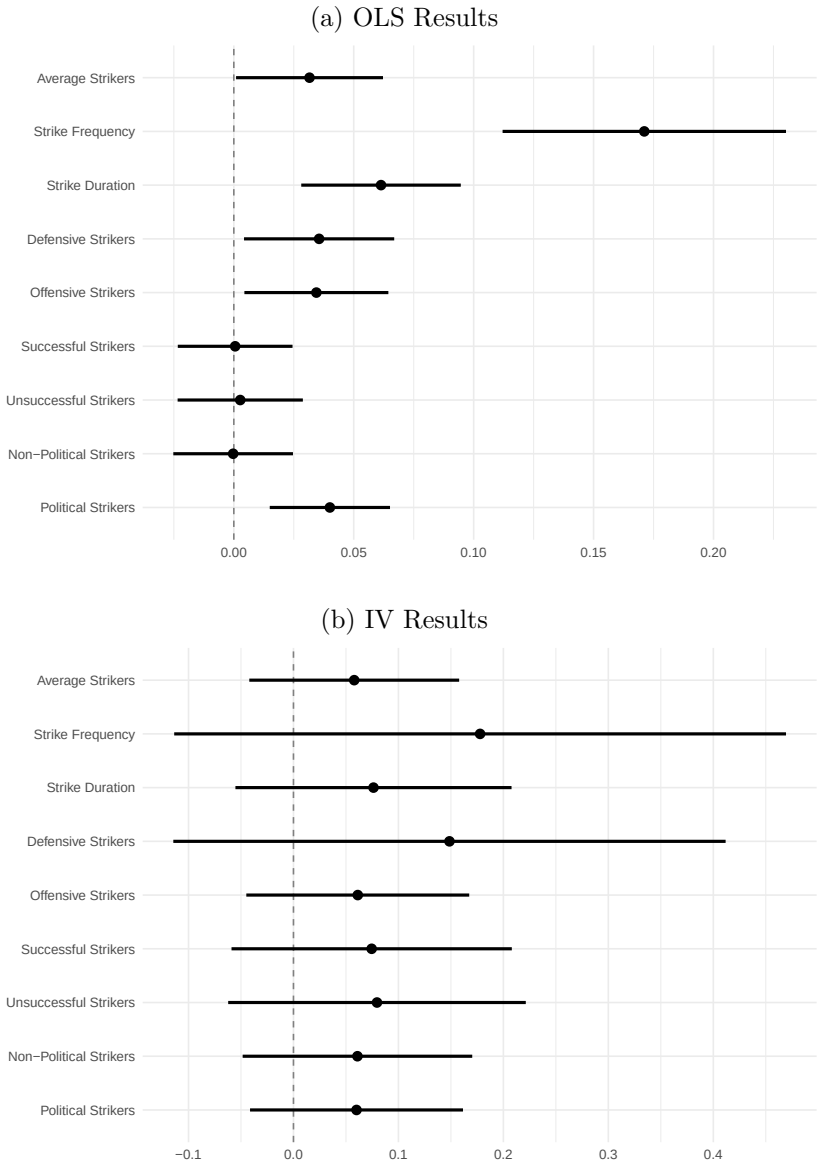



Figure A.7.2: Labor unrest and franchise reform voting, measuring strike intensity in various ways

*Notes:* Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted or is absent without excuse during vote. MPs who abstained or were absent with excuse are excluded. Explanatory variables are various constituency-level measures of strike intensity 1899–1905 (arcsinh transformed): average strikers (average annual number of strikers), strike frequency (average annual number of strikes), strike duration (average annual number of working days lost due to strikes), defensive strikers (average annual number of strikers with demands against wage decrease or working time increases), offensive strikers (average annual number of strikers with demands for wage increase or working time reductions), successful strikers (average annual number of strikers whose demands were met), unsuccessful strikers (average annual number of strikers whose demands were not met), political strikers (average annual number of strikers in wildcat strikes), non-political strikers (average annual number of strikers in union-approved strikes). Controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, landownership inequality, urbanization rate, non-German language share. Bars indicate 95% confidence intervals.

Table A.7.1: Labor unrest and roll-call voting on franchise reform: robustness

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | voting (yea/nay+unexcused) | | | | | | | |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike intensity | 0.055*** | 0.040*** | 0.039*** | 0.035** | 0.033** | 0.025 | 0.034** | 0.032** |
| | (0.011) | (0.013) | (0.014) | (0.014) | (0.014) | (0.015) | (0.014) | (0.015) |
| Latitude | | 0.088 | 0.081 | 0.076 | 0.071 | 0.053 | 0.148** | 0.141** |
| | | (0.067) | (0.069) | (0.070) | (0.070) | (0.071) | (0.072) | (0.071) |
| Longitude | | 0.024* | 0.022* | 0.019 | 0.013 | 0.007 | −0.008 | −0.019 |
| | | (0.013) | (0.013) | (0.013) | (0.014) | (0.014) | (0.014) | (0.015) |
| Elevation | | 0.002** | 0.002** | 0.002** | 0.002** | 0.002* | 0.002** | 0.002** |
| | | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Temperature | | 0.304** | 0.279** | 0.250** | 0.246** | 0.190 | 0.172 | 0.144 |
| | | (0.121) | (0.122) | (0.125) | (0.125) | (0.131) | (0.126) | (0.126) |
| Precipitation | | 0.009*** | 0.009*** | 0.008*** | 0.010*** | 0.009*** | 0.004 | 0.003 |
| | | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Distance to River | | | −0.025 | −0.022 | −0.026 | −0.023 | −0.043* | −0.045** |
| | | | (0.021) | (0.021) | (0.021) | (0.021) | (0.022) | (0.022) |
| Distance to Coal | | | 0.003 | 0.004 | 0.001 | 0.007 | −0.023 | −0.023 |
| | | | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) |
| Employment share industry | | | | 0.218** | 0.210** | 0.207* | 0.194* | 0.224* |
| | | | | (0.103) | (0.104) | (0.106) | (0.113) | (0.116) |
| Land inequality | | | | | 0.294 | 0.312* | 0.324* | 0.282 |
| | | | | | (0.184) | (0.183) | (0.179) | (0.176) |
| Share urban | | | | | | 0.183 | 0.092 | 0.166 |
| | | | | | | (0.138) | (0.137) | (0.141) |
| Share protestant | | | | | | | −0.428*** | −0.306*** |
| | | | | | | | (0.083) | (0.096) |
| Share foreign | | | | | | | | 0.365** |
| | | | | | | | | (0.147) |
| Observations | 375 | 375 | 375 | 375 | 375 | 375 | 375 | 375 |
| Adjusted $R^2$ | 0.065 | 0.162 | 0.161 | 0.164 | 0.167 | 0.170 | 0.226 | 0.241 |

*Notes:* Table shows results from estimating equation 1.1 at the MP level, adding controls separately. Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted nay or is absent without excuse during vote. MPs who abstained or were absent with excuse are excluded. *Strike intensity* is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). Standard errors, clustered at the constituency level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7.2: Labor unrest and roll-call voting on franchise reform: robustness

| | *Dependent variable:* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | voting (yea/nay+unexcused) | | | | | | | Strike intensity | voting (yea/nay+unexcused) |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | First Stage | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Strike intensity | 0.032** | 0.070*** | | | 0.032** | 0.013 | 0.032** | | 0.058 |
| | (0.015) | (0.016) | | | (0.014) | (0.014) | (0.015) | | (0.048) |
| Strike intensity*k | | | 0.133*** | | | | | | |
| | | | (0.027) | | | | | | |
| log(Strikers+1) | | | | 0.039** | | | | | |
| | | | | (0.016) | | | | | |
| Strikes 1871-1882 | | | | | | 0.303*** | | | |
| | | | | | | (0.054) | | | |
| Price Shock | | | | | | | | 5.334*** | |
| | | | | | | | | (0.932) | |
| Sum of IV shares | | | | | | | | −6.777** | −0.030 |
| | | | | | | | | (2.782) | (0.516) |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Employment Shares Separately | NO | NO | NO | NO | YES | NO | NO | NO | NO |
| Kleibergen Paap F-Statistic | | | | | | | | 32.76 | |
| Anderson Rubin CI | | | | | | | | | -0.03–0.16 |
| Observations | 375 | 350 | 375 | 375 | 375 | 375 | 375 | 375 | 375 |
| Adjusted $R^2$ | 0.241 | 0.300 | 0.274 | 0.244 | 0.287 | 0.288 | 0.241 | 0.599 | 0.231 |

*Notes:* Table shows results from estimating equation 1.1 at the MP level, with different transformations of the explanatory variable, adding additional controls, using the instrumental variable approach described in section 2.4. Column (1) is our baseline specification. Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted nay or is absent without excuse during vote in columns (1) through (7) and column (9), and strike intensity in column (8). Strike intensity is average annual number of strikers in a constituency transformed in different ways: Using the inverse hyperbolic sine (arcsinh) in columns (1), (2), and (5) through (9); using the inverse hyperbolic sine after multiplying with the optimal scale factor of 0.006 following Aihounton and Henningsen (2021) in column (3); and using the logarithm after adding 1 in column (4). Column (5) controls for the employment share of 16 different industrial sectors separately. Column (7) excludes the largest strike in 1905. All columns control for longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, and non-German share. Standard errors, clustered at the constituency level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7.3: Labor unrest and roll-call voting on franchise reform: robustness

| | *Dependent variable:* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | voting (yea/nay+unexcused) | | | | | | | Strike intensity | Strike intensity x Bourgeois support | voting (yea/nay+unexcused) |
| | OLS | OLS | OLS | OLS | OLS | OLS | OLS | First Stage | First Stage | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Strike intensity | 0.008 | 0.037** | | | 0.012 | 0.002 | 0.008 | | | 0.053 |
| | (0.015) | (0.017) | | | (0.015) | (0.015) | (0.015) | | | (0.049) |
| Strike intensity x Bourgeois support | 0.432*** | 0.327*** | | | 0.370*** | 0.276*** | 0.433*** | | | 0.660*** |
| | (0.084) | (0.088) | | | (0.087) | (0.102) | (0.085) | | | (0.162) |
| Strike intensity*k | | | 0.026 | | | | | | | |
| | | | (0.020) | | | | | | | |
| log(Strikers+1) | | | | 0.011 | | | | | | |
| | | | | (0.016) | | | | | | |
| Strike intensity*k x Bourgeois support | | | 0.249*** | | | | | | | |
| | | | (0.059) | | | | | | | |
| log(Strikers+1) x Bourgeois support | | | | 0.389*** | | | | | | |
| | | | | (0.078) | | | | | | |
| Price Shock x Bourgeois support | | | | | | | | | 0.769*** | |
| | | | | | | | | | (0.074) | |
| Bourgeois support | −0.578*** | −0.406*** | −0.185** | −0.485*** | −0.476*** | −0.328* | −0.579*** | 0.545*** | 0.288* | −1.001*** |
| | (0.144) | (0.150) | (0.088) | (0.130) | (0.150) | (0.169) | (0.144) | (0.163) | (0.148) | (0.283) |
| Strikes 1871-1882 | | | | | | 0.199*** | | | | |
| | | | | | | (0.066) | | | | |
| Price Shock | | | | | | | | 5.050*** | | |
| | | | | | | | | (0.931) | | |
| Sum of IV shares | | | | | | | | −6.086** | −0.103 | −0.243 |
| | | | | | | | | (2.776) | (0.146) | (0.528) |
| Geographic Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Development Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Employment Shares Separately | NO | NO | NO | NO | YES | NO | NO | NO | NO | NO |
| Kleibergen Paap F-Statistic | | | | | | | | 29.44 | 109.11 | |
| Observations | 375 | 350 | 375 | 375 | 375 | 375 | 375 | 375 | 375 | 375 |
| Adjusted $R^2$ | 0.295 | 0.332 | 0.297 | 0.295 | 0.324 | 0.309 | 0.295 | 0.607 | 0.959 | 0.251 |

*Notes:* Table shows results from estimating equation 1.1 at the MP level, always adding an indicator for Bourgeois support and its interaction with the strike measure, and using different transformations of the explanatory variable, adding additional controls, using the instrumental variable approach described in section 2.4. Column (1) is our baseline specification. Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted nay or is absent without excuse during vote in columns (1) through (7) and column (10), strike intensity in column (8), and strike intensity x bourgeois support in column (9). Strike intensity is average annual number of strikers in a constituency transformed in different ways: Using the inverse hyperbolic sine (arcsinh) in columns (1), (2), and (5) through (10); using the inverse hyperbolic sine after multiplying with the optimal scale factor of 0.06 following Aihounton and Henningsen (2021) in column (3); and using the logarithm after adding 1 in column (4). Column (5) controls for the employment share of 16 different industrial sectors separately. Column (7) excludes the largest strike in 1905. All columns control for longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, urbanization rate, landownership inequality, and non-German share. Standard errors, clustered at the constituency level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7.4: Labor unrest and franchise reform voting: OLS, city sample

| | *Dependent variable:* |
|---|---|
| | Voting (Yes/Unexcused+No) |
| | *OLS* |
| Strike intensity | 0.127*** |
| | (0.043) |
| Geographic Controls | YES |
| Industry Controls | YES |
| Observations | 83 |
| Adjusted $R^2$ | 0.333 |

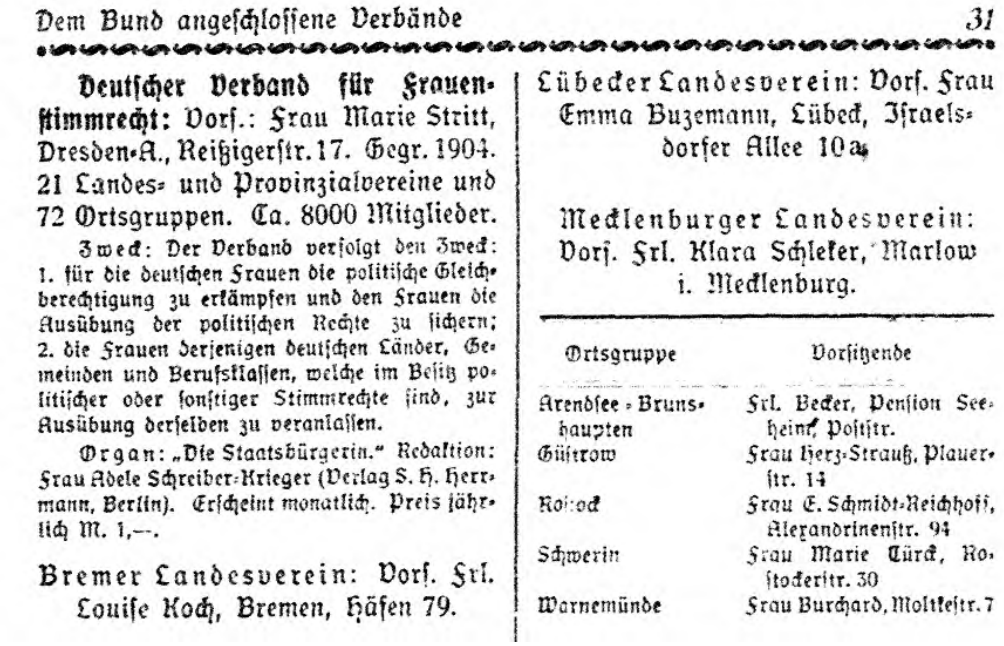
*Notes:* Table shows results from estimating equation 1.1 at the MP level, restricting the sample to members of parliament from constituencies in which one of the cities from our city sample (with more than 25,000 inhabitants) is located. Dependent variable is one if an MP voted yea on the reform bill and zero if an MP voted nay or is absent without excuse during vote. Strike intensity is average annual number of strikers in a constituency transformed using the inverse hyperbolic sine (arcsinh). We do not observe any constituencies in this subsample without signees, thus we cannot perform the interaction of labor unrest and bourgeois support. Controls: longitude, latitude, elevation, temperature, precipitation, distance to navigable river, distance to coal, industrial employment share, Protestant share, landownership inequality, urbanization rate, non-German language share. Standard errors, clustered at the constituency level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

# Appendix B

# Appendix to Chapter 2

## B.1 Supplementary figures and tables

### B.1.1 Supplementary figures



Dem Bund angeschlossene Verbände 31

**Deutscher Verband für Frauenstimmrecht:** Vors.: Frau Marie Stritt, Dresden-A., Reißigerstr. 17. Gegr. 1904. 21 Landes- und Provinzialvereine und 72 Ortsgruppen. Ca. 8000 Mitglieder.

Zweck: Der Verband verfolgt den Zweck: 1. für die deutschen Frauen die politische Gleichberechtigung zu erkämpfen und den Frauen die Ausübung der politischen Rechte zu sichern; 2. die Frauen derjenigen deutschen Länder, Gemeinden und Berufsklassen, welche im Besitz politischer oder sonstiger Stimmrechte sind, zur Ausübung derselben zu veranlassen.

Organ: „Die Staatsbürgerin." Redaktion: Frau Adele Schreiber-Krieger (Verlag S. H. Herrmann, Berlin). Erscheint monatlich. Preis jährlich M. 1,—.

Bremer Landesverein: Vors. Frl. Louise Koch, Bremen, Häfen 79.

Lübecker Landesverein: Vors. Frau Emma Buzemann, Lübeck, Israelsdorfer Allee 10a.

Mecklenburger Landesverein: Vors. Frl. Klara Schleker, Marlow i. Mecklenburg.

| Ortsgruppe | Vorsitzende |
|---|---|
| Arendsee-Brunshaupten | Frl. Becker, Pension Seeheim, Poststr. |
| Güstrow | Frau Herz-Strauß, Plauerstr. 14 |
| Rostock | Frau E. Schmidt-Reichhoff, Alexandrinenstr. 94 |
| Schwerin | Frau Marie Türck, Rostockerstr. 30 |
| Warnemünde | Frau Burchard, Moltkestr. 7 |

Figure B.1.1: Data extract: Suffragette clubs

*Notes:* The figure shows an extract of the BDF yearbook from 1916 on the location of suffragette clubs.

*Source:* Bund Deutscher Frauenvereine (1912–1918).

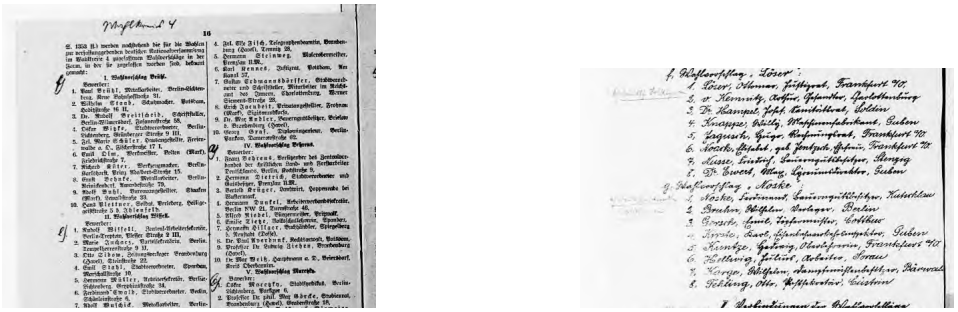


Figure B.1.2: Data extract: Candidates

*Notes:* The figure shows extracts of the list of all running candidates for the 1919 election of the Weimar Republic's national assembly.

*Source:* Reichsamt des Innern (1919).

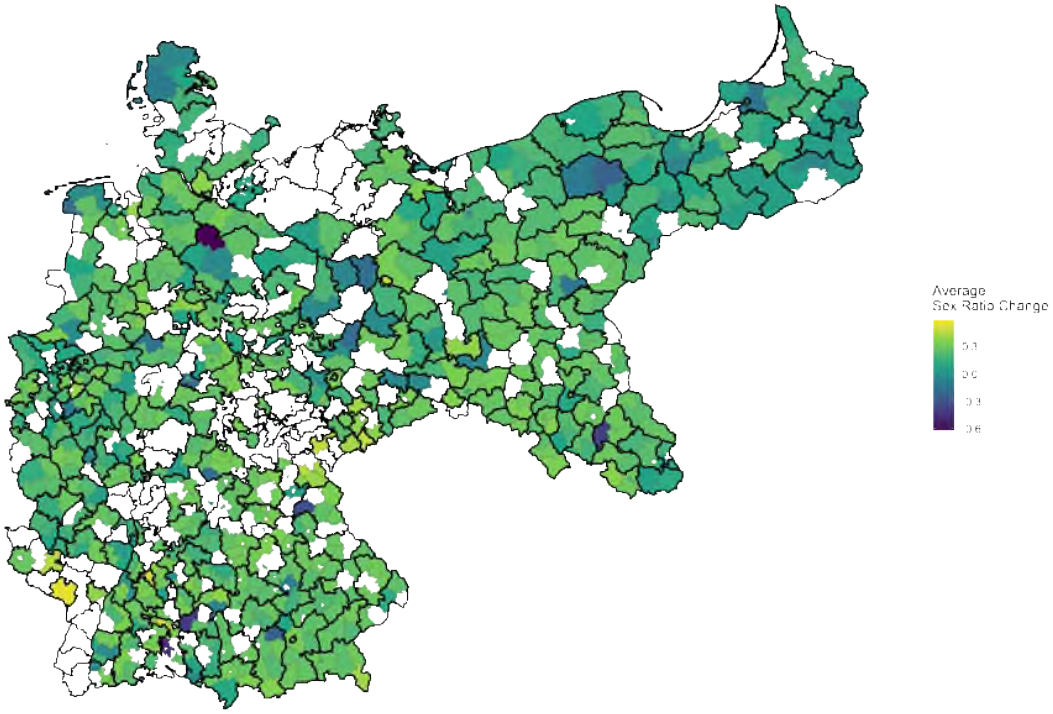
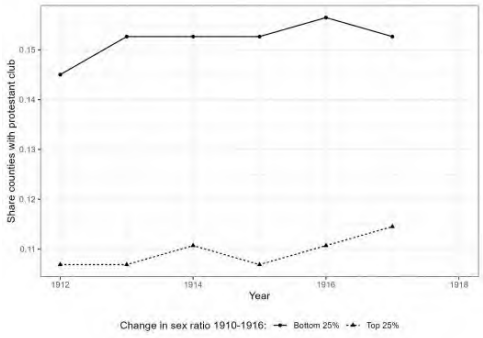
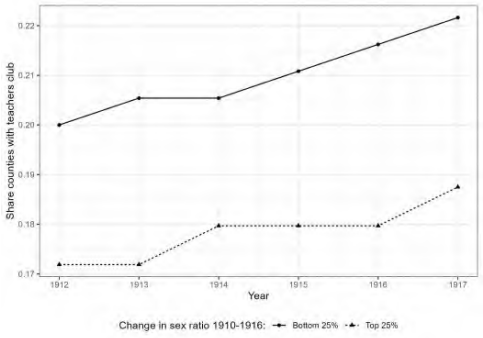



Figure B.1.3: Map of the missing men and recruitment areas
*Notes:* The map shows the average change of the female-to-male ratio from before the war (1910) to during the war (1916) in German counties in the borders of 1907. The bold black borders are the borders of recruitment areas (*Landwehrbezirke*). The sample only includes neighbouring counties which lie on opposite sides of a border of a recruitment area, restricting to neighbouring pairs with the longest border of at least 2km.
*Source:* Own illustration based on Kaiserliches Statistisches Amt (1915); Kriegsernährungsamt (1916); Reichsamt des Innern (1914).




(a) Share of counties with a protestant club by intensity of missing men




(b) Share of counties with a teachers' club by intensity of missing men

Figure B.1.4: The evolution of other women's clubs over time

*Notes:* The figure shows the evolution of the share of counties with a women's club for counties which experienced the least changes in the sex ratio between 1910 and 1916 (bottom 25% of distribution, solid line) and for those which experienced the most change (top 25%, dashed line). Part (a) shows the evolution of the German Protestant Women's Association, Part (b) shwos the evolution of the General German Women Teachers' Association.

*Source:* Own illustration. See Table 2.1 for data sources.

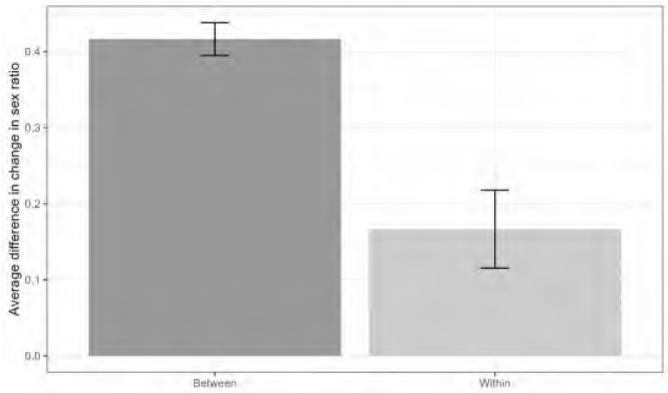



Figure B.1.5: The average difference in treatment indicator

*Notes:* The figure shows the average difference in the treatment indicator, one for counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated), and zero for counties which experienced very little change (bottom 25%) between neighbouring county pairs that share a border of at least two kilometres, restricting the sample to the neighbouring county pair with the longest border. The dark gray bar shows the average difference for county pairs laying on opposite sides of a recruitment area border, and the light gray bar shows the average difference for county pairs laying within the same recruitment area.

*Source:* Own illustration based on Kaiserliches Statistisches Amt (1915); Kriegsernährungsamt (1916); Reichsamt des Innern (1914)

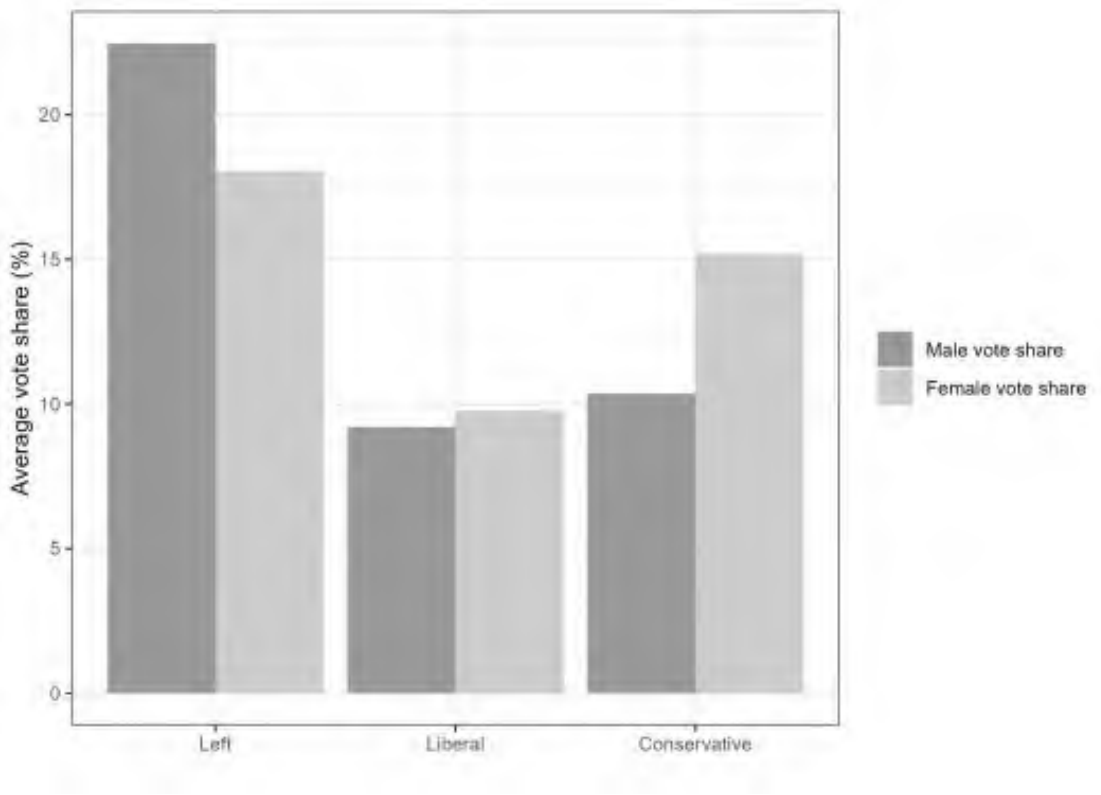



Figure B.1.6: Average voting behaviour 1919–1930

*Notes:* The figure shows the average vote shares of left, liberal and conservative parties in all elections of 1919–1930 for which votes were separately counted by gender and by party. Left parties are the Social Democrats (*Sozialdemokratische Partei Deutschlands*) and the Communist Party of Germany (*Kommunistische Partei Deutschlands*); liberal parties are the German People's Party (*Deutsche Volkspartei*) and the German Democratic Party (*Deutsche Demokratische Partei*); and conservative parties are the German National People's Party (*Deutschnationale Volkspartei*) and the Centre Party (*Zentrum*).

*Source:* Own illustration based on Bremme (1956).

## B.1.2 Supplementary tables

Table B.1.1: The effect of missing men and female labour force participation on suffragette clubs

| | (1) Baseline | (2) Low FLP change | (3) High FLP change | (4) Baseline | (5) Low FLP change | (6) High FLP change |
|---|---|---|---|---|---|---|
| Top 25% sex ratio change × post | 0.028* | 0.012 | 0.030* | 0.022 | 0.009 | 0.040 |
| | (0.017) | (0.049) | (0.017) | (0.017) | (0.026) | (0.026) |
| Controls × year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.869 | 0.901 | 0.866 | 0.879 | 0.863 | 0.895 |
| N | 3840 | 792 | 3048 | 3648 | 1728 | 1920 |

*Notes:* The table shows the underlying regressions of Figure 2.10. In columns (1) and (4), we replicate the baseline results of equation 2.2 for the subsample of counties for which we have employment data. In columns (2) and (3) and (5) and (6) we compare the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%), separately for counties with a high increase in female labour force participation (above median) and counties with a low increase in female labour force participation (below median), once between 1913 and 1918 (columns (2) and (3)) and once between 1907 and 1925 (columns (5) and (6)). All regressions control for year and county fixed effects, the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level, and additionally at the regional level for regressions using the employment data from 1913 to 1918. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

Table B.1.2: The effect of missing men and National Women's Service participation on suffragette clubs

| | (1) Baseline | (2) Without NFD | (3) With NFD |
|---|---|---|---|
| Top 25% sex ratio change × post | 0.038** | 0.004 | 0.124*** |
| | (0.016) | (0.010) | (0.047) |
| Controls × year FE | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| $R^2$ | 0.893 | 0.874 | 0.891 |
| N | 3549 | 2387 | 1162 |

*Notes:* The table shows the underlying regression of Figure 2.11, where we compare the evolution of local suffragette clubs (binary definition) in counties which experienced a large increase in the sex ratio from 1910 to 1916 (top 25%, treated) to that in counties which experienced very little change (bottom 25%), separately for counties with a local chapter of the NFD and without one. All regressions control for year and county fixed effects, the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, the presence of pre-war clubs, the pre-war sex ratio, the share of urban population, the share of protestants, and the share of foreign population in 1900, all interacted with year fixed effects. Standard errors are clustered at the recruitment area level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* See Table 2.1.

## B.2 Bounding the cross-sectional analysis

We follow the bounding approach by Oster (2019) to gauge to what extent unobserved confounding factors could reduce or increase the estimated effects of missing men on local suffragette clubs during WWI, assuming strong unobservable and non-random determinants of sex ratio changes. The approach estimates bounds from the difference between the effect when estimated unconditionally ($\dot{\beta}$) to the estimate obtained when conditioning ($\widetilde{\beta}$) on observed characteristics, i.e. 0.024 when only using state FEs (column (1) of Table B.2.1) and 0.094 when including the full set of controls (column (3) in Table 2.6 in the main text, displayed again for convenience in column (2) of Table B.2.1). This difference is scaled by movements in the associated $R^2$ values.

Oster (2019) suggests a simplified formula to compute approximate bounds $\beta^*$ around the conditional estimate of the effect of missing men $\widetilde{\beta}$ as follows:

$$\beta^* \approx \widetilde{\beta} - d\left(\dot{\beta} - \widetilde{\beta}\right)\frac{R^{max} - \widetilde{R}}{\widetilde{R} - \dot{R}}, \tag{B.1}$$

where $\dot{R}$ and $\widetilde{R}$ denote the $R^2$ from the unconditional and conditional regression (i.e. 0.092 vs. 0.716). $R^{max}$ is set by the researcher and determines to what extent observed and unobserved factors combined can explain the overall variation in the likelihood to keep a suffragette club open during the war. We set a value of $R^{max} = 1.3\widetilde{R}$ as suggested by Oster (2019) who argues that with a higher $R^{max}$ estimates in more than 10% of cases in a set of well-published Randomised Control Studies would appear unstable (null effect cannot be rejected or bound outside the 99.5% confidence interval).

The parameter $d$ governs the degree of proportionality of selection on observables to selection on unobservables and is set by the researcher. Values of 1 and -1 compute bounds for scenarios in which selection on unobservables is as strong as selection on observables and operates in the same or opposite direction as selection on observables. In our case this means that we compute bounds for omitted local variables that are correlated both with missing men and the local presence of a suffragette club and which determine suffragette club presence during the war to the same extent as our extensive set of controls, including pre-war club presence, sex ratio prior to the war and pre-war industry and age structure. Thus, a choice of $d = 1$ is a conservative assumption in our setting.

Columns (3) and (4) of Table B.2.1 present the results of the bounding analysis based on such a choice of $d = 1$ and $d = -1$, respectively. The estimated bounds lie within the 95% confidence interval of the controlled estimate and are sizeable in magnitude. Even the

lower bound estimate of 0.07 indicates that a mean sex ratio change of 0.215 results in a 1.5 percentage point higher likelihood to keep a local suffragette club open during the war (15% of the baseline probability to have a local suffragette club)—and this is for rather conservative assumptions on the degree of unobservable non-random determinants of sex ratio changes. Thus, we can rule out that plausible values of non-random determinants of sex ratio changes fully drive our main results.

Table B.2.1: Coefficient bounds under unobserved non-random sex ratio changes

| | (1) Restricted model | (2) Controlled model | (3) Bound for $\delta = 1$ | (4) Bound for $\delta = -1$ |
|---|---|---|---|---|
| Estimate | 0.024 | 0.094 | 0.141 | 0.06 |
| 95% CI | (-0.110, 0.157) | (0.010, 0.178) | | |
| $R^2$ | [0.092] | [0.716] | | |

*Notes:* Columns (1) and (2) show the coefficient estimates and 95% confidence intervals of the coefficient estimate on the change in the sex ratio from 1910 to 1916 in the cross-sectional regression in equation 2.4 as well as the associated R-squared of the respective regressions. The dependent variable is the presence of a suffragette club during WWI. Estimates in column (1) only control for state FEs. Estimates in column (2) additionally control the share of men employed in industry and military in 1907, the share of men between 12 and 18 in 1910, and the share of urban population, the share of protestants, and the share of foreign population in 1900, as in columns (3) of Table 2.6. Columns (3) and (4) report the upper and lower bounds of the effect of missing men when the maximum R-squared that could be obtained by including unobserved characteristics is set to $R^{max} = 1.3\widetilde{R}$, for selection of high sex-ratio-change regions the same (column (3)) and opposite (column (4)) direction as selection on observables. Confidence intervals are computed based on standard errors clustered clustered at the recruitment area level. *Source:* See Table 2.1.

# Appendix C

# Appendix to Chapter 3

## C.1 Data

### C.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 Jaro-Winkler (ABE-JW) Algorithm (Abramitzky et al., 2021), and adjust their method according to my data availability. The ABE-JW method uses variables that are unlikely to change over time, namely a person's place of birth, name, and age. To reduce computational requirements, only individuals with the same first letters of the first and last name, the same place of birth, and an age difference of up to 5 years are compared (so-called blocking). Only ca. 40% have information on their place of birth. Because of this, I cannot reasonably block on the place of birth without not linking a majority of the observations. In contrast, I do know the exact date of birth the observations instead of only their self-reported age as in the census data for which the ABE-JW method was derived. This alleviates the issues connected to only knowing individuals' ages, such as rounding of reported age and differences in age at different points in time of reporting. Therefore, I block on the date of birth and on the first letters of the first and last name instead. Following ABE-JW, for each of the possible matches within a block, I then calculate the string distance of the first name, last name, and place

of birth where available using the Jaro-Winkler (JW) string distance and restrict links to individuals for whom all available JW distances are less than or equal to 0.1.[C.1.1][C.1.2] The ABE-JW method links two datasets where every individual only shows up once in each dataset, so a possible match is only linked if it is unique, and there are not multiple entries that are close to the original. In my case, however, I am linking observations to other entries from the same dataset, and links to multiple entries are plausible because a person may show up more than twice in the archival records. I therefore do not restrict links to only those entries which have only one plausible match. I then treat all linked individuals as only one observation going forward. Of the originally 594,967 observations, my algorithm links 121,561 observations to another entry, leaving 473,406 observations of probably unique individuals.

Since the archival data does not include the gender of the individuals, I use the data on first names and combine this with information on name frequency based on gender from the Corpus of First Names in the Netherlands published by Meertens Instituut (2025). Of the 34,831 unique first names in my dataset, 11,802 (33.9%) are part of the Corpus of First Names. To include names with slightly different spellings, I calculate the JW-distance between first names and assign the same gender to a name with a sufficiently similar name that is part of the Corpus of First Names (a JW-string distance of up to 0.1, as suggested by Abramitzky et al. (2021)). This yields an addition of 17,417 names. In total, I can assign a gender probability to 29,219 or 83.9% of all unique first names. Based on this, I calculate the probability of a given name to be male or female and classify names for which at least 70% of individuals with that name are either male or female respectively. All other names are classified as uncertain. 78.9% of all unique first names can be classified as either male of female using the cutoff of 70%. Since some persons have multiple first names (either because they have a middle name, or because two observations with differently spelled names were linked to the same individual), I use the mode of each person's first names' genders to assign their gender.[C.1.3] In total, I can assign a gender to 93.7% of individuals in the archival records. For the relevant cohorts of 1922 and 1924, there are 10.7% female and 84.2% male individuals.

To link the places of birth recorded in the archival records to the municipalities of the 1971 census, I use a fuzzy merge and complement it with a list of over 3,500 hand-coded

[C.1.1] The place of birth is reported for 30.5% of all entries.

[C.1.2] Following ABE-JW, I use a weight of 0.1, which puts more weight on the first character of a string.

[C.1.3] 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.

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

## C.2 Replication of main results

In section 3.4.3.1, I use Eurobarometer survey data to test for effects of forced labor conscription on mental health. To ensure that this approach is valid, I replicate the main findings with this dataset. I define the following treatment and control group, consisting of one age group respectively: The older control group consists of men born in 1920 or 1921, the older treatment group are men born in 1922 or 1923.

I estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \lambda_t + \epsilon_{it} \tag{C.1}$$

where $\lambda_t$ are wave fixed effects to control for differences in survey design between different waves. The outcomes of interest $Y_{it}$ are employment status and income measured in 12 bins. Figure C.2.1 shows the results. While I find no significant differences in income, I again find that individuals in the treatment group are 5.1% less likely to participate in the labor market which is 10.4% of one standard deviation. Figure C.2.2 shows the results for family formation (a dummy for being married and a dummy for having children), where I find no significant differences.

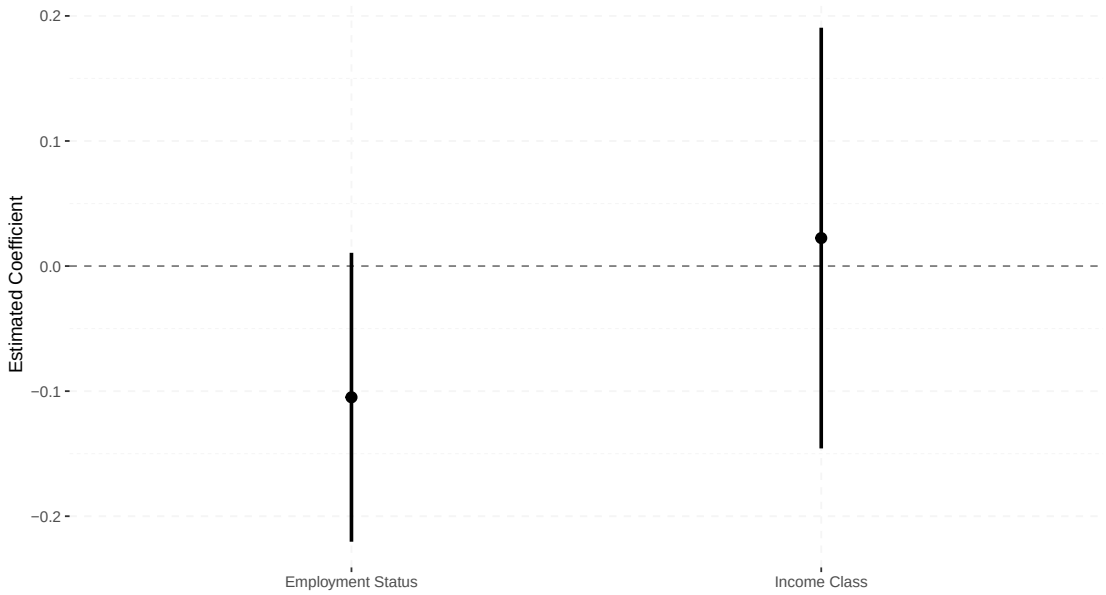



Figure C.2.1: Effects of forced labor conscription on labor market outcomes using 1975-1944 Eurobarometer

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

## C.3 Supplementary figures

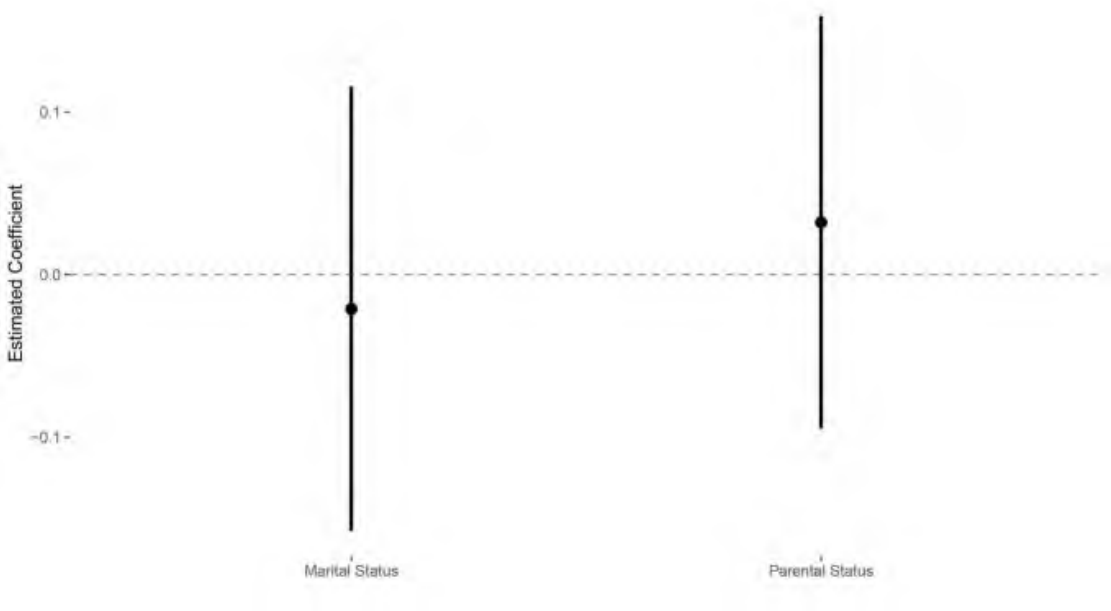



Figure C.2.2: Effects of forced labor conscription on family formation using 1975-1944 Eurobarometer

*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.

## C.4 Supplementary tables

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

Table C.4.1: RDD effects of forced labor conscription on labor market outcomes

| | *Dependent variable:* | |
|---|---|---|
| | Employment Status | Income Bin |
| | (1) | (2) |
| RDD Estimate | $-0.007^{***}$ | $-0.029^{**}$ |
| | (0.002) | (0.013) |
| Observations | 151056 | 141064 |
| Bandwidth | 15 months | 15 months |
| Dependent Variable Range | Employed: Yes/No | 0-5 (4,000 Dutch guilder range) |
| Mean Dependent Variable | 0.941 | 2.918 |

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

Table C.4.2: Robustness of RDD effects of forced labor conscription on employment

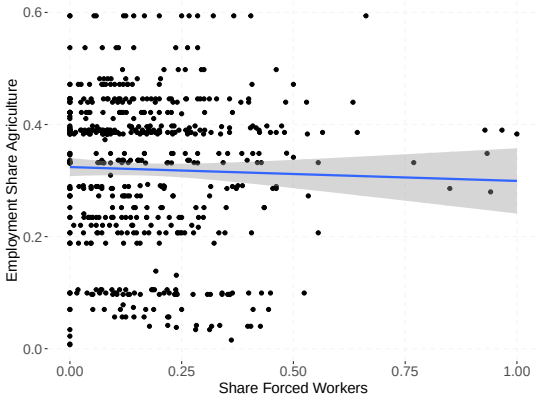
| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Employment Status | | | | | | | |
| | Baseline | Linear | Running Var x Treat | Half BW | Double BW | Uniform Kernel | Logit | Incl. Hunger Winter Regions |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| RDD Estimate | $-0.007^{***}$ | $-0.007^{***}$ | $-0.007^{***}$ | $-0.006^{*}$ | $-0.006^{***}$ | $-0.005^{***}$ | $-0.090^{***}$ | $-0.005^{**}$ |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.002) | (0.028) | (0.002) |
| Observations | 151056 | 151056 | 151056 | 70447 | 294139 | 356624 | 356613 | 174386 |
| Bandwidth | 15 months | 15 months | 15 months | 30 months | 7.5 months | 15 months | 15 months | 15 months |

*Notes:* $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01. The table shows regression results based on the 1971 Census estimating a RDD with different specifications. Employment status is a dummy variable for a person being employed.

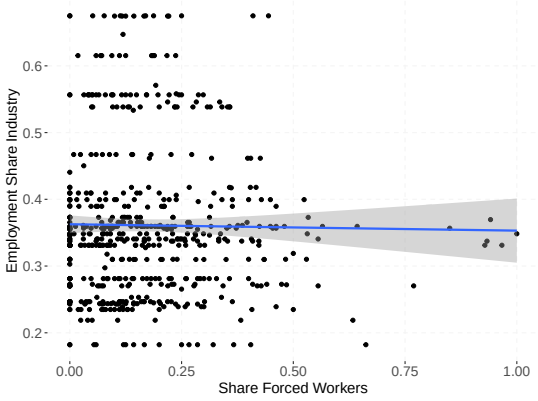
Table C.4.3: Robustness of RDD effects of forced labor conscription on income

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | Income Bin | | | | | | |
| | Baseline | Linear | Running Var x Treat | Half BW | Double BW | Uniform Kernel | Incl. Hunger Winter Regions |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| RDD Estimate | $-0.029^{**}$ | $-0.029^{**}$ | $-0.029^{**}$ | $-0.013$ | $-0.033^{***}$ | $-0.025^{***}$ | $-0.020^{*}$ |
| | (0.013) | (0.013) | (0.013) | (0.019) | (0.009) | (0.009) | (0.012) |
| Observations | 141064 | 141064 | 141064 | 65787 | 274683 | 333035 | 162851 |
| Bandwidth | 15 months | 15 months | 15 months | 30 months | 7.5 months | 15 months | 15 months |

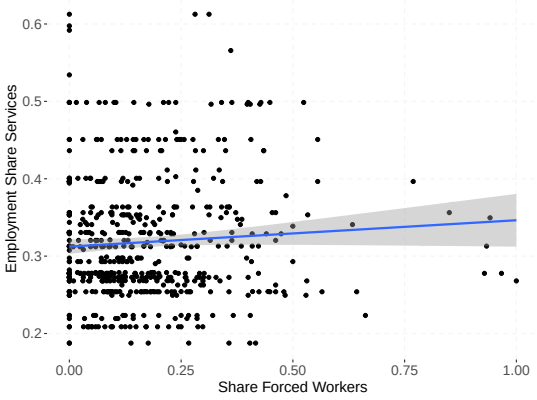
*Notes:* $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01. The table shows regression results based on the 1971 Census estimating a RDD with different specifications. Income bin is measured from 0 to 5 in steps of 4,000 Dutch Guilder.




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




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




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

Figure C.3.1: Correlation of forced labor intensity and industry composition

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

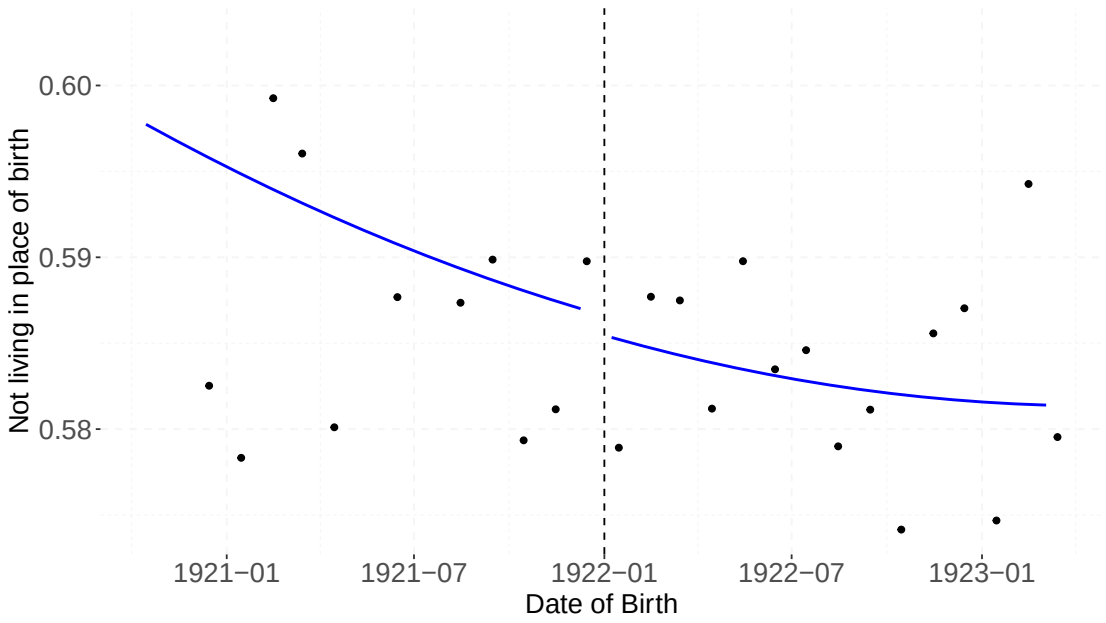



Figure C.3.2: RDD Effects of forced labor conscription on moving based on 1971 Census
Estimate: $\beta_1$ = -0.0012 (0.0044)

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

Table C.4.4: Placebo RDD effects of forced labor conscription on employment

| | *Dependent variable:* | | | | |
|---|---|---|---|---|---|
| | Employment Status | | | | |
| | Baseline | Cutoff 31.12.1915 | Cutoff 31.12.1916 | Cutoff 31.12.1917 | Cutoff 31.12.1918 |
| | (1) | (2) | (3) | (4) | (5) |
| RDD Estimate | −0.007*** | −0.0001 | −0.004 | −0.002 | −0.001 |
| | (0.002) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 151056 | 124770 | 126116 | 125550 | 130968 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.5: Placebo RDD effects of forced labor conscription on income

| | *Dependent variable:* | | | | |
|---|---|---|---|---|---|
| | Income Bin | | | | |
| | Baseline | Cutoff 31.12.1915 | Cutoff 31.12.1916 | Cutoff 31.12.1917 | Cutoff 31.12.1918 |
| | (1) | (2) | (3) | (4) | (5) |
| RDD Estimate | −0.029** | 0.012 | 0.006 | 0.003 | 0.001 |
| | (0.013) | (0.015) | (0.015) | (0.015) | (0.014) |
| Observations | 141064 | 116517 | 117774 | 117245 | 122305 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.6: RDD effects of forced labor conscription on education

| | *Dependent variable:* |
|---|---|
| | Educational Attainment |
| RDD Estimate | −0.006* |
| | (0.004) |
| Observations | 125016 |
| Bandwidth | 15 months |
| Dependent Variable Range | Finished Secondary Education: Yes/No |
| Mean Dependent Variable | 0.188 |

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

Table C.4.7: RDD effects of forced labor conscription on family formation

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

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

Table C.4.8: RDD effects of forced labor conscription on not living in place of birth

| | *Dependent variable:* |
|---|---|
| | Not living in place of birth |
| RDD Estimate | −0.001 |
| | (0.004) |
| Observations | 151080 |
| Bandwidth | 15 months |
| Dependent Variable Range | Married: Yes/No |
| Mean Dependent Variable | 0.908 |

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

Table C.4.9: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by share of forced workers

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Employment Status | | | Income bin | | |
| | Baseline | More Hiding | More Forced Work | Baseline | More Hiding | More Forced Work |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RDD Estimate | −0.007** | −0.010* | −0.007* | −0.003 | −0.003 | −0.001 |
| | (0.003) | (0.006) | (0.004) | (0.018) | (0.032) | (0.022) |
| Observations | 62309 | 18873 | 43436 | 57557 | 16994 | 40563 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.10: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience: Share of damaged housing

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Employment Status | | | Income bin | | |
| | Baseline | Low War Exposure | High War Exposure | Baseline | Low War Exposure | High War Exposure |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RDD Estimate | −0.007* | 0.001 | −0.013*** | −0.003 | −0.004 | −0.015 |
| | (0.004) | (0.005) | (0.005) | (0.018) | (0.028) | (0.026) |
| Observations | 56772 | 25141 | 31631 | 57557 | 23478 | 29539 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.11: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by adversity of forced labor experience: Distance to labor education camps

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Employment Status | | | Income bin | | |
| | Baseline | Low Camp Exposure | High Camp Exposure | Baseline | Low Camp Exposure | High Camp Exposure |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RDD Estimate | −0.006* | 0.010** | −0.022*** | −0.003 | 0.014 | −0.021 |
| | (0.004) | (0.005) | (0.005) | (0.019) | (0.026) | (0.027) |
| Observations | 58361 | 29111 | 29250 | 54501 | 27185 | 27316 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.12: Heterogeneous RDD effects of forced labor conscription on need for assistance

| | *Dependent variable:* | | | | | | |
|---|---|---|---|---|---|---|---|
| | Need for Assistance | | | | | | |
| | Baseline | More Hiding | More Forced Work | Low War Exposure | High War Exposure | Low Camp Exposure | High Camp Exposure |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| RDD Estimate | −0.001 | −0.007 | 0.002 | −0.003 | 0.005 | −0.008** | 0.009** |
| | (0.003) | (0.005) | (0.003) | (0.004) | (0.004) | (0.004) | (0.004) |
| Observations | 62319 | 18201 | 43443 | 25145 | 31636 | 29115 | 29255 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.13: Effects of forced labor conscription on life satisfaction using 1975-1944 Eurobarometer

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

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

Table C.4.14: Heterogeneous RDD effects of forced labor conscription on labor market outcomes by similarity of sectoral composition

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Employment Status | | | Income bin | | |
| | Baseline | Different Sectors | Similar Sectors | Baseline | Different Sectors | Similar Sectors |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| RDD Estimate | −0.006* | −0.010* | −0.003 | −0.003 | 0.017 | −0.022 |
| | (0.004) | (0.006) | (0.005) | (0.019) | (0.028) | (0.025) |
| Observations | 58361 | 26171 | 32190 | 54501 | 24440 | 30061 |
| Bandwidth | 15 months | 15 months | 15 months | 15 months | 15 months | 15 months |

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

Table C.4.15: Effects of forced labor conscription on labor market outcomes using 1975-1944 Eurobarometer

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

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

Table C.4.16: Effects of forced labor conscription on family formation using 1975-1944 Eurobarometer

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

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

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**Note**: All URLs were last accessed 2025-06-14.

# Appendix D

# Software

The following specialist software was used in the preparation of this thesis:

- LaTeX(Lamport, 1986)
- `Overleaf` (Hammersley and Lees-Miller, 2025)
- `QGIS` (QGIS Development Team, 2023)
- R 4.3.3 (R Core Team, 2024), *with packages*:
  - `boot` (A. C. Davison and D. V. Hinkley, 1997; Angelo Canty and B. D. Ripley, 2024)
  - `broom` (Robinson, Hayes, and Couch, 2025)
  - `dplyr` (Wickham, François, et al., 2023)
  - `doParallel` (Corporation and Weston, 2025)
  - `expss` (Demin, 2025)
  - `fastDummies` (Kaplan, 2025)
  - `fixest` (Bergé, 2018)
  - `foreign` (R Core Team, 2025)
  - `foreach` (Microsoft and Weston, 2025)
  - `fs` (Hester, Wickham, and Csárdi, 2025)
  - `geonames` (Rowlingson, 2025)
  - `geosphere` (Hijmans, 2024)
  - `ggplot2` (Wickham, 2016)

- `haven` (Wickham, Miller, and Smith, 2023)
- `Hmisc` (Harrell Jr, 2025)
- `htmltools` (Cheng et al., 2025)
- `httr` (Wickham, 2025)
- `ivmodel` (Kang et al., 2023)
- `ivpack` (Baiocchi, Cheng, and Small, 2014)
- `kableextra` (Zhu, 2024)
- `lme4` (Bates et al., 2015)
- `lubridate` (Grolemund and Wickham, 2011)
- `magrittr` (Bache and Wickham, 2022)
- `openxlsx` (Schauberger and Walker, 2025)
- `plyr` (Wickham, 2011)
- `processx` (Csárdi and Chang, 2025)
- `purrr` (Wickham and Henry, 2025)
- `raster` (Hijmans, 2025a)
- `rdd` (Dimmery, 2016)
- `readstata13` (Garbuszus and Jeworutzki, 2025)
- `readxl` (Wickham and Bryan, 2025)
- `robomit` (Schaub, 2021)
- `rvest` (Wickham, 2024)
- `sandwich` (Zeileis, 2004, 2006; Zeileis, Köll, and Graham, 2020)
- `sf` (Pebesma, 2018; Pebesma and Bivand, 2023)
- `stargazer` (Hlavac, 2022)
- `stringdist` (van der Loo, 2014)
- `stringi` (Gagolewski, 2022)
- `stringr` (Wickham, 2023)
- `tmap` (Tennekes, 2018)
- `terra` (Hijmans, 2025b)
- `texreg` (Leifeld, 2013)

  - `tidygeocoder` (Cambon et al., 2021)
  - `tidyr` (Wickham, Vaughan, and Girlich, 2024)
  - `tidyverse` (Wickham, Averick, et al., 2019)
  - `vtable` (Huntington-Klein, 2024)
  - `writexl` (Ooms, 2025)
  - `xml2` (Wickham, Hester, and Ooms, 2025)
  - `xtable` (Dahl et al., 2019)

  and their dependencies.

- Stata 17 (StataCorp, 2021)
- `TeXstudio` (Zander et al., 2022)

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# Appendix E

# Curriculum Vitae

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**Research Interests**

Economic History, Labor Economics, Political Economy

**Education**

| | |
|---|---|
| since 12/2019 | Ph.D. Candidate in Economics – Cologne Graduate School, first supervisor: Prof. Dr. Erik Hornung, second supervisor: Prof. Anna Bindler, Ph.D. |
| 03/2024 – 04/2024 | Research Visit Erasmus School of Economics, Rotterdam, visiting Prof. Dr. Anne Gielen |
| 10/2017 – 12/2019 | M.Sc. Economics (Research Track), University of Cologne, Grade: 1.0 |
| 10/2013 – 07/2017 | B.Sc. Economics, University of Cologne, Grade: 1.1 |
| 01/2015 – 06/2015 | Erasmus Exchange, Universidade Católica Portuguesa, Lisbon |

**Working Papers**

| | |
|---|---|
| 2024 | Worker Displacement and Labor Market Success: Evidence from Forced Labor Conscription during WWII *ECONtribute Discussion Paper* 338 |

**Work in Progress**

Missing Men and Women's Fight for the Vote (with Barbara Boelmann)
Rewriting the Social Contract: Elite Response to Labor Unrest (with Erik Hornung and Noam Yuchtman)
Forced Melting Pot: The Impact of Exposure to Foreigners on Long-Term Economic Relations (with Ann-Kristin Becker)

**Publications**

2024 Transparency and forecasting: the impact of conditioning assumptions on forecast accuracy (with Katja Heinisch and Christoph Schult), *Applied Economic Letters,* 1-5. https://doi.org/10.1080/13504851.2024.2388870

**Conference and Seminar Presentations**

2025 KIZ Political Economy Workshop, ifo Institute Munich
2025 Internal Seminar, JKU Linz
2025 Internal Seminar, University of Hohenheim
2025 Internal Seminar, University of Stuttgart
2025 Internal Seminar, LISER, Luxembourg
2025 Internal Seminar, Rockwool Foundation Berlin
2025 Internal Seminar, BECC at Max Planck Institute, Freiburg
2024 Cluster Seminar, DIW Berlin
2024 EALE Conference, Bergen
2024 Economic History Colloquium, University of Bayreuth
2024 Essex PhD Conference in Applied Economics
2024 Internal Seminar, Erasmus School of Economics Rotterdam
2023 Research Seminar Economic History, WU Vienna
2023 VfS Annual Conference, Regensburg
2023 Annual Economic History Association Meeting, Pittsburgh (poster)
2023 European Historical Economics Society Conference, Vienna
2023 Cologne FRESH Meeting
2023 Economic History Society Annual Conference, Warwick
2023 V. Congress for Economic and Social History, Leipzig
2022 2nd ECONtribute Young Economist Workshop, Cologne
2018 IWH-CIREQ-GW Macroeconometric Workshop, Halle (poster)

### Research and Professional Experience

| | |
|---|---|
| since 10/2019 | Research and Teaching Assistant, University of Cologne |
| 10/2018 – 09/2019 | Student Research and Teaching Assistant for Prof. Dr. Erik Hornung, University of Cologne |
| 08/2018 – 09/2018 | Internship at Halle Institute for Economic Research (IWH) |
| 09/2015 – 04/2017 | Student Assistant at Talanx Asset Management GmbH, Cologne |

### Scholarships

| | |
|---|---|
| since 09/2024 | Funding Job Market Year, ECONtribute Cluster of Excellence, Bonn-Cologne |
| 02/2020 – 12/2022 | Young Econtribute Program Fellow, ECONtribute Cluster of Excellence, Bonn-Cologne |
| 10/2017 – 09/2019 | Research Track Economics (non-material support from the Cologne Graduate School) |
| 11/2017 – 09/2018 | Deutschlandstipendium (German federal scholarship) |

### Teaching Experience

| | |
|---|---|
| 04/2025 – 05/2025 | Data Analysis and Econometrics, Exercise Session, University of Cologne |
| 04/2024 – 09/2024 | Seminar Economics (undergraduate), University of Cologne |
| 04/2023 – 09/2023 | Seminar in Long-Run Development and Labor (graduate), University of Cologne |
| since 10/2019 | Supervision of Bachelor Theses, University of Cologne |
| 10/2019 – 03/2023 | Seminar in Empirical Economics and Economic History (undergraduate), University of Cologne |
| 04/2019 – 07/2024 | Economic History (undergraduate), Exercise Session, University of Cologne |

### Contributions to the Profession

| | |
|---|---|
| | Refereeing for the Journal of Economic Inequality |
| since 04/2022 | Co-Organization of Internal Applied Micro Seminar, University of Cologne |
| 05/2023 | Co-Organization of Cologne FRESH Meeting |
| 12/2019 – 09/2020 | Co-Organization of Networking for Female Economists, University of Cologne |

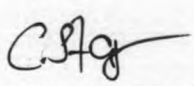
### Other Qualifications

| | |
|---|---|
| Languages | German (native), English (Professional proficiency), Spanish (basic) |
| Computer Skills | R, LaTeX, Matlab, Stata |

# Appendix F

# Eidesstattliche Erklärung

**gemäß § 9 Abs. 5 der Promotionsordnung vom 01.08.2022:**
Hiermit versichere ich an Eides statt, dass ich die vorliegende Dissertation selbstständig und ohne die Benutzung anderer als der angegebenen Hilfsmittel und Literatur angefertigt habe. Weitere Personen, neben den ggf. in der Arbeit aufgeführten Koautorinnen und Koautoren, waren an der inhaltlich-materiellen Erstellung der vorliegenden Arbeit nicht beteiligt. Alle Stellen, die wörtlich oder sinngemäß aus veröffentlichten und nicht veröffentlichten fremden Werken dem Wortlaut oder dem Sinn nach entnommen wurden, sind als solche kenntlich gemacht. Ich versichere an Eides statt, dass diese Dissertation noch keiner anderen Fakultät oder Universität zur Prüfung vorgelegen hat; dass sie - abgesehen von den angegebenen Teilpublikationen und eingebundenen Artikeln und Manuskripten - noch nicht veröffentlicht worden ist, sowie, dass ich eine Veröffentlichung der Dissertation vor Abschluss der Promotion nicht ohne Genehmigung des Promotionsausschusses vornehmen werde. Die Bestimmungen dieser Ordnung sind mir bekannt. Darüber hinaus erkläre ich hiermit, dass ich die Leitlinien der Universität zu Köln zur Sicherung guter wissenschaftlichen Praxis gelesen und sie bei der Durchführung der der Dissertation zugrundeliegenden Arbeiten und der schriftlich verfassten Dissertation beachtet habe und verpflichte mich hiermit, die dort genannten Vorgaben bei allen wissenschaftlichen Tätigkeiten zu beachten und umzusetzen. Ich versichere, dass die eingereichte elektronische Fassung der eingereichten Druckfassung vollständig entspricht. Ich versichere, dass ich nach bestem Wissen die reine Wahrheit gesagt und nichts verschwiegen habe.



Köln, 14.06.2025 *Carola Rose Stapper*