Worker Voice, Surveillance, and Remote Work

Results from a Survey Experiment

Gabriel Lewis

October 25, 2023

Abstract

In a survey experiment, workers demonstrated substantial willingness-to-pay (WTP) for institutions which gave them a voice in the workplace. On average, respondents were willing to sacrifice 3% of wages for a workers' union, 4% for a human resources department (HR) that proactively solicits employee feedback, and 5% for an 'employee resource group' (ERG) in which workers and managers solve workplace problems collaboratively. WTP for these 'worker voice institutions' tended to be substantially higher among women, people of color, and under-40-year-olds. Neither proactive HR nor ERGs acted as strong utility substitutes for unions. WTP for worker voice remained high in the context of remote work and digital surveillance by the employer. I find large WTP for a remote work option (13%), and large negative WTP for digital surveillance (-16%). Surveillance significantly decreased the desirability of remote work by about 1.9% of wages, but not enough for the desire to escape monitoring to explain the desire for remote work. Supplementing these estimates using a Bayesian hierarchical model, I find considerable heterogeneity between individuals, strong correlations between WTPs for all voice institutions, and that WTP for each voice institution is correlated with WTP for freedom from digital surveillance.

"Forming a shop committee was done in a sincere effort to counteract the baneful and destructive effects both in morale and production of constant agitation from sources outside the employees themselves, such agitation being almost wholly of imported union inception"

— Unnamed official of the Virginia Bridge and Iron Co., 1922. (Miller 1922)

1 Introduction

When surveyed, most U.S. workers say that they want workplace institutions which allow them to exercise *voice*: influence over the decisions of the firm, whether through verbal appeals or actions.[1] (Freeman and Rogers 2006; Freeman 2007; Kochan and Kimball 2019) Workers' unions, for instance, may facilitate worker voice, among other functions. (Freeman and Medoff 1979, 1984) Human

[1]This definition is due to Hirschman (1970, 30) in his landmark work on the subject. So defined, worker voice is necessary but not sufficient for worker *power*, defined as the ability of the worker to compel the employer to act as they would not otherwise act to further the interests of the worker (Bowles and Gintis 2008).

Resources Departments (HRs), and the more recently invented Employee Resource Groups (ERGs) may, at least in some forms, also facilitate worker voice. Yet for policymakers and economists, the relevant question is what workers are willing to do, and in particular what they are willing to sacrifice, to gain a voice. Using a novel survey experiment, I estimate workers' willingness to sacrifice wages for the above voice institutions, individually and in combination. This experiment is the first to quantify how much workers value[2] HR and ERGs, to compare workers' valuations of these employer-controlled institutions to workers' valuations of unions, and to estimate whether workers consider HR or ERGs to be utility substitutes for unions.

Worker voice must be studied in context, and increasingly this context includes remote work and digital monitoring or surveillance by employers.[3] I estimate whether workers value voice institutions differently in jobs with digital surveillance or remote work; whether, for example, the presence of surveillance tends to increase the value that workers see in a union. I also estimate how much workers value remote work and digital surveillance, individually and in combination. This allows me to estimate how much workers' desire to avoid monitoring may explain their desire for remote work.

Finally, I move beyond estimates of average willingness-to-pay (WTP) to examine heterogeneity — that is, I estimate how much different workers may disagree about the value of the above voice institutions and work arrangements. I consider differences in WTP by race, gender, age, and occupational class (whether they are a manager or an employee). Additionally, I estimate the entire population distribution of WTP, particularly the variance and the correlations between individuals' WTPs for each job characteristic, using a Bayesian hierarchical model.

My aim is to shed light on an evolving conflict over worker voice. For over a century, some U.S. employers have sought to ensure that if workers do have a voice in their workplace, that voice will be transmitted through employer-controlled institutions (ECIs) rather than through worker-controlled institutions (WCIs) such as unions (O'Sullivan 2001; Bronfenbrenner 2022). In many cases, employers have succeeded, and have attributed their success to their workers' putative preferences for certain ECIs over WCIs.

For example, in the epigraph's Virginia Bridge and Iron Co., managers asserted that once they instated the works council, an ECI in which workers collaborated with managers to solve workplace problems, workers simply preferred to negotiate pay and grievances through the works council rather than through a union, plunging the firm's unionization rate from 90% to 2% (Miller 1922, 164).[4] Nationwide, works councils were so effective at drawing away union members, albeit during a period of considerable legal suppression and outright violence against unions,[5] (Taft 1966; Gitelman

[2]Throughout, my interest is in respondents' *total* expected valuations of the institutions I study, including any instrumental values the institutions may have to the respondents, such as through expected future wages gains. I do not try to separate from the total value any purely intrinsic values of unheeded voice, ineffectual unions, etc. (perhaps valuable due to perceived procedural fairness, irrespective of outcome), although some have tried (see, e.g. Adhvaryu, Molina, and Nyshadham 2019).

[3]I use the term surveillance and monitoring interchangeably to denote a range of activities that I describe in greater detail below. I prefer the term surveillance because my experiment is particularly focused on activities that the employer may undertake at their own discretion, without the knowledge of their employees, with the explicit purpose of identifying or deterring unwanted behavior by the employee.

[4]Meanwhile, average union membership rates in building and transportation trades in the U.S. (a reasonable comparison group) were relatively stable, falling by no more than five percentage points throughout this period (Wolman 1924).

[5]Including at firms overseen by the president and board of Virginia Bridge and Iron [(Library 2010).

1973; Lipold and Isaac 2022) that labor unions successfully pushed to have works councils banned as "employer-dominated unions" under the National Labor Relations Act of 1935, (NLRB, n.d.) which still largely governs U.S. labor relations (Miller 1922; O'Sullivan 2001; Patmore 2016).

Today, instead of works councils, one finds ERGs, in which workers and managers can meet and brainstorm ways to improve quality of life in the workplace, although typically avoiding discussion of pay or benefits. One also finds HR departments, now sometimes tasked not only with administering hiring, firing and payroll (which I will call "standard HR"), but also with engaging employees' feedback through surveys and workshops ("proactive HR"). In effect, ERGs and proactive HR follow the advice of management consultants to engage in "union-proofing" by "giving employees a voice" through ECIs (see, e.g., Projections, Inc. 2022). With such ECIs in place, it is not unusual for managers at major U.S. employers to make assertions that their non-unionized employees "do not need a union because local management and the partners [employees] work shoulder–to–shoulder and can have listening sessions to communicate." (see, e.g. "Starbucks Corporation LLC" 2023, 12)

Taken at face value, these commonplace assertions suggest two related hypotheses about U.S. workers: first, that workers tend to value certain ECIs at least as much they value WCIs (a hypothesis I will call "ECI desirability"); and second, that ECIs act as strong utility substitutes for WCIs by providing an alternative mechanism for worker voice ("ECI substitution"). Under ECI desirability alone, workers with an ECI may still want a WCI. However, under the hypothesis of ECI substitution, an employer can ensure that their workers will not want WCIs at all, merely by instating certain ECIs. I aim to assess how plausible these hypotheses may be.

Notwithstanding the conflict between employers and WCIs, worker voice is one way to solve a fundamental problem of asymmetric information within the firm: for a firm to run efficiently, information must flow from those who have it, who are often workers lower in the firm's hierarchy, to those who need it, who are often decision-makers higher in the firm's hierarchy (Hirschman 1970; Simon 1991). Worker voice is one way to convey this information. The landmark work of Freeman and Medoff (1984), and the vast literature that followed (for an overview, see Barrows 2017), gave a wealth of empirical evidence that unions can in many cases improve the productivity of firms by reducing turnover and ensuring the efficient flow of useful information about production processes (such as time-saving innovations) to decision-makers within the firm. Indeed, some countries such as Germany have both profitable, technologically advanced manufacturing industries and strong unions which are closely involved in the day-to-day cooperative management of the firm (Thelen 1991).

While worker voice is one way that information may flow from workers to employers, monitoring is another, very different way. Where voice increases workers' influence in the firm, monitoring increases managers' influence over workers.[6] Remote work, on the other hand, tends to impede flows of information, both among workers, which unions may find disadvantageous (Ranganathan and Das 2022b), and between workers and employers, which employers may seek to mitigate through digital surveillance. Consequently, to understand the present and future of worker voice in the U.S., one must consider the effects of remote work and monitoring by employers.[7]

[6] It is a formal implication of most standard work-shirk models that profits grow, and wages shrink, when monitoring becomes easier (Bowles and Gintis 2008). Tellingly, most employers who engage in digital monitoring or surveillance report having used it to discipline workers ("6 in 10 Employers Require Monitoring Software for Remote Workers" 2021) ("ExpressVPN Survey Shows Widespread Surveillance on Remote Workers" 2021).

[7] In a 2022 BLS survey, some 28% of U.S. private sector business establishments reported that their employees worked remotely some or all of the time(Bureau of Labor Statistics 2023). Credible estimates of the prevalence

In this study, I consider remote work and surveillance as contextual factors that may interact with worker voice institutions in several ways. If a worker is averse to surveillance, then the presence of surveillance may increase her desire for voice institutions such as a union, ERG, or HR with which to oppose, constrain, or mitigate surveillance and its effects. On the other hand, surveillance might deter some workers from unionized jobs, in the sense that workers might anticipate little personal benefit from having a union, and possibly retaliation by their employer, if their employer can monitor union activities; this has been a serious concern of the National Labor Relations Board (NLRB) (Abruzzo 2022). Additionally, remote workers might be less inclined to form or join worker voice institutions, owing to a lack of communication, trust, or solidarity; this appeared to occur in a recent workplace experiment in India (Ranganathan and Das 2022a).

I design and implement a stated-preference survey experiment, following a robust and growing literature in labor economics in which such experiments are used to quantify workers' WTP for the non-pecuniary "amenities" of jobs (Wiswall and Zafar 2018; Mas and Pallais 2017, 2019; Maestas, Mullen, and Powell 2018; Maestas et al. 2023). Several studies have estimated WTP for job attributes similar to those I study, including WTP for unions with collective bargaining (5-10% of wages) (Hertel-Fernandez, Kimball, and Kochan 2022), WTP for general freedom of self-expression in the workplace (Dube, Naidu, and Reich 2022), and WTP for a remote work option, (Mas and Pallais 2017 (8% of wages); Maestas et al. 2023 (4% of wages)).

In my experiment, each participant repeatedly faces scenarios in which they must choose one of two hypothetical jobs, each presented with a descriptions of wages and various non-wage "amenities." [8] I generate each hypothetical job with a randomized wage and five independently randomized binary nonwage attributes. Briefly (see Section 2.1.1 for more detail), these binary attributes are unionization (present or absent), HR ("standard," or "proactive"), ERG (present or absent), remote work (allowed every day, or none) and monitoring (basic weekly productivity monitoring, or extensive digital surveillance of work and productivity).

The heuristic underlying such an experiment is that if a rational agent chooses the job with the lower wage, then she must prefer her chosen job's nonwage attributes over the alternative job's nonwage attributes, at least as much as the amount of money she has foregone. So the foregone wages will identify (place bounds on) the agent's willingness-to-pay (WTP) for the difference in nonwage attributes. In Section 2.3, I build a utility model to formalize this heuristic, and show how OLS regressions, and Bayesian hierarchical regressions, can be used to consistently estimate average WTP in a heterogeneous population.

Identification of WTP would be extremely difficult to obtain reliably from observational (i.e. non-experimental) data (see the discussion in Lavetti 2023; Maestas et al. 2023). In stated-preference experiments like mine, the main threats to validity are that respondents' choices in an experiment may not reflect their actual preferences or real-world choices, and that survey respondents may differ systematically from the population(s) of interest.

---

of digital surveillance are scarce (see Mast and Kresge 2022 for a recent, comprehensive review); perhaps the best evidence so far (and this is far from ideal) is that the EU's 2019 European Company Survey found that 27% of surveyed establishments used "data analytics for employee monitoring" (Eurofound 2020). Digital monitoring of workers' use of apps, websites, or keystrokes appear to be particularly prevalent among businesses which allow remote work, with estimates from online surveys ranging from 60% ("6 in 10 Employers Require Monitoring Software for Remote Workers" 2021) to 75% ("ExpressVPN Survey Shows Widespread Surveillance on Remote Workers" 2021).

[8] Related designs include asking respondents to rank more than two options, or to report their subjective probability of choosing each option.

One cannot rule out such biases, but they are unlikely to be large enough to compromise my conclusions. Responses in previous labor economic choice experiments have tended to track respondents' real-world choices: for example, authors have typically found strong positive correlations between the attributes of the jobs that respondents actually have and the attributes that respondents appear to prefer on the survey, (Mas and Pallais 2017; Wiswall and Zafar 2018) including in a nationally representative survey (Maestas et al. 2023). I also find such correlations, of considerable magnitude.

Like previous authors, I subject some participants to a simple "attention test," albeit of an apparently novel kind. As a byproduct of my randomization scheme, in some scenarios the respondent faced two jobs with identical descriptions save for differing wages. I found that in these scenarios, 85% of respondents picked the higher-wage (and presumably more desirable) job, passing this attention test, which compares favorably to the 65% attention rate estimated in a previous labor economic choice experiment (Maestas et al. 2023).[9] My findings also hold up reasonably well in the sub-sample consisting solely of respondents who face and pass the attention test. Modeling and adjusting for inattention using a Bayesian method, my parameter estimates tend to be slightly larger, and my conclusions still hold.

I find that my respondents value unions (vs. no unions) at approximately 3% of hourly wages, proactive HR (vs. standard HR) at 4%, and ERGs (vs. no ERG) at 5%. These valuations are large and statistically significant, but have the right order of magnitude; for comparison, union dues for the United Auto Workers are currently around 1.44% of wages (UAW 2015). Among women and people of color, WTP for each of the above voice institutions tends to be considerably greater.

My confidence intervals are sufficiently narrow to indicate that respondents do on average value both proactive HR and ERGs more than they value unions, supporting the ECI desirability hypothesis. However, I also estimate essentially zero substitution between unions and proactive HR, and between unions and ERGs, with confidence intervals narrow enough to rule out substitution effects greater than 1%; thus the evidence is firmly against the ECI substitution hypothesis.

I find that my respondents strongly value remote work, at about 13% of wages, and that they even more strongly disvalue digital surveillance, considering it to be equivalent to a 16% pay cut. My joint experimental design also allows me to assess the hypothesis that workers value remote work primarily as a way to escape monitoring by their employers: if this were so, one would expect that in jobs with high surveillance, adding a remote work option (while keeping surveillance high) would not make the job more valuable to the worker. That is, one would expect a substitution effect between remote work and surveillance that roughly equals the positive effect of remote work without surveillance. I do find a statistically significant substitution effect, but an order of magnitude smaller than the positive effect of remote work that digital surveillance does decrease the desirability of remote work, but that most of the desire for remote work is not explained by the desire to escape surveillance (or, presumably, effort). Because I estimate this substitution effect indirectly, respondents may be more truthful about their motivations for remote work than they would be if asked directly.

I focus primarily on estimating population average WTP. However, one needs more than an average to understand, let alone to make informed policy decisions about, a heterogeneous population (Wiswall and Zafar 2018). My experimental design allows me to study more than just the population average, since each participant in my experiment makes nine choices in total, identifying individual preferences, albeit weakly. Using a Bayesian discrete choice model to corroborate and supplement

[9] Their paper used a slightly different "trick question" test.

my OLS models, I estimate not only population mean WTP (producing estimates that closely match my OLS estimates), but also the population standard deviation and quantiles of WTP for each nonwage job attribute. If population standard deviations are as large as my estimates suggest, then considerable differences exist in the population concerning the value of these job attributes, and that we must be wary of considering only population average WTP in our conclusions.

Correlations in the population distribution of WTP are also of interest, because they tell us how preferences for job attributes tend to be associated with each other in the population, which can help corroborate (but by no means prove) my hypotheses about individuals' underlying motivations. For example, one would expect substantial, positive pairwise correlations between WTPs for unions, proactive HR, and ERGs, under the hypothesis that the desire for each of these attributes is typically motivated in part by an underlying desire for worker voice or power. People who prefer unions, and possibly other forms of worker voice, may also tend to prefer freedom from surveillance. I find that such correlations are substantial, with a few exceptions that I discuss.

My sample is drawn from attendees of an employment center in Holyoke, Massachusetts, a post-industrial city with a large immigrant population (Borges-Méndez 1995), whose median household income is approximately $45,000, just above half the national average.[10] Thus, compared to other social science experiments, which tend to recruit from among college students, (Henrich, Heine, and Norenzayan 2010) my sample is drawn from an older, lower-income, less-white, working population, making it relatively unusual.

My sample does differ in some crucial respects from both the employment center attendee population, for which I have administrative data, and the Massachusetts population, for which I use CPS ASEC data (Flood et al. 2023): namely, the sample has a greater representation of people who are older, women, non-white, and who report having post-graduate degrees. The sample income distribution and unionization rate, however, are quite similar to those of the Massachusetts population. Re-weighting the sample to resemble the Massachusetts population does not undermine my substantive findings, and indeed increases the magnitude of most of my estimates, although it also increases their standard errors.

In Section 2.1, I discuss the survey design, structure, and participant recruitment. In Section 2.2, I describe the sample: sample size, demographics; and labor market characteristics such as income, employment status, and union presence. In Section 2.3, I describe the rational choice model which underpins the econometric models I build in Section 2.4. I report my findings in Section 3, discussing the broader implications in Section 4.

2 Data and Methods

2.1 Survey Design and Structure

Testing of the survey instrument began with informal pilot interviews of visitors to the MassHire Holyoke employment center, to ensure that the survey text and structure were appropriate to the target population. Partners from the MassHire Holyoke employment center then sent recruitment emails to a total of nearly 85,000 current and former MassHire clients. Emails were sent in multiple rounds, beginning with three formal pilot rounds of 1,000 emails each.

[10] U.S. Census Bureau, 2023. QuickFacts. https://www.census.gov/library/publications/2023/demo/p60-279.html

Each email contained a link to the survey, which is hosted on the Qualtrics platform. As an incentive, we offered participants the option to enter a raffle for a $50 Amazon gift card.

The survey begins by asking questions about participants' current or most recent job; in particular, about pay, remote work, and surveillance. The core of the survey consists of nine separate choice scenarios. In each scenario, each participant is asked to choose one of two hypothetical jobs, labeled Job A and Job B, that were described in a two-column table. The first row of the table listed each job's wage, and subsequent rows described each job's non-wage attributes, with one row per job attribute. Beneath each column was a button that the participant could click to indicate that they would prefer to take that job.

The participants were asked to imagine that both hypothetical jobs were identical to their current job, except in the ways that we described. All hypothetical jobs were described as being 40 hours per week, from 8:30 AM to 5 PM.

For each pair of hypothetical jobs, I randomized wages in the following manner. First, a "base wage" was selected: if in the previous section the participant had reported being paid at a rate that translated to a full-time hourly rate between $12.50 and $200, then I used this reported wage as their base (hourly) wage in all nine scenarios; otherwise, I independently drew a base log-wage from a moment-matching Normal approximation of the Massachusetts log-wage distribution (mean = 3.6, variance = 0.49). Next, given the base hourly log-wage, I independently drew log-wages for Job A and Job B from a Gaussian distribution with mean equal to the base logwage, and variance 0.0196, similar to Maestas et al. (2023). Participants were shown wages rounded to the nearest cent. This process ensured that participants tended to see wages plausibly similar to their current one, and that log-wage differences between Job A and Job B were a) independent of any confounders, and b) usually not overwhelmingly large.

In the survey, each hypothetical job had five types of non-wage attribute: unionization, HR, ERG, remoteness, and surveillance. Each type of attribute had two possible categories (which I will describe below), from which I randomly (with probability 1/2 each) selected one per job. Randomization of non-wage attributes was independent across all attributes, jobs, scenarios, and individuals.

2.1.1 Overview of Nonwage attributes

I presented participants with the following non-wage characteristics:

1. Work location [ONE of the following, randomized]
    a) Each weekday, you must commute to your workplace by car or bus, taking about 20 minutes.
    b) You have the option to work remotely every weekday. You can also choose to go to your organization's workplace, which takes about 20 minutes by car or bus.
2. Oversight [ONE of the following, randomized]
    a) Your employer tracks the quality of your work and your overall weekly productivity. Your employer does NOT closely monitor exactly when or how you work, and they do NOT monitor your conversations with co-workers.
    b) Your employer tracks the quality of your work and your overall weekly productivity. They also use audio, video, GPS, and software surveillance technology to closely monitor when, where, and how you are working during work hours, including your conversations with co-workers.

3. Human Resources [ONE of the following, randomized]
    a) Your employer has a standard Human Resources Department (HR). HR administrates hiring, firing, and payroll.
    b) Your employer has a proactive Human Resources Department (HR). In addition to administrating hiring, firing, and payroll, HR runs surveys and workshops to hear employees' thoughts and suggestions about working conditions. If there is something you would like to change about your job, you can talk to HR.
4. Union Status. [ONE of the following, randomized]
    a) This workplace has no workers' union.
    b) In this job, you would be a member of a workers' union. The union is a democratic organization that legally bargains with your employer to seek fair pay and good working conditions for all workers. If there is something you would like to change about your job, you can ask your union to negotiate with management to seek the change. This job's wage already has union dues deducted from it.
5. Employee Resource Group (ERG). [ONE of the following, randomized]
    a) This workplace has no specific program to employee engage employees about job satisfaction or productivity.
    b) In this job, you could choose to join an Employee Resource Group (ERG). The ERG is a management-led group where employees and managers with common interests meet and share ideas for new company policies that would improve job satisfaction and productivity. If there is something you would like to change about your job, you can talk to the ERG. Joining the ERG doesn't come with extra pay or costs.

2.2 Sample Characteristics

The number of participants who answered at least one job hypothetical is 2,614; these participants made a total of 22,222 choices.

There are several important differences between our sample and the MassHire administrative dataset, as well as the Current Population Survey (CPS) sample, as shown in Table 1. Compared to both of these reference populations, the sample has greater representation of people who are women, older, non-white, and who report having post-graduate degrees. These differences will not bias our estimates of WTP among participants in the sample, but will mean that our un-adjusted estimates do not exactly reflect the reference populations. In the appendix, we consider estimates which have been re-weighted to represent the population, showing that our general conclusions still hold, although point estimates do shift to some extent, often becoming larger in magnitude.

Table 1: Sample Demographics

Characteristic	Survey Sample N = 2,614	MHH Admin[1]	ASEC 2022[2]
Age			
(0,22]	2.1%	12%	26%
(22,40]	29%	45%	25%
(40,65]	59%	40%	32%
(65,100]	10%	3.4%	17%
Race			
Asian	4.4%	2.4%	9.6%
Black	8.5%	15%	12%
White	72%	83%	79%
Other	15%		0%
Ethnicity			
Hispanic		47%	12%
Non-Hispanic		53%	88%
Gender			
Female	59%	50%	52%
Male	39%	50%	48%
Non-binary / third gender	1.0%		
Prefer not to say	1.4%		
Children under 18			
No	68%		79%
Yes	32%		21%
Schooling			
Less than High School	2.8%	18%	12%
High School	12%	43%	23%
Some College	22%	25%	20%
Vocational Degree	6.4%	2.8%	
Four-Year College Degree	27%	10%	27%
Post-Graduate	30%	0.7%	19%
Employed			
No	38%		38%
Yes	62%		62%
Hourly Wage Equivalent			
25% Quartile	18		15
Median	26		29
75% Quartile	47		48
Position			
Employee	77%		85%
Manager	23%		15%

Note:

Here I compare the survey sample demographics to two reference populations.

[1] MassHire administrative data: year 2019 attendees.

[2] ASEC 2022, Massachusetts.

I asked respondents to select all worker voice institutions that were present in their workplace, from a list of common institutions. In Table 2 we see that Human Resources departments and Town Hall meetings are fairly common, in 53% and 37% of workplaces. The rate of union coverage, at 13%, is just above the Massachusetts rate, at 10.1% (U. S. Bureau of Labor Statistics 2022). Employee Committees (such as the Employee Resource Group) are nearly as common as unions, at 10%, but "Alt Labor" organizations, which support and represent workers without formal collective bargaining, are rare. However, owing the heterogeneous nature of Alt Labor, it is possible that more exist which do not exactly match the descriptions I gave. I asked in particular about non-union worker organizations which "negotiate regularly with management," because I was interested in those which facilitate worker voice specifically and directly; but many Alt Labor organizations do not negotiate directly with management, instead supporting workers in areas ranging from healthcare and childcare to job training (Fisk 2020). Thus the above figures may underestimate the prevalence of Alt Labor organizations.

I also asked respondents to select which forms of monitoring were, to their knowledge, present in their workplaces. Only 9% of respondents believe that they face software tracking of digital devices, and 7% believe that their location or movements are tracked. This estimate is much lower than the rates estimated in the surveys aggregated by Mast and Kresge (2022), where rates of digital surveillance are usually over 30% or so, and often as high as 60%. This raises the possibility that many of my respondents are unaware that they are being monitored (only 7% even suspect surveillance of an unknown form), in which case digital surveillance would not be "priced in" to respondents' current wages.

Table 2: Characteristics of Current or Most Recent Job

(a) Voice characteristics

Voice mechanism	N	Percent
Alt Labor[1]		
No	2,342	90%
Yes	57	2%
NA	215	8%
Employee Committee[2]		
No	2,136	82%
Yes	263	10%
NA	215	8%
HR		
No	1,011	39%
Yes	1,388	53%
NA	215	8%
Town Hall Meetings		
No	1,444	55%
Yes	955	37%
NA	215	8%
Union		
No	2,071	79%
Yes	328	13%
NA	215	8%

[1] Alt Labor: an organization that is not a formal union, but is worker-led and independent from company management, and negotiates with management on a regular basis.

[2] Employee Committee: a committee of employees that may be sponsored by management, which meets regularly with management to discuss workplace issues.

(b) Monitoring

Monitoring mechanism	N	Percent
Location or movements tracked		
No	2,253	86%
Yes	188	7%
NA	173	7%
Manager visits workplace		
No	1,559	60%
Yes	882	34%
NA	173	7%
No tracking of work, only output		
No	1,377	53%
Yes	1,064	41%
NA	173	7%
Prompt response to communications required		
No	1,912	73%
Yes	529	20%
NA	173	7%
Software tracks digital devices		
No	2,193	84%
Yes	248	9%
NA	173	7%
Worker suspects surveillance of unknown form		
No	2,264	87%
Yes	177	7%
NA	173	7%

2.3 Rational Choice Model

In this section I build a standard utility-maximization model of how a participant might choose between Job A and Job B in each choice scenario. I use the parameters of this model to define an individual's Willingness-to-Pay (WTP) starting wages for nonwage characteristics. In later sections, I will estimate the parameters of this model using my survey data, and construct estimates of WTP for individuals and populations from these parameter estimates.

In my study, each participant i sequentially experiences scenarios $j = 1, \dots, 9$. In each scenario j the participant i must choose between two options: a) hypothetical Job A, which has starting wage $w^A_{i,j}$ and five binary nonwage characteristics which we will represent with vector $x^A_{i,j} = (x^A_{i,j,1}, \dots, x^A_{i,j,5})$; and b) hypothetical job B with $w^B_{i,j}$ and $x^B_{i,j}$ similarly defined. For example, $x^A_{i,j,1} = 1$ could indicate that for individual i in scenario j, job A *does* have a union, while $x^A_{i,j,1} = 0$ would indicate that it *does not* have a union.

When faced with Job A and Job B in scenario j, a rational individual i compares the jobs' respective utilities $u^A_{i,j} = u_i(x^A_{i,j}, w^A_{i,j})$ and $u^B_{i,j} = u_i(x^B_{i,j}, w^B_{i,j})$ [11] and chooses the job with strictly greater utility, unless the jobs have equal utility, in which case the individual may choose either option with equal probability. To model this process, let us define the variable $y_{i,j}$ as an indicator that individual i chooses Job A instead of Job B in scenario j.

Then a fully rational and attentive agent with well-defined preferences would choose job A according to:

$$\Pr(y_{i,j} = 1 | u^A_{i,j}, u^B_{i,j}) = \begin{cases} 1 & : u^A_{i,j} > u^B_{i,j} \\ 0.5 & : u^A_{i,j} = u^B_{i,j} \\ 0 & : u^A_{i,j} < u^B_{i,j} \end{cases} \tag{1}$$

A participant may exhibit some degree of variation $\delta > 0$ in his choices, one that is not attributable to utility or rational beliefs, such as due to irrationality or inattention, particularly when he is nearly indifferent between the available options. When modeling such variation in a Bayesian framework, I will replace Equation 1 with Equation 2:

$$\Pr(y_{i,j} = 1 | u^A_{i,j}, u^B_{i,j}) = \Phi\left(\frac{u^A_{i,j} - u^B_{i,j}}{\delta}\right) \tag{2}$$

where Φ is a cumulative distribution function that is continuous and symmetric about zero. I use the standard Normal CDF. As the "irrationality parameter" δ approaches zero, Φ approaches the function in Equation 1. In the Bayesian model, δ is estimated from the data.

2.3.1 Willingness-to-pay

Suppose the utility of a job is linearly separable in its non-wage characteristics x and its log-wages $\ln(w)$, (Lewbel 2014) so that individual i's utility function can be written as follows:

[11] I will suppress the individual's expectation function in this paper as it is not needed for our problem; if the reader wishes, they may substitute "expected utility" for "utility" throughout this paper.

$$u_i(x, w) = v_i(x) + \ln(w) \tag{3}$$

Here, $v_i(x)$ is called the value function or the valuation of x.

For simplicity, in this subsection I will discuss a single (univariate) nonwage attribute x which takes binary values 0 and 1. Since x is binary, I need only consider the two valuations $v_i^1 = v_i(1)$ and $v_i^0 = v_i(0)$.

I define individual i's logwage willingness-to-pay for $x = 1$ (instead of $x = 0$) as

$$\Delta_i = v_i^1 - v_i^0$$

This means that if fully rational, the individual i chooses Job A according to whether the nonwage value-gain from Job A is greater than the logwage sacrifice:

$$\Delta_i(x^A - x^B) > \ln(w^B) - \ln(w^A)$$

2.3.2 Substitution in non-wage parameters

For simplicity, suppose each job has just two binary nonwage attributes x, z, so I write $u_i(x, z, w) = v_i(x, z) + \ln(w)$, where pairs of attributes (x, z) have arbitrary values $v_i^{1,1} = v_i(1, 1)$; $v_i^{1,0} = v_i(1, 0)$; $v_i^{0,1} = v_i(0, 1)$; $v_i^{0,0} = v_i(0, 0)$.

I can parametrize the difference in nonwage values between Job A and Job B as follows:

$$v_i(x^A, z^A) - v_i(x^B, z^B) = \Delta_i^x(x^A - x^B) + \Delta_i^z(z^A - z^B) + \Delta_i^{x,z}(x^A z^A - x^B z^B) \tag{4}$$

In this case the "marginal" coefficients are defined as $\Delta_i^x = v^{1,0} - v^{0,0}$ and $\Delta_i^z = v^{0,1} - v^{0,0}$, and the interaction term has a difference-in-differences form:

$$\Delta_i^{x,z} = (v^{1,1} - v^{0,1}) - (v^{1,0} - v^{0,0})$$

This interaction term quantifies how the individual's marginal valuation of an option with attribute $x = 1$ (rather than $x = 0$) increases, when that option switches from having attribute $z = 0$ to $z = 1$; or equivalently, how the individual's marginal valuation of an option with attribute $z = 1$ (rather than $z = 0$) increases when that option switches from $x = 0$ to $x = 1$. In other words, $\Delta_i^{x,z}$ quantifies the complementarity (if it is positive) or substitutability (if it is negative) in utility between $x = 1$ and $z = 1$.

2.3.3 Discussion of the utility model

It is an important feature of my experimental design and model that each individual i's rational beliefs and preferences, if these are well-defined, would not change in any relevant way between that individual's nine repeated hypothetical choice scenarios. This is because the experimental scenarios occur in rapid succession, with little chance for the participant's needs, information, cognitive abilities, background circumstances, or other life experiences to change, and with no chance for participants to actually experience (and thereby gain information or satiation from) the consequences of their choices in my experiment. Moreover, participants are given no reason whatsoever to believe that two identically described jobs in different scenarios might have different "unobservable attributes" (attributes other than those I explicitly describe); indeed, participants are asked to imagine that any attributes I do not describe are equal to those of their current job.

So, if two of our scenarios were identical; that is, both Job As had identical descriptions and both Job Bs had identical descriptions; then a rational and attentive participant must in principle have the same beliefs and utilities concerning the same options in both scenarios, and must therefore make the same choice both times — save only if the participant were perfectly indifferent between Job A and Job B. An actual participant might do otherwise, perhaps due to irrationality or inattention; and indeed my econometric methods accommodate this possibility; but one could not plausibly ascribe such variation in an individual's behavior to random changes in utility or rational beliefs.

In this respect my framework differs from that of standard Random Utility Models, which are perhaps more appropriate for empirical analysis of observational data, where virtually any of the factors I have mentioned might plausibly differ between seemingly identical (to the economist) scenarios, including factors such as: unobservable (to the economist) features of the available options, the individual's information or beliefs, individuals' degrees of satiation for different goods or services, the individual's preference themselves, etc. In the more controlled environment and short time-span of our choice experiment, these sources of "random utility" are simply not plausible.

2.4 Econometric methods

2.4.1 Benchmark models: Linear Probability Models

Following Dube, Naidu, and Reich (2022), my baseline econometric model is a linear probability model (LPM) analogous to our linear utility model:

$$y_{i,j} = \alpha + \sum_{k=1}^{5}(x^A_{i,j,k} - x^B_{i,j,k})\beta_k + \gamma(\ln(w^A_{i,j}) - \ln(w^B_{i,j})) + \epsilon_{i,j} \tag{5}$$

where as before, $y_{i,j}$ is the indicator for individual i in scenario j choosing option A instead of option B, and option A and option B have $k = 1, \ldots, 5$ binary nonwage attributes denoted $x^A_{i,j,k}, x^B_{i,j,k}$, respectively. Throughout, we can assume that the logwage effect $\gamma > 0$.

If Equation 5 is a well-specified model, meaning that the true conditional expectation of the outcome $\mathbb{E}[y_{i,j}|X]$ is equal to the right-hand-side without the error term $\epsilon_{i,j}$, then the error term is mean-zero $\mathbb{E}[\epsilon_{i,j}|X] = 0$, and an Ordinary Least Squares (OLS) regression is unbiased.

Fixed effects (e.g. per individual) are not necessary here. By design, individual-specific phenomena, including individual heterogeneity in utility-parameters, cannot be correlated with the non-wage regressors, which I randomized completely exogenously. Wages themselves in our experiment are not fully exogenous, since I drew the hypothetical jobs' wages for each individual from a distribution whose mean was that individual's current (log) wage; but by this construction, *differences* in logwages (and any function of these) are completely independent of individual characteristics.

To estimate substitution effects, I add interaction terms to Equation 5, much as in Equation 4. For example, to estimate substitution effects between attributes 1 and 2 (e.g. the unionization status indicator and the remote work indicator), I would use the following specification:

$$\begin{aligned} y_{i,j} = \alpha + \sum_{k=1}^{5}(x^A_{i,j,k} - x^B_{i,j,k})\beta_k + (x^A_{i,j,1}x^A_{i,j,2} - x^B_{i,j,1}x^B_{i,j,2})\beta_{1,2} \\ + \gamma(\ln(w^A_{i,j}) - \ln(w^B_{i,j})) + \epsilon_{i,j} \end{aligned} \tag{6}$$

Here, $\beta_{1,2}$ describes the degree of complementarity (if it is positive) or substitutability (if it is negative) between attribute-category $x_{i,j,1} = 1$ (e.g. "has a union") and attribute-category $x_{i,j,2} = 1$ (e.g. "has a remote work option").

Owing to my experimental design, the interaction terms are uncorrelated with their corresponding "main" terms, so if a specification with interactions (e.g. Equation 6) is in fact well-specified, then OLS estimates based on a model without these interactions (e.g. Equation 5) will still be unbiased for the average "marginal" effects — averaging over the interaction effects.

To understand what this econometric model says about WTP, we can consider Equation 5 using a heuristic. For a given individual i in a scenario j, suppose initially all attributes are identical at $x^A_{i,j,k} = x^A_{i,j,k} = 0$. Next, suppose we shift one Job A attribute $x^A_{i,j,k}$ to $x^A_{i,j,k} = 1$, but meanwhile we change $\ln(w^A_{i,j,k}) - \ln(w^B_{i,j,k})$ by $\frac{\beta_k}{\gamma}$ units. Then the net change in the right-hand-side is zero; that is, the chooser would be indifferent to the change. This seems to indicate that the chooser is willing to sacrifice up to $\frac{\beta_k}{\gamma}$ in log-wages (or must receive at least that amount, if β_k is negative) to have $x_{i,j,k} = 1$ rather than in $x_{i,j,k} = 0$. Put more simply, $\frac{\beta_k}{\gamma}$ is that individual's willingness-to-pay for $x_{i,j,k} = 1$.

As we shall see in the next section, the above heuristic is essentially correct for the average WTP in a heterogeneous population, if the LPM is well-specified. However, to move beyond heuristics, and to critically evaluate the LPM, we must more precisely connect our model in Equation 5 to the utility models of the previous section.

2.4.1.1 Deriving Population WTP from the Linear Probability Model

In this subsection, I derive the implications of the LPM concerning the population distribution of WTP, and in particular the population average WTP. This allows me to convert LPM parameter estimates into population average WTP estimates. For simple exposition I will consider x as a single binary attribute, and I will consider only a single choice-scenario per individual, suppressing the subscripts that are not needed. This derivation generalizes straightforwardly.

If Equation 5 is a well-specified model of the choices of individuals randomly sampled from a population and maximizing utilities as in Equation 3, then the expected outcome is $\mathbb{E}[y_{i,j}|X] = \Pr(u^A_{i,j} - u^B_{i,j} > 0)$ (we can ignore the measure-zero case of perfect equality). So the LPM asserts the following about the population distribution of logwage WTP, Δ:

$$\begin{aligned} &\Pr(\Delta(x^A - x^B) + \ln(w^A) - \ln(w^B) > 0 | x^A, x^B, w^A, w^B) \\ &\qquad = \alpha + \beta(x^A - x^B) + \gamma(\ln(w^A) - \ln(w^B)) \end{aligned}$$

Consider the simple case where $x^A - x^B = 1$ and let $s = -(\ln(w^A) - \ln(w^B))$ denote the logwage sacrifice. Here, the linear model says

$$\Pr(\Delta > s) = 1 - F(s) = \alpha + \beta - \gamma s$$

where F is the population cumulative distribution function of WTPs Δ. Probabilities are bounded between 0 and 1, so we must consider s bounded between $a = \frac{\alpha+\beta-1}{\gamma}$ and $b = \frac{\alpha+\beta}{\gamma}$ defined such that the probability bounds are met: $\alpha + \beta - \gamma b = 0$ and $\alpha + \beta - \gamma a = 1$. Therefore the support of Δ is bounded between a and b.

If we take the derivative of F with respect to s, then the resulting probability density is constant. In other words, the LPM says that the distribution of logwage WTP is uniform between the bounds we have just derived:

$$\Delta \sim \text{Unif}\left(\frac{\alpha + \beta - 1}{\gamma}, \frac{\alpha + \beta}{\gamma}\right) \tag{7}$$

This implies that the population average (and median) logwage WTP is the following:

$$\mathbb{E}\Delta = \frac{\alpha - 1/2}{\gamma} + \frac{\beta}{\gamma} \tag{8}$$

Notice that the deviation of α from 1/2, which would quantify a (problematic) preference for the arbitrary label "Job A" over the arbitrary label "Job B," contributes an extra term; in our data, reassuringly, $\alpha \approx 1/2$ with high precision (i.e. participants do not care about such labels), so this extra term vanishes. Hence Equation 8 matches our heuristic from the previous section.

If the LPM is well-specified, then substituting one's OLS estimates into Equation 8 produces a biased but consistent plug-in estimator of population mean wage-share WTP. A block bootstrap (grouped by the individual) will produce valid CIs.

2.4.1.2 Limitations of the LPM

The LPM has a limitation worth noting. As we have seen in the previous section, a careful derivation of population mean WTP and its estimator requires that we have a well-specified model of the entire conditional expectation function $E[y_{i,j}|x_{i,j}, w_{i,j}]$. It is an elementary fact that $E[y_{i,j}|x_{i,j}, w_{i,j}] \in [0, 1]$ everywhere, since $y_{i,j} \in [0, 1]$. However, notoriously, fitting the LPM with OLS often produces "predicted probabilities" — which are not only predictions, but also *estimates* of the function $E[y_{i,j}|x_{i,j}, w_{i,j}]$ evaluated at the observed covariate-values — that fall outside $[0, 1]$; indeed, this violation of the probability bounds occurs in roughly 4% of our sample. In such cases, the OLS estimates do not produce a well-specified model of the entire conditional expectation function, strictly speaking.

Still, I find that in practice, because OLS estimates a "best linear approximation" (White 1980) of the cumulative distribution function of WTP, OLS can provide a surprisingly good approximation of the population mean WTP, if not of other features of the distribution. Rather than discarding the LPM altogether, I conclude that it must be corroborated and supplemented with other, more flexible models that also deliver estimates of other quantities that the LPM does not credibly estimate: population quantiles, and population variance and covariance of WTP. As we shall see, a Bayesian model which assumes that population WTP is Normally distributed rather than uniformly distributed also produces estimates of population mean WTP that agree with the results of our LPM, lending credence to both approaches.

2.4.2 Bayesian hierarchical probit model

A Bayesian framework allows us to straightforwardly derive principled estimates and uncertainty quantification about not only the population mean WTP, but also the population covariance of WTP, and even each individual participant's WTP (albeit usually with considerable uncertainty). This more complete picture of population preferences beyond the average WTP allows us to shed light on heterogeneity in preferences and associations between preferences for different job attributes (I reserve a more complete discussion for the relevant section), which is not possible with our baseline OLS models.

In order to supplement the LPM, I use a Bayesian hierarchical Probit model.

The first stage of the Bayesian model is a sampling model that corresponds to Equation 2: for each individual i in scenario $j = 1, \dots, 9$, the probability of choosing Job A ($y_{i,j} = 1$) is

$$\Pr(y_{i,j} = 1 | x_{i,j}, w_{i,j}, \beta_i) = \Phi\left(\frac{u_{i,j}(x^A_{i,j}, w^A_{i,j}) - u_{i,j}(x^B_{i,j}, w^B_{i,j})}{\delta}\right)$$

independently across j, where $\delta > 0$ is an irrationality term to be estimated from the data, Φ is a continuous, symmetric CDF; I use the Normal CDF.

I assume each individual i has a utility function u_i that is linear, with a K-vector of coefficients β_i:

$$u_i(x_{i,j}, w_{i,j}) = x_{i,j}\beta_i + \gamma \ln(w_{i,j})$$

It follows from this definition that the WTP on the wage share scale is $\Delta_i = \beta_i/\gamma$ and the irrationality term is $\delta = 1/\gamma$.

In the second stage of the sampling model, I suppose that the utility parameters β_i vary between individuals, and are drawn independently from a common population distribution with an unknown mean μ and unknown covariance matrix Σ that will be estimated from the data. For simplicity, I begin by assuming Normality, but this is not essential to the method:

$$\beta_i \sim \mathcal{N}_K(\mu, \Sigma)$$

So we may write the sampling model of the data given the unknown parameters in a factored form:

$$f_2(\beta_1, \dots, \beta_n | \mu, \Sigma) f_1(\mathbf{y} | \beta_1, \dots, \beta_n, \gamma) = \prod_i \mathcal{N}_K(\beta_i | \mu, \Sigma) \prod_j \Pr(y_{i,j} | x_{i,j}, w_{i,j}, \beta_i, \gamma)$$

To close the model, we must specify a prior probability distribution $\pi_0(\gamma, \mu, \Sigma)$ for the remaining unknown parameters:

$$\mu \sim \mathcal{N}_p(\mu_0, V_0)$$
$$\Sigma \sim \mathcal{LKJ}$$

I use standard weakly informative priors, centered on zero WTP. By $\mathcal{LKJ}$, I mean the distribution induced by an LKJ prior on the correlation matrix and independent Gamma distributions on the scales. (see Section 5.4).

Together, the prior π_0 and the sampling model (likelihood) f_1, f_2 imply a posterior probability distribution π_n of the unknown parameters given the data, via Bayes' Theorem:

$$\pi_n(\mu, \Sigma, \beta_1, \ldots, \beta_n, \gamma|\mathbf{y}) \propto f_2(\beta_1, \ldots, \beta_n|\mu, \Sigma) f_1(\mathbf{y}|\beta_1, \ldots, \beta_n, \gamma)\pi_0(\mu, \Sigma, \gamma) \tag{9}$$

The posterior distribution quantifies our inferences about the unknown parameters, and functions of the parameters such as $\Delta_i = \beta_i/\gamma$, given the data. From the posterior distribution, we can derive point estimates for each parameter in the form of the posterior mean, such as $\mathbb{E}[\Delta_i|\mathbf{y}, \mathbf{X}]$. From quantiles of the posterior distribution for each parameter, we can also derive confidence intervals (credible intervals) C such that, for example, $\Pr(\Delta_i \in C|\mathbf{y}, \mathbf{X}) = 0.95$.

2.5 A note on interpreting the estimates

Most of my primary estimates are of the expected value to each worker of various combinations of job attributes, averaged across workers. This expected value is based on the job offer text that the survey respondent considers *ex ante*, before they would (if the hypothetical were real) experience whatever job they choose. In a real career, unionization campaign, or job search process, a worker gains information and experience along the way; and her enthusiasm for, say, a union or an ERG, may wax or wane accordingly in a way which she does not anticipate. So, for example, a survey respondent who genuinely prefers an ERG to a union in our survey *ex ante* may not continue to hold that preference, if experiencing an ERG without a union fails to live up to that worker's expectations. It may be that the without direct experience, a respondent cannot entirely anticipate how it would feel, for a different example, to work remotely for a long period of time without ever meeting their co-workers in person, or how that might affect their attitudes toward a union. With this caveat in mind, let us turn to the results of the survey.

3 Results

3.1 Main results

In Table 3, under the OLS Baseline model, we see that workers value unions (vs. no unions) at a wage share of approximately 2%, proactive HR (vs. standard HR) at 4%, and ERGs (vs. no ERG) at 5%, amounts that are statistically and practically significant, and of a plausible magnitude: for comparison, union dues for the United Auto Workers are currently around 1.44% (UAW 2015).

Workers strongly value the remote work option, at about 13% of wages, which is comparable to estimates in Mas and Pallais (2017). Workers even more strongly *dis*value digital surveillance, considering this to be equivalent to a 16% pay cut.

No estimates of union substitution effects are statistically distinguishable from zero in any of the interacted specifications in Table 3. Moreover, the standard errors are small enough to rule out most interaction effects of any practical importance. This rules out perfect substitution, and a great deal of imperfect substitution, between unions and the following: HR, ERGs, remote work, and surveillance.

In the interaction between surveillance and remote work, I estimate a statistically significant substitution effect of -1.9%, but this is an order of magnitude less than the value of remote work (13%), so I can rule out perfect substitution with near certainty. Thus, we can conclude that high surveillance does appear to decrease the desirability of remote work, but the desire for remote work cannot be primarily explained by a desire to escape monitoring.

Proactive HR and high surveillance appear to be complementary to a statistically significant degree in the OLS model, at 1.4% of wages; this may indicate that workers expect that a proactive HR will protect them from some negative effects of surveillance. However, the magnitude of the complementarity is not nearly large enough for a proactive HR to fully alleviate the negative effects of surveillance. Furthermore, it must be noted that for this coefficient, the Bayesian model's 95% credible interval contains zero, although in the Bayesian formalism this does not imply that we fail to reject a null hypothesis.

I find no such interaction effect, either positive or negative, between unions and surveillance, so it appears that at least on average, participants neither expect unions to alleviate the negative effects of surveillance, nor that surveillance would negate the benefits of a union.

Although the statistical assumptions underlying the OLS model and the hierarchical Bayesian model are quite different, their estimates are close enough and their confidence (or credible) intervals are of similar enough width that their conclusions are similar, and mutually corroborating.

Table 3: Willingness-to-Pay for Job Attributes

	OLS Model		Bayesian Model	
	Baseline	Interactions	Baseline	Interactions
Union	0.026	0.024	0.033	0.033
	[0.019, 0.033]	[0.011, 0.036]	[0.022, 0.043]	[0.014, 0.052]
Proactive HR	0.042	0.040	0.046	0.046
	[0.036, 0.048]	[0.030, 0.051]	[0.038, 0.055]	[0.030, 0.063]
ERG	0.050	0.056	0.056	0.064
	[0.044, 0.056]	[0.045, 0.067]	[0.047, 0.064]	[0.047, 0.080]
Surveillance	−0.163	−0.163	−0.186	−0.181
	[−0.172, −0.154]	[−0.178, −0.148]	[−0.196, −0.175]	[−0.196, −0.166]
Remote	0.126	0.136	0.143	0.157
	[0.118, 0.134]	[0.125, 0.147]	[0.133, 0.153]	[0.137, 0.176]
Union × Proactive HR		−0.001		−0.001
		[−0.011, 0.010]		[−0.018, 0.015]
Union × ERG		0.000		−0.003
		[−0.011, 0.011]		[−0.019, 0.014]
Union × Surveillance		0.007		0.006
		[−0.004, 0.017]		[−0.011, 0.023]
Union × Remote		−0.002		−0.001
		[−0.013, 0.009]		[−0.018, 0.015]
Proactive HR × Surveillance		0.014		0.012
		[0.004, 0.024]		[−0.004, 0.028]
Proactive HR × ERG		−0.010		−0.011
		[−0.020, 0.001]		[−0.028, 0.005]
ERG × Surveillance		−0.002		−0.002
		[−0.013, 0.008]		[−0.019, 0.014]
Remote × Surveillance		−0.019		−0.025
		[−0.029, −0.008]		[−0.042, −0.009]

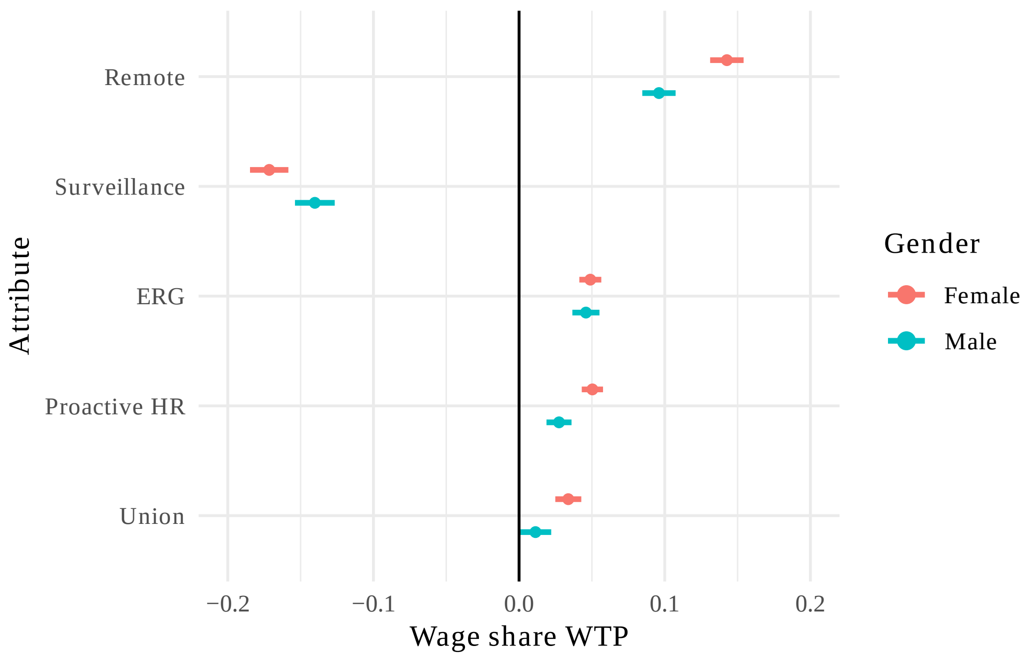
Note:
Estimates and 95% confidence intervals derived from OLS linear probability models (clustered by participant), compared with estimates and 95% credible intervals derived from Bayesian hierarchical models (also clustered by participant).
N = 22,222 choices, by 2,614 participants.

3.2 Heterogeneity

Above, I estimate the population average WTP for certain non-wage job characteristics. Here, I examine population variation in WTP for the same. I consider variation by observable characteristics such as race and gender, but also the unconditional population variance of willingness-to-pay, and correlations between individuals' WTP for each job characteristic, which I estimate using the Bayesian model discussed in Section 2.4.2. Overall, heterogenity is considerable, across virtually every dimension, but the core conclusions of the previous section also tend to hold across most sub-populations I study.

3.2.1 Gender

Figure 1: Willingness-to-Pay by Gender



Note:
Above are OLS estimates and 95% confidence intervals, clustered by participant, calculated separately among male and female respondents, for the average willingness to sacrifice a share of wages (WTP) to have the following job attributes (instead of their alternatives): union (vs. no union), digital surveillance (vs. basic productivity monitoring), remote work option (vs. in-person only), proactive HR (vs. standard HR), ERG (vs. no ERG).

In Figure 1, we see that compared to male respondents, female respondents value all job attributes substantially more (and disvalue surveillance more), except ERGs.

On average, WTP for remote work is estimated to be 14% among women and 10% among men; this large difference has a precedent in Mas and Pallais (2017), who find that women's WTP for work-from-home is nearly twice that of men, although their estimated difference is not statistically significant in their relatively small sample. There is a more distant precedent in Wiswall and Zafar (2018), who estimate a six percentage-point difference in WTP for part-time work, a different but related flexible work arrangement.

In the U.S., women tend to spend approximately 1.6 times as many hours on housework, and 1.9 times as many hours on childcare, as men (Bianchi et al. 2012). This may be a proximate cause for the gender differences in WTP for remote work that I estimate here, and the gender differences in remote work nationally; rates of remote work were 22.5% among women compared to only 17.8% among men.(U.S. Bureau of Labor Statistics 2023)

Some authors have argued that flexible work arrangements (although primarily large gaps in employment, not remote work) contribute substantially to the gender pay gap (Goldin and Katz 2011). While the four percentage-point difference in WTP that I estimate is large, this difference is not sufficiently large to explain the average gender pay gap, which is on the order of fifteen to twenty percent depending on how it is measured (Blau and Kahn 2016).

In my sample, women value proactive HR at 5% of wages, men at 3%, a statistically significant difference. This may be because one of the primary stated functions of HR is to prevent harassment and discrimination in the workplace, and women are significantly more likely to undergo harassment; a recent study found that women were 47% more likely than men to report being harassed in the workplace, and that such harassment was associated with severe health outcomes related to stress (Khubchandani and Price 2015).

It is somewhat surprising that women in my sample value unions at 3% of wages, substantially more than men who value them at 1% (but still with statistical significance). Nationally, the pattern of unionization is reversed: the union membership rate among men is 10.5%, compared to 9.6% among women (U. S. Bureau of Labor Statistics 2023). This illustrates why the compensating differentials approach, which uses correlations in observational data (between, say, gender and labor market outcomes such as wage or unionization), is not necessarily an accurate guide to workers' preferences. It may be that similar to proactive HR, women respondents see unions as additionally valuable for preventing harassment; or for tending to secure desirable benefits; or they simply have a stronger desire for a formal mechanism which will ensure that workers' voices are heard irrespective of gender.

It is also unexpected that the gender difference in WTP for unions and proactive HR is not matched by a commensurate difference in WTP for ERGs, since as we shall see, these desires are all correlated in the sample overall (Table 4).

3.2.2 Race

In Figure 2, we see that WTP varies substantially by race. Compared to white respondents, non-white respondents show greater WTP for every kind of worker voice studied except perhaps proactive HR, but show less WTP for remote work and less aversion to digital surveillance.

This pattern of responses indicates that people of color tend to expect substantially greater gains from worker voice institutions than do whites. This bears on a long-standing puzzle. Historically, unionization rates have long been higher among people of color than among whites (Defreitas 1993), which is still true today, but to a lesser extent; the unionization rate among Black workers (11.6%) is only slightly higher than that for white workers (10.0%) (U. S. Bureau of Labor Statistics 2023). The racial gap in unionization could be explained in at least two ways: first, the industries in which people of color tend to work, such as manufacturing, may happen to be easier to unionize; second,

Figure 2: Willingness-to-Pay by Race

Note: Above are OLS estimates and 95% confidence intervals, clustered by participant, calculated separately among white and non-white respondents, for the average willingness to sacrifice a share of wages (WTP) to have the following job attributes (instead of their alternatives): union (vs. no union), digital surveillance (vs. basic productivity monitoring), remote work option (vs. in-person only), proactive HR (vs. standard HR), ERG (vs. no ERG).

people of color may tend to take unionized positions specifically because of the protections that unions may offer from discrimination.

Rosenfeld and Kleykamp (2012), analyzing CPS data, argued forcefully and convincingly for the latter theory, showing that unionization remains higher among people of color than among whites when one looks within occupations and industries. These authors also cite a wealth of sociological evidence that even though U.S. unions have a well-documented history of racial discrimination, unions have on the balance tended to push for racial equality since they began diversifying their membership in the late 1960s. My estimates corroborate this theory, indicating, to the extent that we can generalize beyond my sample, that people of color do indeed tend to prefer unions more than whites. Conversely, the theory that Rosenfeld advocates provides a plausible mechanism for my estimates: that people of color value unions and other forms of worker voice more than white people at least partly because of the protections that these institutions may afford against discrimination.

3.2.3 Age

Comparing willingness-to-pay by age groups, we see a pattern which confirms some common theories of generational affinities, but confutes others. Participants under 40 years of age are still quite averse to digital surveillance, valuing it at -12% of wages on average, but much less averse than those over

Figure 3: Willingness-to-Pay by Age

Note:
Above are OLS estimates and 95% confidence intervals, clustered by participant, calculated separately among participants younger and older than forty, for the average willingness to sacrifice a share of wages to have the following job attributes (instead of their alternatives): union (vs. no union), digital surveillance (vs. basic productivity monitoring), remote work option (vs. in-person only), proactive HR (vs. standard HR), ERG (vs. no ERG).

40, who value it at -18% of wages on average. Participants under 40 see much greater value in unions (6.3%) than do participants over 40 (1%, with a 95% CI barely excluding zero). Unlike the sample average as a whole, participants under 40 may see more value in unions than in proactive HR (4.5%) or ERGs (5.2%), although the latter difference in point estimates is not statistically significant. Perhaps contrary to common expectations, remote work is substantially more desirable to participants over 40 (13%) than under 40 (10%) in my sample.

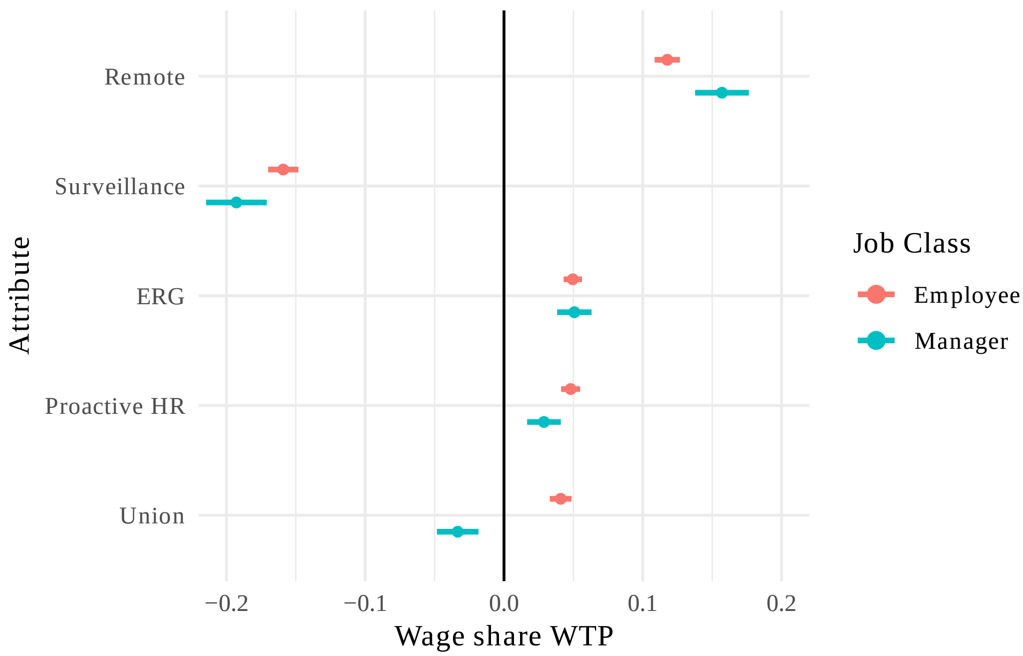
3.2.4 Occupational class

Of all the sub-populations I examine, respondents who are (or were) managers in their current (or most recent) job comprise the only sub-population whose average willingness-to-pay for a union is negative (-3%). However, it is worth noting that the text that described the union to managers said, as it said for all participants, that they too would be members of the union; but managers are not covered under the National Labor Relations Act (Board 23AD), making unionization for managers a slightly fanciful hypothetical. Still, in this hypothetical, it appears that managers on average have a distaste for even unions which would have them as members.

Among non-managerial employees, WTP for unions is 4.1% of wages, only slightly lower than WTP for HR (4.8%) and ERGs (5.0%), from which it is statistically nearly indistinguishable. That is, non-managers see similar value in all the voice institutions that I examine.

Compared to non-managerial employees, managers show significantly greater desire to work remotely (16% versus 12%), significantly less desire for a proactive HR (2.9% versus 4.8%), and significantly more aversion to being subject to digital surveillance (19% versus 16%).

Figure 4: Willingness-to-Pay by Occupational Class



Note:
Above are OLS estimates and 95% confidence intervals, clustered by participant, calculated separately among managers and non-managerial employees, for the average willingness to sacrifice a share of wages (WTP) to have the following job attributes (instead of their alternatives): union (vs. no union), digital surveillance (vs. basic productivity monitoring), remote work option (vs. in-person only), proactive HR (vs. standard HR), ERG (vs. no ERG). Compared to employees, managers show substantially more desire for remote work, more aversion to being monitored, less desire for a proactive HR, and an active aversion even to unions that cover them.

3.2.5 Bayesian hierarchical model

Here, I consider the results of the Bayesian hierarchical model, which allows me to quantify both the variance of WTP for each characteristic, and the correlations between WTP for each characteristic. Concerning population average WTP, Table 3 showed that Bayesian estimates of population average WTP are quite similar those derived from the OLS linear probability model (each model's 95% confidence intervals contain the other model's estimates). This gives us some reassurance about the conclusions of both the LPM and the Bayesian model.

Interestingly, Table 4 indicates that there is a fairly large population standard deviation of WTP for all attributes; that is, WTP varies considerably in the population. For reference, if the population distribution of WTP were Gaussian, so that 32% of the population had WTP at least one standard deviation from the mean, then a 0.155 standard deviation of WTP for unions would imply that 32% of the population had WTP either greater than 19% or less than -13%.

Variance is particularly high for unions, surveillance, and remote work, which might be considered more controversial job attributes than proactive HR and ERGs. Overall, the variation I estimate strongly indicates that population mean WTP, even when estimated precisely, is not sufficient for substantive policy-making, particularly for controversial matters such as unions, surveillance, and remote work.

Table 4: Population WTP: Standard Deviations and Correlations

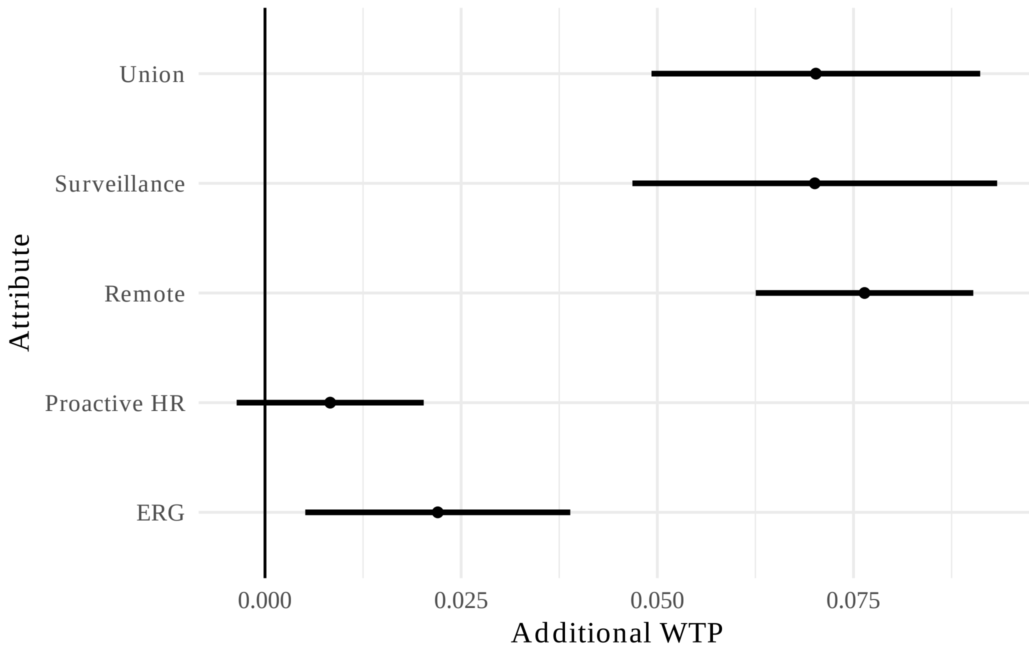
Attribute	Union	Proactive HR	ERG	Surveillance	Remote
Union	0.155	0.130	0.283*	-0.096*	-0.038
Proactive HR	0.130	0.061	0.692*	-0.104	0.059
ERG	0.283*	0.692*	0.066	-0.273*	0.097
Surveillance	-0.096*	-0.104	-0.273*	0.189	-0.290*
Remote	-0.038	0.059	0.097	-0.290*	0.162

Note:
The diagonal terms are the estimated population standard deviation of WTP for each job attribute. The off-diagonal terms are the estimated population correlations between WTP for each characteristic. On off-diagonal terms, the asterisk (*) indicates that the 95% CI for this correlation excludes zero. E.g., there is a positive correlation between a person's WTP for unions and their WTP for ERGs, and the 95% CI excludes zero. These estimates are derived from the Bayesian hierarchical model, and so '*' should not be interpreted as a hypothesis test.

WTP for unions, HR, and ERGs are all significantly positively correlated with each other (albeit less strongly between unions and HR); that is, people who prefer unions also tend to prefer ERGs and HR. People who prefer unions, ERGs, and to a lesser extent HR, also tend to dislike surveillance. All of the above lends credence to the hypothesis that workers perceive unions, proactive HR, and ERGs as related forms of worker voice; and that workers who prefer worker voice institutions also tend to disprefer surveillance.

WTP for surveillance is strongly negatively correlated with WTP for remote work. That is, workers who desire remote work would also tend to desire less surveillance, even if they do not tend to view surveillance as entirely negating the benefits of remote work (see Table 3). It is tempting to attribute the desire for remote work to a desire for autonomy in some general sense, because WTP for remote work and WTP for non-surveillance (which presumably constitutes a form of autonomy) are correlated. However, the desire for remote work is not strongly correlated with the desire for unions or the other voice institutions, weighing against this interpretation.

Figure 5: Greater WTP for each job attribute among participants whose job has that attribute



Note: Above are OLS-based estimates and 95% confidence intervals, clustered by participant. For each job attribute, I estimate a difference in average WTP, between respondents whose job (current or most recent) has that attribute, and respondents whose job does not. Results indicate that having a job with a given attribute is strongly correlated with demonstrating WTP for that attribute, for all attributes save Proactive HR.

3.3 Correlations with current job attributes

It is reasonable to ask whether my experimental estimates have any connection to actual labor market outcomes. As shown in Figure 5, the answer is yes: for each job attribute that I study, WTP for that attribute is higher among those whose jobs have that attribute, than among those whose job does not have that attribute, to a degree that is statistically and practically significant (except for “Proactive HR”). In other words, there are strong correlations between the job preferences that respondents express in my survey and the jobs that they actually have.

The relatively small and insignificant correlation for “Proactive HR” may be explained by the fact that my survey asked only whether someone’s current or most job had an HR or personnel department, not whether that department was “proactive” in soliciting feedback from employees, and so for this attribute, the WTP estimate and the current job attribute that I asked about do not correspond closely to each other.

These correlations cannot be considered directly causal estimates; for one thing, we do not know the direction of causation. For example, we see that those in unionized jobs tend to have substantially higher WTP for unionized jobs. But we do not know to what extent respondents have a pre-existing belief that unions yield utility, which is causing them to take unionized jobs; or to what extent having a unionized job is causing respondents to believe (i.e. informing them) that unions yield utility.

Still, regardless of the direction of causation, if exit from un-desirable jobs is possible for enough workers, then it is reasonable to suppose that the more a person prefers a job attribute, the more likely they are to have a job with that attribute, all else equal. Under this supposition, the strong correlation that we observe, between the job attributes that survey respondents say they prefer and the job attributes that they actually have, provides evidence that respondents are responding to my hypothetical job offers in a way which often reflects their actual preferences.

4 Discussion

Some caution is warranted when extrapolating from the hypothetical to the real, and from the sample to a population. However, participants' responses to my hypothetical job offers do pass attention checks at relatively high rate rates, and when I restrict the sample to respondents who face and pass these checks, the estimates stay reasonably similar Section 5.2. Results are also similar from the hierarchical Bayesian model, which explicitly compensates for inattention. Re-weighting the sample to more closely resemble the Massachusetts population indeed changes my estimates, but still largely preserves the same direction, the same magnitude to within several percent, the same pattern of statistical significance, and the same relative ordering of WTP estimates — although WTP for unions does increase. Finally, for most job attributes, I observe a strong correlation between my estimate of the worker's WTP for each attribute, whether that worker's worker's job actually has that attribute. In all, there is good reason to believe that responses to my survey tend to be informative about actual preferences in the wider population.

Workers in my sample tend to value institutions which give them a voice, and this is particularly true of workers in demographic groups which have historically lacked a voice. On average, workers are willing to sacrifice a substantial fraction of their wage to have each of the worker voice institutions studied: 3% for unions, 4% for proactive HR that actively solicits feedback from employees, and 5% for ERGs in which workers collaborate with management to solve workplace problem. Among women, among people of color, and among under-40s, WTP for unions and for proactive HR are more than twice the WTP of men, whites, and over-40s, respectively — although for all of these sub-populations, WTP for all voice institutions is positive, to a statistically and practically significant degree.

While workers' average WTP for HR and ERGs may be greater than WTP for unions, these employer-controlled institutions do not substitute for unions: workers still tend to want unions, even if they already have a proactive HR or an ERG. Indeed, my Bayesian hierarchical model indicates that individuals who value HR and ERGs also tend to value unions. Taken together, these estimates suggest that merely instituting HR or ERGs would not be a winning strategy for employers seeking to oppose or prevent unionization.

Workers value voice institutions no less (but also no more) in the context of digital surveillance, which workers dislike intensely, considering the presence of surveillance to be roughly equivalent to a 16% pay cut. However, individuals who value worker voice institutions also tend to dislike digital surveillance, and this correlation is substantial. Thus, there appears to be a strong conflict between the two ways an employer may gain information about or from his employees. If the employer subjects his workers to surveillance in order to gain information about their behavior, then not only does he impose a utility penalty on his workers, risking antagonizing them, but this penalty is greater specifically among those workers who are more desirous of worker voice institutions, which provide another avenue of information-sharing. Such a sense of antagonism may be compounded if workers view information-sharing as subject to norms such as gift-exchange, and view surveillance as a violation of such norms (Bowles and Polanía-Reyes 2012; Akerlof 1986). Surveillance may therefore be a counterproductive way to try to ensure the efficient flow of information within the firm.

Respondents' desire for remote work does not appear to be explained by a desire for autonomy *per se*, to the extent that the presence of worker voice institutions or freedom from digital surveillance can be considered forms or indicators of autonomy. Digital surveillance does decrease respondents' WTP for remote work by about two percentage points, but this still leaves the value of remote work at 11% of wages when the worker is under extensive digital surveillance, supporting the conclusion that the desire for remote work is partly, but not primarily, explained by the desire for freedom from monitoring by the employer. If workers' desire for remote work were largely motivated by a more general desire for autonomy, one might expect WTP for remote work to be strongly correlated with WTP for a union or other voice institutions, but I find no such correlation. It may simply be that workers value remote work because it gives them the ability to, during normal breaks in work, do housework, care work, small errands, or simply enjoy the comforts of home.

Even if my findings hold only approximately in the U.S. population, they have several important implications. A policymaker who aims to advance the interests of working people would do well to ensure that workers can have a voice in their own workplaces, including through a union. Such a voice is particularly desired by women, people of color, and younger people, but also by men, whites, and older people. Allowing workers to have a voice only through employer-controlled institutions is unlikely to assuage workers' desires for a union. Workers on average, and women in particular, would be likely to see considerable benefit from policies which enable remote work, and which safeguard workers from surveillance by their employers.

References

"6 in 10 Employers Require Monitoring Software for Remote Workers." 2021. https://digital.com/6-in-10-employers-require-monitoring-software-for-remote-workers/.

Abruzzo, Jennifer. 2022. "Electronic Monitoring and Algorithmic Management of Employees Interfering with the Exercise of Section 7 Rights," October.

Adhvaryu, Achyuta, Teresa Molina, and Anant Nyshadham. 2019. "Expectations, Wage Hikes, and Worker Voice: Evidence from a Field Experiment." Cambridge, MA.

Akerlof, George A. 1986. "Labor Contracts as Partial Gift Exchange." In, 66–92. Cambridge University Press. https://doi.org/10.1017/cbo9780511559594.006.

Barrows, Thomas S. 2017. *What Do Unions Do?: A Twenty-Year Perspective*. Routledge.

Bianchi, Suzanne M., Liana C. Sayer, Melissa A. Milkie, and John P. Robinson. 2012. "Housework: Who Did, Does or Will Do It, and How Much Does It Matter?" *Social Forces* 91 (1): 55–63. https://doi.org/10.1093/sf/sos120.

Blau, Francine, and Lawrence Kahn. 2016. "The Gender Wage Gap: Extent, Trends, and Explanations." https://doi.org/10.3386/w21913.

Board, National Labor Relations. 23AD. "Employee Rights." *National Labor Relations Board*. https://www.nlrb.gov/about-nlrb/rights-we-protect/your-rights/employee-rights.

Borges-Méndez, Ramón F. 1995. "Industrial Change, Immigration, and Community Development:

An Overview of Europeans and Latinos." *New England Journal of Public Policy*, Latinos in a Changing Society, 11 (1). https://scholarworks.umb.edu/nejpp/vol11/iss1/6.

Bowles, Samuel, and Herbert Gintis. 2008. "Power." McMillan.

Bowles, Samuel, and Sandra Polanía-Reyes. 2012. "Economic Incentives and Social Preferences: Substitutes or Complements?" *Journal of Economic Literature* 50 (2): 368–425. https://doi.org/10.1257/jel.50.2.368.

Bronfenbrenner, Kate L. 2022. "In Solidarity: Removing Barriers to Organizing," September.

Buerkner, Paul. 2017. "Brms: An r Package for Bayesian Multilevel Models Using Stan." *Journal of Statistical Software* 80 (1). https://doi.org/https://doi.org/10.18637/jss.v080.i01.

Bureau of Labor Statistics. 2023. "Telework, Hiring, and Vacancies - 2022 Data from the Business Response Survey." News {{Release}}: Data from the {{Business Response Survey}} USDL-23-0539. Bureau of Labor Statistics.

Defreitas, Gregory. 1993. "Unionization Among Racial and Ethnic Minorities." *ILR Review* 46 (2): 284–301. https://doi.org/10.1177/001979399304600205.

Dube, Arindrajit, Suresh Naidu, and Adam Reich. 2022. "Power and Dignity in the Low-Wage Labor Market: Theory and Evidence from Wal-Mart Workers." https://www.ssrn.com/abstract=4216227.

Eurofound. 2020. "Working Conditions - Employee Monitoring and Surveillance: The Challenges of Digitalisation." Publications Office of the European Union, Luxembourg.

"ExpressVPN Survey Shows Widespread Surveillance on Remote Workers." 2021. https://www.expressvpn.com/blog/expressvpn-survey-surveillance-on-the-remote-workforce/.

Fisk, Catherine L. 2020. "Sustainable Alt-Labor." *Chicago-Kent Law Review* 95 (1). https://scholarship.kentlaw.iit.edu/cgi/viewcontent.cgi?article=4305&context=cklawreview.

Flood, Sara, Miriam King, Renae Rodgers, Steven Ruggles, J. Robert Warren, Daniel Backman, Annie Chen, et al. 2023. "IPUMS CPS: Version 11.0 [Dataset]. Minneapolis, MN: IPUMS, 2023." Minneapolis, MN.

Freeman, Richard. 2007. "Do Workers Still Want Unions? More Than Ever." *EPI Briefing Paper*, February.

Freeman, Richard, and James Medoff. 1979. "The Two Faces of Unionism." Cambridge, MA. https://doi.org/10.3386/w0364.

———. 1984. *What Do Unions Do?* New York: Basic Books.

Freeman, Richard, and Joel Rogers. 2006. *What Workers Want*. Updated ed. Ithaca : New York: ILR Press ; Russell Sage Foungation.

Gitelman, H. M. 1973. "Perspectives on American Industrial Violence." *Business History Review* 47 (1): 1–23. https://doi.org/10.2307/3113601.

Goldin, Claudia, and Lawrence F. Katz. 2011. "The Cost of Workplace Flexibility for High-Powered Professionals." *The ANNALS of the American Academy of Political and Social Science* 638 (1): 45–67. https://doi.org/10.1177/0002716211414398.

Henrich, Joseph, Steven J Heine, and Ara Norenzayan. 2010. "The Weirdest People in the World." *Behavioral and Brain Sciences* 33 (2-3): 61–83. https://doi.org/https://doi.org/10.1017/S0140525X0999152X.

Hertel-Fernandez, Alexander, William Kimball, and Thomas Kochan. 2022. "What Forms of Representation Do American Workers Want? Implications for Theory, Policy, and Practice." *ILR Review* 75 (2): 267–94. https://doi.org/10.1177/0019793920959049.

Hirschman, Albert O. 1970. *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States*. Cambridge, MA: Harvard University Press. https://hdl-handle-net.silk.library.uma ss.edu/2027/heb04043.0001.001.

Khubchandani, Jagdish, and James H. Price. 2015. "Workplace Harassment and Morbidity Among US Adults: Results from the National Health Interview Survey." *Journal of Community Health* 40 (3): 555–63. https://doi.org/10.1007/s10900-014-9971-2.

Kochan, and Kimball. 2019. "Unions, Worker Voice, and Management Practices: Implications for a High-Productivity, High-Wage Economy." *RSF: The Russell Sage Foundation Journal of the Social Sciences* 5 (5): 88. https://doi.org/10.7758/rsf.2019.5.5.05.

Lavetti, Kurt. 2023. "Compensating Wage Differentials in Labor Markets: Empirical Challenges and Applications." *Journal of Economic Perspectives* 37 (3): 189–212. https://doi.org/10.1257/jep.37.3.189.

Lewbel, Arthur. 2014. "An Overview of the Special Regressor Method." In, edited by Jeffrey S. Racine, Liangjun Su, and Aman Ullah. Oxford University Press. https://doi.org/10.1093/oxfordhb/9780199857944.013.002.

Library, UVA. 2010. "A Guide to the Edward L. Stone/Borderland Coal Company Papers." University of Virginia Library.

Lipold, Paul F., and Larry W. Isaac. 2022. "Repressing Worker Dissent: Lethal Violence Against Strikers in the Early American Labor Movement." *Labor History* 63 (1): 1–23. https://doi.org/10.1080/0023656X.2022.2053080.

Maestas, Nicole, Kathleen J. Mullen, David Powell, Till Von Wachter, and Jeffrey B. Wenger. 2023. "The Value of Working Conditions in the United States and Implications for the Structure of Wages." *American Economic Review* 113 (7): 2007–47. https://doi.org/10.1257/aer.20190846.

Maestas, Nicole, Kathleen J Mullen, and David Powell. 2018. "The Value of Working Conditions in the United States and Implications for the Structure of Wages." http://www.nber.org/papers/w25204.

Mas, Alexandre, and Amanda Pallais. 2017. "Valuing Alternative Work Arrangements." *American Economic Review* 107 (12): 3722–59. https://doi.org/10.1257/aer.20161500.

———. 2019. "Labor Supply and the Value of Non-Work Time: Experimental Estimates from the Field." *American Economic Review: Insights* 1 (1): 111–26. https://doi.org/10.1257/aeri.20180070.

Mast, Nina, and Lisa Kresge. 2022. "How Common Is Employers' Use of Workplace Management Technologies? A Review of Prevalence Studies."

Miller, Earl J. 1922. "Workmen's Representation in Industrial Government." PhD thesis. https://archive.org/details/workmensrepresenOOmill.

NLRB. n.d. "Interfering with or Dominating a Union (Section 8(a)(2))." https://www.nlrb.gov/about-nlrb/rights-we-protect/the-law/interfering-with-or-dominating-a-union-section-8a2.

O'Sullivan, Mary. 2001. *Contests for Corporate Control: Corporate Governance and Economic Performance in the United States and Germany*. Oxford: Oxford University Press.

Patmore, Greg. 2016. "The US." In. Liverpool, U.K.: Liverpool University Press. https://www.jstor.org/stable/j.ctt1gn6brb.10.

Projections, Inc. 2022. "Giving Employees a Voice." https://web.archive.org/web/20221202114950/https://projectionsinc.com/unionproof/giving-employees-a-voice/.

Ranganathan, Aruna, and Aayan Das. 2022a. "Remote Work Conference." In. Stanford University.

———. 2022b. "Speak Up or Stay Silent: Impact of Remote Work on Worker Mobilization." Slideshow. Stanford University.

Rosenfeld, Jake, and Meredith Kleykamp. 2012. "Organized Labor and Racial Wage Inequality in the United States." *American Journal of Sociology* 117 (5): 1460–1502. https://doi.org/10.108

6/663673.
Simon, Herbert A. 1991. "Organizations and Markets." *The Journal of Economic Perspectives* 5 (2): 25–44. http://www.jstor.org/stable/1942684.
Stan. 2022. Stan Modeling Language Reference Manual. Stan Development Team. https://mc-stan.org.
"Starbucks Corporation LLC." 2023, February.
Taft, Philip. 1966. "Violence in American Labor Disputes." *The ANNALS of the American Academy of Political and Social Science* 364 (1): 127–40. https://doi.org/10.1177/000271626636400113.
Thelen, Kathleen. 1991. *Union of Parts: Labor Politics in Postwar Germany*. Cornell University Press.
U. S. Bureau of Labor Statistics. 2022. "Union Members in Massachusetts." https://www.bls.gov/regions/northeast/news-release/unionmembership_massachusetts.htm.
———. 2023. "Union Members - 2022." https://www.bls.gov/news.release/pdf/union2.pdf.
UAW. 2015. "Dues FAQ." https://uaw.org/dues-faq/.
U.S. Bureau of Labor Statistics. 2023. "BLS Telework Data Table."
White, Halbert. 1980. "Using Least Squares to Approximate Unknown Regression Functions." *International Economic Review* 21 (1): 149. https://doi.org/10.2307/2526245.
Wiswall, Matthew, and Basit Zafar. 2018. "Preference for the Workplace, Investment in Human Capital, and Gender*." *The Quarterly Journal of Economics* 133 (1): 457–507. https://doi.org/10.1093/qje/qjx035.
Wolman, Leo. 1924. "Chapter II. Changes in Union Membership, 1880-1923." In, 29–66. NBER. https://www.nber.org/books-and-chapters/growth-american-trade-unions-1880-1923/changes-union-membership-1880-1923.

5 Appendices

5.1 Appendix: Intermediate results

In Table 5 I display the parameter estimates and standard errors from linear probability models as described in Equation 5 and Equation 6. The outcome-variable is the indicator of choosing Job A, and the regressors are the differences in job attributes $x^A - x^B$. The coefficients therefore describe the effect of each attribute on the probability of choosing a job.

Table 5: Binary OLS Models of Choices

	Baseline	Union Interactions	Voice Interactions
(Intercept)	0.500	0.500	0.500
	(0.003)	(0.003)	(0.003)
Log-Wage	1.195	1.194	1.195
	(0.027)	(0.027)	(0.027)
Union	0.031	0.028	0.028
	(0.005)	(0.010)	(0.010)
Proactive HR	0.050	0.051	0.048
	(0.004)	(0.006)	(0.008)
ERG	0.060	0.060	0.067
	(0.004)	(0.006)	(0.009)
Surveillance	−0.195	−0.199	−0.195
	(0.005)	(0.007)	(0.010)
Remote	0.151	0.152	0.163
	(0.005)	(0.007)	(0.008)
Union × Proactive HR		−0.001	−0.001
		(0.008)	(0.008)
Union × ERG		0.000	0.000
		(0.009)	(0.009)
Union × Remote		−0.002	−0.002
		(0.009)	(0.009)
Union × Surveillance		0.008	0.008
		(0.008)	(0.008)
Proactive HR × Surveillance			0.017
			(0.008)
ERG × Surveillance			−0.003
			(0.008)
Remote × Surveillance			−0.022
			(0.008)
Proactive HR × ERG			−0.012
			(0.008)

Note: OLS estimates and standard errors, clustered by individual. Outcome: 1(Chose Job A). Regressors: Differences in attributes, Job A - Job B. Coefficients therefore quantify the effect of each job attribute on the mean chance of a choosing a job with that attribute. N = 22,222 choices from 2,614 participants.

Judging by the OLS coefficients in Table 5, all job characteristics are of some importance to participants, to a degree that is both statistically and practically significant. Several interaction effects are both statistically and practically significant. These estimates pass two basic validity checks.

By design, wages are independent of the other attributes of the hypothetical jobs, so higher wages must (if money yields utility) make these hypothetical jobs more desirable on average. Respondents indeed respond positively and strongly to higher wages on average, indicating that they tend to read the hypotheticals and respond in a way which reflects their preferences, at least concerning wages. Second, the intercept is essentially 0.5 in all specifications, indicating that respondents have no average preference for the arbitrary label "Job A" over the arbitrary label "Job B".

5.2 Appendix: Attention checks

My survey was constructed such that in 1382 randomly-chosen scenarios that participants faced, both job descriptions were identical save for their wages, which generally differed. A basic test of rationality and attention asks whether in such a scenario, the participant chose the higher-wage job. I found that participants passed this test of rationality and attention 81% of the time. This is substantially higher than the approximately 65% attention estimated (albeit using a slightly different test) in a recent high-quality study (Maestas et al. 2023, 2028).

My Bayesian model also accounts for some degree of irrationality or inattention. A basic assumption of this model is that participants are more likely to respond inattentively or irrationally when faced with smaller differences in utility between jobs. We can test this assumption here by simply regressing the indicator of whether the choice was rational on the absolute difference in log-wages in otherwise identical jobs. I find that this difference is statistically and practically significant, indicating that respondents do respond more rationally when the incentives to do so are greater. This suggests that apparent irrationality in my dataset is often likely to be "rational inattention" of the sort captured by my Bayesian model.

Table 6: Tendency to choose rationally as a function of the wage gap

	(1)
Wage gap	0.317**
	(0.121)
Intercept	0.775***
	(0.018)

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note:
I regress the indicator of whether the agent chose the higher-wage job, on the wage gap between the two jobs, which were otherwise identical.

Restricting my analysis to only the 844 participants who faced and passed the attention check produces results fairly similar to results from the full sample in terms of sign and magnitude, albeit with larger standard errors owing to the severely reduced sample size of 7,479 choices (Table 7).

Table 7: Comparing full sample OLS estimates to attentive subsample OLS estimates

	Full Sample	Attentive Subsample
	(1)	(2)
(Intercept)	0.500***	0.501***
	(0.003)	(0.005)
Log-Wage	1.195***	1.488***
	(0.027)	(0.042)
Union	0.028**	0.021
	(0.010)	(0.017)
Proactive HR	0.048***	0.060***
	(0.008)	(0.016)
ERG	0.067***	0.053***
	(0.009)	(0.015)
Remote	0.163***	0.163***
	(0.008)	(0.013)
Surveillance	−0.195***	−0.199***
	(0.010)	(0.016)
Union × Proactive HR	−0.001	−0.007
	(0.008)	(0.014)
Union × ERG	0.000	0.022
	(0.009)	(0.015)
Union × Remote	−0.002	−0.002
	(0.009)	(0.015)
Union × Surveillance	0.008	0.009
	(0.008)	(0.014)
Proactive HR × Surveillance	0.017*	0.013
	(0.008)	(0.016)
ERG × Surveillance	−0.003	0.004
	(0.008)	(0.015)
Remote × Surveillance	−0.022**	−0.009
	(0.008)	(0.015)
Proactive HR × ERG	−0.012	−0.021
	(0.008)	(0.016)

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Here I compare a regression on the full sample to a subsample containing only participants who faced and passed the attention check. Results are reasonably similar.

5.3 Appendix: Adjusting for Response Patterns

Here I present results in which observations have been re-weighted using three variables: race (white or non-white), gender (male or female), and age (0-40, 40-100), all interacted to form 8 strata. Weights were computed by taking the ratio between the sample proportion and the MassHire population proportion in each stratum.

Table 8: Re-weighted estimates

	Unweighted	Weighted
(Intercept)	0.500	0.501
	(0.003)	(0.004)
Log-Wage	1.195	1.268
	(0.027)	(0.036)
Union	0.028	0.045
	(0.010)	(0.013)
Proactive HR	0.048	0.047
	(0.008)	(0.011)
ERG	0.067	0.070
	(0.009)	(0.012)
Remote	0.163	0.151
	(0.008)	(0.011)
Surveillance	−0.195	−0.166
	(0.010)	(0.014)
Union × Proactive HR	−0.001	0.005
	(0.008)	(0.011)
Union × ERG	0.000	0.008
	(0.009)	(0.011)
Union × Remote	−0.002	−0.001
	(0.009)	(0.011)
Union × Surveillance	0.008	0.005
	(0.008)	(0.011)
Proactive HR × Surveillance	0.017	0.023
	(0.008)	(0.011)
ERG × Surveillance	−0.003	−0.002
	(0.008)	(0.012)
Remote × Surveillance	−0.022	−0.029
	(0.008)	(0.012)
Proactive HR × ERG	−0.012	−0.013
	(0.008)	(0.011)

Note:
The re-weighted estimates are similar enough to the unweighted estimates that our original conclusions largely hold. Coefficients for each of the voice characteristics remain positive, significant, and similar orders of magnitude, except that the Union coefficient increases to be nearer the HR coefficient. The surveillance coefficient increases towards zero slightly, but remains very significantly negative.

5.4 Appendix: Bayesian Modeling

5.4.1 Computational Details

The posterior distribution of our Bayesian model, like most posterior distributions in applied settings, cannot be integrated analytically. So I code my model in the R package `brms` (Buerkner 2017), a high-level interface for Stan *Stan* (2022), a state-of-the-art Markov Chain Monte Carlo (MCMC) sampler that uses a Hamiltonian Monte Carlo algorithm to draw samples from an arbitrarily close approximation of the posterior distribution of our model's parameters given the data. With these samples from the posterior distribution, one can compute parameter estimates by taking the mean, and confidence intervals by computing quantiles.

5.4.1.1 Example R code

This code executes in approximately 15 minutes on a 2021 Macbook Pro with 10 cores and 32 GB memory.

```
library(brms)

baseline_formula <- bf(choice ~ 0 + lwage_diff + union_diff + hr_diff + erg_diff + locat_diff + moni
    (0 + union_diff + hr_diff + erg_diff + locat_diff + monit_diff |analysis_id)
    )

brm_baseline <- brm(
  baseline_formula,
  family = bernoulli("probit"),
  data = hyp_dat,
  iter = 4000,
  init = 0,
  chains = 4,
  cores = 4,
  threads = threading(2),
  backend = "cmdstanr",
  control = list(adapt_delta = 0.8)
)
```

5.4.1.2 Sampler Diagnostics

In MCMC, it is essential to verify (or at least fail to disprove) that the Markov chain has indeed converged sufficiently closely to posterior distribution. All Bayesian estimates presented in this paper come from MCMC runs which pass all standard diagnostics, including no divergences, large effective sample size, and R-hat at unity.

5.5 Differences in logwages are wage shares (even when large or high-variance)

It is well-known that small differences in logs $\Delta = \log(w_B) - \log(w_A)$ can be interpreted as percent differences, using a Taylor approximation of the log function near one. It is also well-known that one can convert from a difference in logs to an exact percent scale using either of two formulas, depending on which term the researcher (often arbitrarily) chooses for the denominator of the percent:

$$\tilde{\Delta}^A = \exp(\Delta) - 1$$

$$\tilde{\Delta}^B = 1 - \exp(-\Delta)$$

Since both transformations of Δ diverge from the identity as Δ gets farther from zero, with error magnitudes larger than 0.01 when Δ has magnitude larger than 0.15 or so, it would seem that for such Δ, differences in logs can no longer be interpreted as percent shares for many economic purposes. Furthermore, if Δ varies considerably in a population, then Jensen's inequality will certainly drive the means of $\tilde{\Delta}^A$ and $\tilde{\Delta}^B$ away from that of Δ, in opposite directions, and to a substantial degree, seeming to forbid interpreting an average Δ as an average wage share.

However, it is perhaps less well-known that if we consider the average of the two percent scales,

$$\tilde{\Delta} = \frac{1}{2}(\exp(\Delta) - \exp(-\Delta)) = \sinh(\Delta)$$

it so happens that the resulting hyperbolic sine transformation is very nearly the identity in a rather wider range around zero, with an absolute error less than 0.01 for inputs with magnitude less than 0.4, and absolute error less than 0.001 for inputs less than 0.2.

In our application, there is no particular reason to pick either percent scale (the labels Job A and Job B are interchangeable. The large majority of the population distribution of logwage WTP is less than about 0.3 (and all estimated population means are much smaller than this), so it is therefore legitimate to interpret a difference in log-wages directly as a wage share, using the average of the two possible wage share scales.