

Labor Market Tightness and Union Activity*

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Abstract

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 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 membership, union elections, and strikes. Overall, our results challenge the notion that labor market tightness significantly drives U.S. unionization.

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1 Introduction

A long-standing idea is that improvements in outside options affect employment relations and behavior in the labor market (Hirschman, 1970; Acemoglu and Wolitzky, 2011; 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; Naidu, 2022; Katz, 2022)¹. 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. 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.² 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.

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

²Davis 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 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 Jäger et al., forthcoming, 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 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 1.97%, 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. A 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.

Our rich survey data allows us to shed light on potential mechanisms that can explain our 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, Greenwood, and Hyatt, 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, Sacerdote, and Scheinkman, 2003; Naidu, 2022; Hertel-Fernandez et al., 2020).

To complement our experimental, worker-level results and further investigate such spillover effects, we draw on three different research designs to analyze more 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, Dorn, and Hanson (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 and Siegel, 2021; Böhm et al., 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. 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 (Lipset and Schneider, 1983; Jarley and Kuruvilla, 1994; Western, 1997; Clawson and Clawson, 1999; Wallerstein and Western, 2000; Farber and Western, 2002; Scruggs and Lange, 2002; Southworth and Stepan-Norris, 2009; Hirsch, 2008; Farber and Western, 2002; Tope and Jacobs, 2009; Ahlquist and Downey, 2023; Pezold, 2023). 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, 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 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; Schmitt and Zipperer, 2009; Nüß, 2023b), 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, 2023a). 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 (Beaudry and DiNardo, 1991; Hagedorn and Manovskii, 2013; Jäger et al., 2020; Autor, Dube, and McGrew, 2023).

The remainder of this paper is structured as follows. Section 2 reviews the U.S. institutional context. Section 3 presents macro evidence from U.S. time series data as well as from a panel of OECD countries. Section 4 introduces our experimental design to study micro-level effects of labor market conditions on unionization decisions and presents our experimental results. In Section 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 6. Section 7 concludes.

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 unioniza-

tion 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.³ 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

³NLRA Section 7 and 8(a)1.

practices⁴. 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 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 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 expectations and the macroeconomic environment (see, e.g., Davis and von Wachter, 2011; Roth and Wohlfart, 2020; He and Kircher, 2023), we find a visibly procyclical movement of the subjective job finding rate.⁵ 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.

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

⁴An 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.

⁵We 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 cyclicity of this variable.

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.

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.

4.1 Data Collection and Sample

Data Collection. We collected the survey data in the U.S. between May 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 1 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 72% 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 A-6, 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 76% participants perceived the survey as non-partisan, and only 7.5% 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 A-7).

4.2 Experimental Design

We include the full questionnaire in Appendix Section A-3. The study was pre-registered in the AsPredicted registry⁶ and ethics approval was granted by the Christian-Albrechts-University Kiel.⁷ Figure 3 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 \$2.⁸

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 A-8 that provides them personalized information about labor market tightness in their narrow labor market cell. In particular, they are shown a

⁶The pre-analysis plan is available here: https://aspredicted.org/XWD_9NB

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

⁸The 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.

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 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.⁹ 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.

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

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 4. 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,

⁹Note 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.

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.

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:

$$Y_i = \beta_0 + \beta_1 T_i \times \underbrace{(\text{Belief}_i^{\text{pre}} - \text{Benchmark}_i)}_{\text{Misperception}_i^{\text{pre}}} + \phi T_i + \theta \text{Misperception}_i^{\text{pre}} + \Theta X_i + u_i, \quad (1)$$

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.¹⁰

Next, to facilitate the interpretation of the causal effects, we follow Jäger et al. (forthcoming) 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:

$$\text{Belief}_i^{\text{post}} = \pi_0 + \pi_1 (T_i \times \text{Misperception}_i^{\text{pre}}) + \lambda T_i + \zeta \text{Misperception}_i^{\text{pre}} + \Theta' X_i + \epsilon_i \quad (2)$$

$$Y_i = \gamma_0 + \gamma_1 \widehat{\text{Belief}}_i^{\text{post}} + \rho \text{Misperception}_i^{\text{pre}} + \Gamma X_i + v_i \quad (3)$$

where Equation 2 denotes the first stage of the 2SLS framework, and Equation 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

¹⁰The 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, γ_1 .

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 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 A-3, which shows substantial misperceptions (SD 28.12). We also document systematic heterogeneity in misperceptions across demographic groups in Appendix Figure A-4.

4.4 Intervention Check: Effects on Worker Beliefs About Labor Market Tightness

Figure 5 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.

In Figure 5 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 0.9 (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 5 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 (-0.013; 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 0.01) while the treatment group substantially changes beliefs as a consequence of treatment so that beliefs (indicated by a lower persistence coefficient of 0.29, SE 0.01). 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 0.096 (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).

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 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 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 2 Column (1) presents residualized estimates corresponding to the intervention check visualized in Figure 5a. As a reminder from the discussion around this figure, our negative coefficient (-0.5752; SE 0.0216)

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 5.56ppt.

First Stage. We present our first stage estimates in Column (2) of table 2, where our main coefficient of interest, corresponding to π_1 in Equation 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 -0.5752 (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 5.7ppt.

Causal Effects on Union Activity. Table 2 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). 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 1) amounting to about 10ppt.¹¹

Column (3) presents an IV coefficient of 0.1033 (SE 0.0508), implying that moving from trough to peak of the business cycle would cause a 1.03ppt increase in a typical worker’s willingness to vote “yes” in a union election, and a 1.97% 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 laid off can be challenging. Here, we report an IV coefficient of 0.1141 (SE 0.0503), which is equivalent to an 1.14ppt 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 0.0996 (SE 0.0477), corresponding to a 0.996ppt increase in organizing drives when moving

¹¹We 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).

from trough to peak of the business cycle.

For strike activities, Column (6) reports an IV coefficient of 0.0781 (SE 0.0474), implying that a 10ppt increase in worker beliefs about labor market tightness leads to a 0.781ppt 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 0.0041 (SE 0.0017), suggesting a 0.041SD 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 0.1858 (SE 0.0496), implying that a 10ppt increase in subjective job finding probability raises workers' interest in looking for new jobs by 1.89ppt. Relatedly, we confirm that worker's concerns about finding a new job after an unexpected layoff decreases (-0.0068; 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 -0.0005 (SE 0.0028), suggesting that moving from trough to peak of the business cycle reduces worker beliefs about management opposition by -0.005ppt, 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 (-0.0658; SE 0.0520), implying that changing beliefs about local labor market conditions by 10ppt, equivalent to moving from recession

to boom, changes workers’ beliefs about being fired for engaging in union activities by 0.6 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 6. 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.

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.¹² We measured willingness to pay for each of the brochures in an incentive-compatible way, following the idea of the Becker-DeGroot-Marschak mechanism.

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

We report results in Table 3 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 4, 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).

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 μ denote workers' beliefs about the wage premium, defined as the difference between the wage when their establishment unionizes versus if it remains non-unionized, $w_u - w_n$.¹³ 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 ϵ 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, θ , 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:

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

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:

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

¹³Here, we let wages be independent of tightness as is frequently assumed (see Lalive, Landais, and Zweimüller, 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.

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.

6 Equilibrium Effects: Evidence from 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, Dorn, and Hanson, 2013). To study labor supply shocks, we instrument for future retirements (Böhm and Siegel, 2021; Böhm et al., 2021) and labor market entrants with the lagged (local) age structure.

6.1 Labor Demand Shock I: Canonical Bartik Approach

6.1.1 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.¹⁴ Letting e_{itj} denote employment in state i and industry j at time t , we construct the national industry growth rate leaving out state i , κ_{itj} , as:

¹⁴See Borusyak, Hull, and Jaravel (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.

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

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:

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

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

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

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

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

where ΔY_{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.

6.1.2 Effects of Labor Demand Shocks from Industry Growth on Union Activity

We report results in Table 5 and also visualize effect sizes in Figures 7 and 10. Table 5 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.

6.2 Labor Demand Shock II: The China Shock Approach

6.2.1 Construction of Labor Demand Shock

We draw on the Chinese import exposure instrument (Autor, Dorn, and Hanson, 2013; Acemoglu et al., 2016), using long differences between 1991 and 1999 and 1999 and 2011 at the commuting zone level.¹⁵ Acemoglu et al. (2016) (AADHP) construct the change in Chinese import penetration in industry j over the long difference, τ , $\Delta IP_{j\tau}$, 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:

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

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

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

The endogenous variable $\Delta IP_{i\tau}^{CZ}$ 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 β 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), 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.

6.2.2 Effects of the China Shock on Union Activity

We report results in Table 6 and also visualize effect sizes in Figures 8 and 10. 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 6¹⁶. 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 6 show the effects on union outcomes.¹⁷ 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

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

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

(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 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 8. 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 25TH percentile Chinese import shock is 0.02 and the 75TH 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.

6.2.3 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:

$$\Delta EPOP_{c\tau} = \frac{\text{Employed}_{c,\tau+10}}{\text{Population}_{c,\tau+10}} - \frac{\text{Employed}_{c,\tau}}{\text{Population}_{c,\tau}},$$

$$\text{Entry}_{c\tau} = \frac{\text{Population}_{c,\tau}^{a \in [10,19]}}{\text{Population}_{c,\tau}},$$

$$\text{Exit}_{c\tau} = \frac{\text{Population}_{c,\tau}^{a \in [55,64]}}{\text{Population}_{c,\tau}}.$$

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

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 τ 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.

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

We report results in Table 7 and Figures 9 and 10. 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.

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, Dube, and McGrew, 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 (Farber, 1984; Ellwood and Fine, 1987). Likewise, employer opposition—itself affected by the institutional environment—has also shaped the evolution of U.S. unionization (Bronfenbrenner, 1996; Wang and Young, 2022; Kochan et al., 2023). Recent work by Hertel-Fernandez, Kimball, and Kochan (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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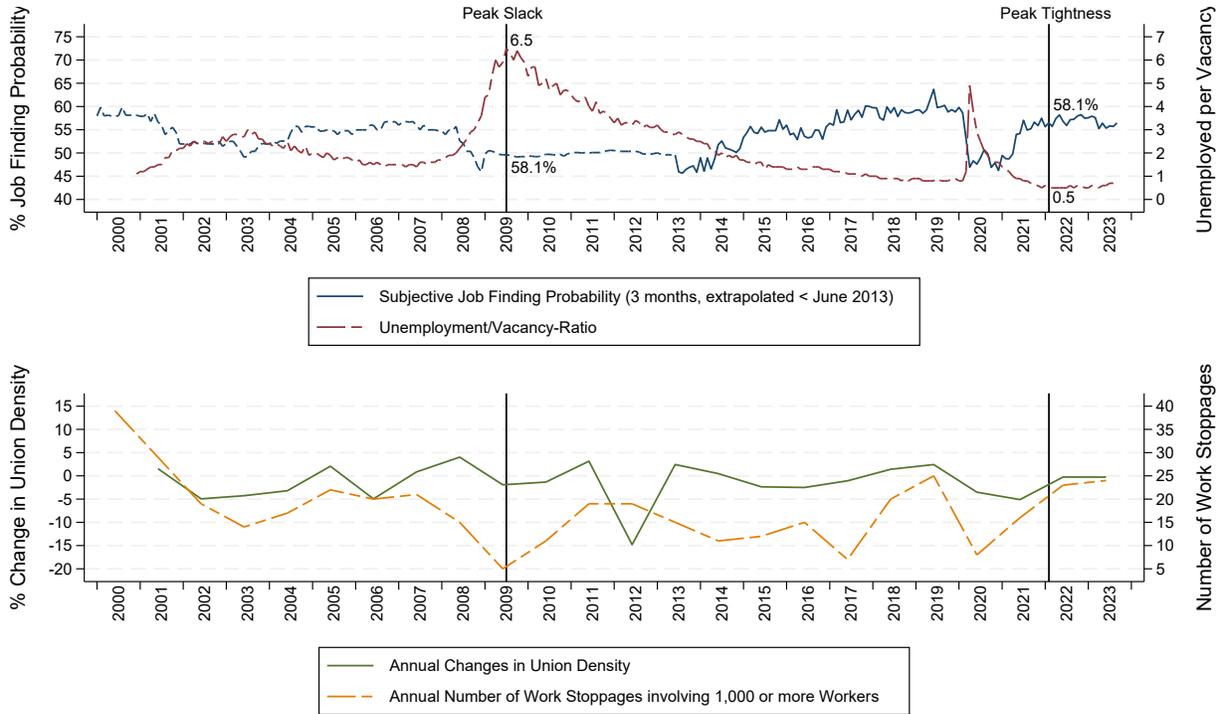
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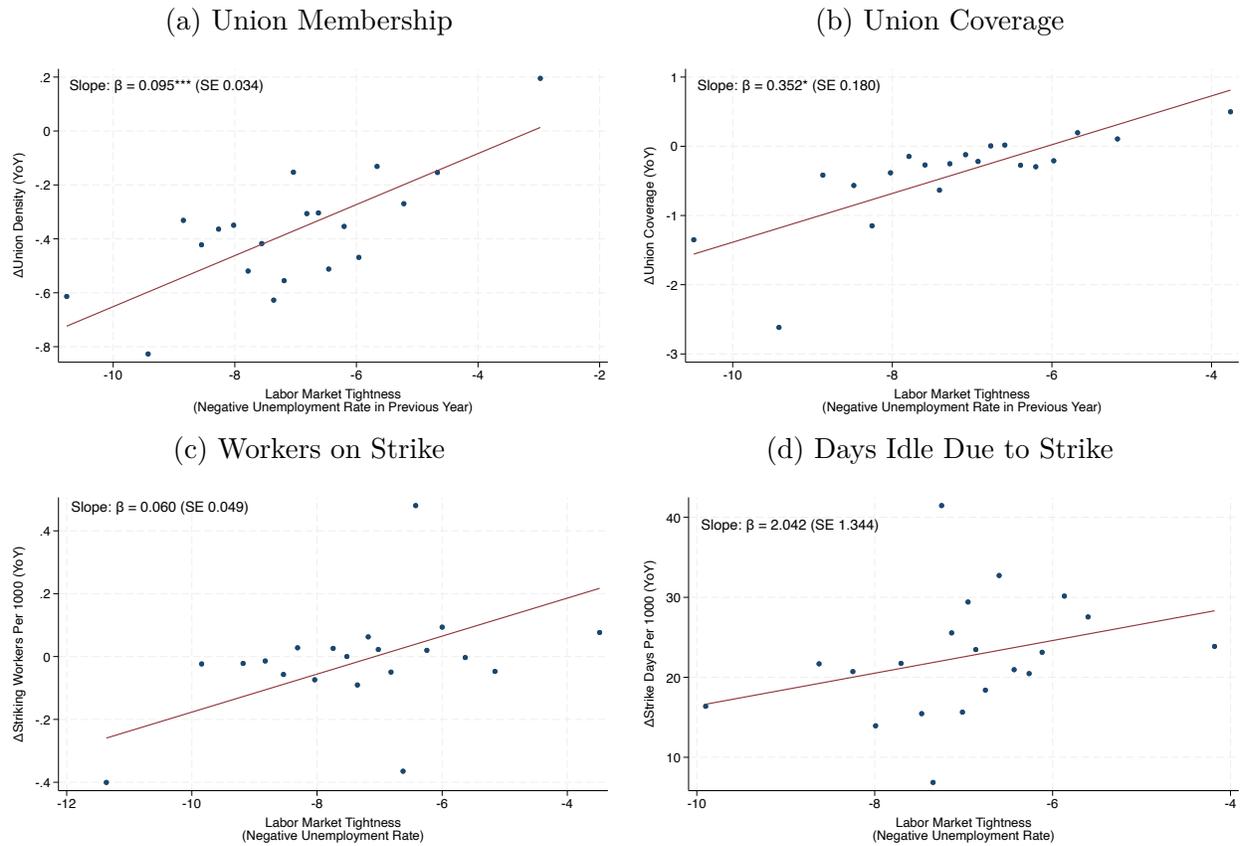
Figures

Figure 1: Labor Market Tightness, Subjective Job Finding Beliefs, and Union Activities



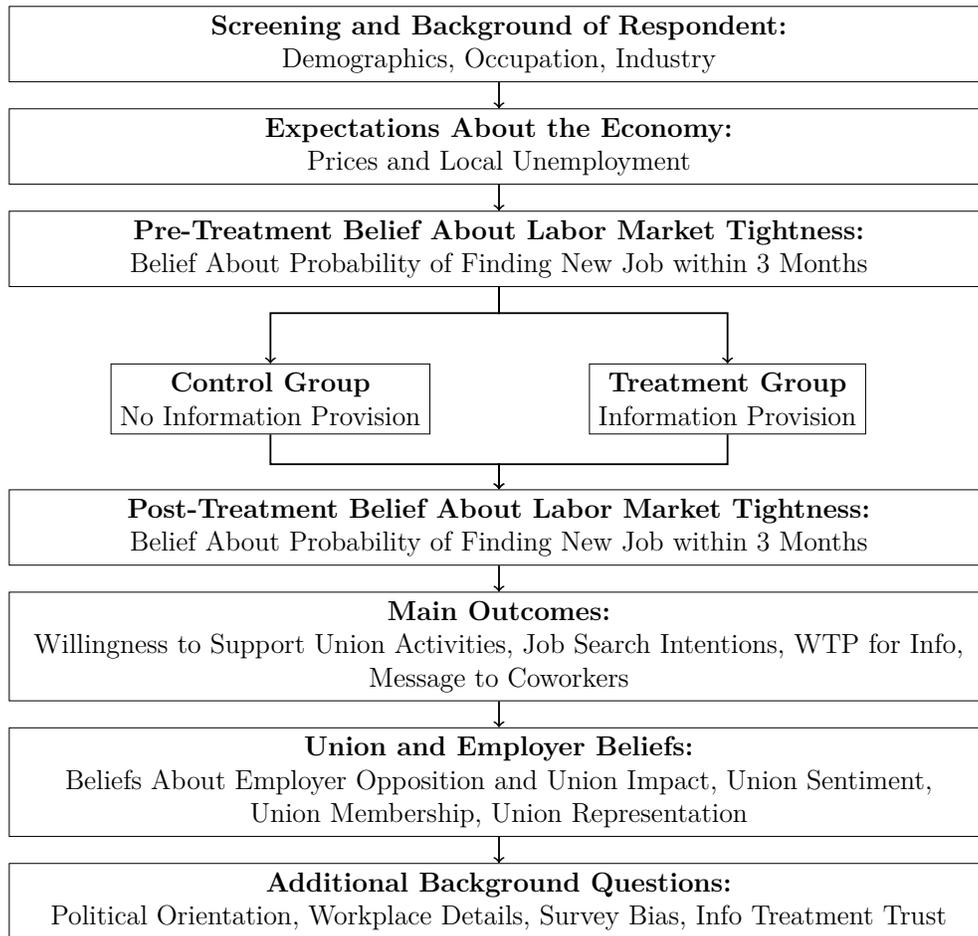
Note: This figure shows the development of labor market tightness in terms of the unemployment/vacancy-ratio and the subjective job finding probability of finding a new job within three months, over time. The number of work stoppages contains all work stoppages involving 1,000 workers or more beginning 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.

Figure 2: Union Activity and Labor Market Tightness in OECD Countries 1960 - 2022



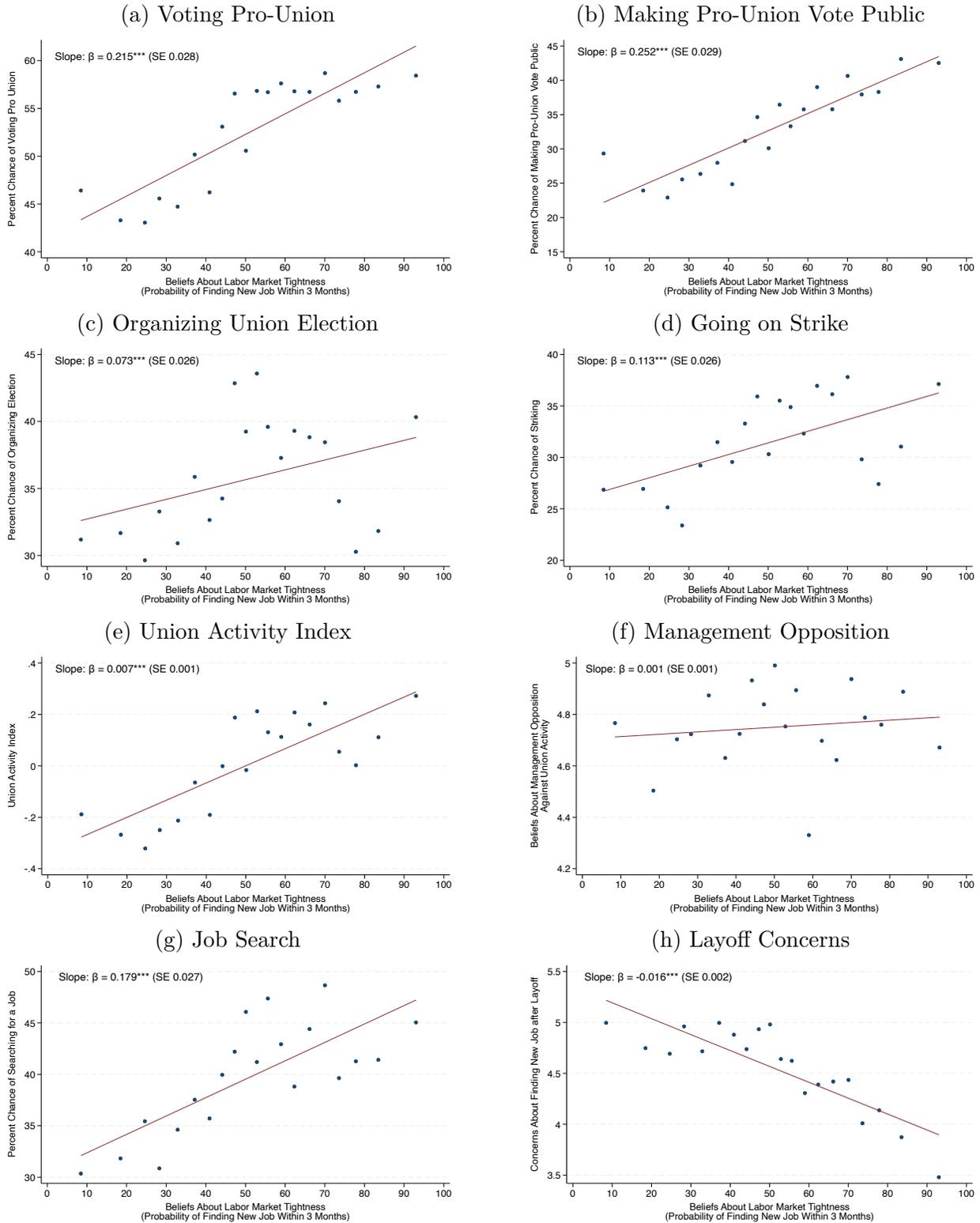
Note: These figures visualize the relationship between different measures of union activity and labor market tightness across OECD countries between 1960 and 2022. The outcome variable in Panel (a) measures the change in the union membership rate, defined as the proportion of employees who are members 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 1000 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.

Figure 3: Experimental Design



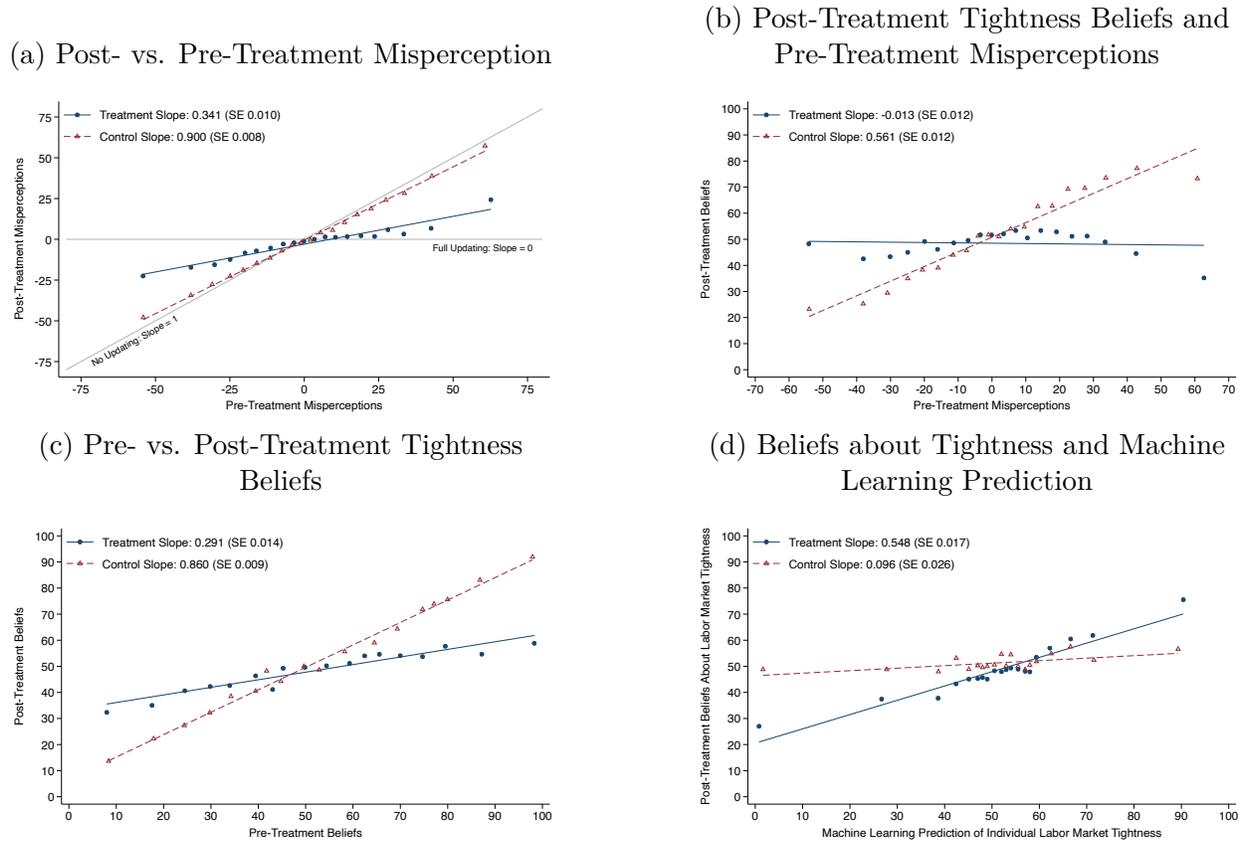
Note: This figure displays a high-level overview of the experimental survey design. The full questionnaire can be reviewed in Appendix Section A-3.

Figure 4: OLS Survey Evidence on Union Support, Job Search, Management Opposition and Workers' Beliefs about Labor Market Tightness



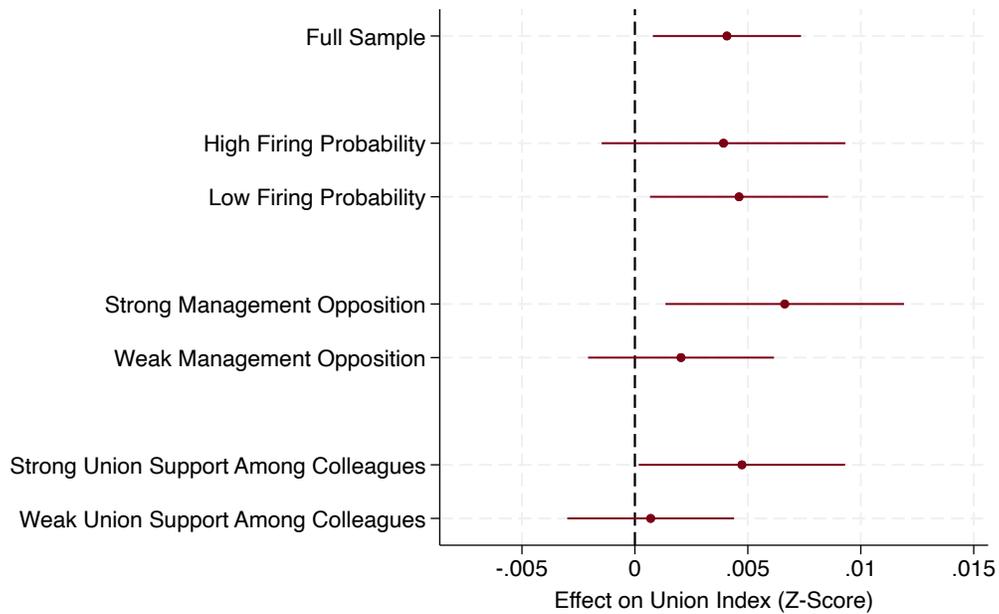
Note: This figure presents binned scatter plots based on control group respondents' beliefs about labor market tightness on the horizontal and respondents' self-reported support of union activity, job search intentions and beliefs about management opposition on the vertical axis. Beliefs about labor 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.

Figure 5: Intervention Check and De-Anchoring in the Survey Experiment



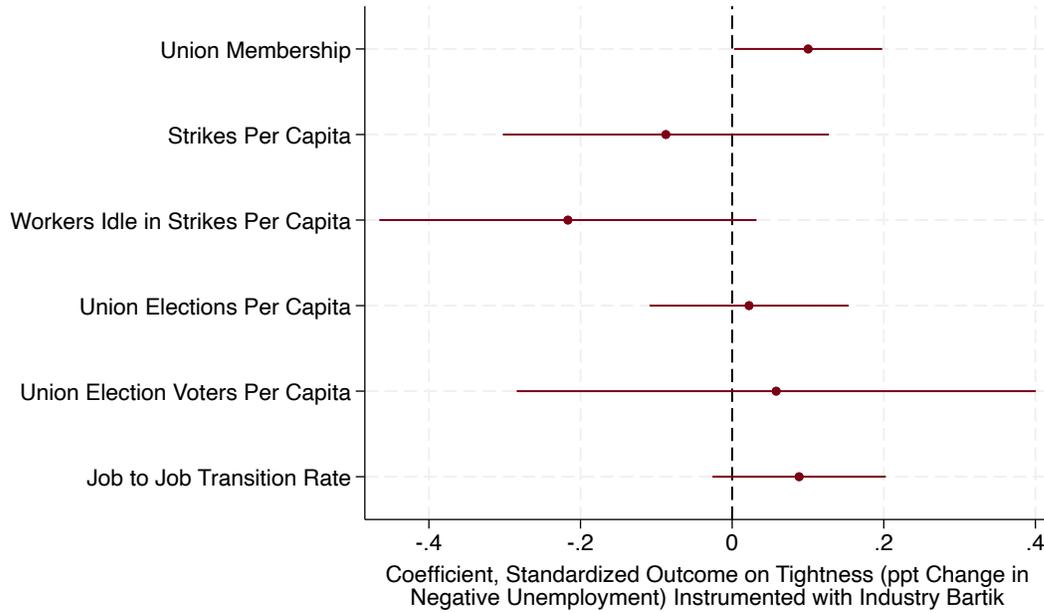
Note: This figure presents binned scatter plots based on our information experiment, where respondents that were randomly allocated to the treatment group received personalized information about their probability of finding a new job within three months (see Section 4.2 for more details on the construction 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.

Figure 6: IV Estimates of Effects of Beliefs About Labor Market Tightness on Union Activity from a Survey Experiment



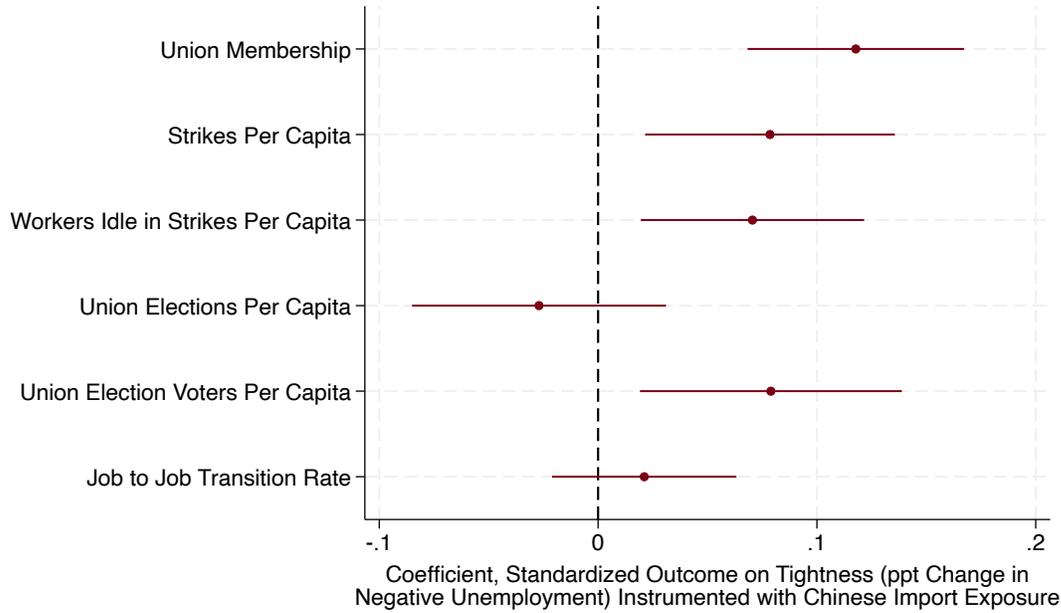
Note: This figure presents IV estimates based on Equation 3 with the union activity index that combines our centered measures of union activity as the outcome variable. Groups are based on a median split of the full sample. “Firing probability” measures respondents beliefs (in percent) about the probability 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.

Figure 7: Bartik Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions



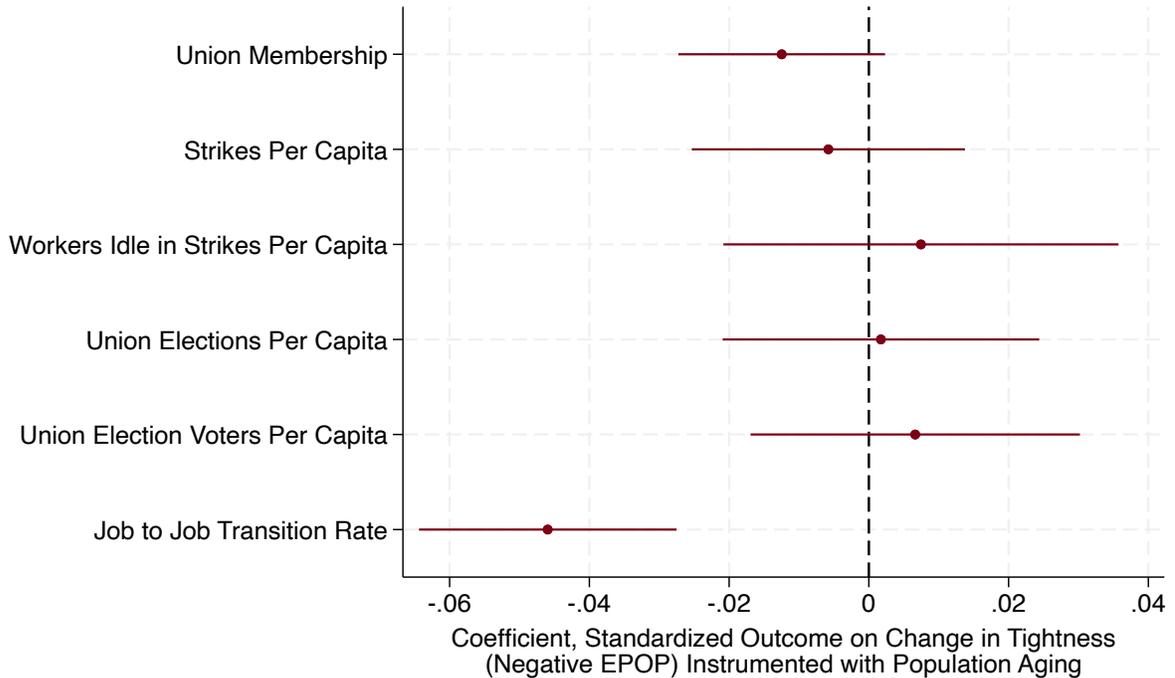
Note: This figure presents the effects of labor market tightness, measured as the negative unemployment rate and instrumented with the canonical Bartik instrument following Bartik (1991), on changes in several measures of union activity and job-to-job transitions. An example for reading this figure would 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 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, Moscarini, and Postel-Vinay (2020). All standard errors clustered at the state level.

Figure 8: China Shock Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions



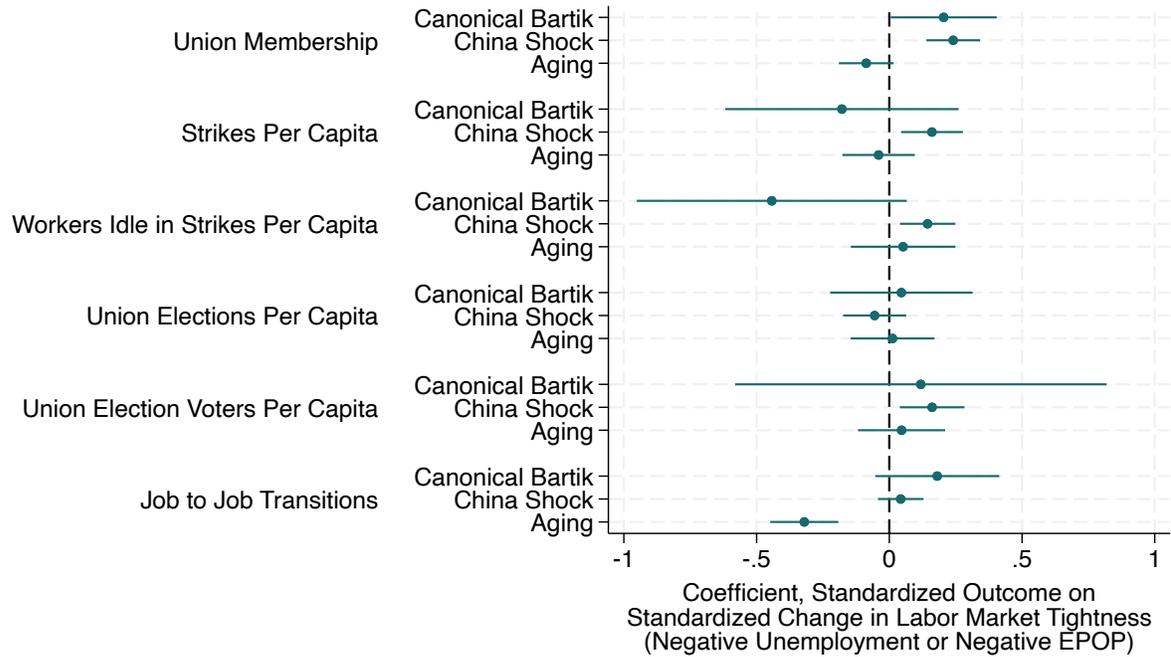
Note: This figure presents effects of the China shock on unionization outcomes, through its effects on changes in labor market tightness that we measure as the negative unemployment rate. An example for reading this figure would be that a one percentage point decrease in the unemployment rate, i.e., 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 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, Moscarini, and Postel-Vinay (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 9: 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 ration, 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 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, Moscarini, and Postel-Vinay (2020). Standard errors are clustered at the county level.

Figure 10: 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 6 and the figure notes below Figure 7, Figure 8 and Figure 9.

Tables

Table 1: Representativeness of the Survey Sample

	(1)	(2)	(3)	(4)	(5)
	U.S. Employed Population	Survey Sample	Treatment Group	Control Group	(3) = (4)
Age					
18-34	0.36	0.36	0.35	0.36	0.490
35-49	0.33	0.33	0.33	0.33	0.804
50-65	0.31	0.31	0.32	0.31	0.645
Gender					
Male	0.53	0.50	0.50	0.50	0.590
Female	0.47	0.50	0.50	0.50	0.590
Race					
European American/White	0.77	0.76	0.76	0.76	0.865
African American/Black	0.13	0.13	0.12	0.13	0.738
Asian	0.07	0.07	0.07	0.07	0.723
Mixed Race/Other	0.04	0.04	0.05	0.04	0.533
Divisions					
New England	0.05	0.05	0.05	0.05	0.516
Middle Atlantic	0.12	0.13	0.13	0.13	0.653
East North Central	0.14	0.15	0.15	0.15	0.612
West North Central	0.07	0.07	0.07	0.08	0.104
South Atlantic	0.20	0.19	0.19	0.19	0.861
East South Central	0.05	0.05	0.06	0.05	0.658
West South Central	0.12	0.12	0.12	0.12	0.809
Mountain	0.08	0.07	0.07	0.07	0.824
Pacific	0.16	0.16	0.16	0.16	0.521
Income					
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	0.653
\$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	0.675
<i>N</i>		5520	2765	2755	

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.

Table 2: 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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Post Estimation Error	Post Tightness Belief	Pro-Union Vote	Pro-Union Vote Public	Organize Election	Willing to Strike	Union Activity Index	Job Search Intention	Layoff Concerns	Management Opposition	Firing Probability
Panel A: OLS											
Treated x Pre-Treat Estimation Error	-0.5584*** (0.0172)	-0.5752*** (0.0216)	-0.0570* (0.0297)	-0.0628** (0.0295)	-0.0571** (0.0278)	-0.0495* (0.0274)	-0.0023** (0.0010)	-0.1077*** (0.0289)	0.0041** (0.0018)	0.0004 (0.0016)	0.0396 (0.0299)
Treated	-2.4392*** (0.3524)	-2.1135*** (0.4445)	-0.6190 (0.7664)	-0.7021 (0.7846)	-0.2435 (0.7382)	0.5765 (0.7476)	-0.0097 (0.0256)	-0.2596 (0.7793)	-0.0119 (0.0475)	-0.0158 (0.0441)	-0.1543 (0.7758)
Pre-Treat Estimation Error	0.8923*** (0.0088)	0.5863*** (0.0132)	0.1374*** (0.0223)	0.1610*** (0.0224)	0.0649*** (0.0206)	0.0684*** (0.0205)	0.0044*** (0.0007)	0.1530*** (0.0215)	-0.0115*** (0.0013)	-0.0017 (0.0012)	0.0337 (0.0215)
Constant	-4.8914* (2.6246)	38.7326*** (3.0480)	45.4217*** (4.2155)	23.1248*** (4.2351)	43.4935*** (4.4118)	34.0189*** (4.3484)	-0.0536 (0.1388)	42.6792*** (4.0301)	5.0917*** (0.2902)	4.4832*** (0.2672)	48.7359*** (4.3727)
Panel B: IV											
Labor Market Tightness Belief			0.1033** (0.0508)	0.1141** (0.0503)	0.0996** (0.0477)	0.0781* (0.0474)	0.0041** (0.0017)	0.1858*** (0.0496)	-0.0068** (0.0030)	-0.0005 (0.0028)	-0.0658 (0.0520)
Pre-Treat Estimation Error			0.0780*** (0.0208)	0.0955*** (0.0203)	0.0966 (0.0194)	0.0204 (0.0194)	0.0020*** (0.0007)	0.0437** (0.0201)	-0.0074*** (0.0013)	-0.0014 (0.0012)	0.0731*** (0.0225)
Constant			41.2498*** (4.5801)	18.5083*** (4.5835)	39.6198*** (4.7320)	31.3092*** (4.6523)	-0.2119 (0.1506)	35.5406*** (4.4300)	5.3427*** (0.3048)	4.4948*** (0.2842)	51.1583*** (4.8194)
Observations	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520
Average of Outcome Control Group	0.26	51.30	52.56	32.98	35.75	31.55	0.01	39.76	4.55	4.75	36.36

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 endogeneous 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.

Table 3: 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
Panel A: Binary Outcome			
IV: Labor Market Tightness Belief	-0.0002 (0.0007)	-0.0013* (0.0007)	-0.0009 (0.0007)
Pre-Treat Estimation Error	-0.0021*** (0.0003)	-0.0014*** (0.0003)	-0.0018*** (0.0003)
Constant	0.9744*** (0.0674)	0.9651*** (0.0694)	0.9792*** (0.0710)
Average of Outcome Control Group	0.66	0.69	0.66
Panel B: Continuous Outcome			
IV: Labor Market Tightness Belief	-0.0071 (0.0060)	-0.0066 (0.0058)	0.0038 (0.0060)
Pre-Treat Estimation Error	-0.0093*** (0.0026)	-0.0113*** (0.0025)	-0.0152*** (0.0025)
Constant	7.4442*** (0.6292)	7.2974*** (0.6379)	6.6087*** (0.6292)
Average of Outcome Control Group	4.59	4.29	4.24
Correlation With Corresp. Main Outcome	0.15	0.13	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 A-3 and a discussion of results in Section 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 (propensity to vote pro union for Column (1) and (2) and the reported job search intentions in Column (3)) at the bottom of the table.

Table 4: Effort and Sentiment in Message about Unionization to Coworkers

	(1)	(2)	(3)	(4)	(5)
	Wrote Message	Pos Sentiment	Word Count	Copied	Time Spent
Panel A: Full Sample					
IV: Labor Market Tightness Belief	0.0008 (0.0007)	0.0006 (0.0006)	-0.0416 (0.0794)	-0.0007*** (0.0003)	-0.0315 (0.2284)
Pre-Treat Estimation Error	0.0011*** (0.0003)	0.0002 (0.0002)	0.0594* (0.0308)	0.0002** (0.0001)	0.3112*** (0.1027)
Constant	0.1017 (0.0683)	0.0381 (0.0529)	1.3096 (4.7842)	0.0570** (0.0228)	23.8903 (18.6408)
Observations	5520	5520	5520	5520	5520
Average of Outcome Control Group	0.34	0.15	16.52	0.03	69.08
Correlation With Corresp. Main Outcome	0.05	0.42	0.03	0.06	0.05
Panel B: Conditional on Drafting					
IV: Labor Market Tightness Belief		0.0005 (0.0010)	-0.1528 (0.1433)	-0.0014*** (0.0005)	-0.4083 (0.4031)
Pre-Treat Estimation Error		-0.0006 (0.0005)	-0.0363 (0.0742)	0.0001 (0.0002)	0.4620** (0.2162)
Constant		0.3419* (0.1785)	18.1107 (12.0590)	0.1788* (0.0990)	161.3409*** (61.9266)
Observations		1817	1817	1817	1817
Average of Outcome Control Group		0.43	48.67	0.08	158.95
Correlation With Corresp. Main Outcome		0.53	0.01	0.07	0.01

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 workers’ propensity to vote “yes” in a union election at the bottom of the table.

Table 5: Bartik Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Negative Δ Unemployment (ppt)	Δ Union Membership	Δ Strikes Per Million	Δ Workers Idle in Strikes Per Thousand	Δ Union Elections Per Million	Δ Union Election Voters Per Thousand	Δ Job to Job Transition Rate
Bartik Instrument (Unweighted)	36.127*** (12.933)						
Bartik Instrument (Weighted)	2.545 (7.501)						
Negative Δ 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 Δ 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 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, Moscarini, and Postel-Vinay (2020). All standard errors clustered at the state level

Table 6: China Shock Results: Effects of Labor Demand Shocks on Union Activity and Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Negative Change in Chinese Imports	Negative Δ Unemployment Rate (ppt)	Employment/Pop (AADHP)	Δ Union Membership	Δ Strikes Per Million	Δ Workers Idle in Strikes Per Thousand	Δ Union Elections Per Million	Δ Union Election Voters Per Thousand	Δ Job to Job Transition Rate
Negative Chinese Exports Elsewhere (Unweighted)	0.589*** (0.033)	0.345*** (0.046)							
Negative Chinese Exports Elsewhere (Weighted)	0.622*** (0.051)	0.194*** (0.071)							
Negative Shock to Chinese Imports (Unweighted)			2.618*** (0.239)	0.389*** (0.074)	0.503*** (0.182)	0.156** (0.064)	-0.466 (0.800)	0.272** (0.106)	0.016 (0.016)
Negative Shock to Chinese Imports (Weighted)			1.638*** (0.460)	-0.060 (0.197)	0.087 (0.247)	-0.285 (0.189)	-2.318** (0.910)	0.159 (0.112)	-0.060 (0.038)
Negative Δ Unemployment Rate (ppt) (Unweighted)				0.664*** (0.165)	0.858** (0.337)	0.266** (0.116)	-0.794 (1.360)	0.464** (0.184)	0.015 (0.015)
Negative Δ Unemployment Rate (ppt) (Weighted)				-0.194 (0.628)	0.281 (0.829)	-0.915 (0.565)	-7.450* (3.993)	0.512 (0.432)	-0.113 (0.073)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Outcome	-0.83	-6.67	45.0	8.13	2.11	0.54	17.0	1.29	2.34
Mean of Difference in Outcome	-0.08	-0.02	0.24	-0.23	-0.16	-0.04	-0.88	-0.07	-0.07
Observations	1444	1444	1444	1444	1444	1444	1444	1444	722
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 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 6. The top two columns use industry-weighted Chinese import changes to other rich countries 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, Moscarini, and Postel-Vinay (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.

Table 7: Demographic Change Results: Effects of Shocks to Labor Scarcity on Union Activity and Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Negative Δ EPOP (ppt)	Δ Union Membership	Δ Strikes Per Million	Δ Workers Idle in Strikes Per Thousand	Δ Union Elections Per Million	Δ Union Election Voters Per Thousand	Δ Job to Job Transition Rate
Share Retiring (Unweighted)	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 Δ 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 Δ 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 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, Moscarini, and Postel-Vinay (2020). Standard errors are clustered at the county level.

Online Appendix of:
Labor Market Tightness and Union Activity

By Chantal Pezold, Simon Jäger, and Patrick Nüß

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A-1 Additional Tables

Table A-1.1: OLS Labor Market Tightness and Union Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	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
Labor Market Tightness Belief	-0.0148*** (0.0016)	0.2031*** (0.0286)	0.2654*** (0.0302)	0.1201*** (0.0261)	0.0776*** (0.0267)	0.0071*** (0.0009)	0.1817*** (0.0272)	-0.0148*** (0.0016)	-0.0148*** (0.0016)	-0.0148*** (0.0016)	0.0005 (0.0003)
Constant	5.9534*** (0.4483)	33.8686*** (7.2083)	41.4706*** (7.1741)	35.7193*** (7.3769)	42.6118*** (6.9311)	-0.1851 (0.2274)	34.4317*** (6.6344)	5.9534*** (0.4483)	5.9534*** (0.4483)	5.9534*** (0.4483)	-0.0037 (0.0769)
Observations	2755	2755	2755	2755	2755	2755	2755	2755	2755	2755	2755

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), and the outcome in Column (7) is the standardized version of this index. Column (8) then reports the results of a regression with an inverse-covariance-weighted combined measure of standardized union activity outcomes. Finally, Column (9) reports estimates of the relationship between beliefs about tightness and job search behavior, where the latter is measured with the question "Over the next 12 months, what is the percent chance that you will look for a new job at a different company?". 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.

Table A-1.2: 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)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		High	Low	Non	Some	Certain	Uncertain	Worse	Equal	Better
Panel A: Union Activity Index										
Labor Market Tightness Belief	0.0041** (0.0017)	0.0046** (0.0022)	0.0024 (0.0026)	0.0041** (0.0018)	0.0058 (0.0042)	0.0041** (0.0020)	0.0049* (0.0029)	0.0023 (0.0024)	0.0093*** (0.0036)	0.0044 (0.0031)
Pre-Treat Estimation Error	0.0020*** (0.0007)	0.0026*** (0.0009)	0.0010 (0.0010)	0.0017** (0.0007)	0.0024 (0.0017)	0.0020** (0.0008)	0.0012 (0.0011)	0.0004 (0.0009)	0.0001 (0.0015)	0.0052*** (0.0012)
Constant	-0.2119 (0.1506)	0.0156 (0.1788)	-0.7624*** (0.2764)	-0.1632 (0.1661)	-0.2716 (0.5431)	-0.3271 (0.2133)	-0.1004 (0.2099)	-0.1515 (0.2395)	0.0685 (0.2628)	-0.6652** (0.3167)
Panel B: Job Search										
Labor Market Tightness Belief	0.0062*** (0.0017)	0.0066*** (0.0021)	0.0052** (0.0027)	0.0069*** (0.0018)	0.0050 (0.0042)	0.0077*** (0.0019)	0.0019 (0.0032)	0.0034 (0.0023)	0.0125*** (0.0037)	0.0053* (0.0030)
Pre-Treat Estimation Error	0.0015** (0.0007)	0.0019** (0.0009)	0.0005 (0.0011)	0.0006 (0.0007)	0.0036** (0.0017)	0.0010 (0.0008)	0.0023* (0.0012)	0.0017* (0.0010)	-0.0020 (0.0016)	0.0031** (0.0012)
Constant	-0.1405 (0.1476)	0.0820 (0.1693)	-0.4734* (0.2771)	-0.0717 (0.1641)	-0.5941 (0.4101)	-0.3889* (0.2077)	0.2884 (0.2276)	-0.1647 (0.2412)	-0.0921 (0.2428)	-0.1866 (0.3337)
Observations	5520	3546	1969	3973	1547	3819	1701	2603	1177	1740

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 endogeneous 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 A-1.3: 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)
	Post Estimation Error	Post Tightness Belief	Pro-Union Vote	Pro-Union Vote Public	Organize Election	Willing to Strike	Union Activity Index	Job Search Intention	Layoff Concerns	Management Opposition	Firing Probability
Panel A: OLS											
Treated x Pre-Treat Estimation Error	-0.5584*** (0.0172)	-0.5752*** (0.0216)	-0.0570* (0.0297)	-0.0628** (0.0295)	-0.0571** (0.0278)	-0.0495* (0.0274)	-0.0023** (0.0010)	-0.1077*** (0.0289)	0.0041** (0.0018)	0.0004 (0.0016)	0.0396 (0.0299)
Treated	-2.4392*** (0.3524)	-2.1135*** (0.4445)	-0.6190 (0.7664)	-0.7021 (0.7846)	-0.2435 (0.7382)	0.5765 (0.7476)	-0.0097 (0.0256)	-0.2596 (0.7793)	-0.0119 (0.0475)	-0.0158 (0.0441)	-0.1543 (0.7758)
Pre-Treat Estimation Error	0.8923*** (0.0088)	0.5863*** (0.0132)	0.1374*** (0.0223)	0.1610*** (0.0224)	0.0649*** (0.0206)	0.0684*** (0.0205)	0.0044*** (0.0007)	0.1530*** (0.0215)	-0.0115*** (0.0013)	-0.0017 (0.0012)	0.0337 (0.0215)
Constant	-4.8914* (2.6246)	38.7326*** (3.0480)	45.4217*** (4.2155)	23.1248*** (4.2351)	43.4935*** (4.4118)	34.0189*** (4.3484)	-0.0536 (0.1388)	42.6792*** (4.0301)	5.0917*** (0.2902)	4.4832*** (0.2672)	48.7359*** (4.3727)
height											
Panel B: IV											
Labor Market Tightness Belief			0.0972* (0.0529)	0.1054** (0.0522)	0.0858* (0.0505)	0.0607 (0.0492)	0.0036** (0.0018)	0.1791*** (0.0508)	-0.0072** (0.0031)	-0.0001 (0.0029)	-0.0667 (0.0527)
Pre-Treat Estimation Error			0.0600*** (0.0202)	0.0882*** (0.0198)	-0.0243 (0.0190)	-0.0029 (0.0188)	0.0012* (0.0007)	0.0226 (0.0192)	-0.0072*** (0.0012)	-0.0010 (0.0012)	0.0514** (0.0213)
Constant			47.2140*** (2.6376)	27.1366*** (2.6109)	31.2995*** (2.5185)	28.7205*** (2.4624)	-0.1807** (0.0874)	30.6465*** (2.5381)	4.9179*** (0.1546)	4.7543*** (0.1459)	39.6335*** (2.6524)
Observations	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520	5520
Average of Outcome Control Group	0.26	51.30	52.56	32.98	35.75	31.55	0.01	39.76	4.55	4.75	36.36

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 endogeneous 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 report robust standard errors in parentheses.

Table A-1.4: Alternative Mechanisms: Experimental Evidence on Labor Market Tightness and Workers’ Beliefs About Effectiveness of Unions, Profit Growth and Union Support Among Colleagues

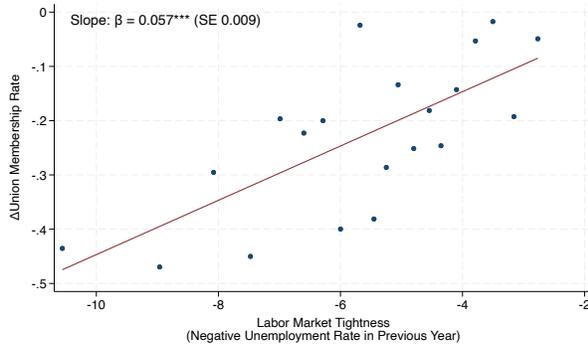
	(1)	(2)	(3)	(4)	(5)
	Wage Premium	Working Condition Premium	Job Security Premium	Employer Profits	Voteshare Colleagues
Labor Market Tightness Belief	0.0333 (0.0295)	0.0248 (0.0406)	-0.0061 (0.0288)	-0.0049 (0.0308)	0.0648 (0.0441)
Pre-Treat Estimation Error	-0.0155 (0.0129)	0.0032 (0.0180)	-0.0070 (0.0136)	0.0087 (0.0143)	0.0350* (0.0197)
Observations	5449	5453	5453	5520	5520

Note: This table shows estimates based on Equation 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.

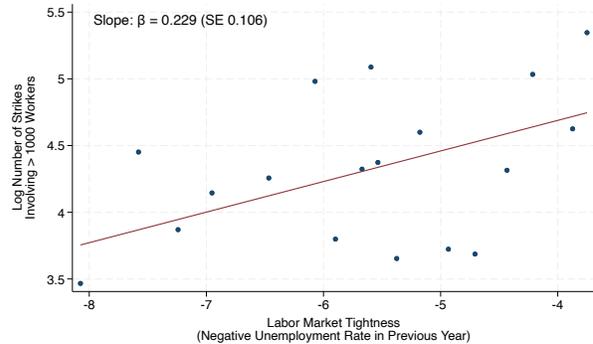
A-2 Additional Figures

Figure A-1: OLS Labor Market Tightness and Union Activity

(a) Δ Union Membership

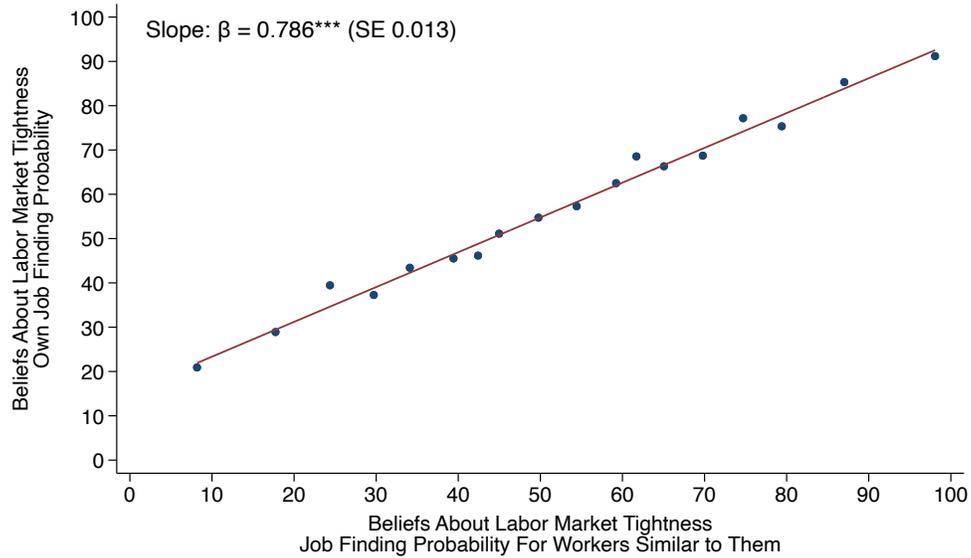


(b) Work Stoppages



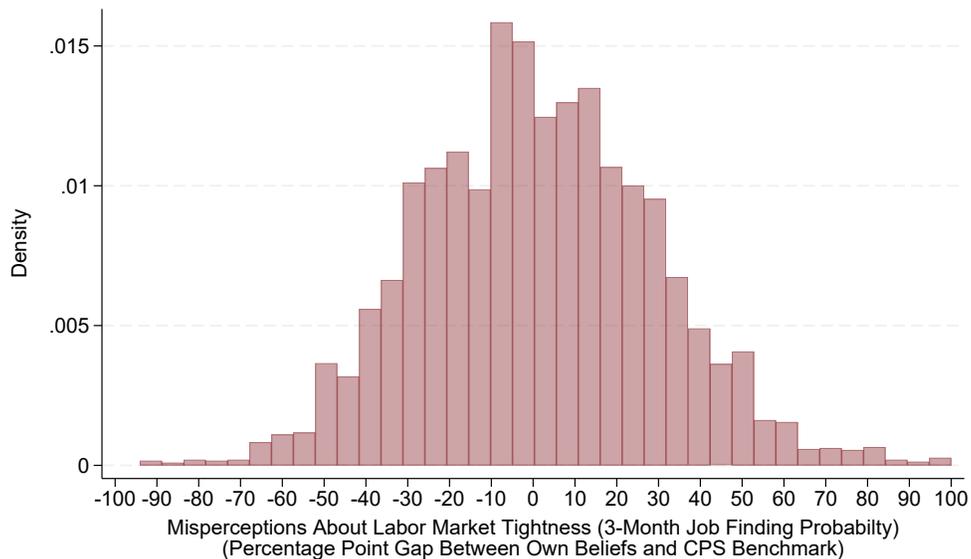
Note: This figure shows binned scatter plots of union activity outcomes and labor market tightness, measured in levels of the previous year’s unemployment rate. In Panel (a) the outcome variable is the year-to-year change in the union membership rate retrieved from the Unionstats.com website and covers 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.

Figure A-2: Workers' Beliefs About Own vs. Similar People's Job Finding Probability



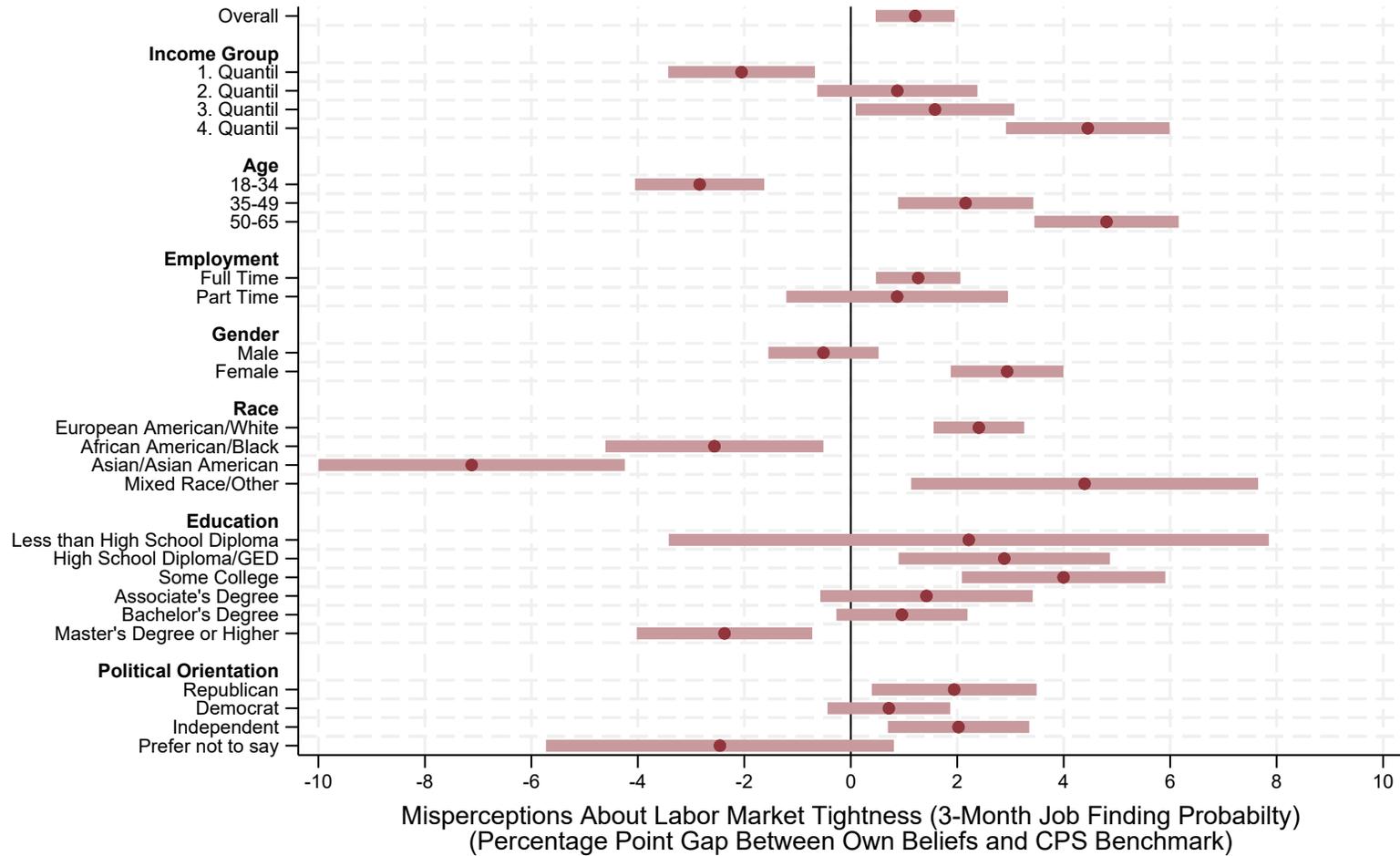
Note: This figure shows a binned scatter plot of respondents' beliefs about their own job finding probability and their beliefs about this probability for workers similar to them along several dimensions relevant to the job market. In particular, we ask them to think of workers employed in the same occupation 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 A-3.

Figure A-3: Distribution of the Misperceptions of Job Finding Probability



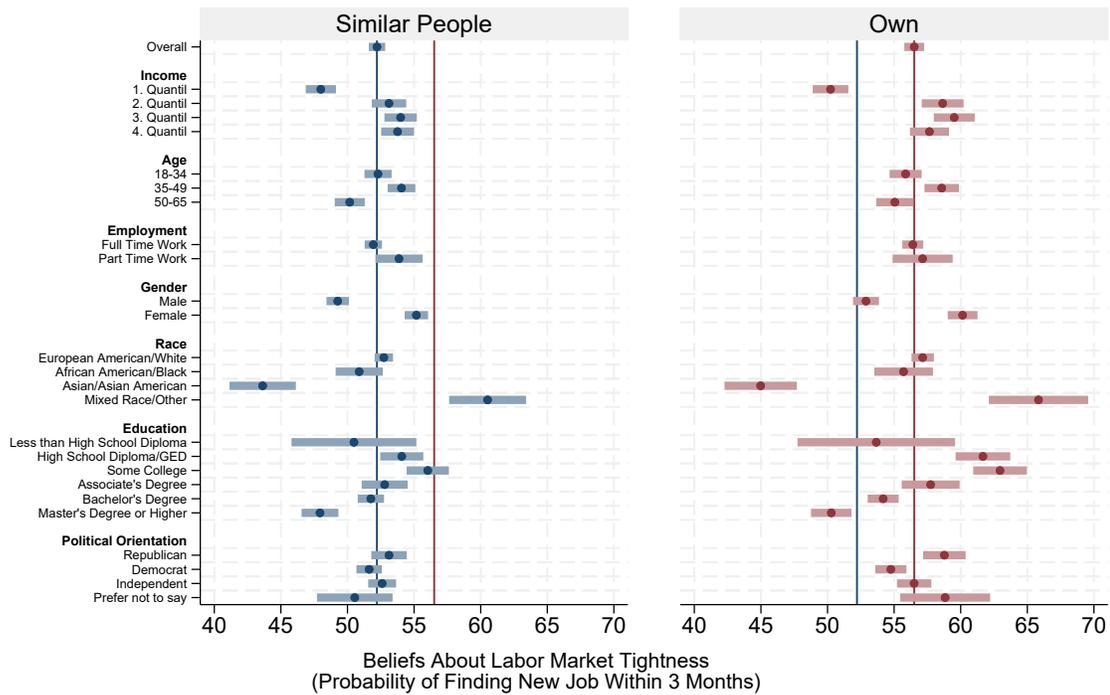
Note: This figure shows the average for respondents' prior misperception of others probability of finding a new job within three months by socioeconomic characteristics. Misperceptions are defined as the difference between respondents beliefs about their probability of finding a new job within three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

Figure A-4: Heterogeneity in Misperceptions of Job Finding Probability



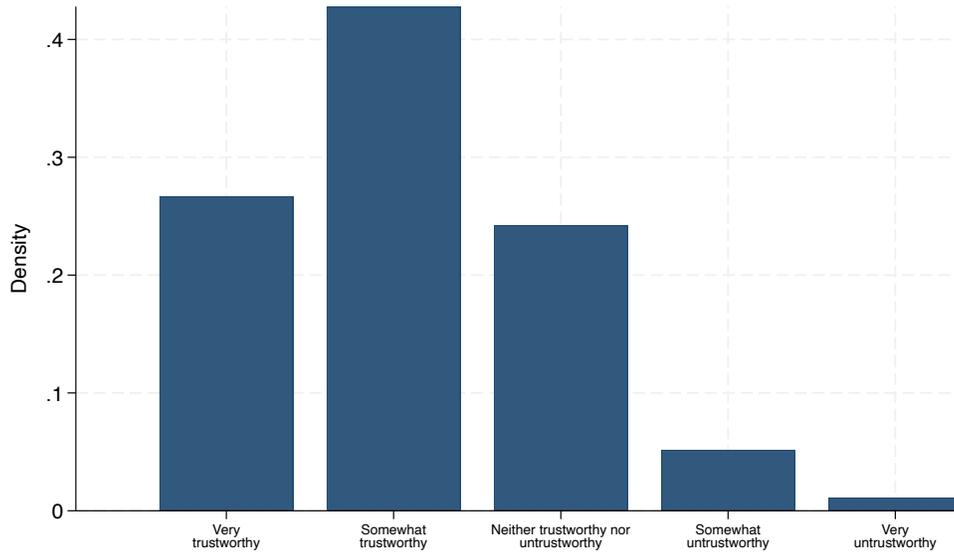
Note: This figure shows the average for respondents prior misperception of others probability of finding a new job within three months by socioeconomic characteristics. Misperceptions are defined as the difference between respondents beliefs about their probability of finding a new job within three months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

Figure A-5: Workers' Beliefs About Own vs. Similar People's Job Finding Probability



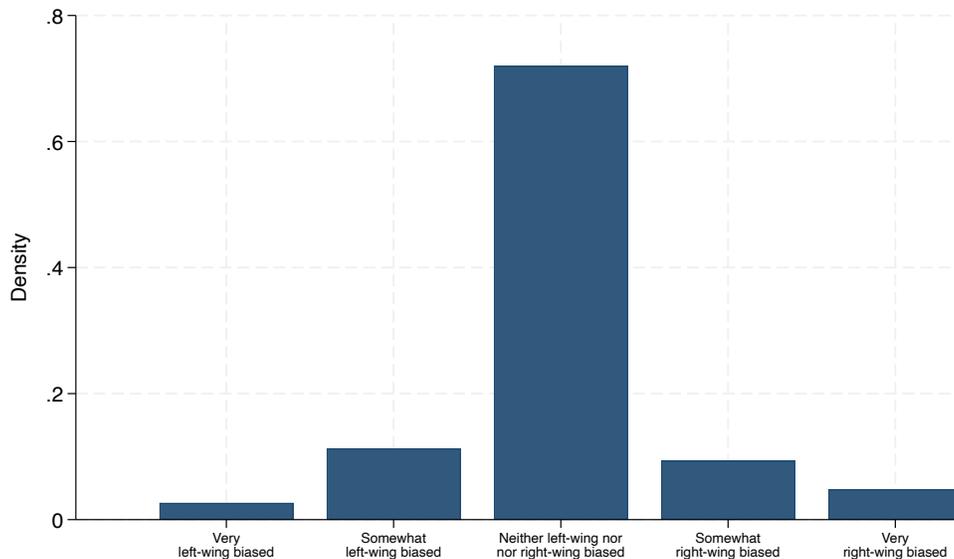
Note: This figure shows the average prior beliefs about their own job finding probability and their beliefs about this probability for workers similar to them along several dimensions relevant to the job market, by subgroups based on socioeconomic characteristics. We ask them to think of workers employed 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 A-3. 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.

Figure A-6: 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 A-7: 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?"

A-3 Questionnaire

A-3.1 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.

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?

A-3.2 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?

A-3.3 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?

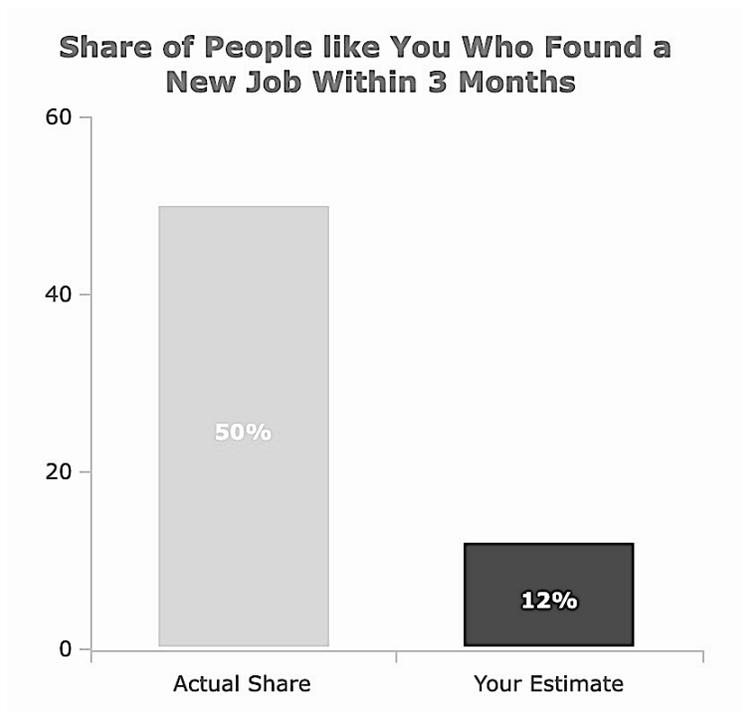
- How sure are you about your estimate in the previous questions?

A-3.4 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 A-8: 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.

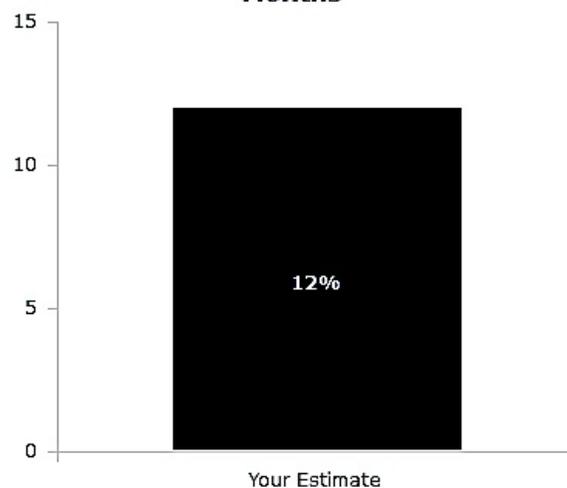
Based on the most recent U.S. Census Data, we calculated that [X]% of people like you found a new job within 3 months.

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

A-3.5 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!

Your Estimate of the Share of People like You Who Found a New Job Within 3 Months



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.

A-3.6 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?

A-3.7 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 %)?

A-3.8 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?

A-3.9 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?

A-3.10 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?