

Nüß, Patrick

**Working Paper**

## Management opposition, strikes and union threat

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## Management Opposition, Strikes and Union Threat

Patrick Nüß

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## Author

### **Patrick Nüß**

Halle Institute for Economic Research (IWH) –  
Member of the Leibniz Association,  
Department of Structural Change and  
Productivity  
E-mail: [patrick.nuess@iwh-halle.de](mailto:patrick.nuess@iwh-halle.de)  
Tel +49 345 7753 705

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Halle Institute for Economic Research (IWH) –  
Member of the Leibniz Association

Address: Kleine Maerkerstrasse 8  
D-06108 Halle (Saale), Germany  
Postal Address: P.O. Box 11 03 61  
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60  
Fax +49 345 7753 820

[www.iwh-halle.de](http://www.iwh-halle.de)

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# Management Opposition, Strikes and Union Threat\*

## Abstract

I estimate management opposition to unions in terms of hiring discrimination in the German labor market. By sending 13,000 fictitious job applications, revealing union membership in the CV and pro-union sentiment via social media accounts, I provide evidence for hiring discrimination against union supporters. Callback rates are on average 15% lower for union members. Discrimination is strongest in the presence of a high sectoral share of union members and large firm size. I further explore variation in regional and sectoral strike intensity over time and find suggestive evidence that discrimination increases if a sector is exposed to an intense strike. Discrimination is positively associated with the sectoral share of firms that voluntarily orientate wages to collective agreements. These results indicate that hiring discrimination can be explained by union threat effects.

*Keywords: correspondence audit, field experiments, industrial relations, labor disputes, management opposition, trade unions, union threat*

*JEL classification: C93, J51, J53, J71*

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# 1 Introduction

The purpose of unions is to organize workers to improve wages and general working conditions, using their key tools of collective bargaining and strikes. Due to their impact on profits, unions commonly face opposition during unionization attempts from management. Management opposition can be expressed by direct (not always legal) actions such as spreading fear, firing unionized workers, and marginally improving working conditions (Freeman and Kleiner, 1990; McNicholas et al., 2019; Bourdieu et al., 2022) or avoiding the hiring of union members (Leap et al., 1990; Saltzman, 1995; Baert and Omeij, 2015). However, little is known about the true extent of management opposition across the whole economy. How widespread are practices of management opposition against individual union members? How is opposition to individual union members linked to the industrial relations system?

To ensure the employee’s constitutional right to join associations to improve wages and working conditions, it is crucial to accurately measure the existence and extent of management opposition to unions. Given both, the illegal character of most management opposition instruments and their sensitive nature, the measured extent of management opposition is likely biased. On the one hand, if management opposition practices vary with firm characteristics and some types of firms or sectors can avoid being caught in illegal practices, analysis of management opposition would underestimate the true extent of opposition. If, on the other hand, unionization is more likely to take place in firms that oppose unions, observed management opposition to unionization attempts would overestimate the true extent of opposition. These biases due to social desirability, selection (Addison and Hirsch, 1989; Freeman and Kleiner, 1990; Dinlersoz et al., 2014), selective measurements (Gall, 2004) and unobserved heterogeneity (Card, 1996) are well known challenges in the industrial relation literature.

To overcome these challenges, I conduct the first-ever large-scale correspondence experiment that allows me to estimate a representative measure of management opposition against union supporters. The key lens through which I assess management opposition to unionization is the mechanism of *hiring*. I sent 13,000 fictitious job applications to real vacancies in the German labor market. Randomly revealing union membership in the resume or a pro-union sentiment via social media accounts linked to fictitious job applications allows me to measure management opposition against union supporters in the hiring process. The equally low cost of opposition for all firms ensures comparability over firms and sectors. For this purpose, I collected data in four waves over the period of 2017 to 2021 providing the opportunity to study effect heterogeneity according to firms, occupations and labor-market regions. This novel dataset allows me to provide insights into which types of firms and sectors are subject to management opposition, independent of the cost of opposition and current unionization attempts. Furthermore, the large dataset in combination with regional and sectoral variation over time allows for investigating variations in labor market conditions and strike activity.

A correspondence experiment on management opposition circumvents many potential biases, which plague many extant studies of the phenomenon: It focuses on behaviors and avoids relying on employers’ and employee statements which are subject to social desirability biases and perceptions. This method avoids selection problems and selective measurement by applications being independent of current union activities and the easy as well as inexpensive opportunity to oppose

union members. Finally, it avoids unobservable differences by the use of fictitious applicants, due to full control over the applicants' characteristics and information access of employers. As a result, correspondence experiments can measure the extent of management opposition against union members in an economy.

I provide evidence for widespread management opposition in Germany. By revealing union membership in the resume, I find that invitations to job interviews are on average 15% lower for union members, compared to their non-union counterparts. This result is robust and replicates in all four years of the experiment. To account for the possibility that revealing a union membership in the resume provides a different signal than just the union membership, I make use of a second experiment using social media accounts. By revealing a pro-union sentiment in social media accounts and the comparison to applicants with otherwise similar social media activities, I can test the existence of discrimination in a subtle way. I find that invitations to job interviews are on average 10% lower for applicants with social media accounts liking and sharing posts from union accounts.

Looking more closely, I show that hiring discrimination is heterogeneous by sector, increasing with the share of union members in the sector. Discrimination is low for real estate and the health sector, and strongest for the public and energy sectors. Hiring discrimination further varies with firm size. There is no evidence of hiring discrimination against union supporters in firms with fewer than 6 employees. Firms with 6 to 50 employees have about 15% lower callback rates for union members, which further increases with firm size. This firm pattern stands against potential concerns about an underestimation of existing management opposition and supports the idea that larger firms are the main venue of union-employee conflicts.

In line with the theory of union threat ([Rosen, 1969](#)), I show that discrimination is strongest among firms in well-organized sectors, which themselves are not covered by a collective agreement. This result indicates that the measured hiring discrimination is driven by employers' fear of unionization, which can increase wages and improve working conditions. While management opposition against union members, in particular hiring discrimination, is illegal, the results suggest that management opposition is a symptom of a well-functioning industrial relations system where unions are recognized as influential. In sectors without employers perceiving unions as influential, employers are less likely to be covered by a collective agreement, and in the absence of a collective agreement, they are less likely to orientate wages to existing sectoral collective agreements.

My paper is most closely related to [Baert and Omey \(2015\)](#) and [Kreisberg and Wilmers \(2022\)](#). [Baert and Omey \(2015\)](#) analyze hiring discrimination against former members of the union youth wing with a correspondence experiment sending 560 applications in the northern Belgian labor market. [Kreisberg and Wilmers \(2022\)](#) conduct a United States based correspondence experiment sending 1025 applications for entry-level jobs in Chicago. While [Baert and Omey \(2015\)](#) find evidence for union based hiring discrimination, particularly in highly unionized sectors, there is no evidence for hiring discrimination in the United States ([Kreisberg and Wilmers, 2022](#)).

My study builds on these papers in two dimensions. First, unlike these studies, which focused on a small labor market region and a narrow set of occupations, my sample size of 13,000

observations and my data collection for several distinct occupations and regions over the period of four years allows me to account for the relevance of labor market conditions, to quantify an unbiased representative extent of management opposition in the German economy. These labor market conditions cover labor market tightness, union strike activity, sectoral coverage by collective agreements, and sectoral share of voluntary orientation on collective agreements. Second, instead of only revealing information about union membership in the resume, I use social media activities to reveal a pro-union sentiment in a more subtle way. This allows me to improve the internal and external validity of hiring discrimination against union supporters, showing that the inclusion of pro-union social media activities via social media leads to results qualitatively comparable to the direct inclusion in the resume.

My paper contributes to the broader literature on management opposition and union organizing. Given the illegal character of some management opposition instruments and their sensitive nature, measures of these illegal activities could provide a biased picture of the extent and distribution of management opposition against unions. A key finding of my analysis is that hiring discrimination against union supporters exists in almost all sectors in Germany, and is strongest in larger firms in strongly organized sectors. While previous studies based on observational data can not identify the extent of management opposition against unions (for example [Bronfenbrenner, 1997](#); [Behrens, 2009](#); [Heery and Simms, 2010](#); [Dinlersoz et al., 2014](#); [Aleks, 2015](#); [McNicholas et al., 2019](#)), they correctly detect larger firms and high union membership firms and sectors as the main venue of union-employee conflicts.

Finally, my study contributes to the literature on hiring discrimination ([Bertrand and Mullainathan, 2004](#); [Kline et al., 2022](#)), particularly to the literature revealing uncommon characteristics in the hiring process ([Tilcsik, 2011](#); [Button and Walker, 2020](#)). By providing evidence for the indirect revelation of uncommon characteristics via social media accounts, I illustrate the potential of social media accounts for the analysis of otherwise uncommon aspects in the resume ([Acquisti and Fong, 2020](#)), without the need to rely on managers' time-consuming internet search of applicants.

The remainder of the paper is organized as follows. Section 2 provides an overview of the institutional setting of Germany's industrial relations system. Section 3 describes the experimental design of the first experiment, revealing union membership in the resume. Section 4 provides the experimental results. Section 5 describes the second experimental design revealing a pro-union attitude via the use of social media accounts. Section 6 provides the experimental results. Section 7 tests various sources of management opposition, including firm characteristics, labor market conditions and union strike activity. Section 8 discusses my results in the context of the literature. Section 9 concludes.

## 2 Institutional Background of Industrial Relations in Germany

The German industrial relations are characterized by a dual system of employment representation. Unions represent their voluntary members by focusing on collective bargaining, while works councils are elected workplace representatives, which function as worker's voice at the firm level. Germany has a long tradition of employee participation that institutionalizes works councils as



a legal basis of union voice. One of the most important aspects of the separation of unions and works councils is the legal ban on collective bargaining for works councils ([Hassel, 1999](#)), which also stipulates that works councils are not allowed to organize a strike.

Due to the separation of unions and works councils, unions' main objective in Germany is improving wages and working conditions through collective agreements, strikes, legal training and consulting with works councils. Germany has a centralized bargaining system that consists of sectoral wage-bargaining between unions and employer associations, firm-level bargaining between individual firms and a union as well as individual wage bargaining between individual firms and employees. For greater flexibility of the bargaining system, opening clauses are widespread phenomena for temporary deviations from collective agreements due to economic circumstances ([Boeri et al., 2021](#); [Jäger et al., 2022](#)). According to the IAB Establishment Panel in 2018, 54% of all employees in Germany were covered by a collective agreement, including 46% of sector collective agreements and 8% firm-level agreements ([Ellguth and Kohaut, 2019](#)). Of the remaining 46%, about half the firms without official coverage by a collective agreement claim to voluntarily base their wages on existing collective agreements.

Works councils have a strong legal basis due to the Works Constitution Act and have an influence on a wide range of firm activities. These activities range from the hiring process to safety management. Works councils are an important institution for the enforcement of anti-discrimination laws, working time laws and workplace security. While works councils are independent of unions, they often work hand in hand. As such, works councils are important for union member recruitment ([Behrens, 2009](#)).<sup>2</sup>

Figure 1 provides insight into the development of the German industrial relations system. The past three decades featured a steady erosion of the industrial relations system. While in 1990, 85% of employees were covered by a collective agreement, in 2018 this coverage decreased to 54% of employees. The same decline is observable for union membership rates, which decreased from 31.2% in 1990 to 16.3% in recent years. This downward trend is also observable for employees' coverage of works councils as well as the membership rates of employers' organizations. With this, Germany matches the general trend of the erosion of the industrial relations in European countries ([Schnabel, 2013](#)).

The erosion of Germany's industrial relations seems to have little effect on unions' strike activities (see Figure 2). From 1993 to 2019 unions' strike activities were stable with around 0.4% of employees participating in strikes and 5.5 lost work days per 1,000 employees. However, while overall strike activities might be unaffected, over the last two decades, the average collective agreement had a contractual period of about two years (compared to an average of one year before 2000) ([WSI-Tarifarchiv, 2020](#), Appendix A-27). This is relevant for two reasons. First, this implies that collective agreements are negotiated less frequently, but with increased strike effort. Second, during the validity of a collective agreement, there is a peace obligation that prohibits unions from further engaging in strikes. In my analysis of the impact of strike activities, I focus on the period 2017 to 2019, a period of strong variation in overall strike activities. Due to the long contractual periods, this should lead to an even stronger regional and sectoral variation of strike activities over time.

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<sup>2</sup>For a more detailed examination of the German model of industrial relations see [Jäger et al. \(2022\)](#).



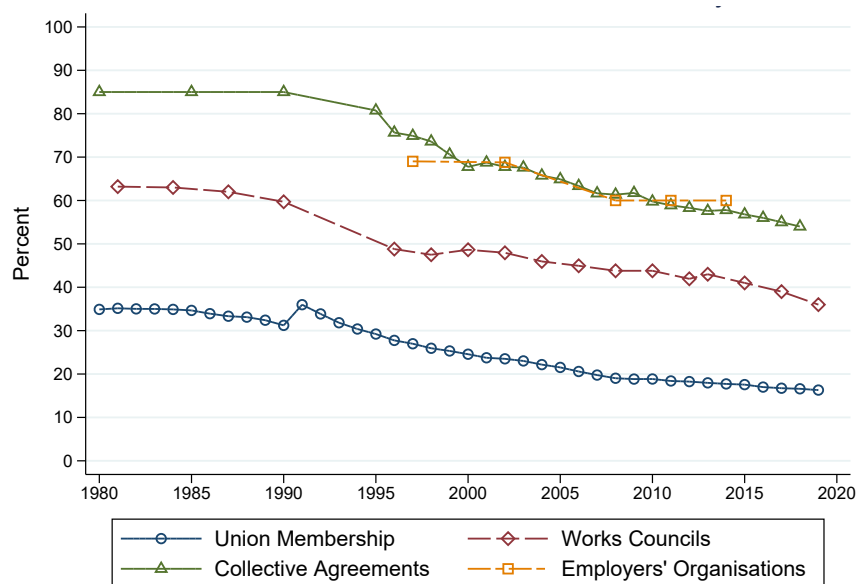


Figure 1: Industrial Relations Trends in Germany

*Source:* J. Visser, ICTWSS Data base. Version 6.0. Amsterdam: Amsterdam Institute for Advanced Labour Studies AIAS November 2019, extended by IAB Establishment Panel Data and the OECD/AIAS ICTWSS Database August 2023.

*Note:* This figure presents the development of Germany's industrial relations over time. Union members are the share of union members among employees. Works councils provides the share of employees, covered by a works council. Collective agreements provides the share of employees covered by a collective agreement. Employers' organisations covers the percentage of firms, that are members of an employer association.

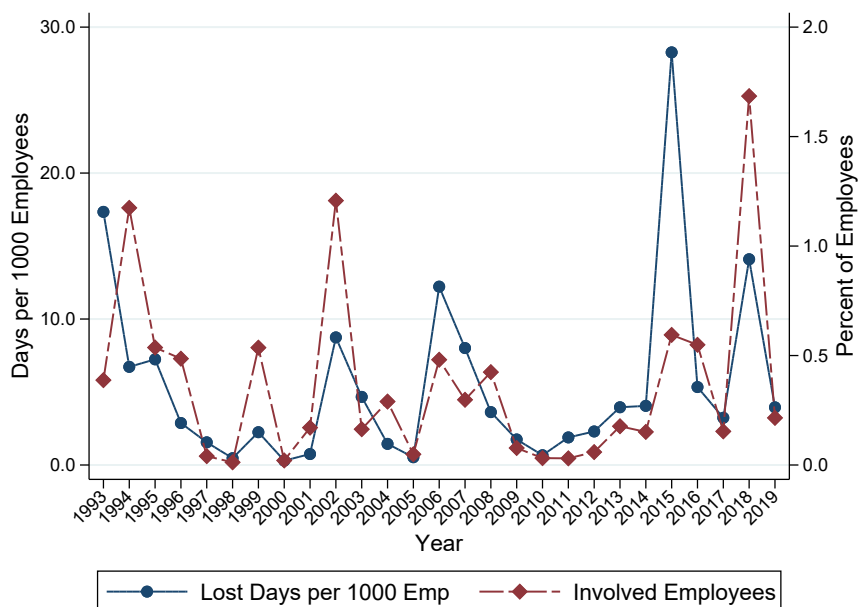


Figure 2: Labor Dispute Trends in Germany

*Source:* Statistics of the Federal Employment Agency; Streikstatistik (Jahreszahlen).

*Note:* This figure presents the development of Germany's strike activities over time. Lost days per 1,000 employees, is constructed based on the total number of lost work days due to a strike in a given year, divided by the number of employees in thousands. Involved employees is constructed based on the total number of employees that participated in a given year in a strike, divided by the total number of employees.

Due to the strength of union activities and the institutional separation of unions wage bargaining from works councils' workers' voice, Germany is thus an ideal setting to conduct an experiment on employers' attitudes toward unions. It allows the separation of union bargaining activities (wage bargaining and strikes) from works councils (workers' voice). In addition, the heterogeneity in Germany's collective bargaining system and accessible data on regional variation provide the opportunity to understand determinants of management opposition and its link to patterns of Germany's industrial relations system.

### 3 Experiment 1

Whenever surveys and observable data are in use, there is the possibility of selection problems and unobservable heterogeneity resulting in systematic biases. For research questions of a sensitive nature, for example discrimination, survey data carries the additional risk of a bias due to social desirability. To avoid such concerns, a common approach is to apply randomization and the use of natural experiments.

In the absence of a natural experiment, correspondence experiments allow for a causal analysis of employers' preferences. This approach is already common for the analysis of labor market discrimination, for example on ethnicity (Bertrand and Mullainathan, 2004), age (Neumark et al., 2019) or gender (Riach and Rich, 2006). These experiments send fictitious job applications to real vacancies. By monitoring the callback rates of companies, this then allows an insight into hiring decisions and a causal interpretation of randomized characteristics of the applications. Such an experiment avoids potential systematic bias in reports of management opposition, selection problems resulting from unions' organizing decisions as well as unobserved determinants of union members and organizing attempts. Correspondence experiments also provide access to the full information set available to employers. Furthermore, a correspondence experiment gives employers independent of firm size an easy opportunity to discriminate against union members to prevent organizing in the long run.

In the first experiment, I sent pairs of applications in random order to each firm. Resumes include standard information as well as hobbies and memberships, with one of them revealing a union membership. The experiment covers five occupations (office clerks, sales, hotel manager, logistic workers and mechatronics technicians) in six federal states (Berlin, North Rhine-Westphalia, Hamburg, Bavaria, Baden-Wurttemberg and Saxony), over the period of 2017 to 2019. The sample of the first experiment consists of 8,714 observations from 4,357 firms.

Vacancy data were collected via the job portal of the German Federal Employment Agency, which is the largest provider of vacancies in Germany.<sup>3</sup> To concentrate on up-to-date vacancies, I searched the website every week for new vacancies that went online during the course of the week. The search engine was set to new vacancies for full-time positions requiring vocational training in their chosen profession, without managerial responsibility. Excluded from the search are firms categorized as temporary employment agencies or service agencies. To reduce the burden for individual firms and to avoid detection, each firm was only included once during each investigation period. The job portal also provides detailed information on firms and the

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<sup>3</sup>Job portal of the German Federal Employment Agency: <https://www.arbeitsagentur.de/jobsuche/>.

application process. The vacancies provided firm information covering the workplace address, firm size and sectoral classification. The vacancies provided detailed information about the vacancy, covering information about the deadline for the applications, the preferred entrance date, the length of the employment contract, the number of open positions and additional needed qualifications.

The applications were sent on Monday and Tuesday evenings in randomized order. Given that my applicants were all in the last month of their current employment contract, the applications were timed between 6 and 8 pm. This was done to make the impression of a still employed applicant in the last month of his current employment. Firm responses were collected via email responses and voicemails linked to the phone number provided with the application. To reduce the burden of the correspondence experiment for firms to a minimum, emails and voicemails were checked and answered every evening.

All applications consist of a cover letter, a resume and certificates. The application design follows real-life cover letters and resumes. All applicants are male, single, have typical German names, are 28 years old, with secondary education, vocational training and 10 years of work experience, including their vocational training. All applicants attest good knowledge of English, with German as their native language. They all have a driver's license and possess necessary occupation-specific skills such as software knowledge or a fork-lift or a truck driver's license. For certificates, the applicants provided copies of their final school certificate and the final certificate of their vocational training, with average marks of two, indicating good grades in the German grading system. As in previous correspondence experiments (for example [Tilcsik, 2011](#); [Button and Walker, 2020](#)), I include voluntary activities and memberships in the resumes. In Germany hobbies and memberships are commonly included in a category called "other/hobbies/memberships" with a membership of an organization related to their hobby. An example application is shown in Figure 3.

For each application pair, one application is randomly selected to include a union membership that is consistent with an applicants previous employer. The chosen unions are part of the Confederation of German Trade Unions (Deutscher Gewerkschaftsbund) which is the head organization of 8 sector-specific unions covering the majority of all union members in Germany.<sup>4</sup> The random assignment of union membership implies orthogonality to all resume characteristics and avoids unions selection process to organize a specific firm, leading to the measure of the causal effect of union membership in the hiring process.

Table 1 provides an overview of the considered regions and occupations of the first experiment. The experiment consists of three waves covering the period August to October in the years 2017 to 2019. The 2017 wave covers four occupations (office clerk, sales manager, hotel manager and logistic worker) in four federal states (Berlin, North Rhine-Westphalia, Hamburg and Bavaria). The following waves consist of the same occupations and regions extended by the occupation of mechatronics technician and the federal states of Baden-Wurttemberg and Saxony. The regions and occupations were expanded to increase variation in labor market tightness and coverage of sectors.

This design covers 31.1% of the German workforce by job type (see Appendix A-3). With

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<sup>4</sup>The chosen unions are the IG Metall (Union of the Metal Industry), Ver.di (United Service Union) and the NGG (Food Workers' Union).

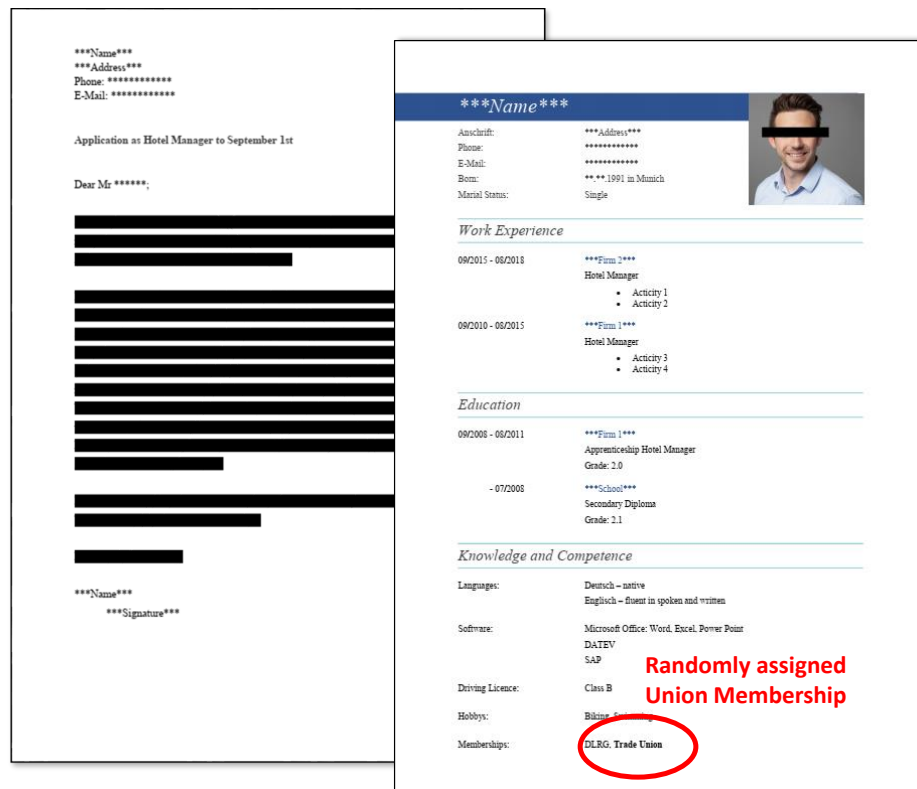


Figure 3: Example Application of Experiment 1

*Note:* The figure presents an example resume of an applicant for a Hotel Manager position of the 2018 wave of the experiment.

respect to economic activities, the chosen 6 out of 16 federal states have an employment share of 64.4% and account for 66.2% of Germany's GDP (see Appendix A-3). In sum, this design allows for the analysis of repeated cross-section data of a sizable part of the German economy.

Table 1: Overview of the Experimental Design

Federal States	2017	2018	2019	Occupations	2017	2018	2019
Berlin	✓	✓	✓	Office Clerk	✓	✓	✓
North Rhine-Westphalia	✓	✓	✓	Sales Manager	✓	✓	✓
Hamburg	✓	✓	✓	Hotel Manager	✓	✓	✓
Bavaria	✓	✓	✓	Logistic Worker	✓	✓	✓
Saxony		✓	✓	Mechatronics Technician		✓	✓
Baden-Wuerttemberg		✓	✓				

*Note:* The table presents an overview of the federal states and occupation considered for the experiment throughout 2017 to 2019.

The analysis focuses on callback rates as main outcomes. These callback categories are based on the two standard definitions of callbacks. Positive callbacks in a strict sense (Callback Category 1) include only explicit invitations to a job interview. Positive callbacks in a broad sense (Callback

Category 2) augment the first definition by including every request for more information, requests related to wages, requests for alternative jobs, and requests to get in contact with the potential employer.<sup>5</sup> In the majority of additional callbacks, the job seeker was requested to contact the potential employer.

Differences in callbacks between union and non-union applicants are interpreted as management opposition in terms of hiring discrimination to unions supporters. As pointed out in Section 2 this is possible due to the institutional setting of Germany's industrial relations system, which allows a clear distinction between works councils (workers voice) from unions (collective bargaining and strikes). This separation allows me to focus on employers' attitudes toward union members separately from employers' resistance to works councils (Behrens and Dribbusch, 2018).

## 4 Results from Experiment 1

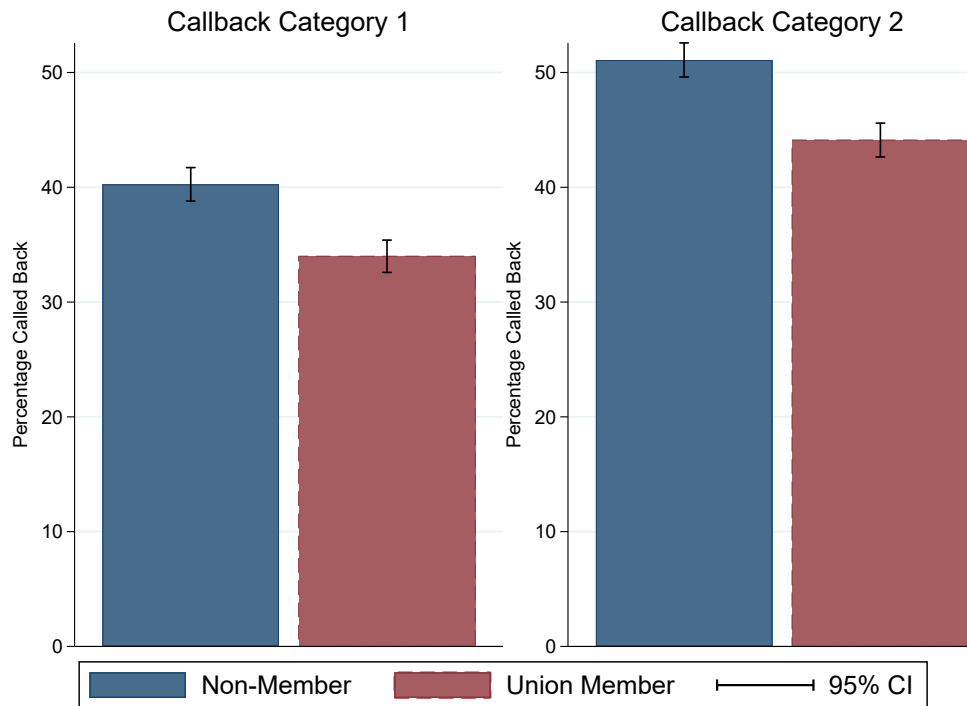


Figure 4: Callbacks by Membership

*Note:* The figure presents the average callbacks of union and non-union members, over the period of 2017 to 2019, for both callback categories. Callbacks are based on the probability of receiving a positive callback based on the Callback Category 1 "Invitation to a job interview" and Callback Category 2 "Any request of an employer".

The full sample of the first experiment consists of 8,714 observations (4,357 firms), with 2,082 observations for 2017, 3,274 observations for 2018 and 3,358 for 2019.<sup>6</sup> Out of the full sample 43.0% of firms invited at least one applicant in terms of the callback category 1. In terms of

<sup>5</sup>A list of typical employer responses and their classification is available in Appendix A-1.

<sup>6</sup>Callback rate variate strongly between occupations reaching from 24.4% to 52.9% for callback category 1 and 33.3% to 64.8% for callback category 2.

callback category 2, 55.8% of all firms invited at least one applicant. Tests on the balance of the application characteristics suggest successful randomization of all characteristics (see Appendix A-4).

I begin by documenting the overall callback rates of the experiment. Figure 4 shows that for the strict definition of callbacks (Callback Category 1), non-union members received a positive callback in 40.3% of the applications, while union members received 34.0% positive callbacks. For the broader definition of callbacks (Callback Category 2), I observe positive callbacks in 51.1% of the applications of non-union members compared to 44.1% for union members. This shows that on average, callback rates are 6.3 to 7.0 percentage points lower for union members.

#### 4.1 Multivariate Analysis of Hiring Discrimination

To explore the robustness of the results, this section examines the sensitivity of the average effect of union membership with respect to a range of control variables in multivariate analysis. In the analysis I focus on a linear probability model. The data contain information on firm size, sector, contract type and the coverage of collective agreements, as well as randomized characteristics of the resumes. The experimental dataset is augmented by the sectoral share of union members (union density), which is calculated based on the German Socio-Economic Panel (GSOEP) and an occupation-region-specific measure for labor market tightness (Vacancy/Unemployment-Ratio), which is calculated based on federal employment agency data.<sup>7</sup>

Equation 1 contains the specified linear probability model, with one of the callback categories as the dependent variable.

$$Callback_{it} = \alpha + \tau Union_i + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (1)$$

The coefficient  $\tau$  is the average percentage point difference in callback rates of revealing a union membership in the resume. The vector  $\mathbf{X}$  covers a wide range of resume, firm level and labor market characteristics. Table 2 presents results with different extents of control variables. On average I observe 6.3 (7.0) percentage points lower callbacks for callback category 1 (callback category 2) by revealing union membership in the application.

I conduct additional sensitivity analyses with a step-by-step extension of the baseline specification of Equation 1, firm fixed effects as well as sub-samples and probit estimates. Overall these analyses are consistent with Table 2 and show that the effect of union membership is negative and significant at the 1% significance level for all specifications and years (see Table A-9).

#### 4.2 Robustness Checks

The already well-developed literature on correspondence experiments reveals three potential pitfalls for the identification of an unbiased and representative measure of hiring discrimination.

First, the Heckman Critique points out that differences in the variance of unobserved characteristics could bias correspondence experiments on hiring discrimination (Heckman, 1998). It

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<sup>7</sup>Summary statistics are available in Appendix A-2.

Table 2: Hiring Discrimination from Revealing Union Membership

	(1)	(2)	(3)	(4)	(5)	(6)
Union	-0.0627*** (0.0063)	-0.0698*** (0.0067)	-0.0627*** (0.0063)	-0.0698*** (0.0067)	-0.0623*** (0.0063)	-0.0696*** (0.0067)
Callback Rate Control Group	0.403	0.511	0.403	0.511	0.403	0.511
N	8,714	8,714	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.004	0.005	0.067	0.074	0.067	0.073
Dependent Variables						
Strict Callbacks (Callback Category 1)	✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓
Controls						
Occupation FE			✓	✓	✓	✓
Federal State FE			✓	✓	✓	✓
Year FE			✓	✓	✓	✓
Firm Controls					✓	✓
Application Controls					✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer". The baseline controls are regional, occupation and year fixed effects. The extended set of controls covers firm controls (collective agreement coverage, contract type, firm size) and application controls (application order, layouts, pictures, previous employers, organization membership, name, address and school). Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

can be shown that for correspondence experiments in labor markets, this is a common bias (Neumark and Rich, 2019).<sup>8</sup> To account for the Heckman Critique, I apply the Neumark Correction (Neumark, 2012), providing no evidence for a bias due to unobservable differences (Appendix A-10).

Second, Neumark et al. (2019) show that correspondence experiments can easily give a wrong picture about the representativeness of discrimination due to non/inadequate weighting. I account for the chance of inadequate weighting by reweighting the sample toward population characteristics. Reweighting by occupational, regional and sectoral employment shares has little effect on the magnitude of hiring discrimination (Table A-11). These results indicate that neither unobservables nor inadequate weighting threatens the unbiased and representative measurement of hiring discrimination in the experiment.

Third, a recent experiment by Acquisti and Fong (2020) raises doubt about the inclusion of voluntary activities and memberships in resumes to identify hiring discrimination. The concern is that the explicit or implicit revelation of uncommon information among applicants could signal personality traits that reveal more than religious, sexual or political orientation, leading to discrimination unrelated to the dimension of interest. When Acquisti and Fong (2020) use a more subtle approach and reveal religious affiliation and sexual orientation via social media accounts, they find little evidence for hiring discrimination. In the context of union memberships, employers might expect higher reservation wages of union members, compared to non-union members. As a result, employers might discriminate against union members due to their anticipation of

<sup>8</sup>Differences in unobservables are indeed a challenge given the fact that early union wage effect literature points out that union members can be better in unobservable characteristics. Card (1996) shows that indeed in the case of union members there is a positive (negative) selection for low (high) skilled workers in terms of unobservables. In the German context, Budde et al. (2023) find no differences in cognitive skills between regular employees and works council members, but show that works council members are a positive selection in terms of wages.



higher wage expectations (Kreisberg and Wilmers, 2022). This implies union membership could be interpreted as a signal for higher reservation wages. When this is the case, lower callback rates for union members are not related to hiring discrimination of union supporters but can be explained by employers favoring applicants with lower wage demands. To address these concerns, I conduct an additional experiment.

## 5 Experiment 2

The last remaining concerns of the experimental design and measurement are the choice to list a union membership in a resume alongside other voluntary activities and memberships, indicating a signal of union membership different from the union membership itself. With regard to union members, potential employers could interpret the membership revelation as a signal of readiness for conflicts or simple naivety.

While the consideration of voluntary activities is common in correspondence experiments, the explicit or implicit revelation of unexpected disclosures among applicants could signal personality traits that reveal more than religious, sexual or political orientation.

To consider these remaining two threats to understanding how employers interpret union membership signaling, I conducted an additional correspondence experiment, using a more subtle treatment to identify hiring discrimination. Furthermore, due to potential concerns of differences in wage expectations, I add an additional callback category to the analysis. If differences in callbacks result from differences in wage expectations, firms should be more likely to ask union members for their expected wages. While callback category 2 contains all positive replies, a subset of these firms asked for expected wages. For this second experiment, I created an additional callback category 3, which contains only callbacks of employers that had requests related to wages.

For this reason the next step, I test how sensitive the results are to how union membership was revealed. In my second experiment, union membership was revealed indirectly via social media (Twitter) accounts rather than listing the membership in a resume as further treatment. This is in the spirit of recent correspondence experiments that use social media accounts to provide information of applicants (in example Baert, 2018; Manant et al., 2019; Acquisti and Fong, 2020; Moritz et al., 2023). The data collection was carried out from August to November 2021, and covered the same regions as the main experiment. The second experiment concentrated on office clerks. The experiment covers the previous treatment and control group sending an application without a union membership (Control Group) and revealing a union membership in the resume (Treatment 1).

In addition, there are two applications similar in content to the control group. However, the signature used in the application includes a Twitter account related to one of two Twitter accounts. The first Twitter account (Treatment 2) likes and retweets daily news, and content related to hobbies and the applicants favorite football club. The second Twitter account (Treatment 3) likes and retweets the same content as the first account, extended by regular likes and tweets from the Twitter accounts of the Confederation of German Trade Unions (DGB: @DGB\_News) and the United Services Union (Ver.di: @\_Verdi).

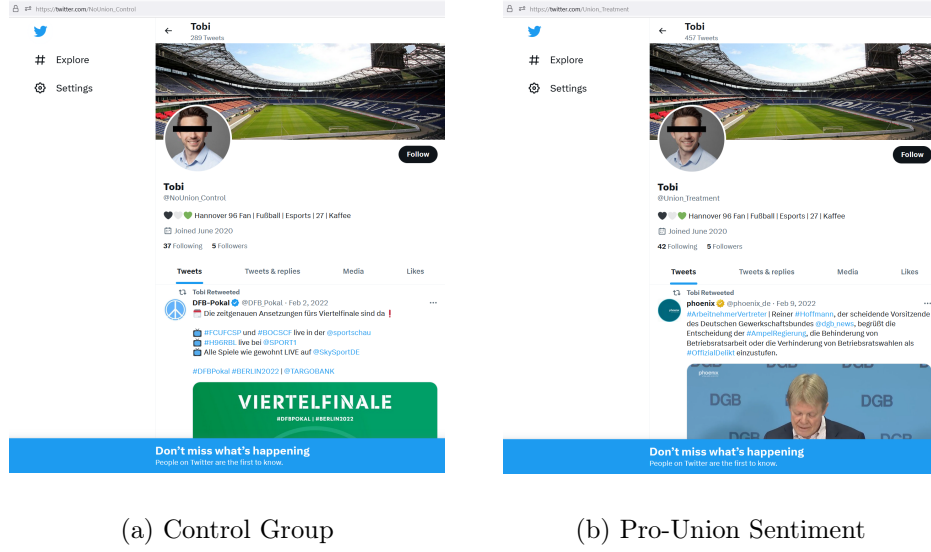


Figure 5: Twitter Accounts of Experiment 2

*Note:* The figures show the online available information on the Twitter accounts during the experiment 2. Panel (a) provides a screenshot of the control group’s Twitter account. Panel (b) provides a screenshot of the Twitter account that signaled a pro-union sentiment by liking and retweeting content of union Twitter accounts as well as other content related to unions.

To reduce the risk of detection, in this second experiment, I sent only one application to each firm, which reduces the randomization of application aspects and the applicants’ history to a minimum.<sup>9</sup> Twitter accounts were created and active 13 months before the beginning of the experiment. The Twitter accounts had same profile pictures and bios. Likes and retweets were scheduled during typical break times (breakfast and lunch), after work and at weekends to avoid any signal of lower productivity during working time.<sup>10</sup> Figure 5 shows the appearance of the used Twitter accounts renamed `@Union_Treatment` and `@NoUnion_Control`. A detailed protocol of the experiment, including the operation of the Twitter accounts is available in Appendix A-8.

Due to the otherwise identical applications and Twitter content, differences in callbacks between the Twitter accounts allow me to identify the causal effect of the applicants’ positive union sentiment, without the previous signal of readiness for conflicts or simple naivety.

## 6 Results from Experiment 2

The full sample consists of 4,531 observations. Assuming about 1/3 of human resource managers would look at the applicants’ social media accounts, I sent applications of the old and new

<sup>9</sup>Using more than one application for each firm would lead to several challenges to the experiment, which increases the likelihood of detection that can bias the experimental results (Weichselbaumer, 2015; Larsen, 2020; Balfe et al., 2023). First, sending two qualitatively similar applications to the same firm, both providing information of social media accounts are likely to increase suspicion. Second, sending two applications to each firm, where both applications provide information of social media accounts, would not allow me to provide similar Twitter activities for both accounts. Providing the same content would further increase suspicion and threaten the identification while providing different content would threaten the identification as well.

<sup>10</sup>To ensure employers screening the Twitter accounts to detect the retweet of union accounts easily, one of the last retweets was most of the time a retweet from a union account.

treatment in a 1:3 ratio. This leads in a control group of 579 observations, treatment 1 of 571 observations, treatment 2 of 1,696 observations and Treatment 3 of 1,685 observations.

With only one application sent to each firm and applications were similar in all aspects except the treatments, no check for successful randomization of the application details is needed. However, due to the potential relevance of firm characteristics, Appendix A-10 provides a balance check for an equal distribution of union and non-union applications regarding firm characteristics. The balancing was overall successful, with smaller deviations among the sectoral distribution of treatments.

To consider the relevance of hiring discrimination against union supporters resulting from expected higher reservation wages, in addition to callback categories 1 and 2, I construct a callback category 3. As a subset of callback category 2, callback category 3 only considers callbacks related to wages. For callback category 1 (Category 2), I observe positive callback rates in 29.0% (39.9%). Out of the full sample of 4,531 observations in the robustness check, 3.1% of all employers had requests related to expected wages.

$$Callback_i = \alpha + \beta_1 Union_{Resume} + \beta_2 Union_{Twitter} + \beta_3 Twitter + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (2)$$

Equation 2 contains the specified linear probability model, with previously defined callback categories as dependent variables. The coefficient  $\beta_1$  is the average percentage point difference in callback rates of revealing a union membership in the resume (old design). The coefficient  $\beta_2$  is the average percentage point difference in callback rates of a pro-union sentiment revealed via the Twitter account. The vector  $\mathbf{X}$  covers a range of control variables related to firm characteristics and regional fixed effects.

Table 3: Treatment Effect the Union Signal in CV and Twitter Accounts

	(1)	(2)	(3)	(4)	(5)	(6)
Union in Resume	-0.0639** (0.0264)	-0.0608** (0.0286)	0.0005 (0.0113)	-0.0665** (0.0261)	-0.0629** (0.0284)	0.0009 (0.0113)
Union on Twitter	-0.0342** (0.0157)	-0.0370** (0.0169)	-0.0040 (0.0057)	-0.0346** (0.0156)	-0.0377** (0.0167)	-0.0039 (0.0058)
Twitter	-0.0001 (0.0223)	0.0100 (0.0237)	-0.0073 (0.0090)	-0.0024 (0.0221)	0.0095 (0.0236)	-0.0065 (0.0090)
Callback Rate Control Group	0.311	0.413	0.031	0.311	0.413	0.031
Observations	4,531	4,531	4,531	4,531	4,531	4,531
Adjusted $R^2$	0.002	0.002	0.000	0.014	0.018	0.005
Dependent Variables						
Strict Callbacks (Callback Category 1)	✓			✓		
Basic Callbacks (Callback Category 2)		✓			✓	
Wage Request (Callback Category 3)			✓			✓
Controls						
Firm Controls				✓	✓	✓
Federal State FE				✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members and pro-union applicants. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview", callback category 2 "Any request of an employer" and callback category 3 "Request regarding wage expectations". The baseline controls are firm controls (collective agreement, contract type, immediately hiring and firm size), regional and sector fixed effects. Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3 presents estimates for union signals in the Resume (Old Design) and in Twitter (New Design). Revealing union membership in the resume replicates the previous findings, including the effect size. Revealing union membership in the resume, based on callback category 1 (Callback Category 2) leads to lower callbacks of 6.5 percentage points (6.2 percentage points), with little effect of control variables. Revealing a pro-union sentiment via social media (Twitter) accounts leads to lower callbacks based on callback category 1 (Callback Category 2) by 3.4 percentage points (3.7 percentage points), which is significant at the 5% level. As not all human resource managers will look at the Twitter account for information, the estimates only cover an intent-to-treat effect. The effect size is therefore a lower bound for the true extent of discrimination. With the significant lower callbacks in a subtle social media treatment, the results rule out a potential bias related to the use of voluntary activities and memberships.

The last remaining threat to interpretation is that hiring discrimination could be driven by perceived differences in reservation wages. Based on the newly created callback category 3, there is no evidence of differences in requests regarding wage expectations between union and non-union members (Table 3 Columns 3 and 6). This suggests that the observed hiring discrimination is driven neither by information provision nor expected differences in reservation wages.

## 7 Mechanisms of Management Opposition

The previous section provides robust evidence for the existence of hiring discrimination against union supporters in response to both revealing union membership in the resume and revealing a pro-union sentiment via social media accounts. In the next step, I provide insights into the origins of management opposition in terms of hiring discrimination. To this end, Figure 6 illustrates the estimated sector-specific extent of hiring discrimination by sectors from the period 2017 to 2019. It shows that not all sectors respond equally strongly to union memberships. Discrimination is lowest for the health sector and strongest among the public and energy sectors. This observation is particularly of interest given that it indicates higher levels of discrimination in sectors with high shares of union members and overall high wages, compared to weakly organized sectors. There is strong sectoral variation in hiring discrimination reaching up to 30 percentage points fewer callbacks by revealing a union membership in the resume. Yet there are various alternative explanations for these differences. These explanations include firm and vacancy characteristics such as firm size and the contract type (Permanent or Temporary), corporate legal forms, labor market conditions, unions bargaining power and strike activities. In what follows, I decompose these aspects and link them to the theory of union threat (Rosen, 1969), employers' fear of unionization, which leads to improvements in wages and working conditions.

If union threat effects can explain hiring discrimination against union supporters, I would expect the share of union members within a sector and firm size to matter. Baert and Omeij (2015) only provide evidence for the share of union members. If union threat effects also exist due to unions' strike activity, theory predicts that hiring discrimination will increase with the intensity of strikes.

In the following, I show that first, hiring discrimination increases with firm size and union density. Second, strikes have an effect on the extent of discrimination. Third, discrimination is associated with the coverage of collective agreements and the voluntary orientation on collective

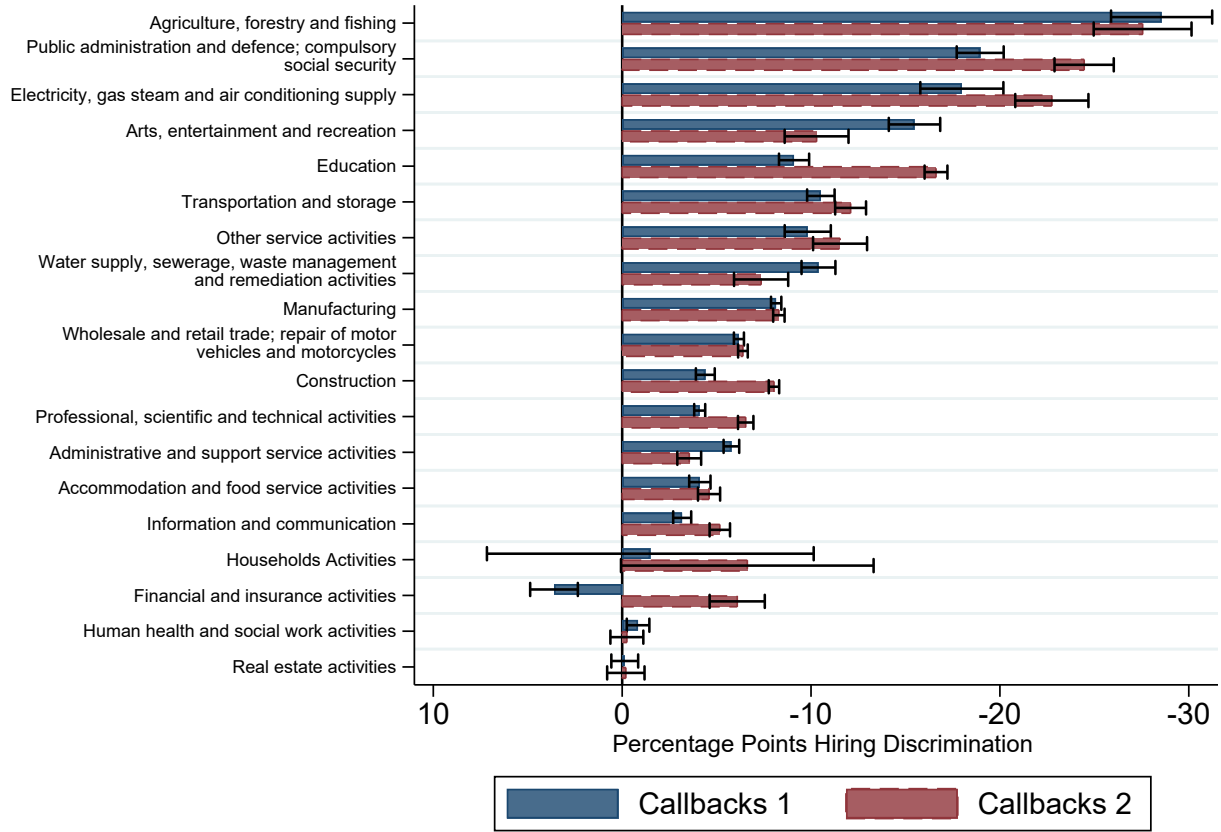


Figure 6: Hiring Discrimination by Sector (2017 to 2019)

*Note:* The figure provides the point estimates of sector-specific hiring discrimination for 2017 to 2019. The coefficient shows the percentage point difference in callbacks between union and non-union applicants. Callbacks are based on the Callback Category 1 "Invitation to a job interview" and Callback Category 2 "Any request of an employer" for union and non-union members. Sectors are ordered by the average effect size of Callback Category 1 and Callback Category 2 within sectors. Clustering at the sectoral level

agreement wage setting even in the absence of a collective agreement.

## 7.1 Management Opposition Heterogeneity

The extent of discrimination may vary with the employer's and applicant's respective bargaining position. In this section, I determine whether there is heterogeneity in hiring discrimination by firm and vacancy characteristics. To capture this heterogeneity, I interact union membership with the existence of a collective agreement (Yes/No), contract type (Temporary Contract, Yes/No), the regional occupation-specific labor market tightness (V/U Ratio), and firm size as well as sectoral share of union members, considering the share of current union members (0 to 1). Equation 3 mirrors Equation 1 augmented by interactions for firm and labor market characteristics.

$$Callback_i = \alpha + \tau Union_i + \mathbf{X}'_{it}\beta + \gamma Union_i \times \mathbf{X}'_{it} + \epsilon_{it} \quad (3)$$

For several dimensions of heterogeneity, the direction of the effects are theoretically ambiguous. For temporary contracts, I expect to observe lower discrimination since temporarily

employed workers are easier to dismiss. The uncertainty of temporary employment can be used as a tool to discipline employees, regardless of their union membership. The expected effect of labor market tightness given by the vacancy/unemployment-ratio (V/U Ratio) on hiring discrimination could increase or decrease hiring discrimination. On the one hand, in a tight labor market the opportunity costs of keeping a vacancy open are higher which might lead employers to ignore unpopular characteristics such as union membership. The reduction in discrimination can be observed in correspondence experiments on ethnic discrimination (Baert et al., 2015). On the other hand, tight labor markets strengthen unions' bargaining positions and employees' willingness to unionize (Pezold et al., 2023). This could amplify the negative signal of a union membership as is the case for the unemployment duration (Kroft et al., 2013; Nüß, 2018). Furthermore, union strike activity is strongly related to the business cycle (McConnell, 1990). If strikes increase the salience of unions' bargaining power, tight labor markets could increase discrimination against union supporters.

Given that unionization is more likely to occur in larger firms (Maranto, 1988; Dinlersoz et al., 2014; Farber, 2015) and that management opposition increases with firm size (Bronfenbrenner, 1997; Behrens, 2009; Aleks, 2015; McNicholas et al., 2019), I expect the threat of unionization and discrimination to increase with firm size as well. The literature related to union density (Blanchflower and Cubbin, 1986; Tsebelis and Lange, 1995; Jansen, 2014; Baert and Omeij, 2015) leads to the expectation that hiring discrimination increases in union density, the sectoral share of union members.

The last variable considered is whether a firm is covered by a collective agreement. On the one hand, firms with a collective agreement might discriminate more strongly against union supporters, because the regular wage negotiations make unions' bargaining power more salient and therefore more threatening. Due to the resulting higher wages of firms covered by a collective agreement, these firms are likely to attract more applicants (Abowd and Farber, 1982; Farber, 1992), which allows them to be more selective and to avoid applicants with less favorable characteristics. On the other hand, due to the collective agreement and resulting wages, employers may no longer fear the threat of potential wage increases. Finally, there is a possible explanation based on unions' strike activity and the legal context of Germany's bargaining system. During the duration of a collective agreement there is a peace obligation for unions. They are not allowed to strike, which constrains the otherwise potential imminent threat of a strike that firms without a collective agreement could fear to a limited period.

The results in Table 4 provide evidence for the relevance of firm and labor market characteristics for hiring discrimination. Columns 1 to 2 and 5 to 6 have controls for firm characteristics. The interaction of union membership with the presence of a collective agreement leads to a reduction in hiring discrimination by 3.5 percentage points and is statistically significant at the 5% level. Contrary to initial expectations, no specification in Table 4 provides evidence for the relevance of temporary contracts. In contrast to Baert and Omeij (2015), I find that hiring discrimination is increasing with firm size. This observation is in line with the expectations of the literature on management opposition and organizing. In a joint analysis of firm characteristics and labor market conditions (Columns 5 and 6) for firms with fewer than 6 employees,

Table 4: Determinants of Hiring Discrimination (2017 to 2019)

	(1)	(2)	(3)	(4)	(5)	(6)
Union	-0.0745*** (0.0099)	-0.0756*** (0.0104)	-0.0353*** (0.0122)	-0.0369*** (0.0128)	-0.0496*** (0.0141)	-0.0473*** (0.0150)
<b>Firm Characteristics</b>						
Union × Collective Agreement	0.0350** (0.0164)	0.0367** (0.0171)			0.0330** (0.0164)	0.0338** (0.0171)
Union × Temporary Contract	0.0015 (0.0159)	-0.0155 (0.0171)			-0.0007 (0.0160)	-0.0163 (0.0172)
Firm Size (Reference Category: 6 to 50 Employees)						
Union × Smaller 6 Employees	0.0374** (0.0189)	0.0515** (0.0202)			0.0342* (0.0189)	0.0482** (0.0203)
Union × 51 to 500 Employees	0.0059 (0.0146)	-0.0022 (0.0156)			0.0110 (0.0149)	0.0030 (0.0158)
Union × Larger 500 Employees	-0.0361 (0.0299)	-0.0704** (0.0320)			-0.0297 (0.0302)	-0.0643** (0.0322)
<b>Labor Market Conditions</b>						
Union × Union Density			-0.2334*** (0.0880)	-0.2807*** (0.0933)	-0.2198** (0.0900)	-0.2500*** (0.0945)
Union × V/U Ratio Standardized			0.0017 (0.0063)	0.0083 (0.0063)	0.0014 (0.0062)	0.0077 (0.0063)
Callback Rate Control Group	0.403	0.511	0.403	0.511	0.403	0.511
Observations	8,714	8,714	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.068	0.075	0.069	0.077	0.070	0.077
Dependent Variables						
Strict Callbacks (Callback Category 1)	✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓
Controls						
Baseline Controls	✓	✓	✓	✓	✓	✓
Firm Characteristics	✓	✓			✓	✓
Labor Market Conditions			✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the determinants for discrimination of union members. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer". The baseline controls are regional, occupation and year fixed effects. Firm characteristics cover collective agreement coverage, contract type and firm size. Labor market conditions cover sectoral union density, calculated based on the German Socio-Economic Panel (GSOEP) by Nace 2, and labor market tightness measured as Vacancy/Unemployment-Ratio standardized with mean 0 and SD 1. Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

discrimination is reduced to insignificance and an effect size of 0 to 1.5 percentage points.<sup>11</sup>

With respect to labor market conditions, an increase in sectoral share of union members is associated with higher discrimination. In line with the industrial relations literature and similar to the findings of Baert and Omey (2015), the results point to union density being an important driver of management opposition. There is no evidence of a link between discrimination and occupation-specific labor market tightness.

Further analysis in Appendix A-11 considers heterogeneity by the type of union the applicant was affiliated with (Table A-15) the corporate legal form (Table A-16), heterogeneity by the type of the collective agreement (Table A-17) and heterogeneity by the gender of the human resource manager (Table A-18). Neither the heterogeneity by union nor the type of collective agreement provides any distinct patterns. With respect to corporate legal forms heterogeneity analysis, the results indicate that almost all forms give evidence to hiring discrimination.

<sup>11</sup>A statistical test of the linear combination of the union dummy and the interaction term for firms with less than 6 employees provides p-values of 0.422 (Column 5) and 0.996 (Column 6) and therefore does not allow me to reject the null hypothesis of lower rates of invitations for union supporters in these firms.



One remaining limitation of this analysis is the absence of information on the existence of works councils; works councils which might be able to suppress employers' hiring discrimination against union supporters. However, the likelihood of having a works council is higher in larger firms, yet discrimination is increasing in firm size. Therefore, even when considering works councils, firm size seems to be of greater influence.<sup>12</sup>

Given the relevance of collective agreements, firm size and union density, these results support the idea that hiring discrimination is linked to unions' bargaining power and threat potential. To better understand union threat effects directly I examine the relevance of strikes on hiring discrimination.

## 7.2 The Link between Management Opposition and Strikes

While the previous research on union threat focused on the share of union members and right-to-work laws (Farber, 2005; Denice and Rosenfeld, 2018; Fortin et al., 2021; Taschereau-Dumouchel, 2020), I highlight the threat potential of strikes. For this reason, the experimental data are merged with federal employment agency data on union strike activity,<sup>13</sup> which allows me to distinguish sectoral strike activities between federal states. To account for strike intensity, I construct a variable of lost working days per 1,000 employees.

This variable is constructed in two versions. The first version covers the sectoral variation in strike activities (nation-wide strike activity within a sector). The second version contains sectoral-state variation in strike activities (strikes within a sector for each federal state).

Considering sectoral variation separately is based on the idea that the threat effect of strikes can exist independently of where a strike occurs. Figure 7 shows the intensity and distribution of strikes between 2017 and 2019. Based on this measure, 7,180 out of 8,714 observations (82.4%) of the experiment could be affected by strikes. While the strike intensity ranges from 0.02 to 57.8 lost working days per 1,000 employees, the sample mean is only 11.5, indicating an overall moderate level of strike intensity.

The sectoral-state variation strike variable accounts for the fact that strike activities within a sector might be concentrated in some federal states, and that the threat effect is only relevant within this federal states. For example, firms in the service sector in Hamburg (north of Germany) might be unimpressed by or even unaware of a strike in Bavaria (south of Germany). Considering this measure decreases the potentially affected observations in the sample to 4,458 out of 8,714 observations (51.2%). However, the strike intensity varies between 0.05 to 200.2 lost working days per 1,000 employees, with a sample mean of 18.2.

Equation 4 contains the linear probability model, with one of the callback categories as a dependent variable. The coefficient  $\tau$  is the average percentage point difference in callbacks between union and non-union members, in the absence of a strike. The coefficient  $\delta$  covers the overall effect of strike intensity on callbacks. The coefficient  $\gamma$  covers the effect of strike intensity

<sup>12</sup>Appendix A-14 provides additional analysis of opposition to works councils, based on the 2015 WSI Works Council Survey.

<sup>13</sup>Due to protection of firm data privacy, sectoral strike activity was aggregated to Nace 2 one-digit sector classification. The final data allow the use of 98.8% to 99.8% of the total variation in strike activity. For more details on the strike data quality see Appendix Table A-19.

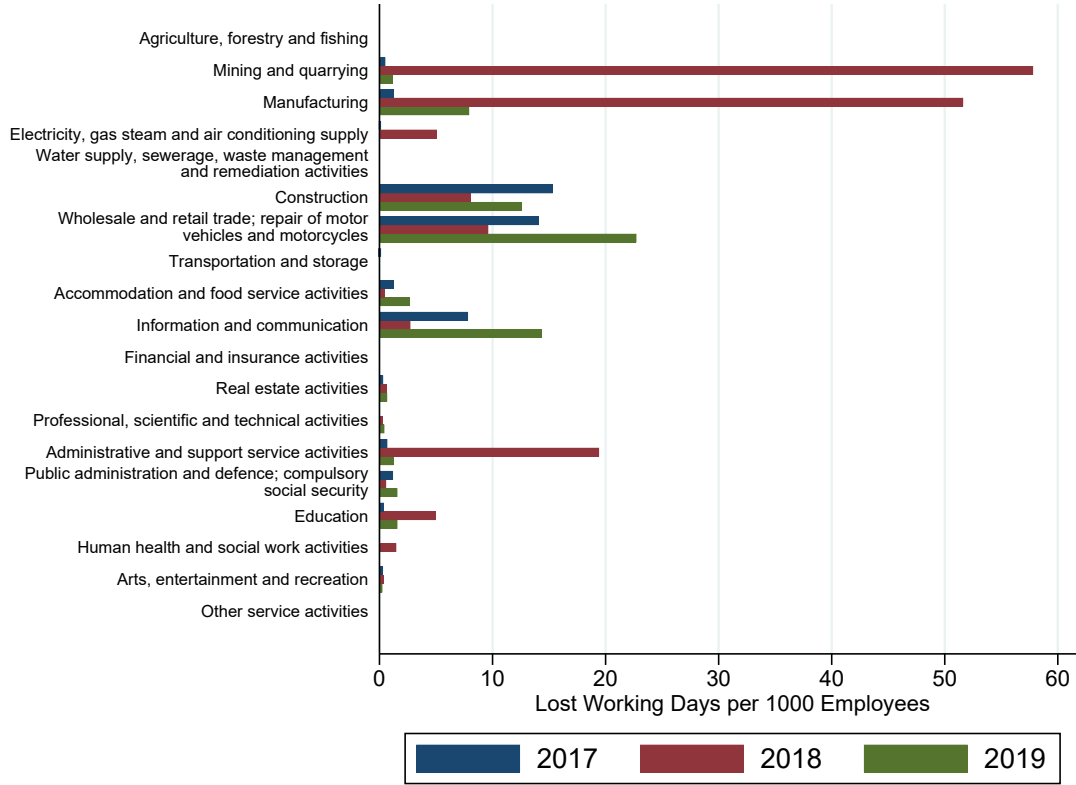


Figure 7: Sectoral Distribution of Strike Intensity (2017 to 2019)

Source: Statistics of the Federal Employment Agency; Own Calculations.

Note: The figure presents the intensity of unions strike activities for each sector and year. Lost work days per 1,000 employees, is constructed based on the number of lost work days due to a strike in a given year for each sector, divided by the number of employees for each sector in thousands.

(within a sector or within a sector-state) on the probability of a positive callback for a union member.

$$Callback_{it} = \alpha + \tau Union_i + \delta Strikes_{it} + \gamma Union \times Strikes_{it} + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (4)$$

The two panels of Table 5 report the results depending on strike intensity based on the full sample (Columns 1 to 4) and a sub-sample restricted to firms not covered by a collective agreement (Columns 5 to 8). The effect of strikes provides a negative coefficient for all specifications in the full sample. It is only a statistical significance at the 10% significance level, for callback category 1 using state and sector variation in strike intensity. The results therefore provide only limited evidence for the relevance of strike activity for hiring discrimination.

In the final step, I exclude firms with a collective agreement from the sample to concentrate on firms that are unlikely to be exposed to a strike (Table 5 Columns 5 to 8). A strike in a firm covered by a collective agreement, could affect firms without collective agreement coverage. Excluding firms covered by a collective agreement allows me to analyze potential spillover effects on firms not covered by a collective agreement (Table 5). Given that strikes mainly take place in firms with a collective agreement, it is plausible to assume that strike activities are an exogenous source of variation of union threat on firms without a collective agreement. This allows me to test for spillover effects of union strike activity in organized firms within the entire sector.

Interestingly, there is a statistically significant effect of strike activity on hiring discrimination at the 5% significance level for both strike measures for the use of state and sector variation in strike activity for callback category 1 but no statistically significant effect when considering callback category 2. The results are consistent with the idea that strikes have spillover effects on other firms, even those not currently covered by a collective agreement, with higher hiring discrimination as a consequence.

Table 5 yields two main results regarding to management opposition towards union supporters. As before, there is clear evidence for management opposition in terms of hiring discrimination of union supporters. There is only weak support of the relevance of strike activities as a driver for hiring discrimination. Back-of-the-envelope calculations suggest that strike activity can account for a maximum of 10% of the total extent of discrimination.<sup>14</sup> Concluding that even when unions' strike activities in Germany has union threat potential that lead to hiring discrimination and given the overall low level of strike activity, this channel is at best only of minor importance in Germany's current industrial relations environment.

Table 5: Strikes Effects on Hiring Discrimination

	Full Sample				Firms without a Collective Agreement			
	Sector Variation		State and Sector Variation		Sector Variation		State and Sector Variation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union	-0.0582*** (-8.57)	-0.0646*** (-8.13)	-0.0573*** (-10.02)	-0.0655*** (-8.28)	-0.0626*** (-9.32)	-0.0690*** (-7.75)	-0.0617*** (-10.55)	-0.0691*** (-7.83)
Lost Working Days per 1,000 Employees (Sec)	-0.0006 (-1.42)	-0.0005 (-1.23)			-0.0006 (-1.65)	-0.0005 (-1.24)		
Union × Lost Working Days per 1,000 Employees (Sec)	-0.0005 (-1.54)	-0.0005 (-1.27)			-0.0006** (-2.56)	-0.0005 (-1.40)		
Lost Working Days per 1,000 Employees (Sec State)			-0.0006** (-2.48)	-0.0005 (-1.47)			-0.0006*** (-3.56)	-0.0005 (-1.53)
Union × Lost Working Days per 1,000 Employees (Sec State)			-0.0006* (-1.67)	-0.0005 (-1.45)			-0.0007** (-2.10)	-0.0006 (-1.60)
Callback Rate Control Group	0.403	0.511	0.403	0.511	0.404	0.512	0.404	0.512
Observations	8,714	8,714	8,714	8,714	7,006	7,006	7,006	7,006
Adjusted $R^2$	0.075	0.086	0.075	0.086	0.069	0.080	0.070	0.080
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer". The baseline controls are regional, occupation and year fixed effects. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Strike activities are measured as sectoral variation in strike activities (nation-wide strike activity within a sector) and sectoral-state variation in strike activities (strikes within a sector for each federal state). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 7.3 Union Threat and its Consequences for Germany's Industrial Relations

In this section I address the skepticism regarding the external validity of correspondence experiments. I do this by linking the previous measured hiring discrimination to the firms coverage by collective agreements. I provide evidence that hiring discrimination is strongly linked to the coverage by collective agreement of a sector.

<sup>14</sup>With a mean strike intensity of 10 lost work days per 1,000 employees over the sample period, discrimination against union supporters increases by 0.6 percentage points ( $0.0006 \times 10$ ), leading to an increase in discrimination of  $\frac{0.0715 - 0.0655}{0.0655} = 0.091$ .

Unions' collective agreements can have spillover on non-unionized firms for several reasons. One explanation is that collective agreements can create norms about fair wages ([Western and Rosenfeld, 2011](#)). If unions affect fairness norms, this can increase turnover and reduce the number of applicants for non-unionized firms. This reduction can force non-unionized firms to orientate wage setting on existing collective agreements. Another explanation is that collective agreements reduce information frictions for employees ([Demir, 2022](#); [Bassier, 2022](#)). Employees that are not aware of wages in outside options, are less likely to bargain over wages or search for another job ([Jäger et al., 2024](#)). Collective agreements can reduce information frictions for employees by providing information about outside options to their current job. In both explanations, collective agreements effect on wage norms and information frictions, the consequence is the same. Firms orientate wages on collective agreements to reduce turnover.

A last explanation for spillover effects of collective agreements are union threat effects ([Rosen, 1969](#)). Firms that are aware of the threat of potential unionization are willing to improve wages and working conditions to discourage employees from joining a union. For this reason, union threat theory predicts wage differences between unionized and non-unionized firms to be lower when firms feel the threat of unionization. Translated into the German industrial relations system, this means a high union threat coincides with a high sectoral collective agreement coverage. If firms are not covered by a collective agreement, in the presence of a high union threat, firms without a collective agreement are more willing to voluntarily orientate on collective agreements in wage setting. The reverse holds for a low union threat: in the absence of a union threat, collective agreement coverage will be low, with little incentive for firms to voluntarily honor collective agreements in their wage-setting process.

Figure 8 tests the relevance of union threat effects for the sectoral coverage of collective agreements. Based on the experimental results and the sectoral collective agreement coverage provided by [Ellguth and Kohaut \(2019\)](#), Figure 8 shows the association between hiring discrimination and collective agreement coverage and the voluntary orientation on collective agreement wage setting.<sup>15</sup> Figure 8 shows the association of sector-specific hiring discrimination with the sectoral share of collective agreements (Panel 8a and 8b) and the share of firms that do not voluntarily orientate on collective agreement wage setting (Panel 8c and 8d). There is a strong correlation of sectoral coverage of collective agreements and hiring discrimination reaching from  $-0.55$  to  $-0.72$ . Sectors with the lowest measures of discrimination are the sectors with the lowest share of collective agreements. I find a similar pattern for firms that are not part of a collective agreement; the higher the share of firms not following collective agreements the lower discrimination is.

While several explanations for the spillover effects of collective agreements exist, only the union threat theory predicts the association between hiring discrimination and collective agreement coverage. It further supports the idea that management opposition in terms of hiring discrimination against union supporters reflects unions' bargaining power within a sector. However, Appendix A-13 indicates that the association of discrimination and the share of (voluntarily orientation on) collective agreements is strongly affected by the observed outliers of the agriculture and public sector.

<sup>15</sup>Sectoral hiring discrimination is reclassified and calculated, according to the reduced sectoral classification of the IAB Establishment panel used by [Ellguth and Kohaut \(2019\)](#).

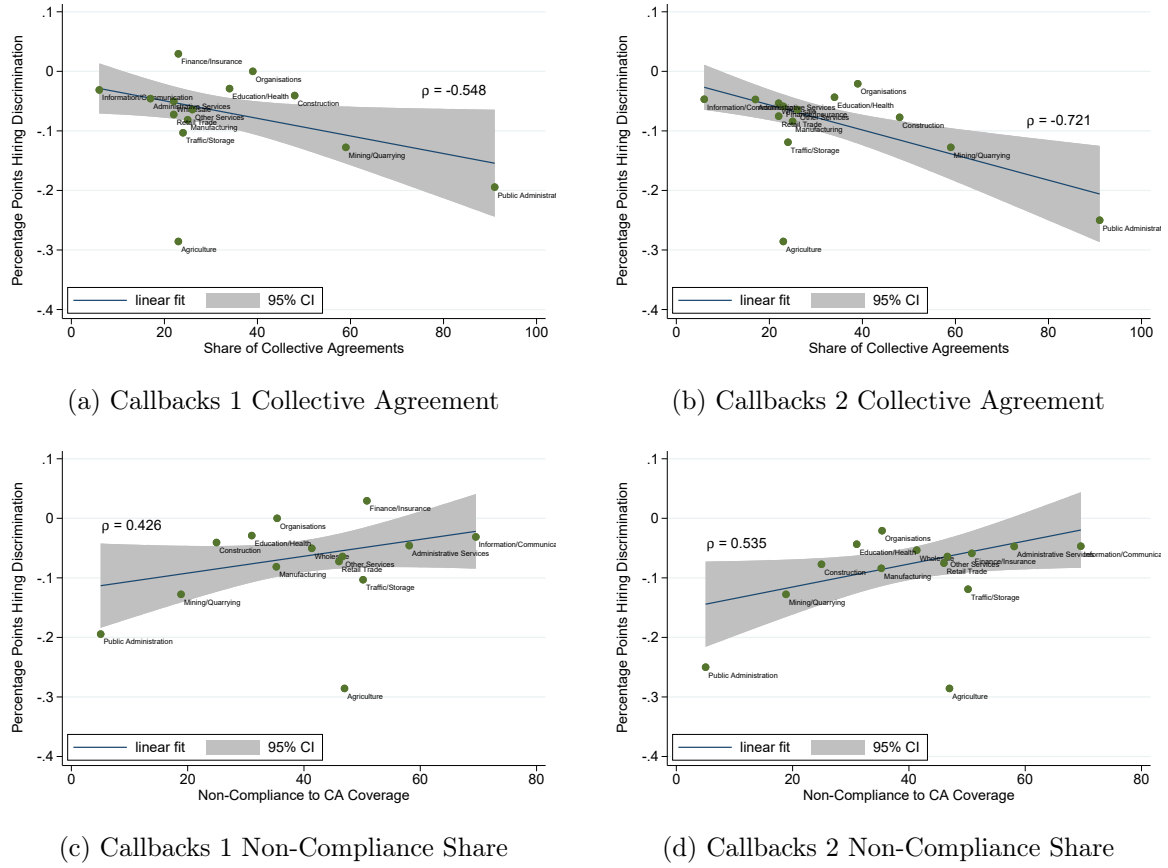


Figure 8: Sectoral Hiring Discrimination and Collective Bargaining Coverage

*Note:* This figure presents the correlation of collective agreement coverage and voluntary orientation on collective agreements with the sectoral extent of discrimination. Hiring discrimination is measured in terms of Callback Category 1 "Invitation to a job interview" and Callback Category 2 "Any request of an employer" by IAB sector classification. Collective agreement coverage of firms based on calculations of the IAB Establishment Panel for 2018 by [Ellguth and Kohaut \(2019\)](#). Weighted by sectoral employment share.

## 8 Discussion

I find evidence for widespread employer discrimination against union supporters in Germany, particularly in larger firms and strongly organized sectors. My results, together with the evidence of hiring discrimination by [Baert and Omev \(2015\)](#) in Belgium are in contrast to the absence of evidence of hiring discrimination by [Kreisberg and Wilmers \(2022\)](#) for the United States. In the following, I argue that these differences can be explained by differences in labor market institutions and union's influence.

[Freeman and Kleiner \(1990\)](#) point out that management opposition against unionization is driven by three factors: 1. the costs related to management opposition, 2. the probability of a successful unionization drive and 3. the costs of successful unionization. By design, correspondence experiments on hiring discrimination provide a low-cost opportunity for opposition and therefore can not explain differences in the extent of discrimination. Furthermore, the costs of successful unionization exist in all countries. With successful unionization and negotiation of a collective agreement, the union wage premium increases the labor share and reduces profits. In contrast, probability of successful unionization strongly depends on existing labor market insti-

tutions. Germany and Belgium have strong employment protection as well as unemployment benefits. These factors reduce the risk of negative consequences of a unionization attempt and make management opposition after hiring employees more difficult. In the United States employees are more vulnerable to the risks associated with unionization, due to the low level of employment protection and unemployment benefits. In this context, employers' use of illegal actions to prevent unionization is widespread (McNicholas et al., 2019), and there are only limited financial consequences when employers are found guilty of the use of unfair labor practices (Stansbury, 2021).<sup>16</sup> Therefore, the results by Kreisberg and Wilmers (2022) are likely to be the result of unfavorable institutions and an era of overall weak unions and worker power (Stansbury and Summers, 2020).

Consistent with the argument of weak unions, my results show that hiring discrimination against union supporters is lower in firms covered by a collective agreement and increases with union density. Hiring discrimination is strongest in firms that have no collective agreement but that are placed in a sector where most other firms are organized and are covered by a collective agreement. These results are in line with the observation by Freeman (1986) and by Wang and Young (2022) that management opposition increases with the wage differential of union and non-union wages.

The internal validity of correspondence experiments is often criticized since they only cover the first stage in the hiring process. However, when firms wish to avoid union members, the easiest way to do so would be to avoid them in the first stage when preventive interventions are easiest. This also allows us to identify employers' attitudes of small firms which otherwise would not be possible to compare relative to the action of larger firms (see Gall, 2004). I improve the external validity of the experiment by revealing a pro-union sentiment via social media accounts, with sensitivity tests for regional and occupational weighting and consider the Heckman Critique. Furthermore, external validity is improved by evidence of sector-specific hiring discrimination and its association with patterns of Germany's industrial relations system, namely the collective agreements coverage and the voluntary orientation on union wage setting.

The experiment concentrates on the analysis of determinants related to unions' bargaining power and threat potential. This does not rule out the existence of additional drivers for discrimination against union members, such as discrimination due to their political orientation (Moens et al., 2024), management's ideology (Dundon, 2002), and employers' expectations of higher sickness absence rates (Goerke and Pannenberg, 2015). While union members might be expected to be on the left of the political spectrum, the voting behavior of the average union member is close to the general population (see Table A-26). Research on employers' hiring behavior related to the political orientation of applications further indicates that negative attributes are strongest associated with the right end of the political spectrum (Moens et al., 2024). If managements' ideology is of high relevance, we would expect strong variation in discrimination by corporate legal form. However, the experiment provides little evidence for heterogeneity in discrimination related to the corporate legal form (see Table A-16). The last remaining aspect is employers' expectations of higher sickness absence rates of union members, which can not be ruled out in the experiment and can be an additional explanation for discrimination. Yet, this is an additional

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<sup>16</sup>This is a pattern that can be shown to hold for US labor law violations in general (Marinescu et al., 2021).

explanation for discrimination, which itself can not explain the patterns of discrimination related to unions' bargaining power and the voluntary orientation of collective agreements.

All applicants of the experiment are male, 28 years old, with vocational training. The existence of gender discrimination (Goldin and Rouse, 2000) as well as age discrimination (Neumark et al., 2019) is well known. Unions provide legal support and fight for equal opportunity, which might affect hiring decisions for female or older workers with union backgrounds more strongly. Furthermore, the results indicate that the main driver for discrimination is union threat potential. Due to larger gains of unionization, the theory of union threat would predict stronger discrimination for women and to increase with age, which would imply that is the measured extent of discrimination against young male workers is a lower bound.

Several correspondence experiments have been conducted that consider labor market tightness as a driver for discrimination. Such evidence exists for ethnic discrimination (Baert et al., 2015) and for unemployment stigmatization (Kroft et al., 2013; Nüß, 2018). As such, one must exercise caution when generalizing findings of correspondence experiments beyond the particular time and place of the experiment in question. Compared to the previous literature based on pure cross-section data, my main experiment covers a period of three years, which allows to control for changes in labor market conditions, replicating the existence of hiring discrimination for each wave.

## 9 Conclusion

In this paper, I conduct the first-ever large-scale correspondence experiment to estimate a representative measure of management opposition in terms of hiring discrimination against union supporters. I send 13,000 fictitious job applications, that reveal union membership in the resume and a pro-union attitude via social media accounts. With this experiment, I provide robust evidence for management opposition in terms of hiring discrimination in the German labor market. Because of Germany's institutional separation of union voice (works councils) and wage bargaining (unions), I can focus on employers' attitudes toward unions' activities to improve wages and working conditions.

A correspondence experiment on management opposition avoids potential systematic bias in reports of management opposition, selection problems resulting from unions organizing attempts as well as unobserved determinants of union memberships and organizing attempts. Furthermore, the experiment provides employers an easy opportunity to discriminate against union members. As such, this correspondence experiment can determine how widespread management opposition against unions is in an economy.

I provide evidence for management opposition in terms of hiring discrimination against union supporters in Germany with strong variation based on firm and sector characteristics. I find no evidence for union-based hiring discrimination in firms with fewer than 6 employees. For larger firms, hiring discrimination exists and increases with firm size. Discrimination increasing with firm size goes against potential concerns about an underestimation of existing management opposition and supports the idea that larger firms are the main venue of union-employee conflicts. These results indicate that employers' discrimination of union supporters is linked to employers'



attitudes toward unions.

My results are in line with recent correspondence experiments by [Baert and Omey \(2015\)](#) and [Kreisberg and Wilmers \(2022\)](#). In contrast to [Kreisberg and Wilmers \(2022\)](#), the results provide weak evidence that unions strike activity during collective bargaining has spillover effects increasing union threat within a sector/region. However, strikes are of minor importance for the total extent of hiring discrimination given Germany's overall low level of strike activity. The results are consistent with the theory of union threat and how the absence of union threat effects is an important reason for companies to leave collective agreements. The absence of a union threat motivates firms to exit collective agreements and also leads to weaker voluntary orientation on collective agreements in the long run. Union threat potential can temporarily mitigate the negative effects on wages and working conditions of eroding industrial relations, which would delay its impact on labor market inequality.

While most attempts of management opposition in particular hiring discrimination against union supporters are of illegal nature, the results suggest that management opposition is a symptom of a well-functioning industrial relations system where unions are recognized as influential. In sectors without employers perceiving unions as influential opponents (for example, in small firms and sectors with low union density), firms are more likely to leave collective agreements and also stop voluntarily adhering to collective agreements in their wage setting. Therefore, the absence of hiring discrimination in some sectors (for example, the health sectors) in Germany, in Belgium ([Baert and Omey, 2015](#)) and the United States in general ([Kreisberg and Wilmers, 2022](#)) suggest worsening wages and working conditions in the future.

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# Appendix of Management Opposition, Strikes and Union Threat

Patrick Nüß

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## A-1 Callback Classification

To analyze employers' interest in the applicants, all responses were categorized into one of two callback categories. These callback categories are based on the two standard definitions in the literature. Positive callbacks in a strict sense (Callback Category 1) include only explicit invitations to a job interview. Positive callbacks in a broad sense (Callback Category 2) augment the first definition by including every request for more information, requests related to wages, requests for alternative jobs, and requests to get in contact with the potential employer. The following provides a list of examples of employer responses for both callback categories.

- **Callback Category 1:** This category covers all callbacks of firms that clearly state an invitation to a job interview.

**Examples:**

- “We invite you to a job interview on January 1st, 2:00pm at...”
- “Please call me to arrange a job interview...”

- **Callback Category 2:** In addition to all positive responses of callback category 1, this callback category covers all responses of firms that stated any kind of interest in the applicant.

**Examples:**

- “Before we arrange a job interview, we have some remaining questions...”
- “Thanks for your application, please call me to talk about your application...”
- “To proceed with your application, please fill out the following document...”
- “We would like to have additional certificates of...”
- “We already filled the mentioned position but we would like to ask you for your interest in this other vacancy...”
- “Given your profile, we also see you in another job that we are currently trying to fill...”
- “You applied for a position a month ago and we just have another free position that might be of interest to you...”
- “We missed to include some important information in the vacancy. Are you still interested in the job if...”
- “Thanks for your interest in our open position, please visit us between 10:00am and 4:00pm over the week and ask for...”
- “Thanks for your application, could you tell us more about your salary expectations...”

## A-2 Summary Statistics of Experiment 1

Table A-1: Summary Statistics Experiment 1

	Mean	SD	Min	Max	N
<b>Callbacks</b>					
Callback Category 1	0.37	0.48	0.00	1.00	8,714
Callback Category 2	0.48	0.50	0.00	1.00	8,714
<b>Union Membership</b>					
Yes	0.50	0.50	0.00	1.00	8714
<b>Occupation</b>					
Hotel Manager	0.13	0.34	0.00	1.00	8,714
Logistics Specialist	0.24	0.42	0.00	1.00	8,714
Mechatronics Technician	0.13	0.33	0.00	1.00	8,714
Office Clerk	0.32	0.47	0.00	1.00	8,714
Sales Assistant	0.19	0.39	0.00	1.00	8,714
<b>Federal State</b>					
Baden-Wuerttemberg	0.14	0.35	0.00	1.00	8,714
Bavaria	0.18	0.38	0.00	1.00	8,714
Berlin	0.19	0.40	0.00	1.00	8,714
Hamburg	0.20	0.40	0.00	1.00	8,714
North Rhine-Westphalia	0.19	0.39	0.00	1.00	8,714
Saxony	0.10	0.30	0.00	1.00	8,714
<b>Year</b>					
2017	0.24	0.43	0.00	1.00	8,714
2018	0.38	0.48	0.00	1.00	8,714
2019	0.39	0.49	0.00	1.00	8,714
<b>Human Resource Manager</b>					
Male	0.44	0.50	0.00	1.00	8,714
Female	0.51	0.50	0.00	1.00	8,714
Unknown	0.05	0.22	0.00	1.00	8,714
<b>Vacancy Information</b>					
Immediate Need	0.73	0.45	0.00	1.00	8,714
<b>Collective Agreement</b>					
Collective Agreement	0.20	0.40	0.00	1.00	8,714
<b>Contract Type</b>					
Temporary	0.20	0.40	0.00	1.00	8,714
Permanent	0.67	0.47	0.00	1.00	8,714
Unknown	0.13	0.34	0.00	1.00	8,714
<b>Firm Size</b>					
< 6 Emp.	0.12	0.33	0.00	1.00	8,714
6 to 50 Emp.	0.48	0.50	0.00	1.00	8,714
51 to 500 Emp.	0.34	0.47	0.00	1.00	8,714
500 Emp. <	0.05	0.23	0.00	1.00	8,714
<b>Labor Market Conditions</b>					
Vacancy/Unemployment Ratio	0.43	0.51	0.09	2.64	8,714
Union Density (SOEP)	0.12	0.08	0.00	0.28	8,714
<b>Strikes</b>					
Lost Work Days per 1,000 Emp. (Sector)	10.62	16.24	0.00	61.00	8,714
Lost Work Days per 1,000 Emp. (State/Sector)	9.87	21.11	0.00	226.45	8,714

*Note:* This table reports the summary statistics of the first experiment, revealing union membership in the CV for 2017 to 2019.

Table A-2: Probability of Unequal Treatment due to Union Membership

	Number of Applications	Number of Firms	Neither (1)	At Least One (2)	Both (3)	Non-Union (4)	Union (5)	Net Discrimination	Relative Callbacks
<b>Callback Category 1</b>									
Overall	8,714	4,357	0.570	0.430	0.251	0.115	0.064	0.118	1.16
2017	2,082	1,041	0.622	0.379	0.234	0.103	0.041	0.162	1.22
2018	3,274	1,637	0.513	0.487	0.317	0.114	0.056	0.121	1.16
2019	3,358	1,679	0.516	0.484	0.278	0.136	0.069	0.139	1.19
<b>Callback Category 2</b>									
Overall	8,714	4,357	0.442	0.558	0.345	0.135	0.078	0.102	1.14
2017	2,082	1,041	0.504	0.496	0.306	0.136	0.054	0.167	1.23
2018	3,274	1,637	0.414	0.586	0.340	0.123	0.064	0.100	1.13
2019	3,358	1,679	0.381	0.619	0.393	0.150	0.076	0.119	1.16

*Note:* The table reports the extent of discrimination when only accounting for firms that responded to applications. Net Discrimination is calculated based on the difference of callbacks of union and non-union members, divided by all firms that responded to any application  $\frac{(4)-(5)}{(2)}$ . Relative Callbacks are calculated by firms inviting the non-union members and both applicants, divided by firms only inviting the union member and both applicants  $\frac{(3)+(4)}{(3)+(5)}$ .

### A-3 Representativeness of the Sample

Appendix A-3 provides an overview of the representativeness of the experimental sample. Table A-3 provides insight into the share of the chosen occupations in the general population. With a percentage share of 31.1% the experiment accounts for a sizable share of occupations in the German economy. In addition, Table A-4 shows that the chosen federal states account for about two third of economic activities in terms of gross domestic product (GDP) as well as employment.

Table A-3: Occupation Share on Overall Workforce Germany (2017 to 2019)

Occupation (KldB 2010 Classification)	2017	2018	2019
Mechatronics Technician (26)	1,016,754	1,037,547	1,047,405
Logistics Specialist (51)	1,983,408	2,068,757	2,099,456
Sales Assistant (62)	2,108,518	2,115,039	2,119,702
Hotel Manager (63)	791,216	810,051	832,250
Office Clerk (71)	4,115,097	4,198,279	4,273,455
Sum of Used Occupations	10,014,993	10,229,673	10,372,268
Total Workforce	32,164,973	32,870,228	33,407,262
Percentage Share	31.1%	31.1%	31.0%

*Source:* Statistics of the Federal Employment Agency. Beschäftigte nach Berufen (KldB 2010) (Zeitreihe Quartalszahlen). Own Calculations based on the second quarter of each year.

*Note:* The table reports the share of each occupation considered in the experiment for the workforce. Occupations are counted based on the two-digit occupational classification (KldB 2010).

Table A-4: Representativeness of the Sample by Federal State (2017 to 2019)

	Experiment		Population	
	Raw Data	Percent	Employment Share	GDP Share
<b>Average 2017 to 2019</b>				
Baden-Wurttemberg	1,244	14.28%	22.17%	23.37%
Bavaria	1,548	17.76%	26.33%	27.79%
Berlin	1,696	19.46%	6.97%	6.58%
Hamburg	1,712	19.65%	4.60%	5.37%
North Rhine-Westphalia	1,632	18.73%	32.34%	31.22%
Saxony	882	10.12%	7.59%	5.68%
<b>2017</b>				
Baden-Wurttemberg	0	0	0	0
Bavaria	580	27.86%	37.53%	39.34%
Berlin	540	25.94%	9.81%	9.10%
Hamburg	494	23.73%	6.53%	7.54%
North Rhine-Westphalia	468	22.48%	46.13%	44.02%
Saxony	0	0	0	0
<b>2018</b>				
Baden-Wurttemberg	628	19.18%	22.02%	23.19%
Bavaria	486	14.84%	26.42%	27.86%
Berlin	604	18.45%	6.98%	6.57%
Hamburg	586	17.90%	4.60%	5.37%
North Rhine-Westphalia	566	17.29%	32.40%	31.39%
Saxony	404	12.34%	7.58%	5.62%
<b>2019</b>				
Baden-Wurttemberg	616	18.34%	21.97%	23.06%
Bavaria	482	14.35%	26.39%	27.84%
Berlin	552	16.44%	7.08%	6.74%
Hamburg	632	18.82%	4.63%	5.42%
North Rhine-Westphalia	598	17.81%	32.43%	31.29%
Saxony	478	14.23%	7.49%	5.63%

*Source:* Statistics of the Federal Employment Agency; Federal Statistical Office of Germany; Own calculations.

*Note:* The table reports the distribution of experimental observations at the state level and compares them with their relative relevance in terms of employment and gross domestic production (GDP).

Table A-5: Representatives of the Sample by Occupation (2017 to 2019)

	Experiment		Population
	Raw Data	Percent	Employment Share
<b>Average 2017 to 2019</b>			
Hotel Manager	1,132	12.99%	7.92%
Logistics Specialist	2,058	23.62%	20.17%
Mechatronics Technician	1,108	12.72%	10.29%
Office Clerk	2,786	31.97%	40.97%
Sales Assistant	1,630	18.71%	20.66%
<b>2017</b>			
Hotel Manager	382	18.35%	8.78%
Logistics Specialist	578	27.76%	22.20%
Mechatronics Technician	0	0%	0
Office Clerk	564	27.09%	45.62%
Sales Assistant	558	26.80%	23.39%
<b>2018</b>			
Hotel Manager	414	12.65%	7.89%
Logistics Specialist	716	21.87%	20.28%
Mechatronics Technician	554	16.92%	10.24%
Office Clerk	1,058	32.32%	40.95%
Sales Assistant	532	16.25%	20.63%
<b>2019</b>			
Hotel Manager	336	10.01%	7.80%
Logistics Specialist	764	22.75%	20.33%
Mechatronics Technician	554	16.50%	10.17%
Office Clerk	1,164	34.66%	41.11%
Sales Assistant	540	16.08%	20.39%

*Source:* Statistics of the Federal Employment Agency; Own data.

*Note:* The table reports the relative share of experimental observations at the occupational level and compares them with their relative relevance in terms of employment.

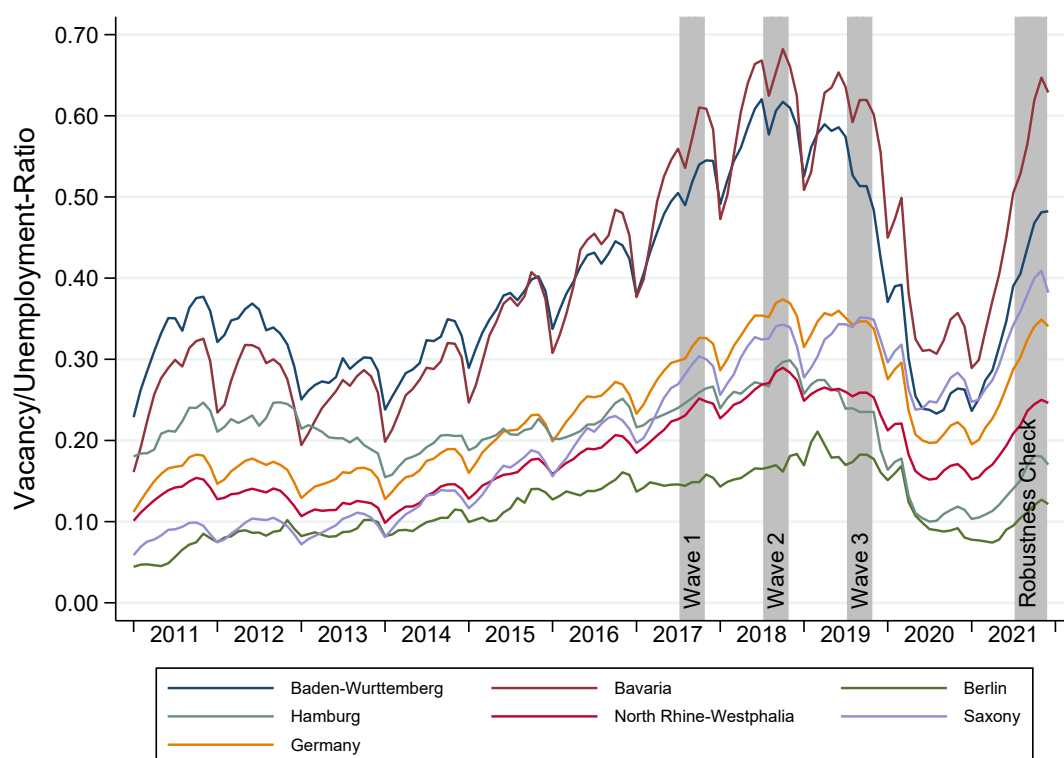


Figure A-1: Regional Labor Market Tightness over Time

*Source:* Statistics of the Federal Employment Agency: Arbeitsmarkt in Zahlen - Arbeitsmarktstatistik - Arbeitsmarkt nach Berufen. Own calculations.

*Note:* Monthly data. The shaded areas indicate the implementation period of the experiment. Wave 1 from August to October 2017; Wave 2 from August to October 2018; Wave 3 from August to October 2019. Robustness Check from August to November 2021.

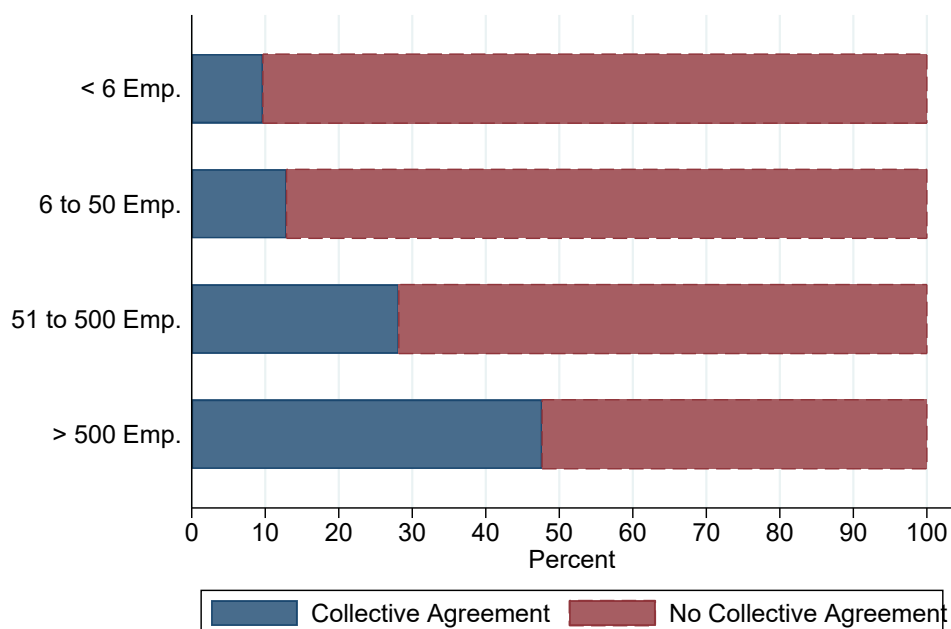


Figure A-2: Collective Agreement Coverage by Firm Size of the Experiment

*Source:* Own Data, covering all three experimental waves from 2017 to 2019.

*Note:* The figure presents the distribution of the collective agreement coverage of the experimental data, collected from 2017 to 2019.

## A-4 Randomization Checks Wave 2017 to 2019

Table A-6: Randomization Check (2017)

	No Membership		Union Membership		Difference	
	Mean	SD	Mean	SD	$\Delta$	t
<b>Name</b>						
Name 1	0.509	0.500	0.491	0.500	0.018	(0.83)
Name 2	0.491	0.500	0.509	0.500	-0.018	(-0.83)
<b>Picture</b>						
Face 1	0.490	0.500	0.510	0.500	-0.020	(-0.92)
Face 2	0.510	0.500	0.490	0.500	0.020	(0.92)
<b>Layout</b>						
Layout 1	0.515	0.500	0.485	0.500	0.030	(1.36)
Layout 2	0.485	0.500	0.515	0.500	-0.030	(-1.36)
<b>School</b>						
School 1	0.513	0.500	0.487	0.500	0.026	(1.18)
School 2	0.487	0.500	0.513	0.500	-0.026	(-1.18)
<b>Address</b>						
Address 1	0.514	0.500	0.486	0.500	0.028	(1.27)
Address 2	0.486	0.500	0.514	0.500	-0.028	(-1.27)
<b>Membership</b>						
Membership 1	0.510	0.500	0.490	0.500	0.020	(0.92)
Membership 2	0.490	0.500	0.510	0.500	-0.020	(-0.92)
<b>Apprenticeship</b>						
Company 1	0.488	0.500	0.512	0.500	-0.024	(-1.10)
Company 2	0.512	0.500	0.488	0.500	0.024	(1.10)
<b>Last Employment</b>						
Company 1	0.494	0.500	0.506	0.500	-0.012	(-0.57)
Company 2	0.506	0.500	0.494	0.500	0.012	(0.57)
Observations	1,041		1,041		2,082	

*Note:* The table provides details of the distribution of randomly assigned application characteristics between the treatment (Union Members) and control group (Non-Union Members). The t-test for mean differences by subcategories tests for the success of the randomization. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A-7: Randomization Check (2018)

	No Membership		Union Membership		Difference	
	Mean	SD	Mean	SD	$\Delta$	t
<b>Name</b>						
Name 1	0.506	0.500	0.494	0.500	0.012	(0.66)
Name 2	0.494	0.500	0.506	0.500	-0.012	(-0.66)
<b>Picture</b>						
Face 1	0.509	0.500	0.491	0.500	0.018	(1.01)
Face 2	0.491	0.500	0.509	0.500	-0.018	(-1.01)
<b>Layout</b>						
Layout 1	0.495	0.500	0.505	0.500	-0.009	(-0.52)
Layout 2	0.505	0.500	0.495	0.500	0.009	(0.52)
<b>School</b>						
School 1	0.486	0.500	0.514	0.500	-0.029	(-1.64)
School 2	0.514	0.500	0.486	0.500	0.029	(1.64)
<b>Address</b>						
Address 1	0.498	0.500	0.502	0.500	-0.004	(-0.24)
Address 2	0.502	0.500	0.498	0.500	0.004	(0.24)
<b>Membership</b>						
Membership 1	0.488	0.500	0.512	0.500	-0.024	(-1.36)
Membership 2	0.512	0.500	0.488	0.500	0.024	(1.36)
<b>Apprenticeship</b>						
Company 1	0.508	0.500	0.492	0.500	0.016	(0.94)
Company 2	0.492	0.500	0.508	0.500	-0.016	(-0.94)
<b>Last Employment</b>						
Company 1	0.486	0.500	0.514	0.500	-0.029	(-1.64)
Company 2	0.514	0.500	0.486	0.500	0.029	(1.64)
Observations	1,637		1,637		3,274	

*Note:* The table provides details of the distribution of randomly assigned application characteristics between the treatment (Union Members) and control group (Non-Union Members). The t-test for mean differences by subcategories tests for the success of the randomization. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table A-8: Randomization Check (2019)

	No Membership		Union Membership		Difference	
	Mean	SD	Mean	SD	$\Delta$	t
<b>Name</b>						
Name 1	0.504	0.500	0.496	0.500	0.009	(0.52)
Name 2	0.496	0.500	0.504	0.500	-0.009	(-0.52)
<b>Picture</b>						
Face 1	0.490	0.500	0.510	0.500	-0.020	(-1.14)
Face 2	0.510	0.500	0.490	0.500	0.020	(1.14)
<b>Layout</b>						
Layout 1	0.504	0.500	0.496	0.500	0.009	(0.52)
Layout 2	0.496	0.500	0.504	0.500	-0.009	(-0.52)
<b>School</b>						
School 1	0.488	0.500	0.512	0.500	-0.024	(-1.42)
School 2	0.512	0.500	0.488	0.500	0.024	(1.42)
<b>Address</b>						
Address 1	0.497	0.500	0.503	0.500	-0.007	(-0.38)
Address 2	0.503	0.500	0.497	0.500	0.007	(0.38)
<b>Membership</b>						
Membership 1	0.498	0.500	0.502	0.500	-0.004	(-0.24)
Membership 2	0.502	0.500	0.498	0.500	0.004	(0.24)
<b>Apprenticeship</b>						
Company 1	0.504	0.500	0.496	0.500	0.009	(0.52)
Company 2	0.496	0.500	0.504	0.500	-0.009	(-0.52)
<b>Last Employment</b>						
Company 1	0.492	0.500	0.508	0.500	-0.016	(-0.93)
Company 2	0.508	0.500	0.492	0.500	0.016	(0.93)
Observations	1,679		1,679		3,358	

*Note:* The table provides details of the distribution of randomly assigned application characteristics between the treatment (Union Members) and control group (Non-Union Members). The t-test for mean differences by subcategories tests for the success of the randomization. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-5 Robustness Check Main Specification

Table A-9 presents several robustness checks to investigate the sensitivity of the estimates for both callback categories. The considered baseline controls are as in the paper the state, occupation and year fixed effects. Columns (2) to (4) present a step-by-step extension of the controls by including firm controls (collective agreement coverage, contract type, firm size), application controls (application order, layouts, pictures, previous employers, organization membership, name, address and school) and labor market tightness measured as occupation- and region specific vacancy/unemployment-ratio. No changes in effect size and statistical significance occur for both Callback Category 1 and Callback Category 2.

As further robustness checks, I consider fixed effect estimates in Column (5), a probit model in Column (6) and the subsamples depending on the years in Column (7) to (9). Table A-9 shows that the effect of a union membership is negative and significant at the 1% significance level for all specifications and callback categories.

The results replicate in all three waves with slight variation in magnitude. On average we observe a decline in callbacks of 6.3 (7.0) percentage points for Callback Category 1 (Callback Category 2) by revealing union membership in the application.

Table A-9: Robustness Checks of the Main Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>Panel A: Callback Category 1</b>									
Union	-0.0627*** (0.0063)	-0.0627*** (0.0063)	-0.0623*** (0.0063)	-0.0623*** (0.0063)	-0.0623*** (0.0089)	-0.1758*** (0.0177)	-0.0596*** (0.0118)	-0.0586*** (0.0101)	-0.0678*** (0.0108)
Callback Rate Control Group	0.403	0.403	0.403	0.403	0.403	0.403	0.337	0.432	0.414
N	8,714	8,714	8,714	8,714	8,714	8,714	2,082	3,274	3,358
(Pseudo) $R^2$	0.067	0.068	0.067	0.067	0.629	0.059	0.064	0.046	0.076
<b>Panel B: Callback Category 2</b>									
Union	-0.0698*** (0.0067)	-0.0698*** (0.0067)	-0.0698*** (0.0067)	-0.0696*** (0.0067)	-0.0696*** (0.0096)	-0.1861*** (0.0180)	-0.0811*** (0.0134)	-0.0577*** (0.0106)	-0.0745*** (0.0114)
Callback Rate Control Group	0.511	0.511	0.511	0.511	0.511	0.511	0.442	0.523	0.542
N	8,714	8,714	8,714	8,714	8,714	8,714	2,082	3,274	3,358
(Pseudo) $R^2$	0.074	0.075	0.075	0.073	0.605	0.061	0.062	0.054	0.083
<b>Alternative Controls and Specifications</b>									
Linear Probability Model	✓	✓	✓	✓	✓		✓	✓	✓
Probit (reported marginal effects at mean)						✓			
Baseline Controls	✓	✓	✓	✓		✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓		✓	✓	✓	✓
Application Controls			✓	✓		✓	✓	✓	✓
Labor Market Tightness			✓	✓		✓	✓	✓	✓
Firm Fixed Effects					✓				
Year 2017 Only							✓		
Year 2018 Only								✓	
Year 2019 Only									✓

*Note:* This table reports multivariate regressions using a linear probability model on the determinants for discrimination of union members. Panel A uses the probability of receiving a positive callback in terms of Callback Category 1 "Invitation to a job interview" as the dependent variable. Panel B uses the probability of receiving a positive callback in terms of Callback Category 2 "Any request of an employer" as the dependent variable. The baseline controls are state, occupation and year fixed effects. The extended set of controls covers firm controls (collective agreement coverage, contract type, firm size) and application controls (application order, layouts, pictures, previous employers, organization membership, name, address and school). Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-6 Heckman Critique and Neumark Correction

The Heckman Critique points out that differences in the variance of unobserved characteristics can bias correspondence experiments on hiring discrimination (Heckman, 1998). If there are unobservable differences between union and non-union members, this could create a bias in hiring discrimination that is either upward or downward. To account for the Heckman Critique, I exploit randomly assigned application layout aspects that affected the callback rates of the applicants to test for heterogeneity in these observables and potential unobservables. This Neumark Correction has been shown to be of relevance in the context of correspondence experiments in labor markets (Neumark et al., 2019) particularly on ethnic discrimination (Neumark, 2012; Carlsson et al., 2014; Becker et al., 2019; Weichselbaumer, 2020; Nüß and Penny, 2022).

To correct for biases of unobservables, the idea is to use potential heterogeneity of application aspects in interaction with the treatment, in this case union membership (Panel A). By using this observable heterogeneity, we are able to infer onto the relative difference of unobservable variance between union and non-union members (Panel B). In the last step, this allows us to decompose the unbiased extent of hiring discrimination from the biased component (Panel C). For a more detailed explanation of the Neumark Correction see Neumark (2012) and Carlsson et al. (2014).

Panel A in Table A-10 shows the marginal effect of union membership on a positive callback based on Callback Category 1 and 2, using a probit model and heteroscedastic corrected probit estimates.

I perform an over-identification test for the assumption that the layout aspects of union and non-union members are equal for all application aspects (Panel B). The p-values of 0.727 and 0.867 do not reject the assumption of over identification. The relative standard deviation of union and non-union in row 4 provides no indication for a meaningful difference in the variance of unobservables. The statistical test that the ratio is equal to 1 (equal variance for observables and unobservables) can not be rejected with p-values of 0.517 and 0.725.

Based on these calculations Panel C of Table A-10 provides the unbiased effect of discrimination against union members (union-level) and the extent of bias due to unobservables (union-variance). Column 1 indicates a decline of callbacks for union members based on callback category 1 (callback category 2) of 6.0 percentage points (8.5 percentage points) significant on the 5% (1%) significance level. The bias due to unobservables accounts for about 0.1 to 0.5 percentage points of lower invitations.

Table A-10 shows the sensitivity analysis related to the Heckman Critique. The results find no evidence for a bias due to unobservable differences. This finding supports my main results for the existence and magnitude of hiring discrimination against union members in Germany.

Table A-10: Neumark Correction for Unobservable Heterogeneity

	(1)	(2)
<b>Panel A:</b>		
<b>Probit estimates</b>		
Union (marginal)	-0.0645*** (0.0272)	-0.0848*** (0.0283)
<b>Heteroscedastic probit estimates</b>		
Union (marginal)	-0.0654*** (0.0274)	-0.0854*** (0.0285)
<b>Panel B:</b>		
Overidentification test: ratios of coefficients on skills for union relative to non-union are equal (p-value)	0.756	0.852
Standard deviation of unobservables, Union/Non-Union	0.961	0.977
Test: Homoscedastic vs. heteroscedastic probit (p-value, Wald test for equal variances)	0.487	0.686
<b>Panel C:</b>		
Union-level (marginal)	-0.0602** (0.0279)	-0.0848*** (0.0285)
Union-variance (marginal)	-0.0052 (0.0075)	-0.0006 (0.0015)
Observations	8,714	8,714
Dependent Variables		
Basic Callbacks		✓
Strict Callbacks	✓	
Controls		
Occupation FE	✓	✓
Federal State FE	✓	✓
Year FE	✓	✓
Application Controls	✓	✓
Application Controls x Union	✓	✓

*Note:* This table reports multivariate regressions using probit and heteroscedastic probit estimates, as well as a decomposition of discriminated related to union membership in level (Union-level) and discrimination related to the variance in unobservables (Union-variance). The dependent variable is the probability of receiving a positive callback in terms of Callback Category 1 "Invitation to a job interview" and Callback Category 2 "Any request of an employer"; The baseline controls are the application order, pictures, organization membership, name, address, school as well as occupation and regional fixed effects; Standard errors, corrected for clustering at the firm level, are in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-7 Robustness by Weighting

Figure A-3 displays the raw experimental data compared to their relative relevance by regions' occupation share (Panel (a)) and employment share (Panel (b)). The figure shows that the occupational share is close to their true share in the population. With respect to regions employment share, there are strong disparities of regional representativeness in the experimental data.

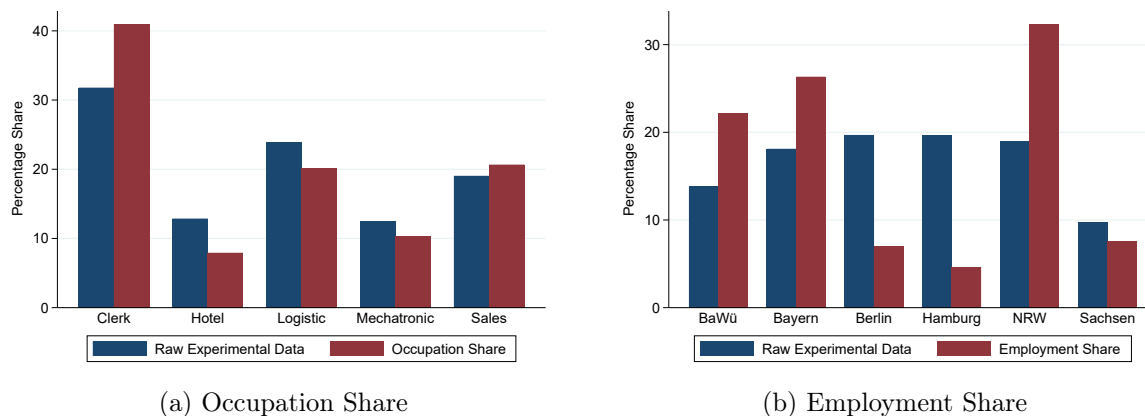


Figure A-3: Representativeness by Occupation and Employment Share

*Note:* This figure presents the relative share of observations in the experimental data, relative to its actual share in the economy. Panel (a) covers the relative occupational share of the experiment and the general economy. Panel (b) covers the relative employment share of all considered federal state of the experiment and the general economy.

While a weighting that is not representative of the regions does not matter for the analysis of the existence of hiring discrimination, it does matter to obtain a representative measure for the German economy. Table A-11 shows the sensitivity of hiring discrimination of the original estimates (Columns 1 and 2) to reweighting. Reweighting by occupational shares (Columns 3 and 4) and by employment shares (Columns 5 and 6) do not affect the extent of hiring discrimination.

Given the large sectoral differences in Germany's industrial relations and the observed heterogeneity of hiring discrimination by sectors, Column 7 and 8 of Table A-11 extend the analysis of weighting to the sectoral employment share. Reweighting to a representative employment share by sectors has a small negative effect on the extent of discrimination increasing it by 0.3 percentage points (Column 7) and 0.8 percentage points (Column 8).

Table A-11: Sensitivity Analysis for Weighting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union	-0.0627*** (0.0063)	-0.0698*** (0.0067)	-0.0611*** (0.0074)	-0.0721*** (0.0079)	-0.0616*** (0.0064)	-0.0694*** (0.0069)	-0.0655*** (0.0091)	-0.0778*** (0.0097)
Callback Rate Control Group	0.403	0.511	0.403	0.511	0.403	0.511		
Observations	8,714	8,714	8,714	8,714	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.067	0.074	0.065	0.076	0.066	0.073	0.068	0.078
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls and Weighting								
Linear Probability Model	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓
Regional Employment Weights			✓	✓				
Occupation Weights					✓	✓		
Sector Employment Weights							✓	✓

*Source:* Statistics of the Federal Employment Agency. Beschäftigte nach Berufen (KldB 2010) (Zeitreihe Quartalszahlen).

*Note:* This table reports multivariate regressions using a linear probability model using three different weighting dimensions. Columns (3) and (4) are weighted by the federal states employment share. Columns (5) and (6) are weighted by the occupational employment share. Columns (7) and (8) are weighted by the sectoral employment share. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer"; The baseline controls are regional, occupation and year fixed effects; Standard errors, corrected for clustering at the firm level, are in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-8 Protocol of the Social Media Accounts

### Twitter Account History and Activities

To cover up the experiment, Twitter accounts of the applicants were created 13 months before the beginning of the experiment. The Twitter accounts had equal profile pictures and bios, had some followers and followed other accounts. Over the 13 months before and during the experiment, the accounts regularly liked and retweeted content related to daily news and the applicants' hobbies particularly related to their favorite football club, equally in content and time. In case of the union related Twitter account, these activities were extended by regular likes and retweets resulting from the Twitter accounts of the Confederation of German Trade Unions (DGB: [@DGB\\_News](#)) and the United Services Union (Ver.di: [@\\_Verdi](#)).

Likes and Retweets were scheduled during typical break times (breakfast and lunch), after work and at weekends to avoid any signal of lower productivity during working time (see Table A-12).

Table A-12: Schedule for Twitter Activities

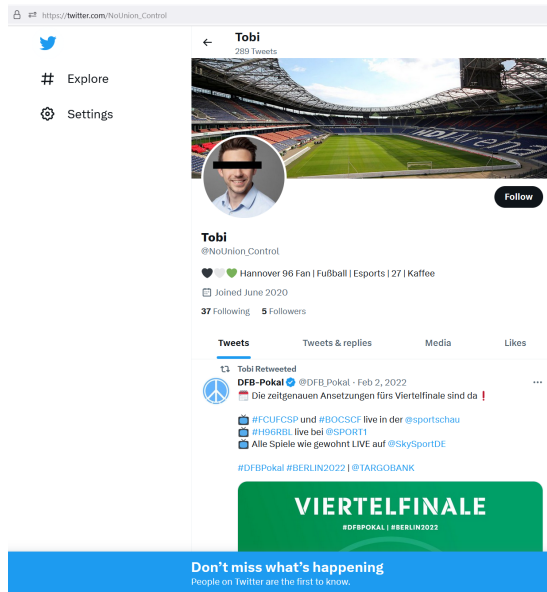
	Work Week					Weekend	
	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday	Sunday
Breakfast	9:00 to 9:15 AM						
Lunch	12:00 to 12:30 PM						
End of Work	After 5:00 PM						

*Note:* This table reports the schedule of activities of both Twitter accounts. The shaded areas indicate the common Twitter activities in terms of likes and retweets. Twitter activities were commonly distributed around typical work pauses for breakfast, lunch, end of work, and during weekends.

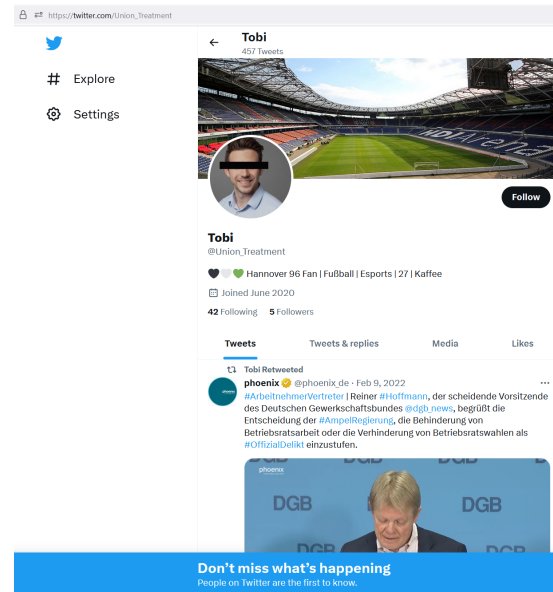
The Twitter accounts (Figure A-4) were active until the beginning of February, two months after the last application was sent. To avoid detection, after one more month, the Twitter accounts were renamed, the pictures deleted and the bio was temporarily changed so that the accounts were no longer to be found. The Twitter accounts provide information comparable to early 2023. For transparency about Twitter activities and compliance to the protocol the Twitter accounts are now available via the following links: Treatment Group: [@Union\\_Treatment](#). Control Group: [@NoUnion\\_Control](#).

### Email Signature

To provide employers information about a pro-union sentiment via Twitter account, employers need easy access to these accounts. The paper randomly assigns one of two Twitter accounts



(a) Control Group



(b) Pro-Union Sentiment

Figure A-4: Twitter Accounts

*Note:* The figures show the online available information on the Twitter accounts during the experiment 2. Panel (a) provides a screenshot of the control group's Twitter account. Panel (b) provides a screenshot of the Twitter account that signaled a pro-union sentiment by liking and retweeting content of union Twitter accounts as well as other content related to unions.

to each application, otherwise similar in every characteristic. All applications were sent with the same email with their cover letter, resume and certificates attached. Most importantly, all emails ended with a general signature which included all contact information (see Figure A-5). If a Twitter account was assigned to the application, a link to one of two Twitter accounts (generic or pro-union sentiment) was added to the signature (see Figure A-5 Panel (b)).

Bewerbung als [REDACTED]

Sehr geehrte [REDACTED],  
anbei übersende ich Ihnen meine Bewerbungsunterlagen für die von Ihnen ausgeschriebene Vollzeitstelle als [REDACTED] mit der Referenznummer [REDACTED]. Durch mein noch bestehendes Arbeitsverhältnis, wäre es mir möglich die neue Stelle zum 01. [REDACTED] anzutreten. Meine Bewerbungsunterlagen erhalten Sie zusammengefasst in dem beigefügten PDF Dokument. Über die Möglichkeit Sie in einem persönlichen Gespräch von meiner Person überzeugen zu dürfen, würde ich mich sehr freuen.  
Mit freundlichen Grüßen  
Tobias [REDACTED]

Tobias [REDACTED]  
Adresse: [REDACTED]  
PLZ: [REDACTED]  
Mobil: [REDACTED]  
Email: [REDACTED]@gmx.de

(a) Plain Signature

Bewerbung als [REDACTED]

Sehr geehrte [REDACTED],  
anbei übersende ich Ihnen meine Bewerbungsunterlagen für die von Ihnen ausgeschriebene Vollzeitstelle als [REDACTED] mit der Referenznummer [REDACTED]. Durch mein noch bestehendes Arbeitsverhältnis, wäre es mir möglich die neue Stelle zum 01. [REDACTED] anzutreten. Meine Bewerbungsunterlagen erhalten Sie zusammengefasst in dem beigefügten PDF Dokument. Über die Möglichkeit Sie in einem persönlichen Gespräch von meiner Person überzeugen zu dürfen, würde ich mich sehr freuen.  
Mit freundlichen Grüßen  
Tobias [REDACTED]

Tobias [REDACTED]  
Adresse: [REDACTED]  
PLZ: [REDACTED]  
Mobil: [REDACTED]  
Email: [REDACTED]@gmx.de  
Twitter: [Twitter.com/\[REDACTED\]](#)

(b) Signature with Twitter Link

Figure A-5: Email and Signatures

*Note:* The figures provide an overview of the emails and signature used during the experiment in 2021. Panel (a) shows the used email and signature in case no Twitter account was assigned to the application. Panel (b) shows the used email and signature in case one of the two Twitter accounts was assigned to the application.



← Account information		← Account information	
Username @ [REDACTED]	>	Username @ [REDACTED]	>
Phone +49 [REDACTED]	>	Phone +49 [REDACTED]	>
Email [REDACTED]	>	Email [REDACTED]	>
Verified No. <a href="#">Request Verification</a>		Verified No. <a href="#">Request Verification</a>	
Protected Tweets No	>	Protected Tweets No	>
Account creation Jun 30, 2020, 10:03:39 AM [REDACTED] (Germany)		Account creation Jun 30, 2020, 10:23:28 AM [REDACTED] (Germany)	
Country Germany	>	Country Germany	>
Languages English, German	>	Languages English, German	>
Gender Male	>	Gender Male	>
Birth date Add your date of birth to your <a href="#">profile</a> .		Birth date Add your date of birth to your <a href="#">profile</a> .	
Age 13-54	>	Age 13-54	>
Automation Manage your automated account.	>	Automation Manage your automated account.	>

(a) Twitter Control

(b) Twitter Treatment

Figure A-6: Twitter Account Settings

*Note:* The figures provide details of the created Twitter accounts and account settings during the experiment in 2021. Both accounts were created in June 2020, were located in Germany, and were public available during the experiment.

## A-9 Summary Statistics of Experiment 2

Table A-13: Summary Statistics Experiment 2

	Mean	SD	Min	Max	N
<b>Callbacks</b>					
Callback Category 1	0.29	0.45	0.00	1.00	4,531
Callback Category 2	0.40	0.49	0.00	1.00	4,531
Wage Request	0.03	0.17	0.00	1.00	4,531
<b>Design</b>					
Union	0.50	0.50	0.00	1.00	4,531
Twitter	0.75	0.44	0.00	1.00	4,531
<b>State</b>					
Baden-Wuerttemberg	0.17	0.38	0.00	1.00	4,531
Bavaria	0.13	0.34	0.00	1.00	4,531
Berlin	0.17	0.37	0.00	1.00	4,531
Hamburg	0.15	0.36	0.00	1.00	4,531
North Rhine-Westphalia	0.29	0.45	0.00	1.00	4,531
Saxony	0.10	0.30	0.00	1.00	4,531
<b>Human Resource Manager</b>					
Male	0.41	0.49	0.00	1.00	4,531
Female	0.53	0.50	0.00	1.00	4,531
Unknown	0.06	0.24	0.00	1.00	4,531
<b>Vacancy Information</b>					
Immediately Hiring	0.76	0.43	0.00	1.00	4,531
<b>Contract Type</b>					
Temporary	0.11	0.31	0.00	1.00	4,531
Permanent	0.77	0.42	0.00	1.00	4,531
Unknown	0.12	0.33	0.00	1.00	4,531
<b>Collective Agreement</b>					
Collective Agreement	0.12	0.33	0.00	1.00	4,531
<b>Firm Size</b>					
< 6 Emp.	0.15	0.36	0.00	1.00	4,531
6 to 50 Emp.	0.52	0.50	0.00	1.00	4,531
51 to 500 Emp.	0.28	0.45	0.00	1.00	4,531
500 Emp. <	0.04	0.19	0.00	1.00	4,531

*Note:* This table reports the summary statistics of the second experiment, revealing union membership in the CV and a pro-union sentiment via Twitter accounts in 2021.

## A-10 Randomization Check Social Media Experiment

Table A-14: Randomization Check Experiment 2

	(1) CV Mean	(2) Union CV Mean	(3) Twitter Mean	(4) Union Twitter Mean	(5) (1)=(2) $\Delta$	(6) (1)=(3) $\Delta$	(7) (1)=(4) $\Delta$	(8) (2)=(3) $\Delta$	(9) (2)=(4) $\Delta$	(10) (3)=(4) $\Delta$
<b>Covered by a Collective Agreement</b>										
Yes	0.14	0.12	0.11	0.13	0.017	0.024	0.007	0.007	-0.010	-0.017
No	0.86	0.88	0.89	0.87	-0.017	-0.024	-0.007	-0.007	0.010	0.017
<b>Contract Type</b>										
Temporary	0.13	0.12	0.10	0.11	0.017	0.035**	0.019	0.017	0.002	-0.015
Permanent	0.73	0.75	0.79	0.77	-0.021	-0.058***	-0.037*	-0.037*	-0.017	0.020
Unknown	0.14	0.13	0.11	0.12	0.003	0.023	0.018	0.020	0.015	-0.005
<b>Immediately Hiring</b>										
Yes	0.75	0.73	0.76	0.77	0.028	-0.007	-0.014	-0.035	-0.042**	-0.007
No	0.25	0.27	0.24	0.23	-0.028	0.007	0.014	0.035	0.042**	0.007
<b>Firm Size</b>										
< 6 Emp.	0.14	0.15	0.16	0.15	-0.009	-0.023	-0.016	-0.014	-0.007	0.007
6 to 50 Emp.	0.52	0.54	0.53	0.52	-0.023	-0.008	0.001	0.015	0.024	0.010
51 to 500 Emp.	0.30	0.27	0.28	0.29	0.029	0.023	0.010	-0.006	-0.019	-0.012
500 Emp. <	0.04	0.04	0.04	0.04	0.003	0.009	0.005	0.006	0.002	-0.004
<b>Federal State</b>										
Baden-Wuerttemberg	0.18	0.18	0.16	0.17	0.004	0.021	0.012	0.016	0.007	-0.009
Bavaria	0.13	0.12	0.13	0.13	0.005	0.001	-0.002	-0.004	-0.007	-0.003
Berlin	0.16	0.18	0.16	0.18	-0.021	-0.008	-0.020	0.013	0.002	-0.011
Hamburg	0.16	0.15	0.15	0.15	0.007	0.007	0.010	0.000	0.003	0.003
North Rhine-Westphalia	0.30	0.30	0.29	0.27	-0.004	0.006	0.024	0.011	0.028	0.018
Saxony	0.08	0.07	0.11	0.10	0.009	-0.027**	-0.024*	-0.036***	-0.033**	0.003
<b>Sectors</b>										
Sector A	0.00	0.00	0.00	0.00	-0.002	-0.001	0.001	0.001	0.002	0.002
Sector B	0.00	0.00	0.00	0.00	-0.002	-0.001	0.000	0.001	0.002	0.001
Sector C	0.13	0.13	0.13	0.13	-0.007	-0.003	0.000	0.005	0.007	0.003
Sector D	0.00	0.00	0.00	0.00	-0.000	-0.000	-0.002	-0.000	-0.002	-0.002
Sector E	0.01	0.01	0.01	0.01	-0.002	-0.001	-0.007*	0.001	-0.005	-0.005
Sector F	0.08	0.08	0.08	0.08	-0.001	-0.001	0.002	0.000	0.003	0.003
Sector G	0.20	0.20	0.21	0.22	0.004	-0.003	-0.018	-0.007	-0.022	-0.015
Sector H	0.02	0.04	0.05	0.03	-0.013	-0.023***	-0.011	-0.010	0.001	0.012*
Sector I	0.01	0.02	0.01	0.01	-0.011*	-0.010**	-0.006	0.001	0.004	0.003
Sector J	0.07	0.07	0.06	0.05	-0.004	0.002	0.018	0.007	0.022*	0.015*
Sector K	0.03	0.02	0.02	0.02	0.001	0.010	0.010	0.009	0.008	-0.000
Sector L	0.06	0.04	0.05	0.04	0.011	0.008	0.016	-0.004	0.005	0.009
Sector M	0.14	0.16	0.18	0.17	-0.018	-0.039**	-0.034**	-0.022	-0.016	0.005
Sector N	0.07	0.06	0.07	0.08	0.006	0.002	-0.007	-0.004	-0.014	-0.009
Sector O	0.02	0.01	0.01	0.01	0.010	0.014**	0.013**	0.003	0.003	-0.000
Sector P	0.03	0.03	0.03	0.04	0.001	0.000	-0.007	-0.001	-0.008	-0.007
Sector Q	0.08	0.06	0.05	0.06	0.020	0.031**	0.019	0.011	-0.000	-0.012
Sector R	0.02	0.01	0.01	0.01	0.012*	0.008	0.010*	-0.004	-0.002	0.002
Sector S	0.04	0.04	0.03	0.03	-0.006	0.007	0.004	0.013	0.009	-0.003
Sector T	0.00	0.00	0.00	0.00	-0.002	0.000	-0.001	0.002	0.001	-0.001
<b>HR Manager Gender</b>										
Female	0.55	0.50	0.52	0.55	0.048	0.027	-0.004	-0.022	-0.052**	-0.031*
Male	0.40	0.44	0.41	0.39	-0.037	-0.009	0.018	0.028	0.056**	0.028
Unknown	0.05	0.06	0.07	0.06	-0.011	-0.018	-0.015	-0.006	-0.003	0.003
Observations	579	571	1,696	1,685	1,150	2,275	2,264	2,267	2,256	3,381

*Note:* The table provides details of the distribution of randomly assigned application characteristics between the control group (Non-Union Members) treatment 1 (Union Members in the CV), treatment 2 (Twitter Account) and treatment 3 (Twitter Account with pro-union sentiment). The t-test for mean differences by sub-categories tests for the success of the randomization. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-11 Heterogeneous Effects by Characteristics

Table A-15: Hiring Discrimination by Union Membership

	(1)	(2)	(3)	(4)
Union	-0.0627*** (0.0063)	-0.0698*** (0.0067)		
Union (IG Metall)			-0.0560*** (0.0182)	-0.0397** (0.0176)
Union (NGG)			-0.0583*** (0.0176)	-0.0636*** (0.0180)
Union (ver.di)			-0.0646*** (0.0073)	-0.0760*** (0.0080)
Callback Rate Control Group	0.403	0.511	0.403	0.511
N	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.068	0.075	0.067	0.074
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Linear Probability Model	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Collective Agreement Type FE	✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members accounting for heterogeneity by union membership. The chosen unions are the IG Metall (Union of the Metal Industry), Ver.di (United Service Union) and the NGG (Food Workers' Union). The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer"; The baseline controls are regional, occupation, year and union type fixed effects; Standard errors, corrected for clustering at the firm level, are in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A-16: Hiring Discrimination by Corporate Legal Form

	(1)	(2)	(3)	(4)
Union	-0.0627*** (0.0063)	-0.0698*** (0.0067)		
Union × Sole Proprietorship			-0.0370* (0.0222)	-0.0154 (0.0241)
Union × Limited Liability Company			-0.0651*** (0.0080)	-0.0710*** (0.0085)
Union × Limited Liability Partnership			-0.0675*** (0.0152)	-0.0852*** (0.0160)
Union × Civil Law Partnership			-0.0672** (0.0309)	-0.1261*** (0.0348)
Union × Stock Corporation			-0.0738** (0.0344)	-0.0940*** (0.0360)
Union × Public Institution			-0.0930** (0.0455)	-0.1047** (0.0467)
Union × Other Forms			-0.0079 (0.0308)	0.0079 (0.0381)
Callback Rate Control Group	0.403	0.511	0.403	0.511
N	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.071	0.078	0.070	0.078
Dependent Variables				
Basic Callbacks		✓		✓
Strict Callbacks	✓		✓	
Controls				
Linear Probability Model	✓	✓	✓	✓
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Baseline Controls	✓	✓	✓	✓
Corporate Legal Form FE	✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members accounting for heterogeneity by corporate legal form. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer"; The baseline controls are regional, occupation, year and corporate legal forms fixed effects; Standard errors, corrected for clustering at the firm level, are in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A-17: Hiring Discrimination by Collective Agreement Type

	(1)	(2)	(3)	(4)	(5)	(6)
Union	-0.0627*** (0.0063)			-0.0698*** (0.0067)		
Union × Without Collective Agreement		-0.0685*** (0.0071)			-0.0745*** (0.0076)	
Union × Collective Agreement		-0.0386*** (0.0142)			-0.0504*** (0.0148)	
Union × Sector CA			-0.0397*** (0.0152)			-0.0490*** (0.0158)
Union × Company CA			-0.0333 (0.0528)			-0.1167** (0.0585)
Union × Unknown CA			-0.0256 (0.0575)			0.0256 (0.0575)
Callback Rate Control Group	0.403	0.403	0.398	0.511	0.511	0.505
N	8,714	8,714	1,708	8,714	8,714	1,708
Adjusted $R^2$	0.067	0.067	0.109	0.075	0.075	0.126
Dependent Variables						
Strict Callbacks (Callback Category 1)	✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓
Controls						
Linear Probability Model	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Collective Agreement Type FE			✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members accounting for heterogeneity by collective agreement type. Information on the existence of a collective agreement is classified into four categories. Without collective agreement: provided no information about a collective agreement. Sector collective agreement: Provided information about their firm being covered by a sectoral collective agreement. Company collective agreement: Provided information about the existence of a firm level collective agreement. Unknown collective agreement: Provides information about the existence of a collective agreement, but does not provide any detailed information about the type of the collective agreement. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer". The baseline controls are regional, occupation, year fixed effects. Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A-18: Human Resource Manager Gender

	(1)	(2)	(3)	(4)
Union	-0.0626*** (0.0063)	-0.0697*** (0.0067)		
Union × Female HR Manager			-0.0608*** (0.0089)	-0.0706*** (0.0096)
Union × Male HR Manager			-0.0617*** (0.0095)	-0.0653*** (0.0100)
Union × Unknown HR Manager			-0.0904*** (0.0266)	-0.1002*** (0.0302)
Callback Rate Control Group	0.403	0.511	0.403	0.511
N	8,714	8,714	8,714	8,714
Adjusted $R^2$	0.067	0.075	0.067	0.075
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Linear Probability Model	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
HR Manager Gender FE	✓	✓	✓	✓

*Note:* This table reports multivariate regressions using a linear probability model on the extent of discrimination of union members accounting for heterogeneity by human resource manager gender. Information on the human resource managers' gender is classified into three categories. Male HR Manager: Provided information on a male contact person. Female HR Manager: Provided information on a female contact person. Unknown HR Manager: Provided information on a male and female contact person or no specific contact person at all. The dependent variable is the probability of receiving a positive callback in terms of callback category 1 "Invitation to a job interview" and callback category 2 "Any request of an employer"; The baseline controls are regional, occupation, year and corporate legal forms fixed effects; Standard errors, corrected for clustering at the firm level, are in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-12 Details on Labor Disputes Data

Due to changes in firms' data protection regulations, strike data are anonymized by the Federal Employment Agency. Since 2018, if fewer than 3 companies are affected by a strike in a sector, they will not provide statistics. This leads to lower quality of strike data particularly when considering state and sector variation in strike activity.

Table A-19 gives an overview of the original quality of the accessible strike data. The table shows the accessible lost working days as a share of total lost working days due to strikes by federal state and Germany in total. Columns 1 to 3 contain information about the publicly available data. Columns 4 to 6 cover the improved aggregated data provided to me by the federal employment agency.

The publicly accessible dataset contains 99.6% of all working days lost due to a strike for Germany as whole and 96.6% when broken down to the state level. While for most sectors and states the dataset still contains most of the lost working days. For Berlin in 2018, the data quality shrinks to a coverage of only 24% of all lost working days. By aggregation to sectoral data on one digit classification instead of Nace 2 two digit classification, the Federal Employment Agency provided me with an improved dataset. This increases the overall quality to 99.8% of all lost working days for Germany and 98.8% coverage of all relevant state level strikes. This improves the strike data quality of Berlin in 2018 from 24% to 96.7%.

Table A-19: Strike Data Quality

	Publicly Accessible Data			After Aggregation		
	2017 (1)	2018 (2)	2019 (3)	2017 (4)	2018 (5)	2019 (6)
Germany	100.0%	99.7%	98.9%	100.0%	99.9%	99.4%
Baden-Wuerttemberg	100.0%	97.8%	93.6%	100.0%	99.8%	97.0%
Bavaria	100.0%	99.2%	87.6%	100.0%	99.6%	99.2%
North Rhine-Westphalia	100.0%	98.1%	93.0%	100.0%	99.5%	96.8%
Hamburg	100.0%	78.2%	91.8%	100.0%	78.2%	91.8%
Berlin	100.0%	24.0%	96.1%	100.0%	96.7%	96.1%
Saxony	100.0%	96.9%	93.3%	100.0%	100.0%	93.3%
Total Germany		99.6%			99.8%	
Total Federal States		96.6%			98.8%	

*Source:* Statistics of the Federal Employment Agency. Streiks nach Wirtschaftsabschnitten der Wirtschaftsklassifikation 2008 (2017-2019).

*Note:* The table provides details about the accessibility of strike date for federal states and Germany in terms of the percentage share of the accessible lost working days on total lost working days due to strikes.



## A-13 Robustness Check Collective agreement

Table A-20: Robustness Sectoral Collective Agreement Coverage and Compliance (Weighted)

	Raw Discrimination 1		Raw Discrimination 2	
	CA Coverage	Non-Compliance	CA Coverage	Non-Compliance
All Sectors	-.548	.426	-.721	.535
Excluding Agriculture	-.619	.489	-.787	.592
Excluding Mining/Quarrying	-.531	.406	-.716	.525
Excluding Manufacturing	-.594	.408	-.741	.533
Excluding Construction	-.603	.476	-.745	.554
Excluding Wholesale	-.545	.425	-.717	.536
Excluding Retail Trade	-.560	.436	-.725	.538
Excluding Traffic/Storage	-.579	.479	-.755	.592
Excluding Information/Communication	-.537	.411	-.721	.538
Excluding Other Services	-.549	.429	-.720	.533
Excluding Finance/Insurance	-.560	.407	-.720	.533
Excluding Education/Health	-.633	.593	-.802	.692
Excluding Administrative Services	-.530	.407	-.699	.493
Excluding Organisations	-.567	.438	-.737	.546
Excluding Public Administration	.037	.053	-.107	.116

*Note:* The table provides correlations for the raw difference in callbacks between union and non-union members with the sectoral coverage by a collective agreement, and sectoral non-compliance in the absence of a collective agreement. Excluding one sector at a time, allows me to test the sensitivity of the results related to specific sectors. Weighted data.

Table A-21: Robustness Sectoral Collective Agreement Coverage and Compliance (Unweighted)

	Raw Discrimination 1		Raw Discrimination 2	
	CA Coverage	No-Compliance	CA Coverage	Non-Compliance
All Sectors	-.353	.307	-.477	.384
Excluding Agriculture	-.672	.589	-.799	.656
Excluding Mining/Quarrying	-.317	.265	-.470	.370
Excluding Manufacturing	-.357	.307	-.475	.389
Excluding Construction	-.393	.357	-.504	.418
Excluding Wholesale	-.344	.306	-.465	.385
Excluding Retail Trade	-.354	.306	-.472	.379
Excluding Traffic/Storage	-.368	.329	-.492	.406
Excluding Information/Communication	-.319	.261	-.448	.343
Excluding Other Services	-.351	.303	-.472	.375
Excluding Finance/Insurance	-.331	.259	-.467	.368
Excluding Education/Health	-.362	.344	-.49	.428
Excluding Administrative Services	-.339	.287	-.456	.350
Excluding Organisations	-.394	.344	-.523	.424
Excluding Public Administration	-.053	.078	-.067	.063

*Note:* The table provides correlations for the raw difference in callbacks between union and non-union members with the sectoral coverage by a collective agreement, and sectoral non-compliance in the absence of a collective agreement. Excluding one sector at a time, allows me to test the sensitivity of the results related to specific sectors. Unweighted data.

Table A-22: Robustness Sectoral Collective Agreement Coverage and Compliance (Weighted)

	Estimated Discrimination 1		Estimated Discrimination 2	
	CA Coverage	No-NCompliance	CA Coverage	Non-Compliance
All Sectors	-.548	.435	-.725	.534
Excluding Agriculture	-.621	.501	-.789	.59
Excluding Mining/Quarrying	-.530	.414	-.720	.524
Excluding Manufacturing	-.596	.417	-.743	.533
Excluding Construction	-.597	.48	-.745	.549
Excluding Wholesale	-.544	.434	-.72	.535
Excluding Retail Trade	-.561	.446	-.73	.537
Excluding Traffic/Storage	-.581	.492	-.762	.595
Excluding Information/Communication	-.538	.422	-.728	.543
Excluding Other Services	-.549	.436	-.724	.531
Excluding Finance/Insurance	-.565	.417	-.724	.532
Excluding Education/Health	-.632	.601	-.808	.694
Excluding Administrative Services	-.526	.407	-.702	.486
Excluding Organisations	-.561	.443	-.737	.542
Excluding Public Administration	.002	.082	-.123	.115

*Note:* The table provides correlations for the estimated difference in callbacks between union and non-union members with the sectoral coverage by a collective agreement, and sectoral non-compliance in the absence of a collective agreement. Excluding one sector at a time, allows me to test the sensitivity of the results related to specific sectors. Weighted data.

Table A-23: Robustness Sectoral Collective Agreement Coverage and Compliance (Unweighted)

	Estimated Discrimination 1		Estimated Discrimination 2	
	CA Coverage	Non-Compliance	CA Coverage	Non-Compliance
All Sectors	-.358	.315	-.487	.388
Excluding Agriculture	-.686	.608	-.811	.660
Excluding Mining/Quarrying	-.319	.272	-.482	.376
Excluding Manufacturing	-.361	.315	-.485	.394
Excluding Construction	-.395	.363	-.512	.420
Excluding Wholesale	-.348	.314	-.474	.390
Excluding Retail Trade	-.358	.315	-.482	.382
Excluding Traffic/Storage	-.373	.338	-.503	.412
Excluding Information/Communication	-.323	.271	-.462	.353
Excluding Other Services	-.354	.310	-.481	.379
Excluding Finance/Insurance	-.336	.266	-.477	.371
Excluding Education/Health	-.366	.353	-.501	.434
Excluding Administrative Services	-.341	.292	-.465	.351
Excluding Organisations	-.387	.342	-.525	.421
Excluding Public Administration	-.080	.101	-.079	.065

*Note:* The table provides correlations for the estimated difference in callbacks between union and non-union members with the sectoral coverage by a collective agreement, and sectoral non-compliance in the absence of a collective agreement. Excluding one sector at a time, allows me to test the sensitivity of the results related to specific sectors. Unweighted data.

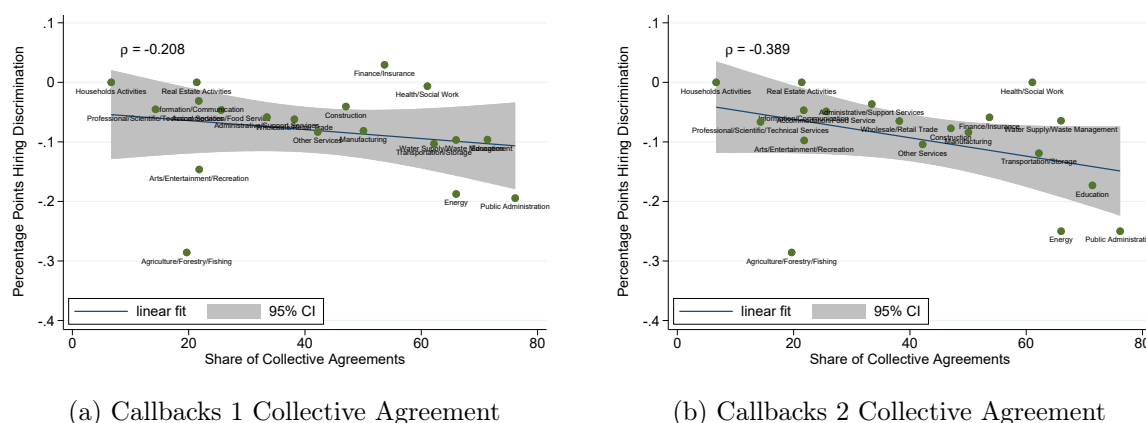


Figure A-7: Sectoral Hiring Discrimination and Collective Agreement Coverage based on SOEP

*Note:* This figure presents the correlation of collective agreement coverage based on data of the German Socioeconomic with the sectoral extent of discrimination measured in the experiment. Hiring discrimination is measured in terms of Callback Category 1 "Invitation to a job interview" and Callback Category 2 "Any request of an employer" by Nace 2 sector classification. Collective agreement coverage of firms based on calculations using German Socioeconomic Panel data for 2019.

Table A-24: Robustness Sectoral Collective Agreement Coverage using SOEP Data (Unweighted)

	Raw Disc 1 CA Coverage	Raw Disc 2 CA Coverage
All Sectors	-.208	-.208
Excluding Agriculture/Forestry/Fishing	-.492	-.492
Excluding Manufacturing	-.208	-.208
Excluding Energy	-.13	-.13
Excluding Water Supply/Waste Management	-.202	-.202
Excluding Construction	-.217	-.217
Excluding Wholesale/Retail Trade	-.206	-.206
Excluding Transportation/Storage	-.197	-.197
Excluding Accommodation/Food Service	-.192	-.192
Excluding Information/Communication	-.179	-.179
Excluding Finance/Insurance	-.271	-.271
Excluding Real Estate Activities	-.158	-.158
Excluding Professional/Scientific/Technical Services	-.183	-.183
Excluding Administrative/Support Services	-.202	-.202
Excluding Public Administration	-.083	-.083
Excluding Education	-.203	-.203
Excluding Health/Social Work	-.27	-.27
Excluding Arts/Entertainment/Recreation	-.266	-.266
Excluding Other Services	-.207	-.207
Excluding Households Activities	-.121	-.121

*Note:* The table provides correlations for the raw difference in callbacks between union and non-union members with the sectoral coverage by a collective agreement, calculated based in the German Socioeconomic Panel data of 2019. Excluding one sector at a time, allows me to test the sensitivity of the results related to specific sectors. Unweighted data.

## A-14 Opposition to Works Councils

In Section 2, I argue that Germany's legal separation of unions' wage bargaining and union voice represented by works councils allows me to analyze management opposition to unions' efforts to improve wages and working conditions, through collective bargaining and strikes.

When employers associate the applicants' union membership with the threat of establishing a works council, they discriminate against union members, not out of fear of improving wages and working conditions, but to limit employee workplace participation. Given that larger firms are more likely to have a works council, opposition against works councils could be an alternative explanation for the observed increase in discrimination in the main experiment (see Section 7.1 Table 4). This would also be in line with the finding that there is no discrimination against union members in firms with fewer than 6 employees, given that employees are allowed to establish a works council if normally at least five employees work in the firm.

To understand the extent of employers' opposition to works councils, Behrens and Dribbusch (2018) conducted a survey of paid union representatives. They provide evidence of employers' opposition to the establishment of a new works council and the reelection of existing works councils. They show employers' actions range from intimidation of works council candidates to firing members of the election committee. In particular, they show that employers' opposition is strongest in firms with 51 to 200 employees. Comparing these results on hiring discrimination with Section 7.1, shows that the patterns do not fit to each other. Behrens and Dribbusch (2018) further shows that opposition to establishing a works council is stronger in owner-managed firms. This result stands in strong contrast to my observation, that there is only weak evidence of hiring discrimination against union members in firms with the legal status of a sole proprietorship. Together, the discrepancies in the association of firm size and management type do not support the idea that employers' opposition to union members is similar to employers' opposition to the establishment of a works council.

To better understand the relationship between employers' opposition to works councils and union members, I extended the analysis to the 2015 WSI Works Council Survey. The advantage of the survey is that it includes questions about employers' cooperation with works councils, also covering employers' activities regarding participation and information rights. The survey further includes detailed information about the firm-specific share of union members as well as information about the share of union members in the works council.

Table A-25 shows that employers oppose the works councils' right to participate more than information access. While there is some heterogeneity in employers' opposition to works councils by sector, no clear pattern related to firm size emerges. In line with hiring discrimination being lower in firms with a collective agreement, management opposition against works councils is 5.8 to 8.9 percentage points lower in firms with a collective agreement. Interestingly, in contrast to hiring discrimination of union members, a high share of union members is associated with less opposition to participation rights of works councils. However, the contrary is true when the works council has a high share of union members.

In conclusion, while there is evidence for management opposition to the (re)election (Behrens and Dribbusch, 2018) and participation rights of works councils (Table A-25), these patterns neither reflect the patterns in Section 7.1 nor those in the previous literature on management

opposition to unions (Bronfenbrenner, 1997; Behrens, 2009; Aleks, 2015; McNicholas et al., 2019). This supports the idea that Germany's legal separation of unions and works councils allows me to analysis discrimination related to unions' wage bargaining.

Table A-25: Employers Opposition to Works Council Rights

	Restricted		
	Participation (1)	Information Access (2)	Any (3)
Constant	0.3911*** (0.0673)	0.1550*** (0.0535)	0.4351*** (0.0667)
Collective Agreement	-0.0871*** (0.0171)	-0.0576*** (0.0137)	-0.0892*** (0.0170)
Firm Size (Reference Category: Smaller 20 Employees)			
20 to 49 Employees	0.0335 (0.0643)	-0.0107 (0.0520)	0.0181 (0.0638)
50 to 499 Employees	0.0902 (0.0619)	-0.0076 (0.0500)	0.0668 (0.0614)
Larger 499 Employees	0.0474 (0.0646)	-0.0265 (0.0519)	0.0331 (0.0641)
Union Density	-0.1146** (0.0462)	-0.1407*** (0.0350)	-0.1272*** (0.0460)
Union Density of the Works Council	0.1961*** (0.0309)	0.1283*** (0.0243)	0.1965*** (0.0308)
Sectors (Reference Category: Mining/Production)			
Investment Goods	0.0005 (0.0285)	0.0356 (0.0216)	0.0239 (0.0284)
Construction	-0.1022** (0.0423)	0.0305 (0.0322)	-0.0853** (0.0427)
Sales	-0.0328 (0.0285)	0.0196 (0.0210)	-0.0147 (0.0285)
Transport and Storage / Hospitality	0.0167 (0.0385)	0.0284 (0.0295)	0.0200 (0.0385)
Information and Communication	0.0658 (0.0497)	0.0774* (0.0419)	0.0700 (0.0493)
Financial and insurance activities	-0.0361 (0.0504)	0.0113 (0.0354)	-0.0411 (0.0507)
Services	0.0567* (0.0332)	0.0464* (0.0255)	0.0545 (0.0332)
Public Service / Education / Health	0.1314*** (0.0285)	0.0730*** (0.0229)	0.1260*** (0.0283)
Others	0.0654 (0.0399)	0.0600* (0.0325)	0.0608 (0.0398)
N	3,666	3,666	3,666
Adjusted $R^2$	0.032	0.014	0.030

Source: WSI Works Council Survey 2015.

Note: This table reports multivariate regressions using a linear probability model on the extent of employers' violation of works council participation rights. The dependent variables are works council members' confirmation of employers restricting their right of participation, access to information or any restriction regarding participation or information rights. Robust standard errors are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A-15 Union Members Political Preferences

Table A-26: 2017 Voting Behavior in Germany

<b>General Population</b>							
CDU/CSU	SPD	Linke	Grüne	FDP	AfD	Others	Total
33%	20.5%	9.2%	8.9%	10.7%	12.6%	5%	99.9%
<b>Union Members</b>							
CDU/CSU	SPD	Linke	Grüne	FDP	AfD	Others	Total
24%	29%	12%	8%	7%	15%	5%	100%

*Source:* Der Bundeswahlleiter / Forschungsgruppe Wahlen e.V.; <https://www.dgb.de>. *Note:* This table reports voting behavior of union members and the general population during the 2017 election of the German federal government.

## A-16 Industrial Relations Background Information

Table A-27: Average Contractual Period of Collective Agreements

Year	West	East	Total
1990	12.3		
1991	12.1		
1992	14.5		
1993	14.0		
1994	13.4		
1995	15.1		
1996	16.2		
1997	16.8		
1998	12.7		
1999	13.8	14.7	14.0
2000	21.5	23.3	21.8
2001	14.1	16.4	14.5
2002	18.1	19.7	18.3
2003	20.4	21.0	20.5
2004	21.8	22.0	21.8
2005	25.2	28.4	25.7
2006	21.6	24.7	22.1
2007	22.2	21.9	22.2
2008	22.2	23.4	22.4
2009	24.3	23.2	24.1
2010	23.6	28.3	24.3
2011	22.6	23.9	22.8
2012	17.9	19.1	18.0
2013	22.4	24.7	22.8
2014	22.2	23.2	22.4
2015	20.9	22.2	21.1
2016	22.8	23.1	22.8
2017	25.3	27.3	25.6
2018	26.3	27.5	26.4
2019	25.2	26.9	25.4
2020	18.7	22.3	19.3

Source: WSI-Tarifarchiv, State 31.12.2020;

<https://www.wsi.de/de/tarifarchiv>

Note: This table reports the development of the average contractual period of a newly signed collective agreement over time.





Halle Institute for Economic Research –  
Member of the Leibniz Association

Kleine Maerkerstrasse 8  
D-06108 Halle (Saale), Germany

Postal Address: P.O. Box 11 03 61  
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60  
Fax +49 345 7753 820

[www.iwh-halle.de](http://www.iwh-halle.de)

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