

# Essays on Management Opposition and Unionization

Inaugural-Dissertation  
zur Erlangung des akademischen Grades eines Doktors  
der Wirtschafts- und Sozialwissenschaften  
der Wirtschafts- und Sozialwissenschaftlichen Fakultät  
der Christian-Albrechts-Universität zu Kiel

vorgelegt von  
Patrick Nüß, M.Sc.  
aus Soltau, Deutschland

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Erstbegutachtung:	Prof. Dr. Menusch Khadjavi
Zweitbegutachtung:	Prof. Dr. Jens Ruhose
Drittbegutachtung:	Prof. Nathan Wilmers, Ph.D

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## Chapter 1

# Introduction to the Dissertation



## 1.1 Trends of Unions' Influence among OECD Countries

Unions have a long history dating back to the 19th-century Industrial Revolution (Jean-Louis et al., 2004). As employees were confronted with long working hours, low wages, and dangerous working conditions, they started to organize collectively to oppose their employers to improve their situation and to lobby for changes in labor law. Since then, unions have become influential players in labor markets all around the world. Nowadays, all industrialized countries provide employees with the constitutional right to the freedom of association in employee associations (unions). While a country's industrial relations and the role of unions vary in their concrete functioning, they share a set of key components that are regulated in any industrial relations system.

Industrial relations cover collective bargaining procedures, conflict resolution procedures, the right to strike, and employee participation, such as employee involvement in firm decision-making on the establishment level or even the company board (Freeman and Medoff, 1984; Jäger et al., 2022b). This allows unions or employee representatives, to influence a wide range of firm behaviors such as bargaining over wages and working conditions, participating in hiring and firing decisions, and controlling for firms' compliance to labor law as well as workplace safety. The country-specific industrial relations are defined by labor law, legal frameworks for co-determination, and the right to strike, which regulates the employees' intervention possibilities. There is strong variation in the legal framework of industrial relations between countries and the compliance to these laws varies as well (ITUC, 2023).

Besides the country-specific industrial relations framework, the share of union members among employees determines union influence. This share is relevant for three reasons. First, union membership rates indicate unions' labor market coverage. Second, unions are financed with membership fees. Therefore, union membership rates are an indicator of unions' financial position and their strength during a potential labor dispute. Third, union membership rates are relevant for non-unionized firms because they indicate the likelihood of unionization attempts. If employers fear unionization, union collective agreements can have spillover effects on non-unionized firms, further reducing wage inequality and improving overall working conditions (Rosen, 1969).

Figure 1.1 presents the development of union membership rates among selected OECD countries from 1960 to 2020. It shows that the share of union members varies strongly between countries and over time. The strength of unions has fluctuated with periods of significant growth and influence followed by periods of decline. Throughout 1960 to 1980, union membership rates increased particularly in European countries. Over this time frame, the average union members rate among OECD countries was close to 40% of all workers. Since then, union membership rates have steadily fallen in almost all industrialized countries, leading to an average OECD union membership rate of 15.8% in 2019.

The highest levels and stability in union membership rates exist among Scandinavian countries and Belgium, which mostly follow the *Ghent system*. The *Ghent system* moves

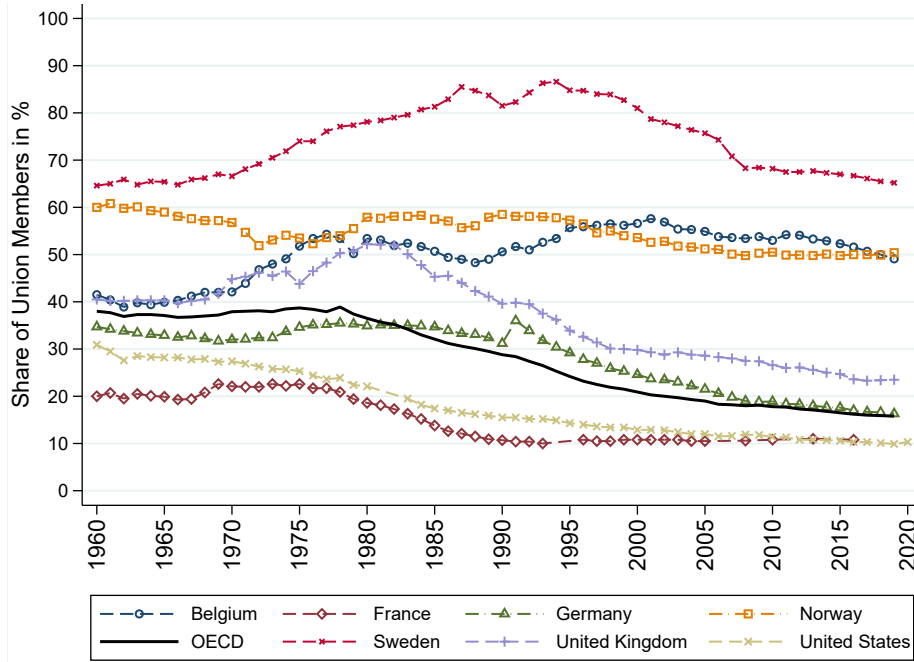


Figure 1.1: Development of Union Membership Rates among OECD Countries

Source: OECD.Stat. Trade Union Dataset.

Note: This figure presents the development of union density, the share of union members among employees, in selected OECD countries. Union density for the OECD as a whole is based on estimated values. Data for Germany before 1990 are best on Western Germany.

part of the welfare state into the responsibility of unions, particularly unemployment insurance (Van Rie et al., 2011). To be covered by unemployment insurance in these countries, it was necessary to join a union. While unions are no longer the only provider of unemployment insurance, which explains the recent decline of union membership rates in these countries, unions are still a common provider of unemployment insurance (Lind, 2009).

To complement the trends in industrial relations, Figure 1.2 presents the development of the collective agreement coverage in the same OECD countries. Over the period of 1960 to 1980, the average collective agreement coverage among OECD countries was close to 50% decreasing to an average of 32.1% in 2019. The collective agreement coverage strongly varies between countries. Despite the sharp decline of union membership rates around European countries, the collective agreement coverage persists and even increased since the 80s. This persistence mainly exists due to countries' institutional settings, typically mandatory memberships in employer associations, which force employers to follow a collective agreement, or the extension of collective agreements by law. The decline in the collective agreement coverage in Figure 1.2 is mainly observable in the United States, the United Kingdom, and Germany.

As a consequence of this decline in unions' influence, unions' bargaining positions weakened, and employers' wage-setting power increased (Erickson and Mitchell, 2007), leading to rising wage inequalities (Card et al., 2013; Farber et al., 2021; Zwysen and Drahokoupil, 2024) and worsening working conditions. To counteract the impact on wage inequality in

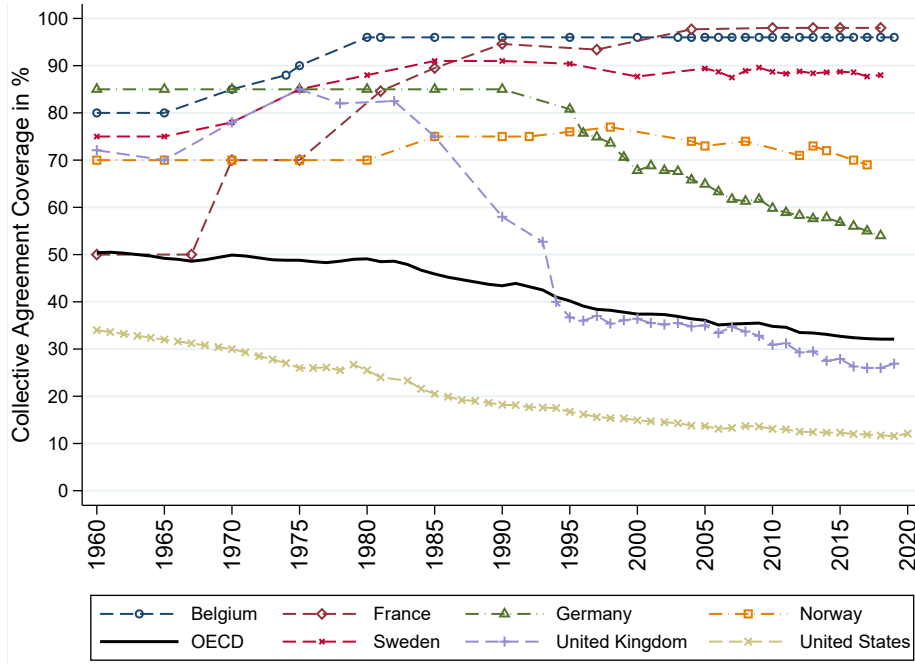


Figure 1.2: Development of the Collective Agreement Coverage among OECD Countries

Source: OECD.Stat. Trade Union Dataset.

Note: This figure presents the development of the collective agreement coverage, in selected OECD countries. Collective agreement coverage for the OECD as a whole is based on estimated values. Data for Germany before 1990 are best on Western Germany.

countries without a high collective agreement coverage, a typical intervention is the introduction of a statutory minimum wage (Lübker and Schulten, 2022).

There is an extensive body of literature and various explanations for the overall trend of declining union membership rates and unions' influence. While unions are financed via membership fees, the positive effects of unions on wages and working conditions are not limited to union members. This public good character of union membership disincentivizes individual employees to join a union (Olson, 1971). To overcome the public good characteristic, unions provide additional exclusive services only for union members. These services cover legal advice, legal support for labor court cases, tax advice, and education, particularly about workers' rights.

Institutions such as labor market regulations and the welfare state play an important role in unions' capability of recruiting new members (Ebbinghaus et al., 2011; Ebbinghaus and Visser, 1999). As pointed out before, the existence of union-based unemployment insurance functions as a club good that reduces the challenges of unions' public goods character. In contrast, government-based unemployment insurance or the extension of collective agreements on non-unionized firms, disincentivizes individual employees to join a union. Low levels of employment protection or labor market segmentation weakens the bargaining position of employees (Dorigatti, 2017) and makes them more prone to management opposition. Other explanations for the decline in union memberships are the shift of economic growth in typically non-unionized sectors (Farber and Western, 2001), changes in social norms (Akerlof, 1980; Booth, 1985; Goerke and Pannenberg, 2004; Ibsen

et al., 2017), and socio-economic, cultural, and demographic changes (Blanchflower, 2007; Blanchflower and Bryson, 2022; Fitzenberger et al., 2011; Leschke and Vandaele, 2018; Schnabel and Wagner, 2005).

All these aspects can explain the overall downward trend in unions' influence. With fewer union members and lower collective agreement coverage, this shifts the responsibility of wage bargaining to individual employees. As a result, the bargaining position of an individual employee becomes more important for their wages and working conditions.

## **1.2 Power Imbalances in the Labor Market**

Labor markets are characterized by power imbalances between employers and individual employees. If only a few potential employers are available for a given occupation and qualification, this weakens an individual employee's bargaining position due to limited outside options. Even in the presence of a large number of potential employers, individual employees' bargaining positions can be reduced by possessing limited information about outside options (Caldwell and Harmon, 2019; Demir, 2022; Jäger et al., 2024; Sockin and Sojourner, 2023), limited mobility (Bassier et al., 2022; Caldwell and Danieli, 2024), and limited opportunities due to discrimination (Hirsch and Jahn, 2015; Hirsch et al., 2010). Despite the absence of information limitations and mobility constraints on online platforms, research suggests that monopsonistic competition continues to exist (Dube et al., 2020), indicating that employers' power over employees even exists in labor markets that favor employees' bargaining positions. This power imbalance grants employers the ability to set wages and control working conditions to their advantage, ultimately allowing them to suppress wages and maintain unfavorable labor conditions (Ashenfelter et al., 2010; Manning, 2013).

In every industrialized country, employees have the freedom to join employee associations (unions) to collectively promote their labor market interests. By organizing in unions, employees can build a bargaining position that matches employers bargaining power over individual employees, leading to better wages (Benmelech et al., 2022; Card, 1996; Schubert et al., 2022), working conditions (Marinescu et al., 2021; Qiu and Sojourner, 2023), and a reduction in income inequality (Card et al., 2013; Farber et al., 2021; Western and Rosenfeld, 2011).

## **1.3 Management Opposition against Unionization**

Just as individual employees have an economic interest in joining unions, employers have an economic interest in preventing the unionization of their workforce to protect firm profits (Freeman, 1986; Freeman and Kleiner, 1990) and performance due to the negative impact of labor disputes (Krueger and Mas, 2004; Naidu and Reich, 2018). Management opposition to union activity may vary in terms of specific actions, but is a common phenomenon in industrialized economies (Gall and Dundon, 2013).

Management opposition can manifest in various forms, including direct actions such as

threatening consequences and firing union-supportive employees (Bronfenbrenner, 1997, 2009; McNicholas et al., 2019) as well as impeding the career advancement of union activists (Brébion, 2022). Additionally, more subtle tactics involve avoiding the hiring of union members (Baert and Omeij, 2015; Leap et al., 1990; Saltzman, 1995) or using non-standard contracts, such as fixed-term agreements, outsourcing, and temporary employment agencies, to make organizing efforts more challenging and potentially suppress wages (Drenik et al., 2020; Goldschmidt and Schmieder, 2017; Hatton, 2014). These tactics collectively restrict employees' rights, weaken the bargaining power of individual employees, and limit their ability to collectively negotiate through unions.

While the literature on management opposition to unionization is not a new area of research within industrial relations and labor economics, it has predominantly been descriptive in nature. The diverse array of management tactics employed to prevent unionization, coupled with challenges related to selection biases and unobservables raise the question of how to accurately measure the true extent of management opposition and understand its impact on the behavior of employees and employers.

## 1.4 This Dissertation

This dissertation aims to improve our knowledge about management opposition towards unions and employees' willingness to unionize in the presence of opposition. It consists of three essays that utilize correspondence experiments, survey experiments, and quasi-experimental methods to learn about management opposition and its relevance for employer and employee decision-making in Germany and the United States.

### 1.4.1 Essay 1: Management Opposition, Strikes and Union Threat

Essay 1 (Chapter 2) presents evidence for management opposition in terms of hiring discrimination in Germany. As a country that is known to be more subtle in its opposition to employee representation, through works councils and unions (Behrens and Dribbusch, 2013), the question arises whether management opposition is a problem at all. Due to the illegal nature of many management opposition tactics, potential biases due to selective measurement, and unobservable actions, measuring management opposition in a representative way for all sectors and firm types is a challenge.

I overcome this challenge with a correspondence experiment. I sent 13,000 fictitious job applications, revealing union memberships in the applications and a pro-union sentiment via social media activities. The use of fictitious job applications allows me to avoid biases due to social desirability, selection, selective measurements, and unobservable heterogeneity, which are likely to affect previous descriptive analyses of management opposition against unionization. By further revealing a pro-union sentiment via social media activities in a more subtle way, I test the robustness of the revelation of union memberships in the application process.

I provide evidence of hiring discrimination against union supporters, with strong variation between sectors and firm types. Callback rates of union members are on average 15%

lower compared to the non-union member counterpart. Discrimination is strongest in large firms without a collective agreement in strongly organized sectors. Discrimination is positively associated with the sectoral share of firms that voluntarily orientate wages to collective agreements. I further show that discrimination increases if a sector is exposed to an intense strike. These results indicate that hiring discrimination can be explained by union threat effects. They further indicate that collective agreements have spillover effects on the wages of firms without a collective agreement, as long as employers fear the possibility of unionization.

#### 1.4.2 Essay 2: Labor Market Tightness and Union Activity

Essay 2 (Chapter 3), which is joint work with Chantal Pezold and Simon Jäger, presents evidence for the moderate effect of the relevance of labor market conditions for employees' willingness to unionize in the United States. As a country that is known for its large share of firms that engage in unfair labor practices during unionization attempts (McNicholas et al., 2019), the question arises of how improvements in labor market conditions in terms of individual employees' outside options affect their willingness to engage in union activities. In tight labor markets, employees may be more willing to engage in union activities, as the risk of retaliation leading to prolonged unemployment is lower (Naidu, 2022). However, the overall impact of labor market tightness is uncertain, as improvements in outside options make moving from one job to another easier. The improved outside options for individual employees may decrease their willingness to engage in union activities, as they might prefer to leave for another employer (Hirschman, 1970, 1993). To learn about how labor market conditions affect individual employees' willingness to engage in union activities and job search, we conduct a large-scale survey experiment among more than 5,000 private-sector workers. We elicit employees' own subjective job-finding probability and that of workers similar to them. We then provide a random subset of workers with information about the actual job-finding probability of workers similar to them, based on the most recent labor market data. We find that an exogenous shift in their beliefs about outside options increases their willingness to engage in union activities, as well as their job search intentions.

To learn about the general equilibrium effects of labor market conditions on union activities, we extend our analysis with three quasi-experimental research designs (Canonical Bartik, China Shock, and Demographic Change). These quasi-experimental designs did not uncover a systematic, substantial link between labor market conditions and broader union activities. We find that labor market tightness moderately increases the share of union members and job-to-job transitions. In contrast, we find that labor scarcity reduces the share of union members and job-to-job transitions. While these results provide evidence for the relevance of labor market conditions for union activity, they only contribute to a small extent to changes in union activities.

### 1.4.3 Essay 3: Management Opposition in Times of Crisis

**Essay 3** (Chapter 4) presents evidence for the relevance of labor market conditions for hiring discrimination against union supporters in Germany. Conventional economic theory predicts a decline in discrimination in labor markets that have difficulties filling vacancies (Blanchard and Diamond, 1994; Pissarides, 2000). The underlying rationale is that a firm with trouble filling a vacancy will prefer to hire an applicant with less popular characteristics over the risk of not filling the position. A tight labor market may make a firm more open to hiring a union member. However, in the case of union members, their bargaining position improves with labor market tightness. The resulting union threat effect could also increase the discrimination of union supporters.

I present empirical evidence on this theoretical ambiguity with a correspondence experiment on hiring discrimination against union supporters. I sent 5,006 fictitious job applications revealing union membership during the economic downturn due to the Covid-19 pandemic in 2020. The design similarities to Chapter 2 allow me to merge both datasets and to exploit regional and sectoral variation in labor market conditions over time.

I show that discrimination against union supporters exhibits pro-cyclical behavior, intensifying during economic upturns and diminishing during economic downturns. In contrast, I find no effect of occupation-specific labor market tightness on discrimination. The cyclicity of discrimination is concentrated among firms that are not covered by collective agreement. These findings support the idea that unions' bargaining power and the perceived threat of unionization are drivers for discrimination against union supporters. My results suggest that the approaching labor shortage is unlikely to dampen management's opposition to unionization.

## 1.5 Implications

Together all three essays show that labor market conditions and management opposition shape employers' and employees' behavior in the labor market.

I provide evidence that management opposition against unions and individual union members exists even in a country like Germany, which is known for its corporatism between employers and employees and is seen as a role model for the design of industrial relations (Jäger et al., 2022a). Yet, while management opposition exists to varying degrees in sectors, my results indicate that management opposition in terms of hiring discrimination against union supporters can be explained by unions' bargaining power and the threat of unionization. Discrimination is more prominent among firms not covered by a collective agreement. Discrimination is higher in sectors with a large share of union members and is related to the business cycle. Firms without collective agreement coverage discriminate in economic upturns, in phases where unionization attempts become more likely and decrease in economic downturns. The sectoral extent of discrimination further correlates with the voluntary orientation of collective agreements of not-covered firms. Together this suggests, first that management opposition in terms of hiring discrimination is driven by the threat of unionization, and second that the threat of unionization leads to spillover

effects of collective agreements on uncovered firms. At the same time, these results also imply that the absence of management opposition in an eroding industrial relations system is an indicator that unions are no longer perceived as influential opponents.

The decline of union membership rates and collective agreement coverage is not unique to Germany but can be found across the OECD (see Figure 1.1 and Figure 1.2). Unions' declining influence impedes their ability to set wages through collective bargaining. Because of their reduced threat potential, positive spillovers in non-organized firms and sectors will be limited.

Furthermore, these results support the idea that firms that have an economic interest in avoiding unionization are more likely to oppose unionization and to discriminate against individual union supporters. This is particularly relevant in countries or sectors with a low or declining collective agreement coverage. Over time this should lead to long-run increases in wage inequality. Such increases in labor market inequality in the presence of monopsonistic labor markets can be prevented by labor market reforms that strengthen unions and extend collective bargaining, for example by the introduction or extension of sectoral wage bargaining. The extension of the collective agreement coverage is expected to lower non-unionized firms' economic interest in opposing unionization and discrimination against individual union supporters. Due to this economic interest, a country that is willing to accept eroding industrial relations, particularly in terms of wage-setting, should ensure compliance with employee rights and minimum wages.

Recent increases in union activities indicate that the tightened post-Covid labor market could lead to a revitalization of unions among OECD countries. In the presence of perceived high levels of management opposition, in tight labor markets, employees may be more willing to take the effort and risk of unionizing. My results suggest that labor market conditions play a minor role in the current unionization drives. I show that labor market conditions in terms of employees' outside options, increase employees' willingness to unionize moderately. However, accounting for general equilibrium effects, we find only limited evidence for the relevance of labor market conditions for union activities. I provide two explanations for this observation. First, improving labor market conditions increases the willingness to quit and look for a new job more than it increases willingness to engage in unionization. Second, my results indicate, that tight labor markets increase employees' willingness to unionize, but simultaneous increases in management opposition to individual union members and unions, which can dampen the aggregate effects on actual unionization.



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## Chapter 2

# Management Opposition, Strikes and Union Threat

## Abstract

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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.

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

**Keywords:** Trade Unions, Industrial Relations, Management Opposition, Union Threat, Labor Disputes, Correspondence Audit, Field Experiments

## 2.1 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 (Bourdieu et al., 2022; Freeman and Kleiner, 1990; McNicholas et al., 2019) or avoiding the hiring of union members (Baert and Omeij, 2015; Leap et al., 1990; Saltzman, 1995). 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; Dinlersoz et al., 2017; Freeman and Kleiner, 1990), 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 bi-



ases, 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 Omeij (2015) and Kreisberg and Wilmers (2022). Baert and Omeij (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 Omeij (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 Aleks, 2015; Behrens, 2009; Bronfenbrenner, 1997; Dinlersoz et al., 2017; Heery and Simms, 2010; 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 (Button and Walker, 2020; Tilcsik, 2011). 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 2.2 provides an overview of the institutional setting of Germany's industrial relations system. Section 2.3 describes the experimental design of the first experiment, revealing union membership in the resume. Section 2.4 provides the experimental results. Section 2.5 describes the second experimental design revealing a pro-union attitude via the use of social media accounts. Section 2.6 provides the experimental results. Section 2.7 tests various sources of management opposition, including firm characteristics, labor market conditions and union strike activity.

Section 2.8 discusses my results in the context of the literature. Section 2.9 concludes.

## 2.2 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 Firm 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>1</sup>

Figure 2.1 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).

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<sup>1</sup>For a more detailed examination of the German model of industrial relations see Jäger et al. (2022).

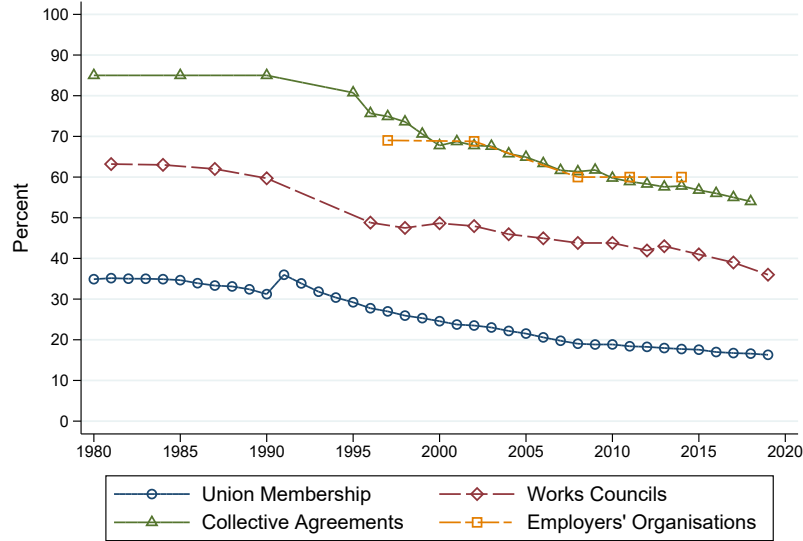


Figure 2.1: Industrial Relations Trends in Germany

*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.

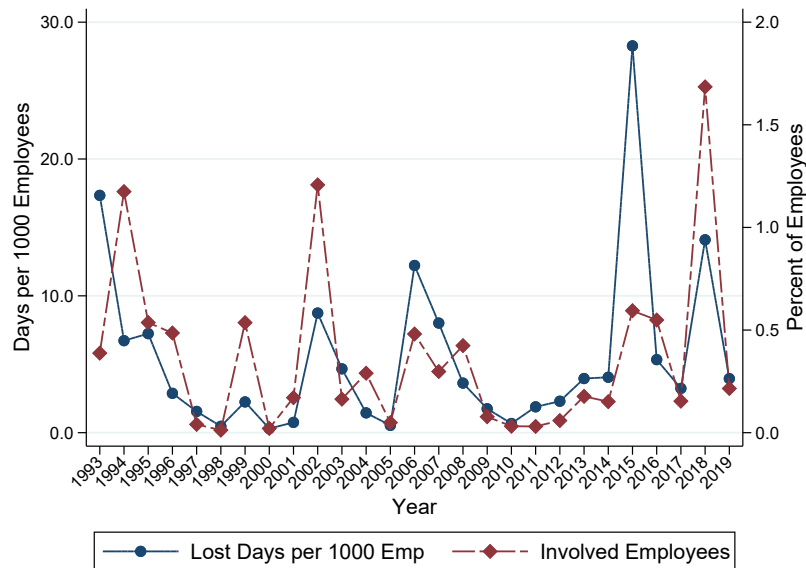


Figure 2.2: Labor Dispute Trends in Germany

*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.

The erosion of Germany’s industrial relations seems to have little effect on unions’ strike activities (see Figure 2.2). 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 2.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.

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.

## 2.3 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>2</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 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 Button and Walker, 2020; Tilcsik, 2011), 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 2.3.

For each application pair, one application is randomly selected to include a union mem-

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<sup>2</sup>Job portal of the German Federal Employment Agency: <https://www.arbeitsagentur.de/jobsuche/>.

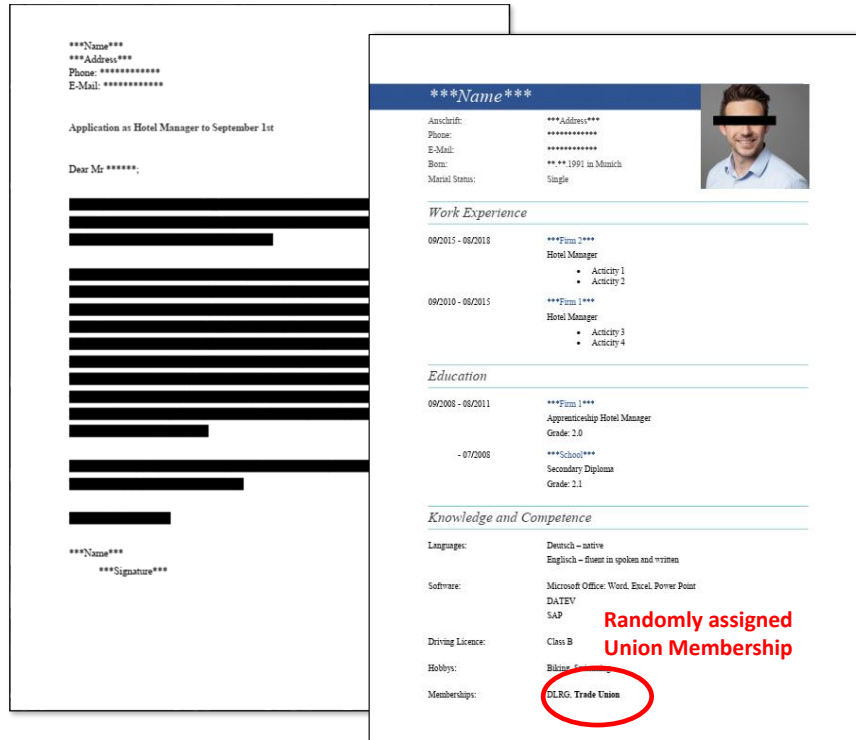


Figure 2.3: Example Application of Experiment 1

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

bership 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>3</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 2.1 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 4.A.3.1). With 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 4.A.3.1). In sum, this design allows for the analysis of repeated cross-section data of a sizable part of the German economy.

<sup>3</sup>The chosen unions are the IG Metall (Union of the Metal Industry), Ver.di (United Service Union) and the NGG (Food Workers' Union).

Table 2.1: 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>4</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).

## 2.4 Results from Experiment 1

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>5</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 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 2.A.4).

I begin by documenting the overall callback rates of the experiment. Figure 2.4 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%

<sup>4</sup>A list of typical employer responses and their classification is available in Appendix 2.A.1.

<sup>5</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.



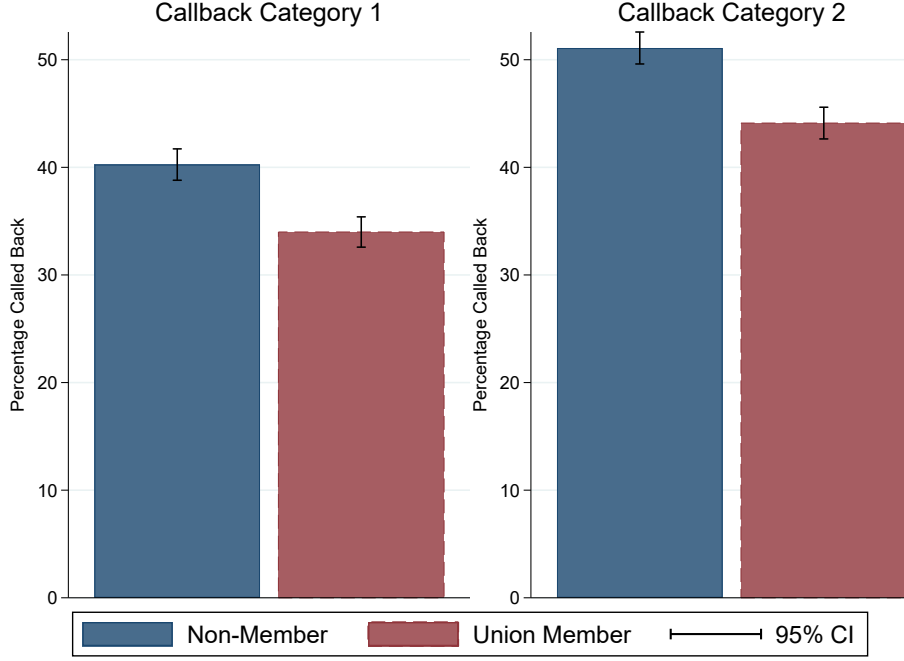


Figure 2.4: Callbacks by Membership

*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".

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.

### 2.4.1 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>6</sup>

Equation 2.1 contains the specified linear probability model, with one of the callback categories as the dependent variable.

$$Callback_{it} = \alpha + \tau Union_i + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (2.1)$$

<sup>6</sup>Summary statistics are available in Appendix 2.A.2.

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 2.2 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.

Table 2.2: 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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

I conduct additional sensitivity analyses with a step-by-step extension of the baseline specification of Equation 2.1, firm fixed effects as well as sub-samples and probit estimates. Overall these analyses are consistent with Table 2.2 and show that the effect of union membership is negative and significant at the 1% significance level for all specifications and years (see Table 2.A.9).

## 2.4.2 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 can be shown that for correspondence experiments in labor markets, this is a common bias (Neumark and Rich, 2019).<sup>7</sup> To account for the Heckman Critique, I

<sup>7</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

apply the Neumark Correction (Neumark, 2012), providing no evidence for a bias due to unobservable differences (Appendix 2.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 2.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 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.

## 2.5 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

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positive selection in terms of wages.

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 Acquisti and Fong, 2020; Baert, 2018; Manant et al., 2019; 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).

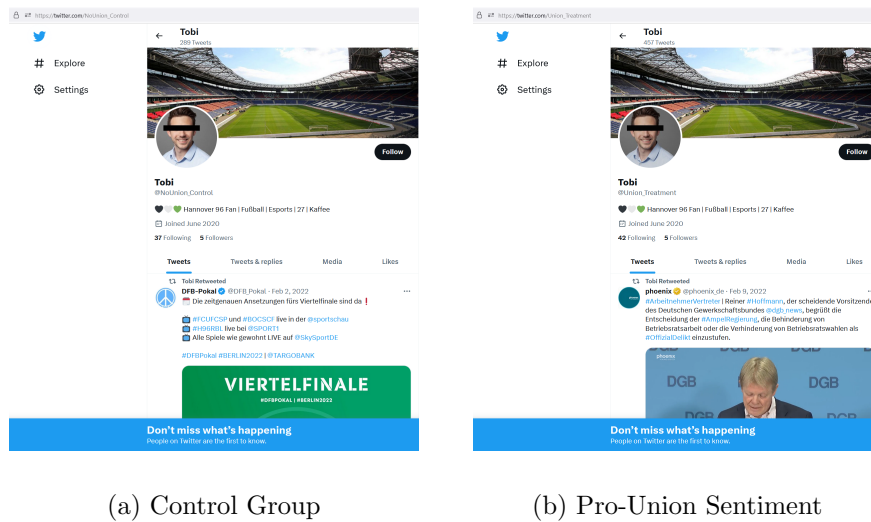


Figure 2.5: Twitter Accounts of Experiment 2

*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>8</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>9</sup> Figure 2.5 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 2.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.

## 2.6 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 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 2.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.

$$Callback_i = \alpha + \beta_1 Union_{Resume} + \beta_2 Union_{Twitter} + \beta_3 Twitter + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (2.2)$$

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<sup>8</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 (Balfe et al., 2023; Larsen, 2020; Weichselbaumer, 2015). 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>9</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.

Equation 2.2 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 2.3: 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 2.3 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 2.3 Columns 3 and 6). This suggests that the observed hiring discrimination is driven neither by information provision nor expected differences

in reservation wages.

## 2.7 Mechanisms of Management Opposition

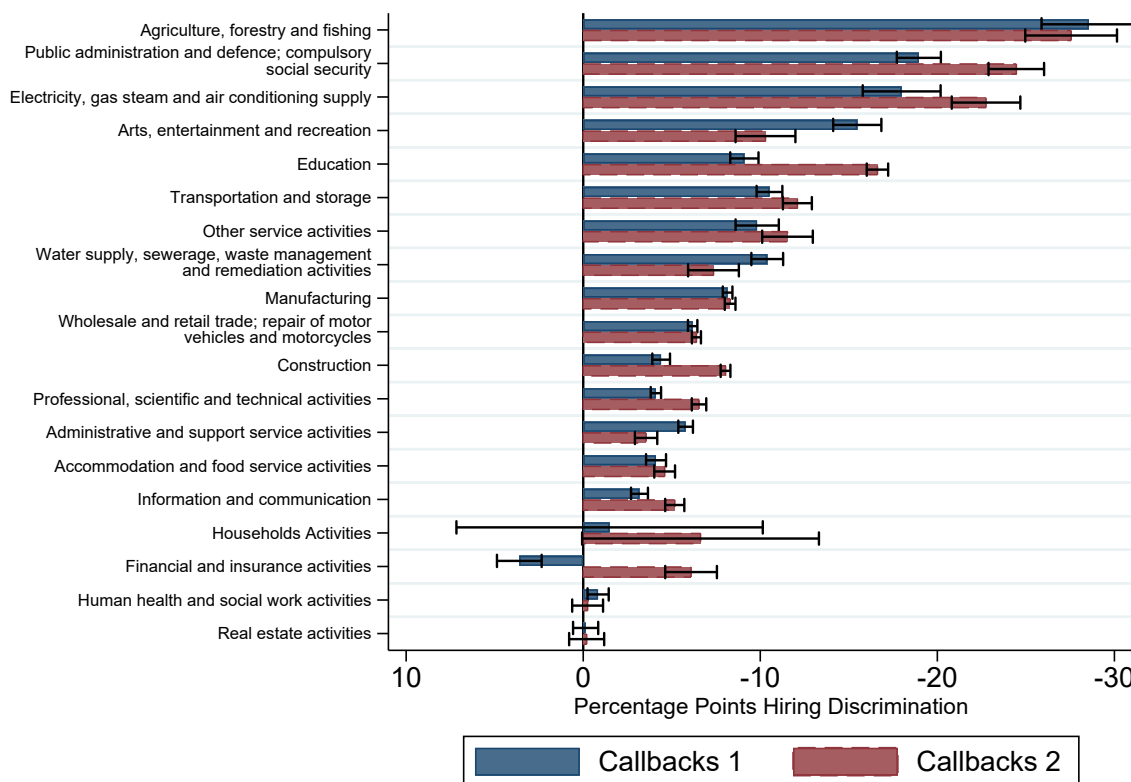


Figure 2.6: Hiring Discrimination by Sector (2017 to 2019)

*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

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 2.6 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 character-

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

### 2.7.1 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 2.3 mirrors Equation 2.1 augmented by interactions for firm and labor market characteristics.

$$Callback_i = \alpha + \tau Union_i + \mathbf{X}'_{it}\beta + \gamma Union_i \times \mathbf{X}'_{it} + \epsilon_{it} \quad (2.3)$$

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 (Dinlersoz et al., 2017; Farber, 2015; Maranto, 1988) and that management opposition increases with firm size (Aleks, 2015; Behrens, 2009; Bronfenbrenner, 1997; 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 (Baert and Omeij, 2015; Blanchflower and Cubbin, 1986; Jansen, 2014; Tsebelis and Lange, 1995) 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 2.4 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 2.4 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, discrimination is reduced to insignificance and an effect size of 0 to 1.5 percentage points.<sup>10</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 Omeij (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 2.A.11 considers heterogeneity by the type of union the

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<sup>10</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.

Table 2.4: 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 $\times$ Collective Agreement	0.0350** (0.0164)	0.0367** (0.0171)			0.0330** (0.0164)	0.0338** (0.0171)
Union $\times$ 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 $\times$ Smaller 6 Employees	0.0374** (0.0189)	0.0515** (0.0202)			0.0342* (0.0189)	0.0482** (0.0203)
Union $\times$ 51 to 500 Employees	0.0059 (0.0146)	-0.0022 (0.0156)			0.0110 (0.0149)	0.0030 (0.0158)
Union $\times$ 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 $\times$ Union Density			-0.2334*** (0.0880)	-0.2807*** (0.0933)	-0.2198** (0.0900)	-0.2500*** (0.0945)
Union $\times$ 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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

applicant was affiliated with (Table 2.A.15) the corporate legal form (Table 2.A.16), heterogeneity by the type of the collective agreement (Table 2.A.17) and heterogeneity by the gender of the human resource manager (Table 2.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.

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>11</sup>

Given the relevance of collective agreements, firm size and union density, these results

<sup>11</sup>Appendix 2.A.14 provides additional analysis of opposition to works councils, based on the 2015 WSI Works Council Survey.

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.

### 2.7.2 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 (Denice and Rosenfeld, 2018; Farber, 2005; 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>12</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 2.7 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 2.4 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 (within a sector or within a sector-state) on the probability of a positive callback for a union member.

$$Callback_{it} = \alpha + \tau Union_i + \delta Strikes_{it} + \gamma Union \times Strikes_{it} + \mathbf{X}'_{it}\beta + \epsilon_{it} \quad (2.4)$$

The two panels of Table 2.5 report the results depending on strike intensity based on

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<sup>12</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 2.A.19.

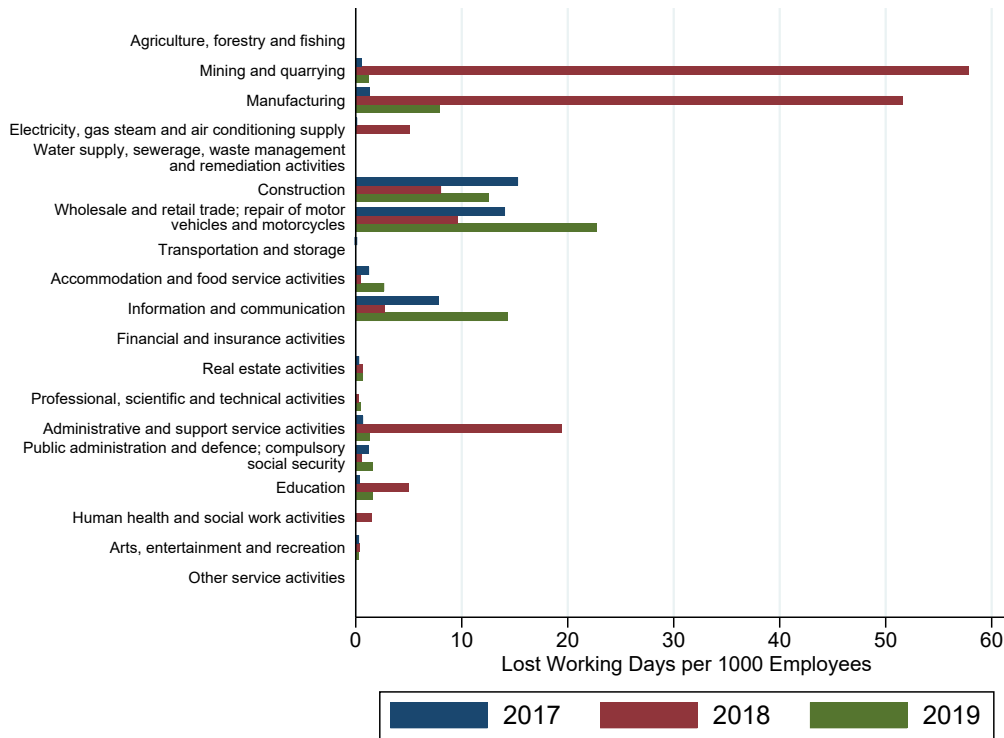


Figure 2.7: Sectoral Distribution of Strike Intensity (2017 to 2019)

*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.

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 2.5 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 2.5). 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 2.5 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>13</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 2.5: 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). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 2.7.3 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.

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

<sup>13</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  $\frac{0.0715 - 0.0655}{0.0655} = 0.091$ .

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 (Bassier, 2022; Demir, 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 2.8 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 2.8 shows the association between hiring discrimination and collective agreement coverage and the voluntary orientation on collective agreement wage setting.<sup>14</sup> Figure 2.8 shows the association of sector-specific hiring discrimination with the sectoral share of collective agreements (Panel 2.8a and 2.8b) and the share of firms that do not voluntarily orientate on collective agreement wage setting (Panel 2.8c and 2.8d). 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 2.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.

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<sup>14</sup>Sectoral hiring discrimination is reclassified and calculated, according to the reduced sectoral classification of the IAB firm panel used by Ellguth and Kohaut (2019).

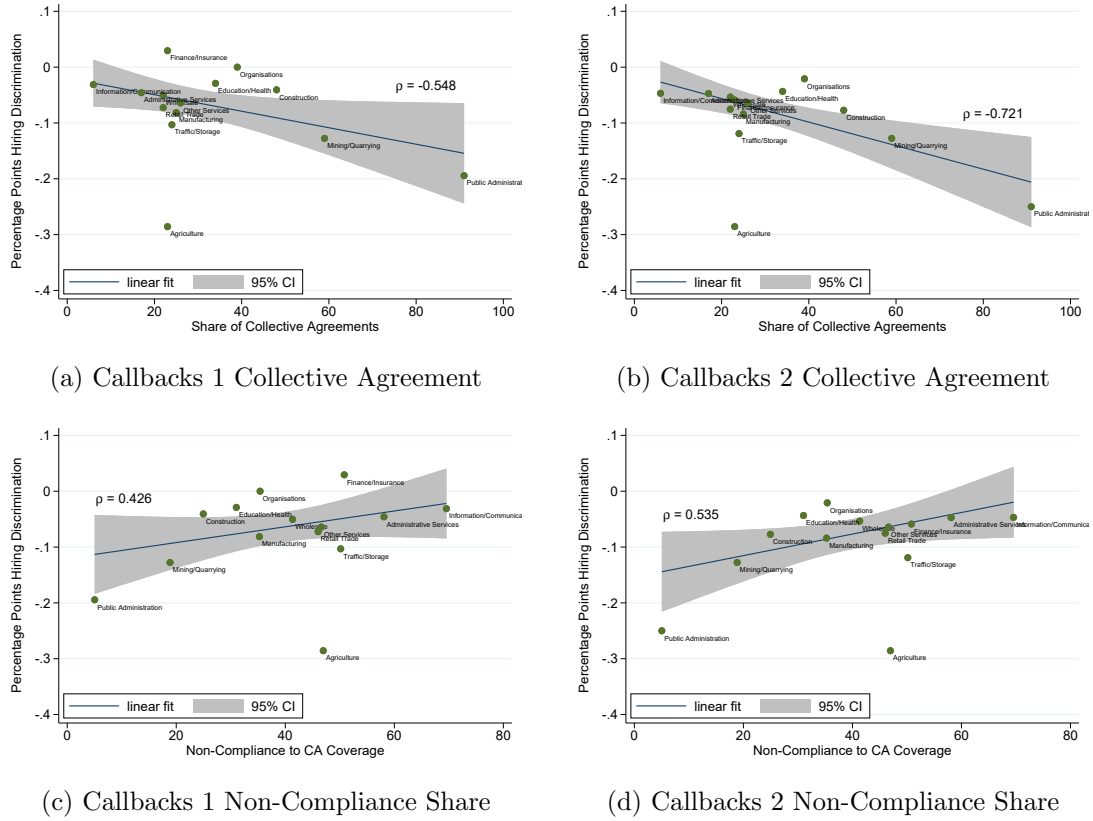


Figure 2.8: Sectoral Hiring Discrimination and Collective Bargaining Coverage

*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 Firm-Panel for 2018 by Ellguth and Kohaut (2019). Weighted by sectoral employment share.

## 2.8 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 Omeij (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 institutions. 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>15</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 2.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

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<sup>15</sup>This is a pattern that can be shown to hold for US labor law violations in general (Marinescu et al., 2021).



provides little evidence for heterogeneity in discrimination related to the corporate legal form (see Table 2.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 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.

## 2.9 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 Omeij (2015) and Kreisberg and Wilms (2022). In contrast to Kreisberg and Wilms (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 Omeij, 2015) and the United States in general (Kreisberg and Wilms, 2022) suggest worsening wages and working conditions in the future.

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## 2.A Appendix to Chapter Two

### 2.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...”



## 2.A.2 Summary Statistics of Experiment 1

Table 2.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 2.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 respondent 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  $\frac{(3)+(4)}{(3)+(5)}$ .

### 2.A.3 Representativeness of the Sample

Appendix 2.A.3 provides an overview of the representativeness of the experimental sample. Table 4.A.3.1 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 2.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 2.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 2.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 2.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.

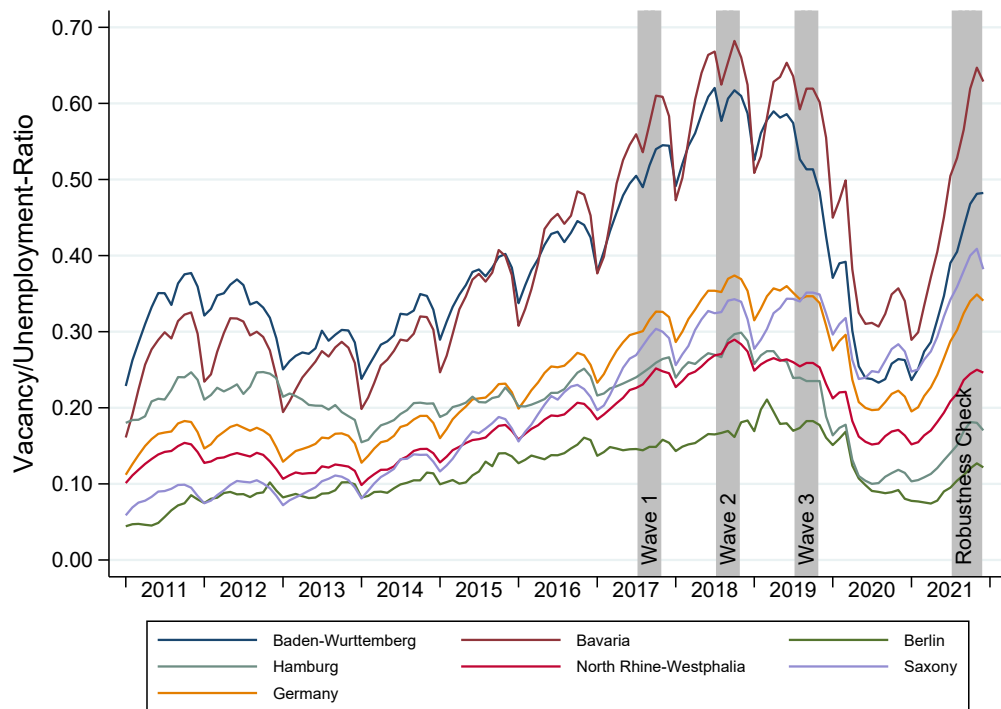


Figure 2.A.1: Regional Labor Market Tightness over Time

*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.

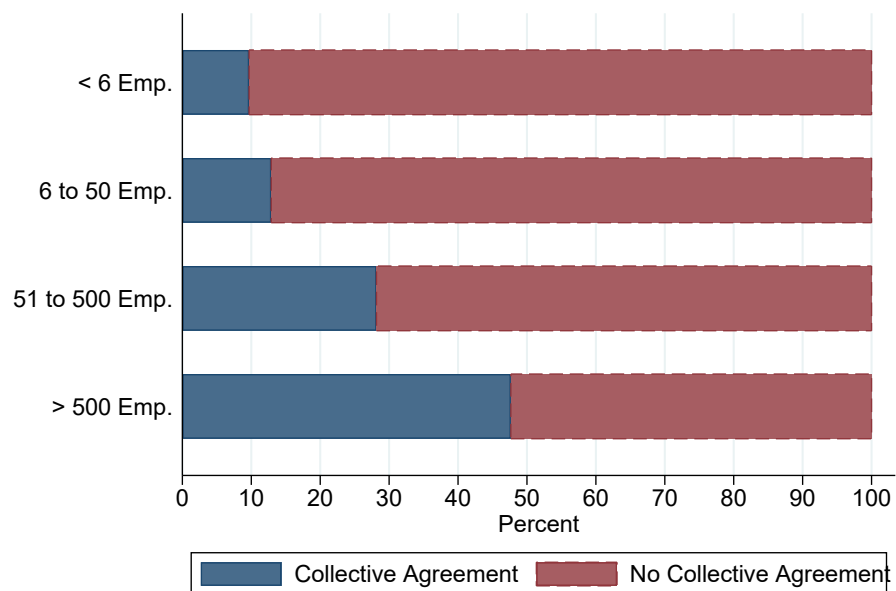


Figure 2.A.2: Collective Agreement Coverage by Firm Size of the Experiment

*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.

## 2.A.4 Randomization Checks Wave 2017 to 2019

Table 2.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 2.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 2.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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



## 2.A.5 Robustness Check Main Specification

Table 2.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 2.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 2.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 <sup>2</sup>	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 <sup>2</sup>	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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### 2.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 (Becker et al., 2019; Carlsson et al., 2014; Neumark, 2012; Nüß and Penny, 2022; Weichselbaumer, 2020).

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 2.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 2.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 2.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 2.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 discrimination 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; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.A.7 Robustness by Weighting

Figure 2.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.

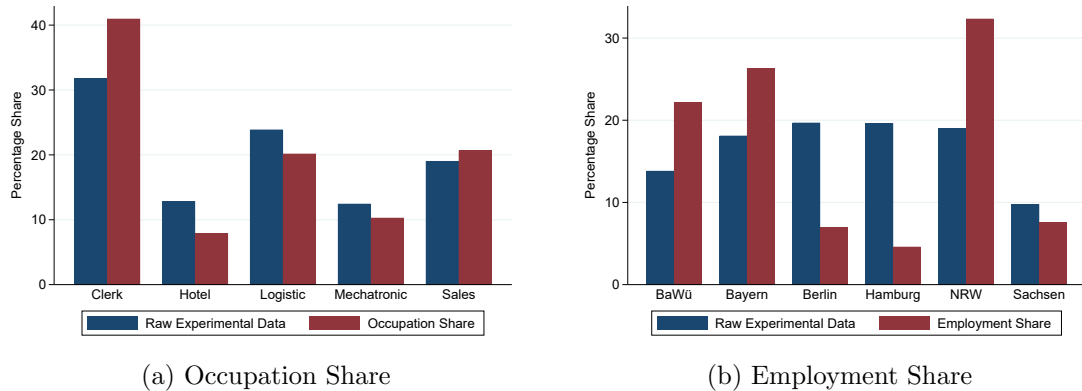


Figure 2.A.3: Representativeness by Occupation and Employment Share

*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 2.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 2.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 2.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; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.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](#)).

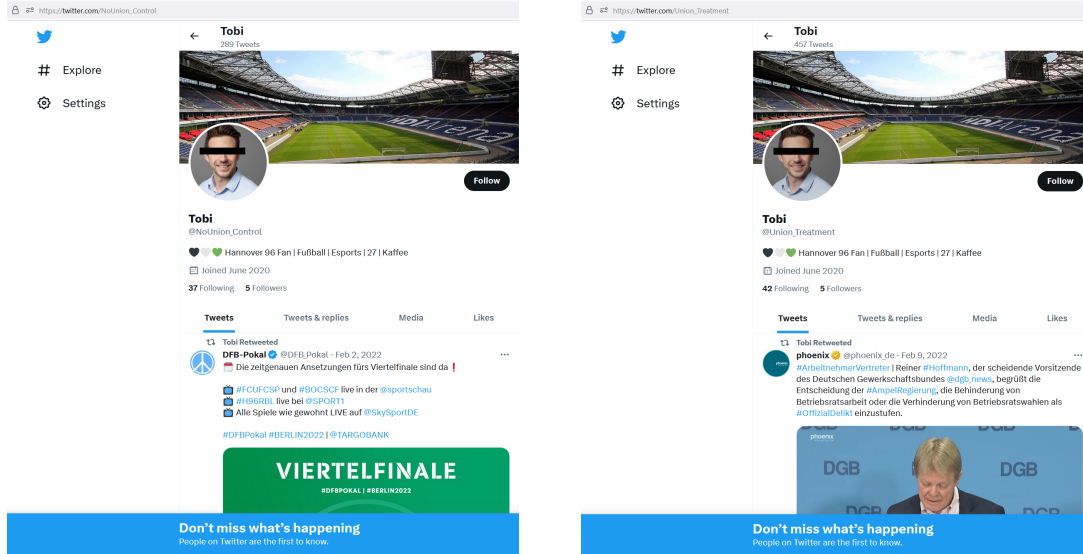
Likes and Retweets were scheduled during typical break times (breakfast and lunch), after work and at weekends to avoid any signal of lower productivity during working time (see Table 2.A.12).

Table 2.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 2.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](#).



(a) Control Group

(b) Pro-Union Sentiment

Figure 2.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.

### *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 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 2.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 2.A.5 Panel (b)).



Figure 2.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.

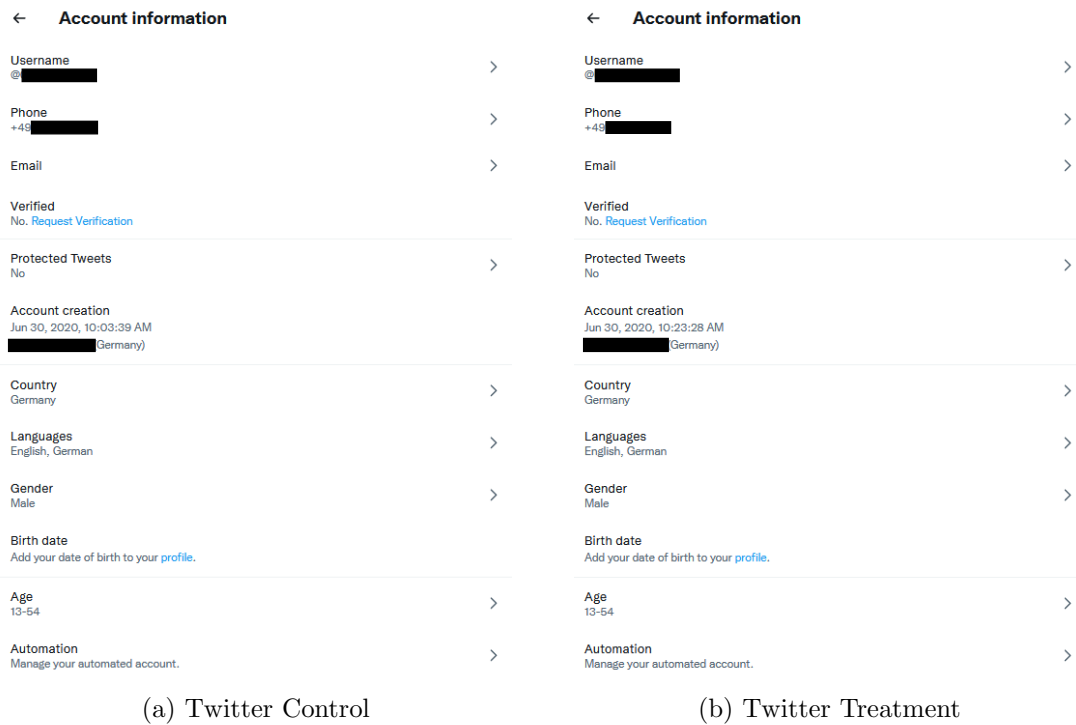


Figure 2.A.6: Twitter Account Settings

*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 publicly available during the experiment.



## 2.A.9 Summary Statistics of Experiment 2

Table 2.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.

## 2.A.10 Randomization Check Social Media Experiment

Table 2.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 subcategories tests for the success of the randomization. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.A.11 Heterogeneous Effects by Characteristics

Table 2.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 2.A.16: Hiring Discrimination by Corporate Legal Form

	(1)	(2)	(3)	(4)
Union	-0.0627*** (0.0063)	-0.0698*** (0.0067)		
Union $\times$ Sole Proprietorship			-0.0370* (0.0222)	-0.0154 (0.0241)
Union $\times$ Limited Liability Company			-0.0651*** (0.0080)	-0.0710*** (0.0085)
Union $\times$ Limited Liability Partnership			-0.0675*** (0.0152)	-0.0852*** (0.0160)
Union $\times$ Civil Law Partnership			-0.0672** (0.0309)	-0.1261*** (0.0348)
Union $\times$ Stock Corporation			-0.0738** (0.0344)	-0.0940*** (0.0360)
Union $\times$ Public Institution			-0.0930** (0.0455)	-0.1047** (0.0467)
Union $\times$ 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 2.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 2.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; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.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 2.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 2.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.

## 2.A.13 Robustness Check Collective agreement

Table 2.A.20: Raw Discrimination Relation to 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 2.A.21: Raw Discrimination Relation to 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 2.A.22: Estimated Discrimination Relation to Sectoral Collective Agreement Coverage and Compliance (Weighted)

	Estimated Discrimination 1		Estimated Discrimination 2	
	CA Coverage	No-Compliance	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 2.A.23: Estimated Discrimination Relation to 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.

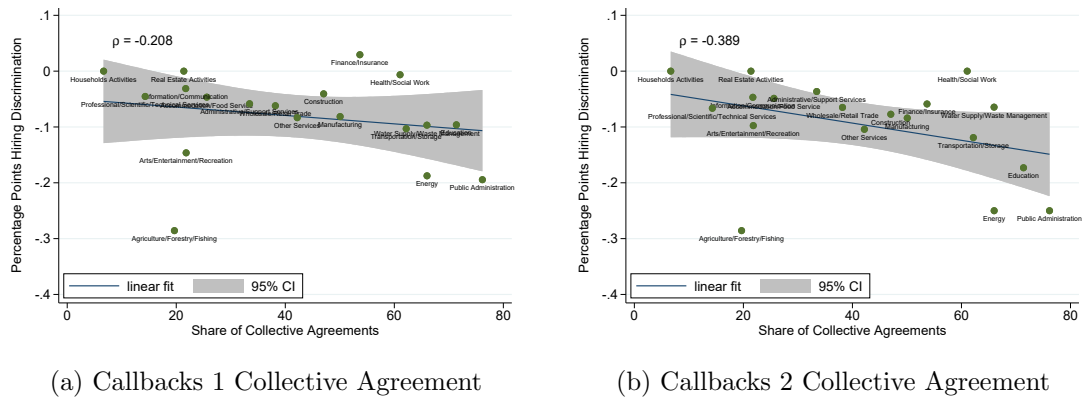


Figure 2.A.7: Sectoral Hiring Discrimination and Collective Agreement Coverage based on SOEP

*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 2.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.

### 2.A.14 Opposition to Works Councils

In Section 2.2, 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 2.7.1 Table 2.4). 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 2.7.1, 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 2.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 2.A.25), these patterns neither reflect the patterns in Section 2.7.1 nor those in the previous literature on management opposition to unions (Aleks, 2015; Behrens, 2009; Bronfenbrenner, 1997; 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 2.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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 2.A.15 Union Members Political Preferences

Table 2.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.

## 2.A.16 Industrial Relations Background Information

Table 2.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.

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# Chapter 3

## Labor Market Tightness and Union Activity

## Abstract

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We study how labor market conditions affect unionization decisions. Tight labor markets might spur unionization, e.g., by reducing the threat of unemployment after management opposition or employer retaliation in response to a unionization attempt. Tightness might also weaken unionization by providing attractive outside alternatives to engaging in costly unionization. Drawing on a large-scale, representative survey experiment among U.S. workers, we show that an increase in worker beliefs about labor market tightness moderately raises support for union activity. Effect sizes are small as they imply that moving from trough to peak of the business cycle increases workers' probability of voting for a union by only one percentage point. To study equilibrium effects, we draw on three quasi-experimental research designs using data from across U.S. states and counties over several decades. We find no systematic effect of changes in aggregate labor market tightness on union activity. Overall, our results challenge the notion that labor market tightness significantly drives U.S. unionization.

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**JEL classification:** J51, J52, J53, J83, E32, D83

**Keywords:** Trade Unions, Industrial Relations, Outside Options, Business Cycles, Employer Retaliation

### 3.1 Introduction

A long-standing idea is that improvements in outside options affect employment relations and behavior in the labor market (Acemoglu and Wolitzky, 2011; Hirschman, 1970; Naidu and Yuchtman, 2013). The recent momentum of union organizing across the U.S. has sparked conjectures about a broader resurgence of the U.S. labor movement driven by tight labor markets. Tight labor markets may induce workers to risk unionizing as potential retaliation from employers may less likely lead to long unemployment (Cohen and Hurd, 1998; Katz, 2022; Naidu, 2022)<sup>1</sup>. However, the sign of the effect of labor market tightness is ambiguous as tightness may also facilitate worker moves to other employers. The increased availability of such exit options may reduce workers’ willingness to engage in costly voice at their current workplace (Hirschman, 1970, 1993). In addition to such trade-offs faced by workers, labor market tightness or scarcity may also have equilibrium effects on employers responses to unionization. Overall, and similar to worker-level effects of tightness, the sign and magnitude of the aggregate effect of tightness or labor scarcity on unionization is theoretically ambiguous (Acemoglu and Wolitzky, 2011).

We contribute to a resolution of this debate by studying how labor market conditions affect unionization. To provide context for our main analysis, we revisit descriptive macro evidence and find suggestive evidence for a positive relationship between unionization tightness in a panel of OECD countries though no clear time series association in the U.S. Our main analysis then proceeds in two steps: first, we conduct a large-scale, representative, and incentivized survey experiment with more than 5,000 U.S. workers. Second, we leverage three quasi-experimental research designs to study equilibrium effects of labor market tightness on union activity.

Our survey experiment measures effects of exogenous shifts in worker beliefs about labor market tightness on their willingness to engage in unionization activities. To obtain micro-level evidence for the effect of changes in local labor market conditions on union activity, we first elicit workers’ beliefs about the probability of finding a new job within three months after unexpectedly being laid off—our measure of beliefs about personal labor market tightness.<sup>2</sup> We measure beliefs both about workers’ own subjective job finding probability and about those of workers similar to them. We then provide a random subset of workers with information about the actual job finding probability of people very similar to them, which we construct based on the most recently available Current Population Survey (CPS) data. In particular, we employ machine learning methods to model the probability of finding a new job within three months after being laid off as a function of workers’ current age, education, local labor market region, occupation and industry. This allows us to obtain an objective benchmark of every worker’s personalized job finding probability that we disclose to participants in the treatment group (similar to the approach

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<sup>1</sup>Indeed, the probability of a plant shutdown increases in response to unionization Wang and Young (2022).

<sup>2</sup>Davis and von Wachter (2011) provide evidence of substantially larger costs of job loss in recession. In addition, they provide survey evidence that workers’ subjective beliefs about labor market conditions respond rapidly to actual changes in labor market conditions. See also He and Kircher (2023) for recent evidence on the effect of aggregate labor market conditions on individual job-finding beliefs.

in Jäger et al., 2024, for wages). Receiving information about labor market tightness meaningfully changes workers’ beliefs about similar workers’ job finding probabilities and also affects their own job search intentions and assessment of the consequences of being laid off.

We find that increases in worker beliefs about labor market tightness lead to only a small rise in intentions to engage in several union activities. A 10ppt increase in workers’ beliefs about the probability of finding a new job within three months implies a one percentage point (ppt), or 1.91%, increase in workers’ willingness to vote pro-union if an election were to be held at their establishment today. We also report estimates for a combined index of union activity outcomes, capturing pro-union vote, willingness to make a pro-union vote public, organizing an election campaign, and intention to go on strike. For this index measure, we find that a 10ppt increase in the subjective job finding probability implies a 0.04SD increase in overall union activity. This ten percentage point increase in the subjective job finding probability roughly corresponds to observed changes from the trough to the peak of a business cycle. We thus interpret the effect sizes we measure to imply small individual-level effects of labor market tightness on unionization intentions. We also measured effects on several revealed-preference measures of union activities—workers’ willingness to engage in writing union-related messages during the experiment as well as willingness to pay for information on unionization—and find precisely estimated zero effects on these outcomes. To capture priors of the effects of labor market tightness on unionization among experts in labor economics, we also conduct an expert survey, where respondents predicted substantially larger, positive effects of tightness beliefs on union activity (measured as the probability to vote for a union).

Our rich survey data allows us to shed light on potential mechanisms that can explain our worker-level results. First, we document that our treatment changes job search intentions and reduces concerns about layoffs. Therefore, the treatment does meaningfully change workers’ perceptions of the external labor market and reduces concerns about job loss. But those changes do not translate into tantamount effects on unionization activities. Second, we assess changes in worker beliefs about anticipated retaliation in case of a unionization attempt. For example, one might expect that management opposition to unionization softens as it may be less effective in a tight labor market. However, we find that workers’ assessment of management opposition to unionization and also the probability of firing in response to a unionization attempt remains unchanged. Thus, the moderate changes in unionization intentions that we measure appear to be driven by workers’ assessment of the external labor market and changes in the (dis)utility of job loss, rather than by changes in the probability with which workers expect to face adverse consequence in the case of a unionization attempt. We can also reject an alternative mechanism related to tightness and profitability. Tightness may raise unionization efforts not by providing retaliation insurance to workers but instead by making unionization more rewarding as tightness signals high product and labor demand and thus high productivity and profits (see Abowd and Farber, 1990; Dinlersoz et al., 2017). We assess workers’ perception of their firms’ profitability and find it to be unchanged by the treatment. Finally, we find evidence that workers’ own unionization decisions crucially hinge on their coworkers’ attitudes—thereby

opening up the possibility that aggregate changes in tightness may trigger larger changes in unionization through social spillovers and strategic complementarities in unionization efforts (Glaeser et al., 2003; Hertel-Fernandez et al., 2020; Naidu, 2022).

To complement our experimental, worker-level results and further investigate such spillover effects, we draw on three different research designs to analyze aggregate, quasi-experimental variation in labor market tightness and labor scarcity. To estimate the impact of demand-driven tightness, we draw on a Bartik (1991) instrument and also analyze exposure to Chinese import competition at the commuting-zone level, following Autor et al. (2013) (see also Ahlquist and Downey, 2023, for a state-level analysis). To study supply-driven changes in labor scarcity (the mediator studied in Acemoglu and Wolitzky, 2011), we predict future retirements (Böhm et al., 2021; Böhm and Siegel, 2021) as well as the cohort size of new labor market entrants using the lagged demographic age structure of a commuting zone. All three approaches lead to similar results: aggregate labor tightness or scarcity does not significantly raise unionization. Effects on most outcomes are not statistically significant, we do find statistically significant effects in some specifications and for some outcomes, though no clear pattern emerges and we can rule out small, positive effects in many specifications. We also elicited experts' beliefs about predicted effect signs and sizes (for the research design with demographic variation) and found that experts predict a substantially stronger, more positive effect of labor scarcity on unionization. All in all, we conclude that the quasi-experimental research designs reveal no systematic effect of labor market tightness or labor scarcity on unionization.

This paper speaks to several strands of literature. First, we contribute to the literature that studies the determinants of unionization, and particularly its decline in the U.S., over recent decades (Ahlquist and Downey, 2023; Charles et al., 2021; Clawson and Clawson, 1999; Farber and Western, 2002; Hirsch, 2008; Jarley and Kuruvilla, 1994; Lipset and Schneider, 1983; Pezold, 2023; Scruggs and Lange, 2002; Southworth and Stepan-Norris, 2009; Tope and Jacobs, 2009; Wallerstein and Western, 2000; Western, 1997). In particular, we contribute to the literature that has investigated the role of the business cycle for union activity. Most closely related to our work, Charles et al. (2021) and Ahlquist and Downey (2023) study the consequences of the China Shock on U.S. union elections and union membership. Jarley and Kuruvilla (1994) and Lipset and Schneider (1983) study the macroeconomic determinants of public approval of labor unions and their leaders, respectively, over time. Their descriptive findings imply that a decrease in the national unemployment rate is associated with an increase in union approval. Relatedly, previous work has found high unemployment to be associated with fewer and shorter strikes (Ashenfelter and Johnson, 1969; Card, 1990; Cramton and Tracy, 1994; Devereux and Hart, 2011). We contribute to this literature the first experimental study on the role of worker beliefs about labor market tightness for individual unionization decisions. Beyond that, we substantially expand the analysis of equilibrium effects of changes in labor market tightness on union activity. We document limited effects from individual-level, experimental variation and no systematic effects from aggregate, quasi-experimental variation in tightness on unionization activity, including strikes.

In addition, we speak to the literature on management opposition to union activity. This literature documents a wide range of employer tactics to discourage unionization (Cohen and Hurd, 1998; Nüß, 2023; Schmitt and Zipperer, 2009), for example, the closure of establishments (Freiberg and Dickens, 1985; Wang and Young, 2022), or the threatening and firing of union supporters (Bronfenbrenner, 1997, 2009; McNicholas et al., 2019). Relatedly, we contribute to the debate on the role of labor scarcity in shaping employers’ incentives to stifle unions and perhaps workers more generally (Acemoglu and Wolitzky, 2011; Nüß, 2024). We contribute evidence for the role that workers’ outside options play in unionization decisions when employer opposition to union activity is strong.

Finally, our work speaks to a body of literature investigating the consequences of labor market tightness and external labor market conditions for wages and worker mobility (Autor et al., 2023; Beaudry and DiNardo, 1991; Hagedorn and Manovskii, 2013; Jäger et al., 2020).

The remainder of this paper is structured as follows. Section 3.2 reviews the U.S. institutional context. Section 3.3 presents macro evidence from U.S. time series data as well as from a panel of OECD countries. Section 3.4 introduces our experimental design to study micro-level effects of labor market conditions on unionization decisions and presents our experimental results. In Section 3.5, we propose a simple framework of unionization under strategic complementarity and employer retaliation. We study equilibrium effects using three quasi-experimental research designs in Section 3.6. Section 3.7 concludes.

## 3.2 U.S. Institutional Setting

The U.S. National Labor Relations Act (NLRA) of 1935 established legal rights for private-sector workers in the U.S. to join a union, bargain collectively with their employer over wages and working conditions, and to participate in strikes. This act is enforced and regulated by the National Labor Relations Board (NLRB), a federal agency primarily tasked with safeguarding the rights of both employers and employees during processes of unionization and collective bargaining.

**Unionization and Union Elections.** Collective bargaining between unions and employers typically occurs at the establishment level. If the employer is supportive, they may choose to voluntarily recognize a union based on evidence of majority support (usually through authorization cards). More commonly, unionization occurs through a union election. To initiate an election, employees need to collect signatures from at least 30% of employees in the affected bargaining unit (a group of employees with a clear and identifiable community of interests). With the initiation of the election process, it is deemed illegal for employers to interfere with worker’s participation in the election. Once a petition has been submitted to the NLRB and has passed a review process, e.g., to ensure common interests of workers within the bargaining unit, the NLRB then schedules and organizes the election. If strictly more than 50% of all employees offer their support in the union election, the elected union is certified to represent the employees in this bargaining unit

in negotiations with the employer. The employer must formally recognize the union and is required to engage in good-faith negotiations with the union over terms and conditions of employment.

**Employer Retaliation.** With the initiation of the election process, it is deemed illegal for employers to interfere with worker’s participation in the election.<sup>3</sup> Yet, the unionization process is often highly contentious. The employer, the union, or the employees can contest the conduct or results of a union representation election and the NLRB can order a new election or take other remedial actions (Frandsen, 2017). Yet, out of all union elections supervised by the NLRB in the period from 2016 to 2017, 41.5% of employers were charged with an unfair labor practice (McNicholas et al., 2019) and several management practices to prevent unionization have been documented. These include, for example, capturing targeted dismissals of employees, threatening of benefit cuts, and targeted surveillance (see, e.g., Bronfenbrenner, 1997, 2009; McNicholas et al., 2019). Employers sometimes threaten to close down plants in response to unionization Bronfenbrenner (1996) and the probability of a plant shutdown indeed increases in response to unionization Wang and Young (2022). Accounting for the financial impact of unionization in comparison of potential legal sanctions when found guilty of unfair labor practices, Stansbury (2021) shows that the current system of sanctions and enforcement of the NLRA incentivizes firms to oppose unionization. All in all, the probability of a concluded contract rises only by about 27 percentage points after a successful, narrow union election (Frandsen, 2021).

**Strikes.** A large majority of private-sector employees, unionized or not, have the right to participate in a protected strike for economic reasons or to stand up against unfair labor practices<sup>4</sup>. Economic reasons capture work stoppages as part of negotiations of wages and working conditions, while strikes targeted at employers to sanction them for unfair labor practices are deemed legal if employers engaged in at least one of the actions described in Section 8 of the NLRA.

### 3.3 Descriptive Macro Evidence: U.S. Time-Series and OECD Panel Data

Before describing our survey experiment and quasi-experimental research designs, we briefly review time series evidence from the U.S. as well as panel data from the OECD countries.

Figure 3.1 reports several time series for the U.S. from the early 2000s to 2023. Panel A shows (inverse) tightness, measured as the number of unemployed workers per vacancy, with peak slack of 6.5 during the Great Recession in 2009 and peak tightness of 0.5 in early 2022. Consistent with existing evidence on the relationship between subjective

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<sup>3</sup>NLRA Section 7 and 8(a)1.

<sup>4</sup>An exception of the right to strike in the private sector are employees in the railroad and airline industries, which are excluded from the right to strike by the Railway Labor Act.



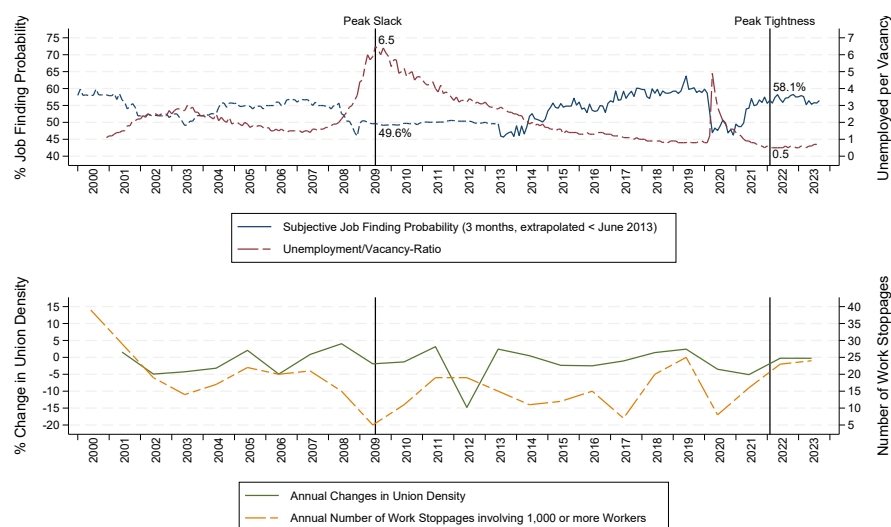


Figure 3.1: Labor Market Tightness, Subjective Job Finding Beliefs, and Union Activities

*Note:* This figure shows the development of labor market tightness in terms of the unemployment/vacancy-ratio and the subjective job finding probability of finding a new job within three months, over time. The number of work stoppages contains all work stoppages involving 1,000 workers or more beginning years. Work stoppages for 2023 are preliminary and only available until October 2023. The change in union density is the annual percentage change of union members in the private sector constructed based on the Current Population Survey. The black lines indicate the maximum and minimum of the unemployment/vacancy-ratio over the sample period. Prior to June 2013, the “mean probability of finding a job in the next three months if job lost today” is extrapolated with the monthly unemployment rate. Sources: U.S. Bureau of Labor Statistics, The Job Openings and Labor Turnover Survey, Annual work stoppages involving 1,000 or more workers, 1947 - Present; New York Fed, Survey of Consumer Expectations; Current Population Survey.

expectations and the macroeconomic environment (see, e.g., Davis and von Wachter, 2011; He and Kircher, 2023; Roth and Wohlfart, 2020), we find a visibly procyclical movement of the subjective job finding rate.<sup>5</sup> We find a relatively flat pattern for the change in union density, with a temporary drop in 2012 not corresponding to a similar change in labor market tightness. For the measure of strikes, the number of large-scale work stoppages, we find more evidence for procyclicality with notable drops in 2009 and 2020 and a rebound in the high-tightness, post-COVID time period. In sum, we find limited indications for a relationship between changes in unionization and labor market tightness in the time series, and stronger evidence for a positive time series relationship between strikes and tightness.

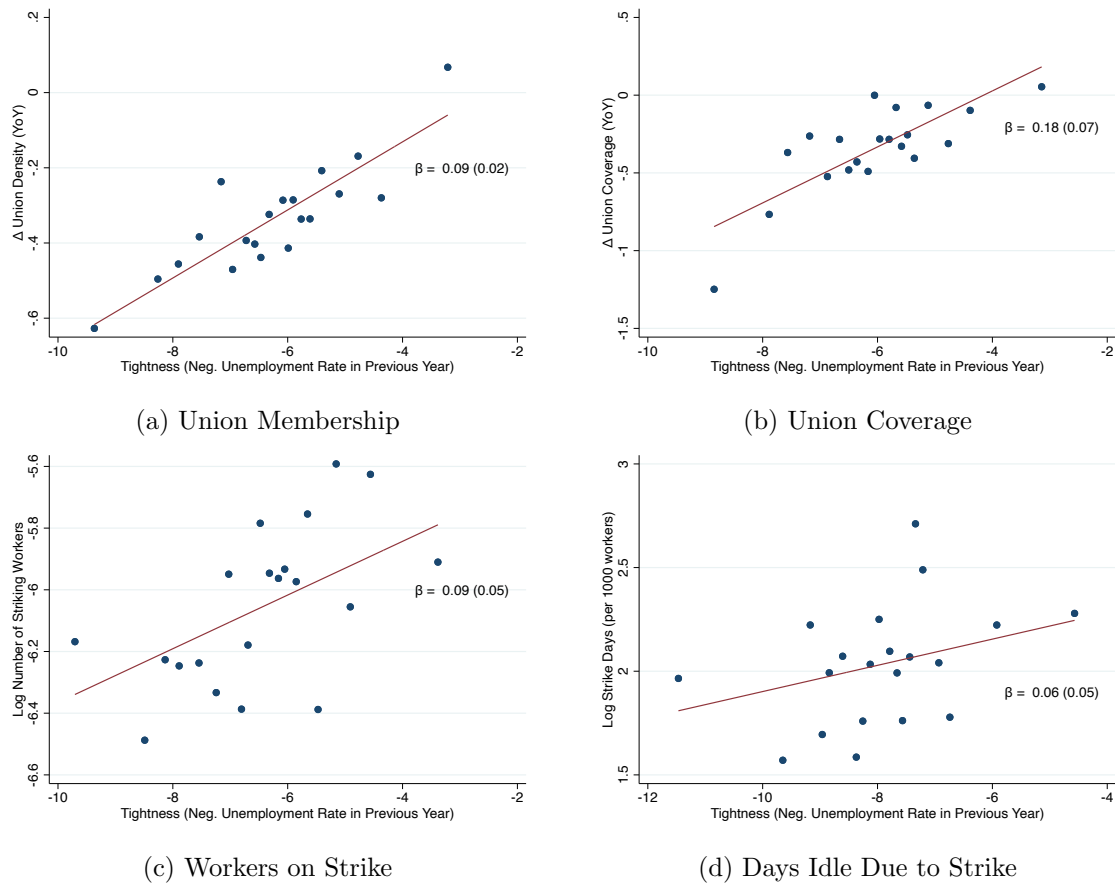


Figure 3.2: Union Activity and Labor Market Tightness in OECD Countries 1960 - 2022

*Note:* These figures visualize the relationship between different measures of union activity and labor market tightness across OECD countries between 1960 and 2022. The outcome variable in Panel (a) measures the change in the union membership rate, defined as the proportion of employees who are members of a labor union among all employees. In Panel (b) the outcome is defined as changes in the number of employees covered by collective agreements in force as a proportion of all employees. The outcome variable in Panel (c) is the change in the number of workers involved in strikes and lockouts (in thousands). Finally, in Panel (d) we rely on data on changes in the number of days not worked due to strikes and lockouts per 1,000 workers. It is measured in terms of the sum of the actual working days during which work would normally have been carried out by each worker involved had there been no stoppage. Data on unemployment, union membership and coverage was retrieved from the OECD/AIAS ICTWSS database and data on strikes was obtained from the International Labour Organization's webpage.

As an additional macro perspective on the relationship between unionization and tightness,

<sup>5</sup>We note that we extrapolated the job finding time series before June 2013 so that, in our visualization, the time period before June 2013 is not informative to assess cyclicalities of this variable.

we provide binned scatter plots of unionization measures across the OECD countries in the time period from 1960 to 2022 against a measure of tightness (see Figure 3.2). Each of the four panels plots the year-on-year change in a unionization measure on the y-axis against tightness measured as the (negative) unemployment rate. A one percentage point decrease in the unemployment rate is associated with a 0.09 percentage point increase in union density (Panel (a)) and an 0.35 percentage point increase in collective bargaining coverage (Panel (b)). Notably, for both of these outcome variables, almost all (binned) observations of changes in unionization are negative, reflecting the average decline in unionization and collective bargaining coverage across OECD countries in the time window we consider. Therefore, the positive coefficients do not necessarily denote an increase in unionization or collective bargaining coverage, but rather indicate that tighter labor markets were associated with a slowed decrease in these measures in the panel of OECD countries we study. For strikes, we find a positive but statistically insignificant relationship between changes in days not worked (per 1,000 workers) and between changes in the number of striking workers (per 1,000 workers) and labor market conditions.

In sum, we find limited evidence of a time series relationship between unionization and labor market tightness in the U.S., though and a moderately more robust relationship between unionization as well as collective bargaining coverage and labor market tightness in a panel of OECD countries.

## 3.4 Micro-Level Evidence From a Survey Experiment

This section starts by describing the data collection process and our sample characteristics. We then introduce the experimental design for recovering micro-level effects of workers' beliefs about labor market tightness on our survey measures of union support. Next, we present descriptive results of workers' beliefs about labor market tightness and conclude with results of this object's causal effect on union support.

### 3.4.1 Data Collection and Sample

**Data Collection.** We collected the survey data in the U.S. between July and September 2023 through the commercial survey company Sago that maintains its own panel of respondents. We coded the survey in Qualtrics and provided the company with a link that they then distributed among their panelists, targeting the specific population we are interested in: private-sector workers between 18 and 65, who are neither self-employed nor hold a chief executive position at their firm. The company remunerates respondents who complete the survey and pass internal quality checks.

**Sample Composition.** Table 3.A.3 shows that our survey sample is representative of U.S. workers employed in the private sector. More precisely, our data is representative of workers along the dimensions age, gender, race, census division and income. All these dimensions were explicitly targeted in the data collection process by setting quotas based

on information from the U.S. Census Bureau’s Current Population Survey (CPS) and the American Community Survey (ACS).

**Survey Quality.** We take several steps to ensure high quality of the collected survey data. Our final sample is comprised of respondents who passed at least one of two attention checks and spent at least 50% of the median response time to complete the survey. In our final sample 73% of respondents passed the first, and 91% of participants passed the second attention check. The median worker took 18 minutes to complete the survey. As presented in Figure 3.A.4, respondents in the treatment group generally perceived the information they were provided with as trustworthy (about 70% of workers) and only a small minority seems to doubt the reliability of the personalized information of labor market tightness they were presented with (about 1% of treatment group respondents). A majority of about 72% participants perceived the survey as non-partisan, and only 7.4% report that they felt the survey was strongly politically biased, but they do not agree on whether it is more right-or left-wing biased (see Figure 3.A.5).

### 3.4.2 Experimental Design

We include the full questionnaire in Appendix Section 3.A.10. The study was pre-registered in the AsPredicted registry<sup>6</sup> and ethics approval was granted by the Christian-Albrechts-University Kiel.<sup>7</sup> Figure 3.1 presents a high level overview of the survey instrument, of which the key elements are described below.

**Pre-Treatment Block.** After eliciting basic respondent characteristics that are needed primarily to target working population quotas, we move on to elicit respondents’ expectations about the U.S. economy and their labor market region, respectively. Next, we ask respondents about an estimate of their own probability of finding a new job within three months after being laid off, and that of people similar to them in terms of occupation, industry and labor market region they work in, as well as considering their age and highest education level that they achieved. As an incentive, the 100 respondents whose estimate is closest to the actual value receive a bonus payment of \$2.<sup>8</sup>

**Information Treatment.** Next, respondents are randomly allocated to either the treatment or pure control group. Those in the treatment group are provided with a visualization similar to the one depicted in Figure 3.A.15 that provides them personalized information about labor market tightness in their narrow labor market cell. In particular, they are shown a screen where two bar charts are displayed—one that represents the actual percent chance of workers similar to them to find a new job within three months and one that shows their own estimate of this object. The former constitutes the information treatment and is based on the U.S. Current Population Survey. This monthly survey is carried out

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<sup>6</sup>The pre-analysis plan is available here: [https://aspredicted.org/XWD\\_9NB](https://aspredicted.org/XWD_9NB)

<sup>7</sup>The IRB approval was received from the Central Ethics Committee of Kiel University (ZEK-17/23, 20.06.2023).

<sup>8</sup>The incentivisation is motivated by concerns of partisan biases observed in previous survey experiments Settele (2022). While recent research on the effect of incentives on belief elicitation indicates that incentives encourage survey participants’ usage of search-engines Grewenig et al. (2022), our individualized information treatments based on recent CPS data decreases the value of “searching for facts” online.

by the U.S. Census Bureau and includes information on a person’s employment status, unemployment duration and several demographics. To construct the personalized estimate for the information treatment, we focus on workers who can be observed in at least two consecutive months and who were unemployed in the first of the two with an unemployment duration of eight weeks or less.<sup>9</sup> We then split the data set into training and test data and run logistic lasso regressions on the training data set with binary employment status in the second month as outcome. Finally, we let lasso select the most important predictors, and predict respondent’s probability of re-employment within three months based on the best out-of-sample performing model that we then program into our own survey in Qualtrics and feed with respondent’s characteristics in real time to provide a personalized information treatment about individual’s job finding probability.

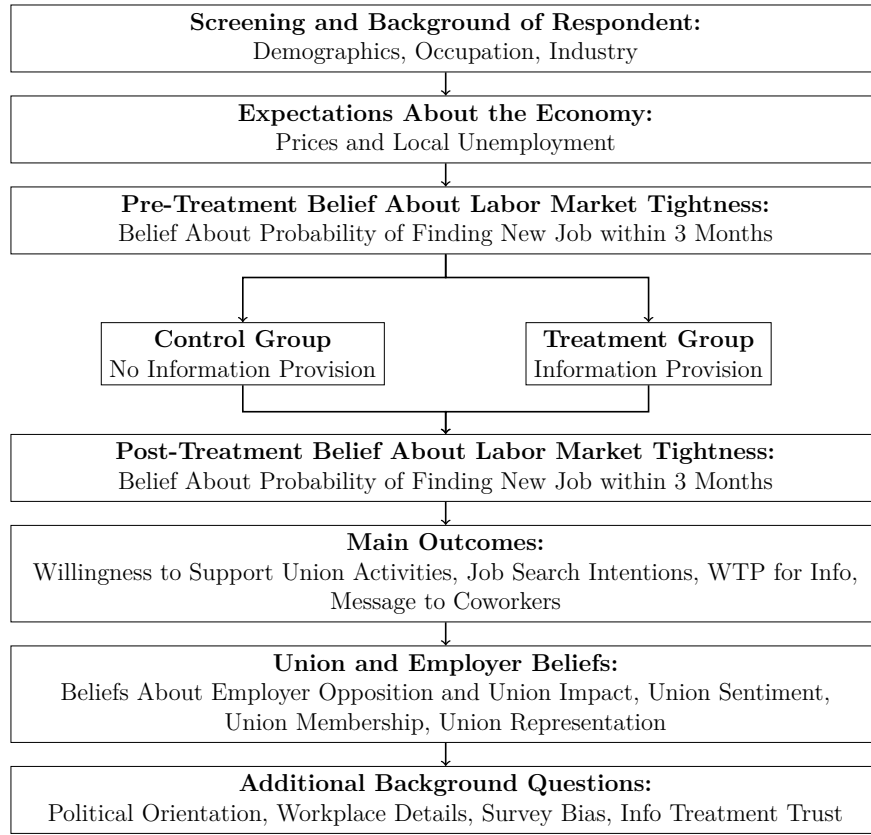


Figure 3.1: Experimental Design

*Note:* This figure displays a high-level overview of the experimental survey design. The full questionnaire can be reviewed in Appendix 3.A.10.

**Post-Treatment Block.** After the treatment, we again measure beliefs about labor

<sup>9</sup>Note that this is a conservative approach and based on the assumption that interviews are carried out roughly at the same point each month. To illustrate this, consider the following example: If a person is in employment in the second of the two consecutive months, but was unemployed the month before with a reported unemployment duration below or equal to 8, clearly the unemployed person made it back to employment within 3 months. In principle, a person could also report eleven weeks as the unemployment duration in the first month and then find a job immediately after, meaning that she made it back to employment within three months. However, we do not observe whether the person who reported eleven weeks of unemployment made it back to employment within 12 or more weeks, leading us to not consider these cases when building our prediction model.

market tightness, i.e., the probability of finding a new job within three months after layoff, to investigate whether respondent's in the treatment group corrected their beliefs based on the information they were provided with. Next, we ask about a series of intended union support activities, job search intentions, beliefs about employer opposition to union activity as well as questions that elicit their willingness to pay for information about unionization and employer retaliation and offer respondents the opportunity to draft a message to their coworkers related to unionization. We close the survey with an additional block on workers' union sentiment, as well as beliefs and knowledge about unions.

**Expert Survey.** To get an independent benchmark for our results, we conducted a survey among 24 experts in labor economics in December 2023. We report more details and results in Appendix 3.A.3.

**Cross-sectional Evidence.** To provide a better understanding of the outcome variables we study and to provide OLS benchmarks for effect sizes in our experiment, we report cross-sectional evidence on the relationship between the unionization and other outcome variables with labor market tightness in Figure 3.2. In this figure, we restrict the sample to workers in the control group whose beliefs about their job finding probability—our measure of subjective labor market tightness—are not affected by the treatment. We find robust, positive relationships between unionization measures and unionization intentions in panels (a) through (e). Workers beliefs about management opposition are uncorrelated with labor market tightness (Panel (f)). In panels (g) and (h), we document that job search intentions are strongly positively and layoff concerns strongly negatively correlated with subjective job finding probabilities.

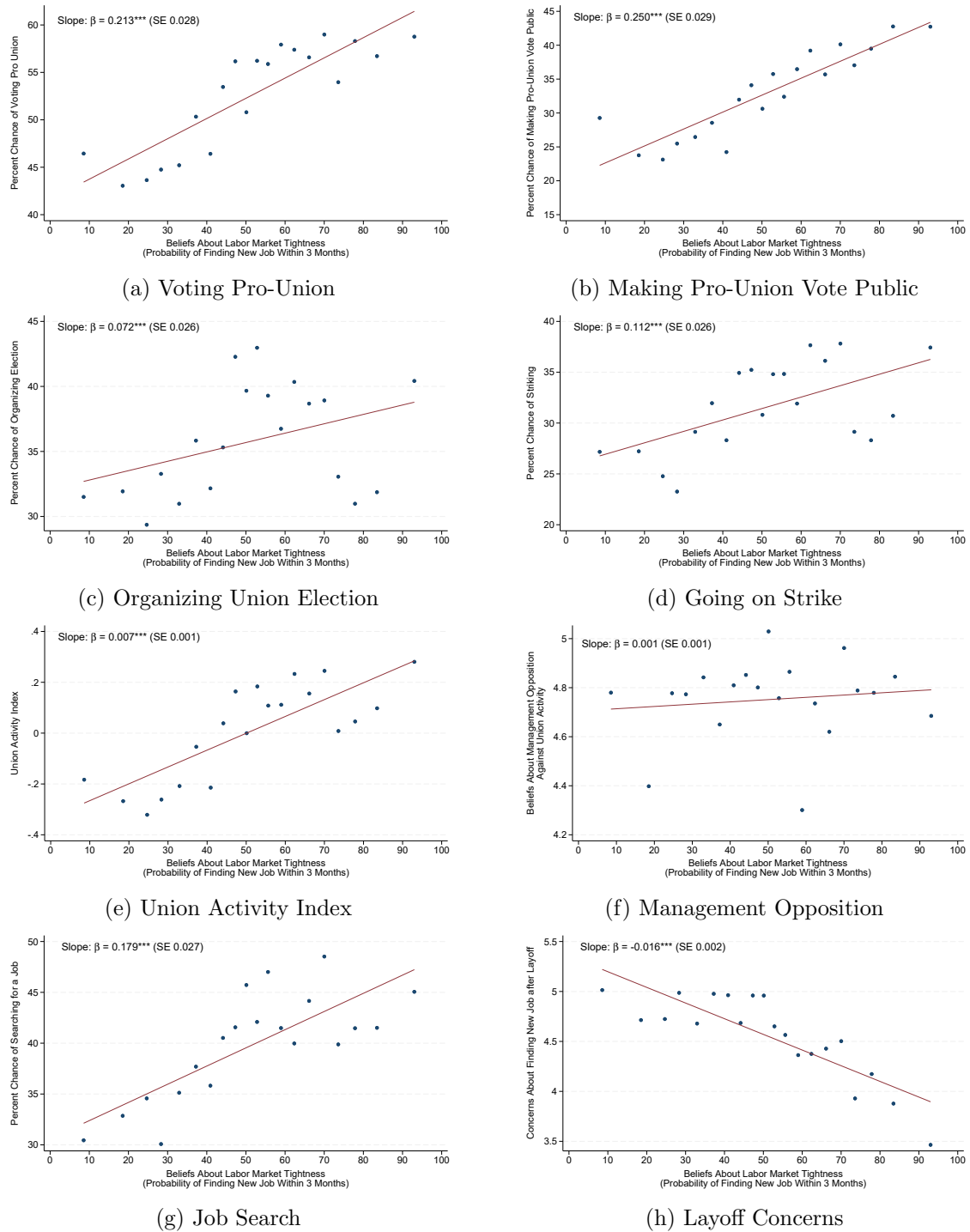


Figure 3.2: OLS Survey Evidence on Union Support, Job Search, Management Opposition and Workers' Beliefs about Labor Market Tightness

*Note:* This figure presents binned scatter plots based on control group respondents' beliefs about labor market tightness on the horizontal and respondents' self-reported support of union activity, job search intentions and beliefs about management opposition on the vertical axis. Beliefs about labor market tightness are measured with the survey question "Suppose you were to lose your job this month. What do you think is the percent chance that within the coming 3 months, you will find a new job that you will accept?". OLS regressions include gender, age group, income group, race, union membership, education, census division, industry and political orientation as controls. We report robust standard errors in parentheses.

### 3.4.3 Identification Strategy

To study the effect of worker beliefs about labor market tightness on their willingness to engage in union activity, we first estimate the following OLS specification, exploiting heterogeneity in the pre-treatment estimation error:

$$Y_i = \beta_0 + \beta_1 T_i \times \underbrace{(\text{Belief}_i^{\text{pre}} - \text{Benchmark}_i)}_{\text{Misperception}_i^{\text{pre}}} + \phi T_i + \theta \text{Misperception}_i^{\text{pre}} + \Theta X_i + u_i, \quad (3.1)$$

where  $Y_i$  represents the outcome variable of interest, for instance, the percent chance of voting “Yes” in a union election or participating in strike activities.  $T_i$  is a binary variable that takes the value one if individual  $i$  is randomly exposed to personalized information about labor market tightness. The variable  $\text{Misperception}_i^{\text{pre}}$  measures the difference between a worker’s pre-treatment belief and the benchmark for the respondent-specific outside options based on our machine learning model (in percentage points).  $X_i$  is a set of control variables, that, by design, are orthogonal to the treatment group.<sup>10</sup>

Next, to facilitate the interpretation of the causal effects, we follow Jäger et al. (2024) and employ an instrumental variable (IV) framework. This allows us to causally estimate the effect of the information treatment on support for or opposition to union activity through the channel of shifting workers’ beliefs about labor market tightness. More precisely, we estimate the following two equations:

$$\text{Belief}_i^{\text{post}} = \pi_0 + \pi_1 (T_i \times \text{Misperception}_i^{\text{pre}}) + \lambda T_i + \zeta \text{Misperception}_i^{\text{pre}} + \Theta' X_i + \epsilon_i \quad (3.2)$$

$$Y_i = \gamma_0 + \gamma_1 \widehat{\text{Belief}_i^{\text{post}}} + \rho \text{Misperception}_i^{\text{pre}} + \Gamma X_i + v_i \quad (3.3)$$

where Equation 3.2 denotes the first stage of the 2SLS framework, and Equation 3.3 the second stage. In the first stage, we instrument post-treatment beliefs about labor market tightness with the random treatment indicator,  $T_i$ , and its interaction with the pre-treatment misperception ( $T_i \times \text{Misperception}_i^{\text{pre}}$ ). In the second stage, we then estimate the causal effect of beliefs about labor market tightness on respondent’s willingness to engage in various forms of union activity. The vector of controls includes the same pre-specified variables as outlined in the description of Equation 3.1.

On average, respondents in our sample overestimate the job finding rate of workers similar to them by 1.2 percentage points (SE 0.4). We provide an overview of the distribution of the misperception in Appendix Figure 3.A.8, which shows substantial misperceptions (SD 28.12). We also document systematic heterogeneity in misperceptions across demographic

<sup>10</sup>The vector  $X_i$  includes (pre-specified) controls for gender, age categories, income group, the race group respondents identify with, education group, political orientation, census state division and industry they work in. The mere purpose of including these covariates is to increase our effective power to detect the treatment effect of interest,  $\gamma_1$ .



groups in Appendix Figure 3.A.9.

### 3.4.4 Intervention Check: Effects on Worker Beliefs About Labor Market Tightness

Figure 3.3 visualizes the effect of our information treatment on worker beliefs about labor market tightness. Establishing that workers in the treatment group indeed used the provided information to update their beliefs about this object is important for our subsequent analysis, where we instrument for worker's post-treatment beliefs about labor market tightness with a treatment group indicator and its interaction with pre-treatment misperceptions. Intuitively, workers in the treatment group who overestimated their probability of finding a new job within three months, i.e., had positive misperceptions, are expected to correct their beliefs about this object downwards, while those with negative misperceptions are expected to shift their beliefs upward in response to the objective personalized information about labor market tightness.

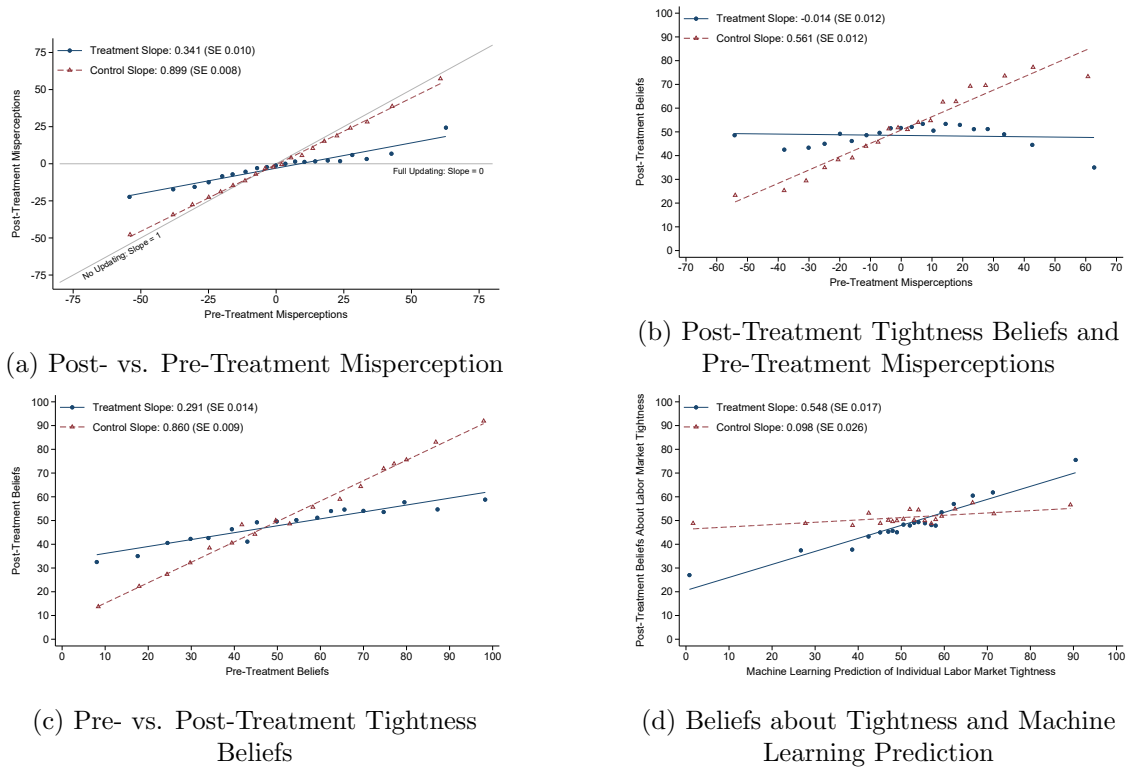


Figure 3.3: Intervention Check and De-Anchoring in the Survey Experiment

*Note:* This figure presents binned scatter plots based on our information experiment, where respondents that were randomly allocated to the treatment group received personalized information about their probability of finding a new job within three months (see Section 3.4.2 for more details on the construction of the treatment information on labor market tightness). Panel a) plots respondents' post-treatment misperceptions about labor market tightness against the pre-treatment misperceptions, separately for the control and treatment group. Misperceptions are defined as the difference between respondents beliefs about their probability of finding a new job within three months and the objective measure of this object that we constructed based on the U.S. Current Population Survey. Panel b) visualizes our first stage and plots respondent's post-treatment beliefs against the pre-treatment misperception. Panel c) plots post-treatment beliefs against pre-treatment beliefs about tightness, and, finally, Panel d) presents binned scatter plots of post-treatment beliefs against the objective measure of respondent's probability of finding a new job within three months.

In Figure 3.3 Panel (a), we plot respondents’ post-treatment misperceptions against their pre-treatment misperceptions about labor market tightness, separately for the control and treatment group. Misperceptions are defined as the difference between a given worker’s belief about tightness in their narrow labor market cell and the objective personalized benchmark that we calculated based on CPS data. Workers in the control group, who were not exposed to the information, have similar misperceptions pre- and post-treatment, yielding a linear slope of 0.899 (SE 0.008), close to perfect persistence. In contrast, participants in the treatment group substantially correct their beliefs based on the information we provided, leading to misperceptions closer to zero and a slope of 0.341 (SE 0.010), substantially smaller than the persistence benchmark in the control group.

Moving closer towards our main analysis, Figure 3.3 Panel (b) plots workers’ post-treatment belief about labor market tightness against their pre-treatment misperception, separately for the treatment and control group—visualizing a non-residualized version of the first stage in our IV regression specification. For the control group, we would expect a positive relationship between pre-treatment misperceptions and post-treatment beliefs, as strong positive misperceptions pre-treatment imply a large positive difference between beliefs and the objective measure of tightness, and this overly optimistic belief is not corrected for respondents in the control group. Indeed, we report a slope of 0.561 (SE 0.012) in the control group, but this relationship vanishes in the treatment group (-0.014; SE 0.012). This is intuitive, as workers in the treatment group who overestimated tightness in their narrow labor market cell, i.e. had positive misperceptions, correct their beliefs downwards in response to the treatment, and, similarly, workers with a negative pre-treatment misperception shift their beliefs upwards in response to the treatment information. In Panel (c), we further document that pre-treatment beliefs about our measure of tightness, the three-month job finding probability, are highly persistent in the control group (coefficient of post- on pre-treatment beliefs of 0.86, SE 0.009) while the treatment group substantially changes beliefs as a consequence of treatment so that beliefs (indicated by a lower persistence coefficient of 0.291, SE 0.014). In Panel (d), we further regress the post-treatment belief on our machine-learning prediction of the job finding rate, separately for the control and treatment group. In the control group (which does not have access to the prediction), we find a positive but small slope of 0.098 (SE 0.026), indicating that our machine learning prediction captures some information that respondents already have access to. In the treatment group, we find a substantially stronger relationship with a slope coefficient of 0.548 (SE 0.017).

### 3.4.5 Main Results: Effects on Union Activity, Job Search, and Beliefs About Employer Retaliation

This section presents our estimates for the causal effects of worker beliefs about labor market tightness on several intended labor market behaviors capturing support of or opposition to labor unions: voting pro-union if an election were held at their establishment tomorrow, making a pro-union vote public, helping to organize an election drive and par-

participating in a strike. We also report effects of changes in beliefs about local labor market conditions on workers’ willingness to pay for information related to unionization and effects on their pro- or anti-union sentiment expressed in messages to their co-workers, that we analyze using large language models (LLMs).

**Intervention Check.** As outlined in Section 3.4.3 we employ an IV estimation strategy to study the causal effect of the information treatment on union activity through the channel of shifting workers’ beliefs about local labor market tightness. Table 3.1 Column (1) presents residualized estimates corresponding to the intervention check visualized in Figure 3.3a. As a reminder from the discussion around this figure, our negative coefficient (-0.5581; SE 0.0172) implies that workers who initially underestimated the probability of finding a new job within three months update their beliefs about this object upwards, and vice versa. In particular, we find that treated workers who underestimated the probability of finding a new job within three months by 10ppt reduce their post-treatment estimation error by 5.58ppt.

**First Stage.** We present our first stage estimates in Column (2) of Table 3.1, where our main coefficient of interest, corresponding to  $\pi_1$  in Equation 3.2, captures the effect of the information treatment on post-treatment beliefs about labor market tightness as a function of respondents pre-treatment misperceptions. We estimate a coefficient of -0.5758 (SE 0.0216), which implies that workers who initially underestimated the probability of finding a new job by 10ppt raise their belief about this object by 5.76ppt.

**Causal Effects on Union Activity.** Table 3.1 Columns (3) through (7) present causal effects on our different measures of union activity. Panel A reports reduced form estimates, while Panel B reports estimates based on the second stage (see Equation 3.3). In what follows, we focus on the interpretation of the estimates in Panel B, as they identify the effects on union support due to exogenous shifts in workers beliefs about labor market tightness.

In order to provide a meaningful interpretation of our effect sizes, we report implied effects of a change in the subjective job finding probability when moving from trough to peak of the business cycle (see Figure 3.1) amounting to about 10ppt.<sup>11</sup>

Column (3) presents an IV coefficient of 0.1006 (SE 0.0508), implying that moving from trough to peak of the business cycle would cause a 1.01ppt increase in a typical worker’s willingness to vote “yes” in a union election, and a 1.91% increase relative to the outcome in the control group). Column (4) shows estimates for a closely related outcome, namely the self-reported probability of making a pro-union vote public, which can be costly when employers oppose unionization in a slack labor market, where finding a new job after being

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<sup>11</sup>We obtain this estimate from the Federal Reserve Bank of New York’s Survey of Consumer Expectations (Armantier et al., 2017). We compare the subjective job finding probability at the maximum of the post-pandemic tightness (0.5 unemployed per vacancy) in 2022 to the probability measured at the peak of the Great Recession (6.5 unemployed per vacancy). We extrapolated subjective job finding probabilities before June 2013 based on a regression of the probability on the monthly unemployed rate after June 2013. We find that the mean probability of finding a job in the next three months if the respondent’s job were lost on the day of the survey varies between 49.6% and 58.2%, leading to a difference of 8.6ppt in the job finding rate. If we instead take April 2020 as the minimum of tightness, we arrive at a similar conclusion (4.9 unemployed per vacancy, 14.7% unemployment rate, subjective job finding rate of 46.95%, leading to an 11.3ppt difference between trough and peak).

laid off can be challenging. Here, we report an IV coefficient of 0.1115 (SE 0.0504), which is equivalent to an 1.11ppt increase in a worker’s willingness to make their vote public when moving from labor market conditions corresponding to a recession up to a boom.

Column (5) reports IV estimates for worker’s willingness to organize an election drive of 0.0995 (SE 0.0477), corresponding to a 0.995ppt increase in organizing drives when moving from trough to peak of the business cycle.

For strike activities, Column (6) reports and IV coefficient of 0.0776 (SE 0.0475), implying that a 10ppt increase in worker beliefs about labor market tightness leads to a 0.776ppt increase in strike participation.

Column (7) combines a centered version of our measures of union activity, presented in Columns (3)-(6) into an equally weighted, standardized index. For this index, we report an IV coefficient of 0.0040 (SE 0.0017), suggesting a 0.040SD increase in overall union activity when moving from labor market conditions corresponding to a recession up to a boom.

**Causal Effects on Job Search and Layoff Concerns.** We also investigate effects of an increase in worker beliefs about labor market tightness on their job search intentions. In Column (8), we report an IV coefficient of 0.1864 (SE 0.0497), implying that a 10ppt increase in subjective job finding probability raises workers’ interest in looking for new jobs by 1.86ppt. Relatedly, we confirm that worker’s concerns about finding a new job after an unexpected layoff decreases (-0.0072; SE, 0.0030) in response to an increase in their beliefs about labor market tightness in their narrow labor market cell.

**Causal Effects on Beliefs About Management Opposition and Employer Retaliation.** An increase in worker beliefs about labor market tightness might decrease their beliefs about the probability of being retaliated against in response to supporting union activity, as replacing workers in tight labor markets is more costly to rational employers who adjust their behavior in response to changes in local labor market conditions. Alternatively, changes in worker beliefs about their probability of finding a new job after layoff (our measure of personalized labor market tightness), might not affect their beliefs about employer retaliation for two reasons. First, our treatment is designed to experimentally vary workers’ beliefs about their “personal probability” of finding a new job, and workers might not expect their employer to respond to changes in outside options of individual workers in their workforce. Second, workers might view the extent to which their employer opposes union activity as an inherent trait that cannot be altered by external forces.

We report small negative effects of changes in beliefs about labor market tightness on beliefs about management opposition and employer retaliation. For beliefs about “overall” management opposition, we report an IV estimate of -0.0004 (SE 0.0028), suggesting that moving from trough to peak of the business cycle reduces worker beliefs about management opposition by -0.004ppt, although this relationship is not statistically meaningful. We also report IV estimates on workers’ belief about being fired if their employer found out they were engaging in union activities—a more extreme version of retaliation. Here, we report IV estimates of slightly larger economic magnitude (-0.0641; SE 0.0520), implying that changing beliefs about local labor market conditions by 10ppt, equivalent to moving

Table 3.1: Experimental Evidence on Effects of Beliefs about Labor Market Tightness on Intended Union Activity, Job Search and Beliefs About Management Opposition

	First Stage			Union Activity			Job Search			Mechanism	
	(1) Post Estimation Error	(2) Post Tightness Belief	(3) Pro-Union Vote	(4) Pro-Union Vote Public	(5) Organize Election	(6) Willing to Strike	(7) Union Activity Index	(8) Job Search Intention	(9) Layoff Concerns	(10) Management Opposition	(11) Firing Probability
<b>Panel A: OLS</b>											
Treated x Pre-Treat Estimation Error	-0.5581*** (0.0172)	-0.5758*** (0.0216)	-0.0559* (0.0298)	-0.0617*** (0.0295)	-0.0571** (0.0278)	-0.0493* (0.0275)	-0.0023** (0.0010)	-0.1082*** (0.0290)	0.0043** (0.0018)	0.0003 (0.0017)	0.0389 (0.0299)
Treated	-2.4315*** (0.3527)	-2.1200*** (0.4448)	-0.5408 (0.7666)	-0.6515 (0.7852)	-0.2330 (0.7387)	0.5973 (0.7484)	-0.0080 (0.0257)	-0.2496 (0.7801)	-0.0142 (0.0475)	-0.0210 (0.0442)	-0.1915 (0.7758)
Pre-Treat Estimation Error	0.8919*** (0.0089)	0.5857*** (0.0138)	0.1357*** (0.0223)	0.1593*** (0.0224)	0.0637*** (0.0207)	0.0678*** (0.0205)	0.0044*** (0.0007)	0.1526*** (0.0216)	-0.0115*** (0.0013)	-0.0018 (0.0012)	0.0331 (0.0215)
Constant	-4.9190* (2.0239)	38.7532*** (3.0477)	45.2321*** (4.2135)	23.0016*** (4.2341)	43.3904*** (4.4115)	33.8952*** (4.3472)	-0.0606 (0.1388)	42.6088*** (4.0312)	5.0882*** (0.2004)	4.4932*** (0.2672)	48.7400*** (4.3714)
<b>Panel B: IV</b>											
Labor Market Tightness Belief			0.1009** (0.0508)	0.1115** (0.0504)	0.0905** (0.0477)	0.0776 (0.0475)	0.0040** (0.0017)	0.1864*** (0.0497)	-0.0072** (0.0030)	-0.0004 (0.0028)	-0.0641 (0.0520)
Pre-Treat Estimation Error			0.0778*** (0.0208)	0.0953*** (0.0204)	0.0635 (0.0194)	0.0201 (0.0195)	0.0020*** (0.0007)	0.0430** (0.0201)	-0.0073*** (0.0013)	-0.0015 (0.0012)	0.0716*** (0.0225)
Constant			41.1943*** (4.5789)	18.5024*** (4.5838)	39.5269*** (4.7313)	31.2143*** (4.6508)	-0.2160 (0.1506)	35.4487*** (4.4309)	5.3527*** (0.3048)	4.4980*** (0.2842)	51.0845*** (4.8184)
Observations	5510		5510	5510	5510	5510	5510	5510	5510	5510	5510
Average of Outcome Control Group	0.29	51.32	52.55	52.96	35.78	31.38	0.01	39.77	4.35	4.75	36.39

**Note:** This table reports results based on the information experiment. Panel A reports OLS coefficients, where “Treated” is a binary variable indicating whether the respondent was part of the treatment group and “Pre-Treat Estimation Error” denotes the difference between the respondent’s estimate of the probability that someone like them, but who lost their job, would find a new job within 3 months and the probability predicted by our machine learning model. Panel B presents IV specifications, using respondent’s beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. See Section 2.3, and in particular Equation 2 for more details. The outcome variables in Column (3) - Column (6) are based on the following survey questions. *Pro-Union Vote*: “If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would vote for a union?”; *Pro-Union Vote Public*: “If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would make your vote public?”; *Organize Election*: “What is the percent chance that you would help organize a union election at your establishment in the coming weeks?”; *Willing to Strike*: “What is the percent chance that you would participate in any strike-related activities?”. The outcome variable in Column (7) combines the centered measures of union activity in a standardized index measure. The outcome variables in Column (8) - (11) are based on the following survey questions. *Job Search Intention*: “Over the next 12 months, what is the percent chance that you will look for a new job at a different company?”; *Layoff Concerns*: “Suppose you were to lose your job this month. How worried would you be about finding a new one?”; *Management Opposition*: “How strongly do you think your management would oppose any type of union activity by you and your colleagues?”; *Firing Probability*: “What do you think is the percent chance that your management would fire you if they found out you support any union activities?”. We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.

from recession to boom, changes workers' beliefs about being fired for engaging in union activities by 0.64 percentage points, but again, we cannot reject that workers' beliefs about labor market tightness do not affect their beliefs about the probability of being retaliated against.

**Heterogeneity in Effects on Union Activity.** Next, we study heterogeneity in the effects of beliefs about local labor market conditions on union activity and present results on our index measure in Figure 3.4. Two connected patterns emerge, that point towards labor market tightness playing a larger role when workers are concerned about management opposition and when the share of union support among co-workers is larger.

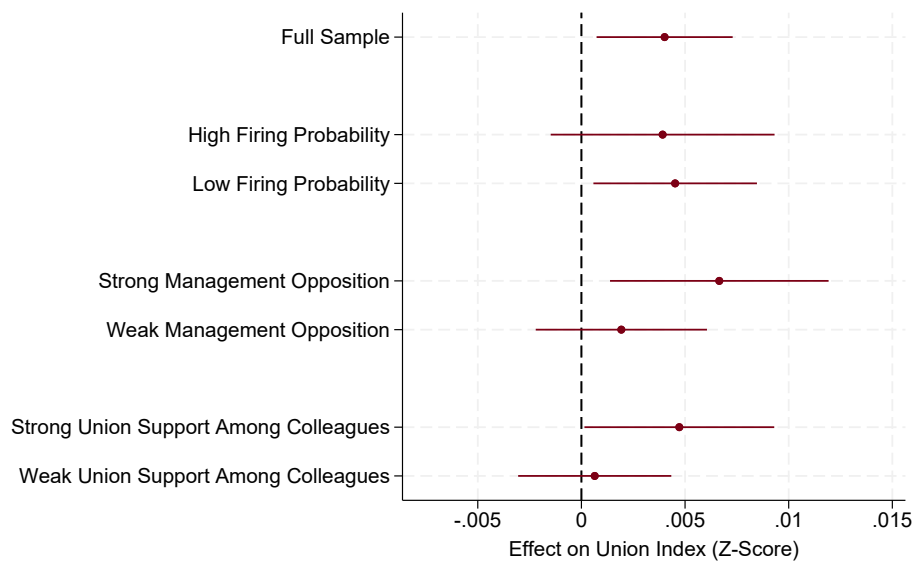


Figure 3.4: IV Estimates of Effects of Beliefs About Labor Market Tightness on Union Activity from a Survey Experiment

*Note:* This figure presents IV estimates based on Equation 3.3 with the union activity index that combines our centered measures of union activity as the outcome variable. Groups are based on a median split of the full sample. “Firing probability” measures respondents beliefs (in percent) about the probability of being fired due to engagement in union activity. “Employer opposition” features a broader measure of workers’ beliefs about how strongly their management would oppose union activity. “Vote share colleagues” measures workers beliefs about the share of their co-workers that would vote “yes” if a union election was held at their establishment today. Regressions include gender, age group, income group, race, union membership, education, census division, industry and political orientation as controls.

First, we document that the effect of worker beliefs about labor market tightness on union activity is more than twice as large in an environment with strong management opposition to unions. This suggests that the more likely their management is to strongly oppose collective action, the more outside options (measured by the ease with which a worker can transition into a new job) matter for their decision to engage in union activity. An important caveat is that this holds true for our measure of “overall” management opposition, that captures concerns about discrimination and career obstacles in response to union activity, but the effect sizes do not meaningfully differ between an environment where managers are perceived to be relatively more likely to layoff workers in response to engaging in union activity.

We also study effect heterogeneity by the share of union support among co-workers. Our

findings are consistent with a strong strategic complementarity between workers' actions, in line with the idea that workers are more likely to engage in collective action when a larger fraction of their colleagues are willing to do the same (Naidu, 2022). In light of concerns about employer retaliation to union activity, being assured the support of a large share of colleagues might also decrease the perceived cost of engaging in collective action, as in the extreme, it is harder for employers to retaliate against the whole workforce compared to selected individuals who voiced their union support.

**Causal Effects on Willingness To Pay For Information.** To create a revealed preference measure of demand for information about unionization, we included an incentive-compatible mechanism to measure workers' willingness to pay for information about unionization, about employer retaliation, and about job search strategies. We offered participants the possibility to bid for three different, carefully crafted information brochures (i) about how to unionize their workplace, (ii) about workers' legal rights in the case of employer opposition to unionization, and (iii) about job search strategies. We measured their willingness to pay for these brochures by forfeiting a part of a potential lottery win.<sup>12</sup> We measured willingness to pay for each of the brochures in an incentive-compatible way, following the idea of the Becker-DeGroot-Marschak mechanism.

We report results in Table 3.2 where we document no correlation between beliefs about labor market tightness and the willingness to pay for information about unionization, employer retaliation, or job search strategies. We consider both a binary variable of a non-zero willingness to pay for information as well as continuous measures of the willingness to pay. We can rule out even small increases in the willingness to pay for information. At the same time, we can rule out that the information we provide is irrelevant: for each brochure, around two thirds of participants have a strictly positive willingness to pay for information, and the average willingness to pay for a brochure is higher than \$4.

**Causal Effects on Writing Union-Related Message To Co-Workers.** As a real effort task, we invited workers to draft a message to coworkers for or against unionizing their workplace. We also prompted workers to share the message they wrote by copying it from the text field we provided for drafting. We report results of tightness beliefs on writing a union-related message in Table 3.3, where we consider both the extensive margin of writing any message (34% of respondents) as well as characteristics of the message and the drafting process. We also run our analysis in the full sample (setting them to zero for workers who do not draft a message) as well as restricting the sample to those who drafted a message. We find no effect of tightness beliefs on the probability of writing a message, on whether the sentiment of the message was positive, on the word count of the message, as well as on the time workers spent drafting a message. We even find a (very small) negative effect on the probability to copy the message. We conclude that tightness beliefs do not meaningfully affect workers' union-related activities (even though a significant share of participants write a message and those who do spend about two and a half minutes of their time on the task).

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<sup>12</sup>We enrolled participants who finished our survey in a lottery with a \$500 prize.

Table 3.2: Full Sample: Willingness To Pay for Information on Unionization, Retaliation and Job Search

	(1)	(2)	(3)
	WTP for Unionization Info	WTP for Retaliation Info	WTP for Exit Info
<b>Panel A: Binary Outcome</b>			
IV: Labor Market Tightness Belief	-0.0002 (0.0007)	-0.0008 (0.0007)	-0.0013* (0.0007)
Pre-Treat Estimation Error	-0.0021*** (0.0003)	-0.0018*** (0.0003)	-0.0014*** (0.0003)
Constant	0.9734*** (0.0674)	0.9783*** (0.0710)	0.9654*** (0.0694)
Average of Outcome Control Group	0.66	0.66	0.69
<b>Panel B: Continuous Outcome</b>			
IV: Labor Market Tightness Belief	0.0045 (0.0060)	-0.0060 (0.0058)	-0.0063 (0.0060)
Pre-Treat Estimation Error	-0.0155*** (0.0025)	-0.0116*** (0.0025)	-0.0096*** (0.0026)
Constant	6.5887*** (0.6293)	7.2805*** (0.6381)	7.4285*** (0.6296)
Average of Outcome Control Group	4.24	4.29	4.59
Correlation With Corresp. Main Outcome	0.27	0.25	0.10
Observations	5520	5520	5520

*Note:* This table shows effects of changes in worker beliefs about labor market tightness on their willingness to pay for information brochures (i) about how to unionize their workplace, (ii) about workers' legal rights in the case of employer opposition to unionization, and (iii) about job search strategies. We consider both a binary variable of a non-zero willingness to pay for information as well as continuous measures of the willingness to pay. *IV: Labor Market Tightness Beliefs* represents the post-treatment belief about outside options, which we instrument with the treatment indicator and its parametric interaction with the estimation error. *Pre-Treat Estimation Error* denotes the difference between the respondent's estimate of the probability that someone like them, but who lost their job, who would find a new job within 3 months and the probability predicted by our machine learning model. The outcome variables in Column (1) - Column (3) are based on standard, incentivized willingness to pay for information questions—we kindly refer the interested reader to the full questionnaire in Appendix Section 3.A.10 and a discussion of results in Section 3.4.5. Regressions include controls for gender, age group, income group, race, union membership education, state, industry, and political orientation. We report robust standard errors in parentheses. To check whether the information we provide is deemed to be relevant by those who are interested in unionization, we print the correlation between our continuous WTP measure and the corresponding main survey outcome (standardized union index for Column (1) and (2) and the standardized job search intentions in Column (3)) at the bottom of the table.



Table 3.3: Effort and Sentiment in Message about Unionization to Coworkers

	(1) Wrote Message	(2) Pos Sentiment	(3) Word Count	(4) Copied	(5) Time Spent
<b>Panel A: Full Sample</b>					
IV: Labor Market Tightness Belief	0.0008 (0.0007)	0.0006 (0.0006)	-0.0442 (0.0795)	-0.0007*** (0.0003)	-0.0347 (0.2289)
Pre-Treat Estimation Error	0.0012*** (0.0003)	0.0002 (0.0002)	0.0599* (0.0309)	0.0002** (0.0001)	0.3103*** (0.1029)
Constant	0.1033 (0.0684)	0.0379 (0.0529)	1.3156 (4.7855)	0.0571** (0.0228)	23.6864 (18.6458)
Observations	5510	5510	5510	5510	5510
Average of Outcome Control Group	0.34	0.15	16.54	0.03	69.16
Correlation With Corresp. Main Outcome	0.02	0.27	0.03	0.05	0.05
<b>Panel B: Conditional on Drafting</b>					
IV: Labor Market Tightness Belief		0.0006 (0.0010)	-0.1502 (0.1435)	-0.0014*** (0.0005)	-0.3952 (0.4021)
Pre-Treat Estimation Error		-0.0006 (0.0005)	-0.0385 (0.0743)	0.0001 (0.0002)	0.4513** (0.2166)
Constant		0.3375* (0.1784)	17.6706 (12.0585)	0.1781* (0.0991)	160.0727*** (61.9076)
Observations		1814	1814	1814	1814
Average of Outcome Control Group		0.43	48.67	0.08	158.95
Correlation With Corresp. Main Outcome		0.48	0.04	0.07	0.06

*Note:* This table shows effects of changes in worker beliefs about labor market tightness on their willingness to draft a message to their coworkers for or against unionization, and conditional on drafting, how much effort they put into this message. Estimates in Panel A are based on the full sample of respondents, while Panel B includes only those who drafted a note (expressing *any* sentiment towards union, positive or negative). We draw on OpenAi’s large language model (LLM) “DaVinci” and ask it to classify messages into pro-vs. anti-union sentiment. The LLM’s classification aligns strongly with the classification of a subset of messages that an independent human coder categorized into positive vs. negative union sentiment (correlation of about 0.8). *Pre-Treat Estimation Error* denotes the difference between the respondent’s estimate of the probability that someone like them, but who lost their job, who would find a new job within 3 months and the probability predicted by our machine learning model. *IV: Labor Market Tightness Beliefs* represents the post-treatment belief about outside options, which we instrument with the treatment indicator and its parametric interaction with the estimation error. Regressions include controls for gender, age group, income group, race, union membership education, state, industry, and political orientation as controls. We report robust standard errors in parentheses. To check whether workers inclined to support unionization are also those who are interested in writing a message and exerting effort doing so, we print the correlation between our different outcomes variables and standardized union index for at the bottom of the table.

**Additional Heterogeneity Analysis.** We further study various heterogeneity in the effects of beliefs about local labor market conditions on union activity. We find smaller variations in treatment effect heterogeneity by socioeconomic characteristics. Treatment effects are stronger among individuals with higher levels of education, younger age groups, African-Americans, and Democrats (Table 3.A.8). We find that respondents’ beliefs about the impact of unions play an important role in the treatment effect on union activity. The treatment effect is driven by respondents who believe that unions have positive effects on wages, working conditions, and reduce the job loss probability (Table 3.A.9). In contrast, we find no heterogeneity based on firm characteristics, such as current working conditions or the relation with management (Table 3.A.10).

### 3.5 Conceptual Framework: Unionization under Strategic Complementarity and Employer Retaliation

In this section, we develop a simple framework that highlights strategic complementarity as a key component of workers' decision to unionize in the presence of management opposition to union activity. In this framework, the probability of being retaliated against decreases with the share of co-workers supporting a union, while the likelihood of organizing success increases in the support among colleagues, as shown in Naidu (2022). Our exposition considers an individual's decision to vote for a union. However, our model isomorphically extends to workers' decisions to organize or to engage in other pro-union activities.

We express the individual worker's benefit of voting for a union as:

$$\underbrace{(\mu - d)}_{\text{Net Premium}} \underbrace{(q(1, V) - q(0, V))}_{\text{Prob Pivotal Vote}} + \epsilon,$$

where we let  $\mu$  denote workers' beliefs about the wage premium, defined as the difference between the wage when their establishment unionizes versus if it remains non-unionized,  $w_u - w_n$ .<sup>13</sup> We let  $d$  represent union dues and  $q(v, V)$  the probability of an organizing drive or of the vote being successful, where  $v \in \{0, 1\}$  is the worker's own vote and  $V$  denotes the share of co-workers believed to be voting pro-union. The parameter  $\epsilon$  captures workers' idiosyncratic preference for the act of voting for the union. Workers face the following costs from pro-union behavior:

$$\underbrace{(\mathbb{E}(w) - J(\theta))}_{\text{Threat of Unemployment}} \underbrace{(f(1, V, \theta) - f(0, V, \theta))}_{\text{Marginal Retaliation Prob}},$$

where  $f(v, V, \theta)$  represents the probability of employer retaliation (e.g., due to targeted dismissals of employees McNicholas et al. (2019), or establishment closures as in Wang and Young (2022)) as a function of workers' own pro-union behavior, the pro-union behavior of her co-workers, and labor market tightness. We assume  $f(1, V, \theta) - f(0, V, \theta)$  to be decreasing in  $V$ , as it is harder for an employer to impose individualized punishments on workers for unionizing if their colleagues are also engaged in unionization efforts. Beyond that, it seems plausible that the probability of being retaliated against decreases with labor market tightness,  $\theta$ , as employers internalize the increasing costs of replacing a given worker (and the vacancy filling probability falls in tightness in most matching functions).  $J(\theta)$  denotes the value of unemployment which increases in tightness—in a tighter labor market, the expected duration of unemployment is lower, making unemployment less costly.

Then a worker votes for the union when the following holds:

$$(1 - f(1, V, \theta))(w_n + (\mu - d)q(1, V)) + \epsilon + f(1, V, \theta)J(\theta) - \\ ((1 - f(0, V, \theta))(w_n + (\mu - d)q(0, V)) + f(0, V, \theta)J(\theta)) > 0.$$

<sup>13</sup>Here, we let wages be independent of tightness as is frequently assumed (see Lalive et al., 2015 for an example, and Jäger et al., 2020 for evidence). Our model can be extended to let wages and the wage premium depend on tightness.

To facilitate interpretation, let  $f_u = f(1, V, \theta) - f(0, V, \theta)$  represent the probability that a worker gets laid off specifically for supporting union activity and  $q_u = q(1, V) - q(0, V)$  the probability of being pivotal. Then we can simplify the above condition to:

$$\underbrace{(\mu - d)}_{\text{Net Premium}} \underbrace{(1 - f(1, V, \theta))}_{\text{Prob of Staying}} q_u - f_u \underbrace{(\mathbb{E}(w|v=0) - J(\theta))}_{\text{Threat of Unemployment}} + \epsilon > 0,$$

with  $\mathbb{E}(w|v=0) = q(0, V)(\mu - d) + w_n$  representing the expected wage if the worker does not vote for a union. In this stylized framework, an increase in labor market tightness makes a worker more likely to unionize, as it reduces the threat of unemployment if the employer retaliates, and the probability of this event occurring. Additionally, the assumption that  $f_u$  is decreasing in  $V$  provides intuition for the role of co-workers' support for unionization for an individual workers' decision to unionize, as the probability of job loss due to supporting union action decreases as the share of co-workers participating in union organizing rises.

### 3.6 Equilibrium Effects: Three Quasi-Experimental Research Designs

To complement our experimental survey evidence on the effect of labor market conditions on union activity, we study whether labor market tightness increases union activity in the cross-section. We draw on three designs in which we leverage quasi-experimental variation in local labor market tightness, and examine the effect of tightness on union membership, union elections, and strikes. To instrument for shocks to labor demand that increase tightness, we use a Bartik instrument (Bartik, 1991) and commuting-zone level exposure to Chinese import competition (Autor et al., 2013). To study labor supply shocks, we instrument for future retirements (Böhm et al., 2021; Böhm and Siegel, 2021) and labor market entrants with the lagged (local) age structure.

#### 3.6.1 Labor Demand Shock I: Canonical Bartik Approach

##### *Construction of Labor Demand Shock*

We construct a state-level Bartik instrument (Bartik, 1991), equipping us with variation in labor demand that is driven by plausibly exogenous national shocks to industries. The Bartik instrument uses variation in national industry employment growth interacted with baseline employment shares in local labor markets. We use 100 three-digit NAICS industries and construct the relevant national employment growth variable for a given state using a leave-one-out approach, as is common in the literature.<sup>14</sup> Letting  $e_{itj}$  denote employment in state  $i$  and industry  $j$  at time  $t$ , we construct the national industry growth

<sup>14</sup>See Borusyak et al. (2022) for a discussion of the leave-one-out design as well as a proof that exogenous national industry shocks are a sufficient condition for identification.

rate leaving out state  $i$ ,  $\kappa_{itj}$ , as:

$$\kappa_{itj} = \sum_{\substack{k \\ k \neq i}} \frac{e_{ktj}}{e_{k,t-1,j}} - 1.$$

Then we construct the state share of employment  $s_{ibj}$  in industry  $j$  in baseline year  $b$ , which we set as 1983, the year before we observe any outcomes:

$$s_{ibj} = \sum_j \frac{e_{ibj}}{\sum_j e_{ibj}}.$$

We interact these to calculate the predicted employment change  $\hat{\Delta}_{it}^e$  of a state  $i$  in year  $t$ :

$$\hat{\Delta}_{it}^e = \sum_j s_{ibj} \kappa_{itj}.$$

We then use the predicted employment change,  $\hat{\Delta}_{it}^e$ , as an instrument for the negative unemployment rate (in percentage points),  $\Delta u_{it}^-$ , to capture a measure of labor market tightness. We estimate the following IV specification with state and year effects:

$$\begin{aligned} \Delta u_{it}^- &= \alpha_i^{\text{FS}} + \beta_t^{\text{FS}} + \gamma^{\text{FS}} \cdot \hat{\Delta}_{it}^e + \epsilon_{it}^{\text{FS}}, \\ \Delta Y_{it} &= \alpha_i^{\text{SS}} + \beta_t^{\text{SS}} + \gamma^{\text{SS}} \cdot \hat{\Delta}_{it}^e + \epsilon_{it}^{\text{SS}}, \end{aligned}$$

where  $\Delta Y_{it}$  denotes changes in the unionization outcomes we consider. We estimate the specification with 2SLS and cluster standard errors at the state level.

We get administrative data on employment at the state level from the Quarterly Census of Employment and Wages (QCEW). The QCEW aggregates employers' reports of wages and employment to state unemployment insurance (UI) agencies and we draw on data from 1983 to 2022. It includes public sector workers, and we include them in our main specification (public sector employment is spatially concentrated and a useful source of regional labor demand shocks), though our results below are robust to excluding them. Our data does not include informal workers, agricultural workers, and the self-employed.

### *Effects of Labor Demand Shocks from Industry Growth on Union Activity*

We report results in Table 3.1 and also visualize effect sizes in Figures 3.A.11 and 3.A.14. Table 3.1 presents results using the canonical Bartik instrument. The first row in Column (1) confirms that the instrument is a meaningful predictor of changes in labor market

tightness, measured as changes in the negative unemployment rate. Columns (2) through (6) present results on changes in our union activity outcomes, and Column (7) presents second stage results for job-to-job transitions. We look at *changes* in outcomes because the canonical Bartik instrument predicts *changes* in local labor demand. If tightness induces more union activity, we would estimate positive effects on union activity outcomes. In the unweighted specification (which we focus on because it has the stronger first stage), IV coefficients on changes in union membership, the number of union elections and the number of workers voting in union elections are indeed positive, in line with the hypothesis that labor market tightness spurs unionization, but only the positive coefficient on union membership is statistically significant. The IV coefficient on changes in the number of workers idle and the number of strikes is negative, but only the effects on changes in the number of workers idle is marginally statistically significant. The coefficients are economically small. For example, a one percentage point decrease in the unemployment rate is associated with a 0.34 increase in the number of union elections per million workers per year, relative to an average of 21.5 elections per million workers per year. Confidence intervals are fairly wide so we cannot rule out an increase of up to 2.6 elections per million workers per year at the upper end of the 95% confidence interval.

### 3.6.2 Labor Demand Shock II: The China Shock Approach

#### *Construction of Labor Demand Shock*

We draw on the Chinese import exposure instrument (Acemoglu et al., 2016; Autor et al., 2013), using long differences between 1991 and 1999 and 1999 and 2011 at the commuting zone level.<sup>15</sup> Acemoglu et al. (2016) (AADHP) construct the change in Chinese import penetration in industry  $j$  over the long difference,  $\tau$ ,  $\Delta IP_{j\tau}$ , by dividing the change in Chinese imports,  $\Delta M_{j\tau}^{UC}$ , in a given industry by baseline U.S. consumption in that industry (where  $Y_{j,91}$  measures industry shipments,  $M_{j,91}$  are industry imports and  $E_{j,91}$  represent industry exports):

$$\Delta IP_{j\tau} = \frac{\Delta M_{j\tau}^{UC}}{Y_{j,91} + M_{j,91} - E_{j,91}}.$$

They instrument for local labor market exposure to Chinese imports with the weighted average across industries in the local labor market of Chinese exports to other rich countries in a given industry  $\Delta IPO_{j\tau}$ . The weights,  $\frac{L_{ij\tau}}{L_{i\tau}}$ , correspond to industry employment shares in the local labor market:

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<sup>15</sup>We use the updated instrument in Acemoglu et al. (2016) for our analysis, which includes 2011 as the final year.

Table 3.1: Bartik Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bartik Instrument (Unweighted)	Negative $\Delta$ Unemployment (ppt)	$\Delta$ Union Membership	$\Delta$ Strikes Per Million	$\Delta$ Workers Idle in Strikes Per Thousand	$\Delta$ Union Elections Per Million	$\Delta$ Union Election Voters Per Thousand	$\Delta$ Job to Job Transition Rate
	36.127*** (12.933)						
Bartik Instrument (Weighted)	2.545 (7.501)						
Negative $\Delta$ Unemployment (ppt) (Unweighted)		0.506** (0.245)	-0.365 (0.447)	-0.669* (0.382)	0.379 (1.111)	0.067 (0.195)	0.044 (0.029)
Negative $\Delta$ Unemployment (ppt) (Weighted)		4.463 (13.312)	-3.540 (18.693)	-11.259 (52.352)	0.046 (8.134)	2.337 (9.029)	0.058 (0.055)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Mean of Outcome	-5.64	8.85	2.86	1.05	21.5	1.32	2.39
Mean of Difference in Outcome	0.14	-0.26	-0.20	-0.06	-0.99	-0.05	-0.03
Observations	1950	1950	1800	1800	1850	1850	1400
Clusters	50	50	50	50	50	50	50

*Note:* This figure presents the effects of labor market tightness, instrumented with the canonical Bartik instrument following Bartik (1991), on several measures of union activity and job-to-job transitions. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. Union membership data and job-to-job transitions are constructed at the state-level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Strikes and workers idle per strike are constructed at the state-level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description. Job to job transitions are only asked about in the CPS beginning in 1994 and the corresponding time series data constructed from this data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). All standard errors clustered at the state level

$$\Delta IPO_{j\tau} = \frac{\Delta M_{j,\tau}^{OC}}{Y_{j,88} + M_{j,88} - X_{j,88}}$$

$$\Delta IPO_{i\tau}^{CZ} = \sum_j \frac{L_{ij\tau}}{L_{i\tau}} \Delta IPO_{j\tau}.$$

The endogenous variable  $\Delta IPO_{i\tau}^{CZ}$  is a weighted industry average of actual changes in Chinese imports, again weighting with local labor market employment shares.

$$\Delta IP_{i\tau}^{CZ} = \sum_j \frac{L_{ij\tau}}{L_{i\tau}} \Delta IP_{j\tau}.$$

This results in a specification of the form:

$$\Delta E_{i\tau} = \alpha_\tau + \beta \Delta IP_{i\tau}^{CZ} + e_{i\tau},$$

where  $\Delta E_{i\tau}$  is the percentage point change in the employment to population ratio in a commuting zone in a given long difference (1991 to 1999 or 1999 to 2011) and  $\beta$  captures the effect of (instrumented) Chinese import exposure. We estimate the model using 2SLS and cluster standard errors at the commuting zone level.

These local labor market effects of the China shock create exogenous variation in labor market tightness. There are direct effects of the China shock on exposed labor markets (exposed industries see employment reductions), but if local labor demand were perfectly elastic these would be offset by a reallocation of labor to other unaffected industries. Clearly, however, local labor demand is not perfectly elastic, and Acemoglu et al. (2016) find that there is no rise in employment in unaffected industries, which they attribute to the countervailing force of declining aggregate demand in the affected local labor markets. We therefore think that these act as substantial aggregate demand shocks at the local level.

We also build on the analysis of Ahlquist and Downey (2023) and Charles et al. (2021), who study the consequences of the China shock for union membership at the industry and state levels. They find that Chinese import competition reduced union membership in directly affected industries, but actually led to an increase in union membership in more exposed states. Relative to their paper, we are able to provide more precisely estimated estimates of the effects of the China shock on union membership in local labor markets because we construct union membership and shock exposure at the commuting zone level. In particular, instead of relying on a measure of state-level exposure to China's import penetration, we construct a continuous exposure measure at the commuting zone level, equipping us with additional meaningful variation. We use CPS microdata to assign respondents to their counties or metropolitan areas of residence (which is disclosed for 70% of individuals, we assign the remainder to the non-metropolitan areas of their state).

Additionally, we add commuting zone-level strike and union election data that we geocode from published location information in federal agency reports (the FMCS for strikes and the NLRB for union elections).

To follow the long difference design of Acemoglu et al. (2016), we construct five year rolling averages of our unionization outcomes around the years of the long differences. In other words, the change in union membership from 1991 to 1999 is measured as the average union membership in a commuting zone 1989–1993 subtracted from average membership 1997–2001. This increases the precision of these long difference outcomes and the results are very similar to simple three-year moving averages. An alternative approach that would allow us to use all of the unionization outcome data and improve precision would be to use all the rolling eight-year long differences. Again, results are qualitatively similar and we report them in our Online Appendix.

### *Effects of the China Shock on Union Activity*

We report results in Table 3.2 and also visualize effect sizes in Figures 3.A.12 and 3.A.14. We replicate the results of Acemoglu et al. (2016) with employment to population ratio as an outcome in the second row of Column (1) of Table 3.2<sup>16</sup>. The first row shows the same specification, but here we do not weight commuting zones by their baseline population in 1991. This yields a more negative effect that is much more precisely estimated. We think both of these specifications are defensible, but weakly prefer the unweighted one for the following reasons. The unweighted one is justified by a view of each commuting zone as a self-contained natural experiment in the effect of local aggregate demand shocks on union activity. Variation in local labor market tightness might be greater in small commuting zones, increasing the precision of the estimates (though measurement error in union membership in survey data from the CPS is also more of a concern in small commuting zones; the strike and election results rely on administrative data for which measurement error is less of a concern). If there is no treatment effect heterogeneity, or if our estimand is the structural parameter of the average commuting zone treatment effect of tightness on unionization, the unweighted approach is simply a more efficient estimator. However, if there is heterogeneity in treatment effects by population, and if our estimand is the effect of tightness on unionization faced by the average person in the population, only the weighted estimator is consistent.

Columns (4) to (8) of Table 3.2 show the effects on union outcomes.<sup>17</sup> For the sake of completeness, we include the change in Chinese imports as endogenous variable in rows three and four of the Table. For quantitative interpretation, we prefer to use the change in the (negative) unemployment rate as endogenous variable, which we report in columns five and six.

Across the unionization outcomes, in unweighted specifications, we find sizeable, positive effects on union activity for four out of five outcome variables as a consequence of labor

<sup>16</sup>The same estimate can be found in column 1 of Table 7 in Acemoglu et al. (2016)

<sup>17</sup>We also report effects on job-to-job transitions in column (9).



Table 3.2: China Shock Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

	(1) Negative Change in Chinese Imports (0.003) (0.003)	(2) Negative $\Delta$ Unemployment Rate (ppt) (0.006) (0.006)	(3) Employment/Pop. (AADHP) (0.239) (0.239)	(4) $\Delta$ Union Membership (0.074) (0.074)	(5) $\Delta$ Strikes Per Million (0.182) (0.182)	(6) $\Delta$ Workers Idle in Strikes Per Thousand (0.156** (0.064)	(7) $\Delta$ Union Election Votes Per Thousand (-0.466 (0.800)	(8) $\Delta$ Union Election Votes Per Thousand (0.272** (0.106)	(9) $\Delta$ Job to Job Transition Rate (0.016 (0.016)
Negative Chinese Exports Elsewhere (Unweighted)									
Negative Chinese Exports Elsewhere (Weighted)	0.022*** (0.001)	0.194*** (0.071)	1.638*** (0.460)	-0.000 (0.197)	0.087 (0.247)	-0.285 (0.180)	-2.218** (0.910)	0.159 (0.112)	-0.060 (0.098)
Negative Shock to Chinese Imports (Unweighted)									
Negative Shock to Chinese Imports (Weighted)									
Negative $\Delta$ Unemployment Rate (ppt) (Unweighted)				0.661*** (0.165)	0.558** (0.337)	0.266** (0.116)	-0.794 (1.860)	0.461** (0.184)	0.015 (0.015)
Negative $\Delta$ Unemployment Rate (ppt) (Weighted)				-0.104 (0.028)	0.281 (0.829)	-0.915 (0.265)	-7.450* (3.093)	0.512 (0.192)	-0.113 (0.073)
Year Fixed Effects	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$	$\checkmark$
Mean of Difference in Outcome	-0.83	-0.67	45.0	8.16	2.11	0.51	1.76	1.29	2.34
Observations	1444	1444	1444	1444	1444	1444	1444	1444	1444
Clusters	722	722	722	722	722	722	722	722	722

*Note:* This table presents effects of the China shock on unionization outcomes, through its effects on changes in labor market tightness (measured as the negative unemployment rate). The China shock is estimated as in Acemoglu et al. (2016) (AADHP) using long differences between 1991 and 1999 and 2011 and 1999 with local labor markets defined as commuting zones. Following AADHP, we annualize all the changes, dividing by 12 for the 1999–2011 long difference and 8 for the 1991–1999 long difference. Standard errors are clustered at the commuting zone level. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. The top two columns use industry-weighted Chinese import changes to instrument for Chinese import exposure in U.S. commuting zones. The bottom two columns use Chinese imports to other rich countries to instrument for unemployment at the commuting zone level. We do not report results for the effect of unemployment on the employment to population ratio, instrumenting with Chinese imports, though it is large, because we do not think that this is particularly interpretable. Rather, we view the AADHP results as confirmation that there is a first stage in the relationship between Chinese imports and local labor market tightness. Union membership data and job-to-job transitions are constructed at the commuting zone level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Job to job transitions are only asked about in the CPS beginning in 1994, so are available for only one of the two long differences (1999 to 2011). The time series of job-to-job transitions constructed from CPS data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). Strikes and workers idle per strike are constructed at the commuting zone level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description.

market tightness (implying that exposure to Chinese imports reduced unionization). For example, a 1ppt decrease in the unemployment rate is associated with a 0.66ppt increase in the unionization rate. The non-positive effect is on the change in the number of union elections (per million workers) where we find an effect of -0.794 (SE 1.360).

The positive association between tightness and unionization revealed in the unweighted specifications disappears in the weighted specifications. We find a mixed pattern of positive and negative point estimates and substantially wider confidence intervals compared to the unweighted specifications. The effect is only marginally significant at -7.450 (SE 3.993) for one of the outcome variables, the number of union elections.

We plot the results of the unweighted specifications in Figure 3.A.12. To put union membership and union election voters on a comparable scale, we standardize the outcome variables by dividing by their standard deviation. We also standardize the employment to population ratio used in Acemoglu et al. (2016) for comparison: this effect is smaller in magnitude and much more precisely estimated than the negative effects we see on union outcomes. This is intuitive as employment to population data from administrative data is a much less noisy outcome than, for example, strikes—we have only 14,000 strikes over the entire sample period, an average of 20 per commuting zone over the years 1984–2020, and only 15 of those years enter our rolling average. Given this lack of power, we are only able to detect very large results.

The 25<sup>TH</sup> percentile Chinese import shock is 0.02 and the 75<sup>TH</sup> percentile is a 0.11 percentage point annual increase in import penetration (i.e., going from a 0.83% Chinese import share to 1.93% over 10 years). The union membership results suggest that moving from a shock of 0.02 to 0.11 in annual import penetration (a 0.2 ppt rise to a 1.1 ppt rise in industry-weighted Chinese import consumption share) reduces union membership by 0.35 percentage points over 10 years (or 0.08 standard deviations). The overall increase in Chinese import penetration from 1991 to 2011 is 2.02 percentage points, suggesting that Chinese import exposure reduced unionization by 0.79 percentage points over the period. National unionization fell by 4.8 percentage points over that period; we think that attributing one sixth of that decline to sagging local demand due to Chinese import competition is likely an overstatement. The union membership results can also be driven by compositional changes as a result of the China shock, not just aggregate demand: if the direct effect of the China shock is the closure of unionized manufacturing firms, that will reduce unionization in exposed local labor markets by an additional channel besides local aggregate demand. There can also be other compositional effects on employment that happen due to Chinese import exposure separate from aggregate demand, like if displaced manufacturing workers moved into less unionized sectors like retail or if unaffected industries with high unionization in the same local labor markets that were linked to the affected firms saw declines in employment (see Acemoglu et al. (2016) for a discussion of the input-output linkages). Ahlquist and Downey (2023) discuss this possibility to rationalize their positive results, arguing that at the state level, the China shock seems to drive a reallocation into employment in more highly unionized services sectors like education and healthcare. The large effects could also be attributable to fixed characteristics of the

commuting zones exposed to Chinese imports, following Acemoglu et al. (2016) we have two long differences and no commuting zone fixed effects. Also, because the weighted results are insignificant and opposite-signed, the effects are driven by smaller commuting zones and therefore do not aggregate up to explaining any of the 4.8 percentage point decline in union membership.

### 3.6.3 Labor Supply Shock: Demographic Change

#### *Construction of Labor Supply Shock*

We construct an instrument for labor scarcity based on labor supply shocks using the lagged age structure of the population. The endogenous variable and instruments in this research design more closely capture labor scarcity rather than labor market tightness (and thereby more closely map to the model in Acemoglu and Wolitzky, 2011). We exploit predictable variation in the share of the population in a county that will be of working age in the future. Specifically, we use two instruments: the share of the population that is between the ages of 10 and 19, whom we call “future entrants,” and the share of the population between the ages of 55 and 64, whom we call “future retirees.” We then run long-difference specifications, analyzing the change in union outcomes over 10-year periods, as above, and the change in labor supply driven by predictable features of the cohort structure. This builds off work in Poterba (2004), DellaVigna and Pollet (2007), Böhm and Siegel (2021) and Böhm et al. (2021), who use predictable variation in the cohort structure to instrument for product demand and worker experience. Our main specification uses both future entrants and future retirees as instruments for the employment to population ratio, and our first stage can be written as:

$$\Delta EPOP_{i\tau} = \alpha_\tau + \phi \text{Entry}_{i\tau} + \lambda \text{Exit}_{i\tau} + \gamma_i + e_{i\tau}.$$

We define the objects as follows:

$$\begin{aligned} \Delta EPOP_{c\tau} &= \frac{\text{Employed}_{c,\tau+10}}{\text{Population}_{c,\tau+10}} - \frac{\text{Employed}_{c,\tau}}{\text{Population}_{c,\tau}}, \\ \text{Entry}_{c\tau} &= \frac{\text{Population}_{c,\tau}^{a \in [10,19]}}{\text{Population}_{c,\tau}}, \\ \text{Exit}_{c\tau} &= \frac{\text{Population}_{c,\tau}^{a \in [55,64]}}{\text{Population}_{c,\tau}}. \end{aligned}$$

The variable  $\text{Population}_{c,\tau}$  measures the population in county  $c$  in year  $\tau$ , the  $a \in [10, 19]$  indicates the subset of the population aged between 10 and 19 in county  $c$  in year  $\tau$  (and analogously for  $a \in [55, 64]$ ). The variable  $\text{Employed}_{c,\tau}$  measures the number of employed individuals in county  $c$  in year  $\tau$ .

Our second-stage specification is defined as follows:

$$\Delta Y_{c\tau} = \alpha_{\tau} + \beta \widehat{\Delta EPOP}_{c\tau} + \gamma_i + e_{c\tau},$$

for various unionization outcomes  $y$ , with base years  $\tau$  ranging from 1990 to 2012, and horizon for the difference of 10 years. As instruments, we use both the share of future entrants divided by the current working age population and the share of future retirees in the current working age population (we could add these together and create a single variable that is the predicted change in the working age population, but prefer the more flexible specification where each enters separately in the prediction of  $\Delta EPOP_{c\tau}$ ). We estimate the specification using 2SLS and cluster standard errors at the county level. We report both unweighted and weighted (by population) estimation results.

One concern with this design when we look at union membership as an outcome is that older workers are more likely to be unionized, so the share of future retirees in the working age population might directly affect union membership rates if older workers who exit are not replaced by younger unionized workers. A larger drawback of this design is that counties with younger age structures can be systematically different in other ways; the share of children in the population is higher in high-fertility states in the South and West. We attempt to alleviate this concern by including county fixed effects.

#### *Effects of Labor Supply Side Shocks from Demographic Change on Union Activity*

We report results in Table 3.3 and also visualize effect sizes in Figures 3.A.13 and 3.A.14. We find a first stage  $F$ -statistic of 72.7 and effects of future entrants and future retirees on the employment to population ratio that both go the expected direction and are individually strongly predictive of changes in the employment-to-population ratio.

We present results with the negative employment to population ratio as the endogenous variable, so the prediction of labor scarcity fostering unionization would imply positive coefficients. The IV estimates offer mixed evidence. We find positive point estimates for all outcomes in the weighted specifications; in the unweighted specifications, we find two negative and three positive estimates for effects on unionization. For union membership, we find a negative effect of -0.060 in the unweighted specification that is marginally significant (SE 0.036). In the weighted specification, we find a positive but statistically insignificant effect of 0.085 (SE 0.110). We find no systematic pattern of effects on strikes. The most positive and statistically significant effects we find are on union elections and union voters in the weighted specification. For the effect on union election voters per thousand, we find a statistically significant estimate of 0.261, implying that a labor-supply driven decrease in a county's employment to population ratio by one percentage point would lead to an increase in 0.26 voters per thousand (average number of union voters per thousand in a given year is 1.32 in our observation window). The upper limit of the 95% confidence interval for this variable is an increase of 0.38 voters per thousand workers.

Table 3.3: Demographic Change Results: Effects of Shocks to Labor Scarcity on Union Activity and Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share Retiring (Unweighted)	Negative $\Delta$ EPOP (ppt)	$\Delta$ Union Membership	$\Delta$ Strikes Per Million	$\Delta$ Workers Idle in Strikes Per Thousand	$\Delta$ Union Elections Per Million	$\Delta$ Union Election Voters Per Thousand	$\Delta$ Job to Job Transition Rate
	0.201*** (0.050)						
Negative Share Joining (Unweighted)	0.343*** (0.034)						
Share Retiring (Weighted)	0.259*** (0.065)						
Negative Share Joining (Weighted)	0.409*** (0.042)						
Negative $\Delta$ EPOP (ppt) (Unweighted)		-0.060* (0.036)	-0.071 (0.122)	0.037 (0.072)	0.074 (0.492)	0.043 (0.079)	-0.035*** (0.007)
Negative $\Delta$ EPOP (ppt) (Weighted)		0.085 (0.110)	0.278** (0.131)	0.237 (0.204)	1.853*** (0.477)	0.261*** (0.065)	-0.024 (0.025)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Mean of Outcome	-45.2	7.35	1.45	0.33	12.3	0.95	2.33
Mean of Difference in Outcome	0.26	-1.74	-1.11	-0.23	-7.41	-0.67	-0.27
Observations	71847	71847	65597	65597	65597	65597	59347
Clusters	3125	3125	3125	3125	3125	3125	3125

*Note:* This table presents effects of labor scarcity on unionization outcomes at the county level. We construct an instrument for labor supply shocks using the age structure of the population. In doing so, we exploit predictable variation in the share of the population that will be of working age in the future. Specifically, we use two instruments: the share of the population that is between the ages of 10 and 19, who we call “future joiners,” and the share of the population between the ages of 55 and 64, who we call “future retirees.” We then run long difference specifications, looking at the change in union outcomes over 10-year periods, as above, and the change in labor supply driven by predictable features of the cohort structure. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. Union membership data and job-to-job transitions are constructed at the county level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Strikes and workers idle per strike are constructed at the county level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description. EPOP is missing for 7 counties around New Orleans in 2005 and 2006 due to Hurricane Katrina, affecting 28 long differences. Job to job transitions are only asked about in the CPS beginning in 1994 and the corresponding time series data constructed from this data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). Standard errors are clustered at the county level.

### 3.7 Conclusion

Our study embarked on an exploration of how labor market conditions, particularly labor market tightness and labor scarcity, influence individual and collective unionization activities. Our large-scale survey experiment involving over 5,000 U.S. workers revealed that increased perceptions of labor market tightness lead to a slight rise in the propensity to engage in union activities. However, this effect, while statistically significant, remains modest. Correspondingly, our analysis of the aggregate impact of tightness through three quasi-experimental designs did not uncover a systematic, substantial link between labor market conditions and broader union activities, including membership, elections, and strikes.

Our evidence points to an explanation for why effects of tightness on unionization may be limited. In our experiment, we found that tighter labor markets increased quit behavior. These patterns resonate with analyses of the most recent episode of labor market tightness documenting an association between labor market tightness and wage increases, with notable wage gains concentrated among workers switching jobs (Autor et al., 2023). In turn, analyses of the effect of changes in the value of nonemployment find only limited effects on wages within existing employment relationships (Jäger et al., 2020). These findings could help to explain why tighter labor markets do not significantly boost unionization efforts. As workers perceive better outside options, their reliance on unions for wage bargaining or job security may diminish and they may instead prefer to leverage external market conditions directly. On net, when workers in our experiment learn that the exit alternative is more attractive, more of them plan to exercise it rather than use the availability of more attractive exit options to increase their voice in their current workplace.

An important limitation of our survey experiment lies in its focus on individual-level effects of labor market tightness on unionization intentions, which may not fully capture broader, potentially more impactful social spillover effects. While we observed a small increase in individual willingness to engage in union activities with perceived improvements in labor market conditions, small individual-level effects could translate into larger equilibrium effects (see Hertel-Fernandez et al., 2020; Naidu, 2022). However, our quasi-experimental analysis of aggregate variation in labor market tightness did not reveal systematic effects on unionization at a larger scale either, and our estimates allow us to rule out moderate positive effects of tightness on unionization. A limitation is that the quasi-experimental evidence we bring to bear stems from the U.S. Our analysis of a panel of OECD countries pointed to a more robust, positive relationship between unionization and tightness. Our findings thus raise the possibility that tightness may propel unionization and collective bargaining in institutional contexts outside of the U.S.

Our evidence suggests that the dynamics of unionization are influenced by a complex interplay of factors beyond individual and aggregate perceptions of labor market conditions. The absence of pronounced effects of tightness and labor scarcity in aggregate,

quasi-experimental analyses in the U.S. indicates that other factors, possibly including the institutional and legal environment and organizational culture might have a more decisive impact on unionization trends. In the U.S., the institutional and legal environment, for example right-to-work laws, have played an important role in shaping (de-)unionization (Ellwood and Fine, 1987; Farber, 1984). Likewise, employer opposition—itself affected by the institutional environment—has also shaped the evolution of U.S. unionization (Bronfenbrenner, 1996; Kochan et al., 2023; Wang and Young, 2022). Recent work by Hertel-Fernandez et al. (2022) points towards arrangements outside of the current U.S. institutional setup, including sectoral bargaining and worker involvement in management decisions, whose availability may affect U.S. workers’ willingness to unionize.

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## 3.A Appendix to Chapter Three

### 3.A.1 Data Cleaning Procedure

This section describes all stages of the cleaning procedure during and after the survey experiment, covering the sample definition, quality checks during the survey, and the cleaning procedure.

**Sample Definition and Original Data.** The survey targeted private-sector workers aged between 18 and 65, excluding those who were self-employed or held chief executive positions at their respective firms. Respondents that did not qualify in these dimensions were screened out during the begin the survey. In order to ensure respondents attentiveness, two attention checks were incorporated into the survey. Participants were required to pass at least one attention check to remain in the survey. To account for fast or careless responses, we identified and excluded respondents who completed the survey in a time faster than 50% of the median survey time. The original dataset therefore only includes responses, within our targeted population, that passed at least one attention check, completed the entire survey and took at least 8.4 minutes for the survey completion.

**Quality Checks.** While each survey participant got an individualized survey access, we test for multiple survey submissions via duplicates in the IP address and response ID. We do not find a single duplicate.

**Winsorization.** Given the presence of potential outliers in some variables, we applied winsorization to a set of variables. Winsorization involves replacing extreme values with less extreme values, thereby reducing the influence of outliers on statistical analysis. This procedure helps to make the variables more comparable and reduces their sensitivity to outliers while preserving the overall distributional characteristics of the data. Winsorization took place at the bottom and top 2% of responses for: prior and posterior beliefs about outside options, beliefs about own outside options, outside option prediction, union activity intentions, job search intention, beliefs about getting fired due to union activity, union vote intention of colleagues, perceived union density in sector and occupation, beliefs about unions impact on wages, working conditions and job security, as well as expected changes in inflation, regional unemployment, firm profits.

### 3.A.2 Information Treatment Construction

This section describes the construction of the information treatment based on an objective benchmark of every worker’s personalized job finding probability. This personalized job finding probability is provided in terms of the percentage chance of finding a new job within 3 months after an unexpected job loss during the recent tightening of the US labor market.

**The Dataset.** The personalized estimate for the information treatment is based on the U.S. Current Population Survey’s outgoing rotational group. The outgoing rotational group is a longitudinal household survey conducted by the U.S. Census Bureau with frequently changing respondents. Households are subjected to an interview cycle of four

months, followed by a gap of eight months wherein no interviews are conducted, and subsequently, a second round of four-month interviews. Due to information about the employment status and detailed socioeconomic characteristics, this provides access to close to real time information about current labor market dynamics. To provide survey respondents with information tailored to recent labor market dynamics, the analysis concentrates on the period of June 2021 to May 2023, with a sample restriction equivalent to the survey experiment: private-sector workers between 18 and 65, who are neither self-employed nor hold a chief executive position at their firm. For the analysis of the job finding probability of an unexpected job loss equivalent to being fired due to union activities, we further restrict the sample to two additional conditions. First, we focus the analysis on workers who can be observed in at least two consecutive months and who were unemployed in the first of the two with an unemployment duration of eight weeks or less. This restriction is needed to be able to measure the job finding probability within three months. Second, unemployment must result from a job loss, which excludes the end of temporary employment, job leavers and individuals who just entered the labor force. This restriction is needed to construct our objective benchmark on an unexpected job loss equivalent of being fired. These sample restrictions lead to a final sample of 2,896 individuals. We then create a dummy variable equal to one if an individual unemployment duration was eight weeks or less. Out of the remaining sample, 52% of individuals were able to find a new job within three months of unemployment.

**Personalized Information.** The information treatment was constructed using lasso regressions. We split the data into two subsets of training and test data (80%/20%) and run logistic lasso regressions on the dependent variable of finding a new job within three months of unemployment. We allowed for the covariates: age, age<sup>2</sup>, education, occupation, industry, state and the interaction of occupation and state. Finally, we let lasso select the most important predictors, and predict respondent’s probability of re-employment within three months based on the best out-of-sample performing model. The resulting model was then included into our own survey in Qualtrics and feed with respondent’s characteristics to calculate their individualized job finding probability within three month of unemployment in real time in the survey.

### 3.A.3 Expert Survey

We collected data for an expert survey at the end of December 2023 from participants of the NBER Summer Institute in 2022 and 2023 and the Labor Studies program affiliates. Table 3.A.1 reports the results of the survey. Our final sample consists of 24 completed surveys. In line with the targeted pool, a majority of 22 respondents (92%) state that they possess expertise in the domain of labor economics. Out of 24 respondents, 22 (92%) state they are Faculty or Post-Docs. The remaining two respondents state that they are a researcher outside of academia and a graduate student. After a brief introduction to our research design, experts were asked to predict the effect direction and effect size of the survey experiment and quasi-experimental research design based on the county’s employment-to-population ratio.

**Survey Experiment Prediction.** When asked for the expected direction of the causal effect of an increase in workers’ belief about the three-months job-finding probability on workers’ willingness to vote for a union at their workplace, 17% predict a decrease and 25% predict no impact. The remaining 58% correctly predict an increase in workers’ willingness to vote for a union. Next, we asked for the effect size of the probability of voting for a union among workers whose belief about the 3-month job-finding rate increased by 10ppt. The median prediction is 2 ppt with a distribution of predicted effect sizes between -10 ppt and 20 ppt. In Table 3.1 (Panel B, column (3)), we had estimated an actual, implied effect size of 1 ppt so that the average expert expects an effect that is twice as large as the one we estimate. Only accounting for respondents that correctly predicted an increase in workers’ willingness to vote for a union, the median predicted effect size is 4.5 ppt, implying an overestimation of the effect size by four times the actual impact.

**Quasi-Experiment Prediction.** When asked for the expected direction of the causal effect of an increase in labor scarcity (measured as a supply-driven decrease in a county’s employment-to-population ratio) increases, decreases, or does not affect workers willingness to engage in union activities, 8% of respondents predict a decrease in union activities. 58% of respondents predict an increase in union activities. 33% of respondents predict no impact of an increase in labor scarcity on union activities. Next, we asked for the effect size of a one percentage point increase in labor scarcity (measured as a decrease in the employment-to-population ratio) increase/decrease the number of voters in union elections (measured as the difference between the number of voters per thousand workers in the current compared to the previous year). The median prediction is an increase of 2.5 out of 1000 workers with a distribution of predicted effect sizes between -10 and 100 workers; the mean is 12.96. In Table 3.3 (column (6)), we had estimated an actual effect size between 0.043 (SE 0.079) and 0.261 (SE 0.065) depending on the specification. The median expert prediction for the effect size is thus an order of magnitude larger than the one we actually estimate.

Table 3.A.1: Results of the Expert Survey

	Mean	SD	P50	Min	Max	N
<b>Research Field</b>						
Labor Economics	0.92	0.28	1.00	0.00	1.00	24
Industrial Relations	0.21	0.41	0.00	0.00	1.00	24
Political Economy	0.08	0.28	0.00	0.00	1.00	24
Personnel Economics	0.12	0.34	0.00	0.00	1.00	24
Other Economics	0.12	0.34	0.00	0.00	1.00	24
<b>Position</b>						
Faculty or Post-Doc	0.92	0.28	1.00	0.00	1.00	24
Researcher Outside of Academia	0.04	0.20	0.00	0.00	1.00	24
Graduate Student	0.04	0.20	0.00	0.00	1.00	24
<b>Prediction Effect Direction Survey Experiment</b>						
Decreases Union Activity	0.17	0.38	0.00	0.00	1.00	24
No Effect on Union Activity	0.25	0.44	0.00	0.00	1.00	24
Increases Union Activity	0.58	0.50	1.00	0.00	1.00	24
<b>Prediction Effect Direction Quasi-Experiment</b>						
Decreases Union Activity	0.08	0.28	0.00	0.00	1.00	24
No Effect on Union Activity	0.33	0.48	0.00	0.00	1.00	24
Increases Union Activity	0.58	0.50	1.00	0.00	1.00	24
<b>Predicted Effect Size</b>						
Effect Size Survey Experiment	2.46	5.51	2.00	-10.00	20.00	24
Effect Size Quasi-Experiment	12.96	27.31	2.50	-10.00	100.00	24

*Note:* This table reports descriptive statistics of the expert survey conducted in December 2023. After a brief introduction to our research design, experts were asked to predict the effect direction and effect size of the survey experiment and quasi-experimental research design with supply-driven variation in labor scarcity. The predictions were requested based on the following survey questions. *Prediction Effect Direction Experiment:* “Do you predict that an increase in the subjective job-finding probability (within three months), increases, decreases, or does not impact workers’ willingness to vote for a union at their workplace?”; *Prediction Effect Direction Quasi-Experiment:* “Would you predict that an increase in labor scarcity (measured as a decrease in a county’s employment-to-population ratio) increases, decreases, or does not affect workers’ willingness to engage in union activities?”; *Effect Size Experiment:* “Consider someone whose belief about the 3-month job-finding rate increased by 10ppt as a consequence of receiving information about the job finding probability of workers similar to them. By how much would their stated probability to vote for a union in a union election increase?”; *Effect Size Quasi-Experiment:* “By how much would a one percentage point increase in labor scarcity (measured as a decrease in the employment-to-population ratio) increase the number of voters in union elections (measured as the difference between the number of voters per thousand workers in the current compared to the previous year)?”.



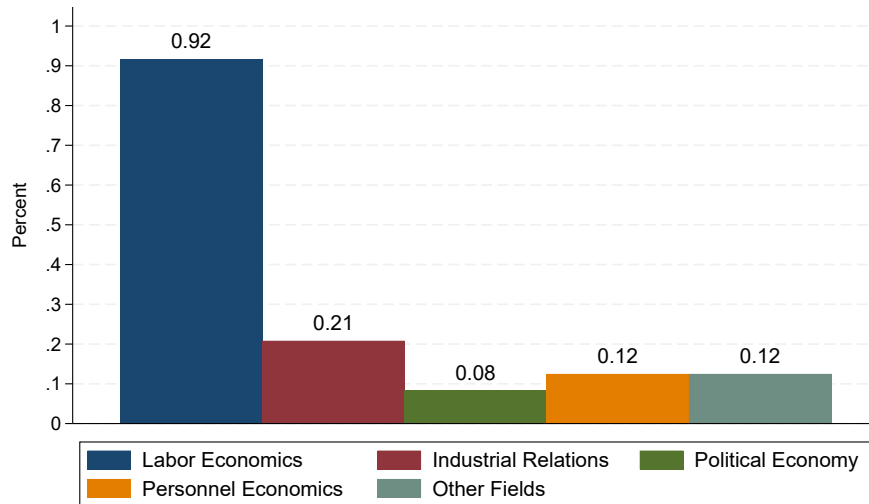


Figure 3.A.1: Experts' Stated Field of Expertise

*Note:* This figure shows the distribution of the experts' stated field of research/specialization, given the opportunity of multiple answers.

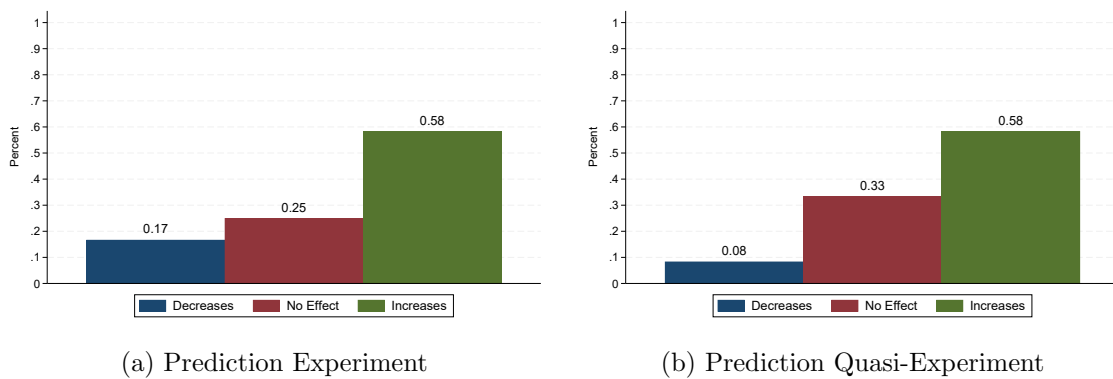


Figure 3.A.2: Predicted Experimental and Quasi-Experimental Effect Direction

*Note:* This figure shows the distribution of experts' predicted effect direction for the experimental and quasi-experimental research design, respectively. Panel (a) covers experts' responses to the question "Do you predict that an increase in the subjective job-finding probability (within three months), increases, decreases, or does not impact workers' willingness to vote for a union at their workplace?". Panel (b) covers experts' responses to the question "Would you predict that an increase in labor scarcity (measured as a decrease in a county's employment-to-population ratio) increases, decreases, or does not affect workers' willingness to engage in union activities?".

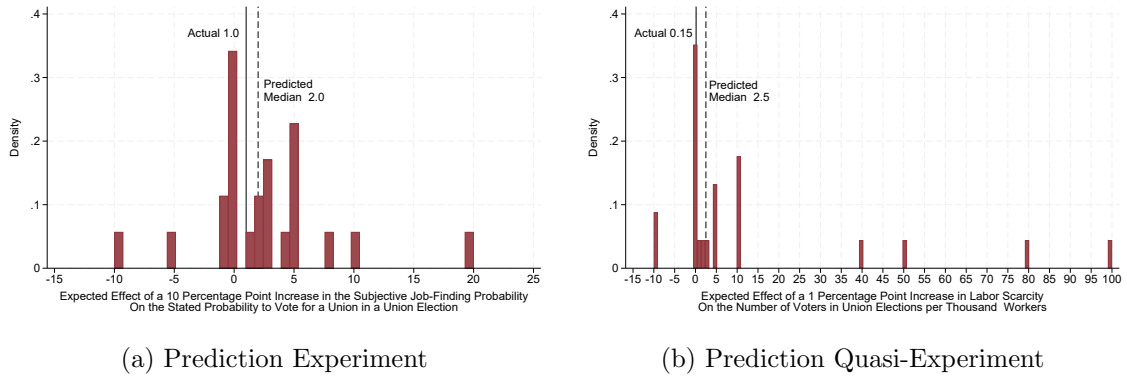


Figure 3.A.3: Predicted Experimental and Quasi-Experimental Effect Size

*Note:* This figure shows the distribution of experts' predicted effect size for the experimental and quasi-experimental research design, respectively. Panel (a) covers experts' responses to the question "Consider someone whose belief about the 3-month job-finding rate increased by 10 ppt as a consequence of receiving information about the job-finding probability of workers similar to them. By how much would their stated probability to vote for a union in a union election increase/decrease?". The actual value is the estimate from Table 3.1 (Panel B, column (3)). Panel (b) covers experts' responses to the question "By how much would a one percentage point increase in labor scarcity (measured as a decrease in the employment-to-population ratio) increase/decrease the number of voters in union elections (measured as the difference between the number of voters per thousand workers in the current compared to the previous year)?" The actual value is based on the average of the estimates from Table 3.3 (column (6)).

### 3.A.4 Summary Statistics

Table 3.A.2: Survey Experiment Summary Statistics

	N	Mean	SD	P50	Min	Max
<b>Panel A: Survey Information</b>						
Survey time	5,510	26.09	40.72	18.12	8.44	2,319.60
Treated	5,510	0.50	0.50	1.00	0.00	1.00
Estimate certainty	5,510	3.75	0.96	4.00	1.00	5.00
Treatment trust	2,758	2.11	0.90	2.00	1.00	5.00
Perceived political bias	5,510	3.02	0.71	3.00	1.00	5.00
<b>Panel B: Sociodemographics</b>						
Full time employee	5,510	0.86	0.35	1.00	0.00	1.00
Female	5,510	0.50	0.50	0.00	0.00	1.00
Age	5,510	41.32	11.96	40.00	18.00	65.00
University degree	5,510	0.66	0.47	1.00	0.00	1.00
Union member	5,510	0.07	0.25	0.00	0.00	1.00
Union sentiment	5,510	5.08	1.46	5.00	1.00	7.00
Political orientation	5,510	2.21	0.87	2.00	1.00	4.00
<b>Panel C: Labor Market Beliefs</b>						
Own search beliefs	5,510	56.53	28.26	50.00	2.00	100.00
Prior search beliefs	5,510	52.21	23.23	50.00	6.00	100.00
Posterior search beliefs	5,510	49.90	20.84	50.00	5.00	98.00
Expected unemployment increase	5,510	43.43	27.51	45.00	0.00	100.00
Inflation expectation	5,510	10.90	23.26	9.00	-100.00	100.00
<b>Panel D: Survey Outcomes</b>						
Union vote chance	5,510	52.17	29.77	50.00	0.00	100.00
Union vote public chance	5,510	52.60	31.78	50.00	0.00	100.00
Organize an election chance	5,510	35.59	29.07	35.00	0.00	100.00
Strike participation chance	5,510	31.78	28.86	30.00	0.00	100.00
Job search intention	5,510	39.63	29.81	38.00	0.00	100.00
<b>Panel E: Real Outcomes</b>						
Time spent on coworker message	5,510	67.81	148.98	18.50	0.00	4266.70
Copied coworker message	5,510	0.03	0.16	0.00	0.00	1.00
Willingness to pay for employer retaliation information	5,510	4.60	3.83	5.00	0.00	10.00
Willingness to pay for job search information	5,510	4.25	3.77	5.00	0.00	10.00
Willingness to pay for unionization information	5,510	4.20	3.83	5.00	0.00	10.00
<b>Panel F: Workplace characteristics</b>						
Firm with 250 and more employees	5,510	0.40	0.49	0.00	0.00	1.00
Represented by a union	5,510	0.26	0.44	0.00	0.00	1.00
Months employed at current firm	5,510	53.38	72.78	33.00	0.00	1500.00
Relation to management	5,510	2.08	0.87	2.00	1.00	5.00
Working conditions	5,510	2.03	0.86	2.00	1.00	5.00
<b>Panel G: Workplace Beliefs</b>						
Believed share of union members in industry	5,510	32.57	24.43	30.00	0.00	87.00
Believed share of union members in occupation	5,510	31.58	24.46	30.00	0.00	87.00
Expected change in firm profits	5,510	5.10	20.21	0.00	-50.00	67.00
Beliefs about firing due to union activity	5,510	36.37	29.43	32.00	0.00	100.00
Beliefs about retaliation due to union activity	5,510	4.75	1.67	5.00	1.00	7.00
Mechanism opposition concerns	5,510	2.14	0.83	2.00	1.00	5.00
Mechanism bargaining power	5,510	2.08	0.82	2.00	1.00	5.00
Union working condition premium beliefs	5,510	7.47	28.63	0.00	-75.00	80.00
Union wage premium beliefs	5,510	4.64	17.45	5.00	-100.00	100.00
Union job loss premium beliefs	5,510	-0.24	20.15	0.00	-50.00	60.00

*Note:* This table reports the summary statistics of the survey experiment. Panel A reports summary statistics related to survey and treatment quality. Panel B reports summary statistics of the sociodemographic characteristics of survey respondents. Panel C reports summary statistics about labor market beliefs. Panel D reports summary statistics of the main survey outcomes. Panel D reports summary statistics of the real outcomes. Panel F reports summary statistics of workplace characteristics. Panel G reports summary statistics of respondents' workplace beliefs.

Table 3.A.3: Representativeness of the Survey Sample

	(1)	(2)	(3)	(4)	(5)
	U.S. Employed Population	Survey Sample	Treatment Group	Control Group	(3) = (4)
<b>Age</b>					
18-34	0.36	0.36	0.35	0.36	0.552
35-49	0.33	0.33	0.33	0.33	0.864
50-65	0.31	0.31	0.32	0.31	0.660
<b>Gender</b>					
Male	0.53	0.50	0.50	0.50	0.572
Female	0.47	0.50	0.50	0.50	0.572
<b>Race</b>					
European American/White	0.77	0.76	0.76	0.76	0.889
African American/Black	0.13	0.13	0.12	0.13	0.753
Asian	0.07	0.07	0.07	0.07	0.734
Mixed Race/Other	0.04	0.04	0.05	0.04	0.524
<b>Divisions</b>					
New England	0.05	0.05	0.05	0.05	0.523
Middle Atlantic	0.12	0.13	0.13	0.13	0.667
East North Central	0.14	0.15	0.15	0.15	0.649
West North Central	0.07	0.07	0.07	0.08	0.107
South Atlantic	0.20	0.19	0.19	0.19	0.894
East South Central	0.05	0.05	0.06	0.05	0.606
West South Central	0.12	0.12	0.12	0.12	0.826
Mountain	0.08	0.07	0.07	0.07	0.812
Pacific	0.16	0.16	0.16	0.16	0.506
<b>Income</b>					
Smaller \$23,000	0.25	0.25	0.25	0.25	0.428
\$23,000 to \$41,000	0.25	0.25	0.25	0.25	0.698
\$41,001 to \$70,000	0.25	0.25	0.25	0.25	0.938
Larger \$70,000	0.25	0.25	0.25	0.25	0.629
<i>N</i>		5,510	2,758	2,752	5,510

*Note:* This table compares the composition of our survey sample to the composition of the U.S. employed population based on the Current Population Survey (CPS) of the year 2022 in terms of age, gender, race and location (census division). For national income quartiles, we draw on the American Community Survey (ACS) of the year 2021. In parallel to the minor restrictions imposed during the sampling process of our survey, we exclude the following individuals from the ACS sample that serves as our reference group: individuals aged below 18 and above 65, those who worked in the public sector or served in the military, and individuals who reported to work as chief executives.

### 3.A.5 Survey Response Quality

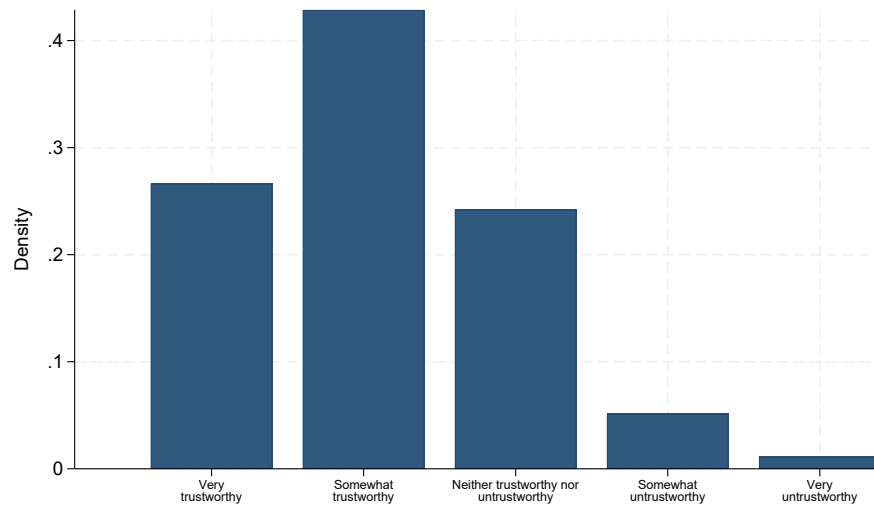


Figure 3.A.4: Perceived Trustworthiness of the Information Treatment

*Note:* This figure shows the distribution of treated respondent's perception of the trustworthiness of the information about labor market tightness that they were provided with. Answers are based on the survey question "At some point in the survey, we provided you with information about the share of people similar to you (but lost their job), who found a new job within 3 months, based on data by the U.S. Census Bureau. Did you find the information we provided you with trustworthy or untrustworthy?".

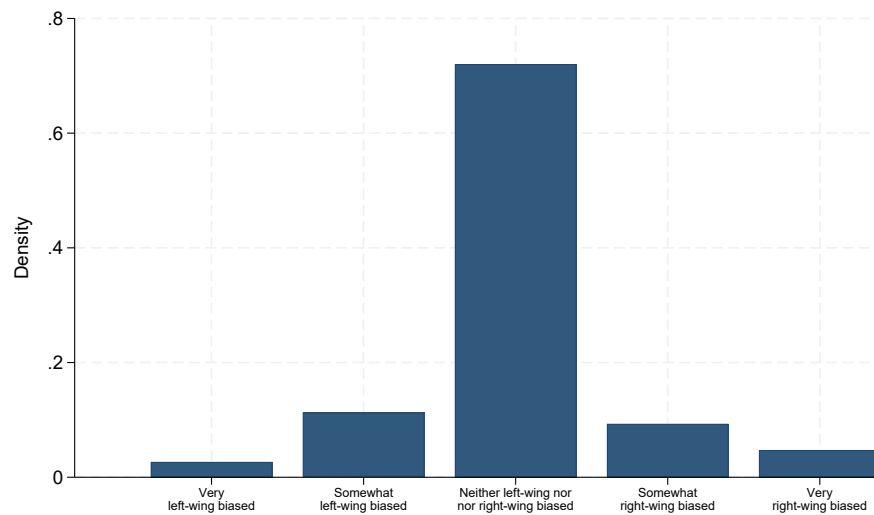


Figure 3.A.5: Perceived Political Bias of the Survey

*Note:* This figure shows the distribution of treated respondent's perception of the political bias of the survey. Answers are based on the survey question "Do you feel this survey was politically biased?".

### 3.A.6 Labor Market Conditions and Union Activity Patterns

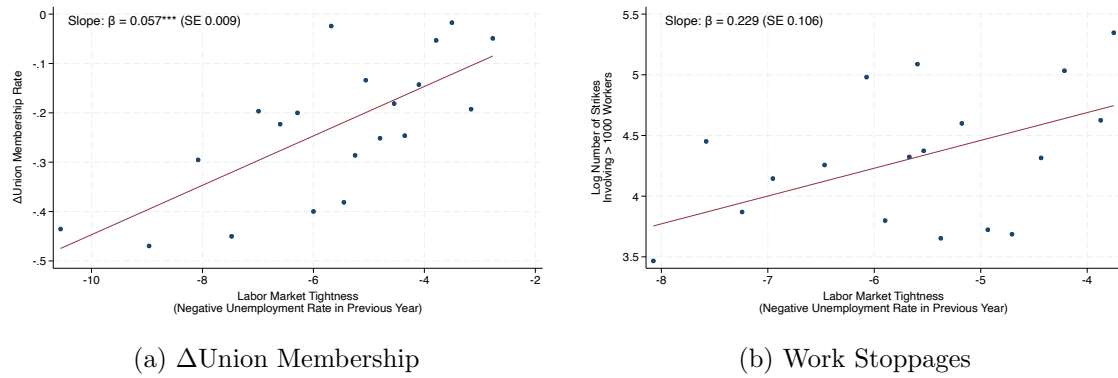


Figure 3.A.6: OLS Labor Market Tightness and Union Activity

*Note:* This figure shows binned scatter plots of union activity outcomes and labor market tightness, measured in levels of the previous year's unemployment rate. In Panel (a) the outcome variable is the year-to-year change in the union membership rate retrieved from the Unionstats.com website and covers the years 1983 to 2022. In Panel (b) the outcome variable is number of work stoppages involving more than 1000 workers and based on data provided by the U.S. Bureau of Labor Statistics for the years 1947 to 2023. We report robust standard errors in parentheses.

Table 3.A.4: OLS Labor Market Tightness and Union Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Labor Market Tightness Belief	Layoff Concerns	Union Vote	Union Vote Public	Willing to Strike	Organize Election	Std Union Index	Job Search	WTP Info Unionization	WTP Info Retaliation	WTP Info Job Search	Pos Message
	-0.0157*** (0.0015)	0.2132*** (0.0254)	0.2766*** (0.0266)	0.1124*** (0.0242)	0.0721*** (0.0241)	0.0213*** (0.0027)	0.1789*** (0.0251)	-0.0184*** (0.0030)	-0.0225*** (0.0030)	-0.0103*** (0.0030)	-0.0006 (0.0007)
Constant	5.5254*** (0.3894)	34.9405*** (6.5796)	36.0229*** (6.8843)	30.6096*** (6.2724)	39.5296*** (6.2369)	-1.2075* (0.7002)	30.1965*** (6.4982)	6.9320*** (0.7875)	7.2786*** (0.7699)	7.9743*** (0.7780)	0.3084 (0.2389)
Observations	2,752	2,752	2,752	2,752	2,752	2,752	2,752	2,752	2,752	2,752	935

*Note:* This table shows estimates based on the full sample of qualified respondents in the control group. *Labor Market Tightness Belief* denotes respondents' estimate of the probability that someone like them, but who lost their job, would find a new job within 3 months. The outcome variable in Column (1) is measured by the question "Suppose you were to lose your job this month. How worried would you be about finding a new one?". The outcome variables in Column (2) - Column (5) are based on the following survey questions. *Union Vote*: "If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would vote for a union?"; *Union Vote Public*: "If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would make your vote public?"; *Organize Election*: "What is the percent chance that you would help organize a union election at your establishment in the coming weeks?"; *Willing to Strike*: "What is the percent chance that you would participate in any strike-related activities?". The outcome variable in Column (6) is the sum of the standardized outcomes in Column (2) - (5). The outcome variable in Column (7) is measured by the question *Job Search*: "Over the next 12 months, what is the percent chance that you will look for a new job at a different company?". The outcome variables in Column (8) - Column (10) contain workers willingness to pay for information about unionization (Column 8), employer retaliation (Column 9), and job search advice (10). The outcome variable in Column (11) is a binary variable that measures the pro-union sentiment in workers' message to coworkers, conditional on writing a message. We control for gender, age group, income group, race, union membership education, state, industry, and political orientation as controls. We report robust standard errors in parentheses.

### 3.A.7 Outside Option Perceptions

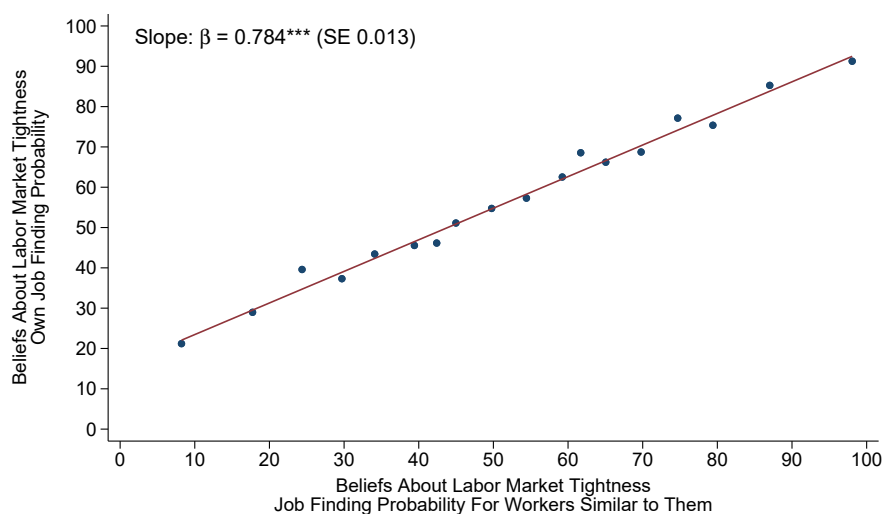


Figure 3.A.7: Workers' Beliefs About Own vs. Similar People's Job Finding Probability

*Note:* This figure shows a binned scatter plot of respondents' beliefs about their own job finding probability and their beliefs about this probability for workers similar to them along several dimensions relevant to the job market. In particular, we ask them to think of workers employed in the same occupation and industry, who work in the same state, have the same level of education, and the same age as the respondent. For more details, please see Appendix Section 3.A.10.

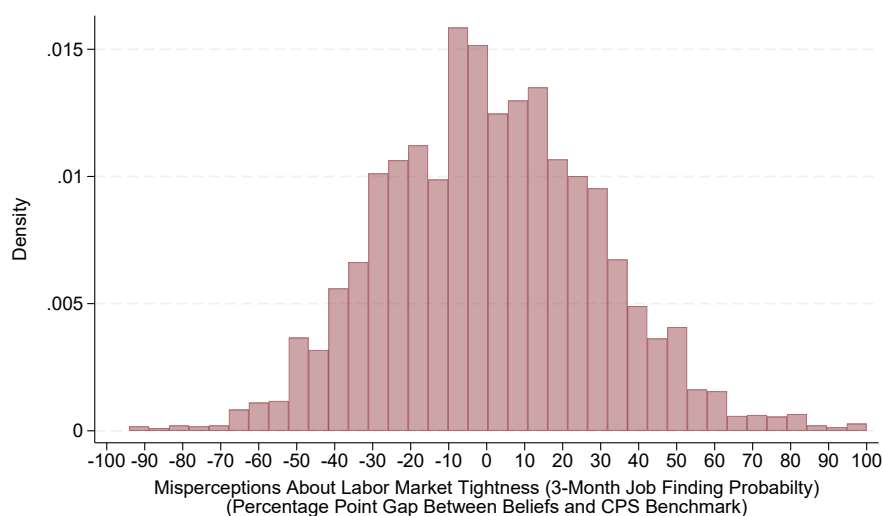


Figure 3.A.8: Distribution of the Misperceptions of Job Finding Probability

*Note:* This figure shows the average for respondents' prior misperception of others probability of finding a new job within three months by socioeconomic characteristics. Misperceptions are defined as the difference between respondents beliefs about their probability of finding a new job within three months and the objective measure of this object that we constructed based on the U.S. Current Population Survey.



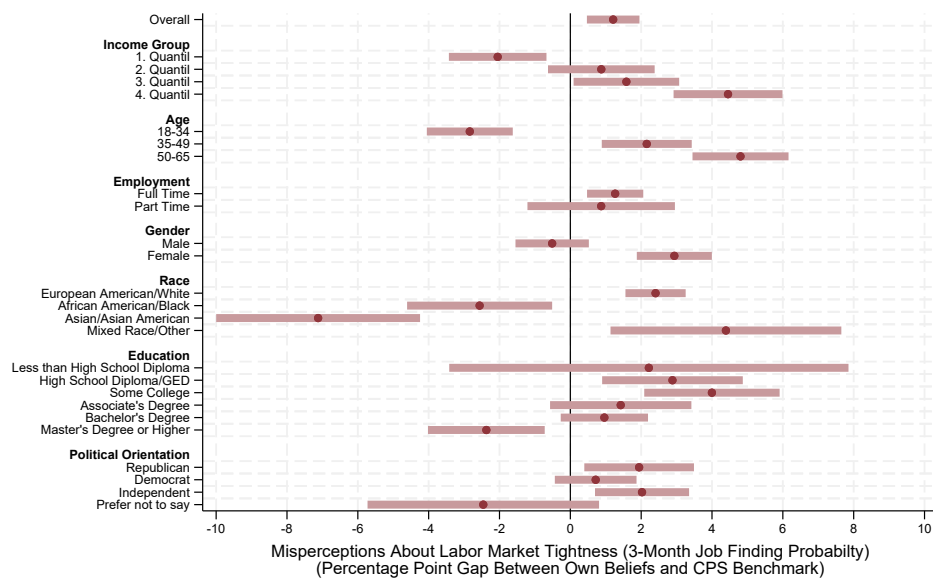


Figure 3.A.9: Heterogeneity in Misperceptions of Job Finding Probability

*Note:* This figure shows the average for respondents prior misperception of others probability of finding a new job within three months by socioeconomic characteristics. Misperceptions are defined as the difference between respondents beliefs about their probability of finding a new job within three months and the objective measure of this object that we constructed based on the U.S. Current Population Survey.

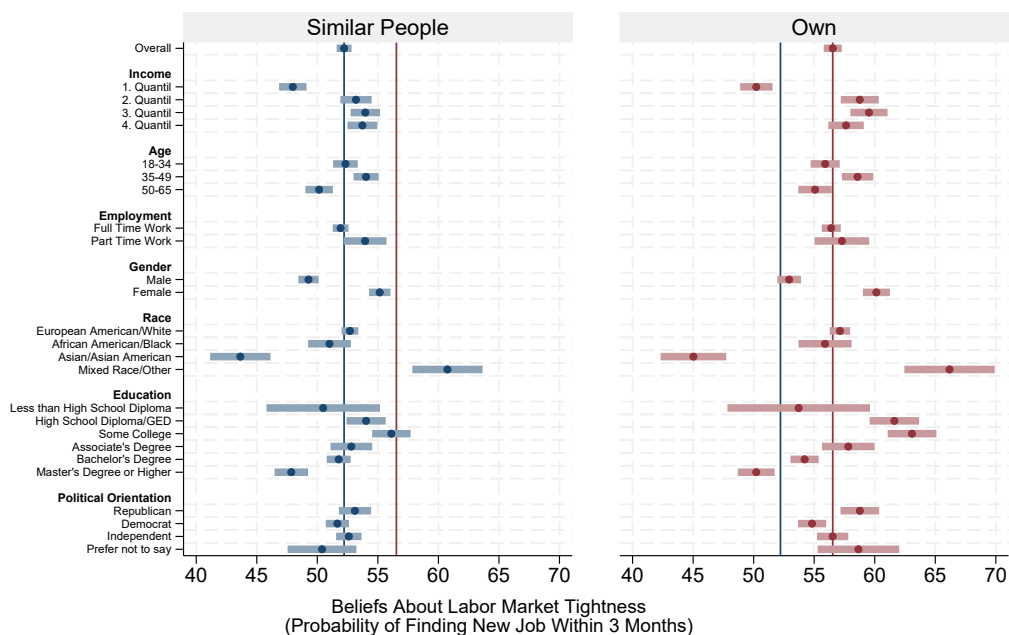


Figure 3.A.10: Workers' Beliefs About Own vs. Similar People's Job Finding Probability

*Note:* This figure shows the average prior beliefs about their own job finding probability and their beliefs about this probability for workers similar to them along several dimensions relevant to the job market, by subgroups based on socioeconomic characteristics. We ask them to think of workers employed in the same occupation and industry, who work in the same state, have the same level of education, and the same age as the respondent. For more details, please see Appendix Section 3.A.10. The blue line corresponds to the average beliefs of the job finding probability for workers similar to themselves. The red line corresponds to the average beliefs of their own job finding probability.

### 3.A.8 Treatment Effect Heterogeneity

Table 3.A.7: Alternative Mechanisms: Experimental Evidence on Labor Market Tightness and Workers' Beliefs About Effectiveness of Unions, Profit Growth and Union Support Among Colleagues

	(1)	(2)	(3)	(4)	(5)
	Wage Premium	Working Condition Premium	Job Security Premium	Employer Profits	Voteshare Colleagues
Labor Market Tightness Belief	0.0363 (0.0299)	0.0246 (0.0485)	-0.0162 (0.0336)	-0.0029 (0.0310)	0.0658 (0.0443)
Pre-Treat Estimation Error	-0.0174 (0.0120)	-0.0088 (0.0194)	-0.0135 (0.0146)	0.0075 (0.0135)	0.0343* (0.0184)
Observations	5,510	5,510	5,510	5,510	5,510

*Note:* This table shows estimates based on Equation 3.3. *Labor Market Tightness Belief* denotes respondents' estimate of the probability that someone like them, but who lost their job, would find a new job within 3 months. The outcome variables in Columns (1) - (3) are based on survey questions that ask respondents for their beliefs about the respective union premium. The outcome variable in column (4) is based on respondents beliefs about growth of their employer's profits within the next 12 months. The outcome in the final column measures workers' beliefs about the share of their colleagues that would for a union if an election was held at their establishment. We control for gender, age group, income group, race, union membership education, state, industry, and political orientation. We report robust standard errors in parentheses.

Table 3.A.5: Robustness of Beliefs about Labor Market Tightness on Intended Union Activity and Job Search

	Full Sample		Attention		Political Bias		Estimate certainty		Perceived job search advantage		
	(1)	(2) High	(3) Low	(4) Non	(5) Some	(6) Certain	(7) Uncertain	(8) Worse	(9) Equal	(10) Better	
<b><u>Panel A: Outcome Union Activity Index</u></b>											
Labor Market Tightness Belief	0.0040** (0.0017)	0.0046** (0.0022)	0.0024 (0.0026)	0.0039** (0.0018)	0.0060 (0.0042)	0.0041** (0.0020)	0.0047 (0.0030)	0.0018 (0.0023)	0.0097*** (0.0036)	0.0047 (0.0031)	
Pre-Treat Estimation Error	0.0020*** (0.0007)	0.0026*** (0.0009)	0.0009 (0.0010)	0.0017** (0.0007)	0.0022 (0.0017)	0.0020** (0.0008)	0.0012 (0.0011)	0.0005 (0.0009)	-0.0002 (0.0015)	0.0050*** (0.0012)	
Constant	-0.2160 (0.1506)	0.0117 (0.1789)	-0.7670*** (0.2763)	-0.1636 (0.1662)	-0.2891 (0.5403)	-0.3311 (0.2132)	-0.0978 (0.2105)	-0.1275 (0.2394)	0.0289 (0.2618)	-0.6793*** (0.3168)	
<b><u>Panel B: Outcome Job Search</u></b>											
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0066*** (0.0021)	0.0052* (0.0027)	0.0068*** (0.0018)	0.0055 (0.0042)	0.0078*** (0.0019)	0.0017 (0.0032)	0.0034 (0.0023)	0.0122*** (0.0037)	0.0056* (0.0030)	
Pre-Treat Estimation Error	0.0014** (0.0007)	0.0018** (0.0009)	0.0005 (0.0011)	0.0007 (0.0007)	0.0034** (0.0017)	0.0010 (0.0008)	0.0023* (0.0012)	0.0017* (0.0010)	-0.0018 (0.0016)	0.0030*** (0.0012)	
Constant	-0.1441 (0.1476)	0.0782 (0.1693)	-0.4723* (0.2771)	-0.0723 (0.1641)	-0.6147 (0.4100)	-0.3942* (0.2077)	0.2952 (0.2282)	-0.1662 (0.2415)	-0.0883 (0.2410)	-0.1980 (0.3336)	
Observations	5,510	3,544	1,966	3,968	1,542	3,812	1,698	2,594	1,180	1,736	

*Note:* This table reports robustness checks of union activity and job search, based on IV specifications, using respondent's beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. Panel A reports the results for the standardized union activity index as the outcome, accounting for *Pro-Union Vote*, *Pro-Union Vote Public*, *Organize Election* and *Willing to Strike*. Panel B reports the results for the standardized job search intention measure as the outcome. Column (2) and Column (3) are based on a sample split of respondents, passing both attention checks (Column 2) and the remaining respondents (Column 3). Column (4) and Column (5) are based on a sample split of respondents who perceived no political bias in the survey (Column 4) and respondents who perceived at least some left- or right-leaning political bias (Column 5). Column (6) and Column (7) are based on a sample split, respondents' certainty of their prior labor market tightness belief estimates. Column (6) contains respondents who are at least somewhat sure about their estimate, compared to the rest (Column 7). Columns (8) to (10) are based on a sample split of respondents' beliefs about their own job search (dis)advantage, relative to others. We calculated this variable using their likelihood of finding a new job within three months minus their beliefs of others' likelihood of finding a new job within three months. A positive value implies, that respondents believe they are more likely to find a new job (Column 8). A zero implies that individuals do not think that they are different from others in their beliefs of finding a new job within three months (Column 9). A negative value implies that respondents believe they are less likely to find a new job within three months relative to others (Column 10). We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.

Table 3.A.6: Experimental Evidence on Effects of Beliefs about Labor Market Tightness on Intended Union Activity, Job Search and Beliefs About Management Opposition (Version Without Control Variables)

	First Stage			Union Activity			Job Search			Mechanism	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: OLS	Post Estimation Error	Post Tightness Belief	Pre-Union Vote	Pre-Union Vote Public	Organize Election	Willing to Strike	Union Activity Index	Job Search Intention	Layoff Concerns	Management Opposition	Firing Probability
Treated x Pre-Treat Estimation Error	-0.5582*** (0.0174)	-0.5748*** (0.0218)	-0.0516* (0.0300)	-0.0555* (0.0303)	-0.0490* (0.0291)	-0.0388 (0.0283)	-0.0020** (0.0010)	-0.1049*** (0.0296)	0.0045** (0.0018)	0.0001 (0.0017)	0.0388 (0.0302)
Treated	-2.3968*** (0.3547)	-2.2679*** (0.4758)	-0.7376 (0.7076)	-0.8786 (0.8170)	-0.3175 (0.7390)	0.4504 (0.7744)	-0.0148 (0.0268)	-0.1885 (0.7980)	0.0012 (0.0481)	-0.0001 (0.0450)	-0.0914 (0.7891)
Pre-Treat Estimation Error	0.8991*** (0.0086)	0.5610*** (0.0130)	0.1117*** (0.0225)	0.1442*** (0.0224)	0.0226 (0.0214)	0.0326 (0.0206)	0.0032*** (0.0008)	0.1239*** (0.0216)	-0.0114*** (0.0013)	-0.0011 (0.0012)	0.0127 (0.0211)
Constant	-0.5749*** (0.2197)	50.7782*** (0.3205)	32.4424*** (0.5800)	32.8230*** (0.5800)	35.7585*** (0.5614)	31.3454*** (0.5508)	0.0060 (0.0193)	39.6547*** (0.5662)	4.3607*** (0.0336)	4.7536*** (0.0314)	36.3729*** (0.5580)
height											
Panel B: IV											
Labor Market Tightness Belief			0.0956* (0.0529)	0.1038** (0.0522)	0.0877* (0.0505)	0.0609 (0.0492)	0.0036** (0.0018)	0.1799*** (0.0508)	-0.0076** (0.0031)	0.0000 (0.0029)	-0.0648 (0.0327)
Pre-Treat Estimation Error			0.0597*** (0.0203)	0.0889*** (0.0198)	-0.0256 (0.0190)	-0.0034 (0.0188)	0.0012* (0.0007)	0.0223 (0.0092)	-0.0071*** (0.0012)	-0.0011 (0.0012)	0.0498** (0.0213)
Constant			47.3283*** (2.6398)	27.2327*** (2.6138)	31.2964*** (2.5176)	28.7471*** (2.4638)	-0.1797** (0.0875)	30.6278*** (2.5392)	4.9409*** (0.1545)	4.7488*** (0.1460)	39.5459*** (2.6509)
Observations	5,510	5,510	5,510	5,510	5,510	5,510	5,510	5,510	5,510	5,510	5,510
Average of Outcome Control Group	0.29	51.32	52.55	32.96	35.78	31.58	0.01	39.77	4.55	4.75	36.39

**Note:** This table reports results based on the information experiment. Panel A reports OLS coefficients, where “Treated” is a binary variable indicating whether the respondent was part of the treatment group and “Pre-Treat Estimation Error” denotes the difference between the respondent’s estimate of the probability that someone like them, but who lost their job, would find a new job within 3 months and the probability predicted by our machine learning model. Panel B presents IV specifications, using respondent’s beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. See Section 2.3, and in particular Equation 2 for more details. The outcome variables in Column (3) - Column (6) are based on the following survey questions. *Pro-Union Vote*: “If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would vote for a union?”; *Pro-Union Vote Public*: “If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would make your vote public?”; *Organize Election*: “What is the percent chance that you would help organize a union election at your establishment in the coming weeks?”; *Willing to Strike*: “What is the percent chance that you would participate in any strike-related activities?”. The outcome variable in Column (7) combines the centered measures of union activity in a standardized index measure. The outcome variables in Column (8) - (11) are based on the followin survey questions. *Job Search Intention*: “Over the next 12 months, what is the percent chance that you will look for a new job at a different company?”; *Layoff Concerns*: “Suppose you were to lose your job this month. How worried would you be about finding a new one?”; *Management Opposition*: “How strongly do you think your management would oppose any type of union activity by you and your colleagues?”; *Firing Probability*: “What do you think is the percent chance that your management would fire you if they found out you support any union activities?”. We report robust standard errors in parentheses.

Table 3.A.8: Heterogeneity Analysis based on Socioeconomic Characteristics on Intended Union Activity and Job Search

Full Sample	Sex		Education		Income		Age		Political Orientation			Race					
	(1) Full sample	(2) Male	(3) Female	(4) No College	(5) College	(6) High	(7) Low	(8) 18 to 34	(9) 35 to 49	(10) 50 to 65	(11) Republican	(12) Democrat	(13) Independent	(14) White	(15) Black	(16) Asian	(17) Mixed Race/ Others
Panel A: Outcome Union Activity Index																	
Labor Market Tightness Belief	0.0040*** (0.0017)	0.0066** (0.0028)	0.0018 (0.0020)	0.0009 (0.0024)	0.0073*** (0.0023)	0.0036 (0.0023)	0.0043* (0.0024)	0.0050* (0.0027)	0.0041 (0.0027)	0.0020 (0.0032)	0.0021 (0.0035)	0.0086*** (0.0025)	-0.0021 (0.0034)	0.0022 (0.0019)	0.0123*** (0.0040)	0.0105 (0.0105)	0.0060 (0.0059)
Pre-Treat Estimation Error	0.0020*** (0.0007)	0.0035*** (0.0011)	0.0001 (0.0008)	0.0004 (0.0010)	0.0024*** (0.0009)	0.0014 (0.0009)	0.0026*** (0.0010)	0.0018* (0.0010)	0.0017 (0.0011)	0.0026* (0.0014)	0.0006 (0.0014)	0.0011 (0.0010)	0.0049*** (0.0013)	0.0025*** (0.0007)	-0.0010 (0.0018)	0.0011 (0.0044)	-0.0005 (0.0032)
Constant	-0.2160 (0.1506)	-0.1659 (0.1795)	-0.9450*** (0.2677)	-0.1979 (0.2152)	-0.3948** (0.1729)	0.3838 (0.3945)	-0.5402*** (0.1695)	-0.3878 (0.2826)	-0.4366 (0.3405)	-0.3735* (0.1913)	0.2131 (0.2520)	-0.4848** (0.2293)	-0.0147 (0.3876)	-0.0193 (0.1659)	-1.1380** (0.5158)	0.1284 (0.9898)	0.5911 (0.7807)
Panel B: Outcome Job Search																	
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0070*** (0.0027)	0.0051** (0.0021)	0.0021 (0.0024)	0.0098*** (0.0022)	0.0073*** (0.0022)	0.0048* (0.0025)	0.0048* (0.0028)	0.0049* (0.0027)	0.0097*** (0.0030)	0.0102*** (0.0035)	0.0079*** (0.0024)	0.0017 (0.0033)	0.0061*** (0.0019)	0.0068* (0.0041)	0.0073 (0.0091)	0.0028 (0.0067)
Pre-Treat Estimation Error	0.0014** (0.0007)	0.0021** (0.0010)	0.0006 (0.0009)	0.0003 (0.0010)	0.0014* (0.0009)	0.0003 (0.0009)	0.0022** (0.0010)	0.0015 (0.0011)	0.0002 (0.0011)	0.0008 (0.0013)	-0.0010 (0.0016)	0.0009 (0.0010)	0.0039*** (0.0012)	0.0011 (0.0007)	0.0010 (0.0019)	0.0036 (0.0041)	-0.0008 (0.0038)
Constant	-0.1422 (0.1476)	-0.2509 (0.1690)	-0.1517 (0.3026)	0.0944 (0.2061)	-0.5492*** (0.1676)	-0.5913 (0.4238)	-0.1135 (0.1688)	0.0338 (0.3209)	-0.7569** (0.3445)	-0.3880** (0.1709)	-0.3139 (0.2558)	-0.4890* (0.2537)	0.5740 (0.3815)	-0.0524 (0.1617)	-0.2190 (0.5816)	-0.5882 (0.8734)	0.2058 (1.0415)
Observations	5,510	2,756	2,754	1,856	3,654	2,746	2,764	1,920	1,830	1,721	1,901	2,125	1,738	4,191	693	382	244

*Note:* This table reports robustness checks of union activity and job search, based on IV specifications, using respondent's beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. Panel A reports the results for the standardized union activity index as the outcome, accounting for *Pro-Union Vote*, *Pro-Union Vote Public*, *Organize Election* and *Willing to Strike*. Panel B reports the results for the standardized job search intention measure as the outcome. Column (2) to Column (3) are based on a sample split of sex. Column (4) to Column (5) are based on a sample split of respondents education. Column (6) to Column (7) are based on a sample split of the median income. Column (8) and Column (10) are based on a sample splits of age. Column (11) to Column (13) are based on a sample split of respondents stated political orientation. Column (14) to Column (17) are based on a sample split of respondents race. We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.

Table 3.A.9: Heterogeneity Analysis based on Union Impact Beliefs on Intended Union Activity and Job Search

	Full Sample (1)	Union Wage Effect			Union Working Conditions Effect			Union Job Loss Effect		
		(2) Increase	(3) None	(4) Decrease	(5) Improve	(6) None	(7) Decline	(8) Increase	(9) None	(10) Decrease
Panel A: Outcome Union Activity Index										
Labor Market Tightness Belief	0.0040** (0.0017)	0.0067*** (0.0022)	-0.0030 (0.0032)	0.0054 (0.0034)	0.0063*** (0.0024)	0.0005 (0.0026)	0.0019 (0.0035)	0.0024 (0.0037)	0.0022 (0.0021)	0.0106*** (0.0031)
Pre-Treat Estimation Error	0.0020*** (0.0007)	0.0026*** (0.0009)	0.0026* (0.0015)	0.0005 (0.0013)	0.0028*** (0.0010)	0.0011 (0.0011)	0.0023* (0.0014)	0.0022 (0.0014)	0.0015 (0.0009)	0.0022* (0.0012)
Constant	-0.2160 (0.1506)	-0.4647** (0.2020)	0.2800 (0.2720)	-0.1012 (0.3513)	-0.4299** (0.1906)	-0.2261 (0.2833)	-0.0268 (0.2973)	-0.8506*** (0.2854)	0.1431 (0.2522)	-0.6925*** (0.2737)
Panel B: Outcome Job Search										
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0062*** (0.0022)	0.0045 (0.0033)	0.0101*** (0.0037)	0.0028 (0.0024)	0.0094*** (0.0029)	0.0100*** (0.0037)	0.0051 (0.0037)	0.0082*** (0.0022)	0.0048 (0.0031)
Pre-Treat Estimation Error	0.0014** (0.0007)	0.0013 (0.0009)	0.0016 (0.0015)	0.0014 (0.0014)	0.0023** (0.0010)	0.0019* (0.0011)	0.0001 (0.0014)	0.0024* (0.0014)	0.0009 (0.0010)	0.0029*** (0.0012)
Constant	-0.1441 (0.1476)	-0.3061 (0.2099)	0.5147** (0.2609)	-0.6387* (0.3490)	-0.1074 (0.1921)	-0.1246 (0.3186)	-0.2482 (0.3341)	-0.4918* (0.2660)	0.0339 (0.2375)	-0.2281 (0.2899)
Observations	5,510	2,917	1,142	1,451	2,722	1,369	1,419	1,547	2,136	1,827

*Note:* This table reports robustness checks of union activity and job search, based on IV specifications, using respondent's beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. Panel A reports the results for the standardized union activity index as the outcome, accounting for *Pro-Union Vote*, *Pro-Union Vote Public*, *Organize Election* and *Willing to Strike*. Panel B reports the results for the standardized job search intention measure as the outcome. Column (2) to Column (4) are based on a sample split of respondents beliefs about unions impact on wages. Column (5) to Column (7) are based on a sample split of respondents beliefs about unions impact on working conditions. Column (8) to Column (10) are based on a sample split of respondents beliefs about unions impact on job losses. We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.

Table 3.A.10: Heterogeneity Analysis based on Workplace Characteristics on Intended Union Activity and Job Search

	Full Sample		Tenure		Management Relationship		Working Conditions		Vote Share Colleagues		Firm Size	
	(1) Full sample	(2) Long	(3) Short	(4) Good	(5) Not good	(6) Good	(7) Not good	(8) High	(9) Low	(10) Large	(11) Small	
<b>Panel A: Outcome Union Activity Index</b>												
Labor Market Tightness Belief	0.0040** (0.0017)	0.0044** (0.0022)	0.0041* (0.0025)	0.0042** (0.0020)	0.0048 (0.0030)	0.0041** (0.0019)	0.0044 (0.0032)	0.0047** (0.0023)	0.0006 (0.0019)	0.0062*** (0.0023)	0.0020 (0.0024)	
Pre-Treat Estimation Error	0.0020*** (0.0007)	0.0000 (0.0009)	0.0035*** (0.0009)	0.0028*** (0.0008)	-0.0000 (0.0013)	0.0020*** (0.0008)	0.0017 (0.0013)	0.0024*** (0.0009)	0.0001 (0.0008)	0.0010 (0.0009)	0.0028*** (0.0010)	
Constant	-0.2160 (0.1506)	-0.4625 (0.2907)	-0.3175* (0.1863)	-0.4404** (0.1823)	0.0980 (0.2491)	-0.4804*** (0.1802)	0.1217 (0.2696)	-0.1688 (0.1697)	-0.6259** (0.2482)	-0.2958 (0.3381)	-0.1211 (0.1813)	
<b>Panel B: Outcome Job Search</b>												
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0091*** (0.0021)	0.0025 (0.0026)	0.0052*** (0.0019)	0.0076*** (0.0029)	0.0054*** (0.0019)	0.0058* (0.0031)	0.0053** (0.0025)	0.0064*** (0.0022)	0.0070*** (0.0023)	0.0050*** (0.0024)	
Pre-Treat Estimation Error	0.0014** (0.0007)	-0.0008 (0.0009)	0.0038*** (0.0010)	0.0015** (0.0008)	0.0016 (0.0013)	0.0010 (0.0008)	0.0035*** (0.0013)	0.0012 (0.0009)	0.0006 (0.0009)	0.0001 (0.0010)	0.0024*** (0.0010)	
Constant	-0.1441 (0.1476)	-1.0260*** (0.2675)	0.0986 (0.1820)	-0.1108 (0.1828)	-0.1587 (0.2439)	-0.1910 (0.1830)	-0.0272 (0.2621)	-0.0736 (0.1839)	-0.4527* (0.2597)	0.0763 (0.4200)	-0.1382 (0.1741)	
Observations	5,510	2,742	2,768	3,970	1,540	4,005	1,505	2,733	2,777	2,183	3,327	

*Note:* This table reports robustness checks of union activity and job search, based on IV specifications, using respondent's beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. Panel A reports the results for the standardized union activity index as the outcome, accounting for *Pro-Union Vote*, *Pro-Union Vote Public*, *Organize Election* and *Willing to Strike*. Panel B reports the results for the standardized job search intention measure as the outcome. Column (2) to Column (3) are based on a sample split of the median job tenure (33 months). Column (4) to Column (5) are based on a sample split of respondents stated relationship with management. Column (6) to Column (7) are based on a sample split of respondents stated working conditions. Column (8) and Column (9) are based on a sample split of the median respondents beliefs about the share of colleagues voting for the union (44%). Column (10) to Column (11) are based on a sample split of firm size, classifying firms with at least 250 employees as large firms. We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.



Table 3.A.11: Heterogeneity Analysis based on Economic Expectations on Intended Union Activity and Job Search

	Full Sample		Outside Option Perceptions		Unemployment Expectations		Inflation Expectations		Profit Expectations		
	(1)	(2)	(3)		(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Overestimated	Underestimated		Increase Likely	Increase Unlikely	Inflation	Deflation	Increase	Unchanged	Decrease
<b>Panel A: Outcome Union Activity Index</b>											
Labor Market Tightness Belief	0.0040** (0.0017)	0.0043** (0.0020)	0.0038 (0.0029)		0.0017 (0.0023)	0.0059** (0.0024)	0.0039** (0.0017)	0.0080 (0.0051)	0.0058** (0.0028)	0.0042* (0.0022)	0.0012 (0.0058)
Pre-Treat Estimation Error	0.0020*** (0.0007)	-0.0033*** (0.0011)	0.0080*** (0.0015)		0.0013 (0.0009)	0.0015 (0.0010)	0.0015** (0.0007)	0.0021 (0.0019)	0.0008 (0.0011)	0.0024*** (0.0009)	0.0011 (0.0022)
Constant	-0.2160 (0.1506)	0.1714 (0.2157)	-0.5407** (0.2130)		0.1585 (0.1990)	-0.6917*** (0.2395)	-0.1503 (0.1627)	-0.4560 (0.3740)	-0.8018*** (0.2544)	-0.2660 (0.2697)	-0.0350 (0.3477)
<b>Panel B: Outcome Job Search</b>											
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0066*** (0.0020)	0.0063** (0.0028)		0.0048** (0.0023)	0.0059** (0.0023)	0.0054*** (0.0017)	0.0111** (0.0047)	0.0027 (0.0029)	0.0076*** (0.0021)	0.0069 (0.0058)
Pre-Treat Estimation Error	0.0014** (0.0007)	-0.0017 (0.0011)	0.0032** (0.0015)		0.0003 (0.0009)	0.0018* (0.0010)	0.0011 (0.0007)	0.0017 (0.0019)	0.0018 (0.0012)	0.0008 (0.0009)	0.0032 (0.0023)
Constant	-0.1441 (0.1476)	0.0285 (0.2156)	-0.3393 (0.2154)		0.1124 (0.1943)	-0.3977* (0.2405)	-0.0916 (0.1579)	-0.1116 (0.4614)	-0.2436 (0.2753)	-0.3884 (0.2695)	0.1744 (0.2918)
Observations	5,510	2,738	2,682		2,622	2,888	4,400	1,105	1,835	2,885	790

*Note:* This table reports robustness checks of union activity and job search, based on IV specifications, using respondent's beliefs about labor market tightness (measured by the probability of finding a new job within three months) as the endogenous variable. Panel A reports the results for the standardized union activity index as the outcome, accounting for *Pro-Union Vote*, *Pro-Union Vote Public*, *Organize Election* and *Willing to Strike*. Panel B reports the results for the standardized job search intention measure as the outcome. Column (2) to Column (3) are based on a sample split of respondents prior misperceptions about their outside options. Column (4) to Column (5) are based on a sample split of the median stated beliefs about the likelihood of an increase in regional unemployment over the next 12 months (45%). Column (6) to Column (7) are based on a sample split of respondents beliefs about whether prices will increase or decrease over the next 12 months. Column (8) to Column (10) are based on a sample split of respondents beliefs whether firm profits will increase, stay the same or will decrease over the next 12 months. We include gender, age group, income group, race, union membership, education, census division, industry, and political orientation as controls and report robust standard errors in parentheses.

### 3.A.9 Quasi-Experimental Results

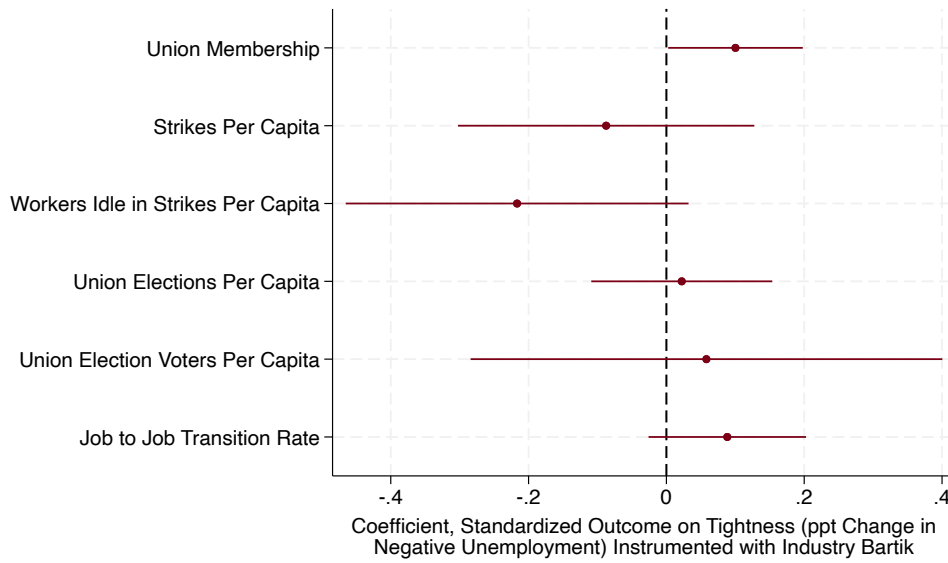


Figure 3.A.11: Bartik Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

*Note:* This figure presents the effects of labor market tightness, measured as the negative unemployment rate and instrumented with the canonical Bartik instrument following Bartik (1991), on changes in several measures of union activity and job-to-job transitions. An example for reading this figure would be that a one percentage point decrease in the unemployment rate, i.e., an increase in tightness, corresponds to an about 0.1SD increase the union membership rate. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. Union membership data and job-to-job transitions are constructed at the state-level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Strikes and workers idle per strike are constructed at the state-level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description. Job-to-job transitions are only asked about in the CPS beginning in 1994 and the corresponding time series data constructed from this data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). All standard errors clustered at the state level.

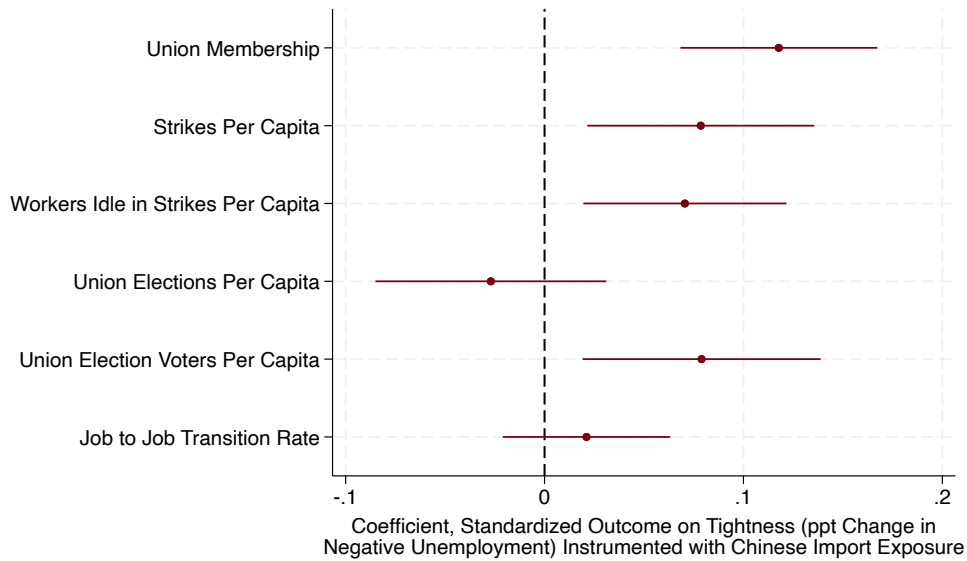


Figure 3.A.12: China Shock Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

*Note:* This figure presents effects of the China shock on unionization outcomes, through its effects on changes in labor market tightness that we measure as the negative unemployment rate. An example for reading this figure would be that a one percentage point decrease in the unemployment rate, i.e., an increase in tightness, is associated with an about 0.12SD increase in the union membership rate. The China shock is estimated as in Acemoglu et al. (2016) (AADHP) using long differences between 1991 and 1999 and 1999 and 2011 and with local labor markets defined as commuting zones. Following AADHP, we annualize all the changes, dividing by 12 for the 1999–2011 long difference and 8 for the 1991–1999 long difference. Standard errors are clustered at the commuting zone level. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. Union membership data and job-to-job transitions are constructed at the commuting zone level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Job-to-job transitions are only asked about in the CPS beginning in 1994, so are available for only one of the two long differences (1999 to 2011). The time series of job-to-job transitions constructed from CPS data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). Strikes and workers idle per strike are constructed at the commuting zone level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description.

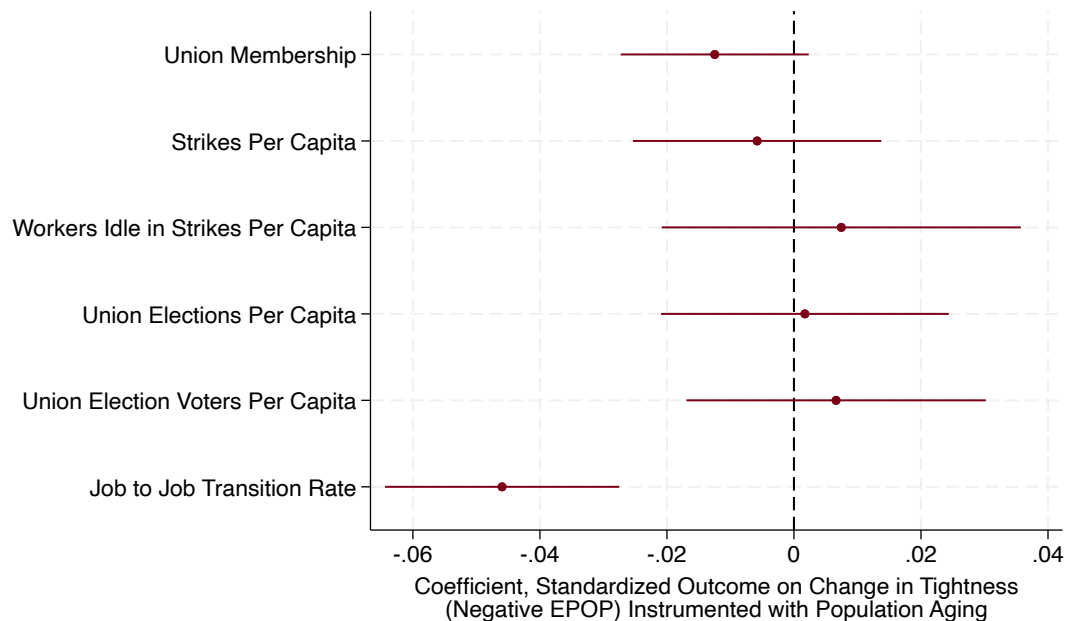


Figure 3.A.13: Demographic Change Results: Effects of Shocks to Labor Scarcity on Union Activity and Job-to-Job Transitions

*Note:* This figure presents effects of labor supply shocks from demographic change on unionization outcomes at the county level. An example for reading this figure would be that a one percentage point decrease in the negative employment to population ratio, i.e., an increase in scarcity, is associated with an about 0.013SD decrease in union membership. We construct an instrument for labor supply shocks using the age structure of the population. We exploit predictable variation in the share of the population that will be of working age in the future. Specifically, we use two instruments: the share of the population that is between the ages of 10 and 19, who we call “future joiners,” and the share of the population between the ages of 55 and 64, who we call “future retirees.” We then run long difference specifications, looking at the change in union outcomes over 10-year periods, as above, and the change in labor supply driven by predictable features of the cohort structure. A detailed description of the construction and data collection for the above analyses can be reviewed in Section 3.6. Union membership data and job-to-job transitions are constructed at the county level using the CPS. CPS respondents in 400 counties and approximately 200 metro areas have their location disclosed, which amounts to about 70% of the sample. For the remaining 30% of respondents, we assign them to the non-disclosed counties in the state with probabilities corresponding to county population shares. Strikes and workers idle per strike are constructed at the county level using data from the FMCS, a federal agency that offers mediation services during strikes. They publish data on 14,000 strikes between 1984 and 2020. Forest Gregg has collected them here: <https://github.com/labordata/fmcs-work-stoppage>. Union elections and voters in union elections use data from the NLRB as compiled and geocoded by Wang and Young (2022); see their paper for a further description. EPOP is missing for 7 counties around New Orleans in 2005 and 2006 due to Hurricane Katrina, affecting 28 long differences. Job-to-job transitions are only asked about in the CPS beginning in 1994 and the corresponding time series data constructed from this data reveals a decline in transitions, but this is due in part to a change in the CPS sampling procedure, see Fujita et al. (2020). Standard errors are clustered at the county level.

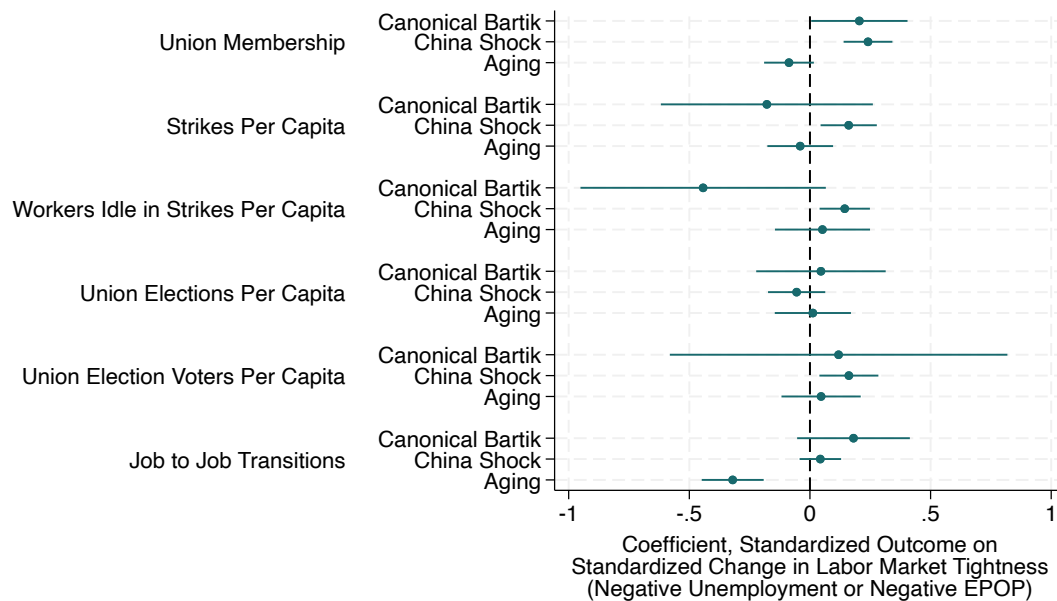


Figure 3.A.14: Evidence from Three Quasi-Experimental Designs: Effects of Shocks to Labor Scarcity on Union Activity and Job-to-Job Transitions

*Note:* This figure presents estimates from three different quasi-experimental designs that capture the effects of labor market tightness or scarcity on union activity outcomes and job-to-job transitions. We report effects of standardized changes in tightness (measured as the negative unemployment rate in the canonical bartik and china shock approaches, and as the negative employment to population ratio in the demographic change design) on standardized measures of unionization activity and job transitions. For a detailed description of the designs, please see Section 3.6 and the figure notes below Figure 3.A.11, Figure 3.A.12 and Figure 3.A.13.

### 3.A.10 Questionnaire

#### *Consent, Screening and Background of Respondents*

- Welcome, and thank you for your interest in completing this survey! This survey is conducted for academic research purposes only, and the research is non-partisan. The research will be based on this survey, which will take approximately 15 minutes to complete. You should know the following:

- Whether you take part is up to you. Your participation is completely voluntary, and you can choose not to take part.
- You can also agree to take part and later change your mind. Your decision will not be held against you.

Should you decide to participate, please keep in mind:

- You will have the opportunity to earn bonus money, depending on how much effort you put into your answers.
- If you complete the survey, you will also be automatically enrolled in a lottery to win \$500.
- All the answers you provide will remain anonymous and will be treated with absolute confidentiality.
- If you have questions, concerns, or complaints, please contact the research team at [pezold@iza.org](mailto:pezold@iza.org).

Do you agree to participate in this survey?

- What is your current employment status?
- Are you employed by either the federal, state or local government?
- Where do you currently live?
- How old are you?
- Which state do you work in?
- How would you describe your ethnicity/race?
- What is your sex?
- What was your total pre-tax wage and salary income over the last 12 months? This covers wages, salaries, commissions, cash bonuses, tips, and other money income received from your employer.
- What is the highest level of education you have completed?
- *Attention Check 1:* Most modern theories of decision-making recognize that decisions do not take place in a vacuum. Individual preferences and knowledge, along with situational variables, can greatly impact the decision process. To demonstrate that you've read this much, please select both strongly agree and strongly disagree below. Please tell us whether you agree or disagree with the following statement: "The U.S. economy is in good shape right now."
- The next questions ask about your main job. By main job, we mean the one at

which you usually work the most hours.

Which broad occupation category below best describes the occupation you work in?

- Which broad occupation category below best describes the occupation you work in?
- *If management occupation:* Are you a c-level manager or higher ranking supervisor who works together closely with the management of your firm?
- Which broad industry group do you work in?

#### *Expectations about the National and Local Economy*

- Over the next 12 months, do you think that, on average, prices will be falling or increasing?
- By how much do you think prices will [increase/fall] in the next 12 months (in %)?
- How likely do you think it is that one year from now, the unemployment rate in [states] will be higher than it is now? (Note that 0 would be very unlikely and 100 would be very likely)
- *Attention Check 2:* The next question addresses the following problem. In surveys like this one, there are sometimes participants who don't read the questions carefully and just click through the questionnaire quickly. As a result, there are many random answers that falsify the results of the study. To show that you are reading our questions carefully, we ask that you select both very interested and somewhat interested below.

How interested are you in news about the U.S. economy?

#### *Pre-Treatment Beliefs about Labor Market Tightness*

- Suppose you were to lose your job this month. What do you think is the percent chance that within the coming 3 months, you will find a new job that you will accept?
- To reward your effort in taking this survey, we will award an extra payment to the 100 respondents whose answer to the following question is closest to the true answer.
- Now imagine workers who are very similar to you when it comes to the characteristics listed below. In particular, imagine people who...
  - Work in [occupation]
  - Work in the [industry]
  - Work in [state]
  - Have the same highest educational level as you do ([education])
  - Are [age] years old.

Please answer the following important question with people in mind who are similar to you based on the above characteristics!

- Based on the most recent data from the U.S. Census Bureau, we calculated the share of people like you (but who lost their job) that found a new job within 3 months that they are willing to accept.

We ask you for your best guess of this share on the next page: if your estimate is among the 100 respondents whose answer is closest to the truth, you will receive an extra payment for your effort.

- This question is really important, and you have the chance to earn bonus money. Note that you will be asked to explain how you arrived at your estimate. Please give us your best guess!

What share of people like you (but who lost their job) will find a new job within 3 months that they are willing to accept?

- How sure are you about your estimate in the previous questions?

#### *Treatment Block*

- On the following pages you will receive information about the labor market opportunities for people like you - please read them very carefully!
- You estimated that [X]% of people like you (but who lost their job) would find a new job within 3 months.

Based on the most recent U.S. Census Data, we calculated that [X]% of people like you found a new job within 3 months.

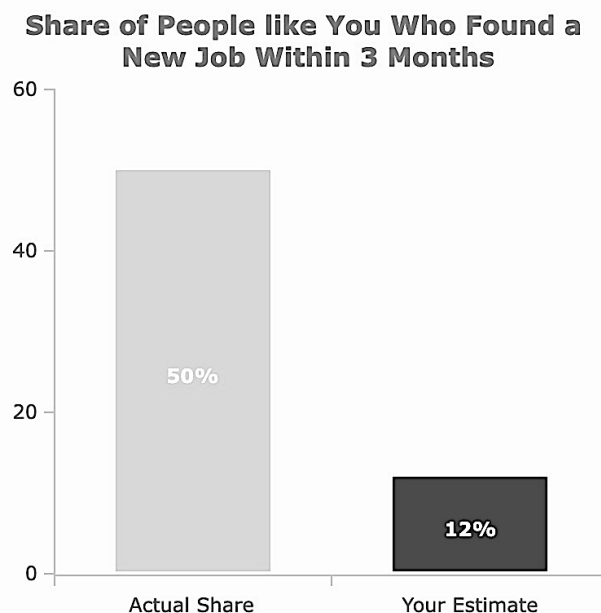


Figure 3.A.15: Visualization of Information Treatment

*Note:* This figure shows an example of the personalized information provided to respondents in the treatment group. The left bar represents the estimated share of people that found a new job within 3 months after layoff based on the U.S. Current Population survey. The right bar shows the person's own estimate of that same object.

- So, compared to what you thought, the share of people like you who found a new job within 3 months after losing their job is actually [greater/smaller] - it is [X]%! Please review this information carefully, as you will not be able to go back.



- Did you over- or underestimate the share of people like you (but who lost their job) that found a new job within three months?
- You estimated that [X]% of people like you (but who lost their job) would find a new job within 3 months.

Based on the most recent U.S. Census Data, we calculated that [X]% of people like you found a new job within 3 months.

By how much did you [underestimate/overestimate] the share of people like you who would find a new job within 3 months?

#### *Control Block*

- You estimated that [X]% of people like you will find a new job within 3 months that they are willing to accept. - please read them very carefully!

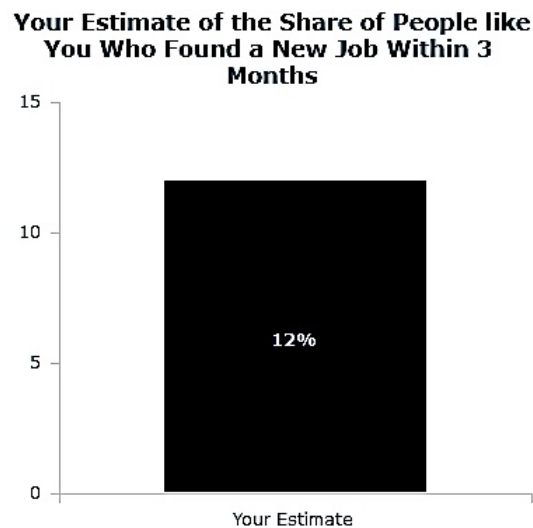


Figure 3.A.16: Visualization of the Control Group

*Note:* This figure shows an example of the personalized information provided to respondents in the control group. The bar shows the person's own estimate of that same object.

#### *Post-Treatment Beliefs about Labor Market Tightness*

- Now that you had a little more time to think about this question:  
What share of people like you (but who lost their job) will find a new job within 3 months that they are willing to accept?

#### *Outcomes*

- If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would vote for a union?

- If an election were held today to decide whether employees like you should be represented by a union, what is the percent chance that you would make your vote public?
- What is the percent chance that you would help organize a union election at your establishment in the near future?
- What is the percent chance that you would participate in any strike-related activities in the near future?
- Next, we invite you to draft a message to your co-workers for or against unionizing your workplace.

This is a great moment to take time to communicate your stance on this topic, as you have just thought a lot about union activity! You can take your time for writing the message - we know that this can easily take up to 4 minutes (or longer).

Of course, you can also click “Continue” if you do not want to draft a message.

- *Reminder:* If you want, you can share the message with your coworkers by copying it here and pasting it to social media, or any messenger, later. Importantly, please make sure the message is still contained in the text field below before you move on to the next question.

In case you would like to share your message with your co-workers, please make sure you copied the message from the text field. If you want to move on to the next page please click the “Continue” button again.

- Suppose you were to lose your job this month. How worried would you be about finding a new one?
- Over the next 12 months, what is the percent chance that you will look for a new job at a different company?
- By taking this survey, you are automatically enrolled in a lottery to win \$500. In a few days, you will know whether you won the \$500. The payment will be made to you in the same way as your regular survey pay, so no further action is required on your part.
- Are you interested in information about unionizing your workplace?

If you are, you can forfeit part of your gain (should you win the \$500 lottery) in exchange for this information. We will randomly assign a price between \$0 and \$10. If the price is lower than what you are willing to pay for this information in case you win the lottery, we will provide you with the information at the end of the survey.

Note: The information brochure is the result of careful research, and you cannot easily find the information online.

In case you win the \$500 lottery, how much are you maximally willing to pay (0-10\$) to receive detailed information about how to unionize your workplace?

- Are you interested in information about your legal rights in the case of employer opposition to union activity?

If you are, you can forfeit part of your gain (should you win the \$500 lottery) in

exchange for this information. We will randomly assign a price between \$0 and \$10. If the price is lower than what you are willing to pay for this information in case you win the lottery, we will provide you with the information at the end of the survey.

Note: The information brochure is the result of careful research, and you cannot easily find the information online.

In case you win the \$500 lottery, how much are you maximally willing to pay (0-10\$) to receive detailed information about your legal rights in the case of employer opposition to union activity?

- Are you interested in information about how to improve your job search?

If you are, you can forfeit part of your gain (should you win the \$500 lottery) in exchange for this information. We will randomly assign a price between \$0 and \$10. If the price is lower than what you are willing to pay for this information in case you win the lottery, we will provide you with the information at the end of the survey.

Note: The information brochure is the result of careful research, and you cannot easily find the information online.

In case you win the \$500 lottery, how much are you maximally willing to pay (0-10\$) to receive detailed information about how to improve your job search?

- Over the next 12 months, do you think that your employer's profits will be falling or increasing?
- By how much do you think your employer's profits will [fall/increase] in the next 12 months (in %)?

#### *Union Sentiment, Beliefs and Knowledge*

- Next, we would like you to think about your workplace management's attitude towards workers who support union activity.

How strongly do you think your management would oppose any type of union activity by you and your colleagues?

- What do you think is the percent chance that your management would fire you if they found out you support any union activities?
- To what extent do you agree or disagree with the following statement?

Being able to find a new job more quickly makes it easier for workers to negotiate with their current employers, as it increases their bargaining power.

- To what extent do you agree or disagree with the following statement?

When workers are able to find a new job more quickly, they are less concerned about management opposition to union activities, because it is easy to start working at a different firm in case their current employer retaliates.

- Are you a member of a labor union? Note that if "yes" is selected, we will ask you to specify what union you are a member of.
- *If a member of a labor union:* Please specify the union you are a member of in the text field below.

- At your workplace, are employees like you represented by a union?
- *If not represented by a union:* For the next few questions, please think ahead to 12 months from now, and suppose that you are working in the exact same job at your current workplace.

We will ask you to think about how the conditions at your workplace would develop in the coming year if it remains not unionized vs. if it were to unionize.

- *If not represented by a union:* A year from now, what is the percent chance that your employer would substantially improve working conditions other than pay (in %)?
  - Suppose your workplace remains not unionized, what is the percent chance of improvements in working conditions?
  - Suppose your workplace becomes unionized, what is the percent chance of improvements in working conditions?
- *If not represented by a union:* A year from now, by how much do you think your employer would change your pay (in %)? Note that a negative percent change reflects a deterioration in pay, and a positive percent change reflects an improvement.
  - Suppose your workplace remains not unionized, what % change in pay would you expect a year from now?
  - Suppose your workplace becomes unionized, what % change in pay would you expect a year from now?
- *If not represented by a union:* What do you think is the percent chance that you lose your job within the next 12 months?
  - Suppose your workplace remains not unionized, what is the percent chance that you lose your job within the next 12 months?
  - Suppose your workplace becomes unionized, what is the percent chance that you lose your job within the next 12 months?
- *If represented by a union:* For the next few questions, please think ahead to 12 months from now, and suppose that you are working in the exact same job at your current workplace.

We will ask you to think about how the conditions at your workplace would develop in the coming year if it remains unionized vs. if it were to become de-unionized.

- *If represented by a union:* A year from now, what is the percent chance that your employer would substantially improve working conditions other than pay (in %)?
  - Suppose your workplace remains unionized, what is the percent chance of improvements in working conditions?
  - Suppose your workplace becomes de-unionized, what is the percent chance of improvements in working conditions?
- *If represented by a union:* A year from now, by how much do you think your employer will have change your pay (in %)? Note that a negative percent change reflects a deterioration in pay, and a positive percent change reflects an improvement.

- Suppose your workplace remains unionized, what change in pay would you expect?
- Suppose your workplace becomes de-unionized, what change in pay would you expect?
- *If represented by a union:* What do you think is the percent chance that you lose your job within the next 12 months?
  - Suppose your workplace remains unionized, what is the percent chance that you lose your job within the next 12 months?
  - Suppose your workplace becomes de-unionized, what is the percent chance that you lose your job within the next 12 months?
- Please give us your best guess: What share of workers that work in the same industry as you are members of a union? (100% would mean you think that everyone in your industry is a member, and 0% means you think no one is a member)
- What do you think: What share of workers that work in the same occupation as you are members of a union? (100% would mean you think that everyone in your occupation is a member, and 0% means you think no one is a member)
- What would you say are the main goals of labor unions?
- Do you approve or disapprove of labor unions?
- According to a nationally representative survey, some people in the U.S. approve of labor unions, but are not part of one. Why do you think some people support unions, but hesitate to join one?

#### *Additional Background Questions*

- We're almost done! In the remaining questions, we would like to learn a little bit more about you and your workplace.
- What do you consider to be your political affiliation?
- Approximately how many employees are working at the establishment you are employed at?
- In general, how would you describe relations at your workplace between management and employees?
- In general, how would you describe the working conditions at your workplace?
- If an election were held today at your workplace to decide whether employees like you should be represented by a union, what share of your colleagues do you think would vote for a union?
- Has there been any unionization attempt of employees at your workplace in the last 24 months?
- For how long (in months) have you been employed at your current workplace?
- Did you switch jobs within the last 12 months?

*Survey Quality and Debrief*

- Do you feel this survey was politically biased?
- *If part of the treatment group:* At some point in the survey, we provided you with information about the share of people similar to you (but lost their job), who found a new job within 3 months, based on data by the U.S. Census Bureau. Did you find the information we provided you with trustworthy or untrustworthy?
- Do you have any remarks about this study that you would like to share with us?

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## Chapter 4

# Management Opposition in Times of Crisis



## Abstract

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I study how hiring discrimination against union supporters is affected by labor market conditions. While conventional economic theory predicts discrimination to decrease in tight labor markets, discrimination against union supporters could also increase due to unions' improved bargaining power and the threat of unionization. I sent 5,006 fictitious job applications revealing a union membership in the application. I show that discrimination is pro-cyclical in employment, increasing in an economic upturn and decreasing in an economic downturn. There is no effect of occupation-specific labor market tightness on discrimination. Cyclical discrimination is concentrated among firms without a collective agreement. These results suggest discrimination can be explained by unions' bargaining power and the threat of unionization. This indicates that the emerging labor shortage is unlikely to dampen managements' opposition to unionization.

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**JEL classification:** J23, J51, J53, J71, C93

**Keywords:** Trade Unions, Hiring Discrimination, Labor Shortage, Labor Demand, Labor Market Tightness, Field Experiments

## 4.1 Introduction

Firms' opposition to union activities is a common occurrence in industrialized economies (Gall and Dundon, 2013). To avoid unionization, firms employ a variety of anti-union measures from subtly hindering the careers of union activists (Bourdieu et al., 2022; Brébion, 2022) to hiring discrimination (Baert and Omei, 2015; Nüß, 2023). But will this opposition continue in an era of labor shortage?

Conventional economic theory predicts a decline in discrimination in labor markets that have difficulties filling vacancies (Blanchard and Diamond, 1994; Pissarides, 2000). The underlying rationale is that a firm with trouble filling a vacancy will prefer to hire an applicant with less popular characteristics over the risk of not filling the vacancy. Yet, tight labor markets also improve unions' bargaining position and individuals' willingness to unionize (Pezold et al., 2023). This improved bargaining position also increases management incentives to oppose unionization attempts (Freeman, 1986). The extent of hiring discrimination against union supporters throughout the business cycle is therefore theoretically ambiguous. Discrimination may decrease when it becomes difficult to fill a vacancy, while it may increase due to the threat of unionization and higher wage demands.

I present empirical evidence on this theoretical ambiguity with a correspondence experiment on hiring discrimination against union supporters in Germany. I sent 5,006 fictitious job applications revealing union membership during the economic downturn due to the Covid-19 pandemic in 2020. I further complement the data with previously collected experimental data from 2017 to 2019 (Nüß, 2023). Combining both datasets creates a cross-section time-series of 13,720 observations. This allows me to study the effect of labor market conditions on hiring discrimination from 2017 to 2020. The design similarities and the use of 5 occupations in 6 federal states over a period of 4 years allows me to disentangle sector-specific business cycle effects (a proxy for unions' bargaining power) from the occupation-specific labor market tightness (a proxy for labor shortage). As a result, I can test for both opposing mechanisms and quantify their relative relevance.

I provide evidence for the relevance of the pro-cyclical of hiring discrimination against union supporters. There is evidence of discrimination during the Covid-19 pandemic in 2020. However, discrimination is only half the size of 2017 to 2019. Accounting for regional labor market conditions over the period of 2017 to 2020, I show that discrimination is pro-cyclical. An increase in a sector's cyclical labor demand increases discrimination. This cyclical effect is concentrated among firms not covered by a collective agreement. In contrast, discrimination is unaffected by occupation-specific difficulties in filling a position, which indicates that the emerging labor shortage is unlikely to dampen management opposition to unionization.

I contribute to the literature on management opposition against unionization (Bronfenbrenner, 2009; Freeman and Kleiner, 1990; Gall and Dundon, 2013; Wang and Young, 2022), particularly on discrimination of employee representatives and union activists (Baert and Omei, 2015; Bourdieu et al., 2022; Brébion, 2022; Goerke and Pannenberg, 2021; Kreisberg and Wilmers, 2022; Nüß, 2023) by examining the influence of labor market

conditions. Three recent studies conduct correspondence experiments to identify hiring discrimination against union activists: Baert and Omeij (2015), Kreisberg and Wilmers (2022), and Nüß (2023). Together these experiments find a link between union threat potential and hiring discrimination. Employers are inclined to discriminate against union supporters when they perceive the possibility of unionization within their firm. This behavior is more prevalent in larger firms operating in sectors with a stronger union presence. I contribute to this literature by decomposing labor market conditions into cyclical labor demand and occupation-specific labor market tightness and show that discrimination against union supporters is pro-cyclical. Discrimination is stronger if a sector has higher cyclical labor demand, but is unaffected by difficulties to fill vacancies. This further supports the idea that union threat effects can explain the discrimination of union supporters. I further contribute to the literature on how labor market conditions affect union activities (Ahlquist and Downey, 2023; Charles et al., 2021; Naidu, 2022; Pezold et al., 2023). Pezold et al. (2023) find moderate effects of labor market tightness on individuals' willingness to unionize, but this does not translate into aggregate union activities. With evidence for management opposition increasing in tight labor markets, I provide a potential explanation for the moderate effect: a simultaneous increase in employees' willingness to unionize as well as employers' willingness to oppose unionization.

My paper builds on previous correspondence experiments that account for labor market conditions in hiring discrimination. Most correspondence experiments that consider labor market conditions rely on cross-sectional data, for example in the analysis of ethnic discrimination (Baert et al., 2015) and unemployment duration (Kroft et al., 2013; Nüß, 2018). One exception is Carlsson et al. (2018) who conducted an analysis on the relevance of labor market conditions for ethnic discrimination in the Swedish labor market, gathering data from three separate correspondence experiments, with some regional overlap. In contrast to Baert et al. (2015), they find no impact of labor market conditions on hiring discrimination. Another recent exception is the analysis by Nüß (2023) on hiring discrimination against union supporters in a repeated experiment from 2017 to 2019. While this experiment exploits variation between regional labor markets, the data collection of all waves was conducted in a strong and stable labor market with little variation in overall labor market conditions.

Lastly, I contribute to the growing recent literature on how labor market tightness affects firm performance and decision making (Autor et al., 2023; Baert et al., 2015; Bossler and Popp, 2022; Carlsson et al., 2018; Hangartner et al., 2022; Le Barbanchon et al., 2023). This literature shows that tight labor markets reduce employers' hiring standards, increase wages, and have the potential to reduce discrimination of minority groups. I show that these common effects do not translate to lower discrimination of union supporters.

The remainder of this paper is structured as follows. Section 4.2 discusses aspects in Germany's labor market institutions that become relevant when studying potential business cycle effects. Section 4.3 summarizes the experimental design. Section 4.4 summarizes the empirical strategy, giving insight into the construction of labor market conditions indicators and the model specifications. Section 4.5 presents the results. Section 4.6 discusses

and analyzes the effect heterogeneity by firms' collective agreement coverage relevance. Finally, Section 4.7 concludes.

## 4.2 Germany's Labor Market Institutions

The German model of industrial relations is set in an environment with both a strong welfare state and high levels of employment protection. Workers' representation is legally separated into two spheres: works councils that serve as a workers' voice at the firm level and unions whose primary role is negotiating collective agreements. While workers in firms with five or more employees have the right to establish a works council, less than 40% of the workforce are represented by one. A similar pattern is observable in the collective agreement coverage of only 54% (Ellguth and Kohaut, 2019).

In Germany, collective agreement coverage is independent of union membership status. To avoid the resulting free-riding problem, unions provide additional services to union members, such as legal services. Access to a unions' legal support system is one of the main reasons why workers join and become organized (Chadi and Goerke, 2023). Due to the legal support, dismissing union members is more difficult and costly than non-union employees (Goerke and Pannenberg, 2010, 2011).

The high levels of employment protection and institutional employee participation make labor adjustments at the extensive margin (firing of employees during economic downturns) more difficult (Kim et al., 2018). As a result, hiring employees for permanent positions raises the stakes of the hiring process. To prevent rigidity in the adjustment of labor demand, Germany's industrial relations system provides several instruments, such as hardship clauses for delayed implementation of wage increases, and opening clauses for deviations from the collective agreement (Boeri et al., 2021; Jäger et al., 2022). Another approach is the use of working time policies to prevent layoffs, for example using working time accounts (Herzog-Stein and Zapf, 2014) and short-time work (Balleer et al., 2016; Giupponi et al., 2022; Herzog-Stein et al., 2022) for temporary working time reductions.

Germany's industrial relations is therefore characterized as a corporatist system that provides workers representation, employment protection, and a centralized but flexible wage bargaining system. In international comparison, the *Global Rights Index* of the *International Trade Union Confederation* classifies Germany among the countries with the lowest rates of union rights violations (ITUC, 2023). Despite relatively low rates of labor rights violations, there is evidence of employer anti-union activities (Hertwig et al., 2021; Nüß, 2023), opposition during works council elections (Behrens and Dribbusch, 2018), violations of employee participation rights (Behrens and Dribbusch, 2020), and direct sanctions of works council activities (Brébion, 2022).

## 4.3 Experimental Design

This section provides a general overview of the correspondence experiment, providing details on the chosen regions, occupations, how union membership is revealed in the ex-

periment, and the data collection of vacancies and callbacks.

### 4.3.1 General Design

To allow the construction of a cross-sectional time series and to consider labor market conditions, the experimental design is a close replication of Nüß (2023). The design covers the same federal states (Berlin, North Rhine-Westphalia, Hamburg, Bavaria, Saxony, and Baden-Württemberg) and occupations (Office Clerk, Sales Manager, Hotel Manager, Logistic Worker, and Mechatronics Technician). This coverage represents 30.8% of all employees in 2020 (see Appendix Table 4.A.3.1). Between August and November 2020, I sent pairs of fictitious job applications randomly revealing union membership in applications. The applications included all the necessary documents typically required for the application process in Germany, such as a cover letter, a resume, and relevant certificates. Certificates attest to the applicant’s good grades. All applicants were male, were born in Germany with common German names, and were 28 years old. The applicants completed vocational training and had 10 years of relevant work experience in two firms. The names, layouts, and work history are different, the applications and their content are qualitatively similar. Due to the randomization of names, pictures, addresses, layouts, previous work history, and the order of applications, all application characteristics are orthogonal to the union membership by design and allow me to estimate the extent of discrimination against union supporters. Besides my original data collection in 2020, I utilize data collected by Nüß (2023) for the pre-pandemic period from 2017 to 2019, to encompass variation in labor market conditions over time.

### 4.3.2 Revealing Union Membership

To make union membership salient to the employer, I reveal a union membership in the application. This approach is common among prior correspondence experiments investigating discrimination against union supporters in hiring practices (Baert and Omeij, 2015; Kreisberg and Wilmers, 2022; Nüß, 2023). I do this via the categories “hobbies/memberships”, which contain randomly assigned leisure activities and related memberships. To test for the relevance of union memberships, one out of the two applications for each firm included a randomly assigned union membership related to their previous employment. I use three different unions of the Confederation of German Trade Unions (*DGB*). The *IG Metall* for applications as a logistic worker and mechatronics technician, *Ver.di* for applications as an office clerk and sales assistant, and *NGG* for applications as a hotel manager. An example application is available in the Appendix 4.A.11.

One common concern about the revelation of uncommon characteristics, such as revealing a union membership, is that they could signal other individual characteristics. If, for example, employers associate union members with specific personality traits or political orientations, discrimination could result from these traits instead of union membership. If this is the case, we would expect that such signals decrease in relevance if it becomes more difficult to fill a position, while they should be irrelevant for discrimination related to the threat of unionization.

### 4.3.3 Data Collection

To ensure comparability of the newly collected data to the previous data, the data collection strictly follows the procedure of Nüß (2023). For the collection of vacancies, I use the job portal of the Federal Employment Agency.<sup>1</sup> The job portal of the Federal Employment Agency is the most important job platform, is of high representativeness, and contains more than 40% of all German vacancies over recent years (Bossler et al., 2020). Every week, the portal was searched for new vacancies listed in the last seven days. The considered vacancies had to fulfill the following criteria: full-time positions, vocational training required, and no managerial responsibility. Vacancies needed to be from the firms themselves, excluding temporary employment or service agencies. Setting these restrictions ensured reliable information on the workplace's sector and an accurate fit of the applicants' work experience. The job portal further provides detailed information on firm size, sectoral classification, contract type (Temporary/Permanent Positions), and collective agreement coverage.

From August to November 2020, I sent pairs of applications to each firm via email. The first application was submitted on Monday evening, and the second on Tuesday evening. The submission of all applications occurred exclusively within the time frame of 6 to 8 pm. The time frame was used to uphold the impression of active employment and to reduce the risk of detection due to suspicious behavior. Firms were able to contact the applicants via email and phone. The phones were linked to a digital voicemail which recorded all messages and redirected them to the email accounts. To minimize the burden of the experiment for firms, the emails and voicemails were monitored and responded to every evening after 5 pm.

Callback rates of the applicants are classified into two categories. Callback category 1 is a dummy variable equal to 1 if applicants got a callback that indicated an invitation to a job interview, otherwise the variable is equal to zero. Callback category 2 covers all responses contained in callback category 1 plus all additional responses that indicated firms had an interest in the applicant. These additional responses mainly covered imprecise statements recorded via voice mail asking the applicant to call them back, as well as requests for additional information and documents from the applicant.

## 4.4 Empirical Strategy

This section provides an overview of the empirical strategy. It discusses the reasoning and construction of the labor market condition variables, the model specification, and the assumptions necessary for a causal interpretation of the impact of labor market conditions on hiring discrimination.

### 4.4.1 Measuring Labor Market Conditions

To account for alternative mechanisms related to labor market conditions, the first challenge in the analysis lies in disentangling occupation-specific labor market tightness from

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<sup>1</sup>The job portal is available over the following link: <https://www.arbeitsagentur.de/jobsuche/>.

sectoral business cycle effects. The second challenge is the need to separate regional-specific characteristics, such as political attitudes and regional exposure to unobserved characteristics, from labor market conditions.

I measure labor market conditions in two ways that consider the two opposing mechanisms (occupation-specific labor market tightness and unions' bargaining power) for the overall extent of hiring discrimination. First, I use data of the Federal Employment Agency on vacancies and unemployment for each occupation, federal state, and year, to construct a vacancy/unemployment-ratio (number of open vacancies divided by the registered unemployed for these occupations). The vacancy/unemployment-ratio uses occupational classification (KldB 2010) data, considering the sum of vacancies and unemployment of all skill levels (Helper, Professional, Specialist, and Expert) for the main analysis. This variable approximates the regional difficulty of filling an open vacancy for each occupation. Second, I collect sectoral employment data from the Federal Employment Agency to construct a business cycle variable for cyclical labor demand. By focusing on cyclical fluctuations of labor demand, I can correct for sectoral trends when approximating unions' and employees' temporary bargaining power. In addition to these two opposing channels, I account for unions' sectoral bargaining power by the construction of sectoral union density, the sectoral share of union members. This measure is constructed using data from the German Socio-Economic Panel (GSOEP) wave of 2019. Union density is known to affect discrimination of union supporters (Baert and Omey, 2015; Nüß, 2023) and reflects unions' bargaining power, unrelated to short-term labor market changes.

While the construction of the vacancy/unemployment-ratio is straightforward, the construction of sectoral business cycle fluctuations is more of a challenge. Due to the Covid-19 pandemic and the extensive use of short-time work (STW) in all sectors of the German economy, relying on sector-specific employment gives an imprecise figure of the true sector-specific labor demand. During peak Covid, employees faced on average a 22-hour cyclical decline in hours worked per quarter (Herzog-Stein et al., 2022). During the period of the experiment employees still faced an average cyclical decline of 10 working hours per quarter. Even when employed, actual labor demand was below what employment statistics indicate. Given the extensive use of short-time work, I correct the sector-specific labor demand using the sector-specific employment equivalent of cyclical short-time work usage provided by the Federal Employment Agency.

Figure 4.1 provides an overview of the development of labor market conditions over time. The series shows a steady growth in total employment, as well as a steady tightening of the labor market until the Covid-19 pandemic. Figure 4.1a shows the initial series of total employment, compared to the short-time work corrected employment. As expected due to the expansion of short-time work, the original series of total employment notably underestimates the decline in labor demand. While total employment between 2019Q4 and 2020Q2 declined by 417,172 employees, the short-time work corrected measure decreased by 2,627,138 employees, more than six times the original measure of labor demand. This illustrates the temporary decline in labor demand for 2020, which would be undetected without accounting for the usage of short-time work. Figure 4.1b shows the development of

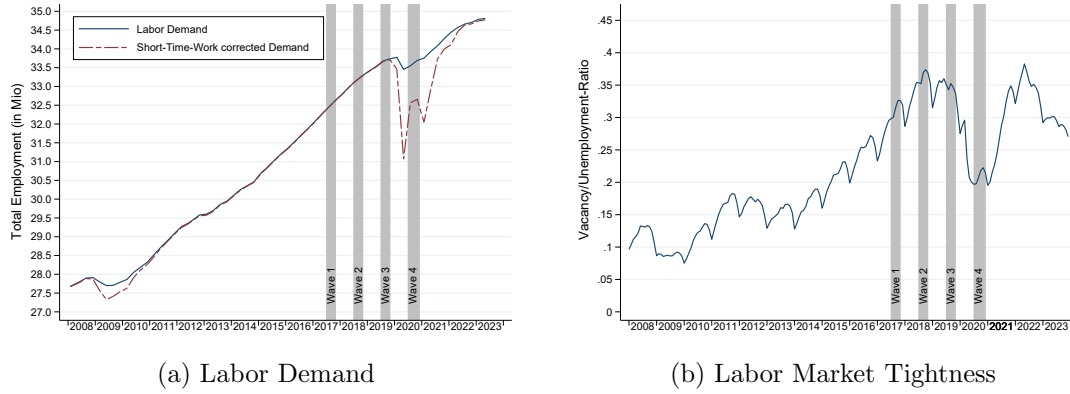


Figure 4.1: Labor Market Conditions during the Experiment

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This figure presents the development of the German labor market over time. Panel a) presents, the labor demand as the total employment subject to social security contributions in Germany and Short-time work corrected labor demand constructed by quarterly total employment subject to social security contributions, minus the employment equivalent of cyclical short-time work usage. Panel b) presents the vacancy/unemployment-ratio as the monthly total number of unemployed, relative to the total number of open vacancies.

the vacancy/unemployment-ratio, with a steadily tightening labor market until the Covid-19 pandemic. During the initial experiment (2017-2019), there were on average 35 open vacancies per 100 registered unemployed, but available vacancies declined on average to 20 vacancies per 100 registered unemployed during the data collection in 2020. Both measures of labor market conditions therefore provide the necessary variation for an analysis of the period 2017 to 2020.

Due to the strong disparities between labor demand and the short-time work corrected measure, the rest of the paper uses a constructed sector-specific labor demand. This measure is based on the sector-specific employment and corrects for the sector-specific short-time work employment equivalent. To decompose the sectoral business cycle component from its long-run trend, the final business cycle variable is constructed based on the logarithm of total sectoral employment (corrected for short-time work) and the application of the Hodrick-Prescott-Filter ( $\lambda = 1,600$ ) or the Hamilton-Filter at the country level. These filters lead to sectoral employment gaps that can be interpreted as the percentage deviations of cyclical employment from its long-run trend.

Table 4.1 provides summary statistics for the constructed labor market condition variables for both the initially constructed variables (Panel A) and the resulting experimental variation (Panel B). Both statistics provide qualitatively similar insights. The extension of the experiment to the period of the Covid-19 pandemic shows strong changes in overall labor market conditions.<sup>2</sup>

**Vacancy/Unemployment-Ratio:** While in the original experiment from 2017 to 2019, the average vacancy/unemployment-ratio of the covered regions and occupations was 0.43

<sup>2</sup>Table 4.A.1.1 and Table 4.A.1.2 provide an extended version of Table 4.1 accounting for employment gaps not corrected by short-time work, supporting the concern that employment gaps without consideration of short-time work do not cover the true variation in labor market conditions.



Table 4.1: Summary Statistics of Labor Market Conditions (2017 to 2020)

	Mean	SD	Median	Min	Max	N
<b>Panel A: Original Variation</b>						
	<u>V/U-Ratio (by State and Occupation)</u>					
2020	0.27	0.31	0.16	0.03	1.13	30
2017 to 2019	0.49	0.56	0.26	0.09	2.64	76
Full Sample	0.43	0.51	0.24	0.03	2.64	106
	<u>Employment Gap HP STW corrected</u>					
2020	-1.46	2.22	-0.67	-7.86	2.37	20
2017 to 2019	1.49	2.48	0.66	-0.81	13.14	55
Full Sample	0.71	2.74	0.45	-7.86	13.14	75
	<u>Employment Gap Ham STW corrected</u>					
2020	-1.79	3.29	-0.85	-10.41	6.00	20
2017 to 2019	2.68	3.77	1.77	-2.17	16.22	55
Full Sample	1.49	4.13	1.26	-10.41	16.22	75
	<u>Union Density</u>					
2019	0.13	0.10	0.10	0.00	0.41	20
<b>Panel B: Experimental Variation</b>						
	<u>V/U-Ratio (by State and Occupation)</u>					
2020	0.24	0.29	0.12	0.03	1.13	5,006
2017 to 2019	0.43	0.51	0.23	0.09	2.64	8,714
Full Sample	0.36	0.45	0.20	0.03	2.64	13,720
	<u>Employment Gap HP STW corrected</u>					
2020	-2.37	1.91	-1.42	-7.86	2.37	5,006
2017 to 2019	2.29	2.56	1.70	-0.81	13.14	8,714
Full Sample	0.59	3.25	0.67	-7.86	13.14	13,720
	<u>Employment Gap Ham STW corrected</u>					
2020	-3.13	2.52	-2.09	-10.41	6.00	5,006
2017 to 2019	3.63	4.02	2.00	-2.17	16.22	8,714
Full Sample	1.16	4.82	1.77	-10.41	16.22	13,720
	<u>Union Density</u>					
2020	0.12	0.07	0.08	0.00	0.41	5,006
2017 to 2019	0.12	0.08	0.08	0.00	0.28	8,714
Full Sample	0.12	0.07	0.08	0.00	0.41	13,720

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This table reports descriptive statistics about labor market conditions for the data collection for 2020 in comparison with Nüß (2023). Panel A provides the summary statistics resulting from the original variation provided by the design. Panel B provides the summary statistics resulting from the experimental data collection. The employment gap HP (STW corrected) contains the Hodrick–Prescott filtered ( $\lambda = 1,600$ ) seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage at the country level, for each sector based on NACE 2 one digit sector classification. The employment gap Ham (STW corrected) contains the Hamilton filtered seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage at the country level, for each NACE 2 one-digit sector. The vacancy/unemployment-ratio is the average of the total number of open vacancies relative to the monthly total number of unemployed, for each occupation, federal state, and year during the experiment.

(SD 0.5) (43 vacancies per 100 unemployed), the vacancy/unemployment-ratio fell to 0.24 (SD 0.3) (24 open vacancies per 100 unemployed) in 2020, which is close to the overall labor market tightness in Germany. The joined sample provides a wide range of regional and occupation-specific labor market tightness from 0.03 (Hotel Manager) to 2.64 (Mechanics Technician).

**Employment Gap Hodrick-Prescott-Filter:** For the sector-specific short-time work corrected employment gap, the average sector's employment is 2.3% (SD 2.6) above its long-run trend throughout 2017 to 2019. This changed during the Covid-19 pandemic to an average employment gap of 2.4% (SD 1.9) below its long-run trend in 2020, implying an average change in cyclical employment (labor demand) by 4.7%. On the sectoral level, variation in the employment gaps ranges from 7.9% below to 13.1% above its long-run trend.

**Employment Gap Hamilton-Filter:** For the sector-specific short-time work corrected employment gap, the average sector's employment is 3.6% (SD 4.0) above its long-run trend throughout 2017 to 2019. This changed during the Covid-19 pandemic to an average employment gap of 3.1% (SD 2.5) below its long-run trend in 2020, implying an average change in cyclical employment (labor demand) by 6.7%. On the sectoral level, variation in the employment gaps reaches from 10.4% below to 16.2 % above its long-run trend. This larger cyclical component, compared to the Hodrick-Prescott-Filter is a common observation. It stems from the filter's one-sidedness, instead considering future values as the two-sided Hodrick-Prescott-Filter does (Hamilton, 2018; Quast and Wolters, 2022).

The last remaining challenge of separating regional-specific unobservables from labor market conditions can be addressed by the dataset, covering four years. By replicating the original experiment of Nüß (2023) for the same occupations and regions during the economic downturn of the Covid-19 pandemic and later on merging both experiments, I obtain a cross-section-time-series that allows me to control for regional unobservables. Furthermore, the available vacancy/unemployment-ratio and the sector-specific labor demand allow me to test two opposing mechanisms that drive hiring discrimination against union supporters.

#### 4.4.2 Specification

In this section, I provide an overview of the model specification used in the analysis. Equation 4.1 presents the initial specification of the linear probability model used for the analysis of the Covid-19 pandemic in 2020. The dependent variable covers the callback categories 1 and 2.

$$Callback_i = \beta_0 + \tau Union_i + \mathbf{X}_i' \beta + \epsilon_i \quad (4.1)$$

The coefficient  $\tau$  represents the average percentage point difference in callback resulting from the disclosure of a union membership in the application. A negative coefficient implies that union members receive fewer callbacks than their non-union counterparts. The vector

$\mathbf{X}$  represents the regional- and occupation-fixed effects in the main specification. It further contains firm controls on the collective agreement coverage, contract type, and firm size, as well as controls for application characteristics such as application order, layouts, pictures, previous employers, organization membership, name, address, and school.

To account for within-region variation of labor market conditions over time, I conduct a cross-section time series analysis with the full sample from 2017 to 2020. For this analysis, I use the labor market condition variables (V/U-Ratio, Employment Gap HP STW corrected, and Employment Gap Ham STW corrected) constructed in Section 4.4.1. Equation 4.2 presents the linear probability model incorporating the callback categories 1 and 2 as dependent variables. With the full sample I can exploit variation in  $Callback_{itso}$  for applicant  $i$  in year  $t$  for federal state  $s$  with occupation  $o$ . The coefficient  $\tau$  represents the average percentage point difference in callbacks associated with the disclosure of union membership after accounting for labor market conditions. For labor market conditions ( $LMC$ ), I consider the previously constructed regional and occupation-specific vacancy/unemployment-ratio, the sector-specific short-time work corrected employment gaps, and sectoral union density constructed based on the 2019 wave of the German Socio-Economics Panel (GSOEP). Finally, the interaction of the union membership dummy and labor market conditions tests for the relevance of labor market conditions for hiring discrimination against union supporters. To account for sector compositional changes due to the Covid-19 pandemic, all regressions in the full sample are weighted by the sectoral employment share and standard errors are clustered at the sectoral level.

$$Callback_{itso} = \beta_0 + \tau Union_{itso} + \delta LMC_{itso} + \gamma Union \times LMC_{itso} + \mathbf{X}'_{itso} \beta + \lambda Year_t + \lambda State_s + \lambda Occupation_o + \epsilon_{itso} \quad (4.2)$$

The causal interpretation of the impact of labor market conditions on hiring discrimination, based on Equation 4.2, relies on three assumptions. First, compositional changes in sectors and firms do not affect the extent of discrimination. If firms that discriminate more (for example larger firms) are more strongly affected by the pandemic and they post relatively fewer vacancies, there would be a decline in the average effect of discrimination of union members, independent of labor market conditions. I address this concern by reweighting all analyses toward their sectoral employment share and with additional sub-group heterogeneity analysis.

Second, the assumption of the exogeneity of the Covid-19 pandemic shock. The intensity of the Covid-19 pandemic shock could be associated with the previous intensity of discrimination. I test for plausible exogeneity of the Covid-19 pandemic by analyzing the relationship between the pre-pandemic extent of discrimination in 2019 and the sectoral economic downturn during 2020. These analyses indicate no systematic relationship between the pre-pandemic extent of discrimination and the 2020 economic downturn (see Table 4.A.12.1).

Third, the assumption that labor market data reflect current labor market conditions and

employers need to be aware of these labor market conditions. If firms have no awareness of recent changes in the access to applicants, it cannot affect their hiring decisions. One reason for limited awareness of labor market conditions is the irregularity of new hires, which is the case for smaller firms. Another reason is that larger firms are more likely to have a human resource department, that is specialized in hiring and therefore, more likely to be aware of changes in access to applicants. This assumption will be tested among other robustness checks following the analysis of the main specifications. This analysis does not indicate that limited awareness of labor market conditions affects the extent of discrimination.

## 4.5 Experimental Results

In this section, I provide an analysis of the newly collected data in 2020 and an analysis of the full sample of 2017 to 2020, using various indicators for labor market conditions. The final sample consists of 13,720 applications sent to 6,857 firms. 5,006 of these applications (2,503 firms) were newly collected during the economic downturn of the Covid-19 pandemic in fall 2020. The remaining sample consists of 8,714 applications (4,357 firms) collected during the previous experiment by Nüß (2023), containing 2,082 applications for 2017, 3,274 applications for 2018 and 3,358 applications for 2019.

The applications were qualitatively similar across all years. This makes a time-series comparison of all callback rates feasible. Tests of the application characteristics suggest that the overall randomization was successful (see Appendix Table 4.A.2.1). The occupational composition is comparable to the relative share in the overall economy (see Appendix Table 4.A.3.2). In contrast, the near equal distribution of observations between federal states leads to an overrepresentation of smaller federal states (Berlin, Hamburg, Saxony) relative to larger federal states (Baden-Wurtemberg, Bavaria, North Rhine-Westphalia) (see Appendix Table 4.A.3.3).

### 4.5.1 Discrimination Before and During the Covid-19 Pandemic

In this section, I provide an initial analysis of the newly collected 5,006 applications during the Covid-19 pandemic in 2020 and compare them to the results of the pre-pandemic data. This analysis is based on Equation 4.1 using a linear probability model incorporating the callback categories 1 and 2 as dependent variables.

Table 4.1 Panel A, provides the results of the 2020 data of the Covid-19 pandemic. It shows that across all specifications and callback categories, union members are 3.0 to 3.6 percentage points less likely to get a positive callback compared to the applicants without union membership. These results are robust to the inclusion of firm and application characteristics in Column (5) and Column (6). They are further robust to the use of firm fixed-effects and the use of logit or probit models (See Table 4.A.5.1). Reweighting to the regions' representative employment and GDP shares or the sectoral employment share reduces discrimination to a level of 1.5 to 2.5 percentage points lower callbacks for union members, with partial loss of statistical significance (See Table 4.A.5.2).

Table 4.1: Hiring Discrimination from Revealing Union Membership in 2020

	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Year 2020</b>						
Union Membership	-0.0304*** (0.0085)	-0.0348*** (0.0096)	-0.0304*** (0.0085)	-0.0348*** (0.0096)	-0.0317*** (0.0084)	-0.0363*** (0.0094)
Callback Rate Control Group	0.303	0.426	0.303	0.426	0.303	0.426
N	5,006	5,006	5,006	5,006	5,006	5,006
Adjusted $R^2$	0.001	0.001	0.041	0.050	0.047	0.058
<b>Panel B: Year 2017 to 2019</b>						
Union Membership	-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			✓	✓	✓	✓
Federal State			✓	✓	✓	✓
Year			✓	✓	✓	✓
Firm Controls					✓	✓
Application Controls					✓	✓

*Note:* The table reports results based on the data for 2020, using a linear probability model in Equation 4.1. The dependent variable is the probability of receiving a positive callback following Callback Category 1 and 2. The baseline controls are regional-, occupation- and year-fixed effects. The firm controls cover collective agreement coverage, contract type, and firm size. Application controls cover the application order, layouts, pictures, previous employers, organization membership, name, address, and school. Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

For comparison to the pre-pandemic period, in Table 4.1 Panel B, I provide a reanalysis of Nüß (2023) for 2017 to 2019, showing that union members are 6.2 to 7.0 percentage points less likely to get a positive callback compared to the applicants without union membership. In comparison, these results show that during the Covid-19 pandemic, discrimination of union supporters was about half to a third of the size compared to the tighter labor market during the pre-pandemic period.

Due to the unique effect of the pandemic on the overall functioning of the labor market, some challenges in the comparison remain, particularly concerning the composition of available jobs and the usage of short-time work.

This initial analysis does not account for compositional changes in sectors and firms, which are known to affect hiring discrimination. To consider the importance of this concern, Figure 4.A.4.1 and Figure 4.A.4.2 show the composition of the new and previously collected data. The share of firms with less than 51 employees increased by 4.8 percentage points during the 2020 data collection, indicating that composition changes in firm characteristics could explain the decline in discrimination during the Covid-19 pandemic. However, robustness checks based on discrimination by firm size heterogeneity also find lower discrimination within firm sizes (see Figure 4.A.6.1 and Table 4.A.6.1) indicate that compositional changes alone can not explain the decline in discrimination.

#### 4.5.2 Hiring Discrimination and Labor Market Conditions (2017 to 2020)

To better account for compositional changes, I use time variation and alternative channels of labor market tightness and business cycle effects. In the next step, I conduct a cross-section time series analysis with the full sample from 2017 to 2020. I address the concern of compositional changes by reweighting all analyses toward their sectoral employment share. Due to the limited number of observations for the *Mining and Quarrying* sector ( $N = 2$ ) and *Activities of Households as Employers* ( $N = 4$ ), these observations are dropped from the final sample, since they do not provide any within variation in labor market conditions.

Table 4.2 reports the results of separate inclusion of the labor market condition indicators (vacancy/unemployment-ratio, the employment gaps using the Hodrick-Prescott-Filtered and the Hamilton-Filter, and the short-time work corrected employment gaps based on both filter methods). All labor market condition variables are standardized with a mean 0 and a SD of 1.

A requirement for the reliable analysis of labor market conditions in hiring discrimination is that the labor market condition variables affect the callback rates of the non-union applicants (Carlsson et al., 2018). Table 4.2 provides evidence that this is the case. The labor market conditions, measured as the vacancy/unemployment-ratio as well as the short-time work corrected employment gap, have a significant impact on callback rates. A tighter labor market results in higher callback rates. The relatively small effect size and insignificance of the uncorrected employment gaps underline the initial concerns about using the employment measure without accounting for short-time work. The rest of the paper will therefore exclude the original employment gaps and focus on the short-time work corrected employment gap in the remaining analysis.

Table 4.2 further provides the first evidence of how labor market conditions affect hiring discrimination against union supporters. For all measures, I obtain a negative coefficient for the interaction of the union membership with the labor market conditions. This indicates that an improvement in labor market conditions decreases the callback rates of union members relative to non-union applicants. There is no statistically significant impact of the vacancy/unemployment-ratio. The short-time work corrected employment gaps provide weak evidence for a link between discrimination and cyclical labor demand. Two out of four specifications find a statistically significant effect of the employment gaps on callbacks on the 5% significance level (Column 7 and Column 9) and one at the 10% significance level (Column 10). However, the low statistical significance is likely the result of the weak relationship between the employment gap and the callback category 2, which is insignificant in both regressions. Table 4.2 further reports that discrimination against union supporters increases with union density, the share of union members in the sector.

To account for the opposing mechanisms of labor market tightness and business cycle effects, Table 4.3 reports the combined analysis of the alternative mechanisms (occupation-specific labor market tightness and unions' bargaining power). Columns (1) and (2) use the Hodrick-Prescott-Filter and Columns (3) and (4) use the Hamilton-Filter when estimating employment gaps.

Table 4-2: Hiring Discrimination and Labor Market Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Union	-0.0504*** (0.0068)	-0.0571*** (0.0074)	-0.0511*** (0.0067)	-0.0576*** (0.0073)	-0.0509*** (0.0068)	-0.0577*** (0.0074)	-0.0520*** (0.0068)	-0.0584*** (0.0073)	-0.0523*** (0.0068)	-0.0588*** (0.0074)	-0.0440*** (0.0064)	-0.0506*** (0.0070)
V/U Ratio	0.0302*** (0.0153)	0.0288*** (0.0146)										
Union x V/U Ratio	-0.0076 (0.0049)	-0.0046 (0.0051)										
Employment Gap HP			0.0140* (0.0085)	-0.0002 (0.0091)								
Union x Employment HP Gap			-0.0085 (0.0056)	-0.0057 (0.0064)								
Employment Gap Ham					0.0120 (0.0090)	0.0026 (0.0097)						
Union x Employment Ham Gap					-0.0095* (0.0056)	-0.0095 (0.0064)						
Employment Gap HP (STW Corrected)							0.0292*** (0.0106)	0.0132 (0.0113)				
Union x Employment HP Gap (STW Corrected)							-0.0128** (0.0058)	-0.0097 (0.0066)				
Employment Gap Ham (STW Corrected)									0.0316*** (0.0114)	0.0191 (0.0120)		
Union x Employment Ham Gap (STW Corrected)									-0.0146** (0.0060)	-0.0127* (0.0068)		
Union Density											-0.0057 (0.0073)	-0.0074 (0.0080)
Union x Union Density											-0.0189*** (0.0072)	-0.0195** (0.0079)
N	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714
Adjusted ( $R^2$ )	0.062	0.069	0.062	0.069	0.062	0.069	0.063	0.069	0.063	0.069	0.063	0.070
Dependent Variables												
Strict Callbacks (Callback Category 1)	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓		✓		✓
Controls												
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions separately. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. The control variables cover regional-, occupation- and year-fixed effects. Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1,600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to the sectoral employment share. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.3: Hiring Discrimination and Labor Market Condition Mechanisms

	(1)	(2)	(3)	(4)
Union Membership	-0.0458*** (0.0064)	-0.0519*** (0.0069)	-0.0462*** (0.0064)	-0.0524*** (0.0069)
Union Density	-0.0048 (0.0074)	-0.0070 (0.0081)	-0.0062 (0.0074)	-0.0075 (0.0081)
Union Membership x Union Density	-0.0194** (0.0075)	-0.0203** (0.0082)	-0.0187** (0.0075)	-0.0198** (0.0082)
V/U Ratio	0.0268* (0.0154)	0.0270* (0.0146)	0.0272* (0.0154)	0.0266* (0.0146)
Union Membership x V/U Ratio	-0.0026 (0.0053)	0.0002 (0.0055)	-0.0028 (0.0053)	0.0002 (0.0055)
Employment Gap HP (STW Corrected)	0.0270** (0.0107)	0.0110 (0.0114)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0142** (0.0059)	-0.0118* (0.0067)		
Employment Gap Ham (STW Corrected)			0.0311*** (0.0114)	0.0189 (0.0121)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0147** (0.0062)	-0.0134* (0.0070)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.064	0.071	0.065	0.071
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1,600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to the sectoral employment share. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



On average, callback rates are 4.5 to 5.2 percentage points lower for union members. A one standard deviation increase in the vacancy/unemployment-ratio increases overall callbacks by 2.7 percentage points. In line with the separate inclusion of all labor market condition variables, there is no economically relevant nor statistically significant effect of the vacancy/unemployment-ratio for discrimination. These results imply that difficulties in filling a position will not reduce discrimination against union supporters.

Column (1) and Column (2) show the effect of cyclical employment based on the Hodrick-Prescott-Filter. A one standard deviation increase in cyclical employment increases overall callbacks by 1.1 to 2.7 percentage points. However, the effect is 1.2 to 1.4 percentage points lower for union members relative to non-union applicants. With stronger effect sizes and significance levels, Column (3) and Column (4) show that the Hamilton-Filter employment gap provides qualitative similar results. A one standard deviation increase in cyclical employment increases callbacks by 1.9 to 3.1 percentage points. The effect is 1.3 to 1.5 percentage points lower for union members. These results indicate that discrimination is pro-cyclical and increases in economic upturns.

Lastly, Table 4.3 shows that sectoral union density matters for the extent of discrimination. With no direct effect of union density on callbacks, the results show that union members have lower callbacks in strongly organized sectors. A one standard deviation increase in union density decreases callbacks for union members by 1.9 to 2.0 percentage points.

Taken together, the results provide evidence for the relevance of business cycle effects and union density as relevant factors for the extent of hiring discrimination against union supporters. These results suggest that discrimination increases with improving labor market conditions and unions sectoral strength. All these results are robust to the exclusion of sectors (see Table 4.A.7.5 and Table 4.A.7.6), sectoral employment weighting of 2018 (Table 4.A.6.2), and accounting for labor market conditions of the quarter before the experiment (Table 4.A.7.1). Excluding individual occupations shows the results are robust in four out of five cases. Excluding mechatronics technicians increases the impact of the vacancy/unemployment ratio, leading to higher discrimination against union members with labor market tightness. Lastly, in Appendix 4.A.10, I account for potential non-linearity in the relation of the vacancy/unemployment-ratio and hiring discrimination. These analyses indicate stronger sensitivity of below-median tightness positions. They react strongly to labor market conditions and indicating lower callbacks for union members. The opposite is true for positions in above-median tightness. They react less to further tightening of the labor market and invitations of union members, if observable, is in favor of union members. However, due to the absence of statistical discrimination for most specifications, this is at most suggestive evidence for non-linear effects of labor market conditions on hiring discrimination.

The small effect sizes of the vacancy/unemployment-ratio and business cycle effects on hiring discrimination might be the result of limited labor market data quality and limited awareness of firms. Labor market data quality might be at risk if job search behavior has changed during the Covid-19 pandemic. The next section will therefore test for the robustness of the findings, making use of alternative measures for labor market conditions.

The second challenge is that hiring firms need to be aware of current labor market conditions. If firms have no awareness of recent changes in the access to applicants, it can not affect their hiring decisions. Table 4.A.10.4 tests this assumption by the exclusion of firms smaller than 6 employees from the analytical sample. If the weak effects are driven by limited awareness of labor market conditions, we would expect stronger effects by the exclusion of smaller firms. By exclusion of smaller firms, the association of labor market conditions and callbacks increases by about 50%. However, the effect size of the interactions of union membership and labor market conditions is unaffected. This implies that limited awareness affects callbacks, but provides no explanation for the weak relationship of labor market conditions and discrimination.

### 4.5.3 Alternative Measures of Labor Market Conditions

This section tests the robustness of my results using alternative measures for labor market conditions. Given the unique circumstances of the Covid-19 pandemic, there is the risk that conventional labor market indicators such as the employment gap and vacancy/unemployment-ratio points a false picture of actual labor market conditions. This could be the case if job search behavior changed during the Covid-19 pandemic.

Recent evidence on job search behavior of the unemployed during the Covid-19 pandemic by Balgova et al. (2022) shows that they sent fewer applications than in comparable economic downturns. Faberman et al. (2022) further find a temporary decline in labor supply on the intensive and extensive margin during the first year of the Covid-19 pandemic. Both results indicate the possibility of an upward bias in the labor market condition variables. This unobserved variation in labor market conditions could lead to relatively weak effects of labor market conditions on callbacks and their relevance for discrimination.

I address this concern by exploiting variations in callback rates of the control group (non-union applicants) as an alternative measure of labor market conditions. Recent correspondence experiments with a comparable approach are Kroft et al. (2013), Baert et al. (2015) and Carlsson et al. (2018). The underlying idea is that the observed behavior of firms during the experiment should serve as a reliable indicator of the actual state of the labor market. If on average a similar applicant in one region gets more callbacks than in another region, this implies a higher demand/higher labor market tightness than in the other region.

Table 4.4 reports the results using the alternative labor market condition measure based on the average callback rates (Category 1 and 2) of non-union applicants using variation by federal state and year. In line with Section 4.5.2, I find that discrimination increases with better labor market conditions. A one standard deviation increase in the alternative labor market condition measure increases callbacks by 9.4 to 9.9 percentage points. This increase is only about two-thirds the size for union members. A one standard deviation increase in the alternative labor market condition measure, increases callbacks by 3.0 to 3.4 percentage points less for union members, relative to the non-union applicants. Comparing the relative effect size of labor demand and union density indicates that labor demand is more important than the previous labor market condition variables suggested. A one

Table 4.4: Alternative Labor Market Conditions using Callbacks by State and Year

	(1)	(2)	(3)	(4)
Union Membership	-0.0499*** (0.0067)	-0.0564*** (0.0073)	-0.0435*** (0.0064)	-0.0498*** (0.0069)
Alternative Labor Market Tightness	0.0980*** (0.0191)	0.0944*** (0.0165)	0.0991*** (0.0191)	0.0951*** (0.0165)
Union Membership x Alternative Labor Market Tightness	-0.0303*** (0.0064)	-0.0339*** (0.0074)	-0.0306*** (0.0064)	-0.0342*** (0.0074)
Union Density			-0.0061 (0.0072)	-0.0076 (0.0079)
Union Membership x Union Density			-0.0192*** (0.0072)	-0.0198** (0.0078)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.066	0.074	0.068	0.075
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model using the alternative labor market condition measure. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. The alternative measure of labor market tightness is based on the average callback rates (Category 1 and 2) of non-union members for each federal state in each year. Standard errors, corrected for clustering at the firm level, are in parentheses. Own sample and the sample from Nüß (2023). Alternative measures of labor market conditions are standardized with a mean of 0 and a standard deviation of 1. All estimates are reweighted to the sectoral employment share. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

standard deviation change in labor demand increases discrimination to almost twice the size of one standard deviation change in sectoral union density. These results are robust to other specifications of the alternative labor market condition measure (see Table 4.A.9.1).

## 4.6 Collective Agreement Heterogeneity

In this section, I account for the potential effect heterogeneity by collective agreement coverage. While the bargaining position of unions and employees is generally better in tight labor markets (Hirsch et al., 2018; Tope and Jacobs, 2009; Webber, 2022), there are two reasons to expect differences in firms responses depending on the collective agreement coverage. These reasons are the threat of unionization and Germany's corporatist and flexible bargaining system.

The threat of unionization has higher relevance for firms not covered by a collective agreement. These firms have the incentive to oppose unionization to avoid increasing wages and decreasing profits. This incentive increases with the wage differential between current wages and wages paid by a collective agreement (Freeman, 1986). Under better labor market conditions, employees in firms that are currently not part of a collective agreement have a stronger bargaining position. In weak labor markets, the resulting weaker bargaining position of unions and employees is unlikely to lead to substantial improvements in bargaining outcomes after successful unionization or strikes (Kleiner and Freeman, 1987; Massenkoff and Wilmers, 2024). Furthermore, due to limited outside options, the conse-

quences of employer opposition (for example firing or career hindrance) are also higher (Pezold et al., 2023). In contrast, tight labor markets improve employees' bargaining position and reduce the impact of employer opposition. As a result, I expect the threat of unionization due to willingness to unionize and wage demands to increase in tight labor markets, if a firm is not covered by a collective agreement.

The threat of unionization is expected to be less relevant for firms covered by a collective agreement. While higher rates of unionization are associated with higher union activities (Jansen, 2014), additional union members have only limited effects on wages. Furthermore, while firms covered by a collective agreement discriminate less in economic upturns, it is less obvious how labor market conditions affect their hiring decisions during economic downturns. In economic upturns, organized companies might not perceive additional union members as a threat, given that higher profits allow them to cooperate, provide demanded wages, and avoid conflicts. In contrast during economic downturns, particularly crises, conflicts between employers and unions may arise from employee representations of resistance to wage cuts and mass layoffs (Budde et al., 2023; Fackler et al., 2024; Kim et al., 2018). Due to their legal support, even with the opportunity of dismissal, unionized employees are more difficult and costly to dismiss (Goerke and Pannenberg, 2010, 2011). This downward rigidity in wages and labor demand can intensify conflicts between employers and employees. Despite these downward rigidities, instruments to ensure internal flexibility could counteract employers' opposition to unions during crises, improving the functioning of corporatism during economic downturns. There is evidence for improvements in employer-works council cooperation during the Covid-19 pandemic, also in firms not affected by the pandemic (Behrens and Brehmer, 2022). Similarly, employers' attitudes towards unions may have gradually improved over time. During the Covid-19 pandemic, new collective agreements were created in several firms and sectors that led to a top-up of short-time work allowances received by employees (Schulten and Müller, 2020). This improved corporatism could therefore reduce discrimination of union supporters as well.

Given these possibilities, I expect pro-cyclical discrimination in firms without collective agreement coverage, meaning lower discrimination against union supporters during the economic downturn of the Covid-19 pandemic. In light of wage and employment rigidities and unions' opposition to rationalization through wage cuts and employee dismissal, discrimination with collective agreement coverage is expected to be counter-cyclical, meaning it decreases in economic upturns and increases in economic downturns.

For this analysis, I rely on Equation 4.1, using the same linear probability model and specification from section 4.5.1, but split the sample by collective agreement coverage. Figure 4.A.10.1 reports the estimated coefficients for differences in callbacks between union and non-union applicants, accounting for collective agreement coverage.<sup>3</sup> Without collective agreement coverage Figure 4.A.10.1 reports discrimination of 7 to 8 percentage points during the economic upturn from 2017 to 2019. The same estimates for the Covid-19 pandemic in 2020 report discrimination of 3 percentage points. In contrast, there is no

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<sup>3</sup>Table 4.A.8.1 provides statistical details of the estimation procedure.

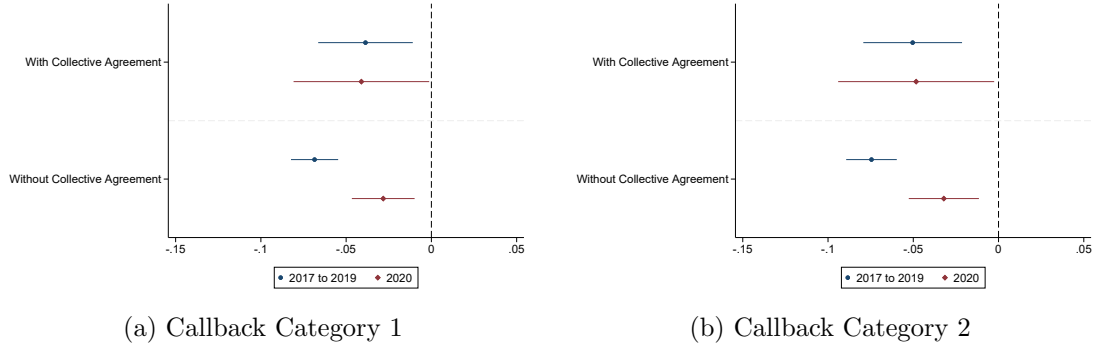


Figure 4.1: Distribution of Hiring Discrimination by Collective Agreement

*Note:* The figure presents estimates of the linear probability model based on equation 4.1, accounting for effect heterogeneity by collective agreement coverage for the 2020 data collection, in comparison to Nüß (2023). 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.

indication of a change in discrimination for firms with collective agreement coverage between both periods. For both experiments, I find that discrimination of union members results in approximately 5 percentage points less callbacks. This indicates that hiring discrimination is related to firms' fear of unionization. Considering the opposing mechanisms of occupation-specific labor market tightness and business cycle effects in a sample split of firms with and without collective agreement coverage supports this interpretation (See Table 4.A.10.4), suggesting a decoupling of labor market conditions and discrimination in unionized firms.

These results indicate that after unionization and the negotiation of a collective agreement, employers' opposition is reduced and no longer depends on the business cycle. One explanation for this is that the German industrial relations wage bargaining system encourages cooperation between employers and employees. The system's mixture of firm and sector-level agreements, its instruments of hardship and opening clauses, and a variety of working time policies help reduce the extent of conflicts between employees and management.

An alternative explanation are management learning effects. Management opposition could be driven by ideology and management's unfamiliarity with unions and the uncertainty of their activities after unionization (Dundon, 2002). Recent evidence for such a channel is Matzat and Schmeißer (2022), who find a causal link between unionization and management's increasing support for left-leaning political parties. A comparable pattern is observable with the implementation of a new works council in German establishments, which lead to temporary reductions in productivity, but long-term productivity gains (Mueller and Stegmaier, 2017).

## 4.7 Conclusion

In this study, I explore the relevance of labor market conditions for hiring discrimination against union supporters with data from two correspondence studies revealing union memberships in the application.

Using a range of labor market condition indicators, I show that hiring discrimination is affected by cyclical labor demand and decreased during the Covid-19 pandemic to half of the pre-pandemic size. My results indicate that hiring discrimination is linked to the business cycle and unions' bargaining power rather than occupation-specific difficulties in filling a position.

The second key finding is that hiring discrimination primarily stems from employers' fear of unionization. Discrimination is strongest in large firms without collective agreement coverage during boom phases. Employers are inclined to discriminate against union supporters when they expect the possibility of unionization within their firm. This tendency is more prevalent in larger firms operating in sectors with a stronger union presence. These firms, however, do not perceive the same union threat in an economic downturn leading to lower discrimination rates against union supporters. I find no such relationship of labor market conditions in firms covered by a collective agreement. One explanation is that the German wage bargaining system encourages cooperation between employers and employee representatives.

The overall findings suggest that an important force behind hiring discrimination against union supporters is the potential threat of unionization of firms that are presently not covered by collective agreements. The results indicate that the emerging labor shortage observed in many industrialized countries is unlikely to dampen management's opposition to unionization. My results further point to an alternative explanation of why tight labor markets have limited effects on unionization activities. While labor market tightness increases individual willingness to unionize, simultaneous increases in management opposition can dampen the aggregate effects on actual unionization.

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## 4.A Appendix to Chapter Four

### 4.A.1 Comparison of Labor Market Conditions Indices

Table 4.A.1.1: Summary Statistics of Labor Market Conditions by Design (2017 to 2020)

	Mean	SD	Median	Min	Max	N
<b>V/U-Ratio (by State and Occupation)</b>						
2020	0.27	0.31	0.16	0.03	1.13	30
2017 to 2019	0.49	0.56	0.26	0.09	2.64	76
Full Sample	0.43	0.51	0.24	0.03	2.64	106
<b>Employment Gap HP</b>						
2020	-0.42	1.41	-0.15	-4.61	2.44	20
2017 to 2019	0.53	0.98	0.27	-0.85	3.96	55
Full Sample	0.28	1.18	0.23	-4.61	3.96	75
<b>Employment Gap HP STW corrected</b>						
2020	-1.46	2.22	-0.67	-7.86	2.37	20
2017 to 2019	1.49	2.48	0.66	-0.81	13.14	55
Full Sample	0.71	2.74	0.45	-7.86	13.14	75
<b>Employment Gap Ham</b>						
2020	-0.67	2.61	-0.09	-7.06	6.14	20
2017 to 2019	1.34	1.57	1.36	-2.21	6.82	55
Full Sample	0.80	2.09	0.83	-7.06	6.82	75
<b>Employment Gap Ham STW corrected</b>						
2020	-1.79	3.29	-0.85	-10.41	6.00	20
2017 to 2019	2.68	3.77	1.77	-2.17	16.22	55
Full Sample	1.49	4.13	1.26	-10.41	16.22	75
<b>Union Density</b>						
2019	0.13	0.10	0.10	0.00	0.41	20

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This table reports descriptive statistics about labor market conditions for the data collection for 2020 in comparison with Nüß (2023). The summary statistics are based on the characteristics of the experimental design. The employment gap HP (STW corrected) contains the Hodrick–Prescott filtered ( $\lambda = 1,600$ ) seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage at the country level, for each sector based on NACE 2 one digit sector classification. The employment gap Ham (STW corrected) contains the Hamilton filtered seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage at the country level, for each NACE 2 one-digit sector. The vacancy/unemployment-ratio is the average of the total number of open vacancies relative to the monthly total number of unemployed, for each occupation, federal state, and year during the experiment.

Table 4.A.1.2: Summary Statistics of Labor Market Conditions by Experimental Data (Extended)

	Mean	SD	Median	Min	Max	N
<b>Vacancy/Unemployment-Ratio</b>						
2020	0.24	0.29	0.12	0.03	1.13	5,006
2017 to 2019	0.43	0.51	0.23	0.09	2.64	8,714
Full Sample	0.36	0.45	0.20	0.03	2.64	13,720
<b>Employment Gap HP</b>						
2020	-0.83	1.12	-0.51	-4.61	2.44	5,006
2017 to 2019	0.93	0.91	0.52	-0.85	3.96	8,714
Full Sample	0.29	1.31	0.26	-4.61	3.96	13,720
<b>Employment Gap HP STW corrected</b>						
2020	-2.37	1.91	-1.42	-7.86	2.37	5,006
2017 to 2019	2.29	2.56	1.70	-0.81	13.14	8,714
Full Sample	0.59	3.25	0.67	-7.86	13.14	13,720
<b>Employment Gap Ham</b>						
2020	-1.52	1.86	-0.96	-7.06	6.14	5,006
2017 to 2019	1.75	1.44	1.53	-2.21	6.82	8,714
Full Sample	0.55	2.25	0.60	-7.06	6.82	13,720
<b>Employment Gap Ham STW corrected</b>						
2020	-3.13	2.52	-2.09	-10.41	6.00	5,006
2017 to 2019	3.63	4.02	2.00	-2.17	16.22	8,714
Full Sample	1.16	4.82	1.77	-10.41	16.22	13,720
<b>Union Density</b>						
2020	0.12	0.07	0.08	0.00	0.41	5,006
2017 to 2019	0.12	0.08	0.08	0.00	0.28	8,714
Full Sample	0.12	0.07	0.08	0.00	0.41	13,720

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This table reports descriptive statistics about labor market conditions for the data collection for 2020 in comparison with Nüß (2023) due to the joined sample. The summary statistics are based on the experimental data collection. The employment gap HP (STW corrected) contains the Hodrick–Prescott filtered ( $\lambda = 1600$ ) seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage, for each sector based on NACE 2 one digit sector classification. The employment gap Ham (STW corrected) contains the Hamilton filtered seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage, for each sector based on NACE 2 one-digit sector classification. The vacancy/unemployment-ratio is the average of the monthly total number of unemployed relative to the total number of open vacancies for each occupation, region, and year, during the experiment.

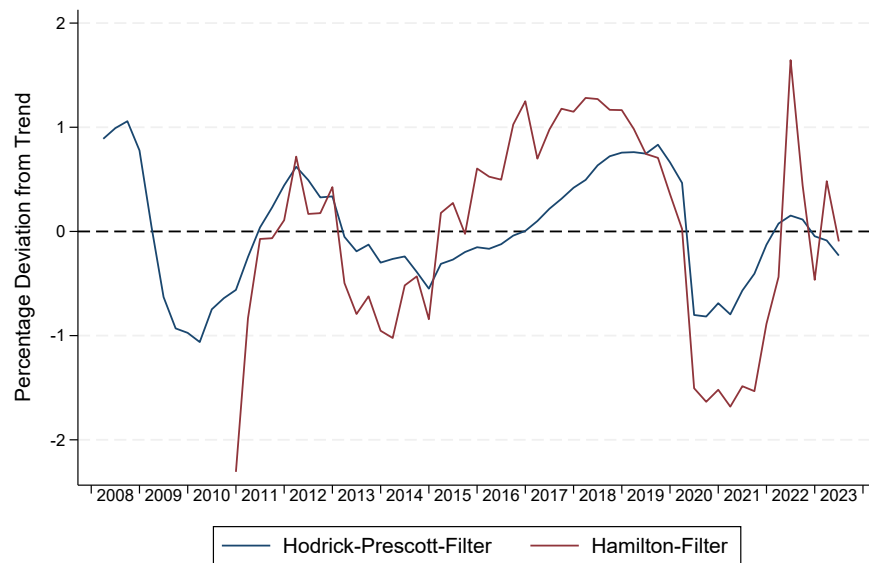


Figure 4.A.1.1: Cyclical Employment Gaps by Filter Methods over Time

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This figure presents the development of the cyclical employment gaps, based on the Hodrick–Prescott-Filter ( $\lambda = 1600$ ) and the Hamilton-Filter over time. The employment gaps are constructed with the seasonally adjusted log of total employment subject to social security contributions in Germany.

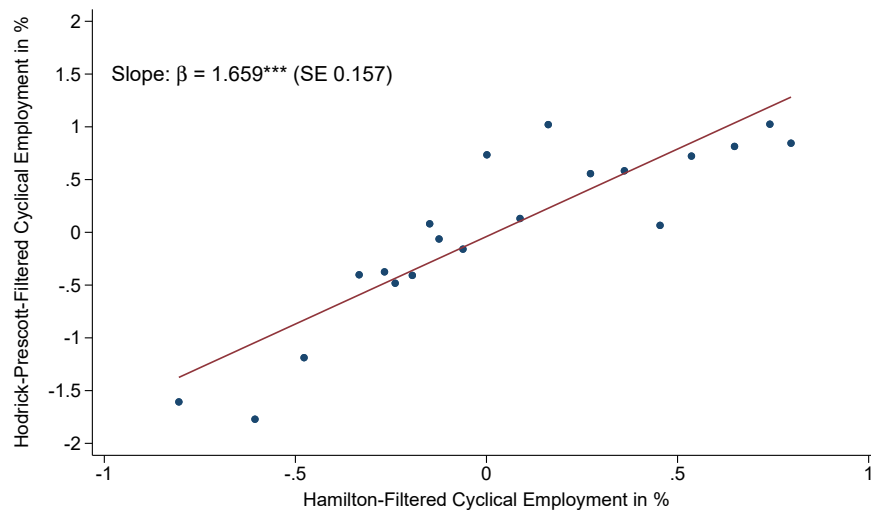


Figure 4.A.1.2: Relationship of the Cyclical Employment Gaps

*Source:* Federal Employment Agency, Own Calculations.

*Note:* This figure presents the relationships of the cyclical employment gaps, based on the Hodrick–Prescott-Filter ( $\lambda = 1600$ ) and the Hamilton-Filter over time. The employment gaps are constructed with the seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage in Germany.

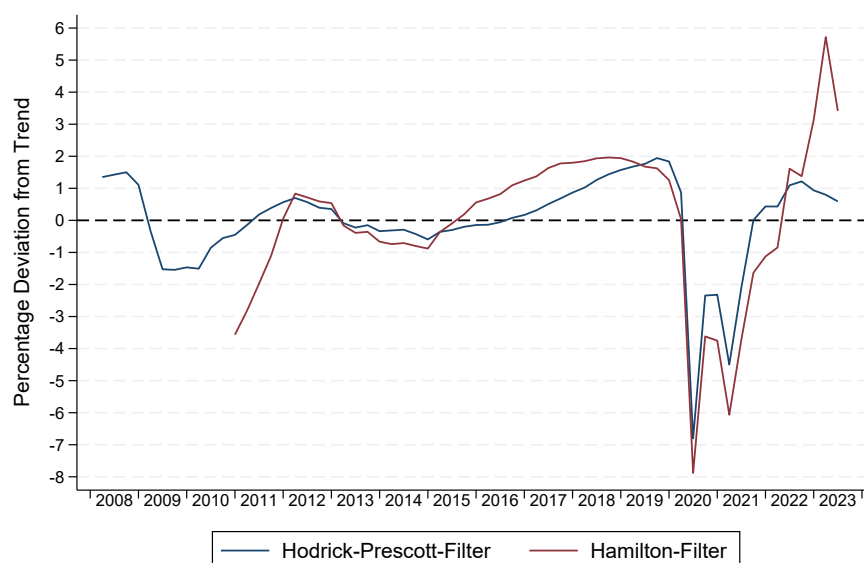


Figure 4.A.1.3: Short-Time Work corrected Cyclical Employment Gaps by Filter Methods over Time

Source: Federal Employment Agency, Own Calculations.

Note: This figure presents the development of the cyclical employment gaps, based on the Hodrick-Prescott-Filter ( $\lambda = 1600$ ) and the Hamilton-Filter over time. The employment gaps are constructed with the seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage in Germany.

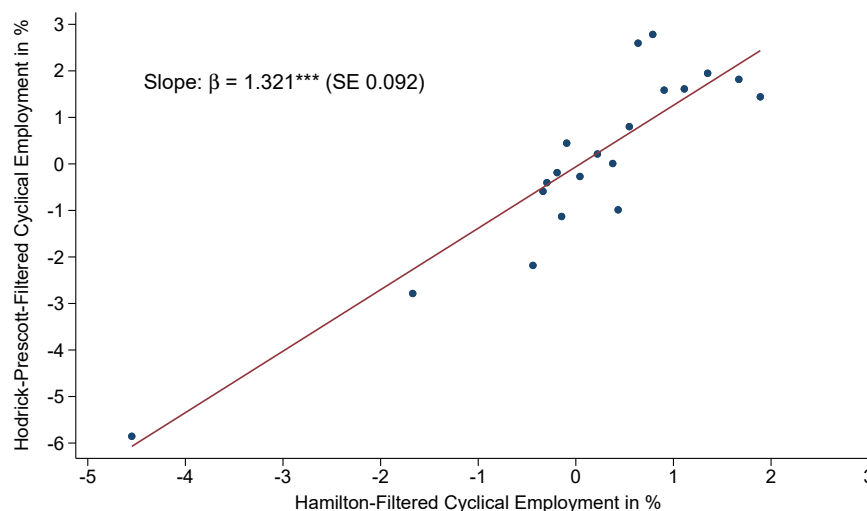


Figure 4.A.1.4: Relationship of the Short-Time Work corrected Cyclical Employment Gaps

Source: Federal Employment Agency, Own Calculations.

Note: This figure presents the relationships of the cyclical employment gaps, based on the Hodrick-Prescott-Filter ( $\lambda = 1600$ ) and the Hamilton-Filter over time. The employment gaps are constructed with the seasonally adjusted log of total employment subject to social security corrected by the employment equivalent of short-time work usage in Germany.

## 4.A.2 Randomization Check

Table 4.A.2.1: Randomization Check 2020

	(1)		(2)		(3)	
	No Membership		Union Membership		Difference	
	Mean	SD	Mean	SD	$\Delta$	t
<b>Order</b>						
First	0.487	0.500	0.513	0.500	-0.025*	(-1.78)
Second	0.513	0.500	0.487	0.500	0.025*	(1.78)
<b>Name</b>						
Name 1	0.503	0.500	0.497	0.500	0.006	(0.42)
Name 2	0.497	0.500	0.503	0.500	-0.006	(-0.42)
<b>Picture</b>						
Picture 1	0.502	0.500	0.498	0.500	0.004	(0.25)
Picture 2	0.498	0.500	0.502	0.500	-0.004	(-0.25)
<b>Layout</b>						
Layout 1	0.494	0.500	0.506	0.500	-0.012	(-0.88)
Layout 2	0.506	0.500	0.494	0.500	0.012	(0.88)
<b>School</b>						
School 1	0.498	0.500	0.502	0.500	-0.004	(-0.25)
School 2	0.502	0.500	0.498	0.500	0.004	(0.25)
<b>Address</b>						
Address 1	0.494	0.500	0.506	0.500	-0.012	(-0.88)
Address 2	0.506	0.500	0.494	0.500	0.012	(0.88)
<b>Membership</b>						
Membership 1	0.503	0.500	0.497	0.500	0.007	(0.48)
Membership 2	0.497	0.500	0.503	0.500	-0.007	(-0.48)
<b>Apprenticeship</b>						
Apprenticeship 1	0.493	0.500	0.507	0.500	-0.014	(-0.99)
Apprenticeship 2	0.507	0.500	0.493	0.500	0.014	(0.99)
<b>Last Employment</b>						
Company 1	0.501	0.500	0.499	0.500	0.003	(0.20)
Company 2	0.499	0.500	0.501	0.500	-0.003	(-0.20)
N	2,503		2,503		5,006	

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

### 4.A.3 Representatives of the Sample

Table 4.A.3.1: Occupation Share on Overall Workforce Germany (2020)

Occupation (KldB 2010 Classification)	2020
Mechatronics Technicians (26)	1,040,281
Logistics Specialists (51)	2,088,828
Sales Assistant (62)	2,101,399
Hotel Manager (63)	767,035
Office Clerk (71)	4,278,130
Sum of used Occupations	10,275,673
Total Workforce	33,322,952
Percentage Share	30.8%

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

*Note:* The data are based on the second quarter of the year and account for employment on all skill levels (Helper, Professional, Specialist, and Expert).



Table 4.A.3.2: Representatives of the Sample by Occupation (2017 to 2020)

	Experiment		Population
	Raw Data	Percent	Employment Share
<b>Overall</b>			
Hotel Manager	1,508	10.99	
Logistics Specialists	2,860	21.57	
Mechatronics Technicians	2,002	14.59	
Office Clerk	4,804	35.01	
Sales Assistant	2,446	17.83	
<b>2017</b>			
Hotel Manager	382	18.35	8.78
Logistics Specialists	578	27.76	22.20
Mechatronics Technicians	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 Specialists	716	21.87	20.28
Mechatronics Technicians	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 Specialists	764	22.75	20.33
Mechatronics Technicians	554	16.50	10.17
Office Clerk	1,164	34.66	41.11
Sales Assistant	540	16.08	20.39
<b>2020</b>			
Hotel Manager	376	7.51	7.46
Logistics Specialists	902	18.02	20.39
Mechatronics Technicians	894	17.86	10.18
Office Clerk	2,018	40.31	41.52
Sales Assistant	816	16.30	20.46

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

*Note:* The table reports the occupational composition of the collected data of by year.

Table 4.A.3.3: Representatives of the Sample by Federal State (2017 to 2020)

	Experiment		Population	
	Raw Data	Percent	Employment Share	GDP Share
<b>Overall</b>				
Baden-Wurttemberg	2,084	15.19		
Bavaria	2,278	16.60		
Berlin	2,598	18.94		
Hamburg	2,578	18.79		
North Rhine-Westphalia	2,614	19.05		
Saxony	1,568	11.43		
<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
<b>2020</b>				
Baden-Wurttemberg	840	16.78	21.90	22.70
Bavaria	730	14.58	26.38	27.66
Berlin	902	18.02	7.15	7.01
Hamburg	866	17.30	4.63	5.35
North Rhine-Westphalia	982	19.62	32.47	31.59
Saxony	686	13.70	7.48	5.69

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

*Note:* The table reports the federal state composition of the collected data of by year.

#### 4.A.4 Comparability of the Experiments

One concern regarding the comparability of both experiments, even when following an identical design in applications, occupations and regions is that the composition of firms and occupations has changed due to the Covid-19 pandemic. Firms and occupations that more severely impacted by the pandemic might be less likely to look for new employees. As a result, when the composition of firms and occupations that discriminated differently changed, this also affect the overall level of discrimination in the experiment.

Figure 4.A.4.1 and Figure 4.A.4.2 therefore summarize the composition of the experiments before and during the Covid-19 pandemic. Figure 4.A.4.1 indicates 3.2 percentage points increase in the share of firms with less than 6 employees and an increase 4.6 percentage points of firms with 51 to 500 employees.<sup>4</sup> Figure 4.A.4.2 shows strong differences in the composition of occupations in the 2020 experiment compared to Nüß (2023) for the period in 2017 to 2019.

These results provide evidence for the need to reweight of the sample for firm and occupation characteristics.

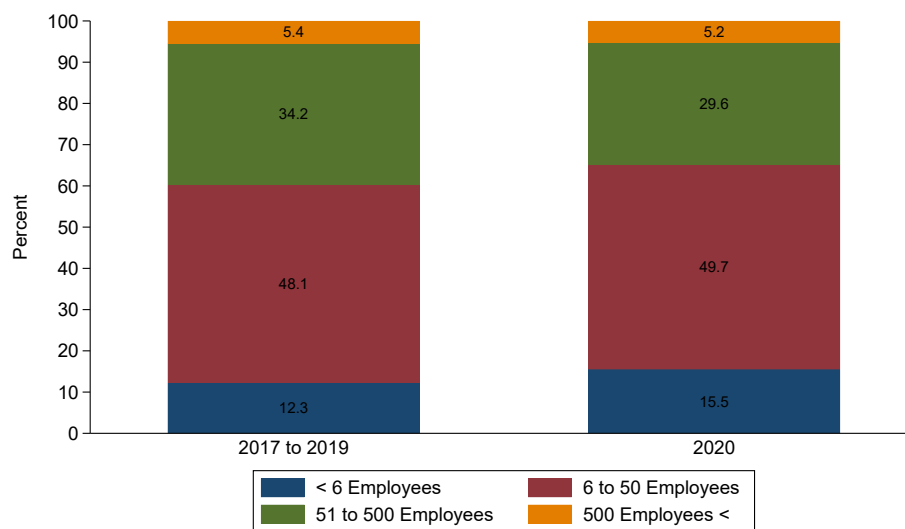


Figure 4.A.4.1: Firm Composition by Firm Size

*Source:* Nüß (2023), Own Data.

*Note:* This figure presents the data composition by occupations for the 2020 data collection relative to the composition by Nüß (2023).

<sup>4</sup>Given the lower levels of discrimination in smaller firms compared to larger firms in Nüß (2023), this should indicate a downward bias in the experiment.

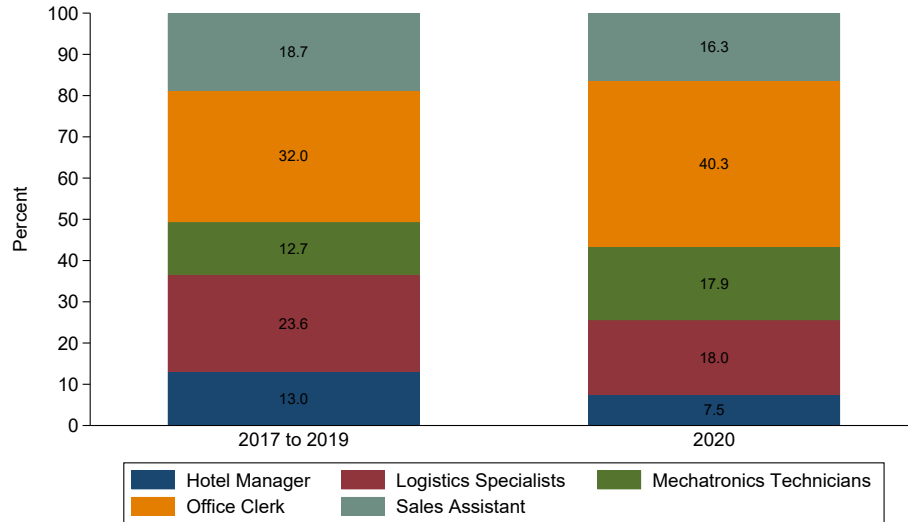


Figure 4.A.4.2: Occupational Composition

Source: Nüß (2023), Own Data.

Note: This figure presents the data composition by firm size for the 2020 data collection relative to the composition by Nüß (2023).

#### 4.A.5 Robustness of the 2020 Results

Table 4.A.5.1: Hiring Discrimination Alternative Estimations (2020)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union Membership	-0.0304*** (0.0085)	-0.0348*** (0.0096)	-0.0303*** (0.0085)	-0.0347*** (0.0095)	-0.0297*** (0.0085)	-0.0343*** (0.0095)	-0.0304** (0.0120)	-0.0348** (0.0135)
Callback Rate Control Group	0.288	0.409	0.288	0.409	0.288	0.409	0.288	0.409
N	5,006	5,006	5,006	5,006	5,006	5,006	5,006	5,006
Adjusted $R^2$ / $PseudoR^2$	0.041	0.050	0.037	0.039	0.037	0.040	0.558	0.527
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Occupation	✓	✓	✓	✓	✓	✓		
Federal State	✓	✓	✓	✓	✓	✓		
Year	✓	✓	✓	✓	✓	✓		
Firm Fixed Effects							✓	✓
Method								
Linear Probability Model	✓	✓					✓	✓
Logit			✓	✓				
Probit					✓	✓		

Note: The table reports results based on the data for 2020, using a linear probability model in Equation 4.1. The dependent variable is the probability of receiving a positive callback following Callback Category 1 and 2. The control variables cover occupation-, sector-, year, and firm-fixed effects. The firm controls cover collective agreement coverage, contract type, and firm size. Application controls cover the application order, layouts, pictures, previous employers, organization membership, name, address, and school. Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.5.2: Sensitivity of Hiring Discrimination to Weighting (2020)

	No Weighting		Weighting by State GDP		Weighting by State Employment		Weighting by Sector Employment		Weighting by Sector Employment of 2018	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Union Membership	-0.0304*** (0.0085)	-0.0348*** (0.0096)	-0.0142 (0.0100)	-0.0246** (0.0113)	-0.0149 (0.0098)	-0.0254** (0.0111)	-0.0224** (0.0101)	-0.0223* (0.0117)	-0.0223** (0.0100)	-0.0226* (0.0116)
Callback Rate Control Group	0.288	0.409	0.275	0.399	0.276	0.399	0.273	0.385	0.274	0.387
N	5,006	5,006	5,006	5,006	5,006	5,006	5,006	5,006	5,006	5,006
Adjusted $R^2$	0.041	0.050	0.036	0.046	0.037	0.047	0.040	0.049	0.039	0.049
Dependent Variables										
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓		✓
Controls										
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports results based on the data collection in 2020, using a linear probability model in Equation 4.1. The dependent variable is the probability of receiving a positive callback following Callback Category 1 and 2. The control variables cover occupation-, state-, and year-fixed effects. *Weighting by State GDP* provides weighting using the federal states' contribution to gross domestic product. *Weighting by State Employment* provides weighting using the federal states' employment share. *Weighting by Sector Employment* provides weighting using the sectoral employment share in 2020. *Weighting by Sector Employment of 2018* provides weighting using the sectoral employment share in 2018. Standard errors, corrected for clustering at the firm level, are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

#### 4.A.6 Firms Awareness of Labor Market Conditions

The analysis and interpretation of labor market conditions for hiring discrimination relies on firms' awareness of current labor market conditions. If firms have no awareness of recent changes in the access to applicants, it can not affect their hiring decisions. In this appendix, I test this by an analysis related to firm size. Firm size is related to the frequency of new hires. Smaller firms have fewer new hires and this irregularity in hiring is likely to affect their awareness of current labor market conditions. Furthermore, larger firms are more likely to have a human resource department, which in turn is more likely to know about current labor market conditions.

Table 4.A.10.4 reports the results of the labor market condition measures, excluding firms with less than six employees. If smaller firms are less aware of labor market conditions, I expect an increase in the effect of labor market conditions on the likelihood of a callback. If this is the case and limited awareness can explain the limited impact of labor market conditions on discrimination, I expect stronger effects of the interaction between union memberships and labor market conditions.

The exclusion of firms with less than six employees increases the relationship of all measures of labor market conditions by 28% to 37%. This supports the idea of limited awareness of the current labor market conditions of smaller firms. Table 4.A.10.4 also reports results for the interaction of union memberships and labor market conditions. Despite the increase in the relationship between labor market conditions and callback rates, there is no change in the effect size of discrimination related to labor market condition measures observable. While these results indicate that limited awareness is at play, there is no indication of its relevance for its relevance of discrimination.

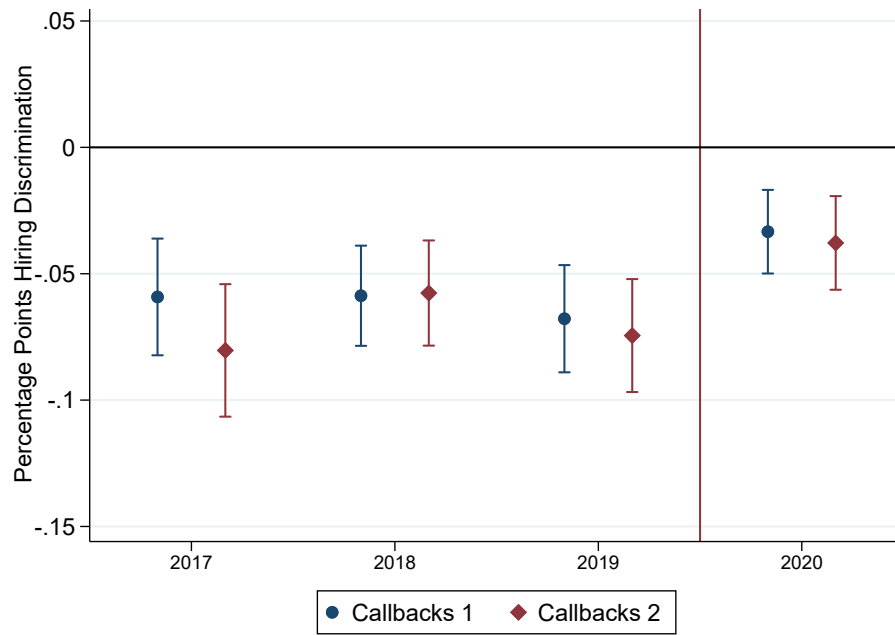
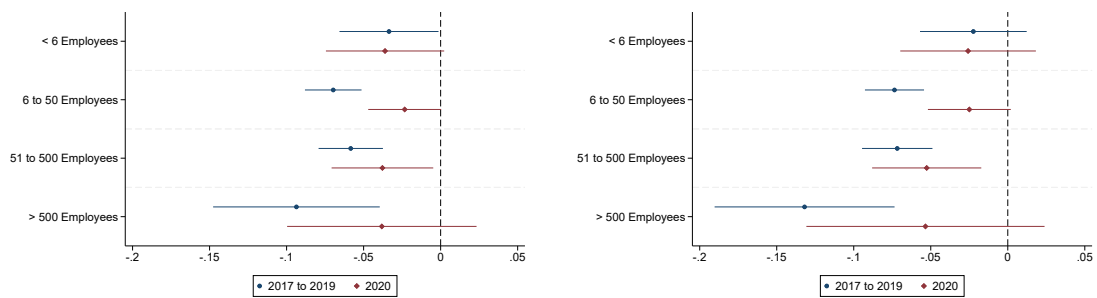


Figure 4.A.5.1: Hiring Discrimination over Time

*Note:* The figure presents estimates of the linear probability model in Equation 4.1, using year specific estimates for the extent of hiring discrimination against union members. The dependent variable is the probability of receiving a positive callback following Callback Category 1 and 2. The baseline controls are regional-, occupation- and year-fixed effects. 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.



(a) Callback Category 1

(b) Callback Category 2

Figure 4.A.6.1: Distribution of Hiring Discrimination by Firm Size

*Note:* The figure presents estimates of the linear probability model based on equation 4.1, accounting for effect heterogeneity by firm size for the 2020 data collection, in comparison to Nüß (2023). 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.

Table 4.A.6.1: Distribution of Hiring Discrimination by Firm Size

	< 6 Employees		6 to 50 Employees		51 to 500 Employees		> 500 Employees	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: Wave 2020</b>								
Union Membership	-0.0361* (0.0197)	-0.0258 (0.0226)	-0.0233* (0.0121)	-0.0249* (0.0137)	-0.0378** (0.0169)	-0.0526*** (0.0181)	-0.0382 (0.0320)	-0.0534 (0.0403)
Callback Rate Control Group	0.294	0.399	0.296	0.415	0.286	0.423	0.202	0.302
N	776	776	2,486	2,486	1,482	1,482	262	262
Adjusted ( $R^2$ )	0.042	0.071	0.041	0.052	0.048	0.057	0.068	0.028
<b>Panel B: Wave 2017 to 2019</b>								
Union Membership	-0.0335** (0.0165)	-0.0223 (0.0178)	-0.0697*** (0.0094)	-0.0735*** (0.0098)	-0.0584*** (0.0107)	-0.0718*** (0.0117)	-0.0936*** (0.0280)	-0.0936*** (0.0280)
Callback Rate Control Group	0.345	0.451	0.387	0.483	0.367	0.482	0.319	0.436
N	1,074	1,074	4,188	4,188	2,982	2,982	470	470
Adjusted $R^2$	0.075	0.074	0.062	0.079	0.082	0.080	0.060	0.060
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Occupation	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* Linear Probability Model; Dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2; Standard errors, corrected for clustering at the sectoral level, are in parentheses. Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.6.2: Labor Market Conditions with 2018 Sectoral Employment Weights

	(1)	(2)	(3)	(4)
Union Membership	-0.0459*** (0.0064)	-0.0521*** (0.0069)	-0.0463*** (0.0064)	-0.0526*** (0.0069)
Union Density	-0.0049 (0.0074)	-0.0069 (0.0081)	-0.0062 (0.0074)	-0.0075 (0.0081)
Union Membership x Union Density	-0.0195*** (0.0075)	-0.0205** (0.0082)	-0.0188** (0.0075)	-0.0200** (0.0082)
V/U Ratio	0.0275* (0.0154)	0.0272* (0.0146)	0.0278* (0.0153)	0.0268* (0.0146)
Union Membership x V/U Ratio	-0.0026 (0.0053)	0.0003 (0.0055)	-0.0027 (0.0053)	0.0003 (0.0055)
Employment Gap HP (STW Corrected)	0.0264** (0.0106)	0.0102 (0.0113)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0143** (0.0060)	-0.0119* (0.0068)		
Employment Gap Ham (STW Corrected)			0.0304*** (0.0113)	0.0181 (0.0120)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0147** (0.0062)	-0.0135* (0.0071)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.064	0.070	0.064	0.071
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* Linear Probability Model; Dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2; Standard errors, corrected for clustering at the sectoral level, are in parentheses; ; Joined Sample of Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 4.A.6.3: Labor Market Condition Mechanisms (excluding Small Firms)

	(1)	(2)	(3)	(4)
Union Membership	-0.0490*** (0.0071)	-0.0582*** (0.0076)	-0.0494*** (0.0071)	-0.0587*** (0.0076)
Union Density	-0.0036 (0.0079)	-0.0097 (0.0086)	-0.0052 (0.0079)	-0.0107 (0.0086)
Union Membership x Union Density	-0.0212*** (0.0081)	-0.0220** (0.0087)	-0.0205** (0.0081)	-0.0214** (0.0086)
V/U Ratio	0.0350** (0.0161)	0.0374** (0.0153)	0.0356** (0.0161)	0.0374** (0.0153)
Union Membership x V/U Ratio	-0.0038 (0.0056)	0.0010 (0.0059)	-0.0040 (0.0056)	0.0011 (0.0059)
Employment Gap HP (STW Corrected)	0.0327*** (0.0112)	0.0193 (0.0120)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0150** (0.0063)	-0.0132* (0.0072)		
Employment Gap Ham (STW Corrected)			0.0374*** (0.0120)	0.0268** (0.0128)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0154** (0.0066)	-0.0156** (0.0075)
N	11,864	11,864	11,864	11,864
Adjusted $R^2$	0.067	0.073	0.067	0.074
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. Firms with fewer than six employees are dropped from this analysis. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

#### 4.A.7 Robustness of Labor Market Condition Mechanisms

Table 4.A.7.1: Labor Market Conditions Previous Quarter

	(1)	(2)	(3)	(4)
Union Membership	-0.0445*** (0.0064)	-0.0509*** (0.0069)	-0.0450*** (0.0064)	-0.0513*** (0.0069)
Union Density	-0.0068 (0.0074)	-0.0075 (0.0081)	-0.0072 (0.0074)	-0.0078 (0.0081)
Union Membership x Union Density	-0.0178** (0.0074)	-0.0188** (0.0081)	-0.0177** (0.0074)	-0.0188** (0.0081)
V/U Ratio (t-1)	0.0230 (0.0147)	0.0220 (0.0142)	0.0227 (0.0147)	0.0217 (0.0142)
Union Membership x V/U Ratio (t-1)	-0.0032 (0.0053)	-0.0009 (0.0055)	-0.0030 (0.0053)	-0.0008 (0.0055)
Employment Gap HP (STW Corrected) (t-1)	0.0127 (0.0091)	-0.0026 (0.0096)		
Union Membership x Employment HP Gap (STW Corrected) (t-1)	0.0127 (0.0091)	-0.0026 (0.0096)		
Employment Gap Ham (STW Corrected) (t-1)			0.0188* (0.0098)	0.0037 (0.0103)
Union Membership x Employment Ham Gap (STW Corrected) (t-1)			-0.0122** (0.0058)	-0.0099 (0.0066)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.064	0.070	0.064	0.070
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio using the second quarter of each year. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter using the second quarter of each year. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.7.2: Labor Market Condition and Collective Agreement Coverage

	Without Collective Agreement				With Collective Agreement			
	(1)	(2)	(3)	(5)	(1)	(6)	(7)	(8)
Union Membership	-0.0461*** (0.0069)	-0.0516*** (0.0075)	-0.0467*** (0.0069)	-0.0524*** (0.0075)	-0.0496*** (0.0160)	-0.0590*** (0.0168)	-0.0495*** (0.0160)	-0.0589*** (0.0168)
Union Density	-0.0024 (0.0075)	-0.0073 (0.0082)	-0.0034 (0.0075)	-0.0076 (0.0082)	-0.0056 (0.0171)	-0.0012 (0.0182)	-0.0075 (0.0170)	-0.0020 (0.0181)
Union Membership x Union Density	-0.0182*** (0.0063)	-0.0204*** (0.0069)	-0.0174*** (0.0063)	-0.0197*** (0.0069)	-0.0174 (0.0188)	-0.0148 (0.0205)	-0.0179 (0.0188)	-0.0156 (0.0205)
V/U Ratio	0.0143 (0.0160)	0.0183 (0.0159)	0.0143 (0.0160)	0.0175 (0.0159)	0.0626** (0.0317)	0.0550* (0.0296)	0.0634** (0.0317)	0.0552* (0.0296)
Union Membership x V/U Ratio	-0.0080 (0.0060)	-0.0042 (0.0062)	-0.0082 (0.0060)	-0.0042 (0.0062)	0.0100 (0.0096)	0.0110 (0.0102)	0.0103 (0.0097)	0.0114 (0.0103)
Employment Gap HP (STW Corrected)	0.0225* (0.0129)	0.0079 (0.0136)			0.0298 (0.0184)	0.0124 (0.0197)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0190*** (0.0070)	-0.0174** (0.0080)			0.0046 (0.0115)	0.0101 (0.0126)		
Employment Gap Ham (STW Corrected)			0.0295** (0.0138)	0.0186 (0.0146)			0.0287 (0.0198)	0.0152 (0.0209)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0192*** (0.0072)	-0.0190** (0.0082)			0.0018 (0.0121)	0.0067 (0.0134)
N	11,180	11,180	11,180	11,180	2,534	2,534	2,534	2,534
Adjusted $R^2$	0.057	0.060	0.057	0.060	0.100	0.124	0.100	0.124
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Occupation	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. Firms with fewer than six employees are dropped from this analysis. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.7.3: Labor Market Conditions Occupation Sensitivity (Hamilton Filter)

	Excluding Hotel Managers		Excluding Logistic Specialists		Excluding Mechatronic Technicians		Excluding Office Clerks		Excluding Sales Assistants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Union Membership	-0.0488*** (0.0070)	-0.0554*** (0.0076)	-0.0406*** (0.0071)	-0.0469*** (0.0078)	-0.0702*** (0.0116)	-0.0827*** (0.0115)	-0.0514*** (0.0085)	-0.0565*** (0.0088)	-0.0429*** (0.0071)	-0.0489*** (0.0076)
Union Density	-0.0036 (0.0076)	-0.0063 (0.0083)	-0.0135 (0.0088)	-0.0127 (0.0099)	-0.0049 (0.0081)	-0.0039 (0.0089)	0.0040 (0.0096)	0.0023 (0.0096)	-0.0091 (0.0078)	-0.0129 (0.0086)
Union Membership x Union Density	-0.0188** (0.0076)	-0.0186** (0.0085)	-0.0130 (0.0092)	-0.0136 (0.0102)	-0.0198** (0.0085)	-0.0208** (0.0093)	-0.0209** (0.0088)	-0.0242*** (0.0086)	-0.0214*** (0.0080)	-0.0229*** (0.0088)
V/U Ratio	0.0280* (0.0154)	0.0252* (0.0146)	0.0256* (0.0155)	0.0239 (0.0148)	0.0566 (0.0525)	0.1427*** (0.0530)	0.0359** (0.0179)	0.0229 (0.0167)	0.0245 (0.0157)	0.0249* (0.0148)
Union Membership x V/U Ratio	-0.0008 (0.0053)	0.0024 (0.0056)	-0.0052 (0.0056)	-0.0017 (0.0060)	-0.0632** (0.0258)	-0.0764*** (0.0260)	0.0019 (0.0058)	0.0058 (0.0058)	-0.0025 (0.0054)	0.0003 (0.0057)
Employment Gap Ham (STW Corrected)	0.0196 (0.0152)	0.0080 (0.0163)	0.0315*** (0.0118)	0.0180 (0.0125)	0.0244** (0.0119)	0.0140 (0.0125)	0.0430*** (0.0145)	0.0281* (0.0149)	0.0330*** (0.0118)	0.0194 (0.0125)
Union Membership x Employment Ham Gap (STW Corrected)	-0.0140* (0.0082)	-0.0128 (0.0095)	-0.0143** (0.0066)	-0.0108 (0.0075)	-0.0057 (0.0072)	-0.0029 (0.0080)	-0.0174** (0.0072)	-0.0150* (0.0080)	-0.0149** (0.0064)	-0.0164** (0.0073)
N	12,206	12,206	10,754	10,754	11,712	11,712	8,916	8,916	11,268	11,268
Adjusted $R^2$	0.064	0.071	0.072	0.078	0.057	0.060	0.032	0.031	0.070	0.078
Dependent Variables										
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓		✓
Controls										
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously, excluding one occupation from the sample at a time. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.7.4: Labor Market Conditions Occupation Sensitivity (Hodrick-Prescott-Filter)

	Excluding Hotel Managers		Excluding Logistic Specialists		Excluding Mechatronic Technicians		Excluding Office Clerks		Excluding Sales Assistants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Union Membership	-0.0482*** (0.0069)	-0.0548*** (0.0074)	-0.0404*** (0.0071)	-0.0467*** (0.0078)	-0.0702*** (0.0116)	-0.0829*** (0.0116)	-0.0511*** (0.0085)	-0.0562*** (0.0088)	-0.0427*** (0.0071)	-0.0485*** (0.0076)
Union Density	-0.0025 (0.0076)	-0.0059 (0.0083)	-0.0118 (0.0089)	-0.0120 (0.0099)	-0.0040 (0.0081)	-0.0037 (0.0089)	0.0039 (0.0096)	0.0013 (0.0096)	-0.0076 (0.0078)	-0.0122 (0.0086)
Union Membership x Union Density	-0.0197*** (0.0076)	-0.0194** (0.0086)	-0.0139 (0.0093)	-0.0142 (0.0102)	-0.0200** (0.0085)	-0.0208** (0.0093)	-0.0209** (0.0088)	-0.0237*** (0.0086)	-0.0221*** (0.0081)	-0.0235*** (0.0088)
V/U Ratio	0.0274* (0.0154)	0.0254* (0.0146)	0.0251 (0.0156)	0.0241 (0.0148)	0.0579 (0.0526)	0.1456*** (0.0530)	0.0352** (0.0179)	0.0227 (0.0166)	0.0239 (0.0157)	0.0251* (0.0148)
Union Membership x V/U Ratio	-0.0007 (0.0053)	0.0024 (0.0056)	-0.0048 (0.0056)	-0.0016 (0.0060)	-0.0637** (0.0258)	-0.0777*** (0.0260)	0.0018 (0.0058)	0.0054 (0.0058)	-0.0022 (0.0054)	0.0004 (0.0056)
Employment Gap HP (STW Corrected)	0.0215 (0.0139)	0.0046 (0.0151)	0.0287*** (0.0111)	0.0114 (0.0118)	0.0182 (0.0112)	0.0037 (0.0118)	0.0301** (0.0139)	0.0125 (0.0143)	0.0311*** (0.0110)	0.0140 (0.0118)
Union Membership x Employment HP Gap (STW Corrected)	-0.0135* (0.0077)	-0.0117 (0.0088)	-0.0151** (0.0063)	-0.0105 (0.0072)	-0.0051 (0.0070)	-0.0015 (0.0078)	-0.0144** (0.0070)	-0.0097 (0.0078)	-0.0155** (0.0061)	-0.0155** (0.0070)
N	12,206	12,206	10,754	10,754	11,712	11,712	8,916	8,916	11,268	11,268
Adjusted $R^2$	0.064	0.071	0.072	0.077	0.057	0.060	0.031	0.031	0.070	0.078
Dependent Variables										
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓		✓
Controls										
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously, excluding one occupation from the sample at a time. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.7.5: Labor Market Conditions Sector Sensitivity for Callback Category 1 (Hamilton-Filter)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Excl. Sec. A	Excl. Sec. B	Excl. Sec. C	Excl. Sec. D	Excl. Sec. E	Excl. Sec. F	Excl. Sec. G	Excl. Sec. H	Excl. Sec. I	Excl. Sec. J	Excl. Sec. K	Excl. Sec. L	Excl. Sec. M	Excl. Sec. N	Excl. Sec. O	Excl. Sec. P	Excl. Sec. Q	Excl. Sec. R	Excl. Sec. S	Excl. Sec. T
<b>Panel A: Callback Category 1</b>																				
Union Membership	-0.0531** (0.0062)	-0.0652*** (0.0064)	-0.0622*** (0.0077)	-0.0614*** (0.0064)	-0.0641*** (0.0064)	-0.0470*** (0.0067)	-0.0415*** (0.0076)	-0.0469*** (0.0066)	-0.0478*** (0.0069)	-0.0468*** (0.0066)	-0.0486*** (0.0063)	-0.0469*** (0.0065)	-0.0446*** (0.0069)	-0.0449*** (0.0068)	-0.0454*** (0.0063)	-0.0454*** (0.0065)	-0.0522*** (0.0064)	-0.0448*** (0.0065)	-0.0464*** (0.0065)	-0.0462*** (0.0064)
Union Density	-0.0062 (0.0077)	-0.0062 (0.0077)	-0.0064 (0.0077)	-0.0064 (0.0077)	-0.0064 (0.0077)	-0.0088 (0.0076)	-0.0222 (0.0079)	-0.0108 (0.0079)	-0.0236 (0.0078)	-0.0352 (0.0075)	-0.0250 (0.0075)	-0.0063 (0.0076)	-0.0085 (0.0076)	-0.0085 (0.0076)	-0.0051 (0.0075)	-0.0076 (0.0075)	-0.0092 (0.0075)	-0.0098 (0.0075)	-0.0070 (0.0075)	-0.0062 (0.0075)
Union Membership x Union Density	-0.0377** (0.0074)	-0.0377** (0.0075)	-0.0377** (0.0105)	-0.0377** (0.0076)	-0.0377** (0.0075)	-0.0377** (0.0076)	-0.0377** (0.0079)	-0.0377** (0.0079)	-0.0377** (0.0078)	-0.0377** (0.0075)	-0.0377** (0.0075)	-0.0377** (0.0076)	-0.0377** (0.0076)	-0.0377** (0.0076)	-0.0377** (0.0075)	-0.0377** (0.0075)	-0.0377** (0.0075)	-0.0377** (0.0075)	-0.0377** (0.0075)	-0.0377** (0.0075)
V/U Ratio	0.0275* (0.0053)	0.0272* (0.0053)	0.0283 (0.0060)	0.0241* (0.0053)	0.0285* (0.0053)	0.0283* (0.0056)	0.0258 (0.0056)	0.0266* (0.0056)	0.0292* (0.0054)	0.0278* (0.0054)	0.0268* (0.0053)	0.0272* (0.0053)	0.0232* (0.0053)	0.0256 (0.0054)	0.0297* (0.0053)	0.0286* (0.0054)	0.035 (0.0056)	0.0269* (0.0053)	0.0272* (0.0053)	0.0272* (0.0053)
Union Membership x V/U Ratio	-0.0028 (0.0053)	-0.0028 (0.0053)	-0.0028 (0.0060)	-0.0031 (0.0053)	-0.0025 (0.0053)	-0.0027 (0.0056)	-0.0026 (0.0056)	-0.0026 (0.0056)	-0.0026 (0.0054)	-0.0026 (0.0054)	-0.0026 (0.0053)	-0.0026 (0.0053)	-0.0026 (0.0053)	-0.0026 (0.0053)	-0.0026 (0.0054)	-0.0026 (0.0053)	-0.0026 (0.0056)	-0.0026 (0.0053)	-0.0026 (0.0053)	-0.0026 (0.0053)
Employment Gap Han (STW Corrected)	0.0306*** (0.0062)	0.0311*** (0.0062)	0.0310*** (0.0062)	0.0287*** (0.0062)	0.0314*** (0.0062)	0.0321*** (0.0062)	0.0307*** (0.0063)	0.0345*** (0.0063)	0.0096 (0.0083)	0.0311*** (0.0082)	0.0345*** (0.0082)	0.0345*** (0.0082)	0.0289*** (0.0082)	0.0306*** (0.0082)	0.0310*** (0.0082)	0.0315*** (0.0082)	0.0303*** (0.0082)	0.0306*** (0.0082)	0.0306*** (0.0082)	0.0311*** (0.0082)
Union Membership x Employment Han Gap (STW Corrected)	-0.0146** (0.0062)	-0.0177** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0177** (0.0062)	-0.0152** (0.0062)	-0.0177** (0.0063)	-0.0152** (0.0063)	-0.0152** (0.0063)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)	-0.0152** (0.0062)
N	13,688	13,714	11,322	13,668	13,602	12,746	9,518	12,998	12,542	13,382	13,392	13,444	12,596	12,880	13,384	13,312	13,166	13,598	13,400	13,714
Adjusted R <sup>2</sup>	0.065	0.065	0.062	0.065	0.065	0.064	0.066	0.066	0.061	0.066	0.066	0.065	0.067	0.066	0.061	0.065	0.065	0.065	0.066	0.065
Dependent Variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Strict Callbacks (Callback Category 1)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously, excluding one sector from the sample at a time. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.7.6: Labor Market Conditions Sector Sensitivity for Callback Category 1 (Hodrick-Prescott-Filter)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Excl. Sec. A	Excl. Sec. B	Excl. Sec. C	Excl. Sec. D	Excl. Sec. E	Excl. Sec. F	Excl. Sec. G	Excl. Sec. H	Excl. Sec. I	Excl. Sec. J	Excl. Sec. K	Excl. Sec. L	Excl. Sec. M	Excl. Sec. N	Excl. Sec. O	Excl. Sec. P	Excl. Sec. Q	Excl. Sec. R	Excl. Sec. S	Excl. Sec. T
<b>Panel B: Callback Category 2</b>																				
Union	-0.0514*** (0.0067)	-0.0524*** (0.0069)	-0.0520*** (0.0083)	-0.0522*** (0.0069)	-0.0532*** (0.0069)	-0.0518*** (0.0072)	-0.0478*** (0.0081)	-0.0521*** (0.0071)	-0.0539*** (0.0075)	-0.0531*** (0.0070)	-0.0536*** (0.0068)	-0.0525*** (0.0070)	-0.0491*** (0.0074)	-0.0535*** (0.0073)	-0.0516*** (0.0068)	-0.0510*** (0.0070)	-0.0603*** (0.0070)	-0.0515*** (0.0070)	-0.0525*** (0.0070)	-0.0524*** (0.0069)
Union Density	-0.0086 (0.0081)	-0.0075 (0.0081)	-0.0082 (0.0100)	-0.0078 (0.0082)	-0.0071 (0.0081)	-0.0109 (0.0083)	-0.0032 (0.0088)	-0.0105 (0.0096)	-0.0029 (0.0083)	-0.0061 (0.0081)	-0.0053 (0.0081)	-0.0078 (0.0082)	-0.0070 (0.0091)	-0.0059 (0.0084)	-0.0119* (0.0072)	-0.0094 (0.0082)	-0.0068 (0.0080)	-0.0072 (0.0081)	-0.0080 (0.0081)	-0.0075 (0.0081)
Union x Union Density	-0.0209*** (0.0081)	-0.0198*** (0.0082)	-0.0197** (0.0115)	-0.0191** (0.0083)	-0.0204*** (0.0083)	-0.0201** (0.0083)	-0.0222*** (0.0086)	-0.0197** (0.0101)	-0.0190** (0.0085)	-0.0194** (0.0082)	-0.0183** (0.0082)	-0.0197** (0.0083)	-0.0229** (0.0092)	-0.0191** (0.0085)	-0.0171*** (0.0064)	-0.0192** (0.0083)	-0.0183** (0.0082)	-0.0203** (0.0082)	-0.0197** (0.0082)	-0.0188** (0.0082)
V/U Ratio	0.0065* (0.0146)	0.0066* (0.0146)	0.0457*** (0.0204)	0.0256* (0.0146)	0.0276* (0.0146)	0.0274* (0.0158)	0.0272* (0.0154)	0.0256* (0.0148)	0.0285* (0.0146)	0.0265* (0.0150)	0.0243* (0.0147)	0.0267* (0.0146)	0.0265* (0.0151)	0.0258* (0.0151)	0.0266** (0.0091)	0.0272* (0.0146)	0.0179 (0.0146)	0.0258* (0.0146)	0.0240* (0.0148)	0.0246* (0.0146)
Union x V/U Ratio	0.0004 (0.0055)	0.0002 (0.0055)	0.0023 (0.0068)	-0.0002 (0.0066)	0.0004 (0.0066)	0.0007 (0.0059)	-0.0020 (0.0058)	0.0011 (0.0059)	0.0006 (0.0056)	-0.0009 (0.0056)	0.0009 (0.0055)	0.0000 (0.0055)	0.0001 (0.0055)	0.0007 (0.0056)	0.0001 (0.0053)	-0.0001 (0.0050)	0.0011 (0.0050)	0.0002 (0.0050)	0.0002 (0.0056)	0.0002 (0.0055)
Employment Gap Ham (STW Corrected)	0.0183 (0.0121)	0.0189 (0.0121)	0.0222* (0.0121)	0.0181 (0.0121)	0.0192 (0.0121)	0.0210* (0.0124)	0.0182 (0.0124)	0.0204* (0.0121)	-0.0074 (0.0164)	0.0188 (0.0121)	0.0247** (0.0120)	0.0185 (0.0121)	0.0178 (0.0125)	0.0283* (0.0142)	0.0185 (0.0121)	0.0289* (0.0121)	0.0105 (0.0123)	0.0180 (0.0123)	0.0211* (0.0123)	0.0189 (0.0121)
Union x Employment Ham Gap (STW Corrected)	-0.0134* (0.0070)	-0.0134* (0.0070)	-0.0134* (0.0070)	-0.0130* (0.0071)	-0.0136* (0.0070)	-0.0129* (0.0071)	-0.0137* (0.0072)	-0.0124* (0.0072)	-0.0178* (0.0065)	-0.0129* (0.0071)	-0.0135* (0.0070)	-0.0138* (0.0070)	-0.0136* (0.0075)	-0.0142* (0.0075)	-0.0128* (0.0069)	-0.0121* (0.0070)	-0.0140* (0.0070)	-0.0131* (0.0071)	-0.0128* (0.0070)	-0.0134* (0.0070)
N	13,688	13,714	11,322	13,668	13,602	12,746	9,518	12,998	12,542	13,302	13,592	13,444	12,596	12,880	13,584	13,412	13,146	13,598	13,400	13,714
Adjusted R <sup>2</sup>	0.071	0.071	0.064	0.071	0.071	0.069	0.074	0.071	0.069	0.073	0.073	0.071	0.071	0.074	0.071	0.070	0.070	0.071	0.072	0.071
Dependent Variables																				
Basic Callbacks (Callback Category 2)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls																				
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously, excluding one sector from the sample at a time. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Niiß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the sectoral level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

#### 4.A.8 Heterogeneity by Firm Characteristics

Table 4.A.8.1: Distribution of Hiring Discrimination by Collective Agreement

	No Collective Agreement		With Collective Agreement	
	(1)	(2)	(3)	(4)
<b>Wave 2020</b>				
Union Membership	-0.0282*** (0.0094)	-0.0321*** (0.0105)	-0.0411** (0.0204)	-0.0483** (0.0235)
Average Callback Rate	0.292	0.417	0.269	0.367
N	4,178	4,178	828	828
Adjusted $R^2$	0.039	0.048	0.069	0.077
<b>Wave 2017 to 2019</b>				
Union Membership	-0.0691*** (0.0071)	-0.0751*** (0.0076)	-0.0386*** (0.0142)	-0.0504*** (0.0148)
Callback Rate Control Group	0.369	0.475	0.378	0.478
N	7,006	7,006	1,708	1,708
Adjusted $R^2$	0.060	0.066	0.111	0.126
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* Linear Probability Model; Dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2; Standard errors, corrected for clustering at the sectoral level, are in parentheses; ; Joined Sample of Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. All estimates are reweighted to their sectoral employment share of 2018.  
\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

#### 4.A.9 Robustness Alternative Labor Market Condition Measure

Table 4.A.9.1: Alternative Labor Market Conditions using Callbacks by Sector and Year

	(1)	(2)	(3)	(4)
Union Membership	-0.0603*** (0.0069)	-0.0688*** (0.0076)	-0.0542*** (0.0066)	-0.0625*** (0.0072)
Alternative Labor Market Tightness	0.0825*** (0.0085)	0.0863*** (0.0086)	0.0827*** (0.0085)	0.0864*** (0.0086)
Union Membership x Alternative Labor Market Tightness	-0.0343*** (0.0073)	-0.0363*** (0.0079)	-0.0341*** (0.0073)	-0.0360*** (0.0079)
Union Density			-0.0071 (0.0071)	-0.0085 (0.0078)
Union Membership x Union Density			-0.0183** (0.0071)	-0.0188** (0.0077)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.078	0.088	0.079	0.089
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model using the alternative labor market condition measure. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. The alternative measure of labor market tightness is based on the average callback rates (Category 1 and 2) of non-union members by sector for each year. Standard errors, corrected for clustering at the firm level, are in parentheses. Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. Alternative measures of labor market conditions are standardized with a mean of 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 4.A.9.2: Alternative Skill Requirements for Vacancy/Unemployment-Ratios

	HP-Filter						Hamilton-Filter					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Union	-0.0458*** (0.0064)	-0.0520*** (0.0069)	-0.0459*** (0.0064)	-0.0520*** (0.0068)	-0.0520*** (0.0069)	-0.0520*** (0.0069)	-0.0462*** (0.0064)	-0.0524*** (0.0069)	-0.0463*** (0.0064)	-0.0525*** (0.0069)	-0.0462*** (0.0064)	-0.0462*** (0.0064)
Union Density WZ08 SOEP 2019 standardized	-0.0049 (0.0074)	-0.0070 (0.0081)	-0.0050 (0.0075)	-0.0070 (0.0081)	-0.0070 (0.0081)	-0.0070 (0.0081)	-0.0062 (0.0074)	-0.0076 (0.0081)	-0.0063 (0.0074)	-0.0076 (0.0081)	-0.0062 (0.0074)	-0.0062 (0.0074)
Union x Union Density WZ08 SOEP 2019 standardized	-0.0194** (0.0075)	-0.0202** (0.0082)	-0.0191** (0.0076)	-0.0201** (0.0083)	-0.0202** (0.0082)	-0.0202** (0.0082)	-0.0187** (0.0075)	-0.0197** (0.0082)	-0.0185** (0.0076)	-0.0196** (0.0083)	-0.0188** (0.0075)	-0.0188** (0.0075)
V/U Ratio Std. Total	0.0265* (0.0151)	0.0263* (0.0145)					0.0268* (0.0151)	0.0259* (0.0145)				
Union x V/U Ratio Std. Gesamt	-0.0027 (0.0053)	-0.0001 (0.0055)					-0.0029 (0.0053)	-0.0001 (0.0055)				
V/U Ratio Std. Voc. Training			0.0231 (0.0146)	0.0233 (0.0144)					0.0235 (0.0146)	0.0229 (0.0144)		
Union x V/U Ratio Std. Voc. Training			-0.0038 (0.0056)	-0.0006 (0.0058)					-0.0040 (0.0056)	-0.0006 (0.0058)		
V/U Ratio Std. exc. Expert					0.0283* (0.0148)	0.0283* (0.0148)					0.0274* (0.0154)	0.0274* (0.0154)
Union x V/U Ratio Std. Exc. Experts					0.0000 (0.0055)	0.0000 (0.0055)					-0.0027 (0.0053)	-0.0027 (0.0053)
Employment Gap HP (STW Corrected)	0.0269** (0.0107)	0.0109 (0.0114)	0.0268** (0.0107)	0.0109 (0.0114)	0.0108 (0.0114)	0.0108 (0.0114)	0.0310*** (0.0114)	0.0189 (0.0121)	0.0309*** (0.0114)	0.0188 (0.0120)	0.0309*** (0.0114)	0.0309*** (0.0114)
Union x Employment HP Gap (STW Corrected)	-0.0142** (0.0059)	-0.0117* (0.0068)	-0.0139** (0.0060)	-0.0116* (0.0068)	-0.0117* (0.0068)	-0.0117* (0.0068)	-0.0146** (0.0062)	-0.0133* (0.0070)	-0.0143** (0.0062)	-0.0132* (0.0071)	-0.0146** (0.0062)	-0.0146** (0.0062)
N	13714	13714	13714	13714	13714	13714	13714	13714	13714	13714	13714	13714
Adjusted $R^2$	0.064	0.071	0.064	0.071	0.071	0.071	0.065	0.071	0.064	0.071	0.065	0.065
Dependent Variables												
Strict Callbacks (Callback Category 1)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Basic Callbacks (Callback Category 2)												
Controls												
Occupation	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously.

#### 4.A.10 Nonlinear Effects

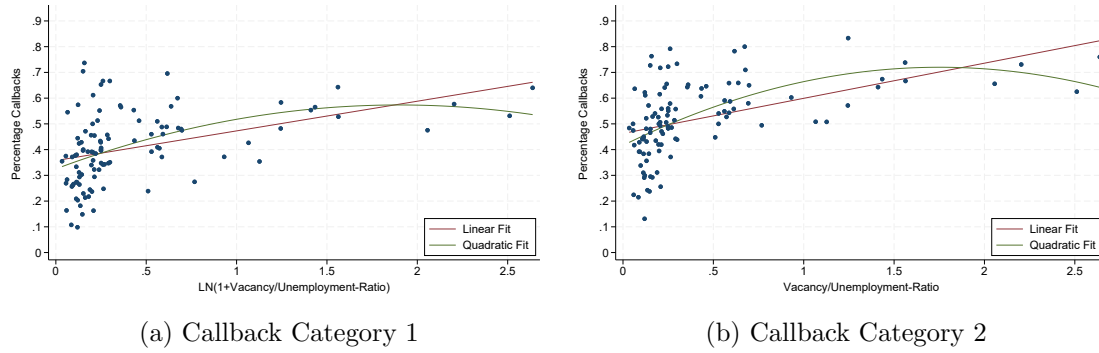


Figure 4.A.10.1: Relationship of Callbacks and Vacancy/Unemployment-Ratio

*Note:* The figures present the relationship of the percentage points of callback rates by region and occupation with the loglinearized vacancy/unemployment-ratio. Panel A: shows the relationship in case of callback category 1. Panel B: shows the relationship in case of callback category 2.

Table 4.A.10.1: Labor Market Condition with Linearized Mechanisms - LN V/U-Ratio

	(1)	(2)	(3)	(4)
Union Membership	-0.0459*** (0.0064)	-0.0521*** (0.0069)	-0.0463*** (0.0064)	-0.0525*** (0.0069)
Union Density	-0.0050 (0.0074)	-0.0073 (0.0081)	-0.0063 (0.0074)	-0.0078 (0.0081)
Union Membership x Union Density	-0.0192** (0.0075)	-0.0200** (0.0083)	-0.0185** (0.0075)	-0.0195** (0.0082)
LN V/U Ratio	0.0386* (0.0209)	0.0517** (0.0206)	0.0389* (0.0209)	0.0510** (0.0205)
Union Membership x LN V/U Ratio	-0.0039 (0.0056)	-0.0016 (0.0059)	-0.0040 (0.0056)	-0.0015 (0.0060)
Employment Gap HP (STW Corrected)	0.0266** (0.0107)	0.0100 (0.0114)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0139** (0.0060)	-0.0114* (0.0068)		
Employment Gap Ham (STW Corrected)			0.0306*** (0.0115)	0.0180 (0.0121)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0143** (0.0062)	-0.0130* (0.0071)
N	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.064	0.071	0.065	0.071
Dependent Variables				
Strict Callbacks (Callback Category 1)	✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓
Controls				
Occupation	✓	✓	✓	✓
Federal State	✓	✓	✓	✓
Year	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the log linearized occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1,600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to the sectoral employment share. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.10.2: Labor Market Condition Median Split Vacancy/Unemployment-Ratio

	Below Median V/U-Ratio				Above Median V/U-Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union Membership	-0.0944 (0.0676)	-0.1345* (0.0804)	-0.1010 (0.0672)	-0.1353* (0.0795)	-0.0555*** (0.0102)	-0.0714*** (0.0108)	-0.0556*** (0.0101)	-0.0716*** (0.0107)
Union Density	-0.0159 (0.0097)	-0.0145 (0.0116)	-0.0174* (0.0097)	-0.0150 (0.0116)	0.0030 (0.0110)	-0.0036 (0.0110)	0.0023 (0.0110)	-0.0035 (0.0110)
Union Membership x Union Density	-0.0044 (0.0099)	-0.0062 (0.0118)	-0.0041 (0.0098)	-0.0065 (0.0117)	-0.0335*** (0.0110)	-0.0330*** (0.0111)	-0.0332*** (0.0110)	-0.0327*** (0.0111)
V/U Ratio	0.4405* (0.2261)	0.6161** (0.2438)	0.4445** (0.2258)	0.6095** (0.2437)	0.0579** (0.0240)	0.0299 (0.0226)	0.0581** (0.0240)	0.0300 (0.0226)
Union Membership x V/U Ratio	-0.1073 (0.1263)	-0.1834 (0.1500)	-0.1213 (0.1258)	-0.1855 (0.1483)	0.0073 (0.0066)	0.0150** (0.0067)	0.0073 (0.0066)	0.0149** (0.0067)
Employment Gap HP (STW Corrected)	0.0222 (0.0141)	-0.0042 (0.0156)			0.0189 (0.0169)	0.0060 (0.0171)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0075 (0.0101)	0.0046 (0.0120)			-0.0152* (0.0085)	-0.0159* (0.0093)		
Employment Gap Ham (STW Corrected)			0.0282* (0.0151)	0.0072 (0.0166)			0.0214 (0.0178)	0.0107 (0.0180)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0040 (0.0104)	0.0054 (0.0122)			-0.0189** (0.0088)	-0.0196** (0.0097)
N	6,864	6,864	6,864	6,864	6,850	6,850	6,850	6,850
Adjusted $R^2$	0.043	0.042	0.044	0.042	0.052	0.060	0.052	0.060
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Occupation	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. Columns (1) to (4) analysis a subsample of observations with below median vacancy/unemployment-ratio. Columns (5) to (8) analysis a subsample of observations with above median vacancy/unemployment-ratio. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.10.3: Labor Market Condition Median Split by Union Density

	Below Median Union Density				Above Median Union Density			
	(1)	(2)	(3)	(5)	(1)	(6)	(7)	(8)
Union Membership	-0.0811*** (0.0189)	-0.0670*** (0.0204)	-0.0814*** (0.0189)	-0.0675*** (0.0204)	-0.0224* (0.0125)	-0.0325** (0.0136)	-0.0229* (0.0124)	-0.0334*** (0.0135)
Union Density	0.0619* (0.0321)	0.0618* (0.0344)	0.0568* (0.0320)	0.0575* (0.0343)	0.0078 (0.0126)	0.0111 (0.0140)	0.0079 (0.0125)	0.0121 (0.0139)
Union Membership x Union Density	-0.0434 (0.0268)	-0.0226 (0.0284)	-0.0428 (0.0269)	-0.0225 (0.0284)	-0.0370*** (0.0120)	-0.0356*** (0.0131)	-0.0364*** (0.0120)	-0.0350*** (0.0131)
V/U Ratio	0.0133 (0.0252)	0.0269 (0.0245)	0.0133 (0.0251)	0.0266 (0.0244)	0.0341* (0.0183)	0.0312* (0.0174)	0.0348* (0.0181)	0.0300* (0.0173)
Union Membership x V/U Ratio	-0.0008 (0.0112)	0.0108 (0.0103)	-0.0012 (0.0112)	0.0107 (0.0102)	-0.0024 (0.0061)	-0.0014 (0.0066)	-0.0023 (0.0062)	-0.0011 (0.0067)
Employment Gap HP (STW Corrected)	0.0339*** (0.0107)	0.0284** (0.0112)			-0.0007 (0.0228)	-0.0260 (0.0241)		
Union Membership x Employment HP Gap (STW Corrected)	-0.0122* (0.0069)	-0.0085 (0.0079)			-0.0179 (0.0113)	-0.0186 (0.0129)		
Employment Gap Ham (STW Corrected)			0.0431*** (0.0113)	0.0394*** (0.0119)			-0.0099 (0.0265)	-0.0245 (0.0277)
Union Membership x Employment Ham Gap (STW Corrected)			-0.0121* (0.0068)	-0.0093 (0.0078)			-0.0206 (0.0128)	-0.0232 (0.0146)
N	7,732	7,732	7,732	7,732	5,982	5,982	5,982	5,982
Adjusted $R^2$	0.060	0.059	0.061	0.061	0.067	0.079	0.067	0.078
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Occupation	✓	✓	✓	✓	✓	✓	✓	✓
Federal State	✓	✓	✓	✓	✓	✓	✓	✓
Year	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. Columns (1) to (4) analysis a subsample of observations with below median union density. Columns (5) to (8) analysis a subsample of observations with above median union density. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4.A.10.4: Labor Market Condition using Firm Fixed-Effects

	Robust Standard Errors				Clustered at the Firm- Level			
	(1)	(2)	(3)	(5)	(1)	(6)	(7)	(8)
Union Membership	-0.0458*** (0.0064)	-0.0519*** (0.0069)	-0.0462*** (0.0064)	-0.0524*** (0.0069)	-0.0458*** (0.0090)	-0.0519*** (0.0097)	-0.0462*** (0.0091)	-0.0524*** (0.0097)
Union Membership x Union Density	-0.0194*** (0.0075)	-0.0203** (0.0082)	-0.0187** (0.0075)	-0.0198** (0.0082)	-0.0194* (0.0106)	-0.0203* (0.0117)	-0.0187* (0.0106)	-0.0198* (0.0116)
Union Membership x V/U Ratio	-0.0026 (0.0053)	0.0002 (0.0055)	-0.0028 (0.0053)	0.0002 (0.0055)	-0.0026 (0.0075)	0.0002 (0.0078)	-0.0028 (0.0075)	0.0002 (0.0078)
Union Membership x Employment HP Gap (STW Corrected)	-0.0142** (0.0059)	-0.0118* (0.0067)			-0.0142* (0.0084)	-0.0118 (0.0095)		
Union Membership x Employment Ham Gap (STW Corrected)			-0.0147** (0.0062)	-0.0134* (0.0070)			-0.0147* (0.0087)	-0.0134 (0.0099)
N	13,714	13,714	13,714	13,714	13,714	13,714	13,714	13,714
Adjusted $R^2$	0.600	0.577	0.600	0.577	0.600	0.577	0.600	0.577
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓
Controls								
Firm Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓

*Note:* The table reports estimates of the linear probability model based on equation 4.2 including all measures of labor market conditions simultaneously. Columns (1) to (4) analysis a subsample of observations with below median vacancy/unemployment-ratio. Columns (5) to (8) analysis a subsample of observations with above median vacancy/unemployment-ratio. The dependent variable is the probability of receiving a positive callback follows Callback Category 1 and 2. Own sample and the sample from Nüß (2023). Labor market tightness is measured by the occupation-specific regional vacancy/unemployment-ratio. The employment gap is measured by the sector-specific (NACE2) employment gap resulting from the Hodrick-Prescott-Filter ( $\lambda = 1.600$ ) and the Hamilton-Filter. The short-time work corrected employment gap is measured by sector-specific employment minus the sector-specific employment equivalent of short-time work usage during the period. Standard errors, corrected for clustering at the firm level, are in parentheses. Union density, vacancy/unemployment-ratio, and the employment gaps are all standardized with mean 0 and a standard deviation of 1. All estimates are reweighted to their sectoral employment share of 2018. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

#### 4.A.11 Example Application

[illegible]

Figure 4.A.11.1: Example Application of Experiment in 2020

*Note:* The figure presents an example resume of an applicant for a office clerk position of the 2020 wave of the experiment.

## 4.A.12 Plausibility of the Covid-19 Shock Exogeneity

Table 4.A.12.1: Discrimination 2019 and Business Cycle in 2020

	HP-Filter				Hamilton-Filter			
	(1)	(2)	(3)	(5)	(1)	(6)	(7)	(8)
Employment Gap HP (STW Corrected)	-0.0180 (0.0140)	-0.0319* (0.0156)	-0.0076 (0.0219)	-0.0181 (0.0197)				
Employment Gap Ham (STW Corrected)					-0.0237* (0.0123)	-0.0318* (0.0172)	-0.0136 (0.0196)	-0.0175 (0.0201)
Union Density			-0.0302 (0.0430)	-0.0400 (0.0379)			-0.0277 (0.0429)	-0.0391 (0.0381)
Constant	-0.0949** (0.0364)	-0.1244*** (0.0302)	-0.0865* (0.0450)	-0.1134*** (0.0323)	-0.0927** (0.0368)	-0.1237*** (0.0302)	-0.0855* (0.0452)	-0.1135*** (0.0325)
N	18	18	18	18	18	18	18	18
Adjusted $R^2$	-0.041	0.030	-0.041	0.131	-0.020	0.041	-0.032	0.132
Dependent Variables								
Strict Callbacks (Callback Category 1)	✓		✓		✓		✓	
Basic Callbacks (Callback Category 2)		✓		✓		✓		✓

*Note:* The table reports estimates of the relationship between the pre-pandemic extent of discrimination in 2019 and the sectoral economic downturn during 2020. The dependent variable is the raw average difference in callbacks between the union and non-union applicants in 2019 for both callback categories, the year before the Covid-19 pandemic shock. Robust standard errors in parenthesis. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



## Bibliography

Nüß, P. (2023). Management Opposition, Strikes and Union Threat. *Available at SSRN* (4560917).

# Chapter 5

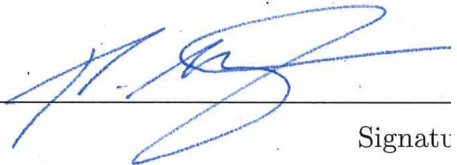
## Declarations

## 5.1 My Declaration

I hereby declare that I have produced my doctoral thesis, "*Essays on Management Opposition and Unionization*" independently and without external assistance, and that I have made a significant contribution as co-author to other scientific articles. I have identified all word-for-word quotations of other authors, as well as comments based closely on other authors' ideas, and I have cited the sources according to the guidelines I received.

Kiel, 27.11.2024

Date, Place



Signature

## 5.2 Declaration on Authors' Contribution to Dissertation Chapter 3

*Authors of chapter:* Chantal Pezold, Simon Jäger, Patrick Nüß

*Title of the chapter:* Labor Market Tightness and Union Activity

*Contributions:* We confirm that all authors contributed equally and substantially to all the stages of the research process including the development of the research question, data analysis, and preparation of the manuscript.

We also confirm that the co-authorship of the manuscript is in compliance to the DFG "Sicherung guter wissenschaftlicher Praxis" (Securing good scientific conduct).

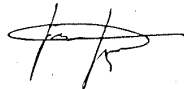
Date: May 13th, 2024.

Signed:

Chantal Pezold



Simon Jäger



Patrick Nüß

