

HOW DOES WAGE INEQUALITY AFFECT THE LABOR MOVEMENT?*

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Abstract

This paper provides causal evidence on how wage inequality among workers affects the labor movement using three complementary research designs: a vignette experiment with union organizers, a natural policy experiment that increased wage inequality among Wisconsin school teachers, and an information intervention during the 2023 Writers Guild of America strike. Across all studies, we find that inequality undermines union strength through multiple channels. First, workers with high individual bargaining power are more likely to withdraw support in unequal environments, preferring individual over collective bargaining. Second, union organizers facing hypothetical choices strategically respond to inequality in ways that may preserve membership but limit redistribution, such as shifting their campaign away from wages and choosing smaller, more homogeneous bargaining units. Taken together, our findings highlight the potential for “inequality traps,” where rising inequality erodes the very institutions designed to counteract it.

JEL Classification: J31, J5, D83, D84, C93.

Keywords: inequality, collective bargaining, pay transparency.

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1 Introduction

The negative correlation between inequality and unionization rates in the U.S. has been extensively documented (Freeman, 1980; DiNardo et al., 1996; Card, 2001). Farber et al. (2021) further provide causal evidence that declining unionization rates since the 1960s have contributed to the rise in income inequality. The other direction of causality—namely, the impact of inequality on the labor movement—has also been theorized (Hirsch, 1982; Acemoglu et al., 2001), but never tested.

Inequality within industries and occupations has risen substantially in the U.S. over recent decades. On the one hand, rising inequality among workers who could share the same union representation may increase union support by galvanizing lower-income workers who stand to benefit from redistribution, or by mobilizing workers concerned with fairness and income security, regardless of their own earnings. On the other hand, rising inequality may challenge unions’ ability to attract, retain, and mobilize members. Indeed, workers with high individual bargaining power may prefer to negotiate individually (Acemoglu et al., 2001) and, even when most workers favor collective representation, pay dispersion can hinder their ability to coalesce around common bargaining priorities.¹ High inequality may also change unions’ objectives. For instance if high earners are less responsive to traditional union messaging on wage compression, labor organizers may strategically shift their focus toward non-wage amenities.

This paper provides causal evidence on how rising wage inequality among workers affects their support for collective bargaining and changes union organizers’ strategies, such as where they campaign, whom they target, and what they demand. An ideal research design would feature exogenous variation in inequality alongside documentation of the labor movement’s response across the economy. While inequality itself is seldom assigned randomly, we identify three research settings that achieve this tall order.

Our first setting is a survey experiment with labor organizers. In a vignette design, organizers evaluate hypothetical firms with identical internal wage structures, but operating in labor markets that differ in wage inequality. Similar to the technological change in Acemoglu et al. (2001), this difference is generated by increasing some workers’ market wages and decreasing others’, while keeping the average market wage constant across vignettes. Organizers are asked to make incentivized strategic decisions (e.g., how to spend organizing budgets) based on their predictions about union support. Through multiple outreach channels,

¹Studying the formation of school districts in the U.S., Alesina et al. (2004) highlight how institutions such as local governments may break down due to the cost of aggregating individuals with heterogeneous preferences. In a similar vein, Goldin and Katz (1999) ascribe the birth of universal secondary education in the U.S. in the state of Iowa, at least in part, to homogeneity in income and wealth within local communities.

we collect responses from nearly 200 organizers representing 26 unions, 15 industries, and 39 U.S. states. Under incentive-compatible choices, organizers direct campaign resources toward environments where workers have more equal labor market opportunities, consistent with their expectation of lower union support in environments where workers have unequal labor market opportunities. Should they pursue a union drive in an unequal environment, they would use strategies to mitigate the impact of inequality on union support. For instance, they are 23.6 percentage points (pp) (p -value < 0.001), or 25%, less likely to disclose market wage information in environments where workers face unequal market wages. Inequality also shapes campaign focus: in unequal environments, organizers are more likely to build campaigns around non-wage amenities at the expense of wage demands. Finally, in more unequal environments, organizers are 14.8pp (168%, p -value < 0.001) more likely to target smaller bargaining units that separate workers with different market wages rather than a single firm-level union. The latter two patterns highlight a trade-off in high-inequality environments: preserving cohesion through smaller units of similar workers and focusing on less divisive demands may increase the chances of union representation, but it may also reduce the union's bargaining strength and ability to push for wage compression.

We find support for our experimental results in administrative data. Using private-sector contracts from the Office of Labor-Management Standards (OLMS) from 2002–2022, as well as Federal Mediation and Conciliation Service bargaining unit data from 2015–2021, we find that unions operating in more unequal industries are less likely to negotiate rigid pay scales, more likely to emphasize non-wage amenities over wage demands in contracts, and represent smaller bargaining units as a share of establishment employment. An analysis of AFL-CIO News content from 1955–1996 reveals that, as income inequality rose, unions' focus on pay-related issues declined from over half of all articles in the 1970s to one-third in the 1990s. While these correlational patterns do not establish causality, they are consistent with our experimental evidence on the numerous ways organizing approaches can respond to inequality, with the potential to exacerbate it.

While our first setting captures organizers' perspectives, our second provides direct evidence on how inequality affects workers' union support. We study the education sector, which has the largest population of U.S. unionized workers. A Wisconsin reform, staggered across ~400 districts, increased wage inequality among public school teachers. We combine this natural experiment with administrative data on union revenue per teacher and individual-level dues payments to study how rising inequality shapes union support.

In 2011, Wisconsin's Act 10 prohibited collective bargaining over pay scales, which had been used to set teachers' pay based on experience and education. This left districts free to adjust teacher pay

individually without union consent, leading to a sharp rise in wage inequality among Wisconsin teachers. Key to our design is that individual bargaining increased pay dispersion only in commuting zones (CZs) with sufficient competition for teachers, creating natural treatment and control regions that differed only in whether they experienced a rise in inequality. Using the Herfindahl-Hirshman Index (HHI) to measure competition among public schools, we confirm that pay dispersion among teachers with the same education and experience (“position”) only grew in below-median-HHI (competitive) areas: between 2010 and 2016, the within-position standard deviation in pay rose by 64.6% in low-HHI districts, while remaining essentially flat in high-HHI districts. Because average wage remained fixed by pre-determined budgets across high and low-HHI districts, and those districts shared similar teacher characteristics (e.g. gender and experience), district characteristics (e.g. rural vs. urban), as well as wage levels, dispersion and growth pre-reform, we can use this setting to isolate the effect of inequality on union support.

We find that, while districts present parallel trends in union revenues per teacher pre-reform, districts exposed to the inequality shock (below-median HHI) experienced large and significant declines in union contributions after the reform took effect, while unexposed districts (above-median HHI) experienced a much smaller decline. Three years later, the gap in union revenues between the two groups reached 58% (p-value = 0.005). We also examine individual choices to pay union dues after the reform and show that the relative drop in union support in districts with rising inequality is driven by teachers whose wages grew the most under flexible pay. This heterogeneity corroborates the economic channel conjectured by [Acemoglu et al. \(2001\)](#), whereby, if inequality is sufficiently large, high earners prefer to bargain individually.

Our third setting is an experiment with Hollywood screenwriters during the 2023 Writers Guild of America (WGA) strike—providing additional evidence on how inequality affects workers’ union support. Leveraging the insight that people typically underestimate pay inequality ([Hauser and Norton, 2017](#); [Cullen and Perez-Truglia, 2022](#); [Stantcheva, 2024](#); [Jäger et al., 2024](#)), we experimentally vary writers’ exposure to information about pay inequality and test whether exposure affects expressions of union support under real stakes. In a baseline survey of 400 WGA members, we establish that writers’ perceived inequality is substantially more modest than actual levels, setting the stage for our experimental intervention.

Following our theoretical framework, premised on [Acemoglu et al. \(2001\)](#), we aim to study a shock to workers’ perceptions of the distance between the pay they could negotiate as an individual, and that of the median voter in the union. Therefore, we distribute a pay report that highlights pay disparities between individual pay and collectively negotiated minimums among Hollywood writers during their 2023 strike.

We distribute it to WGA members 100 days into the strike, a critical juncture when Networks resumed negotiations with the Guild. To measure union support, we ask respondents: “Do most writers think WGA demands meet the needs of all members?” By randomizing whether this question appears before or after the pay report, we can discern whether pay disclosure, as an instrument shifting pay beliefs, impacts writers’ responses. We find that, absent the pay report, only 9% of respondents report a lack of solidarity among writers about WGA demands, compared to 23% after seeing the pay report (difference p-value < 0.001). This shift is driven by respondents with more writing credits—our proxy for productivity. Among high-credit writers, the share reporting that union demands do not meet all members’ needs increases by 20.5 pp (p-value = 0.001), compared to a modest and statistically insignificant 5.5 percentage point increase among low-credit writers (p-value = 0.414). This heterogeneity mirrors patterns among Wisconsin teachers as well media accounts of the WGA strikes.²

Finally, we bridge the designs by estimating union-support–inequality elasticities across our settings and find remarkably similar magnitudes, in the range of -0.1 to -0.2. Using these elasticities and historical changes in within-industry-by-occupation wage dispersion, we conduct a back-of-the-envelope calculation suggesting that rising inequality can account for 5–7% of the long-run decline in union membership since 1980. This magnitude is remarkably comparable to estimates in the opposite direction: [Farber et al. \(2021\)](#) show that declining unionization can explain 5–10% of the inequality growth over the same period. This bidirectional relationship creates the potential for “inequality traps,” whereby collective bargaining, typically a counterforce against inequality, becomes increasingly difficult as wage gaps among workers widen.

This paper builds on a longstanding literature examining the role of unions in shaping wages. This literature has documented the existence of a union pay premium (in the range of 0.1–0.4 log points) and debated unions’ contribution to wage compression ([Freeman, 1980](#); [Card, 2001](#); [DiNardo and Lee, 2004](#); [Lee and Mas, 2012](#); [Biasi and Sarsons, 2022](#); [Fortin et al., 2021](#); [Frandsen, 2021](#); [Farber et al., 2021](#); [Pencavel, 2022](#); [Baker et al., 2024](#); [Dodini et al., 2024](#); [Lagos, 2024](#); [Jäger et al., 2024](#); [Beauregard et al., 2025](#)). We complement this literature by providing empirical evidence on a reverse causal pathway (increased inequality impedes collective bargaining) as well as the underlying mechanisms for the relationship.³ We show that individuals with high outside options predominantly drive reduced support for

²[Banks \(2015\)](#) notes “some of the most egregious infighting during [WGA’s 2007-2008 strike] came from high-profile writers who felt their needs were not being served.” As the 2023 strike continued, *Variety* reported that “WGA began to face stronger internal pressure from a strain of its most highly paid members” ([Littleton et al., 2023](#)).

³[Hirsch \(1982\)](#) includes an empirical section. It estimates a simultaneous equations model using 1970 industry-level data to study the joint determination of unionism and earnings dispersion. [Checchi et al. \(2010\)](#) and [Töngür and Elveren \(2014\)](#)

unions in high-inequality environments. This is consistent with descriptive evidence that support for unions is inversely correlated with one's position in the intra-firm wage distribution (Farber and Saks, 1980). Additionally, we find that, anticipating lower support, union organizers are reluctant to invest resources in high-inequality environments, further reducing the chances of union representation. This result adds to a largely correlational literature examining the characteristics of unionized workers over time and how worker preferences shape union success (Farber, 1989; Defreitas, 1993; Gerstel and Clawson, 2001). It also speaks to research on how heterogeneity by income or race can undermine organizations, including unions, by reducing collective action (Alesina and La Ferrara, 2000; Ferguson, 2016). One way to interpret these effects is through the lens of labor organizing as a public goods problem, in which workers must overcome free-riding to secure collective gains. Theoretical work shows that inequality may weaken incentives to contribute to public goods (Bardhan et al., 2007), while experimental evidence summarized by Fehr and Charness (2025) shows that inequality shapes cooperation, reciprocity, and support for redistribution.

This paper further contributes to an interdisciplinary literature on the political economy of unions, recently surveyed in Kaplan and Naidu (2025). Much of this literature examines unions' external political influence, but fewer papers zoom into unions' internal organization and strategic decision-making, how they shape unionization and bargaining outcomes, and how they are shaped by the economic environment.⁴ The economics literature has mainly examined one organizing strategy: strikes (see Card (1991) for a review and Massenkoff and Wilmers (2024) for recent causal evidence of strikes on wages). But many other strategies remain underexplored.⁵ We leverage a unique opportunity to capture organizers' voices at scale to elicit how inequality shifts a broad range of organizing strategies, such as bargaining unit determination and what information to circulate among workers. Consistent with Kremer and Olken (2009), our evidence suggests that unions adapt to more unequal environments in ways that may not directly serve individuals (e.g., withholding information about market wages) but could still help ensure the sustainability of the labor movement.

Our paper also complements evidence on the role of employers in undermining unions. Theoretical work emphasizes how employers strategically leverage the negative impact of inequality on union: Taschereau-Dumouchel (2020) presents a formal model of employer anti-union strategies in which firms

also offer empirical evidence across several OECD countries and over time that is consistent with the reverse relationship. While informative, these analyses do not rely on exogenous variation in inequality to tease apart the direction of causality.

⁴External evidence focuses on voting (Feigenbaum et al., 2018; Kuziemko et al., 2023; Yan, 2024), campaign finance (Matzat and Schmeißer, 2023), lobbying (Johnson, 2020; Dodini et al., 2024), and intra-party bargaining (Gethin et al., 2022). One exception focusing on internal organization is Boudreau et al. (2025), which looks at how union leaders influence and mobilize workers.

⁵Bronfenbrenner and Juravich (1995) and Bronfenbrenner and Hickey (2004) pioneered descriptive research on many organizing strategies, explaining how tactics that encourage rank-and-file participation increase the chances of union drive success.

over-hire high-skill workers, who are more likely to oppose unions, to dilute pro-union sentiment. On the empirical front, [Babcock et al. \(1996\)](#) show that employers and unions strategically select salary benchmarks that favor their own positions in wage negotiations, and that greater cross-district inequality widens the scope for such selective comparisons. [Farber \(2001\)](#) documents another employer strategy against unions: litigating bargaining-unit scope to increase heterogeneity. Echoing this, [Bronfenbrenner \(1994\)](#) finds nearly one-third of employers sought to divide bargaining units by pitting distinct worker groups against one another. This evidence is reflected in open-ended answers we collected from organizers: one organizer chose to organize the equal environment “because the employer could not pit workers in one job against workers in another.” Taken together, this evidence is consistent with employers taking strategic actions that can amplify the negative effects of inequality on union support we document.

Recent work shows that labor market concentration can disproportionately reduce earnings for higher-paid workers, whose outside options are most affected by limited employer competition ([Jarosch et al., 2024](#); [Dodini et al., 2024](#)). Our findings complement this by highlighting an institutional channel through which similar patterns can arise. In particular, when unions negotiate compressed pay scales in environments with heterogeneous outside options, they may alleviate monopsony power for lower-paid workers while constraining higher-paid workers’ ability to capture rents from individual bargaining. As dispersion in outside options rises, support for collective bargaining among workers with greater individual leverage goes down.

Finally, we contribute to a large and growing literature on the labor market consequences of pay transparency (see [Cullen \(2024\)](#) for a review). Our third study leverages differences in perceptions of inequality and a pay information treatment to proxy for what is empirically hard to implement: randomizing inequality. Prior work finds that revealing pay disparities among coworkers can lower morale or work satisfaction ([Breza et al., 2018](#); [Card et al., 2012](#)). Our paper identifies another unintended consequence: pay transparency often increases workers’ perception of wage dispersion, accentuating the tradeoff between individual and collective bargaining, especially for high-productivity workers.

2 Conceptual Framework

2.1 Model

We minimally adapt the model in [Acemoglu et al. \(2001\)](#) to illustrate how inequality between workers weakens the ability of unions to negotiate compressed pay, attract members, and retain them.⁶

Consider workers who could potentially be part of the same union. After workers join a workplace, denoted C for Current, they discover whether they are high- or low-productivity, with probability $\phi < \frac{1}{2}$ of being high-productivity. High-productivity workers produce $y_h^C = \eta$, while low-productivity workers produce $y_l^C = \alpha$, $0 < \alpha < \eta$ in their current employment.⁷ Workers also have an outside option (denoted as O): high-productivity workers can earn $y_h^O = A\eta$, $A > 1$, after paying a cost $\bar{e} > 0$ to move to their outside option,⁸ while low-productivity workers would earn $y_l^O = 0$ if they exit their current employer.

Firms compete by offering wage contracts that take the following linear form, $w^C(y^C) = \gamma + \beta y^C$, ($\beta \geq 0, \gamma \geq 0$), where γ is the fixed component paid to all workers in the firm regardless of their productivity and β governs the degree of pay inequality between high- and low-productivity workers.

A union is defined as a coalition of workers that imposes a wage contract upon the firm, subject to a zero-profit condition. The wage schedule is determined by pure majority voting. Each worker votes to maximize their own rent from the other type, after observing both their own productivity and that of their co-workers.⁹

The timing of actions proceeds as follows: workers realize their productivity after choosing employment at workplace C but before they decide whether to unionize or not. If there is a union, unionized workers vote over the wage policy. The firm decides whether to accept the contract offer or not. If it accepts the offer, it is committed to paying the contracted wage to all workers who stay. Workers can switch to their outside options by paying switching cost \bar{e} . Finally, production and consumption take place.

Absent a union, workers are paid their marginal product: $w_h^C = \eta, w_l^C = \alpha$. High-productivity workers will leave for their outside option if $A\eta - \eta \geq \bar{e}$. An individual's bargaining power rises as this "no-quitting"

⁶While the source of inequality in the original model in [Acemoglu et al. \(2001\)](#) is skill-biased technical change, the model accommodates any exogenous source of inequality, as defined in Section 2.4. One significant deviation from [Acemoglu et al. \(2001\)](#) is that we do not consider upstream decisions to invest in education.

⁷In this set-up, observing productivity and wages is equivalent as the mapping between them is known. A more realistic context could incorporate systematic misperceptions, such as underestimating the difference between self and others.

⁸This switching cost can be interpreted as paying a firm-specific training cost.

⁹In [Acemoglu et al. \(2001\)](#), this is called a rent-seeking union. The union votes on cross-subsidization of members; workers already have full bargaining power and extract all rents from the employer. More flexible worker-firm bargaining is outside the scope of this current model, though one could extend it to include rent-extraction through nested bargaining between workers and firms.

condition increasingly binds, either because an individual's outside option rises, or their switching costs fall.

Assume all firms employ a continuum of workers with mass at least ϵ . The law of large numbers implies that a fraction ϕ of workers at workplace C will be high-productivity before the union vote. Because $\phi < \frac{1}{2}$, majority voting favors low-productivity workers. The median union member, who has low productivity, will use their voting power to extract rents from high-productivity workers. The problem of the median voter is:

$$\max_{\gamma, \beta} \{\gamma + \beta\alpha\}, \text{ s.t.} \tag{1}$$

$$\gamma + \beta\eta \geq A\eta - \bar{e} \quad \text{No quitting of high-productivity workers condition} \tag{2}$$

$$\pi = -\gamma + [1 - \beta]E y^C \geq 0 \quad \text{Firm's non-negative profit condition} \tag{3}$$

The unique equilibrium is characterized by the following: there exists a threshold $\eta^* = \frac{\bar{e}}{A-1}$, such that: For $\eta > \eta^*$, firms are not unionized and offer $w_h^C(\eta) = \eta, w_l^C(\alpha) = \alpha$. For $\eta \leq \eta^*$, firms are unionized. The union imposes a wage contract with $\beta^* = 1 - \frac{\bar{e} - (A-1)\eta}{(1-\phi)(\eta-\alpha)} \leq 1$, and $\gamma^* = (1 - \beta^*)[\phi\eta + (1 - \phi)\alpha]$. For $\eta > \eta^*$, high-productivity workers leave for their outside option; otherwise, no quitting occurs.

We consider what happens when inequality rises in outside options for high-productivity ($A\eta$) versus low-productivity (0) workers. We characterize a more unequal, competitive (or skill-biased), marketplace by A , and a less unequal, competitive (or skill-biased), marketplace by A' , where $A > A'$. Under greater inequality, A' , workers are less likely to reach an agreement on a pay scale, eliminating all cross-subsidization across high- and low-productivity workers. High-productivity workers exit the unionized sector and thus, union membership falls. Even over ranges of A' where workers still agreed upon a pay scale, the pay scale exhibits less wage compression and could thus also degrade support for the union among lower-productivity workers. To see this, note that β^* is increasing in A (less cross-subsidization). When β^* exceeds 1, unions are unsustainable and no pay scale is agreed upon.

In this model, the underlying reason for unequal outside options is not crucial for assessing the impact of inequality, so long as it is outside of the union's control: the framework can incorporate unproductive reasons why one worker might experience stronger outside options, e.g., lower switching costs.¹⁰ However, one could consider model extensions where this reason matters. Indeed, [Breza et al. \(2018\)](#) emphasize that the effects of pay transparency on morale depend on whether workers view revealed pay disparities as justified,

¹⁰To see this, notice that an unproductive additive shock to inequality would be akin to adjusting the relative switching costs \bar{e} , and lower relative switching cost for high types affects their no-quitting condition similarly to higher A .

which suggests that the source of inequality could be important for how pay differences affect union support.

2.2 Organizer’s Choice of Bargaining Unit

Our model thus far abstracts from a prior choice that the union organizer faces: given a skill distribution of workers at the firm, how broadly should the bargaining unit be drawn? We focus on this strategic choice for two reasons. First, in the baseline model, bargaining unit composition shapes the degree of wage compression, and ultimately whether the union is sustainable. Second, bargaining unit determination is central to the organizing literature and may itself respond to inequality. Consistent with this idea, [Dodini et al. \(2025\)](#) document that bargaining units in the United States have become smaller over time and note that increasingly homogeneous bargaining units mitigate the ways inequality discourages unionization.

We extend the model to ask whether an organizer would choose to expand the workforce they are unionizing to include workers in the next higher tier of earnings potential (their outside option in the non-unionized sector). We characterize when the organizer—maximizing the welfare of the median voter in that union—prefers a narrower, more homogeneous unit over a broader one. This question can be asked by the organizer iteratively of each group with higher outside options.

We extend the baseline to workers $j \in \{1,2,3\}$, with inside-firm productivities $\alpha_1 < \alpha_2 < \alpha_3$ and workforce shares $\phi_1 > \phi_2 > \phi_3$, $\sum_j \phi_j = 1$. We assume $\phi_1 > \frac{1}{2}$, so type-1 workers are the strict majority and the median voter under any coalition.¹¹ Outside options retain the structure of the baseline: $y_1^O = 0$, $y_j^O = A_j \alpha_j - \bar{e}_j$ for $j \in \{2,3\}$ with $A_j > 1$. We impose no proportionality restriction on (A_2, A_3) : outside-option competitiveness may differ across skill levels. The baseline model is nested as the special case $\phi_3 = 0$, $\alpha_1 = \alpha$, $\alpha_2 = \eta$.

The organizer first assesses whether to form the union $\{1,2\}$; this is exactly the baseline problem, with α_2 playing the role of η and feasibility threshold $\alpha_2^* \equiv \bar{e}_2 / (A_2 - 1)$. Only if the $\{1,2\}$ union is feasible does the organizer consider expanding to $\{1,2,3\}$.¹² Given a feasible $\{1,2\}$ union, the organizer considers adding type 3. Let $Ey_{123}^C \equiv \phi_1 \alpha_1 + \phi_2 \alpha_2 + \phi_3 \alpha_3$ denote expected output under the expanded union. The problem of the median voter in $\{1,2,3\}$ is:

$$\max_{\gamma, \beta} \{\gamma + \beta \alpha_1\}, \quad \text{s.t.}$$

¹¹This replaces the condition $\phi < \frac{1}{2}$ from the baseline, where ϕ denoted the share of *high*-productivity workers. The two conditions are symmetric: the majority type controls the preferred contract.

¹²The sequential constraint reflects the practical reality that union organizers build coalitions incrementally. An organizer cannot bypass type 2 to target type 3 alone: the contract must cover a contiguous segment of the skill distribution.

$$\gamma + \beta \alpha_j \geq A_j \alpha_j - \bar{e}_j \quad \text{No-quitting condition for } j \in \{2,3\} \quad (4)$$

$$\pi = (1 - \beta) E y_{123}^C - \gamma \geq 0 \quad \text{Non-negative profit condition} \quad (5)$$

As in the baseline, the median voter minimizes β .¹³ In the unique equilibrium of the expanded union, there exists a threshold \bar{A}_3 such that: for $A_3 \leq \bar{A}_3$, the organizer expands the bargaining unit to $\{1,2,3\}$ with equilibrium piece rate β_{123}^* ; for $A_3 > \bar{A}_3$, the organizer retains the narrower $\{1,2\}$ union. The threshold \bar{A}_3 is defined by $W_1^{123} = W_1^{12}$, where $W_1^{12} = E y_{12}^C - \beta_{12}^* (E y_{12}^C - \alpha_1)$; $W_1^{123} = E y_{123}^C - \beta_{123}^* (E y_{123}^C - \alpha_1)$. The organizer expands if and only if the gain in average output exceeds the cost of reduced compression:

$$\underbrace{E y_{123}^C - E y_{12}^C}_{\text{gain: higher average output}} \geq \underbrace{\beta_{123}^* (E y_{123}^C - \alpha_1) - \beta_{12}^* (E y_{12}^C - \alpha_1)}_{\text{cost: reduced wage compression}} \quad (6)$$

As in the baseline, higher dispersion in outside options limits the degree of wage compression feasible within the union. In this extension, expanding the unit to include higher-productivity workers creates a larger base for redistribution, but their stronger outside options make the coalition more difficult to sustain, because retaining them requires less wage compression. When the gain in average output from expanding the bargaining unit no longer offset the cost of reduced wage compression needed to satisfy the no-quitting constraint, organizers aligned with the median voter will prefer a smaller, more homogeneous union. In this case, higher inequality in the outside options of the top skill group can make *expanding* the bargaining unit unattractive (even when technically feasible): the additional surplus from including a high-productivity type can be more than offset by the wage dispersion required to retain them.

We do not model other potential motivations for expanding the unit, such as fostering worker solidarity or increasing bargaining power through unit size, as workers already have full bargaining power in both the baseline and this extension.

2.3 Mapping the Model to Empirical Settings

We now describe the three empirical settings through the lens of this model. In the organizer experiment, respondents compare two workplaces that differ in the dispersion of outside options ($A' > A$) while holding fixed the average outside option and the internal wage structure. Through the lens of the baseline

¹³The objective is decreasing in β once zero-profit binds, since $E y_{123}^C > \alpha_1$ and the profit condition binds. See Appendix A for more details.

model, moving from the equal to the unequal workplace increases the spread in workers' outside options and bargaining position, making cross-subsidization within a common contract more difficult to sustain. We test for this by asking organizers their predictions about aggregate union support in both workplaces. Through the lens of the model extension in Section 2.2, moving from the equal to the unequal workplace can make a larger bargaining unit less attractive. We directly test this by asking organizers how they would draw bargaining units across the two workplaces.

In the Wisconsin public schools context, we study a reform that introduces individual negotiation for teachers. The reform increases the dispersion in outside options ($\nearrow A$) only in CZs with sufficient competition among schools.¹⁴ The model prediction is that support for collective pay-setting should fall most in competitive CZs, especially among teachers whose individual bargaining power rose the most after the reform.

In the Hollywood setting, the information shock operates through workers' perceptions of inequality within the unionized sector. In particular, the information treatment leads workers to learn that inequality is higher than expected. One prediction of our model for this setting, derived in Appendix B, is that high-types realize that sustaining the proposed contract requires larger transfers to the low-types than they had anticipated, making high-types less likely to support the union.

2.4 Scope of Inequality

A unifying feature of our studies is that we consider inequality among workers who could plausibly share the same union representation—for example, within an occupation, as in our Hollywood and Wisconsin studies, or within a workplace, as in the organizer survey. Focusing on inequality in the labor market opportunities of the members in a potential bargaining unit allows us to adhere closely to the sources of inequality described in Acemoglu et al. (2001). Inequality along this dimension has risen substantially in the U.S. over recent decades. Using one-way decompositions, Appendix Figure A.1 Panel A shows that within-industry inequality accounts for 66% of the growth in earnings inequality between 1980–2015, and Panel B shows that within-occupation inequality accounts for 41% of this growth.

While inequality among workers that could share the same union representation accounts for a substantial share of the growth in labor market inequality, other forms of inequality have also grown, including cross-industry inequality and inequality between workers and employers. Our findings may

¹⁴Because average wages are fixed by pre-determined district budgets, average outside options across districts are equalized.

not extend to these other forms of inequality as the relevant economic channels for redistribution, e.g. profit-sharing or ownership stakes, and bargaining structures may differ.¹⁵

3 Study I: Organizer Survey

Our goal is to understand, from the viewpoint of professional organizers, whether inequality in the individual bargaining power of workers undermines their ability to unionize and, if so, whether organizers adapt their strategies to mediate the relationship between inequality and union support.

3.1 Recruitment

We collected contact information of 2,380 union organizers in the U.S. and Canada in summer 2024 across three channels. First, we collected the email addresses of 1,680 organizers from websites of over 500 national and local organizations spanning all branches of the ten largest U.S. unions. Second, we received a list of 433 publicly available emails from the organizers of 32 unions and compiled by Professor Kate Bronfenbrenner, the Director of Labor Education Research at Cornell University’s School of Industrial and Labor Relations. Finally, in fall 2024, we contacted 267 organizers on LinkedIn who listed “Union Organizer” as a current or former job title. Responses were collected via both email and LinkedIn through January 2025, with participants offered a \$30 gift card as an incentive.

Our final sample contains 182 respondents.¹⁶ Of these, 49% came from our online collection, 34% from Professor Bronfenbrenner’s list, and 17% from LinkedIn outreach. Respondents span 26 unions, 15 industries, 39 U.S. states, and 3 Canadian provinces (96% are U.S.-based). Appendix Table D.1 summarizes the most common unions, industries, and states, as well as individual characteristics. Most respondents are deeply embedded in the labor movement and bring substantial campaign experience, indicating that their views are shaped by considerable time in the field: the median respondent reports 7 years of organizing experience and 66% have lead organizing experience.¹⁷

¹⁵Esteban and Ray (1994) develop a conceptual framework of between-group dispersion that points to other important economic channels when broadening the scope of inequality.

¹⁶We restricted our main sample to respondents who reported a valid email. Results replicate with all respondents (N = 221), including those who did not leave a valid email (Appendix D.1).

¹⁷Lead organizers primarily exist to assist non-union workers in forming local chapters. They work directly for the union and are responsible for directing campaign strategies, overseeing campaign executions and training other organizers.

3.2 Survey Design

Methodology We conduct a vignette experiment where experienced organizers are presented with two hypothetical workplaces, which have different levels of dispersion in workers’ outside options but are otherwise identical. The difference between the low- and high-dispersion scenarios simulates a technological shock (à la [Acemoglu et al., 2001](#)) that increases some workers’ market wages and decreases others’. To isolate a shock to dispersion in outside options, we keep both average market wages and the internal wage structure unchanged. Organizers predict worker support and make strategic organizing choices for each scenario. We randomize scenario order to prevent sequencing bias and find similar results regardless of which environment organizers saw first (Appendix D.2). Finally, we present an incentivized choice of which workplace to allocate scarce resources toward. We truthfully assert that we will direct a significant donation to a union campaign based on their collective responses. We interpret their responses as evidence of how union organizers react to rising inequality.¹⁸

Vignette Design Both scenarios, shown in Figure I and described as Factory A and Factory B, have three types of workers: Quality Control, Metal Worker, and Pipefitter. We select these occupations because they require similar levels of general human capital but are sufficiently specialized that workers cannot easily switch between them. To anchor organizers to a real-life workplace, both vignettes specify that some workers have expressed interest in unionization, but the firm is hostile to unions. We also specify that all positions share an identical internal wage of \$40/hour, reflecting commonly compressed internal wages ([Hazell et al., 2022](#)). We specify that work hours are unpredictable (a non-wage amenity), and the employer refuses to sign an “Open to All” pledge.¹⁹ The key distinction between the two scenarios lies in the structure of market wages: all workers in Factory A face identical market wages of \$48/hour, while for workers in Factory B outside options vary by occupation, simulating a market demand shock that advantages one group (Pipefitters) and disadvantages another (Quality Controller). A third group (Metal Worker) faces the same outside option in both scenarios; however, their peers (Pipefitters and Quality Controllers) face outside options that are ~2 standard deviations higher or lower, respectively, according to the distribution of firm

¹⁸While we also collect data on strategies from organizers’ latest campaigns, our sample is not weighted to represent the U.S. union landscape. Hence, these responses cannot be used to infer the prevalence of organizing strategies in campaigns.

¹⁹The “Open to All” pledge is a commitment business can make to ensure that everyone is welcome and treated fairly, regardless of their race, ethnicity, national origin, sex, sexual orientation, gender identity, immigration status, religion, or disability.

wages within narrow position titles standardized by ADP.²⁰ By designing a symmetric shock for Pipefitters and Quality Controllers, we hold constant the average markdown between internal and market wages. For the rest of the paper, we term Factory A the *equal environment* and Factory B the *unequal environment*. We also term Pipefitters *high outside option* workers, Metal Workers workers *medium outside option* workers, and Quality Control *low outside option* workers.²¹ We include the full survey tool in Appendix Section H.1.

3.3 Inequality and Resource Allocation Across Firms

Our first goal is to understand how inequality affects where organizers choose to organize. In particular, we investigate how inequality shapes organizers' allocation of scarce resources across workplaces.

Survey Question We present organizers the two vignettes side-by-side and ask them: "With limited resources, which factory would you attempt to organize?" We elicit responses using the following incentive-compatible language: "We will direct a significant donation to an organization (not participating in this survey) focused on organizing a workplace closest to Factory A or Factory B, based on the answers we receive." We then ask why they made that choice (open-ended).

Results In their open-ended responses, organizers who opt to organize the equal environment often emphasize solidarity: "3 groups dealing with same pay and scheduling issues makes alignment on goals easier", "Easier to achieve and maintain solidarity among workers", "less chance for infighting." For organizers who select the unequal environment, reasons include: "2/3 are paid well below market rate and low wages could be a point of agitation", "Pipefitters are severely underpaid and we could build a campaign out of activists from that group", "It might be the harder of the two, but ultimately probably more worthwhile."

While these quotes are anecdotal, aggregate responses provide a clear picture: 67% of organizers would attempt to organize the equal rather than the unequal environment. Panel A of Table I reports a statistical test of the null hypothesis that organizers choose randomly (i.e., 50% prefer each environment), which we reject at p-value < 0.001. This result is consistent with their perception that workers in the equal environment are more supportive of unions and outweighs the view that combating inequality in

²⁰We use data from Cullen et al. (2025) to calculate the standard deviation in wages of a labor market narrowly defined by 10,000 standardized position titles.

²¹While we use direct language about outside options and inequality to describe the scenarios in this paper, our subjects did not see these terms at any point during the study.

the unequal environment may be “more worthwhile.”

3.4 Inequality and Perceived Union Support

Given organizers’ preference for the equal environment, we next unpack how they understand the directional relationship between labor market inequality and union support.

Survey Questions First, we tell organizers to assume that workers are informed about market wages, then we ask them to predict wage demands: “What hourly wage increase would each of the following groups demand for themselves in order to ratify the CBA?” Second, we ask them to predict workers’ alignment on those demands: “How easy would it be for all workers to agree on pay scale demands? This means Pipefitters agree with the demands Quality Controllers make, and vice versa.” Third, we measure beliefs about worker exit: “What share of each of the following groups at Factory A/B do you think will apply for a job elsewhere during the union campaign?” Last, we measure predictions about support for a union contract: “Please share your best guess: After all the details are hammered out, what percent of workers at Factory A/B would you expect to proactively vote yes to ratify a contract?”

Results We find that organizers predict the equal environment to be an easier win for the union: the median predicted share of workers voting to ratify a contract in the equal environment is 9pp higher (13%, p -value < 0.001) than in the unequal environment (Table I).

Figure II dives into factors driving organizers’ belief that support would be higher in the equal environment. Panel A shows that despite identical average market wages across environments, expected hourly wage demands in the unequal environment are both more dispersed and higher on average. In the equal environment, expected wage demands for each group (shown under the y-axis headers) are all near the average market wage (\$48) and within 1% of each other. By contrast, in the unequal environment, high outside option workers demand an additional \$6.0 while low outside option workers demand \$4.3 less (both p -value < 0.001). This means that the highest wage demand is 24% higher than the lower one, substantially larger than the 1% difference in the equal environment (difference p -value < 0.001). Next, we examine how organizers expect inequality to impact union members’ ability to agree on a pay scale. Implicitly, this measures if workers believe their peers’ higher outside options justify higher demands, and vice versa. While 37% of organizers say they would find it difficult to bring workers to agree on a pay scale in the equal envi-

ronment, a striking 84% say so in the unequal environment (p-value < 0.001; Table I). Taken together, these results highlight that market wage inequality not only raises some individual wage expectations, but also undermines internal cohesion—making it harder for organizers to unify workers around shared demands.

Panel B of Figure II examines organizers’ beliefs about workers seeking employment elsewhere, as high turnover poses a threat to unions by disrupting campaign momentum and continuity (Simms et al., 2018).²² In the equal environment, organizers expect around 20% of workers in each job to apply elsewhere. The unequal environment is markedly different: high outside option workers become much more likely to seek alternative employment (12.5pp, p-value < 0.001), low outside option workers become substantially less likely to do so (-7.2pp, p-value < 0.001), and medium outside option workers saw minimal change (1.6pp, p-value = 0.074). These numbers translate into a 2.3 pp (11%) increase in overall departures. This pattern underscores a critical challenge facing union organizers under rising inequality: the increased departures among high outside option workers are only partially offset by greater retention among low outside option workers, creating higher turnover rates that could undermine union strength.

3.5 Inequality and Organizing Strategies

We now investigate what campaign strategies organizers would implement in each environment, including what pay information to disclose, which issues to prioritize, and how large the bargaining unit should be. We select these specific strategies because they shape union strength and the effectiveness of worker representation. First, many workers turn to unions for greater pay transparency, and disclosure of market pay can directly confer bargaining power to workers (Jäger et al., 2024; Roussille, 2024).²³ Second, the issues organizers emphasize during a campaign set expectations about what the contract will accomplish. For example, if an organizing drive focuses on pay, then workers will expect wage demands to be more central in negotiations than if the focus was non-wage amenities.²⁴ Finally, the size of bargaining units affects union strength: a union’s threat to withhold labor depends on the collective value of its membership.

²²According to Amazon Labor Union organizer Justine Medina, “The faster the turnover is, the harder it is to organize.” (Brown, 2023). Union efforts at Amazon were undermined by higher turnover, both because Amazon challenged authorization cards signed by former employees and because turnover can make it more difficult to generate and retain support (Herrera, 2021).

²³For example, both the [New York Times Tech Guild](#) and the [American Federation of Teachers](#) emphasize pay transparency as a union benefit. Quoting organizers the New York Times article says: “It’s easy to feel overwhelmed and powerless when you’re not sure whether you’re being paid fairly. Sharing salary data with each other, and having conversations around that data, can build solidarity as you form a union.”

²⁴This is exemplified by organizing guidelines published by the [Communications Workers of America](#), which coach organizers that the first bargaining proposals should focus on the central issues that motivated the campaign. Bronfenbrenner and Juravich (1995) note that campaign success depends both on the issues organizers emphasize and their plans for initial contract negotiations.

Mishel (1986) confirms empirically that unions have more bargaining power when they have higher coverage of a workplace. Farber (2001) documents smaller bargaining units have a higher likelihood of success in representation elections, which he notes has implications for which units organizers target.

Survey Questions First, on pay transparency, we describe: “In the absence of more information, workers generally think that everyone earns what they do for similar work. You have the option to share the pay data you collected with workers. Would you share this pay information with workers?” Respondents choose among three options: publish during the campaign, publish after the campaign, or never publish. Second, we elicit issue priorities. After showing market wages, we inform organizers that hours are unpredictable at both factories and that the employer refuses to sign an “Open to All” business pledge (a commitment to maintaining a welcoming and safe environment for people). We ask: “To maximize support for the union, which of the following issues would be better to focus on during the campaign? Raising Pay, Predictable Hours, or “Open to All” pledge.” Finally, on bargaining unit size, we ask “Would you advise separate bargaining units for these three groups of workers?” This means that, even within a single factory, each worker group would be represented separately, and a strike would pause work for a minority of the firm’s workforce.

Results Figure III shows how labor market inequality shapes organizers’ approach to campaigns. Under the y-axis headers are average responses in the equal environment, while the regression coefficient shows the change when moving to the unequal environment. First, we find that, in the equal environment, almost all organizers (94.5%) would publish the market wage information during the campaign, with only 2.2% publishing the data after the campaign and 3.3% never publishing. In contrast, in the unequal environment, organizers are 23.6 pp less likely (p-value < 0.001) to publish the information during the campaign. Instead, respondents are 10.4 pp more likely to publish only after the campaign and 13.2 pp more likely to never publish (both p-value < 0.001).²⁵

This strategic withholding occurs even though high and medium outside option workers could learn that their market wages exceed internal wages. We interpret this behavior as evidence that unions may strategically reinforce search frictions or dampen the salience of outside options in unequal environments, even when doing so conflicts with the interests of some workers. Since workers can use information on outside options to negotiate higher pay (Jäger et al., 2024), the union’s decision to withhold this data may

²⁵Our theoretical results in Appendix B illustrate how higher inequality can disincentivize revealing pay information.

improve chances of campaign success but at the potential expense of the individual bargaining power of the majority of workers. In other words, organizers respond to inequality by prioritizing collective institutional viability over the interests of some individual members, echoing the model of [Kremer and Olken \(2009\)](#), in which unions that do not implement workers' optimal organizing strategies are more successful.

In addition to vignettes, we also ask organizers how they gathered and shared pay information in their most recent campaigns. 86% of organizers report collecting relevant pay information, but only 35% of those who collected it say that they published the data.²⁶ Aligned with vignette experiment results, organizers working in industries with above-median levels of pay inequality are 14 pp (p-value = 0.065) less likely to report that pay was the most important campaign issue.²⁷

Second, we find that, in the unequal environment, organizers are 10.4 pp less likely (p-value = 0.009) to focus on pay and 9.9 pp more likely (p-value = 0.014) to focus on predictable hours. This suggests that when dispersed outside options make pay a more divisive topic, organizers may shift their campaign focus toward non-wage amenities. This helps maintain worker solidarity but hinders unions' ability to secure wage compression precisely when such efforts are most needed for counteracting rising inequality.

Finally, in the unequal environment, organizers are 14.8 pp (168%, p-value < 0.001) more likely to recommend establishing separate bargaining units (vs. 8.8% in the equal environment). This finding is consistent with [Dodini et al. \(2025\)](#) who argue that smaller, more homogeneous units can help mitigate the disincentives to unionize created by inequality. It also maps onto our model extension, showing that greater inequality in the outside options of workers makes smaller, more homogenous units more attractive.

Collectively, these results highlight the tradeoffs organizers face under rising inequality: they seek reduced transparency, more focus on unifying (non-wage) themes, and smaller bargaining units. In the longer-run, these moves could potentially undercut the union's overall strength and effectiveness in counteracting rising inequality.

²⁶Open-ended responses reveal that organizers gathered pay data using a combination of accessing pay stubs, directly asking workers, and referencing pay scales or contracts from comparable unions. Among those who chose to disclose pay data, organizers report sharing market rates or rates at other unions, while some shared average establishment wages by group or position.

²⁷We classify 2-digit NAICS industries as more unequal if their 2023 national p90/p50 ratio (OEWS) is above the median. We use industry (rather than occupation) because organizers report their primary industry and typically work across occupations.

3.6 U.S. Evidence on Union Strategies

Although our sample of organizers spans a wide range of industries, unions, and geographic areas, it is not necessarily representative. We supplement our analysis with national-level evidence on the relationship between organizers’ strategies and inequality, which echoes the patterns observed in our survey.

We draw on a database of 3,007 private-sector collective bargaining agreements from the Office of Labor-Management Standards (OLMS) Online Disclosure Room. To match the ACS data used to estimate wage ratios, we focus on 602 contracts starting between 2002–2022, of which 451 contain valid industry and state information.²⁸ We classify contracts by whether they have a fixed pay scale, defined as a scale that explicitly sets pay levels by position and experience. We then break contracts into 100-word segments and assign textual topics using the Anchored Correlation Explanation (CorEx) model (Gallagher et al., 2017), following Sockin (2022).²⁹ To track bargaining unit sizes, we draw on Collective Bargaining Notice (F-7) data from the Federal Mediation and Conciliation Service (FMCS), covering 79,822 establishment-level bargaining units between 2015–2021. For each unit, we calculate a “coverage rate” equal to the unit size divided by the establishment size.

Panel A of Figure IV shows that unions in more unequal environments (measured using log p90-p50 wage ratios within industry \times region \times 3-year cells) are less likely to include fixed pay scales in their contracts.³⁰ Panel B shows such unions allocate a smaller share of contract content to wage demands, and Panel C shows they tend to cover a lower share of workers at the same establishment.³¹ Together, the results in these three panels echo the responses of the organizers in our survey, who shy away from wage demands, target smaller bargaining units and predict less agreement over a pay scale in the more unequal environment.

We also explore the relationship between inequality and union campaign focus over time using text from the AFL-CIO News, the official newspaper of the AFL-CIO federation from 1955–1996, which covered union activities, contract negotiations, strikes, and organizing campaigns (University of Maryland Libraries,

²⁸These numbers were pulled on July 13, 2024. The OLMS database is dynamically updated, so these numbers would be somewhat different if pulled on a different day. We exclude all contracts with missing start years in the database. Recent works that apply text analysis techniques on union contracts include Corradini et al. (2025), Arold et al. (2024), and Lagos (2024).

²⁹CorEx is a semi-supervised topic model that allows the researcher to input topic-specific “anchor words” guiding the model to identify coherent topics of interest. Section E.1 for details.

³⁰We focus on within-industry (rather than within-occupation) inequality because the OLMS data report the industry of the employer but not the occupations of workers. We also exploit between-region variation based on evidence that unionism exhibits geographical spillover and clustering (Holmes, 2006).

³¹To account for the possibility that wage ratios are affected by union presence, Appendix Figure E.1 replicates Figure IV using CPS-based wage ratios after subtracting off industry-specific union premium estimates and finds similar patterns.

2024).³² We classify topics for news segments using the same CorEx model as in our contract analysis. Panel A of Appendix Figure E.2 plots the “pay-related” topic share in the AFL-CIO news alongside U.S. income inequality measures (the top 10% income share and the Gini coefficient from Farber et al. (2021)). We find a clear negative relationship: the “pay-related” topic share steadily declined from over half in the early 1970s to just over a third in the 1990s, while income inequality rose sharply. Panel B shows that this decline was accompanied by little change in benefits-related coverage (e.g., insurance, pensions) and a rapid rise in working conditions content (e.g., safety, hours). This is consistent with our survey-based findings that union activities focus more on non-wage amenities in more unequal work environments.

4 Study II: Wisconsin’s Act 10

The organizer survey underscores the negative effects of inequality on union support from organizers’ perspectives. In this section, we directly test whether workers’ union support falls when pay inequality rises.

Our second study is in the public education sector. As of 2024, teachers make up 2% of the U.S. labor force and have the highest unionization rate of any occupation (BLS, 2025). In fact, 18% of all unionized workers in the U.S. are teachers (Lyon et al., 2024). We examine how quasi-exogenous differences in pay dispersion among public school teachers in Wisconsin—triggered by a 2011 policy reform, Act 10—affected individual decisions to support the teachers’ unions.

4.1 Institutional Background

4.1.1 Before Act 10

Prior to 2011, public-sector teachers in Wisconsin enjoyed considerable collective bargaining power but almost no individual bargaining power. The union negotiated a fixed wage schedule that determined pay solely by experience and education, guaranteeing steady pay progression over time without room for individual adjustments (Biasi, 2021; Biasi and Sarsons, 2022). Pay scales also varied little across districts.

³²AFL-CIO News has been widely used by labor historians in archival research (Holloway, 1979; Minchin, 2017; Sheehan, 2024). To our knowledge, we are the first to analyze its text quantitatively. See Appendix Section E.2 for more details.

4.1.2 After Act 10

The pay setting landscape changed abruptly in March 2011, when Governor Scott Walker signed Act 10 into law.³³ Act 10 eliminated collective bargaining over the wage schedule, effectively leaving teachers to negotiate their salaries individually. In response, restoring collective bargaining became a key focus of unions.³⁴

Act 10 also included additional reforms. Teachers' unions had to recertify annually by gathering an absolute majority of member votes in local elections. The reform further changed union dues collection from opt-out to opt-in, on a yearly basis (Godfrey & Kahn, S.C., 2018). It also required workers to contribute more of their pay toward their pension and healthcare, and required school districts to switch to cheaper healthcare plans. These provisions were implemented uniformly across all districts and workers.

Act 10 came into effect in each district when pre-existing collective bargaining agreements (CBAs) expired. Due to differences in negotiating calendars, CBAs expired in 2011 for 216 districts (out of 247 in our dataset), in 2012 for 23 districts, and in 2013 for 8 districts (Appendix Figure F.2; Biasi, 2021; Biasi and Sarsons, 2022; Biasi and Sandholtz, 2024), indicating staggered rollout of the policy across districts.

4.2 Research Design

To study the effect of inequality on union support, our research design exploits the heterogeneous impact of Act 10 on pay inequality across commuting zones (CZs). As shown in Section 4.4, only CZs with sufficient labor market competition for public school teachers experienced a shock to inequality. To interpret this empirical pattern, we begin by discussing the link between labor market structure and post-reform inequality.

The repeal of the uniform pay scale was intended to decentralize bargaining, shifting leverage to individual teachers. However, we argue that this shift in bargaining power was most consequential in CZs with many public school employers who could now compete over individual teachers. In such settings, teachers' individual outside options shifted, with highly desired teachers better positioned to negotiate higher salaries following the reform. By contrast, in highly concentrated CZs where a single district may be the only public school employer, the de facto individual leverage of teachers remained limited despite the de jure shift to individual bargaining. As a result, wages adjusted very little. Importantly, Act 10 did not mechanically create pay dispersion. Instead, it reshaped the bargaining environment—particularly

³³The act, officially a budget repair bill aimed at cutting \$3.6 billion in public employment spending, fundamentally changed operating conditions for public-sector unions, with the greatest impact on teachers. Police and firefighter unions were exempt.

³⁴While initially unsuccessful, this effort was still ongoing in 2024 (Associated Press, 2024).

in more competitive labor markets—in ways that made outside options more actionable. In our theoretical framework, this is akin to a positive shock to A , only experienced in competitive CZs.

Crucially, because district education budgets are determined largely at the state level, the shift in individual bargaining power among teachers in highly competitive districts can lead to greater within-district wage dispersion without altering average wage levels.³⁵ This is key for our research design: in order to isolate the effect of inequality on union support, we need Act 10 to impact union support differentially across more vs. less competitive labor markets through its effects on inequality rather than, for instance, on average wage levels. We validate this empirically in Section 4.4.

This natural experiment, combined with data on union revenues per teacher and teachers' dues payments, allows us to study how rising inequality shapes an organic decision that union members regularly face: paying dues. Moreover, we observe individual teachers wages both during and after the era of pay scales, enabling us to examine heterogeneity in teachers' responses based on their individual bargaining power. We test the hypothesis that teachers with high individual bargaining power are more likely to reduce union support post-Act 10 and, in particular, in highly competitive labor markets where they can best take advantage of their individual bargaining power.

4.3 Data

We combine personnel records of public school teachers with political contribution data to infer union membership. We also bring in union revenues and district-level CBA expiration.

Personnel data We use the PI 1202 All Staff Files from 2007 to 2017, provided by the Wisconsin Department of Public Instruction (WDPI). These files cover all WDPI and school district employees, including all public-school teachers, and report name, gender, birth year, years of experience in Wisconsin public schools, district and school assignment, total salary, fringe benefits, and full-time equivalency (FTE) units. Each row represents a position, and 2% of all individuals hold more than one position in a year. We restrict our sample to teachers working in 416 standard districts and aggregate at the person-year level, retaining the highest-FTE position.³⁶ Our final dataset includes 90,952 full-time teachers observed

³⁵In Wisconsin, funding is primarily determined by state-imposed formulas (Baron, 2022), resulting in similar average wages across districts and little correlation between average pay and local labor market competitiveness (Table III).

³⁶We exclude records with a salary equal to \$0 or missing FTE. We further exclude teachers employed in Cooperative Educational Service Agencies (CESAs), County Children with Disabilities Education Boards (CDEBs), and the State

between 2009 and 2017, with 45,139 teachers observed in 2011.

Union revenues We obtain union finances data from IRS Form 990 filings, which all tax-exempt organizations, including public-sector unions, must submit. These forms report revenues, expenses, assets, and liabilities. We access digitized Form 990s through the National Center for Charitable Statistics (NCCS) at the [Urban Institute \(2016\)](#), match union names from the Wisconsin Employment Relations Commission to filings, and successfully link 52 unions spanning 99 school districts. We calculate revenues per teacher, defined as total revenues (primarily dues reported on the filings) divided by the number of teachers in each union’s represented districts from staff files. We argue that the decision to pay dues offers a direct, “vote-with-your-wallet,” indicator of union support. Since the union’s post-reform campaign centered on reinstating the pay scale, we interpret the decision to pay dues as a tangible expression of support for that objective.

Individual union membership To track which teachers paid union dues each year, we follow the procedure proposed by [Foy \(2024\)](#). Starting from 2016, the state National Education Association (NEA) chapter automatically allocated \$19.99 from each member’s annual dues to its political action committee (PAC), while each of the 13 regional state union chapters directed another \$5 to their respective PACs. We infer union membership by fuzzy-matching teacher names in the staff files to PAC contribution records in the Wisconsin Campaign Finance Information System (WCFIS).³⁷ We treat any teacher appearing in the contribution data as a union member.³⁸ This approach yields a union membership rate of 47% in 2016, providing an individual-level measure of union support in the post-reform period.

Collective bargaining agreements We classify districts by the expiration date of their CBAs prior to Act 10, which determines when each district (and its union) became subject to the law’s changes. We use the dataset first compiled by [Biasi \(2021\)](#), combining information from multiple sources, including union contracts, districts’ employee handbooks, school board meeting minutes, and local news sources.³⁹ The

Department of Public Instruction (DPI). These agencies differ from conventional districts in governance and staffing.

³⁷The WCFIS website can be accessed [here](#). Before performing the match, we clean the names to account for inconsistencies (e.g., variations in middle initials) and ensure that each name uniquely identifies an individual within a filing period.

³⁸This is supported by two facts: most contributions are bunched at \$19.99 and \$5.00, i.e., the automatic deduction amounts (Appendix Figure F.1) and non-members rarely donate to union-affiliated PACs.

³⁹Union contracts generally report the date of the expiration of the agreement. Post-Act 10 school board minutes typically mention whether a contract was set to expire in 2011. The presence of an early version of district employee handbooks is also useful to establish when the post-CBA pay regime was introduced (which typically coincides with the date of the earliest handbook at the latest). When available, the dataset prioritizes information from union contracts, school board minutes, and handbooks. In cases where these documents are unavailable, the records are complemented with information from online local news sources.

dataset contains information on 247 of the state’s 416 districts, covering approximately 70% of all teachers.

4.4 Econometric Specification

We compare districts that were ex-ante more likely to experience an increase in pay inequality—once individual bargaining followed CBA expiration—to those less likely to experience inequality growth, based on differences in local public education sector concentration.

We measure labor market concentration for public school teachers using the Herfindahl-Hirschman Index (HHI), calculated from public school teacher employment across districts within each CZ in 2010:

$$\text{HHI}_j = \sum_{k:c(k)=c(j)} 10000 * s_k^2$$

where j denotes a district, $c(j)$ denotes the CZ of district j , and s_k is the share of public school teachers in CZ $c(k)$ employed in district k . The HHI ranges from 0 to 10,000.

We measure concentration across public school districts—rather than across individual schools, sectors (public vs. private), or industries (education vs. non-education)—for several reasons. First, teachers are employed and contracted at the district level. Second, exits from the public school sector are minimal: in our data, only 4.5% of teachers (excluding retirees) leave the public school system in 2010, indicating that for most teachers, employment outside the public school system is not in their consideration set. This low exit likely reflects non-transferable benefits, amenity differences, and high wages relative to private schools (Taie and Lewis, 2023; Taie and Goldring, 2019).⁴⁰

To visualize our event study, we divide CZs into above- and below-median HHI groups, where the mean HHI is 1928 in the above-median districts, vs. 679 in the below-median ones. Table II and Appendix Figure F.6 confirm that districts in low-HHI areas experience strong growth in within-position wage dispersion, while districts in high-HHI areas see no growth. Specifically, between 2010 and 2016, the within-position standard deviation in pay declined by an insignificant \$250 (or -5.9%) in high-HHI districts, but increased by \$2,647 (64.6%) in low-HHI districts (Table II, column 2). Henceforth, we refer to districts with below-median HHI as “Inequality Shock” districts and districts with above-median HHI as “No Inequality Shock” districts.⁴¹

⁴⁰We find that the exit rate is not impacted by the reform and is similar across districts with and without the inequality shock (Appendix Figure F.4, panel A).

⁴¹Appendix Figure F.5 Panel A shows that within-position salary dispersion at baseline was comparable across HHI

Identifying Assumptions Our design rests on several assumptions. The first is that, in the absence of the reform, union support in districts that do and do not experience a rise in inequality would have followed the same trajectory over time. While this assumption is not directly testable, we provide several pieces of supporting evidence. First, Figure V shows a lack of different pre-trends: per-member union dues evolved in parallel before the reform. Second, we show that high- and low-HHI districts are comparable along other important dimensions pre-reform. In particular, we find no significant differences in pre-reform (2007-10) changes in wages (Table III, Panel (a), Column 1) or fringe benefits, such as health care contributions (Column 2). The levels of these two variables were also comparable in 2010 (Columns 3 and 5) and in 2016 (Columns 4 and 6). Teacher characteristics, such as experience, gender, and share with a Master’s degree, are similar (Panel (b)), and so are district characteristics like locale, population, and political leaning (Panel (c)).

A second assumption is the Stable Unit Treatment Value Assumption (SUTVA), which requires no spillovers between treated and control units. This would be violated if teachers systematically moved between districts with and without rising inequality in response to the policy. Appendix Figure F.4 (panels B and C) rules out this possibility: fewer than 1% of teachers move out of their commuting zone or district in any year, and mobility rates are similar across high- and low-HHI districts both before and after the reform.

Finally, a key feature of our setting is the staggered implementation of Act 10 across districts, based on pre-reform contract expiration dates. This alleviates concerns that other events in 2011 confound our findings. Further, by comparing high- and low-HHI districts, we address concerns that our estimates are confounded by aspects of Act 10 that are not mediated by the presence of competing employers in the CZ.

We compare how union support evolves in districts with more or less susceptibility to an inequality shock due to individual bargaining, based on their 2010 HHIs. Specifically, we estimate the following staggered differences-in-differences model via OLS, separately for high- and low-HHI districts:

$$\ln(r_{jt}) = \sum_{k=-3}^3 \beta_k \mathbb{1}(t - E_j = k) + \theta_j + \tau_t + \varepsilon_{jt} \quad (7)$$

where r_{jt} denotes union membership dues per teacher in district j and year t , and E_j is the expiration year of district j ’s CBA. θ_j and τ_t contain district and year fixed effects, respectively. We normalize $\beta_0 = 0$, such that β_k represents the change in dues revenues per teacher k years after CBA expiration. Standard errors are clustered at the district level.

quartiles and did not correlate with the inequality shock. Panel B shows the increase in inequality shock areas is driven by the 1st HHI quartile (+\$3,076), followed by the 2nd (+\$590), with no significant change in the 3rd or 4th quartiles.

4.5 Results

As shown in Figure V, estimates of β_k for $k < 0$ are statistically indistinguishable from zero in both districts with and without a shock to inequality. In contrast, following expiration ($k > 0$), estimates become negative, large, and statistically significant in districts where inequality rose: revenues per teacher drop by 66% (coefficient = -1.086 , p-value < 0.001) two years after expiration and 73% (coefficient = -1.312 , p-value < 0.001) three years after. In contrast, the decline is significantly smaller in districts without a shock: -18% (p-value = 0.01) after two years and -36% (p-value < 0.001) after three years. By year three, the gap in union revenues between districts that did and did not experience a rise in inequality had reached 58% (p-value = 0.005; Appendix Figure F.8). Estimates are robust to controlling for the shares of teachers in each position by district (Appendix Figure F.9). Appendix Figure F.7 shows that our estimates of β_k are largely unchanged with the estimator proposed by Sun and Abraham (2021), robust to the presence of heterogeneous treatment effects across CBA expiration cohorts that are correlated with the timing of the treatment.⁴²

Estimates by quartile of each district's HHI confirm that the decline in revenues was the largest in the district in the top inequality shock quartile (Appendix Figure F.10). These results indicate that union support, captured by per-teacher membership dues collected, falls only in districts that experience a rise in inequality. Since the differential growth in pay dispersion (measured by the within-position standard deviation) between above- and below-median HHI districts is 68.3% (Table II, column 2, p-value = 0.047), and the difference in union revenue grew by 58% by year three post-reform, we estimate an elasticity of a 0.85% decline in union revenue for a 1% increase in pay dispersion in our setting.

We note that the gap between high- and low-HHI districts gradually widens over time. This may reflect the fact that wages do not adjust overnight following the reform, but rather through a series of renegotiations as teachers learn about their market values and administrators adapt their pay practices to retain and attract talent. Consistent with this, we show that pay dispersion grows over time in districts with an inequality shock (Appendix Figure F.6). In turn, teachers gradually come to better appreciate the inequality among peers and decide each year whether to pay dues to a union campaigning to reinstate the pay scale.

⁴²This test requires a “never treated” group of districts in each subsample. The inequality shock sample contains districts with CBAs expiring in 2011 and 2012; we consider the 2012 cohort as never treated and estimate effects up to one year post-expiration. The no-shock sample contains districts with CBAs expiring in 2011, 2012, and 2013; we consider the 2013 cohort as never treated and estimate effects up to two years post-expiration.

Heterogeneity by Individual-Level Productivity Next, we study how individual-level union support after Act 10 varies with a teacher’s productivity, proxied by their wage gains under individual bargaining. We measure this using the difference between a teacher’s wage in 2016 and the average wage for their position.

We estimate the effect of a rise in inequality on individual union membership in 2016-2017, the first two years membership data are available. We obtain these effects separately for teachers with 2010–16 wage gains above and below the median among all the teachers in the same district, whose pay would have grown in a similar way absent the reform due to movements along the pay scale.⁴³ To account for gender and union tenure potentially confounding the relationship between wage gains and union attachment across high- and low-HHI districts, we control for these characteristics and report their differential effects on the same figure.

The top panel of Figure VI shows that union support is 6.8 pp lower in inequality-shock districts for teachers with above-median wage gains, compared to 3.5 pp lower for those with below-median gains (difference p-value < 0.001).⁴⁴ For comparison, the middle and bottom panels examine heterogeneity by union tenure and gender, respectively, finding no statistically significant differences. Being in a low-HHI district decreases union support by 5.6 and 4.6 pp for above- and below-median tenure teachers (p-value = 0.274), and by 5.7 and 5.0 pp for men and women (p-value = 0.493).

We find this heterogeneity by wage at the macro level as well (Appendix E.3). Using CPS data, Figure E.4 Panels A–D show that between 1984–2019, the U.S. union membership decline is driven disproportionately by exit of middle-to-high wage workers, in both the private and public sectors. Figure E.5 Panel A shows that, while high-wage workers have higher unionization rates in most industries, this high-wage membership premium is smaller in industries with higher inequality. Panel B further shows that higher inequality growth is associated with greater decline in an industry’s high-wage membership advantage.

5 Study III: Experimental Evidence from Hollywood

In this section, we offer a test of whether workers reduce their union support when pay inequality becomes more salient. To do so, we compile and experimentally reveal a pay report. Following our theoretical framework, we aim to study a shock to workers’ perceptions of the distance between the pay they could negotiate as an individual and that of the median voter in the union. Therefore, the report highlights pay

⁴³Considering relative (rather than absolute) wage gains allows us to account for any cross-district differences in pay scales.

⁴⁴Membership differences between inequality shock and no-shock districts are smaller for teachers below the 50th percentile of the wage gain distribution, and grow progressively for those above it (Appendix Figure F.11).

disparities between individual pay negotiations at the mean and median, relative to collectively negotiated minimums, among Hollywood writers during their 2023 strike.

While well-suited to our research question, Hollywood unions have some unique features. Members are more skilled than the average U.S. union worker, and they negotiate collectively across employers, resembling industry-level bargaining common in Europe. Further, workers negotiate pay minimums at the position level but may individually bargain above them, akin to the U.S. sports industry.⁴⁵

5.1 Institutional Background

Similar to other industry-wide unions, the West and East Writers Guilds of America (collectively the WGA) are two guilds that represent over 11,500 film, television, and radio writers (Koblin and Barnes, 2023). The WGA primarily serves to negotiate contracts with the Alliance of Motion Picture and Television Producers, hereafter referred to as the Networks. As such, the Guild has significant influence in the wage-setting process through negotiation of the Minimum Basic Agreement (MBA). The MBA sets position-level pay floors, guaranteeing that Guild members earn at least the agreed-upon minimum for their work.

In May of 2023, the WGA's multi-year contract with the Networks ended, and terms for the subsequent three years were to be negotiated. The WGA went on strike from May 2 to September 27 (148 days) and ratified the new contract on October 9. A central goal of the strike was the renegotiation of position minimums. The WGA demanded a 6% raise to all minimums in the first year of the three-year contract, followed by 5% raises for the remaining two years. They also negotiated over residuals (the component of compensation tied to project sales or ratings), employment duration guarantees, and the role of artificial intelligence in production. We fielded our baseline survey from June 15 to June 30, 2023, when WGA members were in their 6th and 7th weeks of the strike, and our follow-up on August 11, 14 weeks into the strike.

This setting has two key advantages that allow us to experimentally measure the effect of inequality on union support. First, evidence suggests scope for pay information to shift writers' beliefs. Historical pay disclosures in the entertainment industry (whether released or leaked) have generated news coverage highlighting unexpected inequality (Copeland, 2014; Robb, 2021). Moreover, as described in Section 5.2.2, writers anchor their beliefs about typical wages on their own paycheck so providing truthful pay information increases perceived inequality for most subjects—a phenomenon studied in several other contexts (Cullen and Perez-Truglia, 2022; Hauser and Norton, 2017; Jäger et al., 2024; Stantcheva, 2024).

⁴⁵In our union contract sample (Figure IV), 25% set minimums only, while 69% contain a full pay scale (6% contain neither).

Second, this setting presents a unique opportunity to measure high-stakes support. The terms of the contract negotiations were well-known and widely publicized, including to the general public, which enabled writers to develop informed opinions about their union and its role in their labor market.⁴⁶ In this context, expressing negative opinions about the union to a team of researchers at highly visible institutions could reasonably be expected to carry real costs for the union. The WGA made public statements warning writers that any information they communicated to reporters could affect sensitive matters at the negotiating table and they condemned the Alliance of Motion Picture and Television Producers (AMPTP) for leaking negotiation details (Kilkenny and Goldberg, 2023). The union also used writers’ voices to rally public support through social media and news channels (Rice, 2023; Fitzgerald, 2023; Nierman, 2023), recognizing that public sentiment influenced their bargaining power.⁴⁷

Of course, our strike setting also comes with its limitations. First and foremost, we were careful not to intervene in the outcome of the strike or publish any results prior to its conclusion. Below, we discuss how contextual considerations shaped our design choices.

5.2 Baseline Survey

5.2.1 Baseline Survey: Recruitment

We recruited active Hollywood writers, targeting all WGA members through their publicly listed emails (WGA, 2023). Our contact list included 5,244 WGA writers (“contacts”) whom we invited to participate in our study between June 15 and June 23, 2023, closing our survey on June 30. By close, we had received 400 complete responses, or 7.6% of WGA contacts.⁴⁸ Using a wide-array of details available on IMDb (84% match rate), we observe our respondents are comparable to the overall pool of contacts: both groups are 64% male and are similarly experienced in terms of total credits (~39). Respondents are slightly less experienced in terms of earliest credit year.⁴⁹ See Appendix Section G.1 for details.

⁴⁶In addition to receiving extensive coverage from industry publications like *Deadline* (Patten and Robb, 2023), updates on the strike, negotiations, and deal were reported on by national media, including but not limited to the *New York Times* (Barnes and Koblin, 2023) and *NPR* (del Barco, 2023). A U.S. consumer survey in July 2023 found that 60% of respondents reported being aware of issues in the WGA and concurrent SAG-AFTRA (actors) strikes (Rottenberg, 2023).

⁴⁷In August 2023, following polls showing high public support for unions, the AFL-CIO put out a press release expressing that “with this unprecedented level of support, working people in unions are prepared to organize like never before” (AFL-CIO, 2023).

⁴⁸11.7% completed some fraction of the survey. This response rate is similar to studies in related contexts, e.g., Bursztyrn et al. (2021); Cullen et al. (2023).

⁴⁹In the creative arts, credits are used to acknowledge those who participated in the production and often shown at the end of movies. [This Wikipedia entry](#) provides details on the WGA screenwriting credit system.

5.2.2 Baseline Survey: Design

Our baseline survey tests whether writers underestimate our theoretically motivated inequality measure: the distance between their pay rate and the typical pay rate (“the median voter”). We also ask if a pay report would be valuable to them, as a marker of whether the information would be novel and salient. Additionally, we collect salary and work information, which we later use to create the pay report for the follow-up survey.

We collect compensation information using industry-specific language by asking what respondents earn as a percentage above the union minimum: “What percent above the MBA minimum do you typically earn as a [Own Position Title] at [Own Type of Studio]?”⁵⁰ Respondents report pay using a drop-down menu, selecting earnings below, at, or above the minimum in 1% increments up to 100%.

We frame pay in relation to one’s relevant minimum because it enables pay comparisons across contract types (e.g., weekly vs. episodic television) and across position titles. Additionally, minimums are well-known to writers; they are a focal point of the MBA and contract negotiations, and the WGA publishes a detailed “Schedule of Minimums” communicating them to members (WGA, 2023).

We next measure whether subjects anchor their beliefs about others’ pay rates to their own. We ask: “What percent above the MBA minimum do you think a typical [Own Position Title] in the Guild earns from [Own Contract Unit] at a [Own Type of Studio] in the first half of 2023?”, where [Own Contract Unit] can be one week in the writers’ room, one episode, or one feature-length script depending on the respondent’s reported largest source of income. We then ask about their confidence in their answer.

5.2.3 Baseline Survey: Results

The median reported pay rate of WGA members is 6.5% above the minimum, while the mean is 20.8% above. The median writer perceives a mere 3 pp gap between their own pay rate (as percent above the minimum) and that of the “typical” writer in the same position. See Figure G.1 for a visualization of how writers anchor on their own pay rate in their predictions of typical pay: we find a 1 percentage point increase in own pay translates into a 0.5 percentage point increase in typical pay predictions (p-value < 0.001). In reality, the gap between a writer’s pay rate and that of the typical writer in their position is two to

⁵⁰At the survey’s start, respondents identify their narrowly defined position title ([Own Position Title]) using the full set of titles from the union contract. They report whether they work in writing (TV or film), directing, producing, or acting. TV writers select among seven titles and film writers select among low- or high-budget films, depending on which is their largest income source. All respondents also report whether “streaming services” or “traditional studios” constitute a larger income share ([Own Type of Studio]).

three times as large. The median absolute distance between a writer’s pay rate and the median earnings in their position is 8 pp, compared with 5 from the mode and 12 from the mean.⁵¹ In essence, members anchor on their own pay rate and underestimate the pay gap between themselves and other workers.⁵² In addition, only 13% of respondents report being “very” or “extremely” confident in their answer about typical pay.

This widespread uncertainty naturally raises questions about writers’ demand for additional pay information. Our baseline survey also documents that the vast majority of writers (87%) would be interested in a pay report, with a median willingness to pay of ~ \$90 and a mean of ~\$900 (Appendix Section G.2.1). Writers further indicate they would primarily use this information to understand their relative standing in the pay distribution and to improve outcomes of their individual pay negotiations.

5.2.4 Baseline Survey: Frictions to Pay Information Access

Survey Questions Given high demand for greater pay transparency, we seek to understand what frictions writers face in accessing pay information that they value—in particular, understanding why they perceive it would not be fruitful to ask for it from the Guild. We find one answer from the evidence in our organizers’ study, where organizers report rarely publishing salary information and, in our vignette experiment, are less likely to publish pay information in more unequal environments (Section 3.5).

To study this, we tell half of the writers that we are considering petitioning the Guild for pay data to help us produce the report. We then ask if we can include their expressed interests for this information in our petition. For the other half, we instead name the Networks as the recipient of the petition to establish a baseline willingness to request pay information from a third, arguably adversarial, party.⁵³

Results Figure G.4 shows that private willingness to pay (Panel A) does not imply public willingness to petition for the report (Panel B). While 87% of respondents privately indicate interest in the report, only 34% (38%) are willing to make their request public to the Guild (Networks). This points to one plausible source of friction: writers are reluctant to ask for pay information. Further, our evidence suggests that the Guild and Studios are not providing adequate pay information to workers — 80% of WGA members

⁵¹The gap is similarly large if we instead compare to the typical writer across all positions rather than within-position. In this case, the median absolute distance from the median is 6.5 pp, compared with 6.5 from the mode and 20.8 from the mean.

⁵²While they underestimate earnings gaps, they do not systematically under- or overestimate pay rate levels: just as many writers over-guess (37%) as under-guess (39%) typical writer earnings in their position, with a median error of 0pp.

⁵³Both the Networks and Guilds have access to detailed pay data. The Guilds typically require members to report all gross earnings through the process of collecting dues. Networks collect this information directly as contractors. However, neither the Networks nor the Guild publish pay reports beyond aggregate data.

declare they would use a pay report to know where they stand in the distribution. This potential reluctance to share information is consistent with the fact that members may use the report to individually negotiate their contracts, potentially raising costs for Studios and detracting from collective negotiations.

5.3 Follow-up Survey

Our baseline survey shows that WGA members anchor their beliefs about others' pay on their own pay, underestimating the gap between their pay and the typical rate. Writers also value pay information, and would use it in negotiations, but face frictions accessing it from unions or employers. This creates an opportunity for us to experimentally provide new information about pay inequality and test its effect on union support. However, we need to do so in a manner that does not meaningfully interfere with the on-going strike.

5.3.1 Follow-up Survey: Recruitment

On August 11, approximately 100 days into the strike and six weeks after sending the baseline survey, we sent a follow-up survey to 4,716 WGA members (all members for whom we had contact information, or about 40% of total membership).⁵⁴ Three considerations shaped our recruitment strategy: avoiding interference with the ongoing strike, maintaining internal validity by not contaminating our control group, and ensuring that all writers with publicly available emails had equal access to our pay report. The follow-up was therefore deliberately simple. It contained only a summary of baseline results and a single question and was deployed for just 48 hours over one weekend. We waited until after the strike ended before making any results public.

This yielded 310 complete responses during the 48-hour window, 6.5% of our contact list, representing 3% of all WGA members. As shown in Appendix Table G.1 columns (3)-(4), contacts and respondents share similar characteristics across dimensions accessible on IMDb, with respondents having a slightly lower proportion of men than contacts (57.5% vs. 63.6%).

5.3.2 Follow-up Survey: Design

Our follow-up survey aims to measure union support under real stakes using exogenous variation in the salience of pay dispersion. We randomize between asking about union support before (control) vs. after

⁵⁴Aiming to collect a high-stakes union support measure, we focus on our contacts with a clear WGA affiliation. We identify members through self-declared status in the baseline or public information (i.e., we collected their email from the WGA website so we know they are a member). The contact list is somewhat smaller than the baseline survey because we exclude suspected members to ensure verified membership; the follow-up survey did not re-collect membership status to remain concise (Table G.1).

(treatment) respondents see a pay report (Panel A of Figure VII). Panel B shows the report, which contains median, mean, and maximum pay as percents above the minimum, separately for men (+10% median and +25% mean) and women (+3% and +14%), with standard errors for the means. The disclosed information is computed using (self-declared) writing pay rates among WGA members in our baseline survey.

Our intention with this pay report is to underscore pay *gaps*. In the organizer survey and the Wisconsin policy setting, variation in *A* arises from changes in inequality itself, whereas in this setting the pay report serves as an information intervention intended to shift workers' perceptions of *A*. We expect the larger-than-expected gap between their own pay and typical pay to be the most salient takeaway for our subjects, based on our baseline analysis of pay perceptions and prior research on perceptions of pay gaps (Breza et al., 2018; Cullen and Perez-Truglia, 2022; Jäger et al., 2024).⁵⁵ To emphasize disparities, our report presents the mean and median pay rates relative to the negotiated minimums (“scale”), an object that is well known to writers. However, we caution that other signals can be inferred from a pay report. For example, a pay report with surprising information can reflect on the extent of pay transparency among Guild members more broadly.

We survey writers at a critical juncture—more than 100 days into the WGA strike, just as negotiations between the Guild and Networks resumed. The single question we ask is about union support, phrased: “Do most writers think the WGA demands will meet the needs of all WGA members?” The possible answers were: Almost entirely, Mostly, Somewhat, Mostly not, Not at all, I don’t know the WGA demands.⁵⁶

This question was deliberately designed to allow respondents to reveal their own support for the union—or lack thereof—while maintaining plausible deniability. In other words, the question provides space for a strategic response, one that accounts for the high stakes of expressing dissent during an ongoing labor dispute. Indeed, at the time of our survey, any large poll that could be interpreted as a measure of solidarity had real potential to influence the negotiations. The question directly echoes the Guild’s stated goal, prominently expressed in their slogan that “no segment of the membership would be left behind” (WGA Negotiating Committee, 2023). Hence, a positive poll signals trust in the unions’ claims and solidarity with union leaders; a negative poll, by contrast, implicitly questions the Guild’s leadership.

We acknowledge that the question could also have been interpreted at face value by some—as a neutral assessment of whether the publicly stated demands are likely to benefit writers across the pay

⁵⁵Recall that our baseline survey shows that respondents systematically underestimate the gap between their own pay and the typical pay by a factor of 2 (distance to median) or 3 (distance to mean).

⁵⁶Only 11 respondents answered “I don’t know the WGA demands.” This confirms that writers were well-aware of what the WGA was asking for at the bargaining table, and implies that our survey collects informed responses.

distribution. Under this interpretation, respondents might answer based on their beliefs about how the proposed contract affects not just themselves but also their peers across the wage distribution.

Our theoretical model helps distinguish between these two interpretations. If writers are answering strategically, we expect responses to vary systematically by productivity. In particular, our model predicts that heightened awareness of inequality weakens union support among higher-productivity writers. In contrast, if writers interpret the question purely at face value, we would not expect support to vary meaningfully by productivity.⁵⁷ As we describe in the next section, the former prevails.

5.3.3 Follow-up Survey: Results

Figure VIII compares union support by treatment status (whether the respondent answers before vs. after seeing the pay report). Among respondents who answer before seeing the pay report, only 9% of respondents report a lack of solidarity among writers about WGA demands, i.e. answered that most writers do *not* (“Mostly not” or “Not at all”) think WGA demands meet the needs of all members. This figure rose to 23% among those who respond after seeing the pay report—a 156% relative increase (difference p-value < 0.001). Figure IX examines heterogeneity in the treatment effect by individual productivity, and for comparison, by tenure and gender. We proxy individual productivity using credit count⁵⁸ and control for differences in gender and tenure between high- and low-productivity respondents. The decline in union support is primarily driven by high-productivity respondents: the share stating that Guild demands do *not* represent all members rose by 20.5 pp in the treatment group relative to control, compared to only 5.5 pp among low-productivity respondents (DiD p-value = 0.092). This is consistent with Acemoglu et al.’s (2001) theory and echoes our findings from Wisconsin: high types may be especially sensitive to their prospects for individual negotiation. There are no significant differences in responses by tenure (p-value = 0.852) or gender (p-value = 0.200).⁵⁹ In sum, when we present information revealing larger-than-expected gaps in individually negotiated pay, respondents, especially high-productivity ones, become more willing to express negative views about union demands during the high-stakes strike. This is consistent with our theory, in which high-productivity workers anticipate being on the losing end of redistribution.

⁵⁷Exceptions can arise if writers perceive the membership to have a different composition, as a function of their own productivity.

⁵⁸The WGA [Screen Credits Manual](#) explains: “A writer’s credits play an enormous role in determining our position in the motion picture and television industry. Our professional status depends on the quality and number of screenplays, teleplays, or stories that bear our name.” Given the nature of our data, we use quantity as a proxy.

⁵⁹Our results are not sensitive to the inclusion of position fixed effects (Appendix Figure G.2).

Our findings from both Hollywood and Wisconsin shed light on a related conjecture that lies outside our formal theoretical framework: that workers with lower bargaining power might be galvanized by high intra-occupational inequality. We find no supporting evidence. While the negative effect of inequality on union support is more muted among low-productivity or low-bargaining-power workers, it is not positive. Perhaps second-order beliefs (peer effects) dampen this channel: if workers believe their peers are less willing to strike in unequal environments, their own enthusiasm for organizing may diminish. Organizer strategy may also contribute—when inequality is high, organizers may shift attention away from wage demands, reducing the mobilization of workers with lower bargaining power.

6 Taking Stock

Our three designs show that rising wage inequality among workers undermines union support. We now compare findings across settings and discuss implications for the role of inequality in union membership declines.

6.1 Elasticity of Union Support with Respect to Inequality Across Studies

We find that, while our three designs differ in institutional contexts and sources of causal identification, they yield comparable magnitudes on the elasticity of union membership in response to rising inequality.

In the Wisconsin study, between 2010 and 2016, the within-position standard deviation of earnings among public school teachers grew 68.3% more in competitive districts (below-median HHI) (Table II). Meanwhile, by 2016, union membership fell 10.6% in these districts compared to the rest (-5.1 pp from a basis of 48% for no-shock districts). This implies a union-support-inequality elasticity of -0.16.⁶⁰

With stronger assumptions, we can estimate a similar elasticity in the Hollywood context. We find that the median writer estimates the gap between their own pay and typical pay within their position to be 3 pp of scale, while the actual gap in collected salary data is 8 pp of scale, suggesting writers underestimate the true gap by 5 pp. While writers may have only partially updated their beliefs when shown information, we can estimate the perception shift by applying a Bayesian learning coefficient of 0.66 from a similar salary survey experiment (Cullen and Perez-Truglia, 2022) to estimate that posterior beliefs about the pay

⁶⁰We note that the elasticity reported here is a membership elasticity, not the revenue elasticity calculated in Section 4.5. We use membership to maintain comparability with the other two settings, where support is measured at the individual rather than aggregate revenue level. The membership elasticity is smaller in magnitude than the revenue elasticity (-0.85), consistent with our finding that high-wage-gain teachers—who contributed more to union revenue—were more likely to exit.

gap rose by 3.3 pp (110% of the median prior, 3 pp). In response to treatment, expressed union support fell by 15.4% (-14 pp from a 91% basis in Figure VIII). This implies a union-support-inequality elasticity of -0.14, close to our Wisconsin elasticity estimates.

In the organizer survey, we present two workplace vignettes (Figure I): one where co-workers earn equal wages, and another where wages are spaced apart by \$18. Because we started with zero inequality, the percentage change in inequality is infinite, and the standard elasticity formula is not defined. If instead we define inequality change as the growth in the standard deviation of log wage, from 0 to 0.32, combined with the 6.6% drop in predicted mean vote share in Table I (5 pp change from a 76% basis), we obtain a union-support-semi-elasticity of -0.21. We note that this semi-elasticity is not directly comparable to the standard elasticity computed for Wisconsin, as the two measures differ in whether inequality change is expressed in absolute or relative terms. Nevertheless, both estimates are consistent with a meaningfully negative relationship between inequality and union support.

6.2 Inequality and Union Density Over Time

Although they come from different contexts, our elasticity estimates are similar in magnitude. In this section, we use these elasticities to assess the role of rising inequality in the decline of union membership over time in the US. Since unions typically organize workers in the same occupation and industry, we construct a measure of inequality that isolates the relevant “micro-level” inequality by decomposing the variance of log earnings into within and between industry-by-occupation cells, using the CPS (Figure A.1 Panel C). Between 1980 and 2019, the within-cell standard deviation grew by 21%, and the variance by 47%. This magnitude is robust to using alternative cell definitions: inequality growth within one-way industry, occupation, or firm cells all fall in the 18-25% range.⁶¹

A back-of-the-envelope calculation using our elasticity estimates between -0.14 and -0.16 suggests that a 18–25% growth in inequality would depress union membership by 2.5–4.0%. To put this in perspective, union membership rates fell from 22.2% of wage and salaried workers in 1980 (Freeman, 1997) to

⁶¹Figure A.1 Panels A and B show that within-industry and within-occupation standard deviations rose by 25% and 19%, and the variances rose by 56% and 42%, respectively. Hoffmann et al. (2020) similarly find that between-industry and between-occupation differences play a secondary role in rising inequality. Using administrative earnings data, Song et al. (2019) find that within-firm variance rose from 0.43 in 1980 to over 0.5 around 2010, a smaller rise that largely reflects their inability to observe hours worked (Haltiwanger et al., 2023). Studies with hours data (e.g., Lachowska et al. (2023)) report lower inequality levels but similar changes over time. Adjusting for this implies within-firm standard deviation (variance) growth of about 18% (40%) in Song et al. (2019).

10.3% in 2019 (BLS, 2020), a 54% drop. While many factors contributed to this decline, our estimates suggest that rising inequality can account for 4.6–7.4% of the overall union membership decline.⁶² For comparison, Farber et al. (2021) find that the other direction of causality is similarly sized: declines in union membership levels can explain 5–10% of the inequality growth in the late 20th century.

6.3 Coverage and Generalizability

An important question concerns the external validity of our three research settings: do we capture representative modes of organizing in the U.S. and internationally?

In the U.S., mobilization, elections, and collective bargaining occur primarily at the establishment level and may cover a single occupation or several related occupations (Bronfenbrenner, 2000; Dinlersoz et al., 2014). The organizer survey reflects this typical U.S. setting: at the workplace level, with three adjacent but different occupations.⁶³ This aligns with the fact that workplace-level bargaining units in the U.S. can cover a single occupation or multiple related ones, but few (less than 10%) are “wall-to-wall” covering all workers at an establishment (Bronfenbrenner, 2000).

The Hollywood experiment occurs in a relatively small industry, and reflects an *occupational* (or *craft/guild*) unionism model that covers similar workers (writers) across employers and exerts bargaining strength through controlling aggregate occupational labor supply. This setting most closely resembles the sectoral bargaining structure dominant in Europe, where collective bargaining agreements are often at the industry-level, with negotiations occurring across multiple firms (Jäger et al., 2024).⁶⁴

Finally, while studies often exclude public-sector unions, the public sector accounted for 15% of U.S. employment and 49% of unionized workers in 2024 (BLS, 2025). More generally, unionization rates are roughly four times higher in the public than in the private sector across OECD countries (OECD, 2025). Teachers’ unions in particular carry weight, as K-12 teachers employed by state and local governments make up 3% of overall employment but 18% of all union workers.⁶⁵ The Wisconsin study reveals public-sector union support responds to inequality.

⁶²Notably, after 2000, “within” inequality measures stabilize, while union membership falls by 39% between 1980–2000 vs. 24% between 2000–2019 (BLS, 2001). Our estimates imply inequality stabilization explains about a quarter of this deceleration.

⁶³The SOC codes are 51-9061 for quality control, 51-4000 for metal workers, and 47-2152 for pipefitters.

⁶⁴In the U.S., the presence of occupational unions has heavily receded since the early 20th century (Cobble, 2001), but they are still commonplace in the construction and trucking industries, and the Railway Labor Act establishes craft-based multi-employer bargaining for railroad and airline workers (Rhinehart and McNicholas, 2020).

⁶⁵Authors’ calculation based on CPS.

The consistency of negative union-support-inequality elasticities across these institutionally diverse settings suggests that the relationship we document is not an artifact of any particular bargaining structure.

Additionally, trends across OECD countries align with our economic mechanism. Although collective bargaining in many OECD countries operates at the industry level—so the majority of workers are covered by collective agreements—workers still decide whether to join the union. In this context, even as coverage has remained high, union *membership* has declined: membership density in the OECD fell from about 30% in 1985 to 15% by 2023 (OECD, 2025). At the same time, within-industry inequality has also been increasing in the United Kingdom (Faggio et al., 2010) and continental Europe (Zwysen, 2022). Moreover, we find a negative relationship between within-occupation wage inequality across most developed countries, especially those with strong sectoral bargaining such as Sweden (Figure E.6).⁶⁶ This indicates that the negative relationship is not unique to the U.S.

7 Conclusion

This paper provides causal evidence on how wage inequality among workers affects the labor movement. Using three complementary research designs—a vignette experiment with union organizers, a natural experiment among teachers, and an information intervention during the 2023 WGA strike—we document that rising wage inequality undermines union strength through several distinct mechanisms.

Consistent with the theoretical predictions of Acemoglu et al. (2001), the first mechanism operates through weakening solidarity among high individual bargaining power workers as inequality rises. We find evidence of this pattern across multiple settings. In Wisconsin, teachers who experienced above-average wage gains in districts affected by an inequality shock were significantly more likely to stop paying union dues. In Hollywood, high-productivity writers who received information about member pay disparities were 20.5 pp more likely to express doubts about solidarity around union demands during a high-stakes strike, while expressions of solidarity among low-productivity writers remained unchanged.

Beyond workers' response, we show that union organizers facing hypothetical choices about their campaign adapt to inequality in ways that may preserve institutional viability but potentially undermine redistribution. Facing greater dispersion in outside options, organizers report they would shift campaign

⁶⁶We use data from the International Social Survey Programme Work Orientations Survey in years 1997, 2005, and 2015, which surveys workers about their occupation, union membership, and earnings. See Section E.4 for more details

focus away from wages toward less divisive non-wage amenities and advocate for smaller bargaining units that separate workers with different outside options. Under budget constraints, the majority of organizers decide to allocate fewer resources to high-inequality environments despite acknowledging greater potential impact there. These strategic responses represent rational adaptations to rising inequality but may reduce unions' ability to compress wages at scale.

Our results offer a fresh perspective on the joint evolution of inequality and unionization in the United States. While prior research documents a strong negative correlation between these trends (Freeman, 1980; DiNardo et al., 1996; Card, 2001), the prevailing interpretation emphasizes unions' role in reducing inequality. Our results suggest a complementary dynamic: rising inequality may also weaken unions. Indeed, in AFL-CIO News, we find that the labor movement shifts focus to non-wage amenities when inequality rises, and a corresponding negative relationship between industry inequality and the size of bargaining units as well as success negotiating pay scales. This suggests the possibility for “inequality traps”—self-reinforcing dynamics where growing wage dispersion undermines the institutions designed to contain it, thereby making collective action increasingly difficult as earnings gaps widen within occupations.

Important questions remain for future research. First, within-occupation inequality accounts for much of the growth in labor market inequality, but other forms of inequality have also grown, including inequality between workers and employers. Other forms of inequality likely affect labor organizing through different channels and are equally deserving of attention. Second, the mechanisms we identify—withdrawal of high-types, strategic adaptation by organizers, and the redirection of resources away from unequal environments—may operate beyond labor markets in other contexts, such as political organizing and civic society.

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Figures and Tables

Figure I: Market Wages

PANEL A: Equal Environment (Factory A)

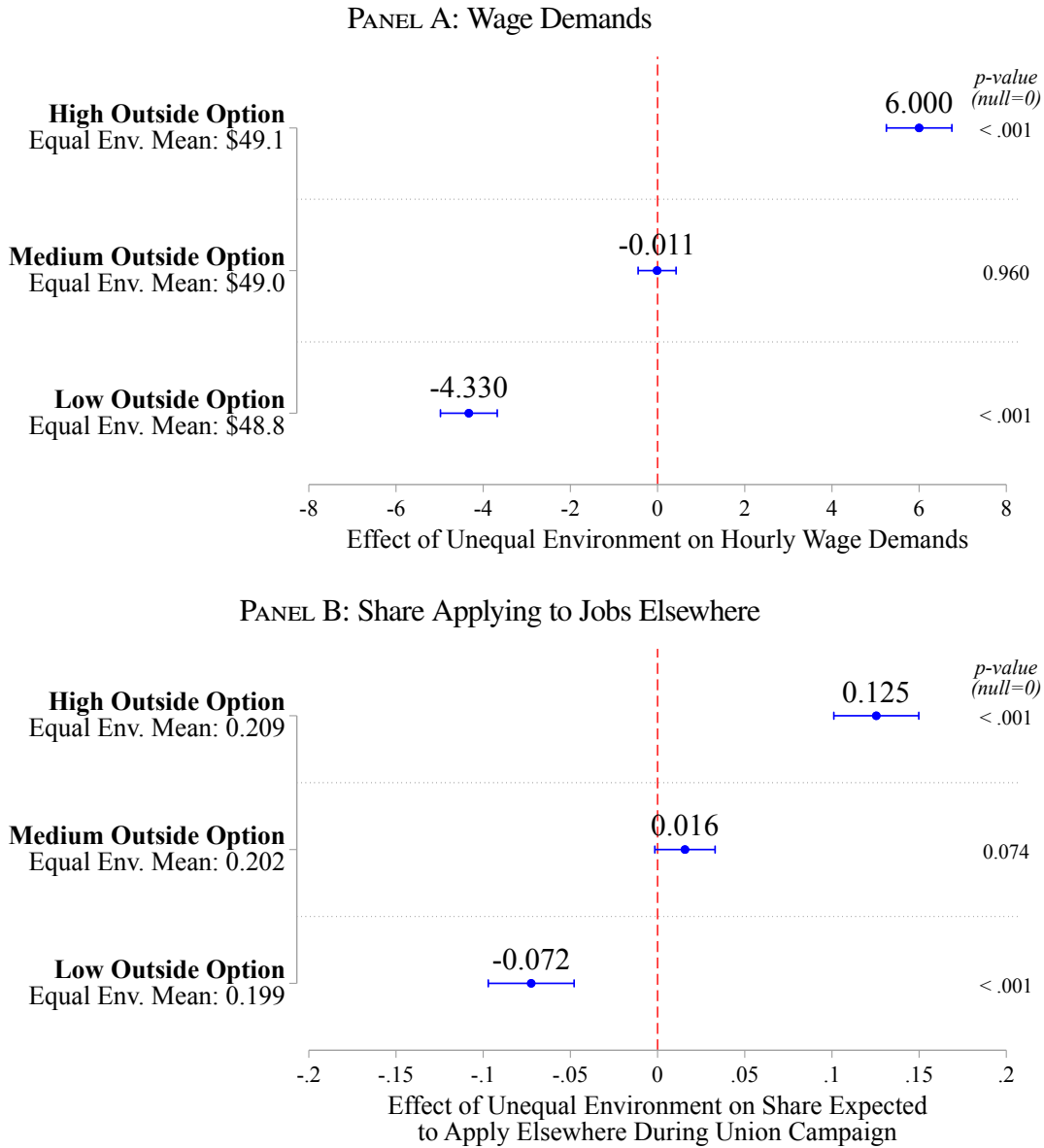
Assignments	Hourly Wage	Research on Market Wage
Quality control (1/3rd)	\$40	\$48
Metal Worker (1/3rd)	\$40	\$48
Pipefitter (1/3rd)	\$40	\$48

PANEL B: Unequal Environment (Factory B)

Assignments	Hourly Wage	Research on Market Wage
Quality Control (1/3rd)	\$40	\$30
Metal Worker (1/3rd)	\$40	\$48
Pipefitter (1/3rd)	\$40	\$66

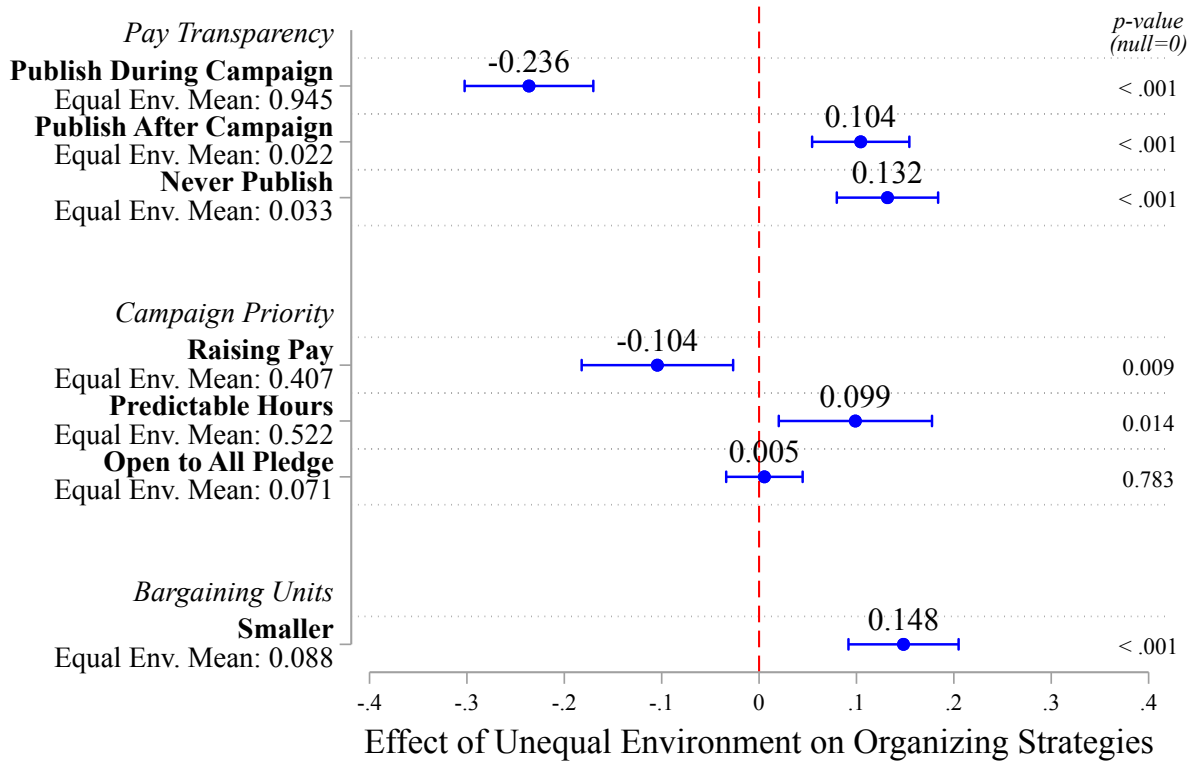
Notes: This figure shows the information on internal and market wages in each vignette from the organizer survey described in Section 3. We randomize the order in which organizers see the two environments. Based on Panel B, we refer to Quality Control workers as having “Low Outside Options,” Metal Workers as “Medium Outside Options,” and Pipefitters as “High Outside Options.” For the full text of the vignette, see Appendix Section H.1.

Figure II: Worker Actions by Outside Option



Notes: This figure shows how predicted worker actions vary across workers with different outside options when market wage inequality increases, from the organizer survey described in Section 3. In Panel A, *Equal Env. Mean* reports the average predicted wage demands by group in the equal environment. In Panel B, *Equal Env. Mean* reports the average share of workers, by group, that organizers expect to apply for jobs elsewhere during the union campaign in the equal environment. Estimates show the effect of moving from the equal environment to the unequal environment on each outcome by group. The market wage for all three groups in the equal environment is \$48; in the unequal environment, they are \$66 for the high outside option group, \$48 for the medium group, and \$30 for the low group. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that each coefficient equals zero and report p-values to the right of the plots. Sample size is 182 organizers.

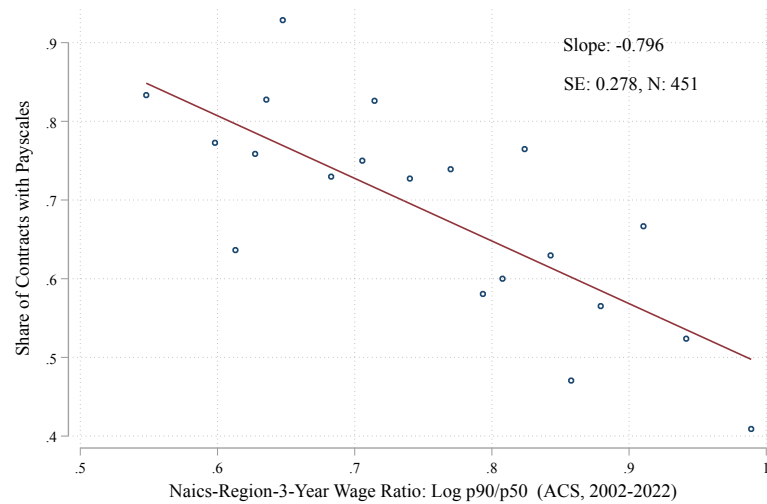
Figure III: Organizing Strategies



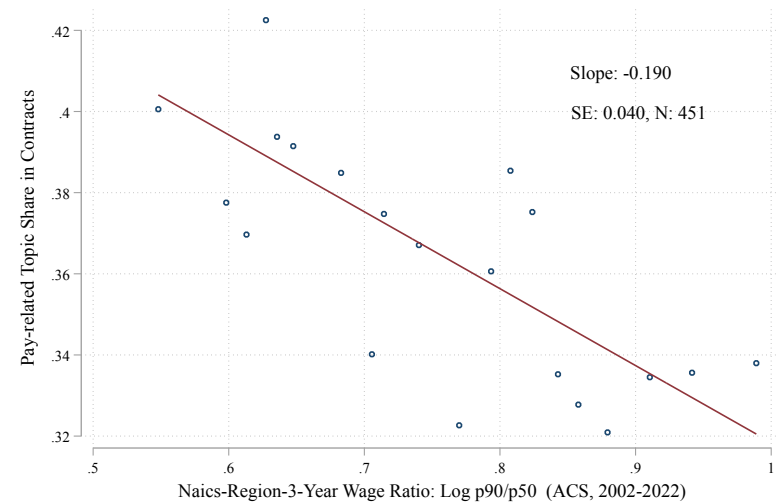
Notes: This figure shows how organizers adapt their campaign strategies when market wage inequality increases, from the organizer survey described in Section 3. Under each outcome specified on the y-axis, *Equal Env. Mean* reports the average outcome in the equal environment, while plotted estimates show the effect of moving from the equal environment to the unequal environment. Specifically, coefficients under header *Pay Transparency* report whether organizers would publish market wage information from Figure I; coefficients under header *Campaign Priority* report which of the three issues organizers would prioritize during the campaign; the coefficient under header *Bargaining Units* reports whether organizers would pursue separate, smaller bargaining units for different worker types. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that each coefficient equals zero and report p-values to the right of the plot. Sample size is 182 organizers.

Figure IV: Cross-sectional Relationships between Industry-Region Inequality and Union Organizing Strategies

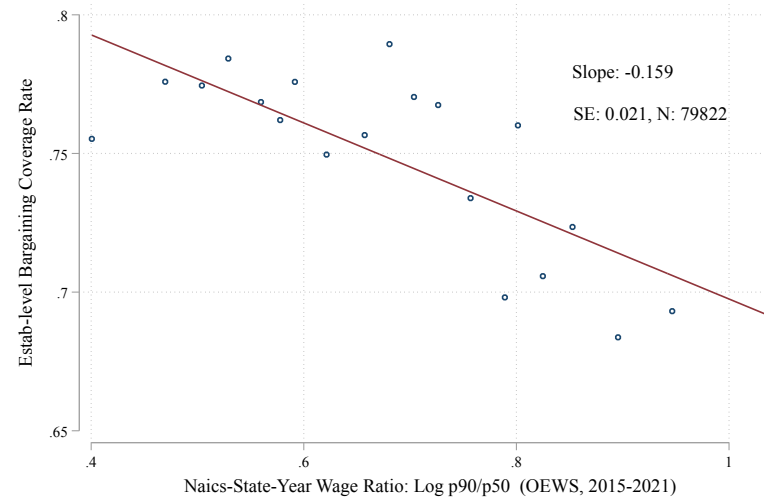
PANEL A: Share of Contracts with Pay Scales



PANEL B: Pay-related Topic Share in Contracts

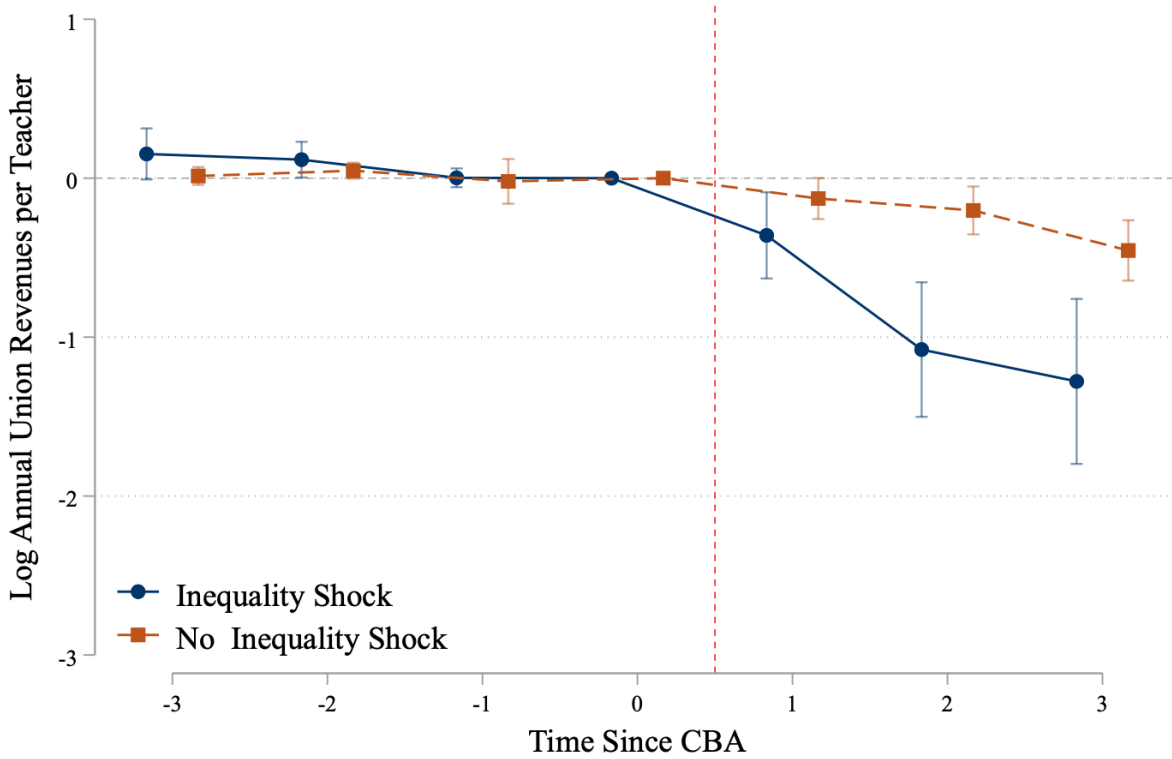


PANEL C: Estab-level Bargaining Unit Coverage Rate



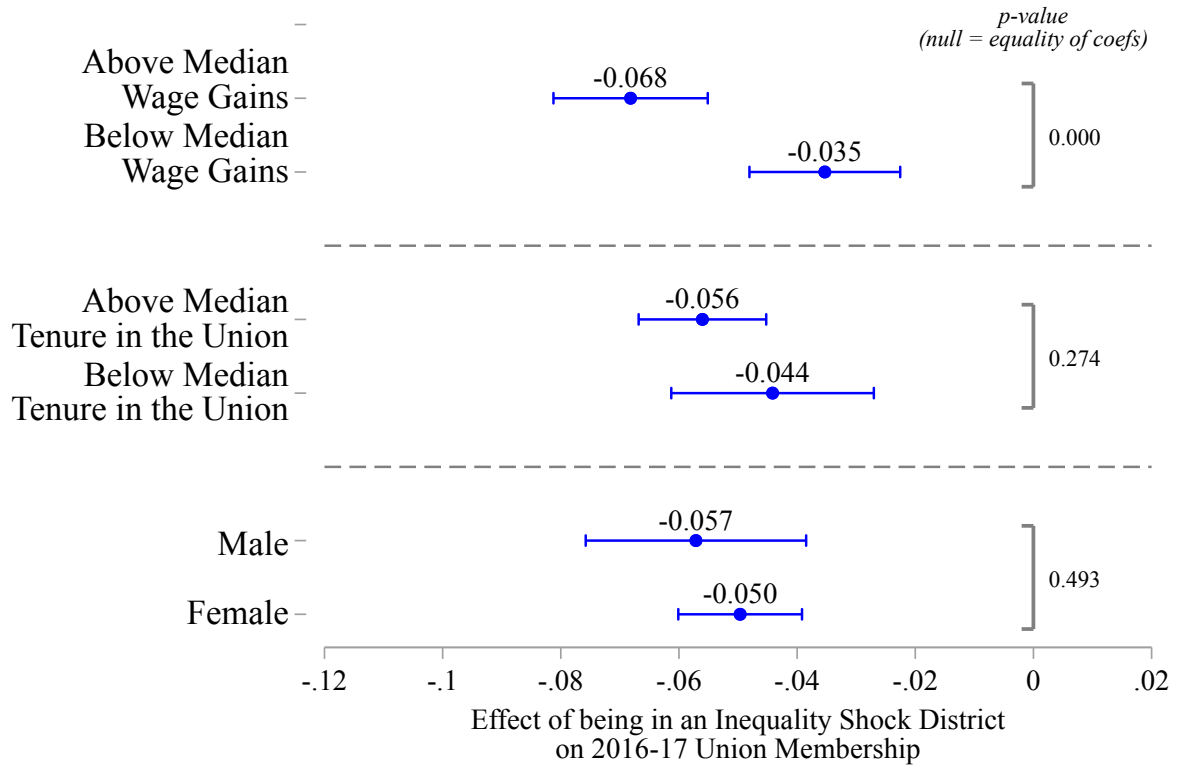
Notes: This figure explores cross-sectional relationships between wage inequality and union organizing strategies. Each panel plots a binscatter of a union outcome against industry-state-year log p90-p50 wage ratios. Panel A shows the pay scale status of 451 contracts between 2002–2022, using data from the Office of Labor-Management Standards (OLMS) Online Public Disclosure Room. Panel B shows the “pay-related” topic share (out of pay, benefits, and conditions) computed using the CorEx model in the same 451 contracts. Panel C shows the size of 79,822 bargaining units as a share of establishment-level employment, using Collective Bargaining Notice (F-7) data from the Federal Mediation and Conciliation Service (FMCS) between 2015–2021. Industry wage ratios (x-axis) are estimated using the best available data for each time range and aggregated at the BEA region \times 3-year level in Panels A and B, and state \times year level in Panel C (see Appendix E.1 for details). Standard errors are twoway clustered at the union and employer levels.

Figure V: Wage Inequality Shocks and Union Revenues



Notes: This figure shows how wage inequality affects union revenues at the school district level, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from equation (7), which regresses log annual union revenues per teacher on event-year dummies (relative to expiration of collective bargaining agreements) with district and year fixed effects. We obtain and display separate coefficients for districts that experience an *inequality shock* (blue series) and those that experience *no inequality shock* (orange series). We classify districts as having an inequality shock based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

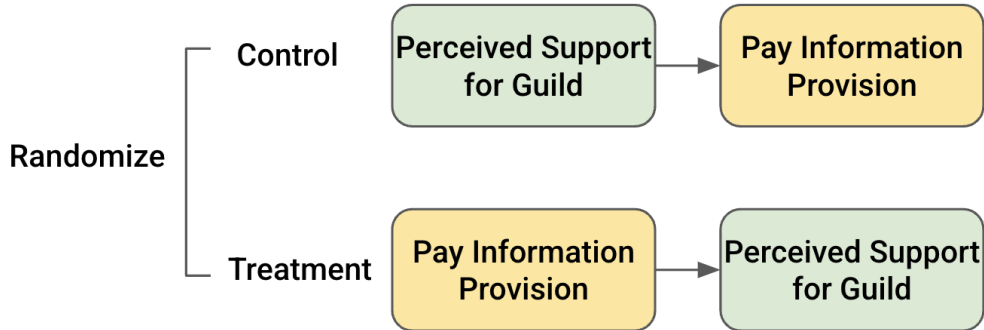
Figure VI: Wage Inequality and Union Membership: Bargaining Power vs. Other Demographics



Notes: This figure explores heterogeneity in the effect of wage inequality on union membership at the teacher level, from the Wisconsin study described in Section 4. Coefficients show the effect on union membership of working in inequality shock districts, defined as having a commuting zone level Herfindahl-Hirschman Index below the state median, relative to no-inequality shock districts. Respondents are grouped by three characteristics: (1) *Above Median Wage Gains* vs. *Below Median Wage Gains*, based on having above/below district-level median 2010–2016 changes in salaries; (2) *Above Median Tenure in the Union* vs. *Below Median Tenure in the Union*, based on having above/below the state median of 13 years of experience; (3) *Male* vs. *Female*. Each regression is estimated using membership data for 2016/17 and controls for the other two pictured characteristics not being directly tested as well as year fixed effects. Point estimates are shown with 95% confidence intervals, using robust standard errors. We test the null hypothesis that coefficients are equal across the split and report the difference-in-difference p-values to the right of the plot.

Figure VII: Follow-up Survey Pay Report and Design

PANEL A: Research Design



PANEL B: Pay Report

At present, our responses suggest the following compensation among WGA writers:

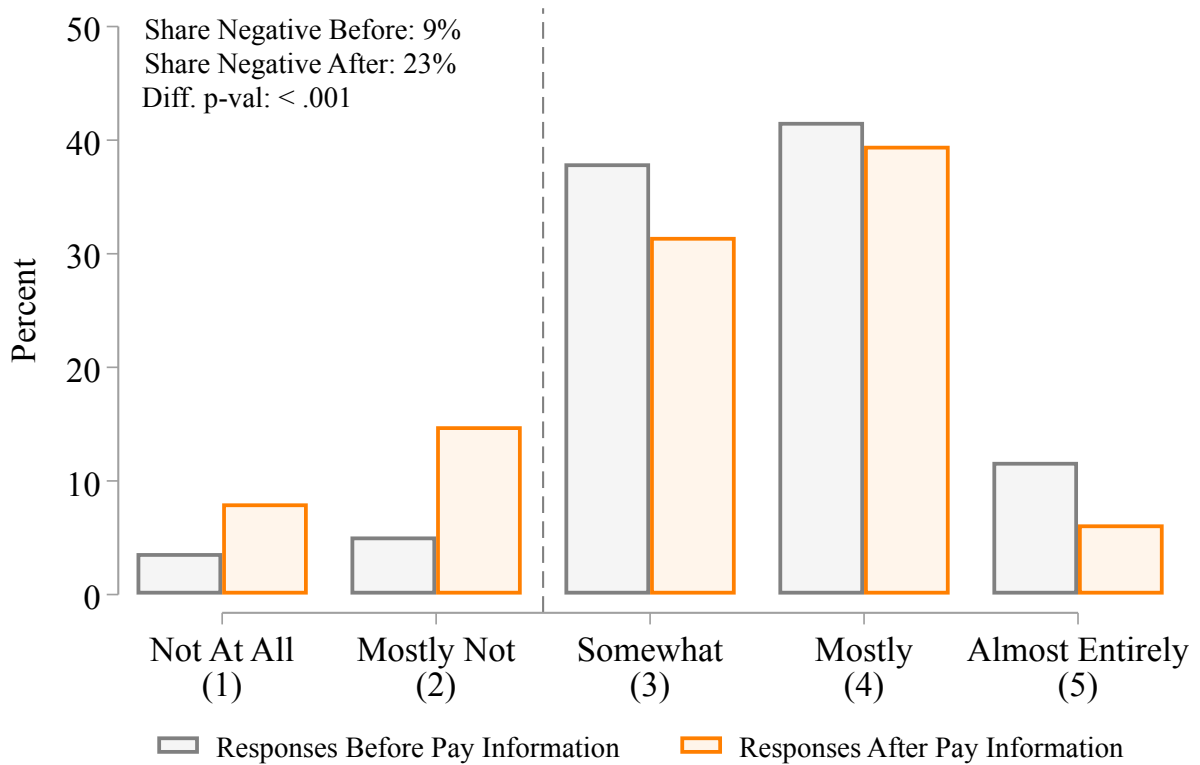
	Male	Female
Median	Scale + 10%	Scale + 3%
Mean	Scale + 25%	Scale + 14%
Maximum	Scale + 100+%	Scale + 100+%

Note: Standard errors on means are 2.2% for men and 2.0% for women.

We expect to add position-level statistics soon.

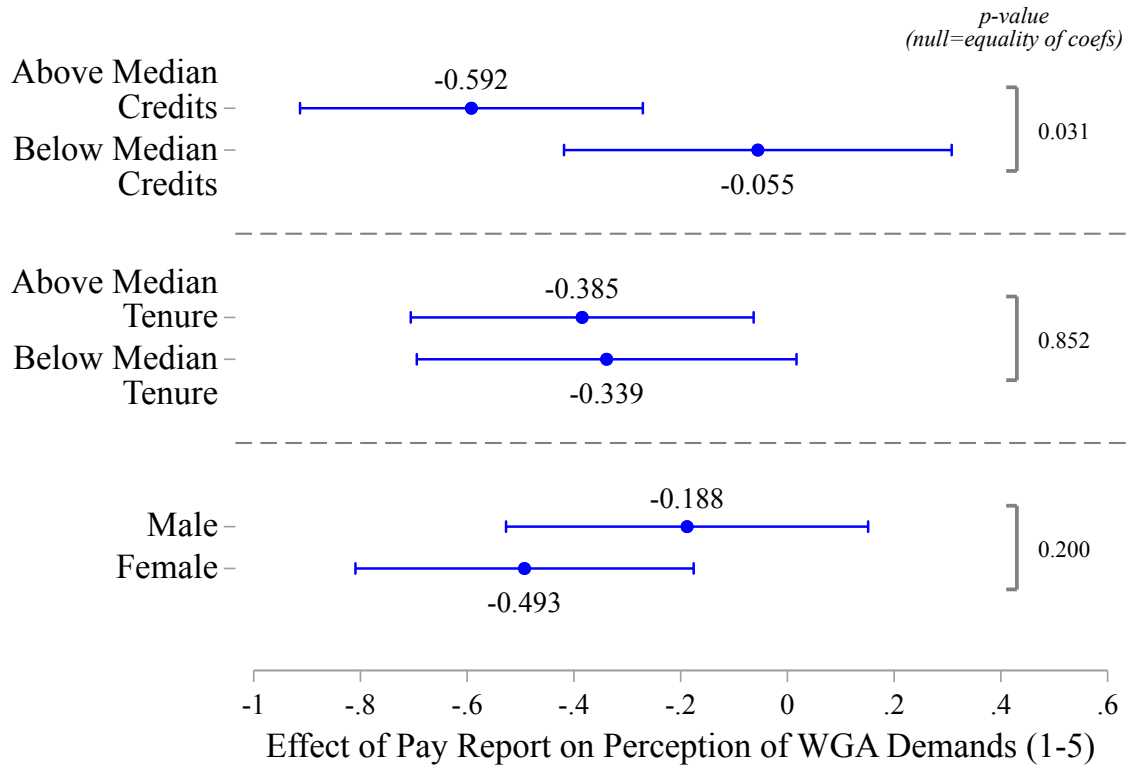
Notes: This figure illustrates the experimental design used to test how pay transparency affects union support among WGA writers, from the Hollywood experiment described in Section 5. Panel A sketches our research design, where we randomize the order in which respondents see pay information and express perceived support for the Guild. *Perceived Support* is measured by asking: “Do most writers think the WGA demands will meet the needs of all WGA members?” Panel B displays the pay report that respondents see in the follow-up (the content of the *Pay Information Provision*). We compute pay statistics based on self-reported pay among WGA members in the baseline survey.

Figure VIII: Writers' Perceptions of Whether WGA Demands Serve Everyone



Notes: This figure shows how pay transparency affects writers' perceptions of whether the WGA's demands serve all members, from the Hollywood experiment described in Section 5. On a 5-point scale, respondents answer the question "Do most writers think the WGA demands will meet the needs of all WGA members?" either before (gray bars) or after (orange bars) they saw the pay report in Figure VII. The shares of negative responses (defined as "Mostly not" or "Not at all") before vs. after seeing the pay report are displayed and their difference tested in the top left corner. Sample size is 299 WGA members. Of the 310 responses in our follow-up survey sample, 11 respondents report that they do not know the WGA demands and are excluded from the analysis.

Figure IX: Heterogeneity in Perception of WGA Demands



Notes: This figure explores heterogeneity in the effect of the pay report on writers’ perceptions of the WGA’s demands, from the Hollywood experiment described in Section 5. Coefficients report the effect of having seen the pay report on answers to the question “Do most writers think the WGA demands will meet the needs of all WGA members?”, using the 1-5 scale shown in Figure VIII (1 = Not at all, 5 = Almost entirely). The sample is 251 WGA members with available credit, experience, and gender data. Respondents are grouped by three characteristics: (1) *Above Median Credits* vs. *Below Median Credits* based on median IMDb credits among individuals from our WGA contact list with the same most recent credit title (e.g., “Story Editor”); (2) *Above Median Tenure* vs. *Below Median Tenure*, based on median years since first IMDb credit among individuals from our WGA contact list with the same most recent credit title; (3) *Male* vs. *Female*, self-reported or otherwise imputed based on first names as described in Section 5.2. Each regression controls for the other two characteristics, out of the three, that are not being tested. Point estimates are shown with 95% confidence intervals, using robust standard errors. We test the null hypothesis that coefficients are equal across groups and report the difference-in-difference p-values to the right of the plot.

Table I: Summary of Organizer Survey Results

	(1)	(2)	(3)	(4)
PANEL A: Choice of Scarce Resource Allocation				
Unequal Environment (=1)	0.33*** (0.03)			
PANEL B: Effect of Unequal Environment on Outcomes				
		<i>High OO</i>	<i>Medium OO</i>	<i>Low OO</i>
<i>Perceived Union Support</i>				
Median Vote Share	-0.09*** (0.02)			
<i>EE Median: 0.80</i>				
Mean Vote Share	-0.05*** (0.01)			
<i>EE Mean: 0.76</i>				
Hourly Wage Demands (\$)		6.00*** (0.38)	-0.01 (0.22)	-4.33*** (0.33)
Share Applying Elsewhere During Campaign		0.13*** (0.01)	0.02* (0.01)	-0.07*** (0.01)
Difficult to Agree on Payscale (=1)	0.47*** (0.04)			
<i>EE Mean: 0.37</i>				
<i>Pay Transparency</i>				
Publish During Campaign (=1)	-0.24*** (0.03)			
<i>EE Mean: 0.95</i>				
Publish After Campaign (=1)	0.10*** (0.03)			
<i>EE Mean: 0.02</i>				
Never Publish (=1)	0.13*** (0.03)			
<i>EE Mean: 0.03</i>				
<i>Campaign Priority</i>				
Raising Pay (=1)	-0.10*** (0.04)			
<i>EE Mean: 0.41</i>				
Predictable Hours (=1)	0.10** (0.04)			
<i>EE Mean: 0.52</i>				
Open to All Pledge (=1)	0.01 (0.02)			
<i>EE Mean: 0.07</i>				
<i>Bargaining Units</i>				
Smaller (=1)	0.15*** (0.03)			
<i>EE Mean: 0.09</i>				
<i>N</i>	182	182	182	182

Notes: This table summarizes results from the organizer survey. Significant at *10%, **5%, ***1%. In Panel A, we report the share of organizers who choose to allocate scarce resources to the unequal environment; the p-value corresponds to a two-sided test of the null hypothesis that the choice rate equals 0.50, or random allocation (Section 3.3). In Panel B, we show the effect of moving from the equal environment to the unequal environment on outcomes, with standard errors clustered at the organizer level in parentheses. Each estimate corresponds to a single regression. Column (1) reports general outcomes, while columns (2)-(4) report outcomes specific to high, medium, and low outside option workers. Outcomes on *Perceived Union Support* (Section 3.4; Figure II) include the predicted share of workers who vote in favor of ratifying a contract, expected hourly wage demands by position, the share of workers expected to apply elsewhere during the union campaign by position, and whether organizers expect it would be difficult to agree on a pay-scale. Remaining outcomes report choices about organizer strategies (Section 3.5; Figure III). *EE Mean* refers to the mean value in the equal environment.

Table II: Wage Inequality and Labor Market Concentration

	Std. dev., 2010	Change in std.dev, 2010-2016
	(1)	(2)
Low HHI (Inequality Shock)	-147.04 (714.86)	2896.42** (1455.42)
Mean dep. var. control	4242.42	-249.67
N (districts)	416	416

Notes: This table reports OLS estimates on the relationship between actual wage inequality and labor market concentration, from the Wisconsin study described in Section 4. The dependent variables are the standard deviation of wages within position in 2010, averaged at the district level (Column 1), and the change in this variable between 2010 and 2016 (Column 2). The independent variable *Low HHI (Inequality Shock)* equals one for districts in commuting zones with a Herfindahl-Hirschman Index below the state median. Positions are defined by district, education, and three-year experience bins. Each observation corresponds to a district and is weighted by the total number of workers. Robust standard errors are reported in parentheses.

Table III: District Characteristics and Labor Market Concentration

Panel (a)	2007-10 change		2010		2016	
	Salary	Benefits	Salary	Benefits	Salary	Benefits
	(1)	(2)	(3)	(4)	(5)	(6)
Inequality Shock	2910.37 (2013.45)	723.73 (1144.04)	1750.89 (1985.60)	546.11 (728.14)	-224.85 (281.32)	258.58 (815.36)
Mean dep. var. control N (districts)	7889.43 416	4764.47 416	52480.31 416	22586.07 416	50935.78 416	23086.52 416

Panel (b)	2010			2016		
	Experience	Master's	Female	Experience	Master's	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Inequality Shock	0.192 (0.390)	-0.018 (0.035)	-0.010 (0.007)	-1.337 (1.323)	-0.020 (0.024)	-0.008 (0.006)
Mean dep. var. control N (districts)	12.48 416	0.50 416	0.73 416	14.14 416	0.41 416	0.74 416

Panel (c)	City	Town	Suburb	Rural	ln(Popul)	Share GOP votes, 2012
	(1)	(2)	(3)	(4)	(5)	(6)
	Inequality Shock	0.080 (0.145)	-0.056 (0.066)	0.012 (0.084)	-0.037 (0.061)	0.323 (0.517)
Mean dep. var. control N (districts)	0.03 416	0.25 416	0.11 416	0.61 416	8.82 416	0.46 416

Notes: This table examines whether labor market concentration is systematically related to observable district characteristics, from the Wisconsin study described in Section 4. Each column reports the OLS estimate of a district characteristic on *Inequality Shock*, which equals one for districts in commuting zones with a Herfindahl-Hirschman Index below the state median. In Panel (a), the dependent variables are average 2007-2010 within-worker changes in salaries and benefits (health care, pension, and life insurance contributions made by the employer, Columns 1-2); average district salaries and benefits conditional on position in 2010 (Columns 3-4); and average district salaries and benefits conditional on position in 2016 (Columns 5-6). In Panel (b), the dependent variables are average teacher experience, share with Master's, and share female in 2010 (Columns 1-3), and the same variables measured in 2016 (Columns 4-6). In Panel (c), the dependent variables are indicators for districts located in a city (Column 1), town (Column 2), suburb (Column 3), or rural area (Column 4); log population (Column 5); and the share of votes for the GOP in the 2012 presidential election at the county level (Column 6). Each observation corresponds to a district and is weighted by the number of workers. Robust standard errors are reported in parentheses.

Online Appendix

A Derivation of Model Extension to Three Productivity Types and the Organizers' Choice of Bargaining Unit Size

This appendix provides a self-contained derivation of the equilibrium in the three-type extension and proves the key results referenced in Section 2.2.

A.1 Setup and Notation

Workers are indexed $j \in \{1, 2, 3\}$ with inside-firm productivities $\alpha_1 < \alpha_2 < \alpha_3$, workforce shares $\phi_1 > \phi_2 > \phi_3 > 0$, $\sum_j \phi_j = 1$, and $\phi_1 > \frac{1}{2}$. The outside option of type j is y_j^O , with $y_1^O = 0$ and $y_j^O = A_j \alpha_j - \bar{e}_j$ for $j \in \{2, 3\}$, $A_j > 1$. The wage contract is $w(y^C) = \gamma + \beta y^C$, $\beta \geq 0$, $\gamma \geq 0$.

A.2 Baseline Union $\{1, 2\}$

The union $\{1, 2\}$ covers types 1 and 2. Expected output within this coalition is

$$E y_{12}^C = \frac{\phi_1 \alpha_1 + \phi_2 \alpha_2}{\phi_1 + \phi_2}. \quad (8)$$

The firm's zero-profit condition binds, giving $\gamma = (1 - \beta) E y_{12}^C$. Since $E y_{12}^C > \alpha_1$ (type 1 is below average), the median voter's payoff $E y_{12}^C - \beta(E y_{12}^C - \alpha_1)$ is decreasing in β ; they minimize β subject to type 2's no-quitting condition. Substituting the binding zero-profit condition and solving:

$$\beta_{12}^* = \frac{A_2 \alpha_2 - \bar{e}_2 - E y_{12}^C}{\alpha_2 - E y_{12}^C} = 1 - \frac{\bar{e}_2 - (A_2 - 1) \alpha_2}{\frac{\phi_1}{\phi_1 + \phi_2} (\alpha_2 - \alpha_1)}. \quad (9)$$

Feasibility requires $\beta_{12}^* \leq 1$, i.e. $(A_2 - 1) \alpha_2 \leq \bar{e}_2$, equivalently $\alpha_2 \leq \alpha_2^* \equiv \bar{e}_2 / (A_2 - 1)$ —the direct analogue of $\eta \leq \eta^*$ in the baseline. Type 1's equilibrium payoff is

$$W_1^{12} = E y_{12}^C - \beta_{12}^* (E y_{12}^C - \alpha_1). \quad (10)$$

A.3 Expanded Union {1,2,3}

With all three types in the union, expected output is

$$Ey_{123}^C = \phi_1\alpha_1 + \phi_2\alpha_2 + \phi_3\alpha_3 > Ey_{12}^C, \quad (11)$$

where the inequality holds because $\alpha_3 > Ey_{12}^C$ (type 3 is above the two-type average). The zero-profit condition gives $\gamma = (1-\beta)Ey_{123}^C$. Substituting, the no-quitting condition for type $j \in \{2,3\}$ becomes

$$Ey_{123}^C + \beta(\alpha_j - Ey_{123}^C) \geq A_j\alpha_j - \bar{e}_j. \quad (12)$$

Provided $\alpha_j > Ey_{123}^C$ (type j is above the union average, which holds for $j = 3$ by construction and generically for $j = 2$ when $\phi_1 > \frac{1}{2}$ and ϕ_3 is small), the minimum piece rate required to retain type j is¹

$$\beta_j^*|_{123} = \frac{A_j\alpha_j - \bar{e}_j - Ey_{123}^C}{\alpha_j - Ey_{123}^C}. \quad (13)$$

The equilibrium piece rate is $\beta_{123}^* = \max(\beta_2^*|_{123}, \beta_3^*|_{123})$, and the expanded union is feasible if and only if $\beta_{123}^* \leq 1$.

A.4 Type 2's Constraint Is Relaxed in the Expanded Union

Proposition 1. *If the {1,2} union is feasible ($\beta_{12}^* \leq 1$), then $\beta_2^*|_{123} \leq \beta_{12}^*$.*

Proof. Let $N_2 \equiv A_2\alpha_2 - \bar{e}_2$, $\bar{E}_{12} \equiv Ey_{12}^C$, and $\bar{E}_{123} \equiv Ey_{123}^C$. Form the difference:

$$\begin{aligned} \beta_2^*|_{123} - \beta_{12}^* &= \frac{N_2 - \bar{E}_{123}}{\alpha_2 - \bar{E}_{123}} - \frac{N_2 - \bar{E}_{12}}{\alpha_2 - \bar{E}_{12}} \\ &= \frac{(\bar{E}_{123} - \bar{E}_{12})[(A_2 - 1)\alpha_2 - \bar{e}_2]}{(\alpha_2 - \bar{E}_{123})(\alpha_2 - \bar{E}_{12})}, \end{aligned} \quad (14)$$

¹If instead $\alpha_2 < Ey_{123}^C$ —which can arise when type 3 is sufficiently productive to pull the union average above type 2's inside-firm productivity—the coefficient $(\alpha_2 - Ey_{123}^C)$ in (12) is negative and the inequality reverses. Type 2's wage $(1-\beta)Ey_{123}^C + \beta\alpha_2$ is then *decreasing* in β : type 2 becomes a net beneficiary of compression, cross-subsidised by type 3 through the higher fixed component. Their no-quitting condition becomes the *upper* bound $\beta \leq [Ey_{123}^C - (A_2\alpha_2 - \bar{e}_2)] / (Ey_{123}^C - \alpha_2)$, which does not constrain the median voter (who minimises β regardless). Type 2's no-quitting condition therefore drops out of the problem entirely in this case.

where the second line follows by expanding and collecting terms (the $N_2\alpha_2$ and $\bar{E}_{12}\bar{E}_{123}$ terms cancel). The denominator is positive because $\alpha_2 > \bar{E}_{123} > \bar{E}_{12}$ under our maintained assumptions. The factor $(\bar{E}_{123} - \bar{E}_{12}) > 0$ because adding the productive type 3 raises average output. Feasibility of the $\{1,2\}$ union requires $(A_2 - 1)\alpha_2 \leq \bar{e}_2$, so the remaining numerator factor is non-positive. Therefore $\beta_2^*|_{123} \leq \beta_{12}^*$. \square

Intuition. Including type 3 raises Ey^C , which raises the fixed component $\gamma = (1 - \beta)Ey^C$ at any given β . Type 2 benefits directly from this higher fixed payment, so a lower piece rate suffices to satisfy their outside-option condition.

When type 2's constraint binds: unconditional expansion. Although relaxed by Proposition 1, type 2's constraint may still be tighter than type 3's when A_2 is large relative to A_3 , i.e. when condition (15) holds:

$$\frac{A_2\alpha_2 - \bar{e}_2 - Ey_{123}^C}{\alpha_2 - Ey_{123}^C} > \frac{A_3\alpha_3 - \bar{e}_3 - Ey_{123}^C}{\alpha_3 - Ey_{123}^C}. \quad (15)$$

In that case $\beta_{123}^* = \beta_2^*|_{123}$. Crucially, the organizer's decision is then trivial.

Proposition 2. *If $\beta_{123}^* = \beta_2^*|_{123}$ (type 2's no-quitting condition is binding in the expanded union), then $W_1^{123} > W_1^{12}$ unconditionally: the organizer always prefers to expand the bargaining unit.*

Proof. Write $\Delta E \equiv Ey_{123}^C - Ey_{12}^C > 0$ and $\Delta\beta \equiv \beta_2^*|_{123} - \beta_{12}^* < 0$ (strict inequality from Proposition 1 when $\beta_{12}^* < 1$). Then:

$$\begin{aligned} W_1^{123} - W_1^{12} &= [Ey_{123}^C - \beta_2^*|_{123}(Ey_{123}^C - \alpha_1)] - [Ey_{12}^C - \beta_{12}^*(Ey_{12}^C - \alpha_1)] \\ &= \Delta E(1 - \beta_{12}^*) + |\Delta\beta|(Ey_{123}^C - \alpha_1). \end{aligned} \quad (16)$$

Both terms are strictly positive: $\Delta E > 0$, $\beta_{12}^* < 1$ (feasibility), $|\Delta\beta| > 0$, and $Ey_{123}^C > \alpha_1$. Therefore $W_1^{123} > W_1^{12}$. \square

Intuition. When the intermediate type faces the more competitive outside market (A_2 large relative to A_3), including the top type simultaneously *raises* average output and *relaxes* the binding constraint. Both effects unambiguously benefit the median voter, so no threshold balancing gains against costs is needed. This highlights a distinct channel through which top-end productivity can strengthen, rather than erode, the viability of a broader bargaining unit: the top type cross-subsidises the fixed component, automatically

satisfying the intermediate type's outside-option condition without forcing the median voter to concede further wage dispersion.

A.5 The Organizer's Threshold

Type 1's payoff under the expanded union is

$$W_1^{123} = Ey_{123}^C - \beta_{123}^* (Ey_{123}^C - \alpha_1). \quad (17)$$

The organizer prefers the expanded union ($W_1^{123} \geq W_1^{12}$) if and only if condition (6) holds. Substituting β_{123}^* from (13) (taking $j=3$ as the binding constraint) and differentiating with respect to A_3 :

$$\frac{\partial \beta_{123}^* |_{123}}{\partial A_3} = \frac{\alpha_3}{\alpha_3 - Ey_{123}^C} > 0. \quad (18)$$

The right-hand side of (6) is therefore strictly increasing in A_3 , while the left-hand side is independent of A_3 . This guarantees a unique threshold \bar{A}_3 at which the organizer is indifferent. For $A_3 \leq \bar{A}_3$, expansion is preferred; for $A_3 > \bar{A}_3$, the organizer retains the $\{1,2\}$ union.

Comparative statics on \bar{A}_3 .

- *Share ϕ_3* : A larger share of type-3 workers raises Ey_{123}^C and thus the left-hand side of (6). At the same time it raises Ey_{123}^C in the denominator of β_3^* , reducing the required piece rate. Both effects favour expansion, so \bar{A}_3 is increasing in ϕ_3 (with off-setting reductions in ϕ_2): more numerous top workers make inclusion more attractive to the median voter.
- *Productivity α_3* : Higher α_3 raises the left-hand side of (6) (higher average output) but also raises $A_3\alpha_3$ (harder to retain type 3), affecting both sides. The net effect on \bar{A}_3 is ambiguous without further parametric restrictions.
- *Switching cost \bar{e}_3* : A higher switching cost for type 3 reduces the piece rate required to retain them (lower β_3^*), lowering the cost of expansion. Therefore \bar{A}_3 is increasing in \bar{e}_3 : more expensive mobility for type 3 makes them easier to include.

B Pay Misperceptions, Transparency and Organizer Choices

B.1 Mechanism 1: Anchoring on own wage for own outside option

In the organizer section, we introduce the idea that, when there are information frictions, organizers can act as *information intermediaries*, and whether or not to reveal information about pay becomes an organizing strategy. When inequality is greater than workers believe it to be, misperceptions can sustain support for a compressed contract. In particular, if high-productivity workers anchor perceptions of their outside options on their current pay (underestimating their outside options in our setting), the union would be able to maintain greater pooled resources for the median voter by reducing high types' perceived bargaining position. In this case, the organizer would have an incentive to withhold pay information rather than reveal it. This tension is especially sharp in high-inequality environments, where large outside-option differences make union failure more likely under full information. Our organizer survey evidence is consistent with this mechanism: when inequality is greater, organizers are less likely to disclose prevailing wages.

Mechanism 2: Anchoring on own wage and productivity for others

In the case of Hollywood, workers may anchor beliefs about others on themselves within the unionized sector. For example, high-type workers may infer that low-type productivity, α , is closer to high-type productivity, η , than it actually is, overestimating average productivity.

Let w_L and w_H denote the wages implied by the initial union contract given misperceptions. High types' misperception leads them to underestimate the size of the transfer implied by the median-voter demand. Define the per-high-type transfer $\tau(w_L; \alpha)$ as the reduction in each high type's wage below her inside productivity η that is required, under zero profits, to finance the gap between the low-type wage w_L and low-type productivity α . The zero-profit condition implies

$$\phi w_H + (1 - \phi) w_L = \phi \eta + (1 - \phi) \alpha, \quad (19)$$

and therefore

$$\tau(w_L; \alpha) \equiv \eta - w_H = \frac{1 - \phi}{\phi} (w_L - \alpha). \quad (20)$$

High types' misperception leads them to underestimate the size of the transfer implied by the

median-voter demand. Instead, they compute this transfer using $\tilde{\alpha}$, the perceived low-type productivity, and so infer a *smaller* transfer:

$$\tau(w_L; \tilde{\alpha}) - \tau(w_L; \alpha) = -\frac{1-\phi}{\phi}(\tilde{\alpha} - \alpha) < 0. \quad (21)$$

The actual per-high-type transfer exceeds what they anticipated by $(1-\phi)(\tilde{\alpha} - \alpha)/\phi$. When high types learn the true α , they revise the implied transfer upward. If the corrected high-type wage lies below their outside option $A\eta - e$, they exit. An organizer may therefore choose not to correct misperceptions.

C Earnings Inequality Within Occupations

C.1 Decomposing Earnings Inequality Over Time: Roles of Industries and Occupations

In this paper, we study how inequality among workers in similar industries, occupations, or firms can affect unionization outcomes. In this section, we situate this inquiry within the broader context of rising U.S. income inequality over recent decades by tracing inequality evolution along both industry and occupation layers using publicly available survey data. For firm-level inequality [Song et al. \(2019\)](#) and [Haltiwanger et al. \(2024\)](#) provide decompositions running between the late 1970s through the 2010s.

We explore this through a within- vs. between-group decomposition of earnings (annual labor income) inequality, using microdata from the March Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). To minimize confounding by compositional changes, we follow conventions in the inequality literature and focus on a group of full-year non-self-employed civilian male workers with age between 20–65 and earnings over half the full-time, full-year minimum wage.² Due to changes in how ASEC handles top earnings over time, we further winsorize earnings above the 99th percentile as in [Hoffmann et al. \(2020\)](#). In [Figure A.1](#), we decompose the variance of log earnings (Y) within vs. between categories using the following identity, first along industries (Panel A), then along occupations (Panel B), and finally along interacted industry-occupation cells (Panel C).³

$$\mathbf{Var}[Y | \text{Year}=t] = \underbrace{\mathbf{E}[\mathbf{Var}[Y | \text{Group}, \text{Year}=t]]}_{\text{within-group variance}} + \underbrace{\mathbf{Var}[\mathbf{E}[Y | \text{Group}, \text{Year}=t]]}_{\text{between-group variance}} \quad (22)$$

²This minimum wage-based earnings threshold is used instead of a full-time filter due to inconsistencies in the reporting of hours over time.

³Panel A uses 3-digit 1990 Census Industry Codes with 236 distinct industries; Panel B uses 4-digit 2010 Census Occupational Classification definitions with 453 distinct occupations; Finally, Panel C uses cells formed by corresponding 2-digit industry and occupation codes, with around 2,000 cells per year (the precise number fluctuates slightly over year as cells with fewer than 2 workers are dropped from analysis.)

Figure A.1: Earnings Inequality Decomposition within and between Industries and Occupations



Notes: This figure presents the decomposition of the variance of log annual earnings into within and between components along occupation and industry boundaries. The source is authors' calculations based on microdata from the March Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). The sample includes full-year non-self-employed civilian male workers with age between 20 and 65 and earnings over half the full-time, full-year minimum wage. Panel A shows the decomposition using 236 industries from the 1990 Census Industry Codes. Panel B shows the decomposition using 453 occupations from the 2010 Census Occupational Codes. Panel C shows the decomposition using around 2000 cells formed by industry and occupation codes at coarser, 2-digit levels (the precise number of cells fluctuate slightly over year as cells with fewer than 2 workers are dropped from analysis).

D Additional Details on Study I: Organizer Survey

D.1 Unrestricted Sample of Respondents

Our main sample of organizers includes only responses where the organizer left a valid email address at the end of the survey (N=182, Section 3.1). This restriction aims to ensure responses are from real organizers and are from our intended respondents. We investigate the consequences of this restriction and replicate our key results using the unrestricted sample of respondents (N=221). We find a similar gap in the median predicted vote share (8pp, $p < 0.001$). The effects of the unequal environment on worker wage demands and exit are virtually identical in both samples (Appendix Figure D.1). Additionally, the effects of the unequal environment on organizing strategies are generally consistent with baseline results for decisions around pay transparency, campaign priorities, and bargaining unit size, although exact magnitudes vary (Appendix Figure D.2). In the unrestricted sample, 69% of respondents prefer to allocate resources to the equal environment, compared to 67% in the restricted sample.

D.2 Robustness to Vignette Order

We present organizers with questions about the equal and unequal environments in a random order. Here, we report results from the main sample separately for those who saw the equal environment first versus those who saw the unequal environment first, in order to confirm that the order in which organizers see vignettes does not drive our results. We find that the median predicted vote share is significantly lower in the unequal environment regardless of order. Those who saw the equal environment first predict 5 pp lower support ($p\text{-value} = 0.036$) in the unequal environment, while those who saw the unequal environment first predict a 9 pp decline ($p\text{-value} = 0.001$). Results about worker demands, worker exit, and organizing strategies are directionally similar to the main results (Figures D.3 and D.4). The coefficients in the sample of respondents who saw the equal environment first are never statistically different from those in the sample of respondents who saw the unequal environment first, with the exception of the effect of the unequal environment on publishing pay information after the campaign (difference $p\text{-value} = 0.061$). However, we find similar negative effects of the unequal environment on organizers' decision to publish pay information during the campaign: the $p\text{-value}$ of the difference in the combined coefficient on publishing pay information *either* after the campaign or never, as opposed to during the campaign, is 0.186.

Additionally, the magnitude of the shift in campaign priorities away from raising pay is larger among those who saw the unequal environment first (-14.6pp) than those who saw the equal environment first (-6.5pp), but not statistically different between the two groups (difference p-value = 0.302). Finally, shares choosing to allocate resources to the equal environment are 67% regardless of which vignette was seen first.

D.3 Recruitment Materials

We sent the following message for survey recruitment starting on July 2, 2024:

Dear [first name],

We are surveying labor organizers. As a token of appreciation, we offer \$30 to survey takers in the form of a gift card. The goal of the survey is to better understand the barriers unions face when organizing a workplace (link below). We would be super appreciative if you could take 10 minutes to complete this - we don't want to lose your voice!

Here is the link to the survey: [personalized link]

We will share with respondents our aggregate insights. Participation in the survey will be anonymous and answers will be aggregated.

Important note on the project: We do not ask any questions on your strategies related to specific campaigns and we never ask organizers which campaigns they worked on. We take very seriously how important it is to keep organizing tactics at a given workplace private. The research is also not funded by any external grant agency: we are using our personal research fund to compensate organizers for their time.

Table D.1: Organizer Summary Statistics

	(1)
<i>National Unions (%)</i>	
AFL-CIO	24.2 (3.2)
IATSE	9.3 (2.2)
SEIU	9.9 (2.2)
<i>Industry (2-digit NAICS) (%)</i>	
Educational Services	14.8 (2.6)
Health Care and Social Assistance	14.3 (2.6)
Transportation and Warehousing	12.1 (2.4)
<i>State (%)</i>	
CA	11.5 (2.4)
MA	7.7 (2.0)
NY	8.2 (2.0)
<i>Individual Characteristics</i>	
Lead Organizer (%)	66.5 (3.5)
Average Years Experience	6.66 (0.26)
<i>Most Recent Organizing Experience</i>	
New Union (vs. Existing) (%)	58.2 (3.7)
Average Year	2022 (0.34)
<i>N Organizers</i>	182

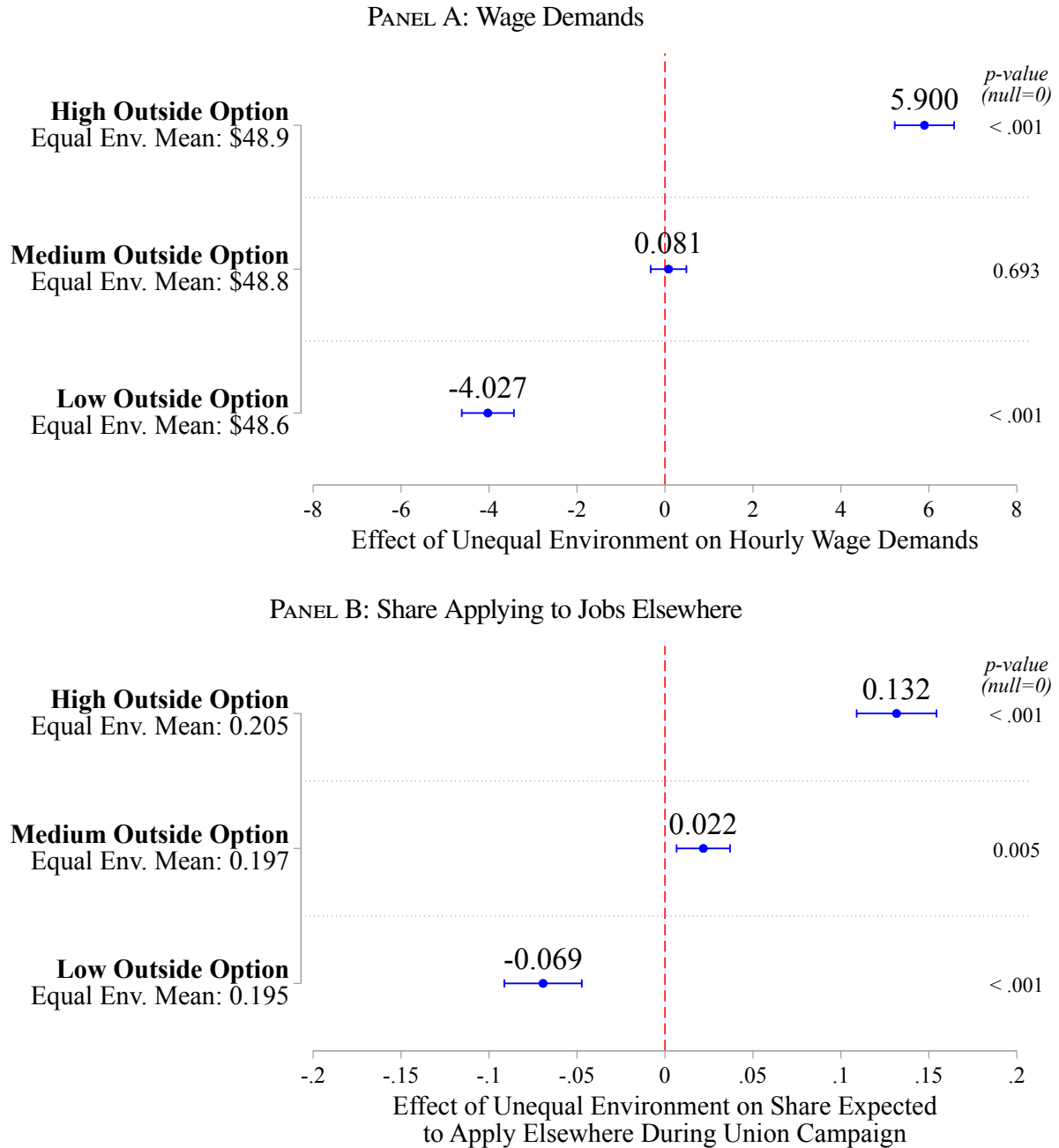
Notes: This table provides summary statistics about our sample of organizers, from the organizer survey described in Section 3. For three categories (union, industry, state), we report the share of organizers in each of the top 3 most common groups. We also report two individual characteristics of organizers: whether they have lead organizing experience and their average years of experience (responses are topcoded at 10 years). Finally, we report two characteristics regarding each organizers' most recent organizing experience: whether they were organizing a new union or an existing one, and the average year of the most recent organizing experience.

Table D.2: Organizer Respondent Composition by National Union

National Union	(1) Respondent %	(2) Contact %	(3) National %
SEIU	11.3	5.4	13.6
IATSE	10.7	16.6	1.2
Unite Here	8.2	6.5	2.1
CWA	5.7	1.7	4.6
TWU	5.0	7.5	1.1
UFCW	4.4	1.5	8.4
AFT	3.1	1.5	12.6
OPEIU	3.1	1.2	0.7
AFSCME	3.1	1.5	9.0
IAM	2.5	1.1	3.8
ATU	2.5	6.7	1.4
IBT	1.9	2.0	8.7
SMART	1.3	0.5	1.4
IBEW	1.3	1.6	5.0
NEA	1.3	0.6	19.9
LIUNA	0.6	0.5	4.1
UAW	0.6	1.6	2.6
IUOE	0.6	0.5	2.9

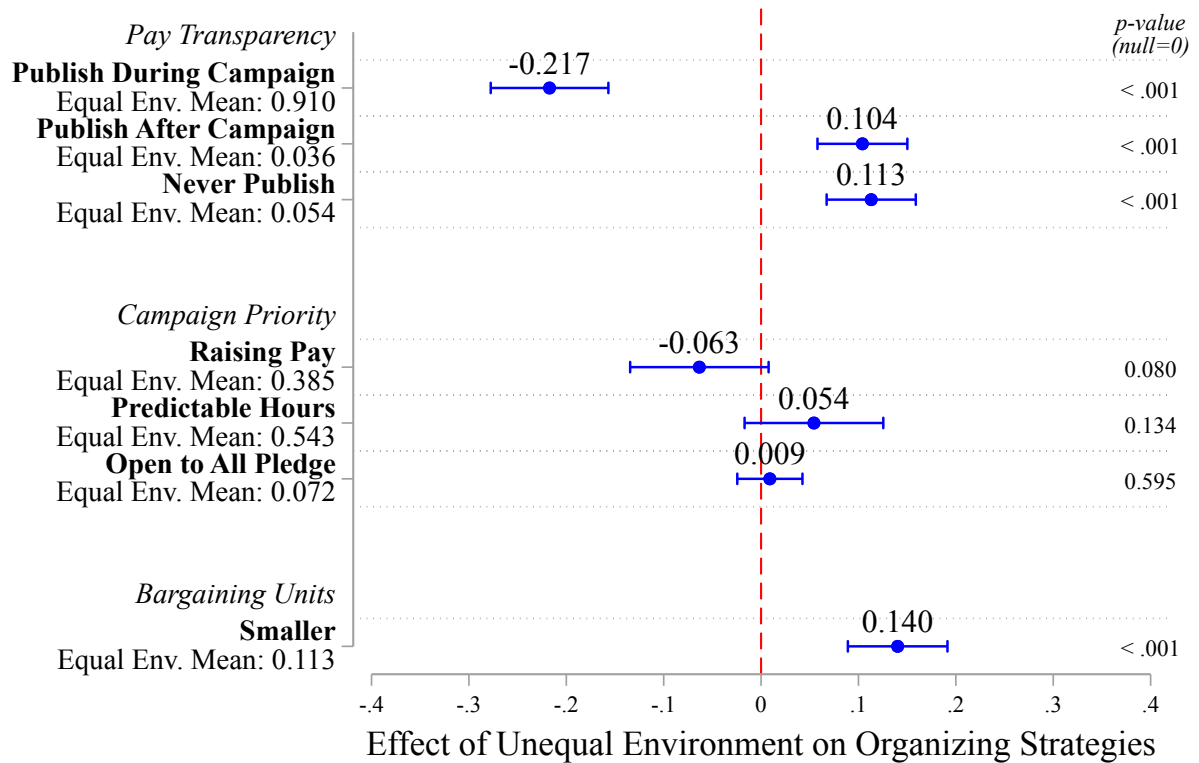
Notes: This table provides summary statistics of the national union organizations from which we had at least 10 contacts for the organizer survey in Section 3. Columns (1)-(2) show the share of respondents and contacts from each organization, respectively. Column (3) reports membership from each organization’s 2024 Office of Labor-Management Standards LM-2 filing, scaled by the BLS estimate that 14.3 million U.S. wage and salary workers were union members in 2024 (BLS, 2025). This statistic should be interpreted as an approximate size benchmark rather than a population share: LM-2 membership is reported using membership categories tracked by each labor organization, while the BLS denominator is restricted to employed U.S. wage and salary workers. As a result, the percentages in Column (3) need not sum to 100. AFL-CIO is excluded from the table, given many other national unions are AFL-CIO affiliated and we cannot distinguish between primary and secondary affiliations in the OLMS data. We identified 27.7% of our respondents and 34.2% of our contacts as having primary affiliation with the AFL-CIO.

Figure D.1: Unrestricted Sample of Organizers: Worker Actions by Outside Option



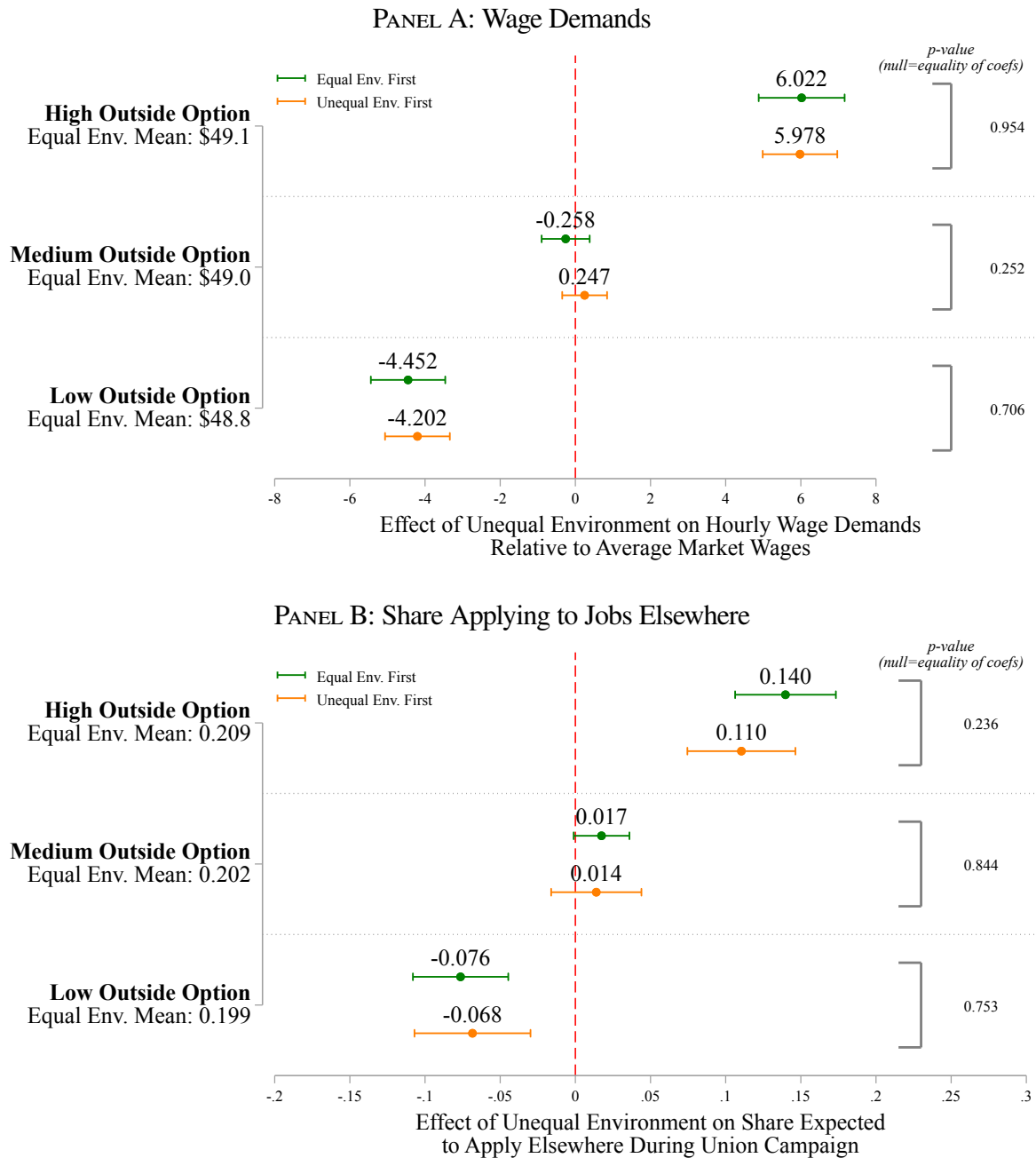
Notes: This figure replicates Figure II using the unrestricted sample of 221 organizers. It shows how predicted worker actions vary across workers with different outside options when market wage inequality increases, from the organizer survey described in Section 3. In Panel A, *Equal Env. Mean* report the average predicted wage demands by group in the equal environment. In Panel B, *Equal Env. Mean* report the average share of workers, by group, that organizers expect to apply for jobs elsewhere during the union campaign in the equal environment. Estimates show the effect of moving from the equal environment to the unequal environment on each outcome by group. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that each coefficient equals zero and report p-values to the right of the plots.

Figure D.2: Unrestricted Sample of Organizers: Organizing Strategies



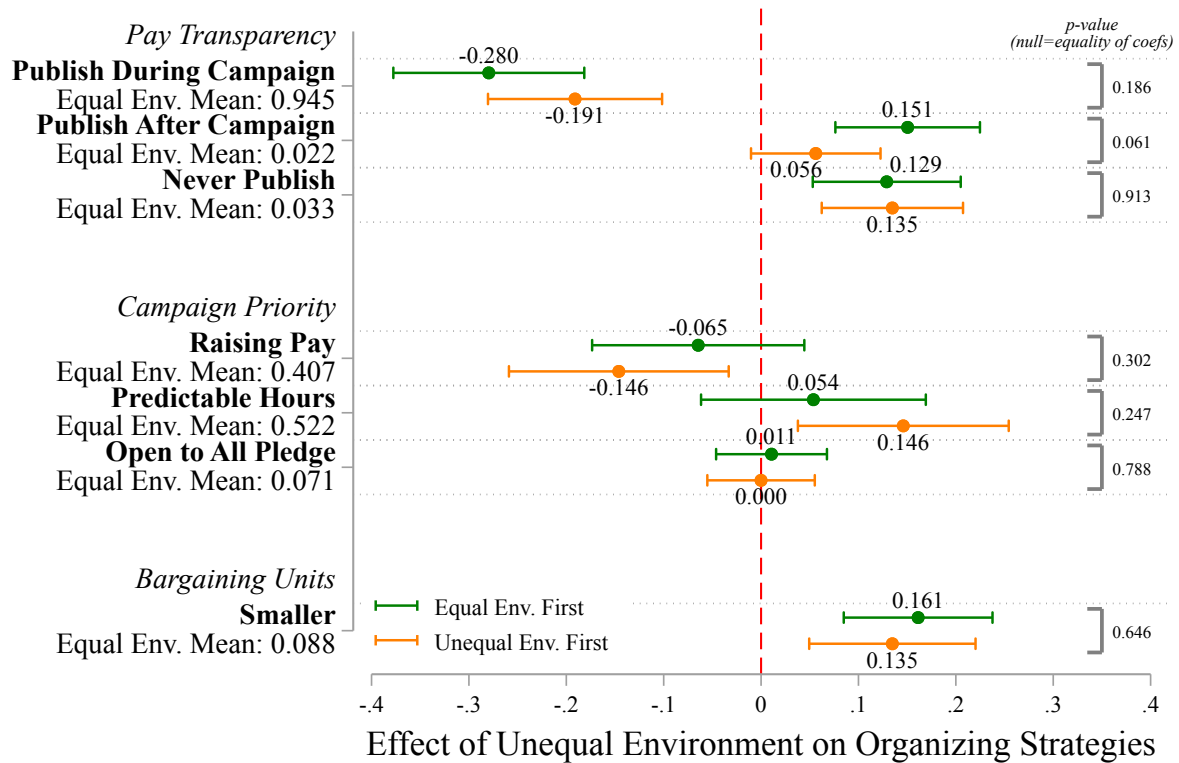
Notes: This figure replicates Figure III using the unrestricted sample of 221 organizers. It shows how organizers adapt their campaign strategies when market wage inequality increases, from the organizer survey described in Section 3. Under each outcome specified on the y-axis, *Equal Env. Mean* reports the average outcome in the equal environment, while plotted estimates show the effect of moving from the equal environment to the unequal environment. Specifically, coefficients under header *Pay Transparency* report whether organizers would publish market wage information from Figure I; coefficients under header *Campaign Priority* report which of the three issues organizers would prioritize during the campaign; the coefficient under header *Bargaining Units* reports whether organizers would pursue separate, smaller bargaining units for different worker types. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that each coefficient equals zero and report p-values to the right of the plot.

Figure D.3: Results by Vignette Order: Worker Actions by Outside Option



Notes: This figure replicates Figure II, split by which vignette organizers see first. It shows how predicted worker actions vary across workers with different outside options when market wage inequality increases, from the organizer survey described in Section 3. In Panel A, *Equal Env. Mean* report the average predicted wage demands by group in the equal environment. In Panel B, *Equal Env. Mean* report the average share of workers, by group, that organizers expect to apply for jobs elsewhere during the union campaign in the equal environment. Estimates show the effect of moving from the equal environment to the unequal environment on each outcome by group, split by vignette order. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that coefficients are equal between the two samples and report p-values to the right of the plots. Sample size is 182 organizers.

Figure D.4: Results by Vignette Order: Organizing Strategies



Notes: This figure replicates Figure III, split by which vignette organizers see first. It shows how organizers adapt their campaign strategies when market wage inequality increases, from the organizer survey described in Section 3. Under each outcome specified on the y-axis, *Equal Env. Mean* reports the average outcome in the equal environment, while plotted estimates show the effect of moving from the equal environment to the unequal environment, split by vignette order. Specifically, coefficients under header *Pay Transparency* report whether organizers would publish market wage information from Figure I; coefficients under header *Campaign Priority* report which of the three issues organizers would prioritize during the campaign; the coefficient under header *Bargaining Units* reports whether organizers would pursue separate, smaller bargaining units for different worker types. Point estimates are shown with 95% confidence intervals, using standard errors clustered at the organizer level. We test the null hypothesis that coefficients are equal between the two samples and report p-values to the right of the plots. Sample size is 182 organizers.

E Evidence on the Labor Movement from National Administrative Datasets

E.1 Data Details for Cross-Sectional Analysis

We present findings on the cross-sectional relationship between within-industry wage inequality and unionization outcomes at the economy-wide level. Section 3.6 discusses some of the key variables involved as well as qualitative interpretations of results; here we discuss in greater detail the data sources and steps involved in the cross-sectional analysis. While ideally inequality would be measured at the occupation level to align with our main analyses, our datasets on unionization lack occupational identifiers but include information on the employer’s industry. Therefore, we map each union contract or unionized establishment to its corresponding industry cell and study the relationship between unionization outcomes and wage inequality within that cell.

The construction of outcome variables in Figures IV and E.1 have been discussed in Section 3.6. In short, Panel A and B of both figures analyze union contracts (formally “collective bargaining agreements”) from the Office of Labor-Management Standards (OLMS) Online Disclosure Room database. The database contains 3,007 private-sector contracts. To match the availability of ACS data used to estimate wage ratios, we focus on 602 contracts with a start year between 2002-2022, of which 451 contain valid industry and state information. In Panel A, we define a fixed pay scale as one that explicitly sets pay levels for each position and level of experience. Using this definition, we classify each contract based on whether it includes such a scale. In Panel B, we break contracts into 100-word segments and assign textual topics for each segment using the Anchored Correlation Explanation (CorEx) model. CorEx is a semi-supervised topic model that allows the researcher to input topic-specific “anchor words” guiding the model to identify coherent topics of interest. We find the numbers of contract segments that discuss “pay-related”, “benefits-related”, and “(working) conditions-related” topics and calculate the relative shares of each topic. We implement the model with 10 topics (3 main and 7 residuals) and anchor strength 5. The anchor words are “pay, salary, wage, pay raise, wage increase” for pay, “benefits, insurance, pension, retirement, vacation” for benefits, and “hours, safety, workplace, injury, hazards” for conditions. The anchor words are initialization inputs to the model and are not meant to be exhaustive for each topic. In Panel C, we track sizes of bargaining units using Collective Bargaining Notice (F-7) data from the Federal Mediation and Conciliation Service (FMCS). The dataset covers 79,822 establishment-level bargaining units between

2015-2021 and contains information on both the size of the bargaining unit and the size of the establishment. For each unit, we calculate a “coverage rate” equal to the unit size divided by the establishment size.

The OLMS contract data, the FMCS F-7 bargaining data, and the NLRB election data all contain industry identifiers of the employer or establishment, allowing us to map each contract or establishment to wage inequality in a particular industry cell. We define industry at the 2-digit North American Industry Classification System (NAICS) level. Our overarching goal is to estimate wage inequality (in the form of log p90-p50 ratio) at the industry \times state \times year level using the most reliable, available survey data for the time period in question, while striking a balance between precision and noise when deciding the level of aggregation. As a result, individual panels of Figures IV and E.1 use somewhat different data sources and cell definitions, the rationales for which are discussed below.

Panel A and B of Figure IV estimate wage ratios using microdata from the American Community Survey (ACS) between 2002 and 2022. Individuals report annual labor earnings, weeks worked, and usual weekly hours in the ACS; however, weeks worked are reported in bins in some years, precluding a reliable calculation of hourly wage. Instead, we use the annual labor earnings among non-self-employed workers with at least 48 weeks worked and at least 30 hours per week (roughly “full-time, full-year workers”) as a proxy. To reduce noise from limited sample size, we estimate the log p90-p50 ratio at the 2-digit NAICS \times 3-year \times Bureau of Economic Analysis (BEA) region level.

Panel C of Figure IV makes use of industry \times state \times year-level wage estimates from the Occupation Employment and Wage Statistics (OEWS) data, which is based on surveys of roughly half of all establishments in the U.S. The OEWS is far more precise than the ACS, but state-industry estimates are only available after 2012 (so it is not used in Panel A and B). OEWS estimates include both the 90th and the 50th percentile of hourly wages, allowing for a direct calculation of the log p90-p50 ratio.

Finally, to address the concern that the wage ratios used in Figure IV may be affected by the presence of unionized workers, Figure E.1 uses CPS microdata, which contains information on union membership, to estimate wage ratios after subtracting industry-specific union premia. The union premium is estimated by regressing, for each 3-year period, log wages on age, age squared, and state, sex, education, industry, and occupation fixed effects, plus industry-specific union membership dummies. The rest of the CPS data construction follows the discussion for Figure E.3 below.

E.2 Data Details for Longitudinal Analysis

We present findings on the relationship between economy-wide inequality and the focus of union campaigns over time. Section 3.6 describes the analysis in general and provides a qualitative interpretation of results; here we discuss in greater detail the steps involved.

To obtain the topic shares used in Figure E.2, we collect and analyze text from the AFL-CIO News, the official newspaper of the national AFL-CIO union federation between 1955 and 1996. We break news articles into 200-word segments and assign textual topics for each segment using the CorEx model with the same anchor words and anchor strength as in our contract analysis. In other words, we run the model with 10 topics (3 main and 7 residuals) and anchor strength 5. The anchor words for the 3 main topics are “pay, salary, wage, pay raise, wage increase” for pay, “benefits, insurance, pension, retirement, vacation” for benefits, and “hours, safety, workplace, injury, hazards” for (working) conditions. (Note the anchor words are initialization inputs to the model and are not meant to be exhaustive for each topic.) Using the model output, we find the number of news segments each year that discuss “pay-related”, “benefits-related”, and “conditions-related” topics and calculate the relative shares of each topic out of the sum of the three. Finally, Panel A of Figure E.2 plots the pay-related topic share against two aggregate inequality measures: the top 10% income share and the Gini coefficient. We use these measures of inequality to mirror those in Farber et al. (2021). The numbers themselves are taken from the replication package that paper.

E.3 Additional Results on Cross-sectional Inequality and Union Density

Here we present patterns in the relationship between inequality and union density cross-sectionally that shed light on causal channels at play. Our theoretical framework predicts that settings with higher wage inequality should exhibit lower union support, particularly among high-wage workers. The selection out of unionization by high-wage earners is one marker of a causal relationship between high individual bargaining power and lower participation in unions.

Figure E.3 begins by examining aggregate union membership patterns (not split by wage). Panel A plots the standard “stock measure”: overall union membership rate against p90–p50 wage ratios, for each industry \times region \times 3-year cell in the Current Population Survey (CPS) between 1984 and 2019.⁴

⁴In the CPS, we use either directly reported wages for hourly workers or salaries divided by hours worked for salaried workers. Like ACS wage ratios in Figure IV, we estimate the log p90-p50 ratio at the 2-digit NAICS \times 3-year \times Bureau of Economic Analysis (BEA) region level to reduce noise. Since the CPS has even smaller sample sizes than the ACS, we

The relationship is highly negative, suggesting that overall union membership rates are lower in more unequal environments. In panel B, we construct a “flow” variable that better captures union formation dynamics, using NLRB election data compiled by [Sojourner and Yang \(2022\)](#) (1977–1999) and [Ferguson \(2018\)](#) (1999–2009). For each cell of industry \times region over a three-year period, we calculate *the share of workers who are covered by union victories, out of the overall employment in that cell*.⁵ This allows us to measure the “instantaneous thrust” of union activities and, unlike win rates among called elections, accounts for the endogeneity of election calls. Panel B shows that victorious union elections, over each 3-year period, also cover a smaller share of the industry-wide workforce in more unequal environments.

Figure [E.4](#) and [E.5](#) then explores potentially heterogeneous impact of wage inequality on union membership between high- and low-wage workers. Figure [E.4](#) divides workers in the CPS into 20 yearly wage ventiles (5-percent bins) and estimates union membership rates within each ventile, for five 7-year periods between 1984 and 2019.⁶ In each period, union membership rates increase with wage and peak between the 70th–90th percentiles, before dropping steeply among workers in the top ten percent. Notably, this *membership-wage gradient* flattens substantially between 1984–2019, suggesting union membership decline is driven disproportionately by middle-to-higher-wage exit. Panel B divides workers by residual wages, after controlling for detailed worker-level observables. Panels C and D repeat the analysis among private-sector and public-sector workers, respectively. Across four panels, we find consistent descriptive evidence that union membership decline is greater among middle-to-higher-wage workers, consistent with our predictions and also our empirical findings in the Wisconsin and Hollywood setting.⁷

Finally, Figure [E.5](#) provides industry-level evidence. Panel A plots union membership gap between high- and low-wage workers (defined using the median wage in each cell) against p90–p50 wage ratios, for each industry \times region \times 3-year cell in the CPS between 1984 and 2019.⁸ The relationship is strongly negative. Pan-

also merge several tiny industry cells (NAICS 21 and 22 into 11, 53 into 52, and 72 into 71). Wage ratios are calculated after subtracting cell-level union premium estimates to address potential effect of unionization on wage ratios. Finally, the proportion of topcoded wages is much higher in the CPS and often exceeds 10% in some industry cells. To address this issue, we impute log p90-p50 ratios for these cells (with topcoding rates over 10%) by calculating their log p80-p50 ratios and then adding the average difference between log p90-p50 and log p80-p50 ratios among all remaining cells.

⁵To calculate the number of workers covered by union victories, we sum the number of workers who are “eligible voters” associated with victorious election events, across all establishments in each industry \times region \times 3-year cell. To measure overall employment, we use our CPS sample (non-self-employed civilian workers aged 20–65 who work at least 20 hours per week with an hourly wage equivalent above one-half of the federal minimum wage), averaging over a three-year period and applying survey weights to scale the Current Population Survey (CPS) sample to the full economy.

⁶1984 is the first year CPS consistently asked all respondents about union membership.

⁷Notably, among workers above the 50th percentile, union membership decline slows after 2000 as “within” inequality measures stabilize, but this deceleration is not seen among workers below the 50th percentile.

⁸Wage ratio construction follows the same procedure as in Figure [E.3](#).

els B further examines correlated movements by long differencing x-axis and y-axis measures between 1984–2019 for each industry cell. The negative relationship suggests that industries experiencing greater inequality growth in this period also saw more high-wage union membership erosion, as our model and findings predict.

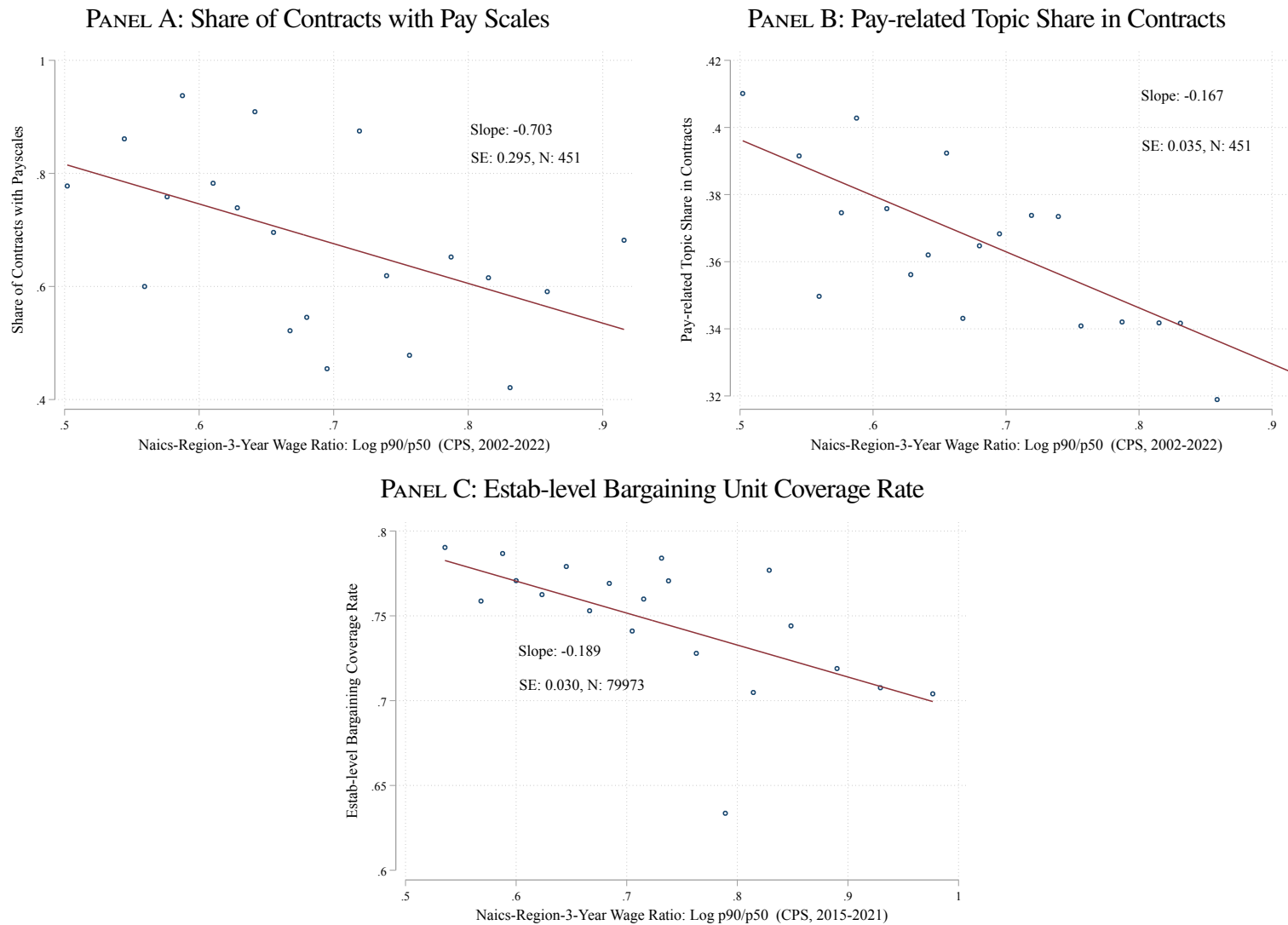
Figure E.4 and Figure E.5 are complementary evidence with slightly different assumptions. Figure E.4, Panel A assumes that historical forces of union decline (other than rising inequality) should have impacted low- and high-wage workers similarly, and Panel B relaxes this condition to hold after conditioning on worker-level observables. Figure E.5, Panel B assumes that industries with more vs. less historical inequality growth are similar in terms of historical forces of union decline (other than rising inequality) that could have led to extra high-wage union decline.

E.4 Data Details for Cross-Country Analysis

We present analysis on the cross-country relationship between union membership and within-occupation inequality in Figure E.6. Here we present additional details on the construction of this figure. We use data from the International Social Survey Programme (ISSP) Work Orientations Survey. We include survey waves II-IV (1997, 2005, and 2015) but exclude Wave I as it measures income differently and includes few countries. We use the crosswalk from Humlum (2021) to crosswalk the ISCO88 codes used in waves II and III to the ISCO08 codes used in wave IV. The ISSP provides a unique opportunity to look at measures of union membership, occupation, and income within the same survey.

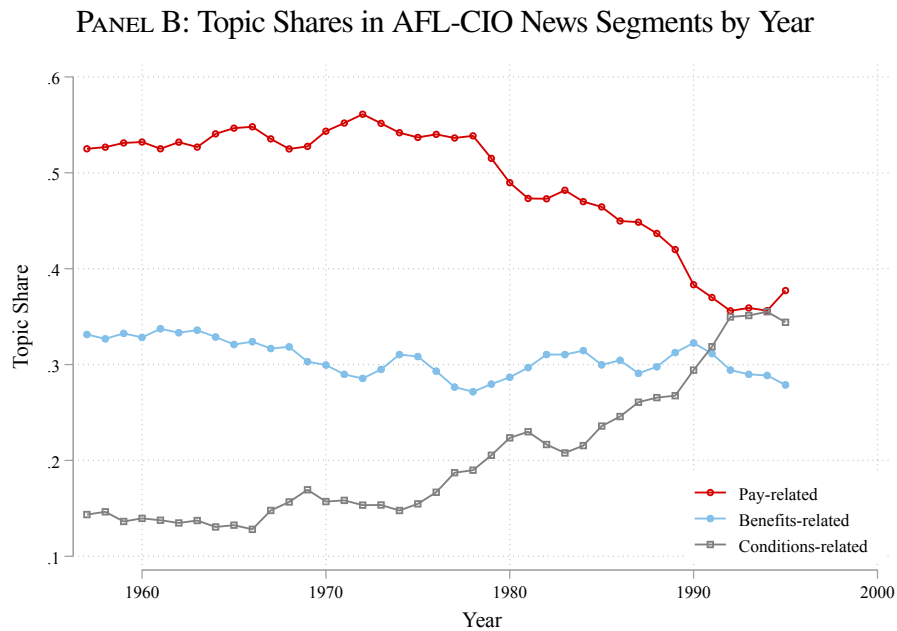
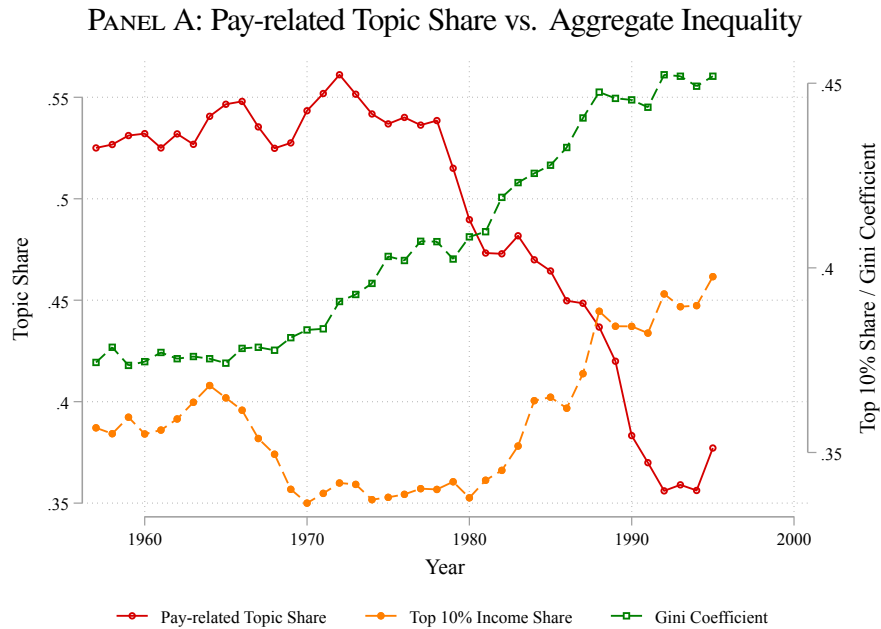
The figure includes the 19 OECD countries that appear in at least two of the three survey waves. Individuals in the survey report their union membership, income, and current occupation. We restrict the sample to individuals currently working for pay, who are not self employed, who report an occupation, income and membership status, and who are aged 20–65. We additionally restrict to wave-occupation-country cells that, after sample restrictions, include at least 10 respondents. In the survey, union membership is measured with the question: “Are you or have you ever been a member of a trade union or similar organisation? If yes: is that currently or only previously?” This allows us to distinguish membership from individuals covered by union contracts, which is particularly important in settings with sectoral bargaining.

Figure E.1: Cross-sectional Relationships between Inequality and Union Organizing Strategies: Adjusting for Union Wage Effect



Notes: Each panel plots a binscatter of a union outcome against industry-region-3 year wage ratios, similar to Figure IV but using CPS wages after subtracting off industry \times year-specific union premium estimates. For a handful of cells with topcoding rates of over 10%, we impute log p90-p50 ratios by calculating their log p80-p50 ratios and then adding the average difference between log p90-p50 and log p80-p50 ratios among all remaining cells. Panel A shows the pay scale status of 451 contracts between 2002-2022. Panel B shows the “pay-related” topic share in the same 451 contracts. Panel C shows the size of 79822 bargaining units as a share of establishment-level employment. The relationships are overall steeper. Standard errors are twoway clustered at the union and employer levels.

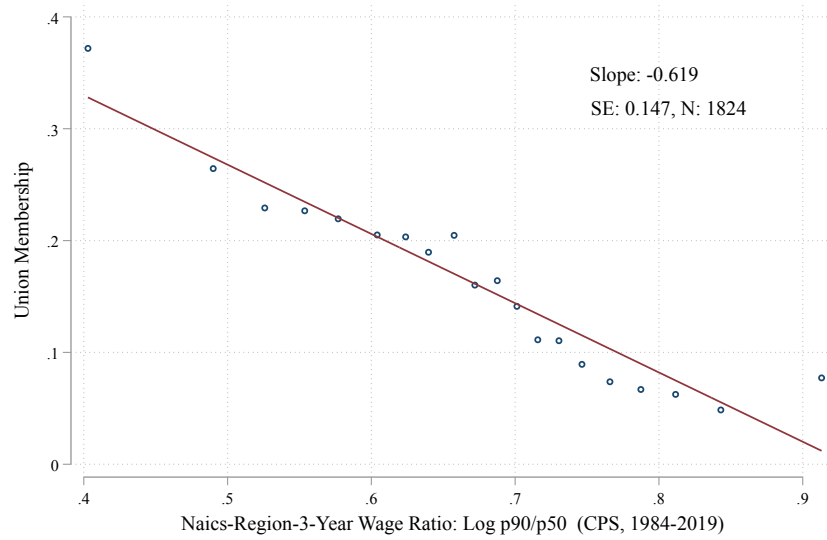
Figure E.2: Time-series Relationships between Coverage Focus of AFL-CIO News and Aggregate Inequality



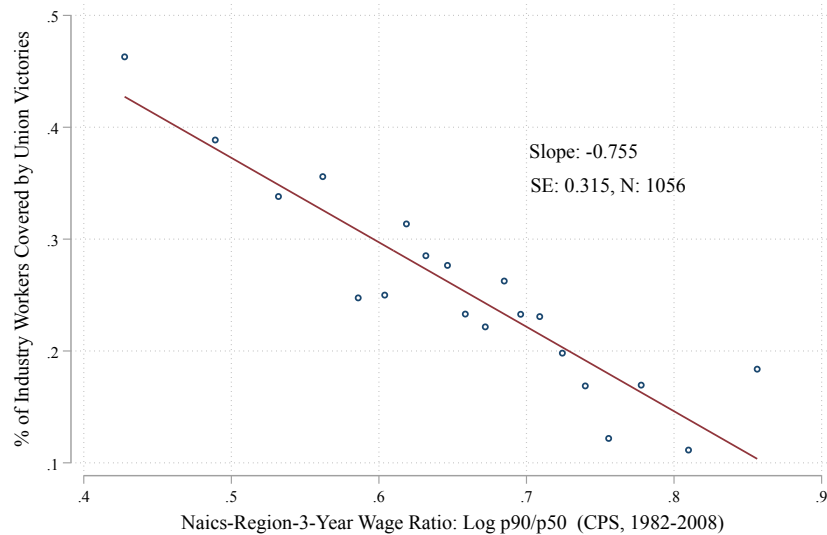
Notes: This figure explores how the focus of AFL-CIO news coverage evolved in relation to rising income inequality over the second half of the 20th century. Mirroring Figure 1 of Farber et al. (2021), Panel A plots the “pay-related” topic share in AFL-CIO News segments on union activities over time, juxtaposed against the top 10% income share (from Piketty et al. (2018)) and the Gini coefficient (using Social Security earnings from Kopczuk et al. (2010)). Panel B plots the evolution of topic shares for all three main topics over time. The topic shares are obtained by running the CorEx model, discussed in detail in Section 3.6.

Figure E.3: Union Membership and Industry Wage Inequality I: Stock vs. Flow

PANEL A: “Stock”: Overall Membership Rate vs. Wage Inequality

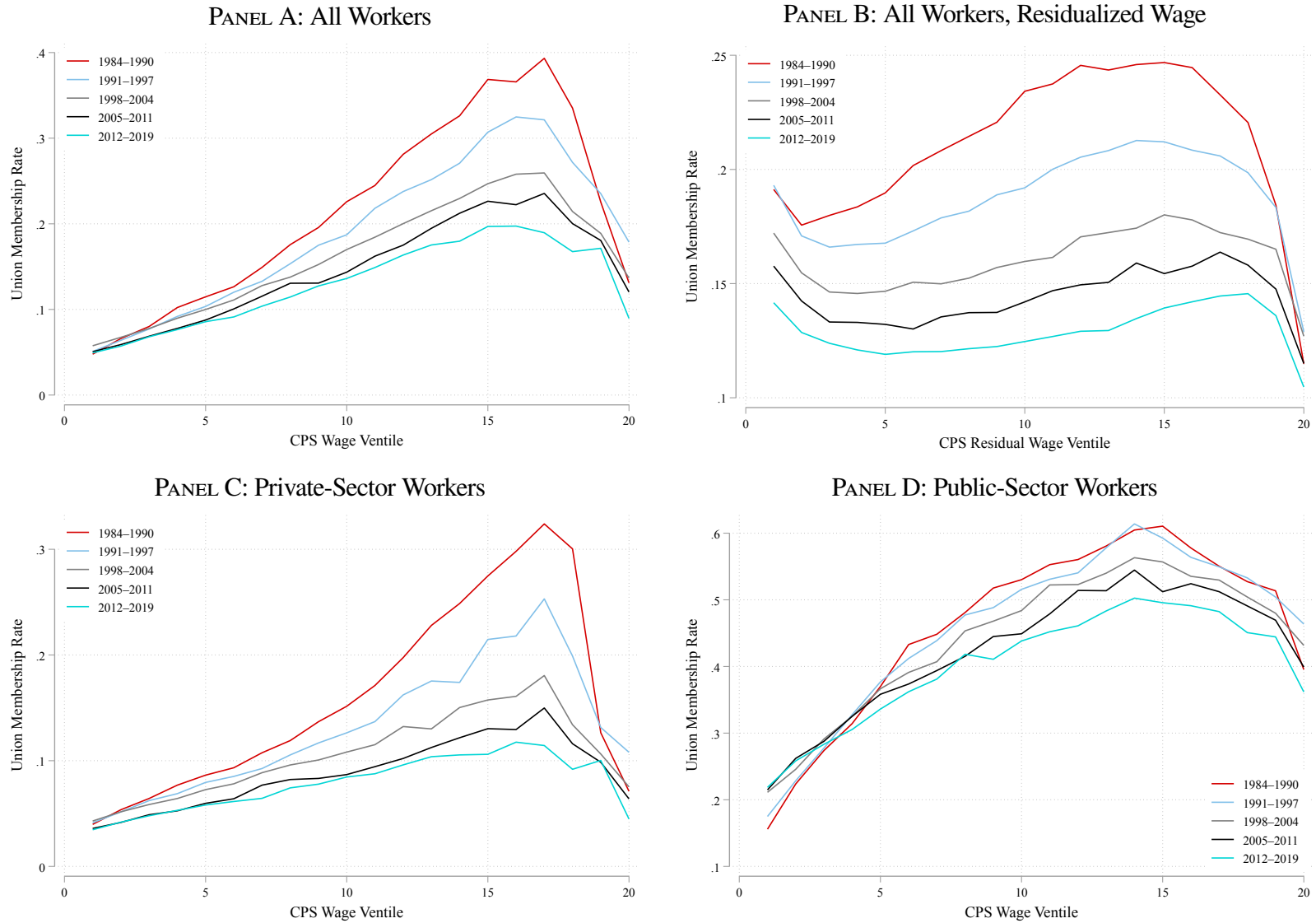


PANEL B: “Flow”: % of Workers Covered by Union Election Victories vs. Wage Inequality



Notes: This figure explores how industry wage inequality is correlated with union membership, using both a “stock” measure of overall membership and a novel “flow” measure of union victory coverage rate. Panel A plots a binscatter of the overall union membership rate against industry-region-3-year log p90-p50 wage ratios between 1984–2019. In Panel B, the y-axis is the percentage of workers covered by union victories as a share of overall industry employment; NLRB election data between 1982–2008 is used to estimate number of workers covered, and Current Population Survey (CPS) data over the same period is used to estimate overall employment. The CPS sample includes non-self-employed civilian workers aged 20–65 who work at least 20 hours per week with an hourly wage equivalent above one-half of the federal minimum wage. Wage ratios are calculated after subtracting estimated cell-level union premium for union workers, and p80-p50 ratios are used to impute p90-p50 ratios for cells with topcoding rates over 10%. Standard errors are twoway clustered at the industry and region levels.

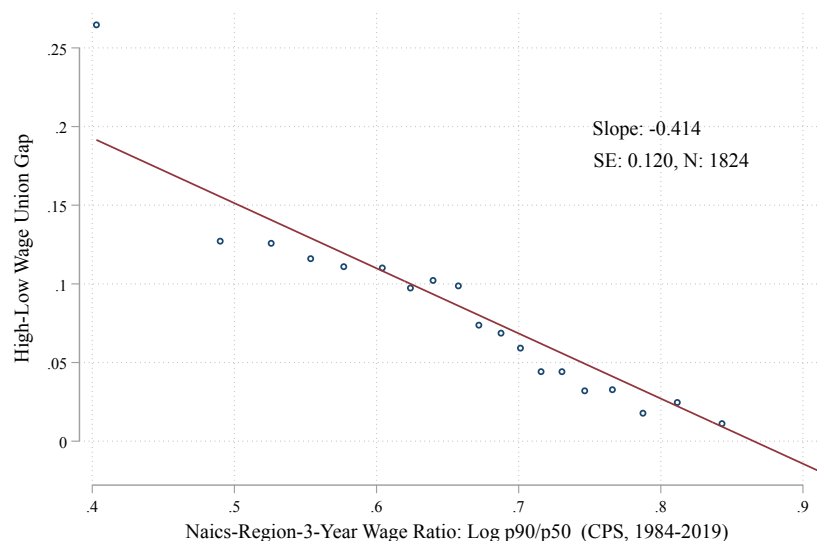
Figure E.4: Union Membership Across the Wage Distribution Over Time



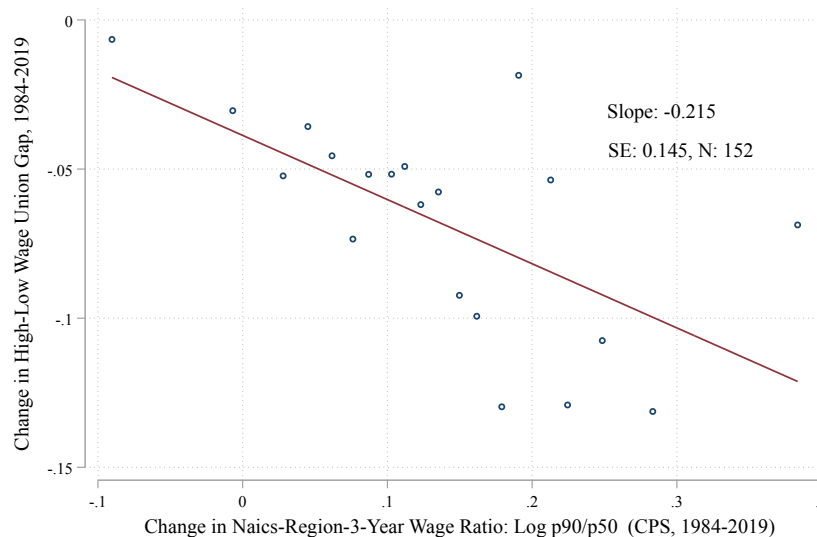
Notes: This figure examines how union membership rates vary across the wage distribution, and how this “union-wage gradient” changes over time. Each panel divides workers into 20 within-year wage ventiles (5-percent bins) and estimates the union membership rate within each cell for 4 time periods (1984–1989, 1994–1999, 2004–2009, 2014–2019). The source is authors’ calculations based on microdata from Current Population Survey (CPS). The CPS sample includes non-self-employed civilian workers aged 20–65 who work at least 20 hours per week with an hourly wage equivalent above one-half of the federal minimum wage. Panel A uses raw wages of all eligible workers. Panel B uses residualized wages after controlling for year-specific age trends and sex, education, state, industry, occupation, and union fixed effects. Panels C and D restrict to private-sector and public sector workers, respectively.

Figure E.5: Union Membership and Industry Wage Inequality II: Heterogeneity by Wage

PANEL A: High-Minus-Low-Wage Membership Gap vs. Wage Inequality

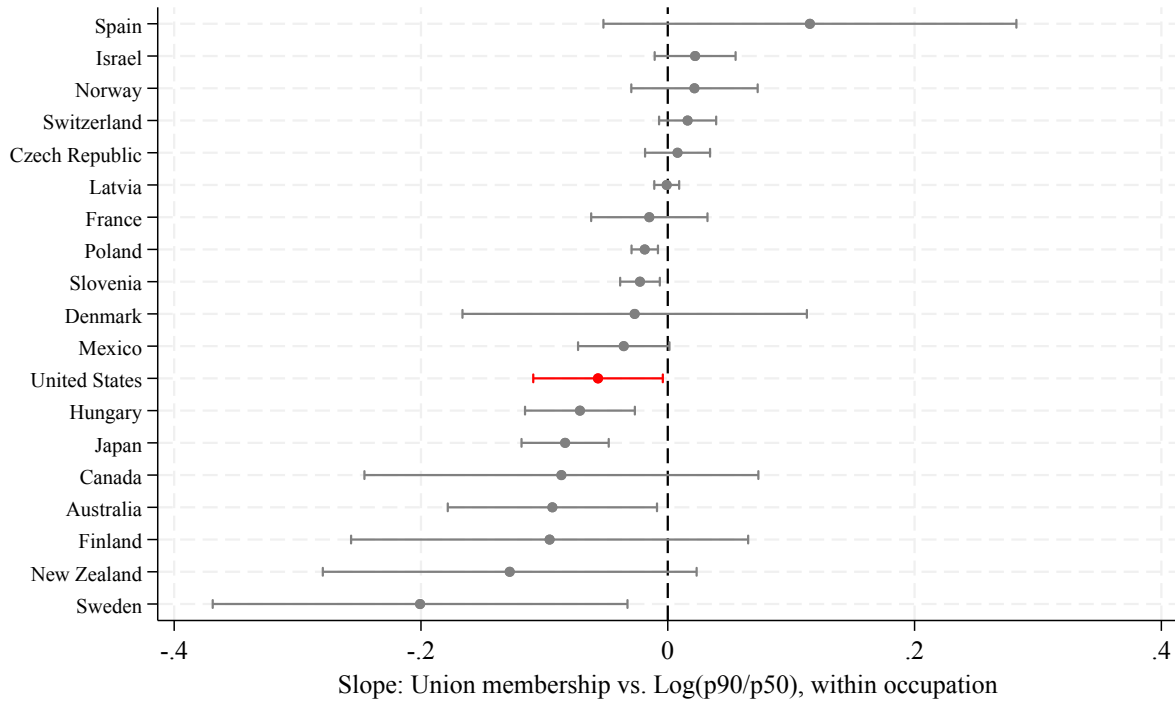


PANEL B: High-Minus-Low-Wage Membership Gap vs. Wage Inequality (Long Differences, 1984–2019)



Notes: This figure explores how industry wage inequality is correlated with union membership advantage among high-wage workers, and how they co-move over time. Panel A plots a binscatter of the membership gap between high- and low-wage workers (defined using the median wage within each industry-region-3-year cell) against industry-region-3-year log p90-p50 wage ratios between 1984–2019. Panels B follows the same setup, but both y- and x-axes become long-differenced changes between 1984–2019, in order to study correlated movements. The CPS sample includes non-self-employed civilian workers aged 20–65 who work at least 20 hours per week with an hourly wage equivalent above one-half of the federal minimum wage. Wage ratios are calculated after subtracting estimated cell-level union premium for union workers, and p80-p50 ratios are used to impute p90-p50 ratios for cells with topcoding rates over 10%. Standard errors are twoway clustered at the industry and region levels.

Figure E.6: Membership vs. Wage Inequality Across OECD Countries



Notes: This figure documents the relationship between union membership and occupational wage inequality across several OECD countries. The sample includes surveyed individuals in the ISSP Work Orientations survey waves II-IV (1997, 2005, and 2015) currently working for pay, who are not self employed, who report an occupation, income and membership status, and who are aged 20–65. We compute the share of union members and the 50th and 90th percentiles of the wage distribution at the 2-digit ISCO code level, within country. The figure reports the coefficients from regressing the share of union members within an occupation on the $\log(p90/p50)$ wage ratio, for each country. See Section E.4 for additional details on construction. Point estimates are shown with 95% confidence intervals. Standard errors are clustered at the occupation level.

F Additional Details on Study II: Wisconsin’s Act 10

F.1 Union Membership Data

To obtain information on individual-level union membership, we follow the method used by [Foy \(2024\)](#). This method involves the following steps.

1. **Clean the campaign contribution data:** We downloaded data on political contributions from the Wisconsin Campaign Finance Information System (WCFIS) [website](#). We scraped all contributions records for all filing periods from 2016 to 2024 in which the Receiving Registrant is the Political Action Committee of the Wisconsin Education Association Council (WEAC) (the state chapter of the National Education Association, one of the two main teacher unions) or of one of the 13 regional branches. These data do not have an individual donor identifier, so we use names to identify each donor (we clean the names to account for inconsistencies, such as variations in middle name initials across time). Because donors may make more than one contribution during each time period, we sum all the contributions an individual makes in a given year to construct a dataset that is unique at the year and individual level.
2. **Clean the teacher records:** As in the rest of the paper, we use the All-Staff Files from the Wisconsin Department of Public Instruction (DPI) for 2007-2017. In keeping with [Foy \(2024\)](#), we restrict the sample to observations in which the salary is not equal to \$0 and the FTE variable is not missing. Since individuals can appear more than once per year if they have multiple work assignments, we keep only the assignment with the highest FTE value so that the final dataset is unique at the individual-year level.
3. **Link the campaign contribution data to the teacher records:** The variables we want to use to link the two files above are name, year, and WEAC region. In the WCFIS contributions data, we observe all three of these variables. However, in the All-Staff records, we can see the district in which an individual works, but not the WEAC region that corresponds to that school district. Therefore, we scraped the WEAC region offices [website](#) to create a crosswalk between school districts and WEAC regional offices, which we merged by school district onto the All-Staff data. We then performed the merge in three steps:

- (a) **Strict Merge:** We first merge the datasets on name, year, and WEAC region.
- (b) **Fuzzy Merge by Year and Region:** For unmatched records, We perform a fuzzy merge on name within the same year and WEAC region.
- (c) **Fuzzy Merge by Year Only:** Finally, We conduct another fuzzy merge on name by year, without requiring a WEAC region match. This step accounts for cases where individuals are linked to multiple districts and may be incorrectly assigned to a region in the all-staff data.
- (d) **Constructing the union membership variable** Following [Foy \(2024\)](#), we assume that if a teacher appears in the contributions data, they are a union member for two reasons: (a) The majority of contributions in the WCFIS data are bunched at the \$19.99 and \$5.00 marks, which are the values that unions automatically redirect from member dues to their PACs. This is displayed in [Figure F.1](#), where the top panel plots the distribution of political donations by teachers to the WEAC PAC and the bottom panel shows the distribution of donations to the regional chapters; (ii) it is uncommon for non-union members to donate to union-affiliated PACs.

F.2 CBA Expiration Dates

We collected information on each district's expiration date of the CBA in place when the reform passed. We use the dataset first compiled by [Biasi \(2021\)](#), constructed combining information from multiple sources, including union contracts, districts' employee handbooks, school board meeting minutes, and local news sources. Union contracts generally report the date of the expiration of the agreement. Post-Act 10 school board minutes typically mention whether a contract was set to expire in 2011. The presence of an early version of district employee handbooks is also useful to establish when the post-CBA pay regime was introduced (which typically coincides with the date of the earliest handbook at the latest). When available, the dataset prioritizes information from union contracts, school board minutes, and handbooks. In cases where these documents are unavailable, the records are complemented with information from online local news sources. The dataset contains information on 247 of the state's 428 districts, covering approximately 70% of all teachers. These data are described in [Figure F.2](#).

F.3 Wisconsin School Districts, Commuting Zones, and Inequality Shocks

Our measure of inequality shock uses the Herfindahl-Hirshman Index of the local public teacher labor market to which each school district belongs to. We use commuting zones (CZs) as the relevant geographic labor market. Wisconsin had 421 school districts in 421, located in 19 commuting zones. Figure F.3 shows the location of CZs (marked by red lines) and school districts (marked by black lines) within the state. Darker colors denote a higher HHI, higher local labor market concentration, and a smaller inequality shock.

One key identifying assumption in our research design is the Stable Unit Treatment Value Assumption (SUTVA), which requires no spillover or interference between treated and control units. In our design, SUTVA would be violated if teachers systematically moved between districts with and without a rise in inequality in response to the policy. To test the plausibility of this assumption, in Figure F.4 we perform an event study of each teacher's probability of moving out of the public sector, across districts, and across CZs, separately for districts with and without an inequality shock in a time window around the expiration of the CBA in each teacher's home district. We do so by re-estimating equation (7) of the paper on individual-level teacher data, using an indicator for teachers moving across districts as the dependent variable. Reassuringly, we find that in any given year, fewer than 1% of teachers move out of their commuting zone, and this mobility is similar across high- and low-HHI districts at all times, including after the reform.

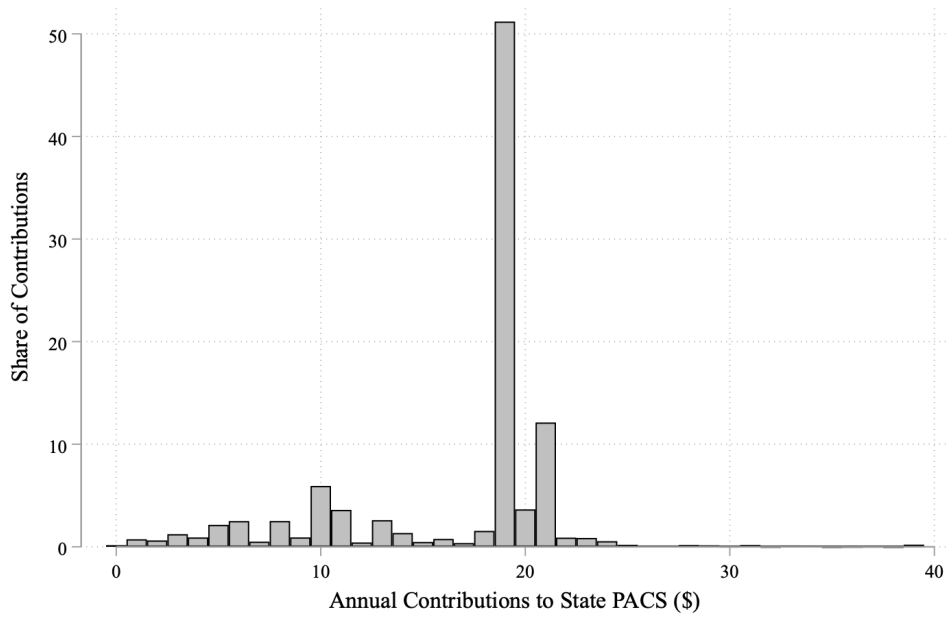
F.4 Union Revenues Data

To compile a panel of union finance data, we use information from the Internal Revenue Service (IRS) Form 990 (the "Return of Organization Exempt From Income Tax"), which most tax-exempt organizations, including labor unions, must file every year. We accessed a database of digitized Form 990s provided by the National Center for Charitable Statistics (NCCS) of the Urban Institute and searched the name of Wisconsin teacher unions, as they appear in the records of the Wisconsin Employment Relations Commission (available at <https://werc.wi.gov/>). We were able to link 99 districts to the records of 52 unions. For our analysis, we compute revenues per teacher dividing total revenues (primarily from membership dues) by the total number of teachers in the districts represented by each union; we obtained the latter from the Staff Files of the Wisconsin Department of Public Instruction.

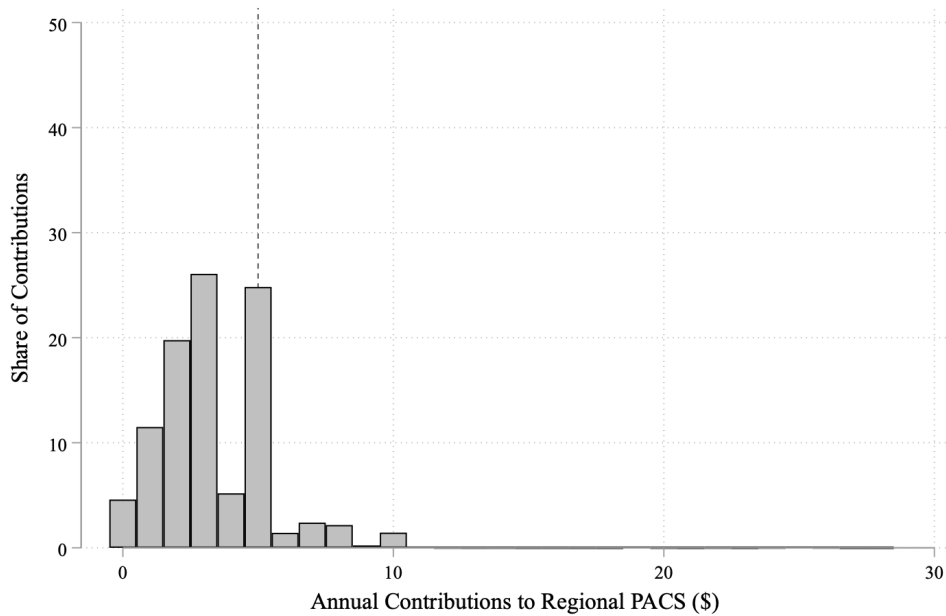
In Figure F.9, we estimate equation (7) of the paper separately for districts in CZs with and without an inequality shock, additionally controlling for each district's share of teachers in each experience group.

Figure F.1: Distribution of Contributions to State and Regional PACs

PANEL A: Contributions to State PACs

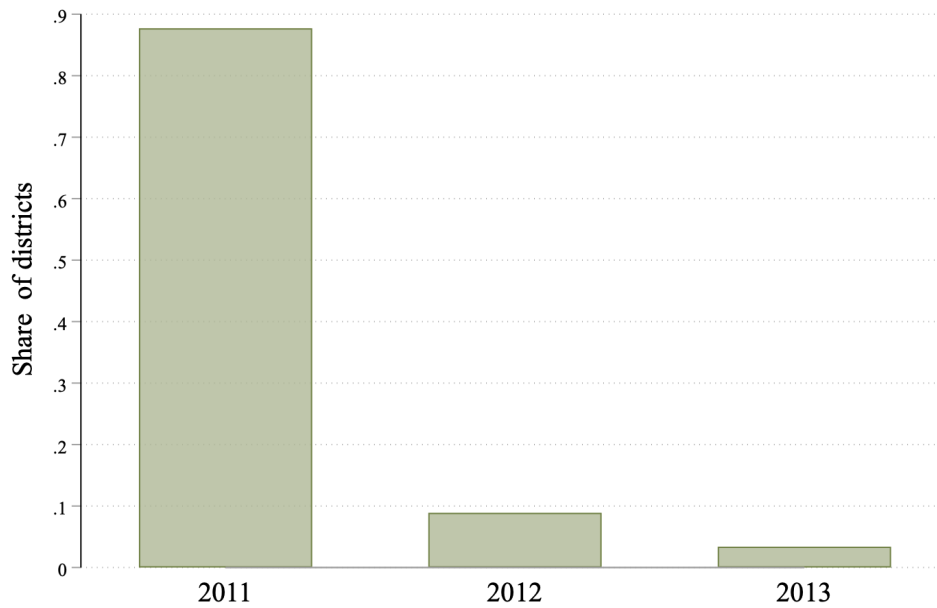


PANEL B: Contributions to Regional PACs



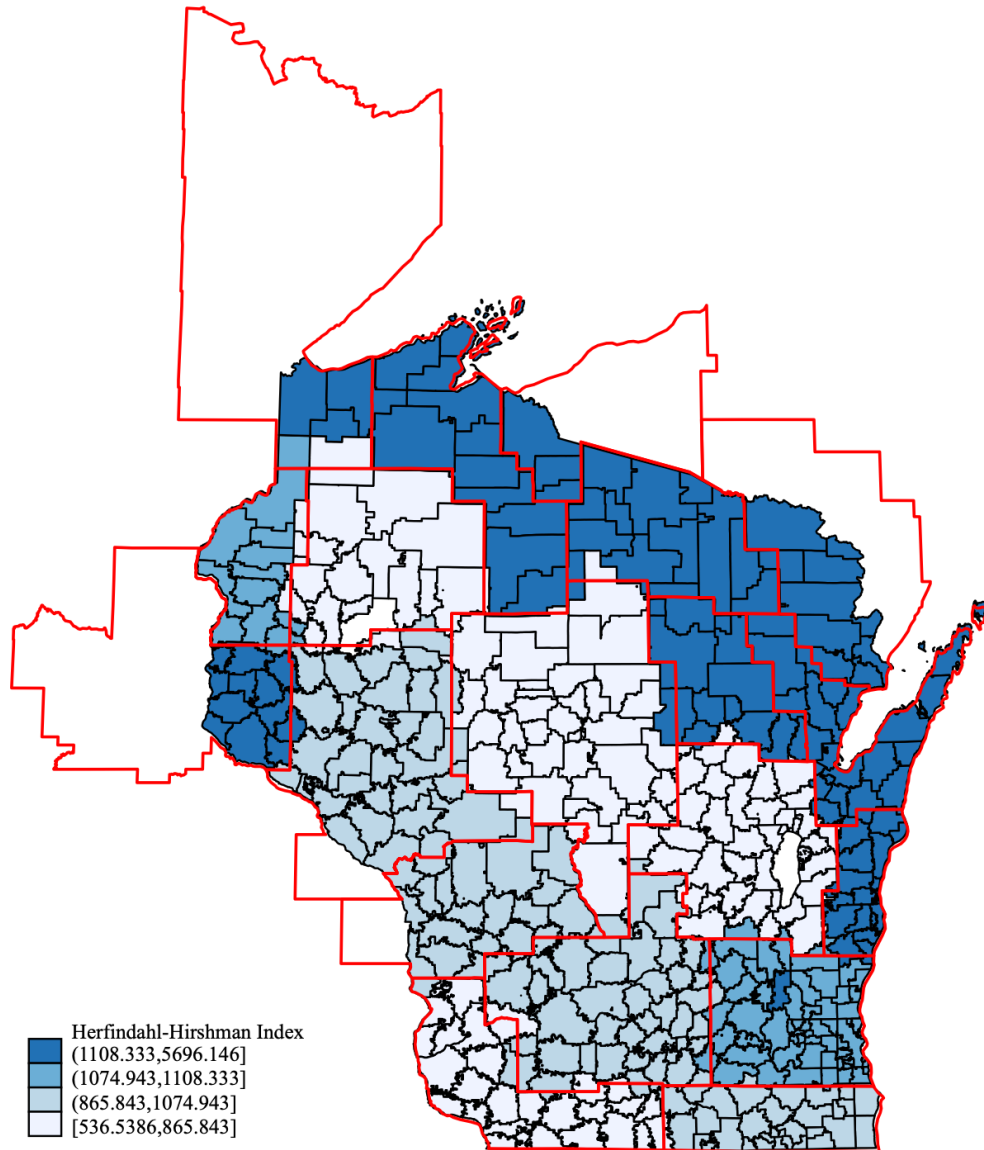
Notes: The figure shows the distribution of annual donations to state political action committees (PACs, panel A) and regional PACs (panel B) in Wisconsin by public-school teachers between 2016 and 2020, from the Wisconsin study described in Section 4.

Figure F.2: Distribution of Wards and Districts by CBA Expiration Dates



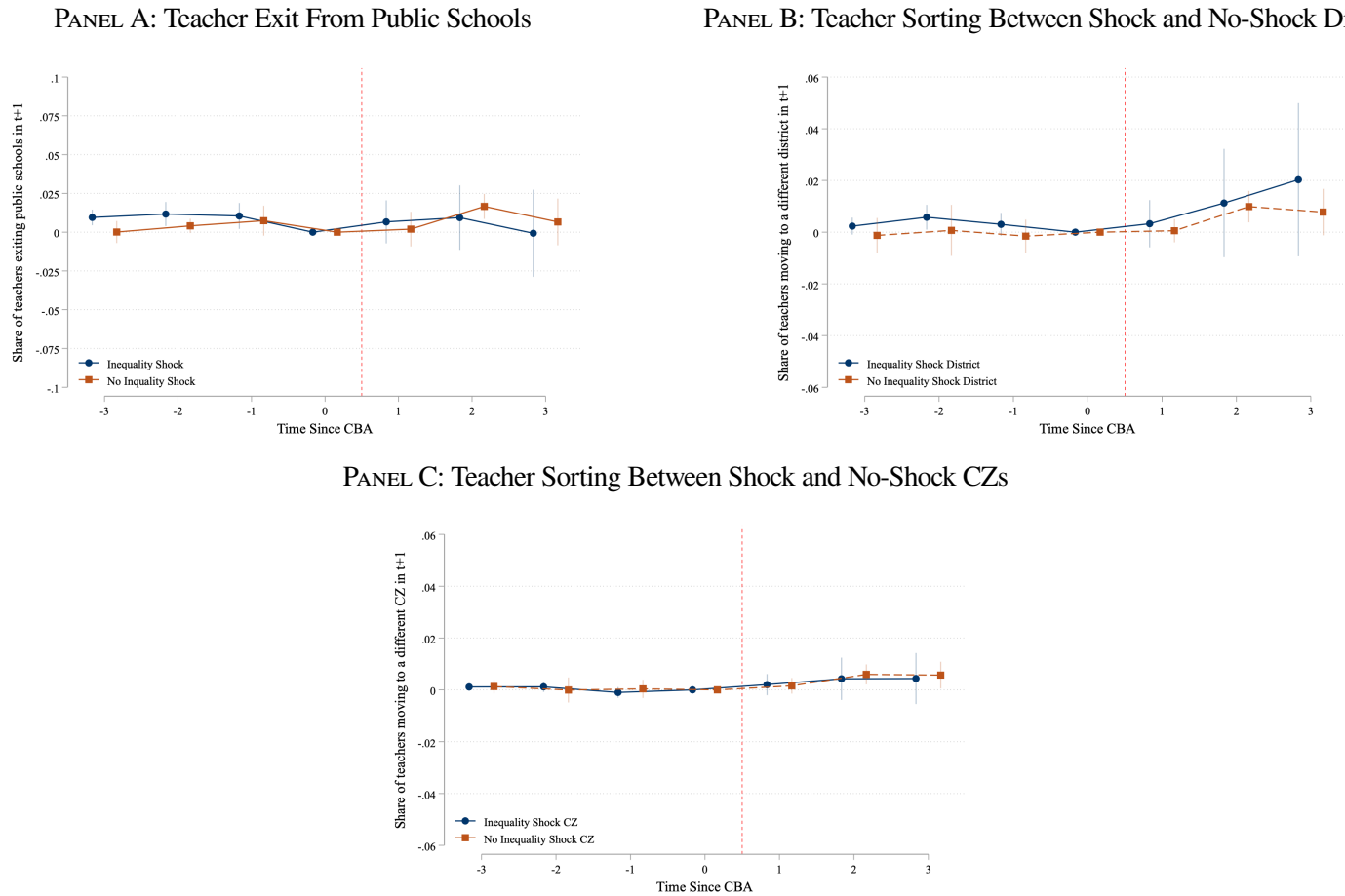
Notes: The figure plots the share of school districts by the year in which their collective bargaining agreements (CBAs) expired, from the Wisconsin study described in Section 4.

Figure F.3: Labor Market Concentration Across Wisconsin School Districts and Commuting Zones



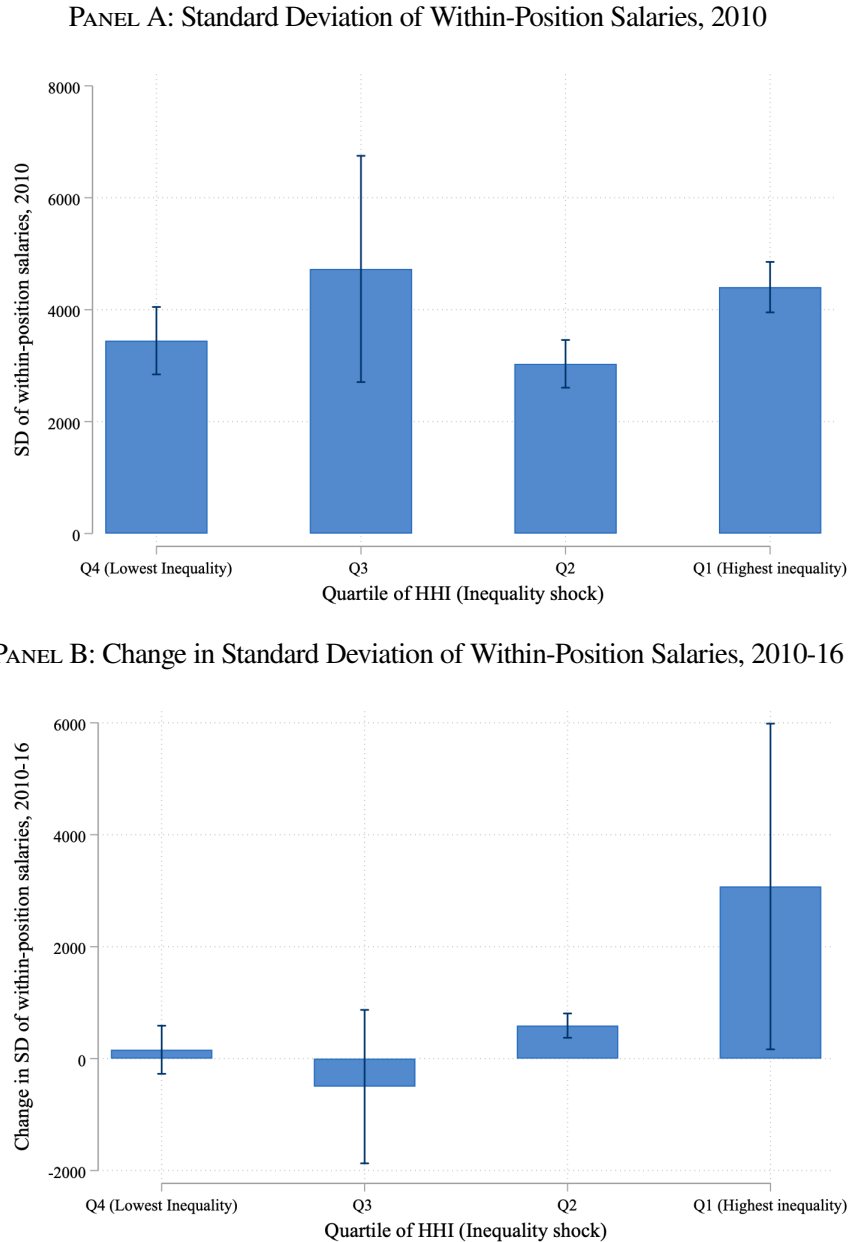
Notes: The map plots the Herfindahl-Hirshman Index (HHI) in teacher employment concentration in 2010, from the Wisconsin study described in Section 4. The HHI is calculated for each commuting zone using the distribution of teachers across school districts within the CZ, with higher values indicating greater concentration. Red lines denote commuting zone boundaries; black lines denote school district boundaries.

Figure F.4: Wage Inequality Shocks and Teachers' Outside Options: Teacher Sorting Out of Public Schools, Across Districts, and Across Commuting



Notes: This figure explores whether teachers' outside options—captured by exit from public schools and their mobility between commuting zones or districts—change over time in districts that did and did not experience a rise in inequality, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from a modified version of equation (7)—instead of revenue, we regress indicators for *teachers exiting public schools* (panel A), *teachers in a district moving to a different district* (panel B), and *teachers in a district moving to a different CZ* (Panel C) on event-year dummies (relative to expiration of collective bargaining agreements) with district and year fixed effects. To capture changes in outside options, in panel A we focus on workers younger than 55, the minimum retirement age for public school teachers in Wisconsin. Blue series refer to districts that experience an *inequality shock*, while orange series refer to districts with no shock. We classify CZs with an inequality shock based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. Coefficients are displayed with 95% confidence intervals and using standard errors clustered at the district level.

Figure F.5: Wage Inequality and Labor Market Concentration, By Quartiles of The Herfindahl-Hirschman Index



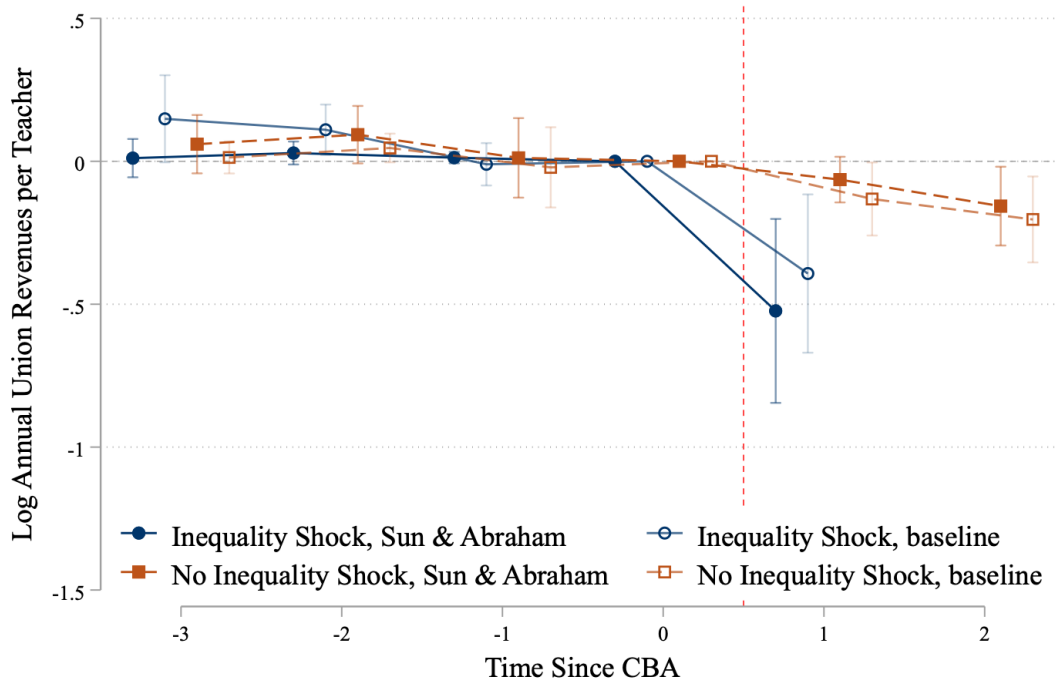
Notes: This figure shows how the within-position standard deviation of salaries changed after Act 10 relative to before, by quartile of the Herfindahl-Hirschman Index, from the Wisconsin study described in Section 4. Each bar corresponds to the coefficient on a dummy for each quartile in a regression with the 2010 standard deviation of within-position salaries (panel A) and the 2010-2016 change in the same variable (panel B). We classify districts in each quartile based on the distribution of the Herfindahl-Hirschman Index in their commuting zone. Observations are weighted by the number of workers. Estimates are displayed with 95% confidence intervals, using robust standard errors.

Figure F.6: Wage Inequality and Labor Market Concentration, Over Time



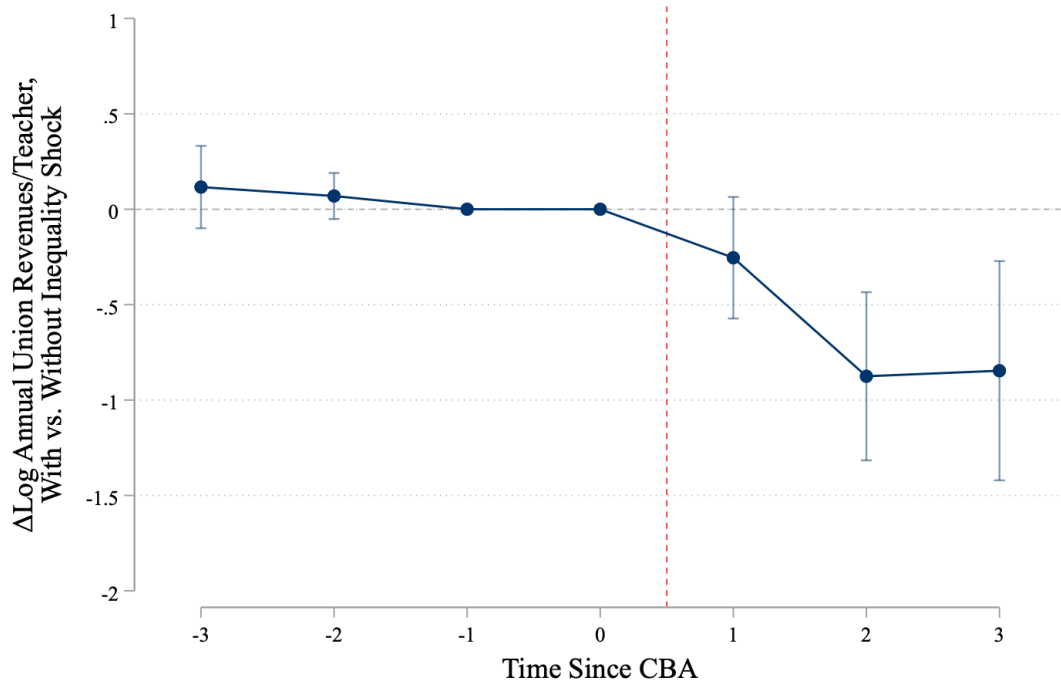
Notes: This figure shows how the within-position standard deviation of salaries evolved in districts with and without an inequality shock by time since CBA expiration, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from equation (7), obtained regressing the average within-position standard deviation of salaries in each district and year on event-year dummies (relative to expiration of collective bargaining agreements) with district and year fixed effects. We obtain and display separate coefficients for districts that experience an *inequality shock* (blue series) and those that experience *no inequality shock* (orange series). We classify districts as having an inequality shock districts based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. Observations are weighted by the number of workers. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

Figure F.7: Wage Inequality Shocks and Union Revenues - Robustness to Negative Weights



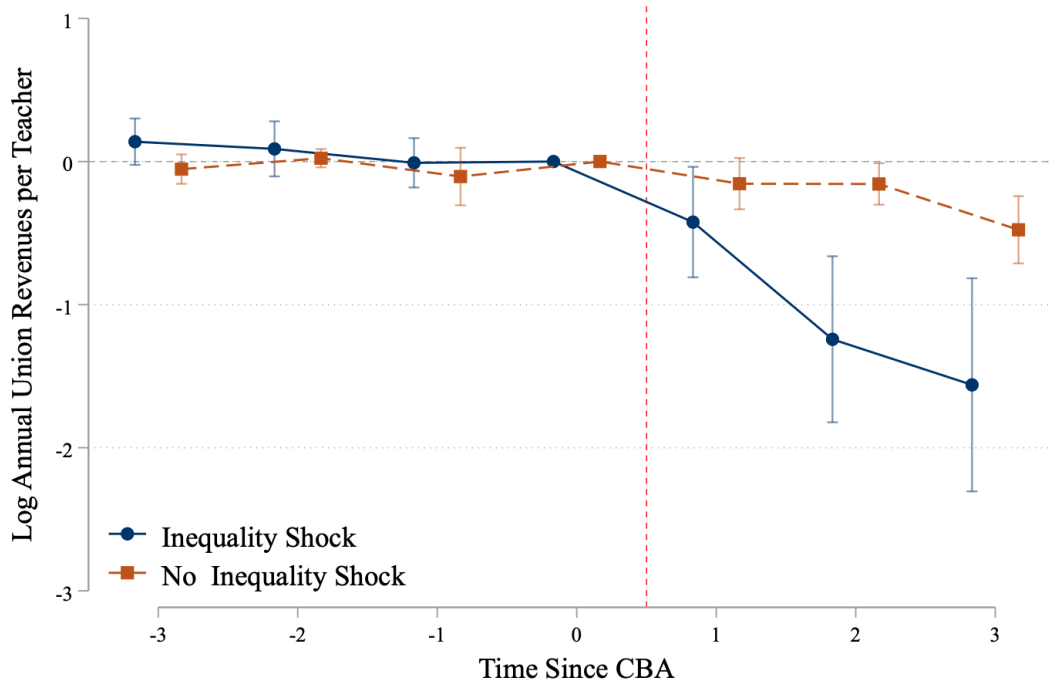
Notes: This figure replicates Figure V implementing the estimator proposed by Sun and Abraham (2021), robust to the presence of heterogeneous treatment effects across cohorts correlated with the timing of the treatment. It shows how wage inequality affects union revenues at the school district level, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from equation (7), which regresses log union revenues per teacher on event-year dummies (relative to expiration of collective bargaining agreements) with district and year fixed effects. We obtain and display separate coefficients for districts that experience an *inequality shock* (blue series) and those that experience *no inequality shock* (orange series). We classify districts as having an inequality shock districts based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. We end our analysis one/two years after a CBA expiration to maintain a set of never-treated districts in each group. Observations are weighted by the number of workers. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

Figure F.8: Wage Inequality Shocks and Union Revenues - Difference Between Shock and No-Shock Districts



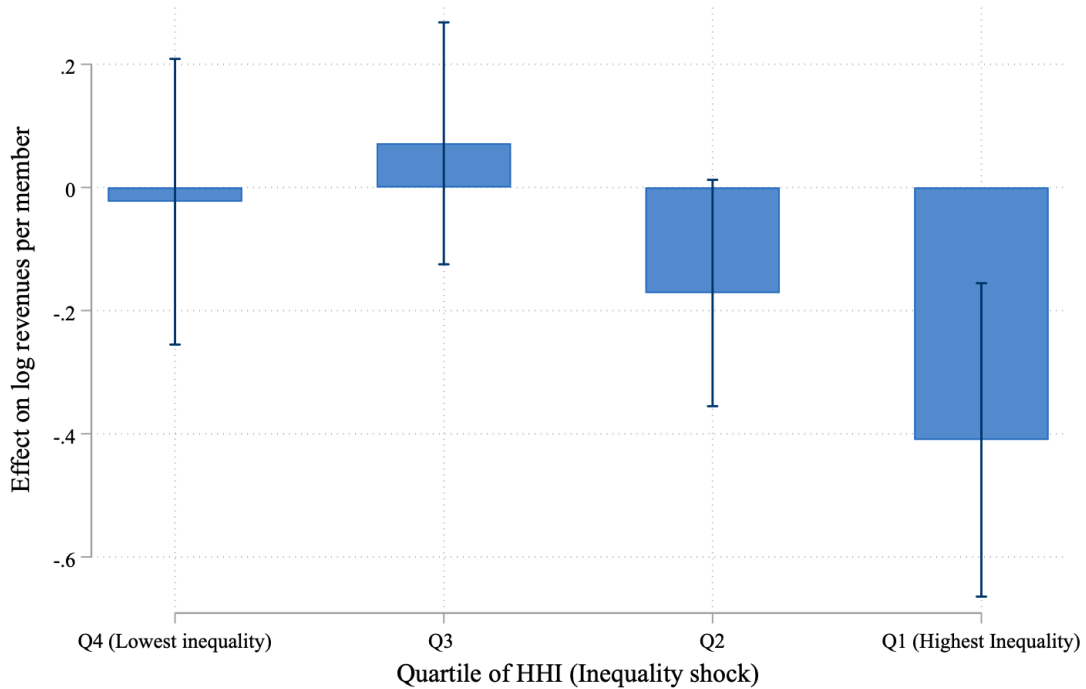
Notes: This figure shows differences in the estimates of coefficients β_k from equation (7), between inequality-shock and no-inequality shock districts. Estimates are obtained regressing log annual union revenues per teacher on event-year dummies (relative to expiration of collective bargaining agreements) with district and year-by-shock fixed effects, pooling data for shock and no-shock districts and further interacting the term $\mathbb{1}(t - E_{d(j)} = k)$ with indicators for inequality shock districts. We classify districts as having an inequality shock districts based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

Figure F.9: Wage Inequality Shocks and Union Revenues - Controlling for Shares of Teachers in Each Position



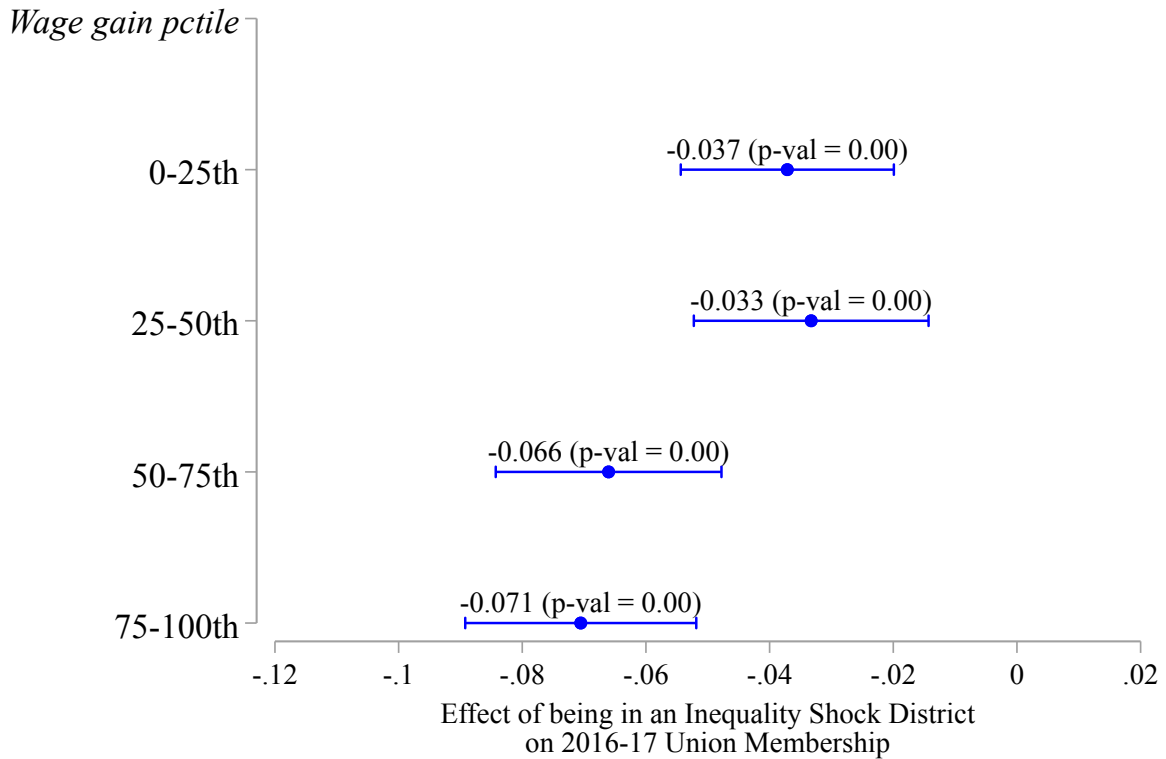
Notes: This figure replicates Figure V while additionally controlling for the distribution of teacher positions in each district. It shows how wage inequality affects union revenues at the school district level, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from equation (7), which regresses log union revenues per teacher on event-year dummies (relative to expiration of collective bargaining agreements) with district and year fixed effects. We obtain and display separate coefficients for districts that experience an *inequality shock* (blue series) and those that experience *no inequality shock* (orange series). We classify districts as having an inequality shock districts based on whether they have a commuting zone-level Herfindahl-Hirschman Index below the state median. Observations are weighted by the number of workers. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

Figure F.10: Union Revenues by Quartiles of the Herfindahl-Hirschman Index



Notes: This figure replicates Figure V by pooling together years before and after a CBA expiration and by splitting districts according to the quartile of their commuting zone-level Herfindahl-Hirschman Index. It shows how wage inequality affects union revenues at the school district level, from the Wisconsin study described in Section 4. Estimates represent coefficients β_k from equation (7), constrained to be zero for $k \leq 0$ and constant for $k > 0$, and obtained by regressing log union revenues per teacher on an indicator for years after a CBA expiration, with district and year fixed effects. We obtain and display separate coefficients for districts in different quartiles of their commuting zone Herfindahl-Hirschman Index. Observations are weighted by the number of workers. Estimates are displayed with 95% confidence intervals, using standard errors clustered at the district level.

Figure F.11: Wage Inequality and Union Membership, by Size of The Wage Gains



Notes: This figure explores heterogeneity in the effect of wage inequality on union membership at the teacher level by size of each teacher’s wage gain, from the Wisconsin study described in Section 4. Coefficients show the effect on union membership of working in inequality shock districts (defined as having a commuting zone level Herfindahl-Hirschman Index below the state median) relative to no-inequality shock districts. Respondents are grouped by whether they have wage gains below the 25th percentile, between the 25th and 50th, between the 50th and 75th, and above the 75th, within their district. Wage gains are defined as 2010-2016 changes in salaries. Each regression is estimated using membership data for 2016/17 and controls for having experience above the median and for gender. Point estimates are shown with 95% confidence intervals, using robust standard errors, and p-values testing for whether the coefficient is different from zero are printed next to the point estimates.

G Additional Details on Study III: Experimental Evidence on the Impact of Inequality in Union Support

G.1 Additional Recruitment and Sample Details

Appendix Table [G.1](#) reports characteristics of both our contact list and respondents. Using individual data from IMDb ([IMDb, 2023](#)), we link 4,373 WGA contacts to an IMDb profile, or 83% of the 5,244 total contacts (Column (1)). In Columns (2), we compare them to the 334 survey respondents we can also link to IMDb (84% of 400 complete responses) on observable characteristics. We report each individual’s earliest credit year, total credits, and a breakdown of credit characteristics.

We additionally report the gender breakdown of contacts and respondents. Although respondents self-report their gender at the end of the survey, to classify the gender of contacts, we use data from the U.S. Social Security Administration which reports gender distributions of first names following [Adukia et al. \(2023\)](#). We analyze first names given to individuals born between 1920 and 2010, classifying a name as female if women comprise more than 50% of all people with that name during this period. We are able to classify the first names of 97% of contacts.

G.2 Additional Results

G.2.1 Demand and Uses for a Pay Report

In our baseline survey, we present respondents with an example of what a pay report could look like (Appendix Figure [G.3](#)). We display two types of such “example” reports: one showing aggregate pay distributions and one showing pay distributions split by gender. We randomize which example respondents see first. Results in this section focus on the aggregate report, and we discuss robustness to randomization order and focusing on the gender-split report (Appendix Section [G.2.2](#)).

Survey Questions We describe the information contained in a pay report as revealing the distribution of pay rates (Appendix Figure [G.3](#), and then we ask, on a 5-point scale: “Do you think we should create such a report?” We align the respondents’ incentive to answer truthfully by reminding them that we would use their responses to decide whether to produce and circulate such a report.⁹ We also measure willingness

⁹We consider “Yes, I would value it significantly” or “Yes, I would be interested to see it” as affirmative interest when we report a binary measure of this question. The incentive compatible language states: “We are considering producing a

to pay (WTP) for the report following the incentive-compatible BDM procedure (Becker et al., 1964). We ask respondents to decide on 5 binary options between receiving the pay report and a financial reward, stating that we will randomly select a preferred option to fulfill for 10 participants if the report is produced. The 5 choices are between the pay report and \$25, \$150, \$500, \$2,000, and \$6,000. We set WTP inside each bin to the bin's mid-point and set the top bin to \$6,000, following Andersen et al. (2006).

After eliciting interest in the report, we ask respondents: “How would you use the report if it were published? Select all that apply.” This question is intended to investigate how pay information might impact respondents’ actions. We present five potential uses (e.g., contract negotiation, labor organizing) and allow for write-ins.

Results Figure G.4 Panel A documents high demand for pay transparency: 87% of respondents express interest in a pay report (left panel), with an average WTP of \$937 (right panel). Specifically, 32% are not willing to pay more than \$25 and 52% are not willing to pay more than \$150, while 27% are willing to pay \$500 or more. This heterogeneity is consistent with pay information being high-value for some respondents (e.g., those expecting to negotiate soon) but not others. In prior work, Cullen and Perez-Truglia (2022) find that one quarter of workers are willing to pay more than one week’s earnings for information about their co-workers’ pay. Panel C shows how respondents declare they would use the pay report. After informational reasons (80% would use the report “to know where they stand in the pay distribution”), the second most common answer is individual negotiation: 70% would use the report to negotiate their future contract (and 22% would use it to renegotiate their current contract). Only 33% would use it for labor organizing. In other words, writers are interested in their relative standing and to have data to improve their individual negotiations.

G.2.2 Robustness

We examine several alternate measures of these results, building on our baseline measures from Figure G.4. First, we document similar interest in the combined pay report only among WGA members who saw the combined pay report before the gender-split pay report (Appendix Figure G.5). Second, we show our measures of interest for the gender-split report (Appendix Figure G.6). There is little difference in

report pertaining to writers’ & directors’ career negotiations at every level. Particularly during this historic renegotiation, we want to understand how providing currently inaccessible information may affect you for better or worse. We will use answers to this 10-minute survey to decide whether to pursue this project and whether to send you the report.”

demand for the overall and split pay reports, with 84% of respondents indicating interest in the split pay report and 87% indicating interest in the overall report.

Third, we show interest in a pay report among the full sample of survey respondents. So far, we focus on responses from WGA members given the high-stakes nature of the institutional context. However, we also recruited non-WGA writers as well as both members of the Directors Guild of America (DGA)—which represents 19,000 directors and members of the directorial team working in media such as film, television, news, and commercials (Sakoui, 2023)—and non-DGA directors. The DGA contract was up for renegotiation around the same time as the WGA’s, and they reached a tentative agreement on June 3 that members ratified on June 23 (DGA, 2023). Our full contact list contained 19,916 writers and/or directors (our “contacts”). We received 1,048 complete responses in total, or 5.3% of all contacts, with 9.0% completing some fraction of the survey. The results are quantitatively very similar to the WGA-only sample: 83% value the report, with a median WTP of \$1,008. In this sample, there is slightly more willingness to petition, as shown in Appendix Figure G.7.

G.3 Recruitment Materials

We sent the following email for baseline survey recruitment starting on June 15, 2023:

Dear [first name],

We are a team of professors from Harvard, MIT, and UBC, with expertise on negotiation. We are considering producing a report pertaining to writers’ & directors’ career negotiations at every level. Particularly during this historic renegotiation, we want to understand how providing currently inaccessible information may affect you for better or worse.

We will use answers to this 10-minute survey to decide whether to pursue this project and whether to send you the report. Your responses will remain confidential, unless you indicate otherwise when prompted during the survey.

If you would like to participate, please continue (accessible on mobile) here.

You can alternatively participate by copy-pasting the URL: [link]

We then sent the following recruitment email to writers on August 11, 2023 for the follow-up sur-

vey:

Dear [first name],

We are a team of professors from Harvard, MIT, and UBC, with expertise on negotiation. Thank you to those who completed our initial survey on pay in the screenwriting and TV writing industry.

As promised, we are following up with you to share our results and ask one question. Your participation is fully confidential.

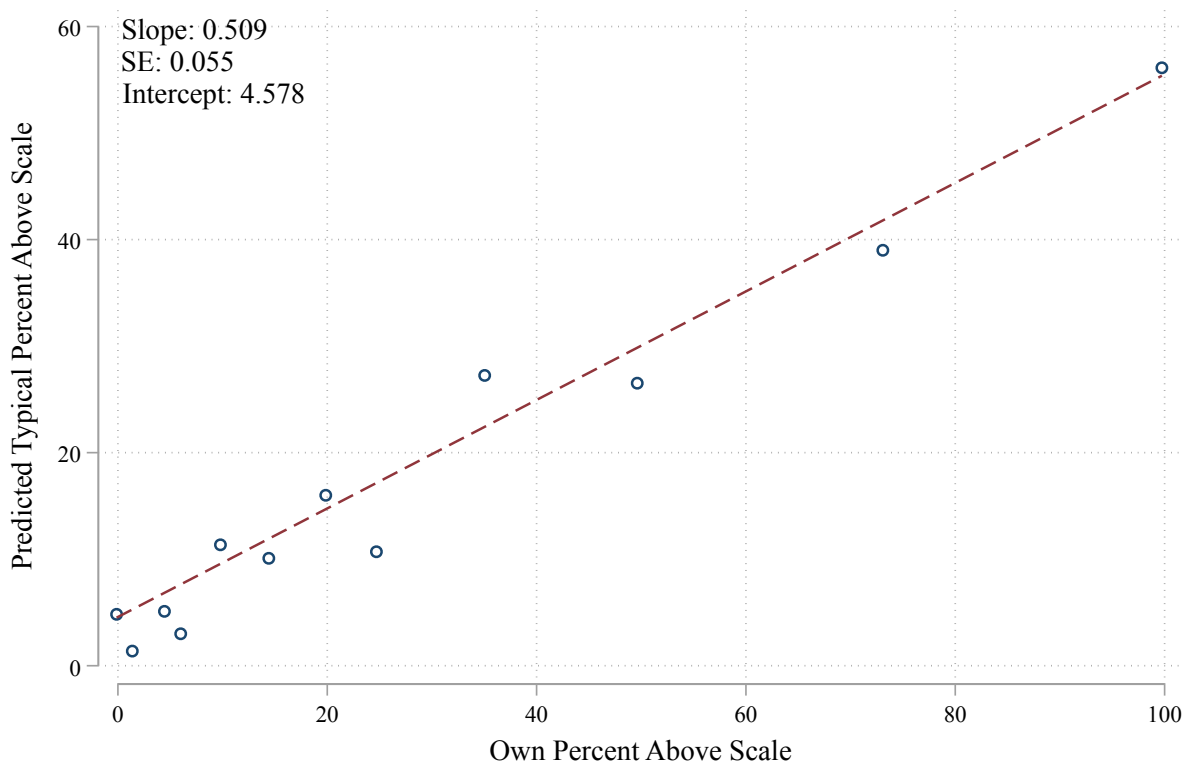
If you'd like to see our results, please continue (accessible on mobile) here. You can also view by copy-pasting the URL to the Harvard Qualtrics platform: [link]

Table G.1: Contact and Respondent Characteristics

	Baseline Survey		Follow-up Survey	
	(1) Contacts	(2) Respondents	(3) Contacts	(4) Respondents
% Male	64.3 (0.7)	63.9 (2.6)	63.5 (0.8)	57.5 (3.1)
Earliest Credit Year	2005 (0.2)	2007 (0.6)	2005 (0.2)	2007 (0.7)
Total Credits	38.8 (1.3)	39.3 (7.0)	40.3 (1.5)	38.9 (6.9)
Credit Type				
% Writing	79.7 (0.5)	83.2 (1.5)	81.2 (0.5)	80.0 (1.9)
% Directing	12.0 (0.4)	9.0 (1.2)	10.8 (0.4)	12.5 (1.6)
% Producing	8.3 (0.3)	7.8 (1.0)	8.0 (0.3)	7.5 (1.1)
Credit Medium				
% TV	80.5 (0.5)	83.6 (1.6)	83.1 (0.5)	82.5 (1.8)
% Movie	11.5 (0.4)	7.5 (1.0)	9.8 (0.3)	7.5 (1.1)
Credit Genre				
% Drama	50.9 (0.6)	53.4 (2.2)	52.1 (0.6)	52.4 (2.5)
% Comedy	45.4 (0.6)	44.4 (2.3)	46.5 (0.7)	46.8 (2.6)
Total Individuals	5,244	400	4,716	310
Individuals with Credit Data	4,376	335	3,935	261

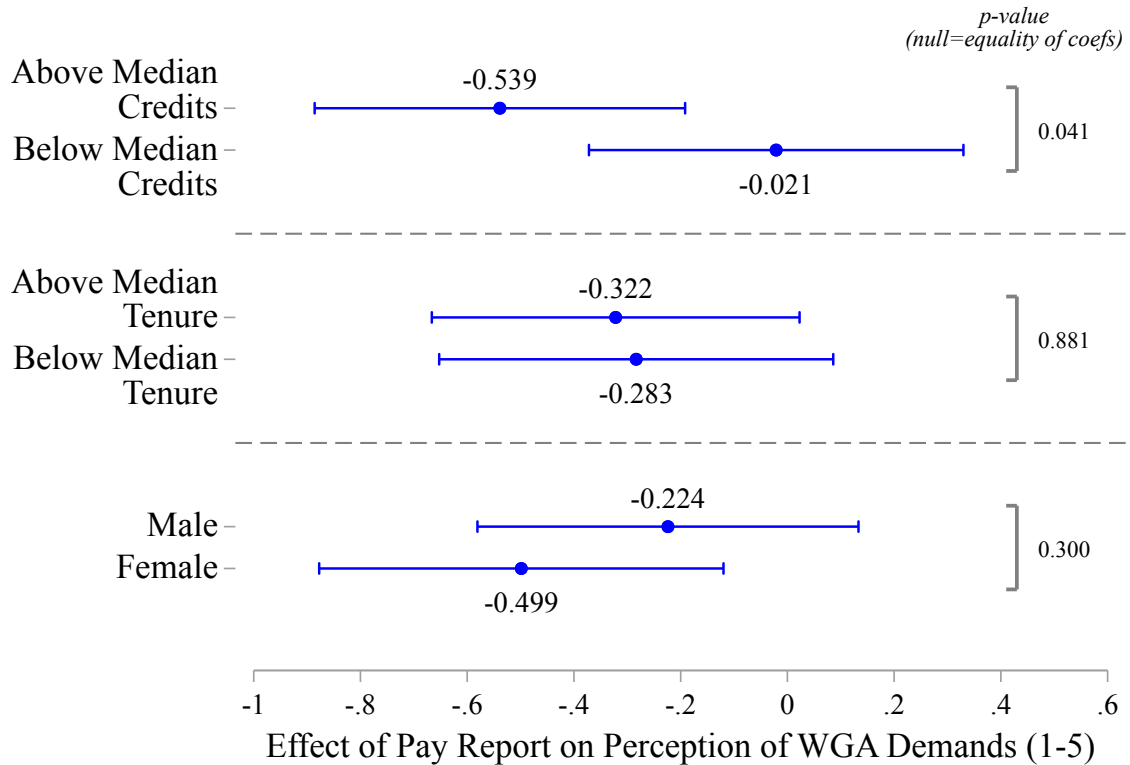
Notes: This table summarizes characteristics of Writers Guild of America (WGA) members contacted for the baseline and follow-up surveys, as well as those who responded, from the Hollywood experiment described in Section 5. Columns (1)–(2) describe the baseline survey, while Columns (3)–(4) describe the follow-up. Data come from IMDb (2023). Statistics are restricted to individuals successfully linked to the IMDb data. *Total Individuals* reports the total population in each group. *Individuals with Credit Data* indicates how many of them are matched to IMDb data, have complete IMDb information, and have first names that allow gender classification using Social Security Administration data. To obtain *% Male*, we classify first names from the contact list by gender. *Credit Type* breaks down the types of credits (writing, directing, producing), which are not mutually exclusive on a given project. *Credit Medium* shows percentages of credits in television and film, which together compose 87% of all titles in the dataset. *Credit Genre* shows percentages of credits in the two most common genres: drama and comedy.

Figure G.1: Own Pay vs. Prediction of Typical Pay



Notes: This figure plots a binscatter of the relationship between a writer’s self-reported percent above scale and their prediction of the typical percent above scale for other writers in similar positions, from the Hollywood experiment described in Section 5. Sample size is 400 WGA members.

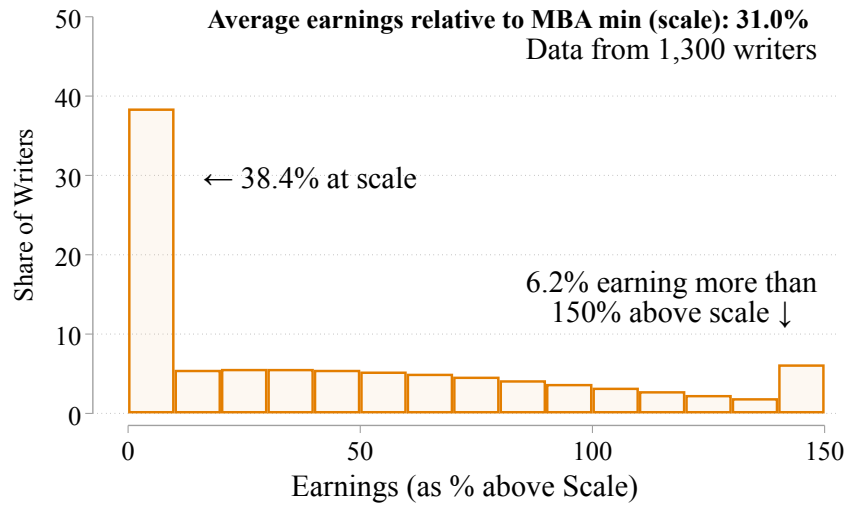
Figure G.2: Heterogeneity in Perception of WGA Demands with Position Fixed Effects



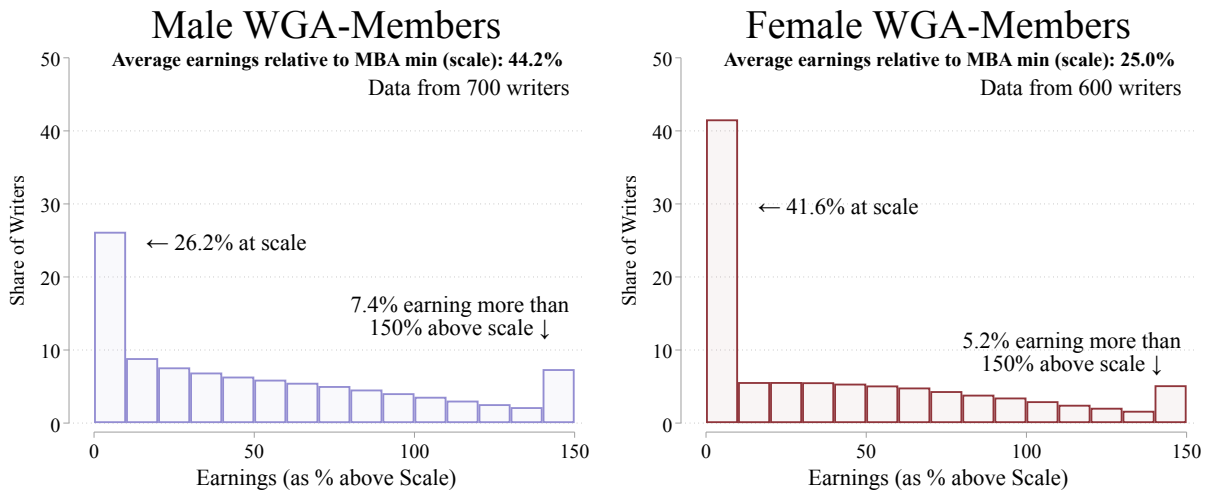
Notes: This figure explores heterogeneity in the effect of the pay report on writers’ perceptions of the WGA’s demands, from the Hollywood experiment described in Section 5. We replicate Figure IX, adding position fixed effects (measured by most recent IMDb credit title). Coefficients report the effect of having seen the pay report on answers to the question “Do most writers think the WGA demands will meet the needs of all WGA members?”, using the 1-5 scale shown in Figure VIII (1 = Not at all, 5 = Almost entirely). The sample is 251 WGA members with available credit, experience, and gender data. Respondents are grouped by three characteristics: (1) *Above Median Credits* vs. *Below Median Credits* based on median IMDb credits among individuals from our WGA contact list with the same most recent credit title (e.g., “Story Editor”); (2) *Above Median Tenure* vs. *Below Median Tenure*, based on median years since first IMDb credit among individuals from our WGA contact list with the same most recent credit title; (3) *Male* vs. *Female*, self-reported or otherwise imputed based on first names as described in Section 5.2. Each regression controls for the other two characteristics, out of the three, that are not being tested. Point estimates are shown with 95% confidence intervals, using robust standard errors. We test the null hypothesis that coefficients are equal across groups and report the difference-in-difference p-values to the right of the plot.

Figure G.3: Proposed Reports

PANEL A: (Mock) Overall Report

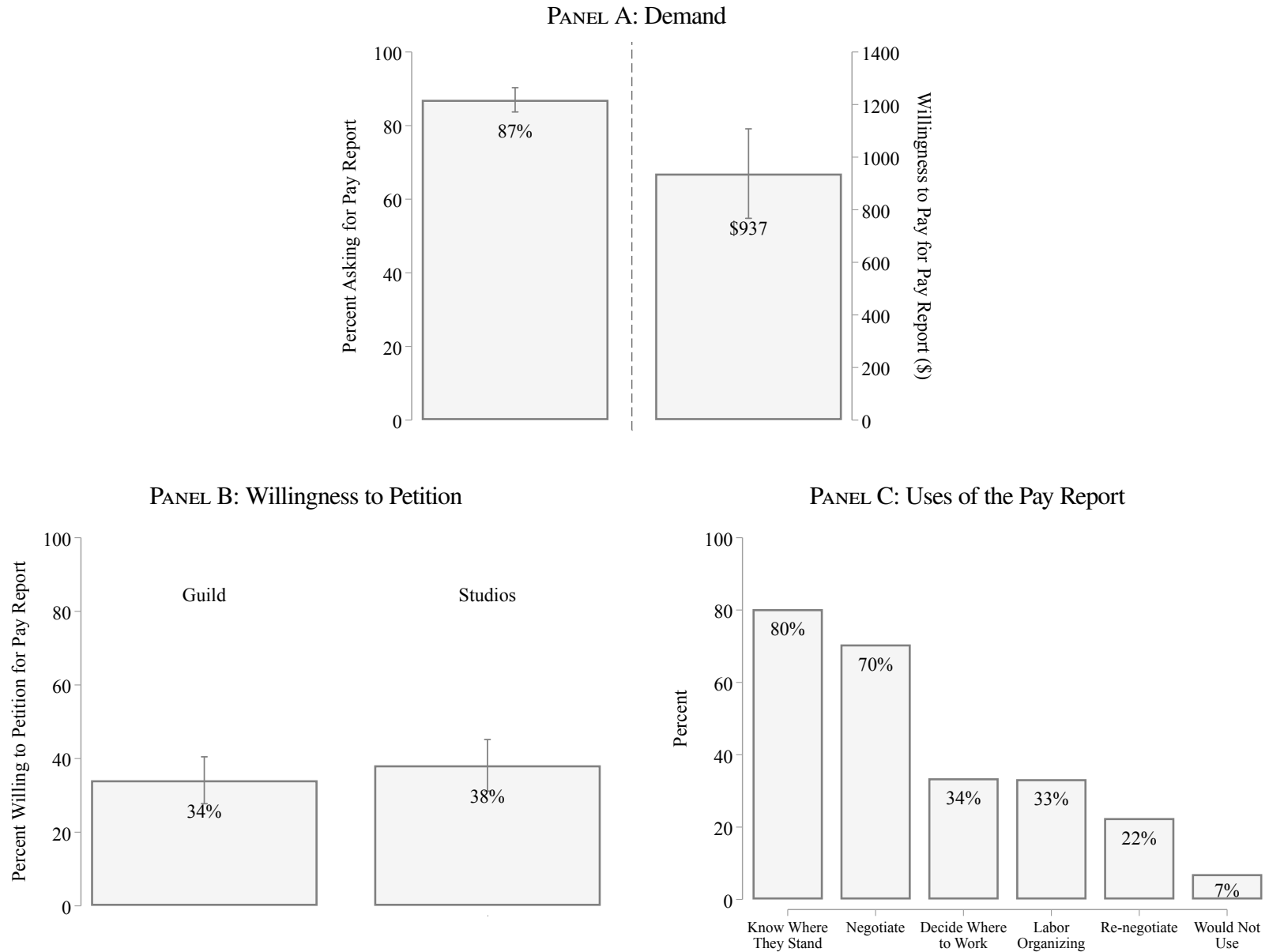


PANEL B: (Mock) Split Pay Report



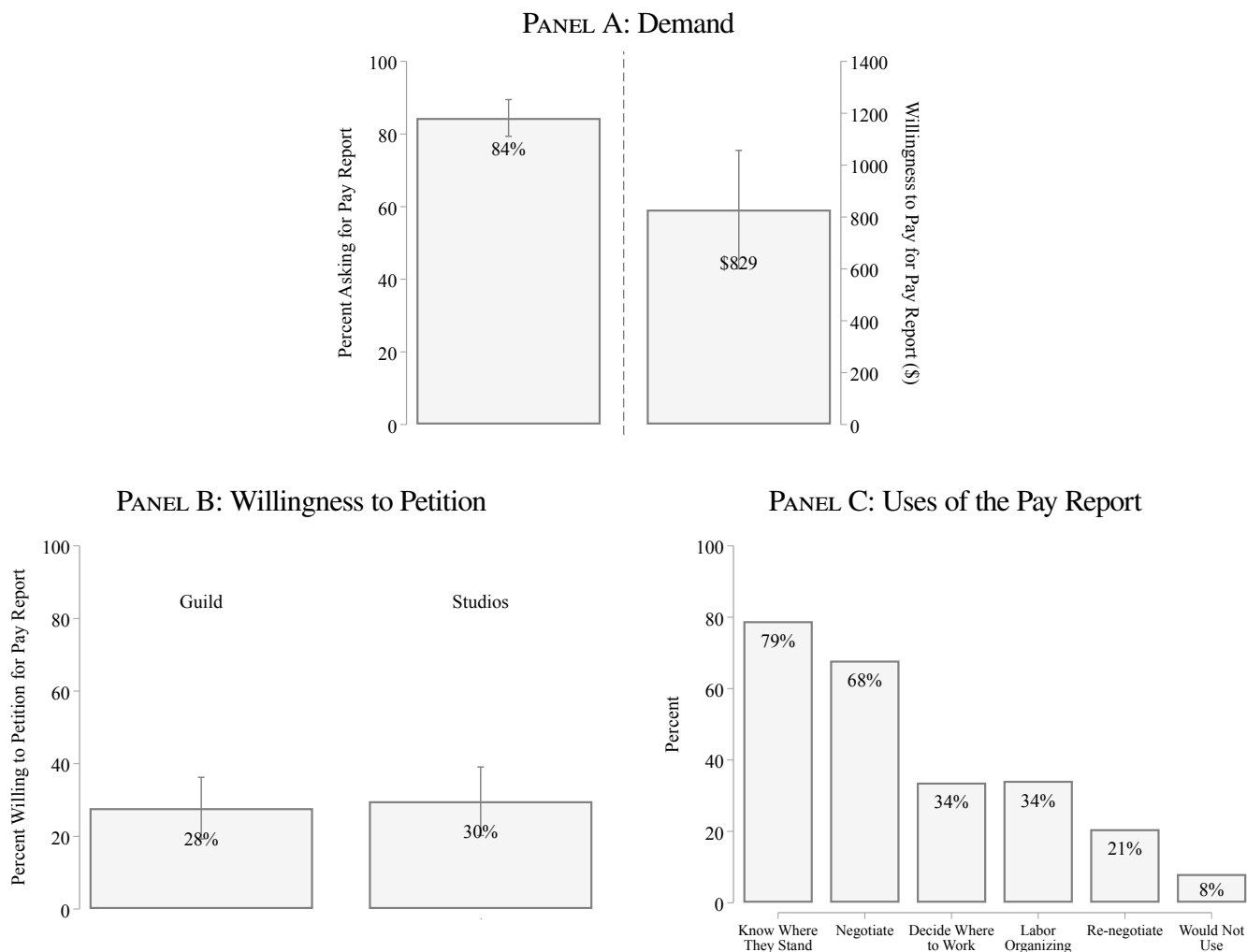
Notes: This figure shows the mock report we included in the baseline survey as an example of what information respondents may receive in the followup, from the Hollywood experiment described in Section 5. This version was shown to film writers; we showed analogous versions to TV writers. The survey included a disclaimer that these reports were made using fake data. For full survey details, see Appendix H.2.

Figure G.4: Baseline Hollywood Survey: Interest in a Pay Report



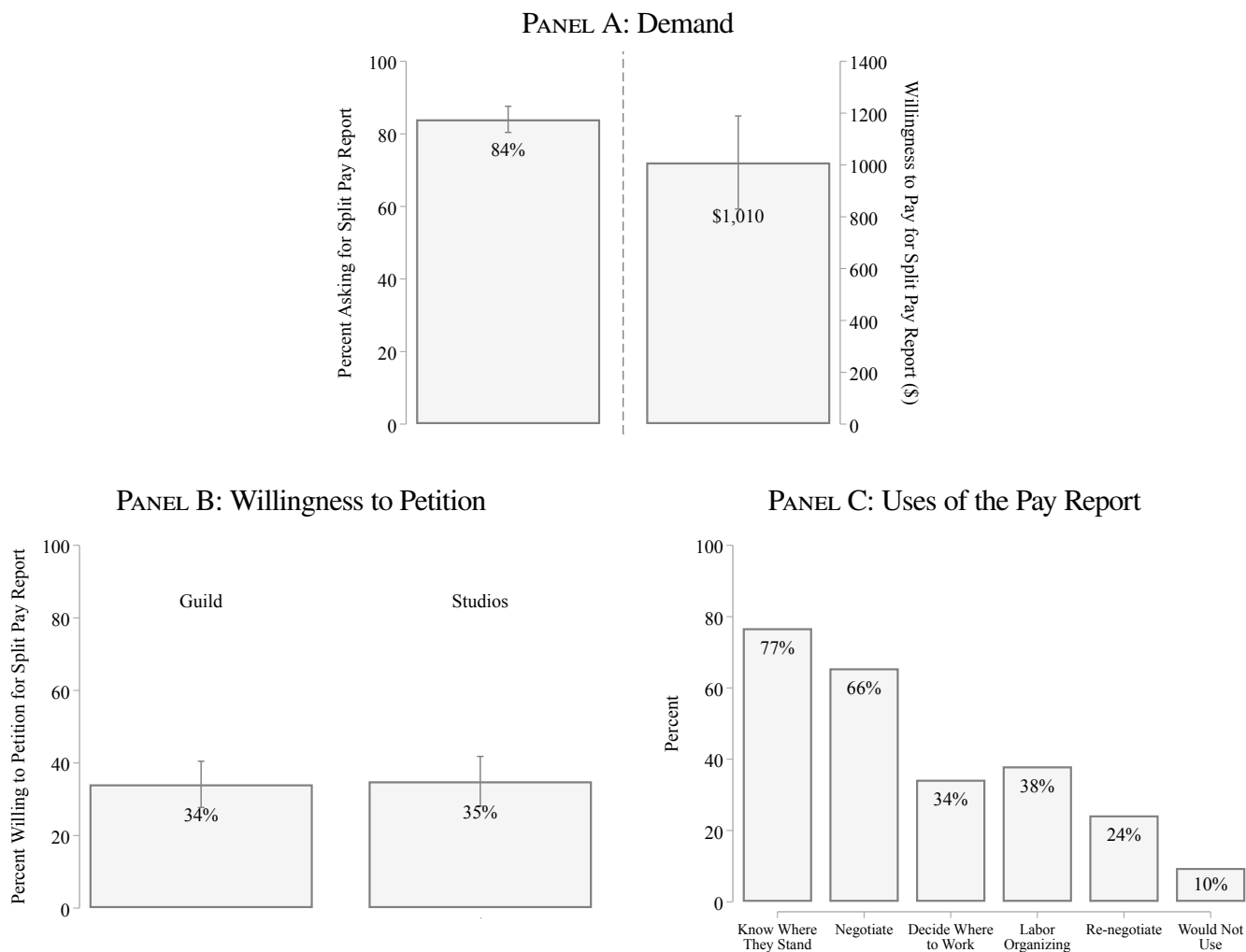
Notes: This figure summarizes writers' interest in and demand for a pay report, from the Hollywood experiment described in Section 5. Panel A shows the share of respondents interested in receiving a pay report and their average willingness to pay, elicited using the incentive-compatible BDM procedure (Becker et al., 1964). Panel B shows the share of respondents willing to publicly petition for pay data from either the WGA or the Networks. Panel C shows respondents' intended uses for a pay report; respondents are allowed to select multiple uses. Estimates are shown with 95% confidence intervals. Sample size is 400 WGA members.

Figure G.5: Baseline Hollywood Survey: Interest in a Pay Report Among Those Who Saw the Overall Pay Report First



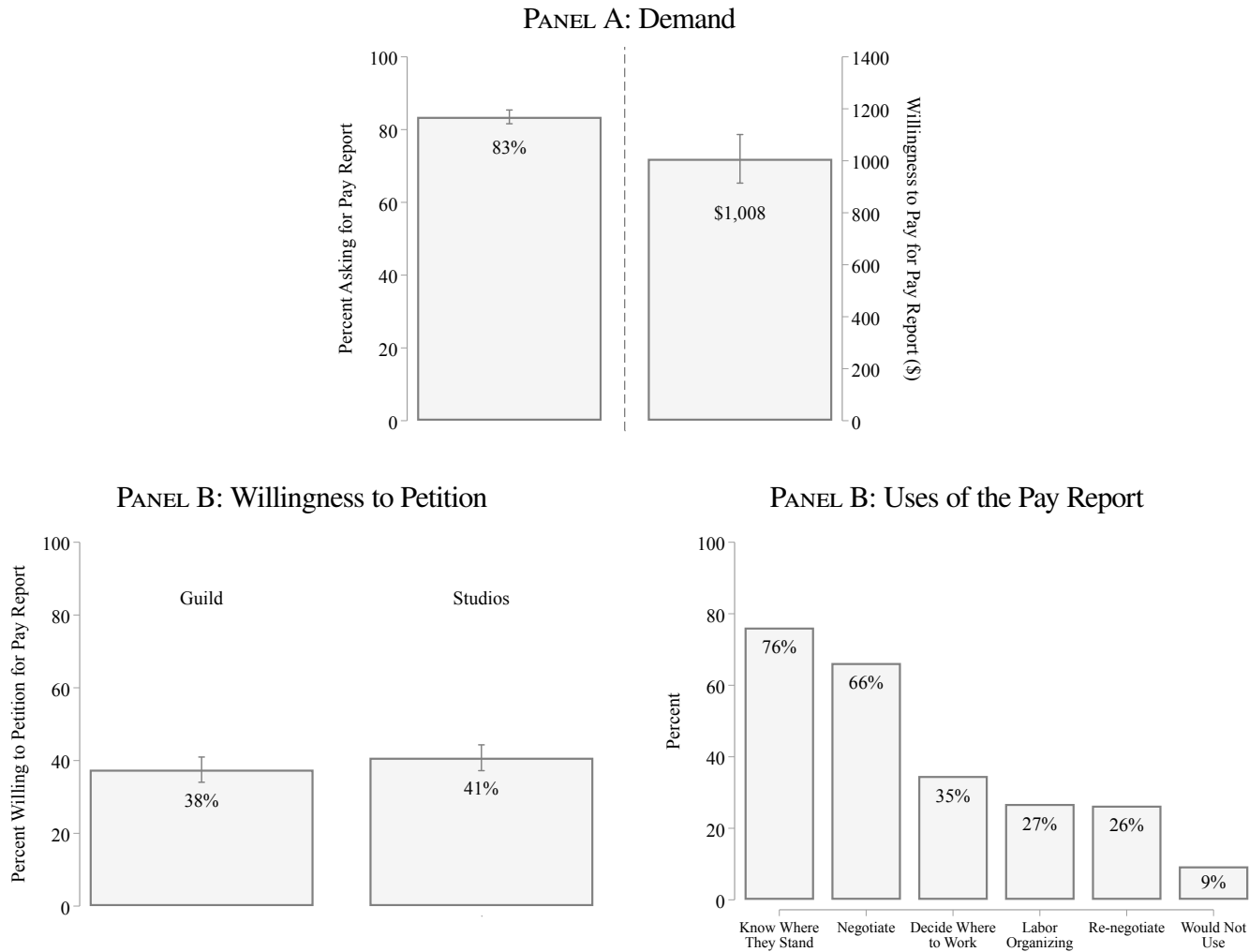
Notes: This figure replicates Figure G.4, restricting to 199 respondents who see the (mock) overall pay report before the (mock) gender-split report. It summarizes writers’ interests and demands for a pay report, from the Hollywood experiment described in Section 5. Panel A shows the share of respondents interested in receiving a pay report and their average willingness to pay. Panel B shows the share of respondents willing to publicly petition for pay data from either the WGA or the Networks. Panel C shows respondents’ intended uses for a pay report, allowing multiple selections. Estimates are shown with 95% confidence intervals.

Figure G.6: Baseline Hollywood Survey: Interest in a Gender-Split Pay Report



Notes: This figure replicates Figure G.4 using questions regarding the gender-split report. It summarizes writers’ interests and demands for a pay report, from the Hollywood experiment described in Section 5. Panel A shows the share of respondents interested in receiving a pay report and their average willingness to pay. Panel B shows the share of respondents willing to publicly petition for pay data from either the WGA or the Networks. Panel C shows respondents’ intended uses for a pay report, allowing multiple selections. Estimates are shown with 95% confidence intervals. Sample size is 400 WGA members.

Figure G.7: Baseline Hollywood Survey: Interest in a Pay Report Among All Writers and Directors



Notes: This figure replicates Figure G.4, expanding the sample to all complete responses, including writers and directors, WGA members and non-WGA members. It summarizes writers' interests and demands for a pay report, from the Hollywood experiment described in Section 5. Panel A shows the share of respondents interested in receiving a pay report and their average willingness to pay. Panel B shows the share of respondents willing to publicly petition for pay data from either the WGA or the Networks. Panel C shows respondents' intended uses for a pay report, allowing multiple selections. Estimates are shown with 95% confidence intervals. Sample size is 1,048 respondents.

H Survey Instruments

H.1 Organizer Survey Instrument

Introduction

We are a team of professors studying worker bargaining power. We want to better understand the barriers organizers are facing to unionizing. We are sending this survey to top organizers and former organizers across the U.S. and Canada.

If you decide to participate, we will share with you our aggregated results so you can access insights on strategies used by other unions.

All individual responses will remain anonymous.

This survey takes 10 minutes. As a token of appreciation, we will transfer you \$30 (as a gift card).

You can only take this survey once.

Here are more details about the study:

Research details:

- Purpose and procedures: You are being invited to participate in a research survey that seeks to understand the factors that facilitate union organizing. The survey takes about 10 minutes to complete.
- Compensation: You will receive a \$30 USD gift card, which will be delivered to you within 10 days of completing the survey.

Confidentiality: Your participation is voluntary. Subjects may decline to answer any or all questions and may decline further participation, at any time, without adverse consequences. Identifiable data linking you to your response (i.e., your email) will be kept in a secure server and will not be made available to anyone other than the researcher. Your email will only be retained for payment purposes and will be deleted following confirmation of payment. De-identified data (meaning that it contains no identifying information about you) may be shared with academic journals as part of their open access policies.

Who to contact with questions or concerns:

Nina Roussille
nroussil@mit.edu

If you feel you have been treated unfairly, or you have questions regarding your rights as a research subject, you may contact the Committee on the Use of Humans as Experimental Subjects at MIT at couhes@mit.edu.

Do you consent to participate?

Yes

No

Eligibility

Q1

Are you, or have you ever been, a union organizer in any capacity?

- I am currently an organizer
- I have been an organizer in the past but am not currently one
- No, I have never been a union organizer

Q2

Condition: Are you, or have you ever been, a union organizer in any capacity? = No, I have never been a union organizer

Are you sure? For the purposes of this study, a union organizer is anyone who has been involved in an effort to organize a workplace or participated in contract renegotiations at a unionized workplace.

- I am currently an organizer
- I have been an organizer in the past but am not currently one
- No, I have never been a union organizer

Vignettes

We are now going to present you with two different hypothetical scenarios. After each scenario, we will ask you a few questions about organizing these workplaces.

Each respondent sees Factory A and Factory B in a randomized order. Each block starts with the Factory A or Factory B introduction, as seen below, and continues with a series of questions that are the same between factories.

Factory A Introduction

Hypothetical scenario: You are trying to get support for the creation of a union at Factory A

Some workers are interested in unionizing, and the employer is known to be hostile to organizers.

To investigate, you've done some research on hourly market pay for workers with similar experience. Workers market rates can vary because local demand for certain skill sets can vary, as well as other reasons. In Factory A, all the workers market rates are approximately \$48. But, the firm pays them \$40. Each assignment makes up one-third of the factory's workers.

The table below shows pay inside and outside the factory.

Assignments	Hourly Wage	Research on Market Wage
Quality control (1/3rd)	\$40	\$48
Metal Worker (1/3rd)	\$40	\$48
Pipefitter (1/3rd)	\$40	\$48

Factory B Introduction

Hypothetical scenario: You are trying to get support for the creation of a union at Factory B

Some workers are interested in unionizing, and the employer is known to be hostile to organizers.

To investigate, you've done some research on hourly market pay for workers with similar experience. Workers market rates can vary because local demand for certain skill sets can vary, as well as other reasons. In Factory B, the workers market rates are varied but everyone is currently paid the same wage. Each assignment makes up one-third of the factory's workers.

The table below shows pay inside and outside the factory.

Assignments	Hourly Wage	Research on Market Wage
Quality Control (1/3rd)	\$40	\$30
Metal Worker (1/3rd)	\$40	\$48
Pipefitter (1/3rd)	\$40	\$66

Q3

In the absence of more information, workers generally think that everyone earns what they do for similar work. You have the option to share the pay data you collected with workers.

Would you share this pay information with workers?

Order of choices randomly flipped

- Yes, I would publish the pay information **during** the union campaign

- Yes, I would publish the pay information **after** the union campaign
- No, I would not publish the pay information

Q4

Now assume all the wage data become known to all workers.

What hourly wage increase (in the first year of the new contract) would each of the following groups demand for themselves in order to ratify the CBA?

	+\$0 (no increase)	+\$2	+\$4	+\$6	+\$8	+\$10	+\$12	+\$14	+\$16	+\$18	+\$20 or more
Quality Control	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Metal Worker	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pipefitter	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Q5

How easy would it be for all workers to agree on pay scale demands?

For instance, this would imply that Metal Workers approve of the pay raises demanded by Pipefitters, and vice versa.

- Difficult
- Somewhat difficult
- Somewhat easy
- Easy

Q6

What share of each of the following groups at Factory {A/B} do you think will apply for a job elsewhere during the union campaign?

	0	10	20	30	40	50	60	70	80	90	100
Quality Control	<input type="text"/>										
Metal Worker	<input type="text"/>										
Pipefitter	<input type="text"/>										

Q7

What hourly wage increase (in the first year of the new contract) could you reasonably expect the employer to agree on?

	+\$0 (no increase)	+\$2	+\$4	+\$6	+\$8	+\$10	+\$12	+\$14	+\$16	+\$18	+\$20 or more
Quality Control	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Metal Worker	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pipefitter	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Q8

Please share your best guess: After all the details are hammered out, what percent of workers at Factory {A/B} would you expect to pro-actively vote yes to ratify a contract?

	0	10	20	30	40	50	60	70	80	90	100
Percent of Workers	▽										

Q9

At this factory, the employer does not provide the workers with predictable hours.

The employer also has thus far refused to sign the Open to All business pledge: a commitment to maintaining a welcoming and safe environment for people — including team members, visitors, customers, vendors and clients — regardless of race, ethnicity, national origin, sex, sexual orientation, gender identity and expression, immigration.

To maximize support for the union, which of the following issues would be better to focus on during the campaign?

Options presented in a randomized order

- Raising pay
- Guaranteed predictable hours
- Signing the Open to All pledge (a commitment to maintaining a welcoming and safe environment)

Q10

Would you advise separate bargaining units for these three groups of workers?

- Yes
- No

Factory Comparison

Q11

With limited resources, which factory would you attempt to organize?

We will direct a significant donation to an organization (not participating in this survey) focused on organizing a workplace closest to Factory A or Factory B, based on the answers we receive.

- Factory A
- Factory B

Q12

You told us that you would attempt to organize Factory {Q11 ANSWER} before {Q11 UNSELECTED}.
Could you explain your reasoning?

Q13

When asked what you would prioritize (Raising pay, Guaranteed predictable hours, or Signing the Open to All pledge), you told us you would prioritize {Q9 ANSWER, FACTORY A} at A and {Q9 ANSWER, FACTORY B} at B.

Could you explain your reasoning?

Descriptive Questions

Q14

Do you agree with this statement? One central reason workers seek out unions is for greater pay transparency

- Agree
- Somewhat agree
- Somewhat disagree
- Disagree

Q15

Consider the workplace that you are currently organizing or most recently organized.

Which of the following best describes that organizing effort?

- Organizing a new union
- Organizing an existing union

Q16

What was the year in which you most recently tried to organize a workplace?

Choice: FROM Before 1990, 1990 ... TO 2023, 2024

Q17

What was the industry?

Choice: NAICS 2-digit industries descriptions (e.g., Agriculture, Forestry, Fishing and Hunting, Retail Trade)

Condition: Text depends on {Q1 ANSWER} and {Q18 ANSWER}

For the following questions, please consider the {workplace/union} you {currently organize/most recently organized}.

Order of the following 2 sections randomized

Worker Priority

Q18

When thinking about the amenities of the job in the workplace you currently organize or most recently organized, how important was pay to workers?

If helpful, you can think back to any worker survey you ran at the time.

- The top priority
- A secondary priority
- A tertiary priority
- Not in the top 3 priorities

Campaign Descriptives

Q19

What were the **three main issues** the union focused on during the campaign?

- Union Recognition
- Health and Safety/PPE
- Forced Overtime
- Wages
- Respect and Dignity
- Fairness
- Health Insurance
- Pensions

- Discrimination
- Too many hours
- Not enough hours
- Working Conditions
- Power
- Representation
- Just Cause
- Grievance
- Scheduling
- Contract
- Service and Product Quality
- Job Security
- Staffing
- Training
- Technological Change
- Surveillance
- Sexual Harassment
- Broken Promises
- Employer Behavior
- Paid Time Off
- Paid Leave
- Childcare
- Pay Transparency
- Immigrant Rights
- Promotional Opportunity
- Voice
- Tuition
- Other: _____

Q20

Please rank three main issues you selected from most important (1) to least important (3).
Choices: Issues selected in{Q19}

Q21

Condition: Text depends on{Q15 ANSWER}

Did you collect pay information relevant to{workers/union members}?

- Yes
- No

Q22

Condition:{Q21 ANSWER} = Yes

How do you access pay information?

This could be information about the pay of{workers/union members} or those at similar establishments.

Q23

Condition:{Q21 ANSWER} = Yes

Did you publish all the anonymized pay data you collected? (eg. report in a newsletter, publish on website, membership-wide email)

- Yes
- No

Q24

What (if any) pay information did you communicate?

Q25

How many years have you worked as an organizer?

Choice: FROM Less than 1 year, 1 year ... TO 9 years, 10+ years

Q26

Do you have experience as the lead organizer on a campaign?

- Yes
- No

Q27

Which country do you have more experience working in?

- U.S.
- Canada

Conclusion

Q28

Please share any feedback, comments, or questions here, or let us know if anything in the survey was unclear.

H.2 Hollywood Guilds Survey Instrument

Introduction

We are a team of professors from Harvard, MIT, and UBC, with expertise on negotiation. We are considering producing a report pertaining to writers' & directors' career negotiations at every level. Particularly during this historic renegotiation, we want to understand how providing currently inaccessible information may affect you for better or worse.

We will use answers to this 10-minute survey to decide whether to pursue this project and whether to send you the report. All responses will be stored on a secure server and your name will never be released unless you indicate otherwise.

By clicking "Yes" below, you consent to participate in the survey.

Do you want to participate?

Here is some key information about the study:

- We are asking you to take part in a research study because you might be a writer or director, or part of the directing team.
- If you agree to be in this study you will be asked to complete a 10-minute online survey.
- We don't believe there are any risks from participating in this research. All responses will be stored on a secure server and your name will never be released. The study would only use aggregate data.

- We cannot promise any benefits to others from your taking part in this research. However, possible benefits to you include helpful career information.
- Your participation is completely voluntary. You can choose not to participate, or you can agree to participate and change your mind later and your decision will not be held against you. Your refusal to participate will not result in any consequences or any loss of benefits that you are otherwise entitled to receive.
- The identified data collected in this survey and IMDb will be exclusively shared among the co-PIs on the study team.

If you have any concerns or complaints about your rights as a research participant and/or your experiences while participating in this study, contact the Research Participant Complaint Line in the UBC¹⁰ Office of Research Ethics at 604-822-8598 or if long distance e-mail RSIL@ors.ubc.ca or call toll free 1-877-822-8598. Taking part in this survey is entirely up to you. If you decide to take part, you may choose to stop filling the survey at any time.

- Yes, I want to participate

Introductory Questions

First, we have some questions to determine which report would be most relevant for you.

Q1

Which roles have you worked in? Select all that apply.

- Writer
- Director or Directing Team
- Producer
- Actor
- Other

Q2

Do you primarily work in TV or film?

- TV
- Film

¹⁰A previous co-author was at UBC hence the UBC IRB language here and in survey recruitment messaging.

Q3

Which type of studio provides a more important source of income for you?

- Streaming service
- Traditional studio

Q4

Condition: If Which roles have you worked in? Select all that apply. = Director or Directing Team

Which type of directing job is most relevant for your income?

- Director
- Unit Production Manager
- First Assistant Director
- Key Second Assistant Director
- 2nd Second Assistant Director
- Additional Second Assistant Director
- Associate Director

Q5

*Condition: If Do you primarily work in TV or film? = TV
And Which roles have you worked in? Select all that apply. = Writer*

Which type of writing job title is most relevant for your income?

- Staff Writer
- Story Editor
- Executive Story Editor
- Co-producer
- Producer
- Co-executive Producer
- Showrunner

Q6

Condition: If Do you primarily work in TV or film? = TV

And Which roles have you worked in? Select all that apply. = Writer

Which type of pay structure for TV writing provides a more important source of income for you?

- Weekly
- Episodic

Q7

Condition: If Do you primarily work in TV or film? = Film

And Which roles have you worked in? Select all that apply. = Writer

Which type of film provides a more important source of income for you?

- Low budget
- High budget

Block: {Film Writer/TV Writer/Director}

Conditional on {Q1 ANSWER} and {Q2 ANSWER} there will be different phrasing of the questions based on position, {Film Writer/TV Writer/Director}, and some additional questions for TV Writers and Directors.

As an example, questions Q8-Q27 are phrased in this sample for Film Writers (also referred to as screenwriters), but there were analogous versions for TV writers and directors.

For Film Writers, we use {Q7 ANSWER} for questions about earnings.

For TV Writers, we use a combination of {Q5 ANSWER} and {Q6 ANSWER} for questions about earnings.

For Directors, we use {Q4 ANSWER} for questions about earnings.

We randomize the order in which the two pay reports (overall vs. split) are displayed. This means half of the sample see questions Q8-Q13 and EQ1-EQ2 first (about the overall report) followed by Q14-Q17 and EQ3-EQ4 (about the split report), and the other half see the opposite order.

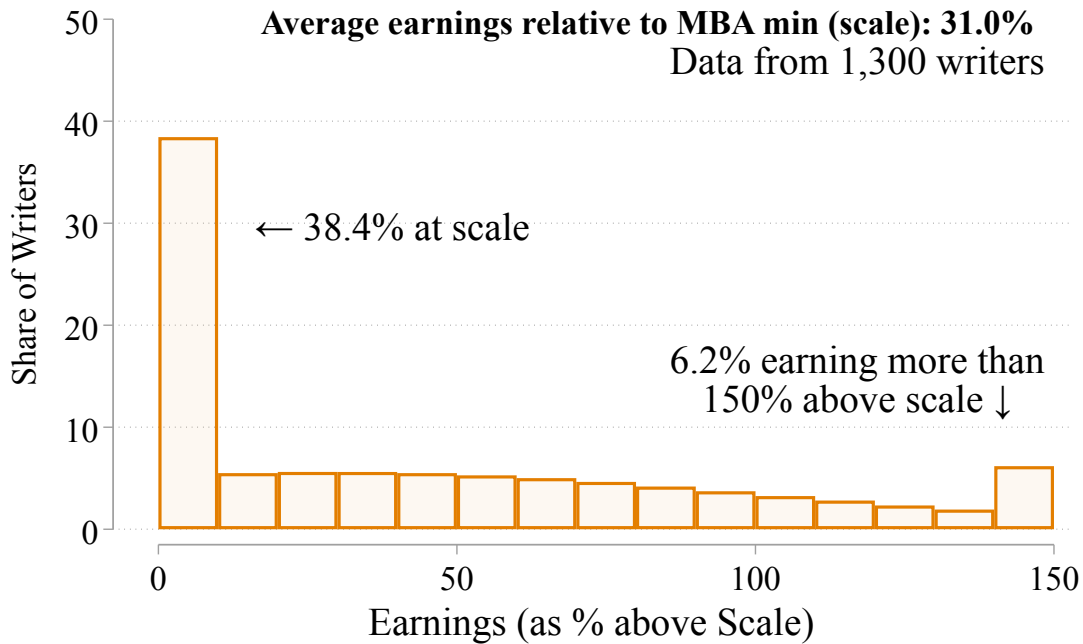
Q8

We will ask you about two reports. Please consider the proposed report showing earnings in the screenwriting industry based on screenwriter contracts in the past year.

Here is an example of what we would report about screenwriters:

WGA-Member

Compensation Relative to MBA Minimums for {Q7 ANSWER} Screenplays at {Q3 ANSWER}s (2023)



(Note: this example uses fake data.)

Do you think we should create such report? The report would be shared with other screenwriters, and we will use your opinion to decide whether to pursue this project. Your response will be strictly confidential.

- Yes, I would value it significantly
- Yes, I would be interested to see it
- Neutral, I would not pay too much attention.
- No, I would not be interested in such a report.
- No, such a report would be harmful to me.

Q9

Instead of creating a report, we could use the same resources to transfer rewards to survey respondents.

Below, we will ask you about 5 hypothetical scenarios. In each scenario, you will be presented a choice between accessing the report on pay OR receiving money.

We will randomly choose 10 survey respondents. If you are one of these 10 lucky respondents, we will randomly select one of your 5 choices to send to you if the report is produced.

As a result, it is in your best interest to respond honestly to these scenarios. Please make your choices below, and at the end of the survey you will find out if you are selected.

Between the following two options in each scenario, which one would you prefer?

	Receive Pay Report	Receive Cash
Pay report or \$25 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$150 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$500 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$2000 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$6000 Cash Payment	<input type="radio"/>	<input type="radio"/>

Q10

How would you use the report if it were published? Select all that apply. If a reason is not included, please describe it in the “other” option.

Options presented in a randomized order

- To personally know where I stand in the pay distribution
- To decide where to work
- To negotiate new contracts
- To re-negotiate existing contracts
- For labor organizing
- I wouldn't use it
- Other

Q11

Condition: If How would you use the report if it were published? Select all that apply. If a reason is not incl... = I wouldn't use it

For which reasons do you think this information would have limited scope? Select all that apply. If a reason is not included, please describe it in the “other” option.

Options presented in a randomized order

- I already can access the information about pay that I need
- Reports like this do not affect compensation or employment
- The proposed report is missing crucial information (e.g., demographics)
- Other

Q12

Condition: randomized treatment – half of the sample see a question about Guild, half of the sample see a question about Networks

You previously said{Q8 ANSWER} about a report on overall screenwriter pay distributions.

We are considering sending a petition to the{Guild/Networks} that would ask for the data they have on pay to complement our own pay data collection for the purpose of understanding and reporting on overall pay.

Would you allow us to include your name and private answer above as part of this public petition?

Yes

No

Q13

Condition: {Q12 ANSWER} = No

What is the main reason why you would refuse to sign this petition?

EQ1 (TV Writers Only)

Condition: only shown one of two bracketed questions

{Which of the position titles below should we produce a report for? So far we have considered gathering data on{Q5 ANSWER}s. But we could produce a similar report for any of the other positions listed below, and we are trying to decide which of these positions the report should cover. The report would be accessible to all. We will use your opinion to decide which reports to create.}

OR

{Which other groups of writers should receive a similar report about their position-level earnings? We will use your opinion to decide which reports to create.}

Select all that apply.

Condition: If Which type of writing job title is most relevant for your income? != Staff Writer

Staff writers

Condition: If Which type of writing job title is most relevant for your income? != Story Editor

Story editors

Condition: If Which type of writing job title is most relevant for your income? != Executive Story Editor

Executive story editors

Condition: If Which type of writing job title is most relevant for your income? != Co-producer

- Co-producers

Condition: If Which type of writing job title is most relevant for your income? != Producer

- Producers

Condition: If Which type of writing job title is most relevant for your income? != Co-executive Producer

- Co-executive producers

Condition: If Which type of writing job title is most relevant for your income? != Showrunner

- Showrunners

EQ2 (Directors Only)

Condition: only shown one of two bracketed questions

{Which of the position titles below should we produce a report for? So far we have considered gathering data on{Q4 ANSWER}s. But we could produce a similar report for any of the other positions listed below, and we are trying to decide which of these positions the report should cover. The report would be accessible to all. We will use your opinion to decide which reports to create.}

OR

{Which other groups of the directing team should receive a similar report about their position-level earnings? We will use your opinion to decide which reports to create.}

Select all that apply.

Condition: If Which type of directing job is most relevant for your income? != Director

- Directors

Condition: If Which type of directing job is most relevant for your income? != Unit Production Manager

- Unit Production Managers

Condition: If Which type of directing job is most relevant for your income? != First Assistant Director

- First Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Key Second Assistant Director

- Key Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != 2nd Second Assistant Director

- 2nd Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Additional Second Assistant Director

- Additional Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Associate Director

- Associate Directors

Q14

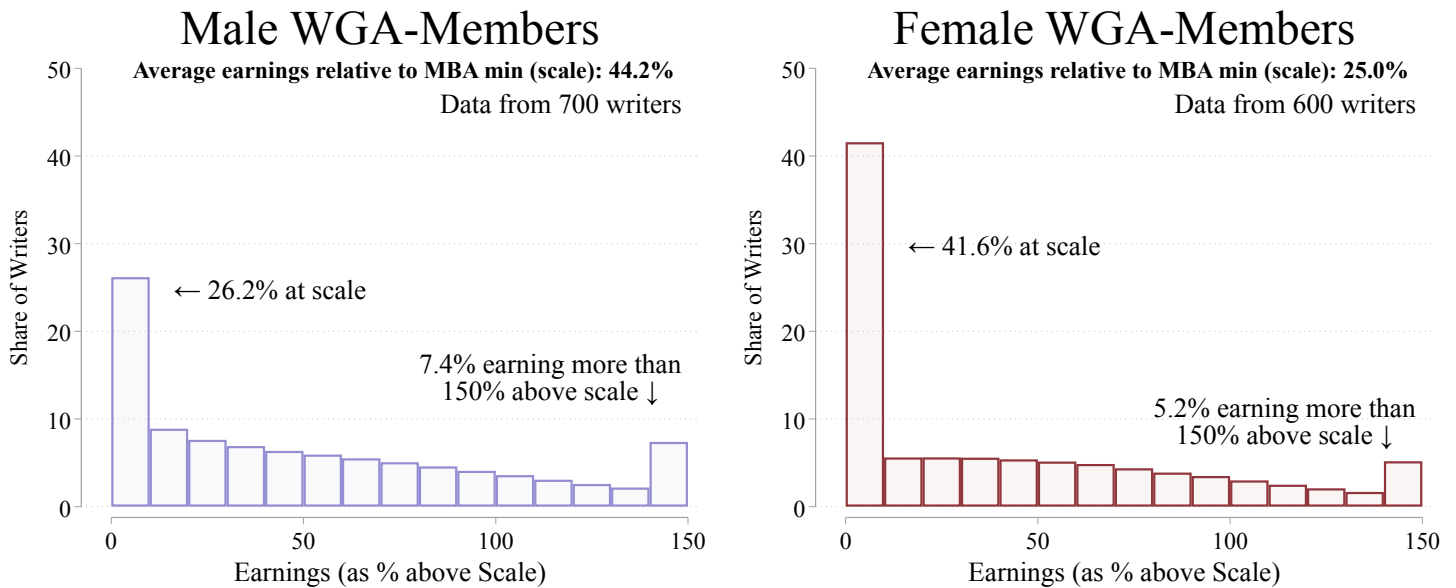
We would like to ask you the same questions about one alternative report.

We can create a different report displaying pay distributions separately for each gender, using the same data on recent contracts.

Here is an example of what we would report about screenwriters:

WGA-Member

Compensation Relative to MBA Minimums for {Q7 ANSWER} Screenplays at {Q3 ANSWER}s (2023)



(Note: this example uses fake data for each graph. To preserve anonymity, we would include non-binary individuals in both distributions.)

Do you think we should create such report? The report would be shared with other screenwriters, and we will use your opinion to decide whether to pursue this project. Your response will be strictly confidential.

- Yes, I would value it significantly
- Yes, I would be interested to see it
- Neutral, I would not pay too much attention.
- No, I would not be interested in such a report.
- No, such a report would be harmful to me.

Q15

Below you are presented with 5 more hypothetical scenarios, now about the pay disparities report.

Between the following two options in each scenario, which one would you prefer?

	Receive Pay Report	Receive Cash
Pay report or \$25 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$150 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$500 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$2000 Cash Payment	<input type="radio"/>	<input type="radio"/>
Pay report or \$6000 Cash Payment	<input type="radio"/>	<input type="radio"/>

Q16

How would you use the report if it were published? Select all that apply. If a reason is not included, please describe it in the “other” option.

Options presented in a randomized order

- To personally know where I stand in the pay distribution
- To decide where to work
- To negotiate new contracts
- To re-negotiate existing contracts
- For labor organizing
- I wouldn't use it
- Other

Q17

Condition: If How would you use the report if it were published? Select all that apply. If a reason is not incl... = I wouldn't use it

For which reasons do you think this information would have limited scope? Select all that apply. If a reason is not included, please describe it in the “other” option.

Options presented in a randomized order

- I already can access the information about pay that I need
- Reports like this do not affect compensation or employment
- The proposed report is missing crucial information
- It would detract from the purpose of collective bargaining
- Other

EQ3 (TV Writers Only)

Condition: only shown one of two bracketed questions

{Which of the position titles below should we produce this report on pay disparities for? So far we have considered gathering data on{Q5 ANSWER}. But we could produce a similar report on pay disparities for any of the other positions listed below, and we are trying to decide which of these positions the report should cover. The report would be accessible to all. We will use your opinion to decide which reports to create.}

OR

{Which other groups of writers should receive a similar report about their position-level earnings? We will use your opinion to decide which reports to create.}

Select all that apply.

Condition: If Which type of writing job title is most relevant for your income? != Staff Writer

- Staff writers

Condition: If Which type of writing job title is most relevant for your income? != Story Editor

- Story editors

Condition: If Which type of writing job title is most relevant for your income? != Executive Story Editor

- Executive story editors

Condition: If Which type of writing job title is most relevant for your income? != Co-producer

- Co-producers

Condition: If Which type of writing job title is most relevant for your income? != Producer

- Producers

Condition: If Which type of writing job title is most relevant for your income? != Co-executive Producer

- Co-executive producers

Condition: If Which type of writing job title is most relevant for your income? != Showrunner

- Showrunners

EQ4 (Directors Only)

Condition: only shown one of two bracketed questions

{Which of the position titles below should we produce this report on pay disparities for? So far we have considered gathering data on{Q4 ANSWER}. But we could produce a similar report on pay disparities for any of the other positions listed below, and we are trying to decide which of these positions the report should cover. The report would be accessible to all. We will use your opinion to decide which reports to create.}

OR

{Which other groups of the directing team should receive a similar report about their position-level earnings? We will use your opinion to decide which reports to create.}

Select all that apply.

Condition: If Which type of directing job is most relevant for your income? != Director

Directors

Condition: If Which type of directing job is most relevant for your income? != Unit Production Manager

Unit Production Managers

Condition: If Which type of directing job is most relevant for your income? != First Assistant Director

First Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Key Second Assistant Director

Key Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != 2nd Second Assistant Director

2nd Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Additional Second Assistant Director

Additional Second Assistant Directors

Condition: If Which type of directing job is most relevant for your income? != Associate Director

Associate Directors

Q18

Condition: randomized treatment – half of the sample see a question about Guild, half of the sample see a question about Networks

You previously said {Q14 ANSWER} about a report on screenwriter pay distributions by gender.

We are considering sending a petition to the {Guild/Networks} that would ask for the data they have on pay to complement our own pay data collection for the purpose of understanding and reporting on pay disparities.

Would you allow us to include your name and private answer above as part of this public petition?

Yes

No

Q19

Condition: {Q18 ANSWER} = No

What is the main reason why you would refuse to sign this petition?

Q20

What do you think is the most relevant MBA minimum for a typical screenwriter in the Guild writing {Q7 ANSWER} films at {Q3 ANSWER}s in the first half of 2023, in dollars?

\$ _____

Q21

What percent above the MBA minimum do you think a typical screenwriter in the Guild writing {Q7 ANSWER} films earns at a {Q3 ANSWER} for one script in the first half of 2023?

Percent above the MBA minimum

Choice: FROM Less than the minimum, 0% more (at the minimum), 1% more, ... TO 100% more

Q22

During your most recent project, do you think you earned a higher, lower, or the same percent above the MBA minimum as the typical screenwriter in the Guild writing {Q7 ANSWER} films working at a {Q3 ANSWER}?

- Higher
- The same
- Lower

Q23

How confident are you in your knowledge of what the typical screenwriter writing for {Q3 ANSWER} in the Guild earns?

- Not confident at all
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Q24

What percent above the MBA minimum do you typically earn writing {Q7 ANSWER} films at {Q3 ANSWER}s for one script in 2023?

Percent above the MBA minimum

Choice: FROM Less than the minimum, 0% more (at the minimum), 1% more, ... TO 100% more

Q25

Among screenwriters in the Guild writing {Q7 ANSWER} films working at {Q3 ANSWER}s, do you think women earn a higher, lower, or the same percent above the MBA minimum as men?

- Higher
- The same
- Lower

Q26

Condition: If Among screenwriters in the Guild writing {Q7 ANSWER} films working... = {Lower/Higher}

You said that you think that among {Q7 ANSWER} films, women earn a {Lower/Higher} percent above the MBA minimum than men.

What percent more do {men/women} make than {women/men}?

Percent more

Choice: FROM 1% more – TO 100% more or more

Q27

How confident are you in your knowledge of the differences in pay between men and women screenwriters in the Guild?

- Not confident at all
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Concluding Questions

Q28

We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demographics we are considering.

Which, if any, breakdowns would you be interested in seeing? Select all that apply.

- Gender
- Race
- Age
- LGBTQ+
- Main genre
- Networks/streaming services
- Other
- None of the above

Q29

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... != None of the above

And Q28 has more than 1 selected options

Please rank the demographics breakdowns you selected above by dragging them from the one you would be most interested in seeing at the top of the list to the one you would be the least interested in seeing at the bottom of the list.

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Gender

- Gender

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Gender

- Gender

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Race

- Race

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Age

- Age

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = LGBTQ+

- LGBTQ+

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Main genre

- Main genre

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Networks/streaming services

- Networks/streaming services

Condition: If We could include a breakdown of pay by other demographics, e.g., race. Below is a list of the demo... = Other

- Other

Q30

Condition: If Do you primarily work in TV or film? = TV

Out of all the episodes you've worked on in the last year, for what percent did you receive formal credit? (0% = none, 100% = all)

	0	10	20	30	40	50	60	70	80	90	100
Percent of episodes	▽										

Q31

Condition: If Do you primarily work in TV or film? = Film

Out of all the films you've worked on in the last year, for what percent did you receive formal credit? (0% = none, 100% = all)

	0	10	20	30	40	50	60	70	80	90	100
Percent of films	▽										

Q32

How would a report on true pay distributions by gender compare to the current views of producers & pay-setting executives?

- It would reveal more pay inequality than they expect
- It would reveal less pay inequality than they expect
- It would be the same pay inequality as what they expect

Q33

What is your gender identity?

- Man
- Woman
- Non-binary
- Other

Q34

How would you describe your race/ethnicity? (Check all that apply)

- American Indian or Alaska Native
- Asian
- Black or African American
- Native Hawaiian or Other Pacific Islander
- White
- Hispanic/Latino
- Other
- Prefer not to disclose

Q35

Condition: If Which roles have you worked in? Select all that apply. = Writer

Are you a member of the WGA?

- Yes
- No

Q36

Condition: If Which roles have you worked in? Select all that apply. = Director or Directing Team

Are you a member of the DGA?

- Yes
- No

Q37

Condition: If Are you a member of the WGA? = Yes

Do you hold an official role in the WGA?

- No
- Yes, Committee Chair/Vice Chair
- Yes, Committee Member
- Yes, Involved but no official position
- Yes, Prior leadership role
- Yes, Other

Q38

Condition: If Are you a member of the DGA? = Yes

Do you hold an official role in the DGA?

- No
- Yes, Committee Chair/Vice Chair
- Yes, Committee Member
- Yes, Involved but no official position
- Yes, Prior leadership role
- Yes, Other

Q39

Condition: If Which roles have you worked in? Select all that apply. = Writers

Is the WGA fully forthcoming with their information of value to you?

- Yes
- No
- Not sure

Q40

Condition: If Which roles have you worked in? Select all that apply. = Director or Directing Team

Is the DGA fully forthcoming with their information of value to you?

- Yes
- No
- Not sure

Q40

*Condition: If Is the WGA fully forthcoming with their information of value to you? = No
Or Is the DGA fully forthcoming with their information of value to you? = No*

What information do they collect that would be useful to make available?

Feedback

Thank you for finishing this survey. Please leave any feedback you have here.

Compensation

Would you prefer a \$20 Amazon gift card or a donation of your choice as a token of appreciation for your time?

If you select donation, please specify which organization you would like us to donate to.

- \$20 Amazon gift card
- \$20 donation

Randomization Outcome

Condition: For participants who were not randomly selected

According to the randomization procedure, any hypothetical questions you answered during this survey will remain hypothetical.

Condition: For 10 randomly selected participants

You have been randomly selected among the 10 participants who will have one of their scenarios implemented. We will be in touch once a determination has been made about the report and no later than July 15th.