Why Join the Union?
Free-riding and Gift-exchange among Health Care Workers

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Abstract

Many collective bargaining systems create a fundamental free-rider problem: union-negotiated benefits are non-excludable, yet union financing depends on voluntary worker contributions. We study what sustains union membership in this setting, focusing on the Minnesota home care sector. Using administrative data on the universe of union-covered workers from 2018–2024, we implement a difference-in-differences event-study design and show that union membership rises sharply around contract-driven wage increases. A 25% increase in the negotiated wage floor leads to a 26% increase in membership among workers experiencing the largest wage gains. We provide evidence that this response reflects gift exchange: only contract-driven wage gains increase membership, ruling out income effects, and the response does not vary with how the raise changes workers' dues, ruling out price effects. A randomized outreach experiment confirms that union contact boosts engagement and membership, but membership gains are concentrated among workers with larger wage increases. Embedding these effects in a model of union public good financing, we show that gift-exchange allows the union to internalize non-members' interests, but not enough to rationalize existing dues rates as optimal for members alone.

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1 Introduction

In many collective bargaining systems, negotiated wages and working conditions extend to all covered workers, regardless of individual union membership (Jäger et al., 2025). In the United States, although bargaining typically occurs at the workplace level, labor law obliges unions to represent every worker in the bargaining unit while restricting the collection of fees from non-members in right-to-work states and the public sector. Growing interest in U.S. sectoral bargaining would extend this dynamic from individual workplaces to entire industries (Dube, 2026).[1] This non-excludability creates a fundamental free-rider problem: all workers benefit from collectively bargained wage floors, but unions must finance themselves through voluntary dues and often cannot offer selective material benefits to promote membership.[2] How unions can sustain voluntary membership and revenue remains an open question.

This paper examines the role of social preferences in sustaining union membership in voluntary settings. Workers may respond to union-secured wage gains with reciprocity, contributing dues because they attribute those gains to the union's bargaining efforts, a form of gift exchange (Fehr and Charness, 2025). Under this view, union membership is not merely a private cost-benefit calculation, but reflects a social preference to support the institution responsible for one's material gains, even when free-riding remains available. Identifying this mechanism requires distinguishing reciprocity from several plausible alternative explanations: workers may simply pay dues out of higher disposable income, as in classic models of charitable giving (Andreoni and Payne, 2013); they may be sensitive to the relative cost of dues, which are often capped as a share of earnings (Barth et al., 2025); or membership may be sustained through repeated interactions and social pressure (Ostrom, 1999; Booth, 1985).

We study these questions in the context of home care aides in Minnesota—workers who assist elderly and disabled individuals with daily living activities in their homes. Home care is among the fastest growing occupations in the U.S., driven in part by increased Medicaid funding for home and community-based care (Shen, 2024; Gottlieb et al., 2025). In Minnesota, a union has represented home care workers employed directly by consumers through publicly-funded programs since 2014, covering roughly 30% of all home care workers in the state. The union negotiates contracts that apply to all covered workers regardless of membership, but following *Harris v. Quinn* (2014), cannot collect fees from non-members, making free-riding individually rational. In 2023, the union negotiated a 25% increase in the sectoral wage floor from $15.25 to $19 per hour effective January 1, 2024. Importantly, the wage floor increase was paired with a legislatively funded increase in the Medicaid reimbursement rate, limiting the scope for reductions in total home care employment in the covered sector.

In partnership with the union, we leverage confidential administrative records covering the universe of union-represented home care workers from 2018–2024 to evaluate the deter-

[1] Recent examples of experimentation with sectoral standards in the U.S. include fast food in California, nursing homes in Minnesota, and rideshare in Massachusetts.

[2] Where unions can offer selective benefits, the gap between coverage and density tends to be smaller (Jäger et al., 2025). In Denmark, Finland, and Sweden, for example, unions administer access to state-subsidized unemployment insurance, known as the Ghent system.

minants of voluntary membership. We combine several empirical strategies that exploit the large wage floor increase to understand the relationship between contract-driven wage gains, reciprocity, and membership.

Using an event-study difference-in-differences design, we show that workers who move from the old to the new wage floor—"treated workers"—are significantly more likely to become union members than workers already above the new floor. Membership begins to rise immediately after the contract is announced, before any wage increases take effect, and continues to grow following implementation, for an average effect of 3.3 percentage points by 2024—a 26% increase relative to the pre-period membership rate of 12.7%. This pattern suggests that workers respond to their own contract-driven wage gains, both anticipated and realized.

One concern is that treated workers have lower attachment to the sector, leading to greater attrition over the follow-up period. This differential exit could bias our membership estimates if workers who leave differ from those who stay in their propensity to join. However, estimates are similar in a balanced panel of workers observed throughout, and remain sizable under alternative assumptions about the membership status of workers who exit the sample (2.5 vs 3.3 p.p.).

We provide experimental evidence that reciprocity toward the union drives the membership response. In a randomized messaging experiment, additional union contact, which raises the salience of the union's role in the wage increase, raises engagement with the union by 62 percent across the board and membership by 58 percent on average. This membership response, however, is concentrated among workers who received a raise and increases with the size of their wage gain, with no effect among workers already earning above the new floor. This heterogeneity is consistent with gift exchange, and is difficult to reconcile with explanations such as warm glow or altruism toward the union, which would predict a membership response regardless of workers' own wage gains.

We also show that income effects, changes in membership dues, updated beliefs about the union's effectiveness, and social pressure cannot explain the rise in membership. Membership begins to rise before any wage gains are realized, which is inconsistent with a pure current income effect. A control function approach further shows that only contract-driven wage gains raise membership: a 10 log-point increase from the contract raises membership by about 1.6 p.p., while the same increase from other sources, such as experience or acuity of care, has no effect. To test sensitivity to membership dues, we exploit a cap in the dues schedule: dues equal 3% of earnings up to $36 per pay period, so dues stop rising above $1,200 in earnings. When we split our difference-in-differences estimates by pre-period hours, which determine whether workers' earnings reach the dues cap under the new wage floor, we find similar membership responses among workers who remain below the cap, those brought to the cap by the raise, and those already at the cap, which is inconsistent with price effects. Leveraging many more years of data, a regression kink design at the $1,200 threshold finds imprecise changes in the slope of membership, with an implied dues elasticity -0.53 (s.e. 0.69), within the range of previous estimates from Norway (Barth et al., 2025; Dodini et al., 2023).

Workers who received the raise may also have concluded that the union is effective and joined to support future gains. But such gains are equally non-excludable, and the membership response is no larger among workers more attached to the covered sector, who would have more at stake in future union gains. Finally, social pressure has little scope in a sector where workers rarely share a physical workplace. Consistent with this, among workers already earning above the new floor, membership does not rise with the share of peers at the same fiscal intermediary who received the raise or were already members, and the response to own wage gains persists, though attenuated, when we compare workers paid through the same intermediary in the same month.

We embed these results in a model of union activity to study how gift-exchange motives affect the financing of public goods, building on classic results in public finance (Atkinson and Stern, 1974). In the model, bargaining expenditures by the union are a public good that can raise wages for members and non-members alike, but are financed only through member dues. The union cares about its members and may also place some weight on non-members' interests. Without gift-exchange, a member-focused union has weaker incentives to provide public goods because non-members do not contribute dues. With gift exchange, however, the marginal return to providing public goods is higher as the gains can attract non-members to join the union.

To evaluate the importance of reciprocity preferences, we combine our estimated gift-exchange and dues elasticities with external estimates of the efficacy of lobbying. Using recent lobbying elasticity estimates, current spending can be rationalized if the union places an altruism weight of about 0.3 on non-members' interests. Gift-exchange thus moves a member-focused union towards internalizing the interests of non-members, but not enough to rationalize observed union dues rates as optimal for members alone.

This paper makes three main contributions. First, our results speak to the literature on union growth and decline in high-income countries and the divergence between coverage and density across countries (Jäger et al., 2025). We provide some of the first evidence on the determinants of voluntary union membership within a U.S. sectoral bargaining setting, leveraging confidential administrative data alongside a randomized experiment. We show that while reciprocity driven by union-negotiated wage gains can support voluntary membership, this behavioral response is modest, highlighting the clear limits of social preferences in overcoming the free-rider problem. Foy (2026) provides complementary evidence from a right-to-work setting among Wisconsin teachers, showing that individual membership decisions respond to beliefs about coworkers' membership rates, consistent with a social pressure mechanism. We instead provide evidence from an atomized labor market setting, an arrangement increasingly common with the growth of home-based health care, remote work, and gig work.

Second, our work directly extends the large literature on social preferences at work to collective action settings. Prior related work has focused on how workers' social preferences affect effort toward employers, documenting gift-exchange in which workers respond to higher wages with increased effort (Akerlof, 1982; DellaVigna et al., 2022; Fehr and Char-

ness, 2025). In these studies, reciprocity usually takes on a private form: workers direct higher effort toward the employer who gave the raise. Our paper features a setting where the "gift" is a collectively bargained wage floor and reciprocity instead requires contributing to a public good/collective organization, where free-riding is an option. Despite this, we find evidence that some workers are willing to voluntarily join the union and pay union dues, particularly when they attribute the wage gain to the union.[3]

Third, our results contribute to a growing literature on health care labor markets and the economics of long-term care. Existing related work has focused on understanding how public financing or immigrant labor shapes the availability and quality of long-term care workforce (e.g., Shen, 2024; Gandhi et al., 2024; Kreider and Werner, 2025; Grabowski et al., 2026). We extend this literature by studying labor market institutions that govern wage-setting in home care, one of the largest and fastest-growing segments within long-term care. We illuminate the role of labor unions as providers of sector-wide public goods—such as negotiated wage floor increases—that can support labor supply in essential care sectors. We assess whether and how care workers are willing to co-finance these public goods through voluntary dues, with direct implications for the political economy of health care delivery in publicly-funded systems.

The remainder of the paper proceeds as follows. Section 2 describes the context and data, while Section 3 outlines a conceptual framework. Section 4 presents the main results on wage gains and union membership. Sections 5–7 explore mechanisms, including income effects, experimental evidence consistent with gift exchange, and alternative explanations. Section 8 discusses implications for union financing, and Section 9 concludes.

2 Context and Data

Across the U.S., 4 million employees work as home care aides (home health and personal care aides). This number has roughly doubled over the last decade and now exceeds employment for PK–12 school teachers (U.S. Bureau of Labor Statistics, 2025b). Given long-term care demands of an aging population, the occupation's projected 17% growth rate over the next decade is faster than 98% of occupations (U.S. Bureau of Labor Statistics, 2025a). At least seven states, including Minnesota, with Medicaid-funded consumer-directed home care programs have union-represented care workers (UC Berkeley Labor Center, 2023). We describe the specifics of the home care sector in Minnesota and then our key data sources and definitions.

2.1 Home care in Minnesota

Home care consists of a range of in-home client care services, including personal care assistance (PCA) with daily living activities and other health-related support. These services are provided through both a traditional, agency-based model and a consumer-directed (CD)

[3]This connects to Finan and Schechter (2012), who show that reciprocity can sustain vote-buying even in secret ballot elections, where individual actions are not directly observable or enforceable.

model. Only care workers under the CD model are represented by a union. We estimate that approximately 30% of the home care workforce in Minnesota operate under the CD model and so are covered by the union contract.

In the Agency model, a licensed home care agency employs workers directly and is responsible for wages, scheduling with clients, and payroll. The agency must follow minimum wage and benefit rules at the state and federal level. It is not subject to the provisions of the union contract. In the CD model, the client acts as the effective employer, hiring and scheduling the individual provider, while administrative tasks such as payroll are handled by fiscal intermediaries (FIs) on behalf of the client. These FIs may be licensed home care agencies who operate both models or individual entities set up exclusively to manage logistics. Workers under the CD model are covered by the union contract and typically have higher wages and guaranteed paid time off and other benefits.[4]

Within the CD model, wage-setting differs across two program types: price-based and budget-based. Under the price-based program, called PCA Choice, wages are closely tied to the reimbursement rates negotiated between the state and the union, resulting in hourly wages anchored to the contractual minimum with variation determined by acuity of care needed by the consumer and by worker experience. In contrast, under budget-based programs—Community Support Grants (CSG) or Consumer-Directed Community Support (CDCS) specifically—the state gives the consumer household an individualized budget and the consumer determines how to allocate it across wages, hours, and other services. This leads to greater dispersion in wages and often allows workers to earn above the contractual minimum.

Importantly, services performed under either the Agency and CD models in Minnesota receive the same Medicaid reimbursement rate from the state (Minnesota Department of Human Services, 2025). As a consequence, increases in CD union wage floors funded by the state Medicaid budget also put upward pressure on wages paid via the Agency model (Nesterak, 2024). However, because private agencies face no requirement to match union wage floors, those operating under both structures have a financial incentive to steer clients and workers toward the Agency model, retaining the difference between the reimbursement rate and whatever wage they pay. Using claims data obtained via a public records request from the Minnesota Department of Human Services, we find that among agencies operating the PCA Choice model, 98% also filed claims under the Agency model.[5] This dual-track structure highlights that private agencies may adjust their labor allocation across models in response to union-negotiated rate increases.

[4]These workers also tend to be family members of the care consumer (Dale et al., 2005), though family relationship status is not directly observable in our data.

[5]We classified 1,051 agencies by whether they ever reported claims under the Agency (traditional PCA) or PCA Choice models over 2018–24: 867 (83%) reported claims under both models, 169 (16%) reported claims under the Agency model only, and 15 (1%) reported claims under PCA Choice only.

2.2 Collective bargaining and dues

Eligible home care workers in Minnesota voted to unionize in August 2014. Since then, a total of five contracts have been negotiated between the state and the union at two-year intervals from 2015-2025. Over this period, the contractual minimum wage has increased from $10.75 to $20 per hour (see Figure 1a). In addition, these contracts introduced paid-time off (PTO), training and retention stipends, holiday pay for all eligible workers, and, most recently, an experience-based wage scale.[6]

Following a 2014 U.S. Supreme Court decision (*Harris v. Quinn*), home care workers cannot be required as a condition of employment to pay any fees to a union to support collective bargaining. However, Minnesota home care workers who choose to become union members pay dues equal to 3% of their gross wages, capped at $36 per pay period, which are automatically deducted from their paychecks. This rate has been the same for the past decade, since the first contract. Given the union's legal obligation to represent *all* workers in the bargaining unit, the union cannot offer selective material benefits to members, but can selectively offer rights related to union democracy (e.g. voting to ratify contracts, to set dues rates, and for officers).

To build and maintain membership, the union undertakes ongoing outreach, concentrated around two kinds of events: contract campaigns and wage floor increases. Figure A.3 plots the number of workers joining the union each month between 2018 and 2024. Joins peak around contract signings, indicated by solid red lines, and the dates on which a new wage floor takes effect, indicated by solid black lines. Organizers use these moments to contact *all* non-members because the gains from collective bargaining are most salient at these times. Given the size of the January 2024 wage increase, the union conducted membership drives throughout the second half of 2023, prior to the experiment; these drives involved phone and text-based outreach as well as door-to-door canvassing in the Twin Cities metro area. During our experiment, the union's outreach to workers in the sample followed the experimental protocol (see Section 6.1). After the experiment concluded, the union conducted outreach again in mid-2024.

2.3 Administrative data on union-covered workers

We access confidential administrative pay records for union-covered home care workers from January 2018 to December 2024 through a research partnership with the homecare workers' union in Minnesota. These records include hourly wage rate, reported hours, gross wages, dues payment, and the FI responsible for processing payment for each pay record. We link these data to the universe of union membership records, which include the date of membership authorization and, if applicable, membership resignation.[7] After processing and cleaning

[6] Under the 2023 contract, workers who log more than 1,000 PCA hours earn 2.5% above the base wage, with the premium increasing at higher experience thresholds to a maximum of 12.5% for workers who log more than 10,000 PCA hours.

[7] To join the union, workers are required to sign a membership authorization form. To resign, they must submit a written notice to the union by mail or email. Our data show that less than 4% of workers formally

the raw data provided in Excel files, we obtain a sample of approximately 3.96 million records at the worker-pay period level from 77,550 unique workers from July 2018– December 2024.[8] Figure 1 shows the evolution of the wage floors, the quarterly total of union-eligible workers and union members, and the membership rate from July 2018 to December 2024.

Using these data, we create a worker-month panel for our primary analysis. For each worker-month, the panel aggregates hours, earnings, and dues across pay records in the given month and then normalizes them by the number of pay records, so that values are expressed on a per-pay-period basis rather than as calendar-month totals.[9] The hourly wage rate is defined separately as the minimum of the reported base hourly wage rate across pay records in the worker-month. If a worker uses multiple FIs within a month, we assign the primary FI as the one where they submit the most hours that month.

We construct a monthly membership variable using membership authorization dates. A worker is considered a union member in all working months following the month they signed a membership authorization form, unless the worker subsequently revokes their membership, in which case they are no longer considered a union member from that month onward.

Unfortunately, these data do not contain any information on workers in the uncovered home care sector—i.e. those workers hired and managed directly by agencies.

2.4 Descriptive patterns

Using the administrative panel, Figure 2 documents three salient features of voluntary membership in the covered sector.

First, membership rates are higher among lower-wage workers. Across all worker-months from 2018–24, higher hourly wage rates are associated with lower membership, conditional on year-month fixed effects (Panel (a); slope$=-0.002$, SE$=0.0003$).

Second, membership rates rise among workers with higher cumulative hours. Panel (b) follows new hires, defined as workers' first month with positive hours after at least six months with zero hours, over a 24-month period, controlling for hire-month fixed effects. The share who become union members rises from about 6% among new hires working fewer than 700 hours to about 11–12% among those working more than 2,500 hours; each additional 1,000 hours is associated with a 1.7 percentage point higher probability of membership (SE$=$ 0.0012).

Third, turnover is high. Panel (c) plots the Kaplan–Meier survival function for new hires, where a worker exit is defined by at least six consecutive months without positive hours. About 41% of new hires leave the covered sector within 12 months and 58% within 24 months.

resigned within 2 years of joining.

[8] Appendix A.1 provides details on these data cleaning procedures.

[9] This normalization avoids a mechanical pattern in monthly totals: because most pay periods are approximately 14 days, a calendar month may contain one, two, or occasionally more pay-period start dates, and this count varies across workers and over time.

Consistent with free-riding incentives, overall membership rates are low (11–14%, Figure 1b), and high turnover means the union must continually recruit new workers to maintain its dues-paying base.

3 Conceptual Framework

We adapt the gift-exchange framework of DellaVigna et al. (2022) to binary union membership $m_i \in \{0, 1\}$ with continuous labor supply and heterogeneous wages. Worker i has latent wage w_i and union salience parameter $a_i \in [0, 1]$, which captures the degree to which worker i is aware of the union and attributes any improvements in employment conditions to union activity. The realized wage is $W_i = \max(WageFloor, w_i)$, reflecting the existence of the wage floor in the covered sector. Earnings are $y_i = W_i h_i$, where h_i denotes labor hours. Union dues follow a piecewise-linear schedule and are capped at \$36 per pay period: $\tau(y) = \min(0.03\, y, 36)$. Workers maximize utility $U(c, h)$ over consumption c and labor hours, where $U_c > 0$ and $U_h < 0$.

Non-pecuniary motives are governed by two parameters: a warm-glow taste ω_U and a gift-exchange parameter ω_G, both constant across workers.[10] The warm-glow parameter captures a fixed taste for union membership that does not vary by wage levels or gains. In contrast, the gift-exchange parameter scales directly by the value of wage gain $(W_i - w_i)$ generated by the contractual wage floor. Workers who receive larger wage increases thus feel a stronger reciprocal obligation to join.

Indirect utilities from membership and non-membership are:

$$V_1^i(W_i) = \max_y U(y - \tau(y),\, y/W_i) + \overbrace{a_i}^{Salience} \underbrace{\Big[\underbrace{\omega_U}_{\text{Warm-Glow}} + \underbrace{\omega_G(W_i - w_i)}_{\text{Gift-Exchange}} \Big]}_{\text{Pro-Union Preferences}}, \tag{1}$$

$$V_0^i(W_i) = \max_y U(y,\, y/W_i)\,. \tag{2}$$

Let $A(W_i)$ be the threshold salience at which a worker is indifferent between membership and non-membership, defined by $V_1^i = V_0^i$. By the envelope theorem, expanding around $\tau = 0$, we get

$$A(W_i) \;\approx\; \frac{U_c(y_i^*,\, y_i^*/W_i)\;\tau(y_i^*)}{\omega_U + \omega_G(W_i - w_i)}, \tag{3}$$

where $y_i^* = \arg\max_y U(y, y/W_i)$ is the non-member optimum and U_c is the marginal utility of consumption. A worker joins that union whenever $a_i \geq A(W_i)$. Aggregating across workers, the total expected membership share is:

$$s = \mathbb{E}_{w_i}[1 - F(A(W_i) \mid w_i)]\,. \tag{4}$$

[10] Given our context of atomistic workplaces, we abstract away from social pressure or custom mechanisms in this framework to keep it parsimonious, and instead evaluate this mechanism empirically in Section 7.1.

Comparative statics. We now assess how the membership share varies with changes in the wage floor W_i, the dues rate τ, and the salience parameter a_i.

First, if unions are invisible ($a_i = 0$ for all i) or all non-pecuniary motives vanish ($\omega_U = \omega_G = 0$), membership is zero. Under binary choice, standard preferences imply complete free-riding. Similarly, if non-pecuniary motives only entail gift-exchange ($\omega_U = 0$) and there is no binding wage premium ($W_i = w_i$), membership is again zero.

If, instead, non-pecuniary motives are present and there is some awareness of the union, a higher binding minimum wage can move s through three channels:

(i) Gift exchange: A higher wage floor lowers the threshold $A(W_i)$ by expanding the denominator, $\omega_G(W_i - w_i)$, and thus increases membership share.

(ii) Income: With concave utility, higher earnings reduce the marginal utility of consumption (U_c), lowering the dues burden and increasing membership.

(iii) Dues: Higher earnings increase the dues paid $\tau(y^*)$, which raises the threshold, lowering membership rates. This channel is zero whenever dues bind at the cap.

In what follows, we first establish the relationship between wage floor increases and membership, and then examine which of these channels operate in our setting.

4 Effects of wage increases on union membership and employment

4.1 Empirical strategy: Difference-in-differences

To estimate the effects of contract-driven wage increases on union membership and employment in the covered sector, we implement a difference-in-differences event-study design centered on the 2023–25 contract. This setting has two features that make it well suited to studying how membership responds to wage changes. First, the contract delivered the largest wage increase since the union's first contract in 2015, raising the wage floor by 25%, from $15.25 to $19.00 per hour, for all union-covered workers effective January 1, 2024 (Figure 1). Second, the timeline separates the announcement of the wage increase from its implementation, allowing us to distinguish responses to anticipated gains from responses to realized gains. A tentative agreement setting out the new wage schedule was reached on January 6, 2023, and members ratified the contract the following month, nearly a full year before the higher wages took effect in January 2024.

Sample: We restrict our estimation sample to workers who supplied any hours during a six-month period before the tentative agreement in January 2023 (the 'pre-period'). A total of 33,560 workers worked during this period, about 89% of all workers active in 2022 and 61% of all workers active between 2022–2024. For this estimation sample of workers, we construct

a monthly panel spanning July 2022 through December 2024.[11] We present results using two versions of this panel. In the unbalanced panel, the sample composition can vary month to month as wages, hours, gross pay, and membership information are conditional on reporting positive hours worked in a given month. In the balanced panel, we restrict to those who worked at least once per quarter throughout the full panel window from 2022Q3 to 2024Q4 (N=17,660), ensuring a consistent set of workers are observed in every period and assessing whether our results are sensitive to sample attrition.[12]

Specifications: Using our sample of pre-period workers, we first present a binary treatment specification, as in equation 5. Pay records report the hourly wage rate, and we measure a worker's wage in a given month as the lowest base wage across their pay records.[13] We define the treatment group as workers earning *exactly* \$15.25 per hour in the pre-period and the comparison group as workers earning at or above the new contract minimum of \$19 per hour. These two groups sit at the opposite ends of the contract's bite: treated workers see the largest possible wage gain under the new contract, a jump of \$3.75 per hour (25% increase), while comparison workers are already paid at or above the new floor and so experience no mechanical wage change from the contract itself. The treatment group constitutes 36.5% of the estimation sample, and the comparison group 13%.

$$Y_{it} = \sum_{k=-6}^{23} \beta_k AtMinWage_{2022,i} \cdot \mathbb{1}[t - t_w = k] + \gamma_i + \delta_t + \varepsilon_{it} \tag{5}$$

Y_{it} is the outcome for individual i in month t, and t_w is fixed at January 2023, the contract announcement date. The coefficients β_k trace out the treatment–comparison gap in each month k relative to announcement, from six months before (k=-6) to twenty-four months after (k=23), normalized to zero in December 2022 ($k = -1$). This window lets us observe both anticipation and realization effects along a single event-time axis, since implementation (January 2024) falls at k=12 within the post-period. γ_i are individual fixed effects and δ_t are year-month fixed effects. Standard errors are clustered at the individual level.

Alongside the event-study estimates, we present pooled difference-in-differences estimates, which separately provide the average treatment effect within the announcement year (2023) and the implementation year (2024). Here, we present both the binary specification as well as a continuous version of equation 5, replacing the $AtMinWage$ term by $\Delta Wage_i = max(0, WageFloor_{2024} - Wage_{2022,i})$, which measures the size of the worker's wage increase under the new wage floor. Workers already earning at or above \$19 have $\Delta Wage_i = 0$, while all others are assigned a value equal to how far below the new floor their pre-period wage was. This allows us to use variation from the full sample, including workers with pre-

[11] We begin the pre-period after a separate, smaller wage increase took effect in July 2022, which raised the wage floor from \$14.40 to \$15.25 per hour, so that the sample is defined under a single wage regime.

[12] Because attachment itself can respond to the contract, we also provide bounds on our membership estimates in Section 4.3.

[13] About 17% of worker-months report more than one wage rate, but fewer than 2 percent include multiple rates within the same pay period. Higher reported rates within the month generally reflect overtime or holiday pay, so taking the minimum provides a measure of the base wage.

period wage between $15.25 and $19.00 who are excluded from the binary specification. Table 1 provides summary statistics comparing the full analysis sample with the binary specification sample.

4.2 Results: Hourly Wage Rate

We start with wages to establish the timing of and compliance with the wage increase. Figure 3a reports our event-study estimates of β_k from equation 5. Following the announcement of the contract in January 2023, the effect remains flat and close to zero until implementation, when it jumps by about $3 per hour in January 2024. The jump falls short of the mechanical $3.75 increase because wages also rose for the comparison group, consistent with spillovers above the wage floor. The steady decline in the effect in 2024 suggests continued spillover increases for the comparison group over this time. The pre-period estimates show a statistically significant pre-trend (F-stat p-value<0.01), but it is very small, about 5% of the implementation effect.

The pooled estimates in Table 2 reflect this timing. The 2023 estimate is small but negative and statistically significant (-$0.045 per hour, column 1), consistent with minor wage growth in the comparison group ahead of implementation. The 2024 estimate shows an average, significant increase in the wage rate by $2.02 per hour for treated relative to comparison workers. The continuous specification shows a similar pattern: each additional dollar of pre-period wage gap raises the 2024 wage rate by $0.51 (column 2). Estimates are similar in the balanced sample (Panel B).

4.3 Results: Union Membership and Dues

Unlike wages, Figure 3b shows that union membership among treated workers rises immediately after the tentative agreement, before any wage gains are realized, with no differential trend in the pre-period (F-stat p-value=0.67). Membership continues to rise after implementation, increasing by about 4 p.p. by late 2024 relative to the comparison group. This timing mirrors new membership sign-ups, which spike around each contract announcement and wage floor increase as the union runs membership drives targeting all non-members (Appendix A.3).

Table 2, columns 3–4, present the pooled estimates. Membership among treated workers rises by 1.1 p.p. in 2023 and 3.3 p.p. in 2024 (Panel A, column 3), a 26% increase over the pre-period mean of 12.7%.[14] The continuous specification shows the dose-response pattern: each additional dollar of the pre-period wage gap raises membership by 0.7 p.p. in 2024 (column 4).[15]

[14] Restricting the comparison group to workers earning at least $20 or $21 in the pre-period yields similar effects in 2024 (see Appendix C.1.2).

[15] This sample includes workers who are already members in the pre-period. Restricting to pre-period non-members, which isolates new joining, yields slightly larger effects: membership rises by 1.2 p.p. in 2023 and 3.8 p.p. in 2024 (Appendix Figure C.1.1 and Table C.1.1, Panel A, column 1.)

We also compute implied dues by applying the dues schedule to members' gross earnings to capture changes in union revenues.[16] Implied dues follow a similar trajectory to membership (Figure 3c), rising by $0.23 per pay period in 2023 and $0.99 in 2024 in the binary specification (Panel A, column 5), a 32% increase over the pre-period mean. In the continuous specification, each additional dollar of pre-period wage gap raises 2024 dues by $0.21 per pay period (column 6), a 6% increase over the pre-period mean.

Robustness to attrition: Given the high churn in the covered sector and the lower baseline attachment of treated workers, treated workers drop out of the unbalanced panel at higher rates than comparison workers (Section 4.4.2). If workers who leave differ in their propensity to join the union, our estimates could partly reflect changes in sample composition rather than changes in membership. We address this in two ways.

First, Table 2 also reports estimates for the balanced panel, which holds composition fixed among workers who remain attached to the covered sector throughout. In this sample, membership rises by 2.7 p.p. in 2024, a 20% increase over the pre-period comparison mean (Panel B, column 3). This estimate is slightly smaller than in the unbalanced sample, but still statistically significant. Effects are similar for the continuous specification and for implied dues.

Second, we bound the membership effect in the unbalanced panel. We exploit the fact that union sign-up and drop dates come from administrative membership records rather than payroll data, and are therefore observed independently of whether a worker appears in a given pay period. For any month in which a worker does not supply hours, we code the worker as a non-member unless they have signed up, and as a member from their sign-up month until any recorded drop date.[17] That is, we carry forward membership in months without hours unless we observe a resignation; we view this as a reasonable assumption, since resignations are rare (fewer than 4% of workers who join resign within two years). Under this approach, membership rises by 1 p.p. in 2023 and 2.5 p.p. in 2024, an increase of 21% over the pre-period mean (Figure C.1.2).

4.4 Results: Employment in the Covered Sector

Because union dues depend on members' earnings and continued work in the covered sector, we next examine how the contract affected employment in the covered sector. We first estimate effects on hours and earnings conditional on supplying hours, and then turn to participation.

[16] Implied dues are an upper bound on union revenues, since actual dues may be deducted with a lag after a worker joins and there may be issues of non-compliance.

[17] This rule fills in roughly 45% of post-period worker-months for treated workers and 27% for comparison workers, compared with 27% and 19% in the pre-period.

4.4.1 Hours and earnings

Figure 4 and Table 3 present estimates for hours and log earnings per pay period, conditional on supplying hours. Hours show no differential trend in the pre-period (F-stat p-value: $p = 0.68$) and rise slightly in 2023 (0.99 hours, Table 3, column 1). They fall after implementation, with the decline emerging a few months into 2024. On average, treated workers work 1.4 fewer hours per pay period in 2024, about 2.5% of the pre-period mean. The pattern is similar in the continuous specification and in the balanced sample.

Given the modest effect on hours and the large increase in the wage rate, log earnings per pay period rise sharply at implementation of the wage increase (Figure 4b).[18] The effect narrows over 2024, reflecting both the declining wage gap (Figure 3a) and the decline in hours, for an average increase of 8 log points in 2024 (Table 3, Panel A, column 3). Results are similar in the continuous specification and in the balanced sample.

These estimates show that workers who remain in the covered sector benefit substantially from the wage increase, with a modest decline in hours. Because they condition on working, however, they do not capture any effect of the contract on participation itself, which we turn to next.

4.4.2 Participation

Figure 4c presents the effect on participation in the covered sector, defined as supplying any hours in a given month. Treated workers become steadily less likely to participate after the selection window: treated workers are 6.4 p.p. less likely to participate in 2023 and 14.2 p.p. less likely in 2024 (Table 3, column 5). Estimates are similar in the continuous specification. We report balanced-sample estimates for completeness, but participation responses there are muted by design: because these workers supply hours in every quarter, participation can vary only from month to month within a quarter.

However, participation already declines differentially within the pre-period, before the contract was announced, suggesting that at least part of this decline is unrelated to the contract. A natural explanation is mean reversion induced by our sample selection. Because the sample requires working during the pre-period, participation is mechanically elevated during the selection window and reverts toward each group's underlying rate afterward, more steeply for treated workers, who are less attached to the covered sector at baseline.[19]

Beyond this mechanical decline, the contract itself may also have reduced participation through two channels. First, it may have reallocated work towards the uncovered sector: because services under the consumer-directed and agency models receive the same Medicaid reimbursement rate but only the former is bound by the union wage floor, fiscal intermediaries operating both models have an incentive to shift clients and workers toward the agency

[18] Pre-period coefficients for earnings are slightly positive relative to December 2022, mirroring the small pre-period wage coefficients, which likely reflect the phase-in of the July 2022 increase in the wage floor.

[19] Appendix C.2 shows participation rates for our main cohort and three earlier cohorts constructed the same way. In every cohort, participation declines more steeply for treated workers than for comparison workers.

model once the floor rises. Second, higher earnings may have led some treated workers to reduce their labor supply or leave home care work, although the growth in total home care employment over this period makes this less likely (Figure A.2). As shown in Section 4.3, our membership estimates are robust to differential attrition regardless of its source. We next use a triple-difference design to separate the mechanical decline from any effect of the contract.

4.5 Triple-difference estimates

To separate the mechanical decline from any effect of the contract, we construct a placebo cohort of workers active in July–December 2019.[20] In both the main and placebo cohorts, we restrict the sample to workers with positive hours in every month of the July–December selection window.[21] Because we are interested in the effect of the contract, we want treated and control workers in both cohorts to enter January with the same employment path. With this restriction, every worker in the sample is employed in each month up to December, so any later difference in participation reflects exits after the selection window rather than differences in attachment within it.[22]

We pool the main and placebo cohorts and estimate the following equation, which compares the treated–comparison gap in the main cohort with the same gap in the placebo cohort:

$$Y_{ict} = \sum_{h=-5}^{21} \beta_h \, \text{Treat}_{ic} \cdot \text{Main}_c \cdot \mathbb{1}[t - t_c = h] + \sum_{h=-5}^{21} \alpha_h \, \text{Treat}_{ic} \cdot \mathbb{1}[t - t_c = h] + \gamma_{ic} + \delta_t + \varepsilon_{ict} \quad (6)$$

Y_{ict} is the outcome for worker i in cohort c in month t, t_c is December of cohort c's selection year, and Main_c indicates the main cohort. The sample includes treated workers ($\text{Treat}_{ic} = 1$), who earned exactly the wage floor during cohort c's selection window, and comparison workers, who earned at least 125% of the floor. The coefficients α_h capture the treated–comparison gap common to both cohorts, while β_h capture the additional gap in the main cohort. γ_{ic} are worker-by-cohort fixed effects and δ_t are year-month fixed effects.[23] Standard errors are clustered at the worker level.

Because the 2021 contract was announced in month 13 of the placebo cohort, the triple-difference estimates are cleanest for months 1–12, when the placebo cohort does not experience any contract event. Estimates after month 13 compare the main cohort with a placebo cohort that also experienced a contract announcement (but not a wage floor increase), so we view these later coefficients with caution.

[20] We use the 2019 cohort as the placebo because it is the only cohort in our data whose selection window, as in the main cohort, begins with a wage floor increase ($12 to $13.25 in July 2019, compared with $14.40 to $15.25 in July 2022) and is followed by at least 12 months without a new contract announcement or floor increase.

[21] This restriction keeps about 57% of the main cohort sample (6,723 treated and 2,684 control workers). The placebo cohort has 3,418 treated and 5,329 control workers.

[22] When we require only ever working in the July–December selection window, participation trends already differ within the window: the triple-difference pre-period coefficients average −0.066 (s.e. 0.009; Appendix Figure C.3.1, panel a).

[23] Because the cohorts never overlap in calendar time, δ_t is equivalent to cohort-by-event-time fixed effects.

Participation: Figure 5, Panel (a), presents the triple-difference estimates for participation. The pre-period coefficients are zero by design, since every worker in the sample works in each month of the selection window. After the selection window, the declining trend in the difference-in-differences estimates (Figure 4c) disappears. Relative to their controls, treated workers in the placebo cohort stop working at about the same rate as those in the main cohort (Appendix Figure C.3.2, Panel a). The triple-difference estimates stay small, between roughly -2 and $+6$p.p. over the following 21 months, with no break at either the announcement ($h = 1$) or implementation ($h = 13$).[24] If anything, they are slightly positive on average over months 1–12, which reflects a sharper drop in participation among placebo treated workers at the onset of the COVID-19 pandemic and a one-month payroll gap at one FI. These results indicate that the decline in the difference-in-differences estimates largely reflected mean reversion rather than a response to the contract.

Other outcomes: Figure 5, panels (b)–(f) present triple-difference estimates for our other outcomes. For wages, membership, and dues, the estimates closely follow the difference-in-differences results: wages are flat before implementation and jump by about \$2.80 by month 13 (January 2024), and membership and dues rises from the contract announcement onward.[25] These results confirm that our main findings are not driven by mean reversion. For hours, however, the 2024 decline in the difference-in-differences estimates is also present in the placebo cohort (see Appendix C.3.2, panel e). Once it is netted out, hours do not fall and, if anything, rise after implementation. Relative to the difference-in-differences, earnings are closer to zero in the pre-period and after the announcement, and increase only once the wage increase is implemented. Results for the full sample of workers with any hours in the selection window are similar (Appendix Figure C.3.1).

5 Do income or price effects explain the membership response?

Having established the link between contract-driven wage gains and membership, we now consider whether standard income or price effects can explain this response.

5.1 Income effects

In our framework, higher earnings can raise membership by lowering the marginal utility of consumption, which reduces the effective burden of dues. Recall that membership rises *before* any wage gains are realized and continues to rise through implementation (Figure 3). This pattern is not consistent with a pure current income effect, but could reflect workers responding to anticipated or permanent income effects.

To test income effects more directly, we assess whether the source of wage variation

[24] Appendix Figure C.3.1, panel a, shows the same result for the full sample that ever worked during the selection window. The post-period estimates stay within a few percentage points of the pre-period trend over months 1–12, showing no systematic decline at either announcement or implementation.

[25] Membership estimates are slightly smaller after month 13, consistent with the placebo cohort's own response to the 2021 contract.

matters. If membership responds to income, workers should respond similarly to wage gains regardless of their source. Reciprocity instead predicts stronger responses to wage gains that are clearly attributable to the union, since workers reciprocate only benefits they credit to the union. Although spillovers to wages above the floor are also driven by the union, we assume these are harder to attribute to the union than the wage floor increase itself. We therefore use a control function approach to distinguish membership responses to contract-driven changes in the wage floor from responses to other sources of wage variation, such as spillovers, experience, or acuity of care.

We implement the control function using the sample described in Section 4.1. First, we estimate the following first-stage regression:

$$\log(\text{wage}_{it}) = \mu \, \text{Post}_t \times \Delta\text{Wage}_i + \gamma_i + \delta_t + \epsilon_{it} \tag{7}$$

The instrument interacts an indicator for months from January 2024 onward, Post_t, with the wage gap $\Delta\text{Wage}_i = max(0, WageFloor_{2024} - Wage_{2022,i})$. We include worker and year-month fixed effects. The residuals $\hat{\epsilon}_{it}$ capture wage variation not driven by the wage floor increase. The first-stage F-statistic is 15,157, indicating a strong instrument, while the instrument explains about 14 percent of within-worker wage variation after the fixed effects.

In the second stage, we include these *residuals* directly into the following regression:

$$\text{membership}_{it} = \beta \log(\text{wage}_{it}) + \lambda \hat{\epsilon}_{it} + \gamma_i + \delta_t + \psi_{it} \tag{8}$$

This formulation allows us to estimate the membership effects of the contract-driven wage floor (β), while separately capturing whether wage variation from other sources generates an *additional* membership effect (λ).[26] Under an income effects explanation, we would expect λ to be positive and significant, as variation driven by non-contract elements should also increase willingness to become a member. We also allow the response to contract-driven wages to vary with the residual by adding the interaction $\log(\text{wage}_{it}) \times \hat{\epsilon}_{it}$ (Garen, 1984; Chay and Greenstone, 2005). Standard errors are clustered at the worker level across both stages.[27]

Table 4 presents the results. A 10 log-point increase in wages is associated with a 0.2 p.p. increase in membership (column 1). Including the first-stage residual raises the coefficient on log wages to 0.161, while the residual enters with a negative coefficient of nearly identical magnitude (-0.166, column 2). This means that a 10 log-point contract-driven wage increase raises membership by about 1.6 p.p., whereas the same increase from other sources has essentially no effect.[28] The relationship between wages and membership thus runs primarily through the contract-driven wage floor. The interaction between the residual and log wages

[26] Conditional on the first-stage residual, β is equivalent to the standard IV estimate, as the endogenous variation is accounted for by the first-stage residuals. In a model with homogeneous compliance λ gives the extent of the bias in OLS, but with heterogeneous compliance the difference between OLS and IV can be interpreted as differential compliance to union vs non-union wage variation.

[27] Bootstrapping standard errors to account for estimated first-stage residuals yields nearly identical standard errors.

[28] Because membership begins to rise in 2023, before the wage floor changes, we also re-estimate the control function excluding worker-months in 2023 and find similar results.

is small and insignificant (column 3), indicating that the response to contract-driven wages does not vary with the size of the non-contract shock.

5.2 Price effects: Costs of membership

Because dues depend on earnings, the wage increase also changed the price of membership. Dues equal 3% of gross earnings up to a cap of $36 per pay period, reached at earnings of $1,200. All treated workers—those who move from the old to the new wage floor—received the same 25% wage increase. Because of the cap, however, this raise affected the price of membership differently across workers. Workers already at the cap experienced an income gain with no change in dues. Workers brought to the cap by the raise experienced an income gain with a smaller increase in dues, since dues stop rising at $36. Workers who remain below the cap experienced an income gain with dues rising by 3% of the full earnings gain. Comparing the below-cap and newly capped groups with workers already at the cap therefore helps isolate the price channel.

We test for price sensitivity by exploiting this heterogeneity within our binary difference-in-differences sample. We classify workers by their pre-period hours into three groups: below the cap (fewer than 60 hours per pay period); newly capped (60 to 79 hours); and already capped (above 79 hours).[29] If membership were sensitive to price, we would expect smaller membership responses among newly capped workers than among those already capped, and smaller still among below-cap workers. Instead, Figure 6 shows that the three groups follow similar paths, with membership rising by about 3 p.p. in 2024. While this subgroup comparison demonstrates that price effects do not drive the membership response, the smaller samples yield wide confidence intervals. To expand samples and quantify the price channel explicitly, we next estimate the elasticity of membership with respect to dues using a regression kink design.

Estimating price sensitivity to dues: We use a regression kink design at the dues cap, where the marginal dues rate changes discontinuously while other covariates are assumed to vary smoothly with earnings, the running variable. The design identifies the price sensitivity of membership from a change in the slope of membership at the dues cap. Below $1,200, workers with higher earnings also pay higher dues; above $1,200, dues are fixed at $36. If higher dues discourage membership, this penalty disappears above the kink, so membership should rise more steeply with earnings above the kink than below it.

For this analysis, we use data at the pay-period level from July 2018 to December 2024. We restrict to standard 14-day pay periods with gross earnings below $2,000 and mean hourly wages of at most $30 (87.5% of worker pay periods).[30]

[29] At the old floor of $15.25, earnings reach the $1,200 cap at about 79 hours per pay period; at the new floor of $19, they reach it at about 63 hours. We use a threshold of 60 hours rather than 63 to increase the sample of newly capped workers. We use hours instead of earnings since the latter can vary idiosyncratically with overtime and holiday pay.

[30] Pay periods may be shorter or longer than 14 days because of variation in pay reporting across FIs. Because the $36 dues cap is applied per pay period, the earnings threshold at which dues stop rising differs with period

Figure 7, Panel (a), confirms the first stage: among members, dues rise linearly at 3% of gross earnings up to $1,200 per pay period, at which point they flatten at $36, producing a clear kink in the dues-earnings relationship.[31]

The validity of the RKD requires that workers do not sort around the $1,200 threshold, so that the density of earnings is smooth there. Appendix Figure C.7.1 shows the raw distribution of earnings per pay period in $20 bins. Earnings heap at 80 hours times common wage rates, for instance $1,220 (80×$15.25), but these heaps appear throughout the distribution, not just near the threshold. Excluding 80-hour periods, the distribution is smooth through $1,200, suggesting no evidence of sorting around the kink.

We estimate the change in the slope of membership with respect to earnings at the dues kink using local-linear regressions on each side of the threshold, with a triangular kernel and the MSE-optimal bandwidth (Calonico et al., 2014). Standard errors are clustered at the worker level. We find a positive but imprecisely estimated change in the slope of membership at the dues kink (Figure 7). At the MSE-optimal bandwidth of $159, membership rises 0.65 percentage points more per $100 of earnings above the kink than below it, which implies a dues elasticity of -0.53 (s.e. 0.69).[32] Results are qualitatively similar across other bandwidth and specification choices, as shown in Figure 7 and Appendix Table C.7.1. This estimate is somewhat smaller than the elasticity of -0.75 implied by estimates from the Norwegian union-dues tax deduction (Dodini et al., 2023), but it is considerably noisier and statistically indistinguishable from zero.

Overall, the results in Sections 5.1 and 5.2 cannot explain the rise in membership we observe from the increase in the wage floor. We now turn to directly ask whether social preferences—specifically, reciprocity—can account for why workers do not fully free-ride.

6 Experimental evidence on reciprocity in union membership

The difference-in-differences estimates show that membership rises most among workers who received the largest raises, and increases with the size of the raise. We also do not find that income or price effects can fully explain the membership response we document. Yet, this evidence alone does not establish that individuals join *because* they attribute wage gains to the union. If membership reflects gift exchange, workers should be more likely to join when the union's role in securing their raise is salient, and this effect should be larger for workers who gained more.

We test this prediction using a randomized outreach experiment that varied how often workers were contacted by the union about the wage increase. Ideally, we would randomize whether workers are told that the union was responsible for the raise; because the union

length. Our restriction ensures that the kink is at $1,200 for all observations.

[31] Above $1,200, dues are within 50 cents of $36 in 90% of member pay periods. Average dues rise slightly above $36 at higher earnings either because the cap is not applied or the worker is paid by two FIs that each cap dues separately.

[32] We convert the change in slope into an elasticity ε by multiplying the change in the slope of membership at the kink by $-1,200$ and dividing by the membership rate near the kink (14.7%).

delivered the messages, this was not feasible. Instead, we vary the intensity of contact, which we interpret as raising the salience of the union's role. In the framework of Section 3, the treatment corresponds to shifting upward the salience parameter a_i. Under gift exchange, the effect on membership should increase with the wage gain $(W_i - w_i)$, whereas warm glow does not depend on the extent of gains secured by the union.

6.1 Experimental design

We partnered with the union to implement a randomized messaging experiment in advance of the scheduled 25% wage floor increase on January 1, 2024. The experiment varied the intensity of outreach, randomizing whether workers received one or two contacts from the union; we refer to two contacts as the "High salience" condition and one contact as the "Low salience" condition. Contacts were also randomized in timing: about half the sample was contacted before the wage increase and the rest after.[33]

The experimental sample consisted of all bargaining unit-eligible workers residing in Minnesota with available contact information who were not union members as of November 2023 (N=25,933). Randomization was conducted at the individual level, stratified by (i) program type (either the PCA choice or the Budget model) and (ii) FI, grouping together small FIs, for a total of 43 strata. We estimate the effect of the additional contact by comparing the High salience and Low salience groups, controlling for strata fixed effects and an indicator for whether contacts were delivered after the wage increase.

All outreach informed workers about the January 2024 wage increase as well as other benefits and offered opportunities to engage with the union, including options to learn more or sign up for membership (see Appendix B.2 for examples).[34] Messages varied by program type to reflect differences in funding structures. In the analysis, we restrict the sample to workers with contactable mobile phone numbers (N=21,353).

We examine two main outcomes. The first is union membership, measured from union records as joining within 90 days of the initial contact.[35] The second is engagement with the union, defined as replying to a text message or registering for a Zoom information session; we exclude negative replies, such as requests to stop contact. Engagement allows us to assess whether additional contact changed how workers interacted with the union, separately from whether they joined.

[33] Individuals were randomly assigned to one of four groups: (i) receive a single contact before the wage increase (Before 1x); (ii) receive two contacts before the wage increase (Before 2x); (iii) would receive a single contact after the wage increase (After 1x); and (iv) would receive two contacts after the wage increase (After 2x). Appendix C.4.1 shows that baseline characteristics are balanced across the four experimental arms.

[34] Implementation varied slightly across groups: While individuals contacted before the wage increase could request more information via text, those contacted after were additionally offered an option to join Zoom information sessions. Due to an implementation error, the membership link was not shared in the initial text messages for the groups contacted before the increase. The union also sent emails for groups contacted before the wage increase; however, only about 10% of the sample had email information, so this was discontinued for groups contacted after the wage increase. See Appendix B.1 for further details.

[35] A fixed window gives both waves the same follow-up period, and the union resumed its own outreach in June 2024, roughly three months after the first message to the groups contacted after the wage increase.

6.2 Results

Table 5 reports the effects of the High salience treatment on engagement with the union (columns 1–3) and union membership (columns 4–6). Receiving a second contact from the union increases positive engagement by 2.7 p.p., a 62% increase relative to the low salience group (column 1). This effect does not vary with workers' wage gains. We measure these with the same wage gap ΔW_i as before, but computed from each worker's baseline hourly wage in November 2023, just prior to the experiment. The interaction with an indicator for a positive wage gap ($\Delta W_i > 0$) is small and statistically insignificant (column 2). In column 3, we interact high salience with the continuous wage gap, and the interaction is again small and insignificant.

High salience also increases membership, by 0.29 p.p. on average, a 58% increase over the low salience group's joining rate of 0.50% (column 4). Unlike engagement, however, this effect is concentrated among workers who gained from the new wage floor. Among workers already earning at or above \$19, high salience has no effect on joining (-0.04 p.p., column 5). Among workers who received any wage floor gain, it increases joining by an additional 0.41 p.p. ($p = 0.079$), for a total effect of 0.37 p.p. The continuous specification shows the same pattern. For workers with no wage gap, high salience has no effect on joining (0.04 p.p., column 6), and each additional dollar of wage gap raises the effect by 0.11 p.p. ($p = 0.087$).[36]

In sum, these results suggest that repeated contact raises union salience and engagement across the board, but converts into membership only for workers who benefit from the wage floor increase. This pattern is consistent with reciprocity. It is harder to reconcile with explanations in which contact simply reduces information frictions or activates pre-existing pro-union sentiment, which would predict a membership response regardless of workers' own wage gains.

7 Discussion

7.1 Alternative explanations

Peer effects or social pressure: Ex ante, the home care sector is not well-suited to understanding peer effects in union membership, as the workplace is highly atomized and workers do not typically come together at common physical locations. Nevertheless, workers may be embedded in other social networks that could influence their decision to join the union. While we do not observe these networks directly, we use variation across fiscal intermediaries (FIs) as a proxy for workers' social environment to assess the strength of this explanation. We do this in two ways.

First, we examine membership responses among workers already earning at or above

[36] Appendix Table C.4.2 presents results disaggregated by contact timing and frequency. The wage-gap gradient in joining is larger in every arm than in the "Before 1x" group, the only one never sent the membership link (Section 6.1). Lowering the cost of joining thus raises membership more among workers who gained from the wage floor, which is inconsistent with information frictions alone driving the response.

the new wage floor of $19 in the pre-period, i.e., the control workers in the DiD analysis. If peer effects operate independently of own wage gains, these workers should become more likely to join when more of their peers are treated, as they observe those peers joining, or when their FI has a stronger pre-existing union culture, as the contract raises the salience of the union. We proxy peer exposure using each worker's pre-period FI, and construct leave-one-out shares of treated peers and pre-existing union members at that FI so that a worker's own status does not mechanically enter their own peer measure.[37] We find no evidence of positive peer effects, whether we measure peer shares in the pre-period or in July–December 2023, just before the wage increase took effect (see Appendix C.5.1).

Second, we exploit within-FI variation in wage gains by adding FI $\times$ month fixed effects to our continuous treatment specification from Table 2, column 4. This is a demanding specification: it absorbs all variation common to workers at the same FI in the same month, including but not limited to peer behavior. The remaining variation comes from differences in own wage gains between workers at the same FI in the same month. We report results using both the worker's current FI and their pre-period FI. The 2024 estimates falls from 0.007 to 0.002–0.003, but remain positive and statistically significant (Appendix C.5.2). The attenuation suggests that part of the membership response operates at the FI level, but FI-level factors cannot account for the membership response on their own.

Investments in union efficacy: An alternative explanation is that workers who received the raise update their beliefs about the union's effectiveness and join as a forward-looking investment: membership strengthens the union, which may in turn deliver larger future gains. Because the current wage increase applies to all covered workers regardless of membership, such updating can affect joining only through expected future gains. However, future contract gains are equally non-excludable, so this mechanism faces the same free-rider problem: an individual worker can benefit from a stronger union without paying dues.

As an additional check, we test whether workers with greater attachment to the covered sector, who have more to gain from a stronger union, are more likely to join following a positive contract outcome. We proxy expected future attachment with attachment in 2022, classifying workers as highly attached if they worked at least 9 months (67% of workers) or at least 1,000 hours (44%) in 2022.[38] Using the binary difference-in-differences specification interacted with the attachment group, we find no evidence that more attached workers respond more (Appendix C.6). In 2024, membership rises by 2.9 p.p. among workers with high attachment by months and 4.2 p.p. among those with low attachment ($p = 0.16$ for equality), and by 3.2 and 3.1 p.p. respectively when attachment is measured by hours ($p = 0.88$). This pattern suggests that forward-looking investment is unlikely to be a primary driver of membership.

[37] Peer shares are computed among workers in our main estimation sample, i.e., those who worked during the pre-period from July–December 2022. We restrict the sample to control workers observed at a single FI during the peer measurement window, so that each worker has a well-defined set of coworkers. This restriction retains 97–99% of control workers.

[38] Rank correlations between 2022 and 2023–2024 are 0.50 for months worked and 0.71 for hours worked.

8 Gift Exchange and Public Good Provision Under Voluntary Membership

In this section, we integrate our results above into a model of union activity. We consider a wage-bill maximizing union choosing how much to expend on bargaining for all workers, while financed by dues levied only on members. The union thus chooses dues and a public good G to maximize the (after-dues) wage bill of members, who are a share s of fixed workforce N.[39] The union puts weight $\alpha \in [0,1]$ on the wage-bill of non-members. In our context, G is political expenditures as well as contract negotiation expenditures (e.g. staff time) conducted on behalf of home care workers in the state. The share of workers belonging to the union weakly increases in the contract wage $w(G)$ and decreases in the dues rate τ.

The economics are similar to the public goods problem of a local government, where taxes and spending alter the government's objective by attracting or repelling citizens and voters. Unions provide public goods to attract reciprocating potential members, as well as to benefit both members and non-members, financed by dues that may deter dues-averse workers. The union therefore solves a variant of the optimal public goods finance problem (Atkinson and Stern, 1974), with endogenous membership:

$$\max_{G,\tau}(1-\tau)w(G)s(w(G),\tau)N + \alpha w(G)(1-s(w(G),\tau))N$$

subject to

$$G = \tau w(G)s(w(G),\tau)N$$

We can write the solution to the union's problem as balancing marginal benefits against costs:

$$\underbrace{\epsilon_{w,G}}_{\text{Effect of G on w}}\left((1-\alpha)(1+\underbrace{\epsilon_{s,w}}_{\substack{\text{Effect of } w \\ \text{on membership}}}) + \alpha\frac{(1+\epsilon_{s,\tau})}{s(w,\tau)}\right) = \tau + \underbrace{\epsilon_{s,\tau}}_{\substack{\text{Effect of } \tau \\ \text{on membership}}}(\tau - 1 + \alpha)$$

where ϵ terms are elasticities.

An altruistic union or social planner who weighted members and non-members equally (i.e. $\alpha = 1$), but was constrained to be financed by members alone, would bargain for the whole sector, with solution given by $\epsilon_{w,G} = s(w,\tau)\tau$.

On the other hand, an altruistic union facing a regime with mandatory dues collection ($s = 1$) would implement the sectoral optimum, given by $\epsilon_{w,G} = \tau$, which corresponds to the social planner's solution for the optimal financing of a productive public good, as in Atkinson and Stern (1974).

[39] Allowing the size of the workforce to respond to the contract wage would introduce disemployment effects that could offset gift exchange. Empirically, our triple difference estimates show that the participation decline among treated incumbents is hard to attribute to the contract (Section 4.4). We cannot estimate the causal effect on entry, but covered employment grew between 2023 and 2025 (Figure 1a), which is inconsistent with a large disemployment effect. We therefore treat N as fixed.

With any $\alpha < 1$, gift exchange ($\epsilon_{s,w} > 0$) increases the union's marginal benefit of public goods provision, *ceteris paribus*. As long as higher G raises wages and this raises the membership rate ($\epsilon_{s,w}, \epsilon_{w,G} > 0$), gift exchange also expands the dues base, making it possible to finance a given level of G with a lower dues rate. These mechanisms provide a channel through which endogenous membership can increase public-good provision relative to a setting without gift exchange.

Back-of-Envelope Quantification: With endogenous membership, for example via the gift exchange mechanism documented above, a member-focused union can internalize non-member interests. How quantitatively powerful is this force, relative to the costs of membership? On the benefit side, we estimate gift-exchange $\epsilon_{s,w}$ of approximately 1.6–1.8.[40] Estimates of $\epsilon_{w,G}$ are necessarily speculative. To illustrate the trade-off, we take a conservative elasticity of bill enactment with respect to lobbying expenditures of 0.13, estimated by Moshary and Slattery (2024), which we use as the elasticity of sector-wide wage increases won as a result of union lobbying expenditures.

On the union cost side, our (imprecise) RKD point estimate implies a dues elasticity of $\epsilon_{s,\tau} = -0.53$. Under a natural baseline case of $\alpha = 0$, the marginal value of union funds is 0.67, implying that a purely member-focused union would cut dues and spend less on the public good than this union does. Alternatively, current spending can be rationalized by an altruism weight of $\alpha^* \approx 0.32$ on non-members, below the equal weighting a social planner would apply. While gift-exchange does push a member-focused union towards providing more public goods, it is not quantitatively strong enough to rationalize the 3% dues rate given the moderate dues elasticity and low elasticity of lobbying. Additional union altruism towards non-members, or other modifications of the union objective function, are required to believe that unions are optimizing.

Application to California's Fast Food Council: As an external application of our framework, consider California's Fast Food Council, which pairs a sectoral wage board with voluntary union membership in the California Fast Food Workers Union. The policy covers limited-service restaurants belonging to national chains with at least 60 establishments, an approximate workforce of around 500,000 (NPR, 2024). Dube (2026) estimates that the wage board's 25% increase in the sectoral wage floor over the state minimum wage resulted in a 10% own wage increase. Our gift-exchange estimate suggests this would lead to a 16–18% increase in membership. The current membership is perhaps 10,000 (the size of the Council's Facebook group following), implying 2% union density. With dues of $20 per month, the wage floor increase would generate an additional $32–36,000 monthly, enough to pay four additional full-time organizers. Given the limits of employer labor demand and the size of the workforce relative to financed staff, it seems challenging to organize and build union density in this sector via contract raises and downstream voluntary dues revenue alone.

[40] We compute $\epsilon_{s,w}$ from the continuous-treatment estimates for 2024 in Table 2. This gives $\epsilon_{s,w} \approx 1.8$ in the unbalanced sample and $\epsilon_{s,w} \approx 1.6$ in the balanced sample.

9 Conclusion

This paper studies why workers voluntarily join a union when free-riding is individually optimal. Using a large increase in the wage floor for covered home care workers in Minnesota, we find that workers who benefit most from the union contract are significantly more likely to join, and that this response operates primarily through reciprocity rather than income or price effects. This gift-exchange margin gives the union a meaningful incentive to internalize the interests of non-members: by raising wages broadly, it can cultivate future membership. Yet, reciprocity alone cannot close the gap between union coverage and density.

Our findings are most relevant where collective bargaining arrangements cover all workers but membership is voluntary, as in right-to-work states and the public sector after *Janus*, and where workers are dispersed across many small employers or work alone, as in home care, domestic work, and platform-based gig work. In such settings, unions cannot rely on workplace social pressure to sustain membership and social preferences only partly mitigate free-riding. Building a durable union presence will therefore likely require institutions that limit free-riding.

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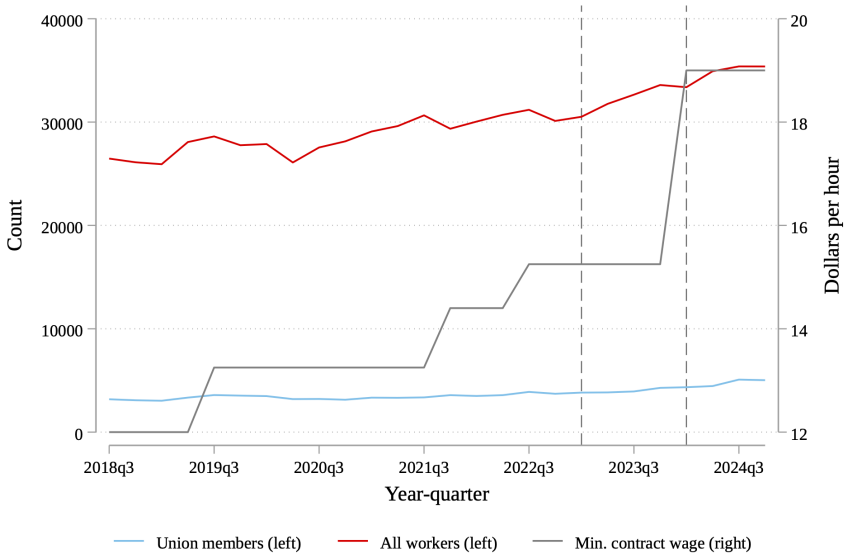
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Figure 1: Trends in the Home Care Union Sector

(a) Number of workers and union members



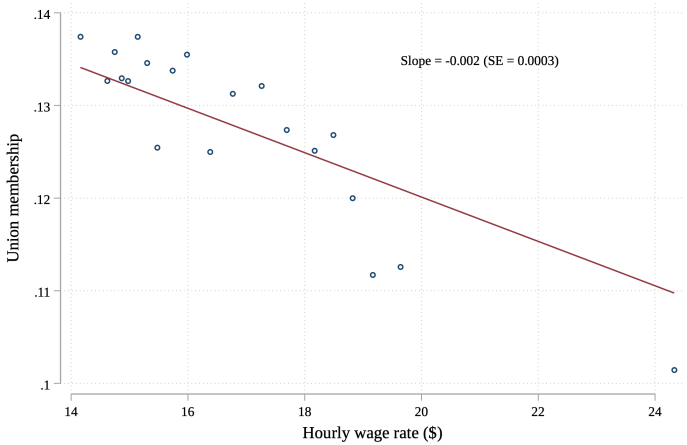
(b) Membership Rate



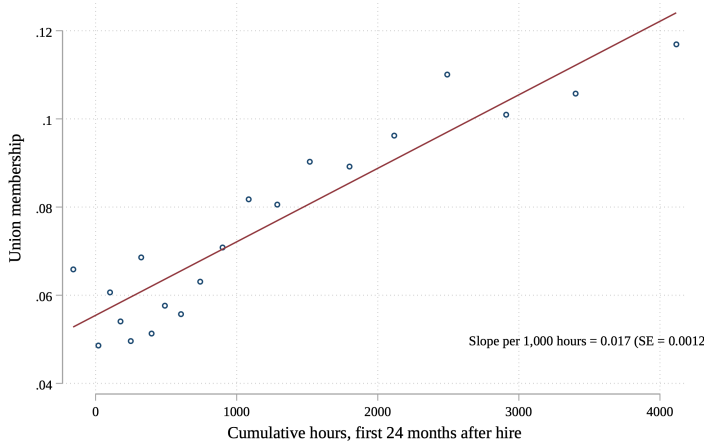
Notes: This figure shows quarterly trends in the number of union-eligible workers and union members, as well as the contract minimum wage, from 2018q3 to 2024q4. The right axis reports the contractual wage floor (dollars per hour). In panel (a), the left axis measures the count of workers and members. In panel (b), the left axis measures the union membership rate. The dashed vertical lines indicate the announcement and implementation of the 2024 minimum wage increase.

Figure 2: Descriptive facts

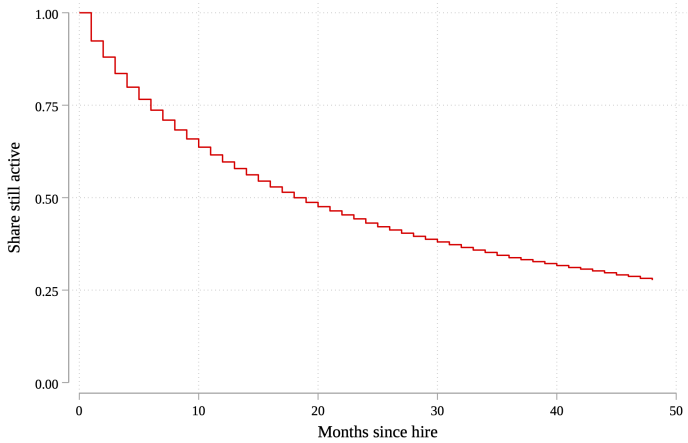
(a) Membership and wages



(b) Membership and cumulative hours



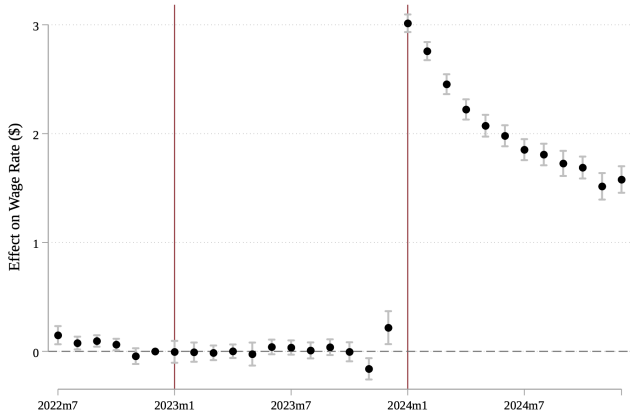
(c) Survival of new hires



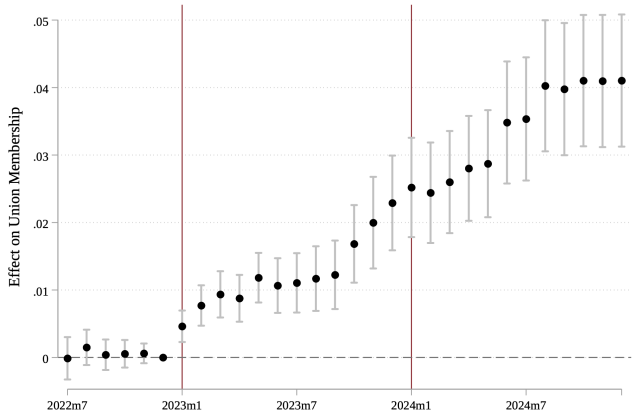
Notes: This figure shows union membership, worker attachment, and tenure among union-covered home care workers. Panel (a) plots membership against the hourly wage rate using all worker-months with positive hours from 2018–24, controlling for year-month fixed effects with standard errors clustered at the worker level. Panel (b) plots membership against cumulative hours worked over a 24-month horizon for new hires (defined as workers with positive hours following at least six consecutive months of zero hours), controlling for hire-month fixed effects. Panel (c) shows the Kaplan-Meier survival function for new hires between July 2018 and June 2024, where exit is defined as at least six consecutive months with no hours, and ongoing spells are right-censored.

Figure 3: Difference-in-differences Estimates: Wages and Membership

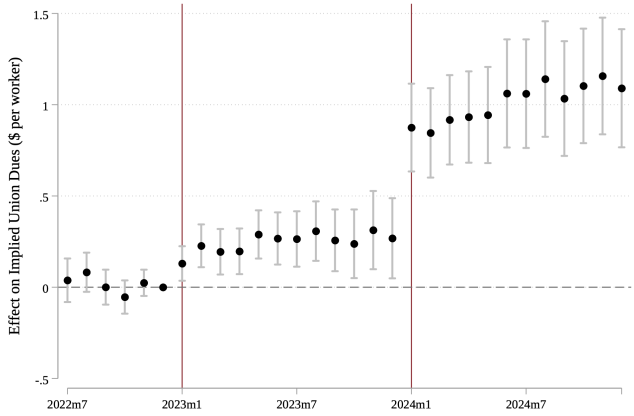
(a) Wage rate



(b) Union membership



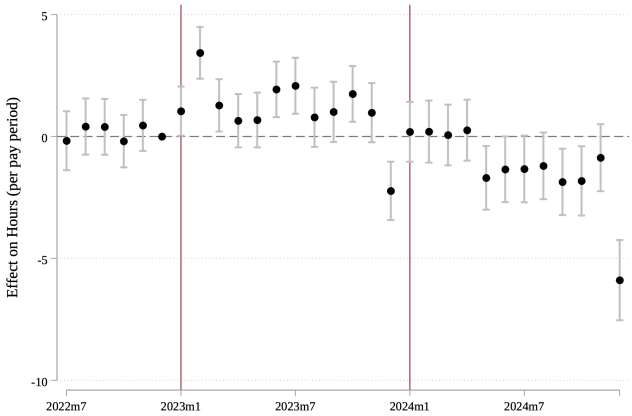
(c) Implied dues ($ per pay period)



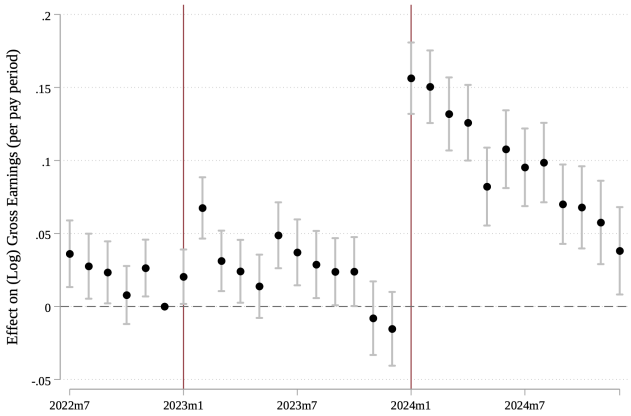
Notes: This figure reports binary difference-in-differences estimates as per equation 5. We define the treatment group as workers earning *exactly* $15.25 per hour in base wages in the pre-period and the comparison group as workers earning at or above the new contract minimum of $19 per hour. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. Sample is restricted to those supplied any hours between July–December 2022. Panel (a) shows the effects on the (minimum) hourly wage rate. Panels (b) and (c) show the effects on union membership and implied dues paid per pay period ($ per worker). All outcomes are conditional on reporting positive hours in the month. Estimates are normalized relative to 2022m12.

Figure 4: Difference-in-differences Estimates: Participation, Hours, and Earnings

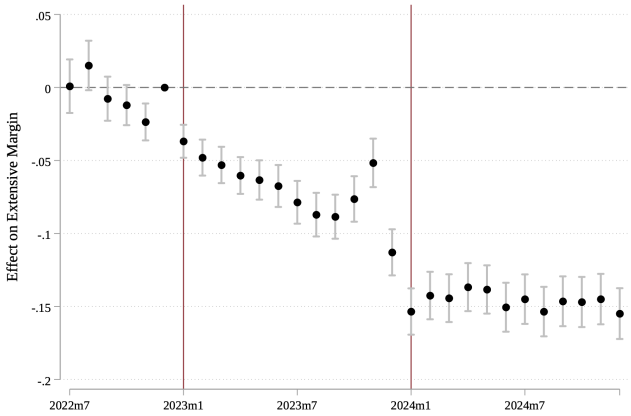
(a) Hours per pay period | working



(b) Log earnings per pay period | working

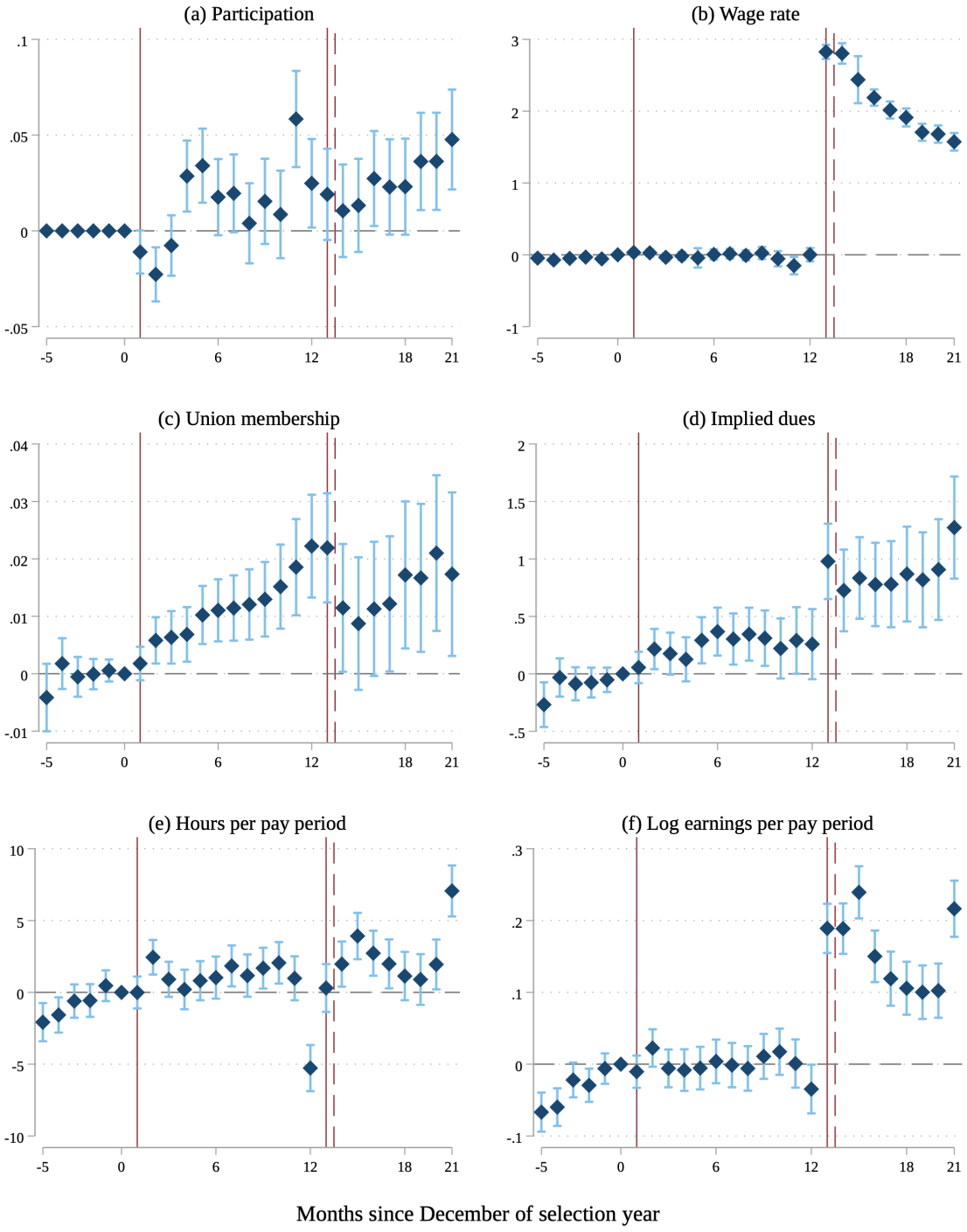


(c) Participation



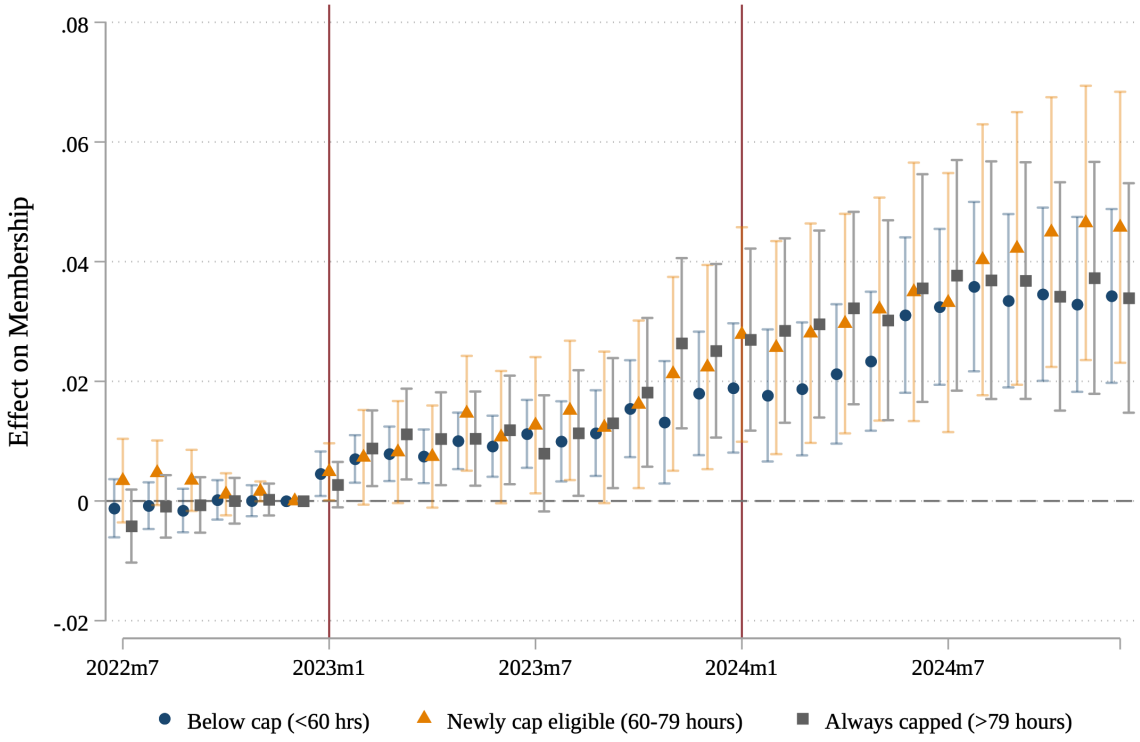
Notes: This figure reports binary difference-in-differences estimates as per equation 5. We define the treatment group as workers earning *exactly* \$15.25 per hour in base wages in the pre-period and the comparison group as workers earning at or above the new contract minimum of \$19 per hour. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. Sample is restricted to those supplied any hours between July–December 2022. Panels (a) and (b) show the effects on hours and gross earnings per pay period, conditional on reporting positive hours in the month, while panel (c) shows participation, defined as supplying any hours in a given month. Estimates are normalized relative to 2022m12.

Figure 5: Triple-difference estimates



Notes: This figure reports triple-difference estimates of our main outcomes using equation 6, which compares the effect in the main 2022 cohort with that in the placebo 2019 cohort. Samples in both main and placebo cohorts are restricted to those who worked every month in the pre-period. Event time is measured in months since December of the selection year, and estimates are normalized to h=0. Solid red lines mark the main cohort's contract announcement (h=1) and the implementation of the increase in the wage floor to $19 (h=13). The dashed red line marks the placebo cohort's 2021 contract announcement in January 2021. Standard errors are clustered at the worker level.

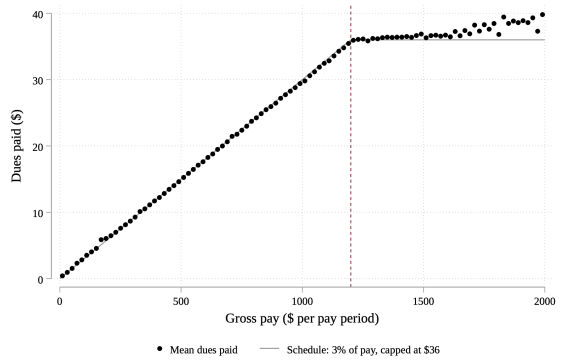
Figure 6: Effects on Membership by Dues Cap Eligibility



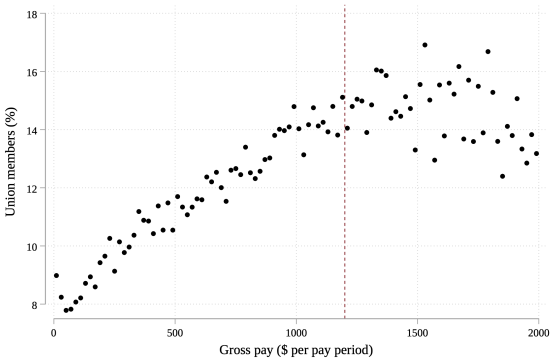
Notes: This figure reports binary difference-in-differences event-study estimates of union membership separately by cap-eligibility of workers. We classify workers by their average pre-period hours per pay period: workers below the dues cap both before and after the raise (fewer than 60 hours), workers newly brought to the cap by the raise (60 to 79 hours), and workers already at the cap before the raise (more than 79 hours). Dues equal 3% of gross earnings up to a cap of $36 per pay period, reached at $1,200 in earnings, or about 79 hours at the old floor of $15.25 and about 63 hours at the new floor of $19. We choose 60 hours as the threshold to increase sample size for the newly cap eligible group. The vertical lines mark the tentative agreement (January 2023) and the implementation of the new wage floor (January 2024). Estimates are normalized to December 2022. Standard errors are clustered at the worker level.

Figure 7: RKD Estimates

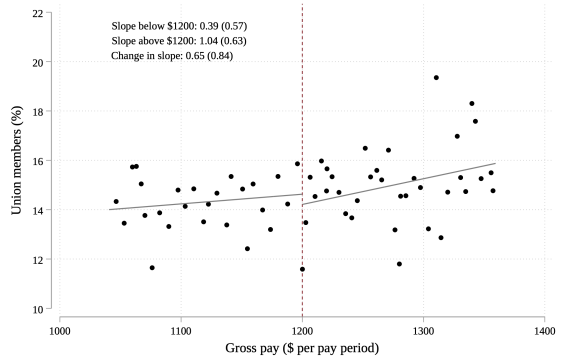
(a) Kink in dues schedule



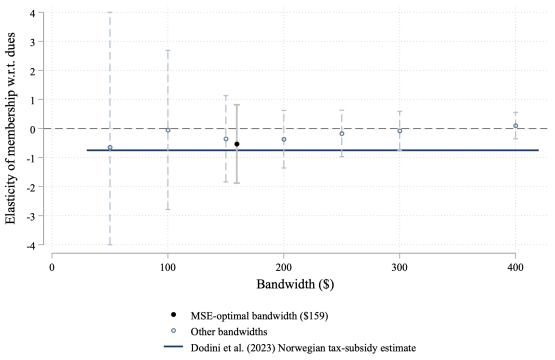
(b) Membership as a function of earnings



(c) Membership and earnings at the kink



(d) Sensitivity of the RKD estimate



Notes: This figure documents the relationship between dues, earnings, and membership and the sensitivity of the regression kink estimate to the bandwidth. Panel (a) plots mean dues paid by members, in $20 bins of gross earnings. Panel (b) plots the share of pay periods in which the worker is a union member, in $20 bins of gross earnings. Panel (c) zooms in on the MSE-optimal bandwidth around the kink ($159). Each dot is the membership rate in a bin holding an equal number of pay periods. The number of bins on each side is chosen by the IMSE-optimal rule of Calonico et al. (2015). The lines are local linear fits on each side of the kink with a triangular kernel. Slopes are in percentage points per $100 of gross earnings, with standard errors clustered by worker in parentheses. Panel (d) plots the regression kink estimate of the elasticity of union membership with respect to dues at the $1,200 dues cap, estimated at different bandwidths. The solid marker is the MSE-optimal bandwidth ($159); hollow markers are alternative bandwidths. Bars show conventional 95% confidence intervals with standard errors clustered by worker, truncated at ± 4. The dark shaded band shows the net-dues elasticity of -0.75 estimated from the Norwegian union-dues tax deduction by (Dodini et al., 2023).

Table 1: Summary statistics at the worker-month level

	Analysis Sample	Binary DiD Sample		
		All	Treated	Comparison
	(1)	(2)	(3)	(4)
Wage rate	17.04	16.81	15.25	21.00
	(2.83)	(3.43)	(0.00)	(4.39)
Gross earnings per pay period	930.89	886.34	741.98	1275.45
	(652.44)	(636.25)	(471.75)	(829.79)
Employment	0.81	0.78	0.77	0.81
	(0.39)	(0.41)	(0.42)	(0.39)
Hours per pay period	53.63	51.58	48.25	60.57
	(35.57)	(33.45)	(30.78)	(38.33)
Union membership	0.13	0.13	0.14	0.10
	(0.33)	(0.33)	(0.34)	(0.30)
Implied union dues per pay period	3.31	3.11	3.13	3.05
	(9.49)	(9.03)	(8.79)	(9.64)
N Workers	33,560	16,591	12,243	4,348

Notes: This table shows summary statistics for main analysis sample of workers (worked at least 1 hour in the pre-period from July–December 2022, prior to the January 2023 contract announcement). Standard deviations in parentheses. Observations are at the worker×month level. Column 1 reports the full analysis sample; Column 2–4 restricts to workers used in the binary difference-in-differences specification. We define the treatment group as workers earning *exactly* \$15.25 per hour in base wages in the pre-period and the comparison group as workers earning at or above the new contract minimum of \$19 per hour. Extensive margin is an indicator for positive hours worked in a month. Wages, gross pay, membership, and dues are conditional on reporting positive hours worked in a given month.

Table 2: Effects on Wages, Union Membership and Dues

	Wage		Membership		Dues	
	(1) Binary	(2) Cont.	(3) Binary	(4) Cont.	(5) Binary	(6) Cont.
Panel A: Unbalanced sample						
Treatment × Post2023	-0.045*	-0.023***	0.011***	0.002***	0.229***	0.047***
	(0.024)	(0.005)	(0.002)	(0.000)	(0.060)	(0.013)
Treatment × Post2024	2.016***	0.511***	0.033***	0.007***	0.994***	0.213***
	(0.040)	(0.007)	(0.004)	(0.001)	(0.128)	(0.027)
Pre-period mean	16.806	17.042	0.127	0.128	3.112	3.312
Observations	329,558	704,439	329,442	704,301	329,442	704,300
Panel B: Balanced sample						
Treatment × Post2023	-0.080***	-0.038***	0.010***	0.002***	0.188**	0.043***
	(0.025)	(0.005)	(0.002)	(0.001)	(0.075)	(0.016)
Treatment × Post2024	1.860***	0.473***	0.027***	0.006***	0.884***	0.199***
	(0.043)	(0.008)	(0.005)	(0.001)	(0.153)	(0.032)
Pre-period mean	17.112	17.248	0.138	0.140	3.523	3.784
Observations	222,472	507,574	222,470	507,572	222,470	507,572

Notes: This table shows pooled difference-in-difference estimates at the worker-month level. The dependent variables are: hourly wage rate, measured as the lowest base wage across a worker's pay records in the month (columns 1–2); an indicator for union membership (columns 3–4); and implied dues per pay period, computed by applying the dues schedule to gross earnings for union members (columns 5–6). Outcomes are conditional on reporting positive hours worked in a given month. Panel A uses the unbalanced sample, which includes all worker-months for workers active in the pre-period; Panel B restricts to the balanced sample of workers who worked at least once per quarter throughout the full panel window. Odd-numbered columns use the binary treatment, while even-numbered columns use the continuous treatment. Treatment×Post2023 and Treatment× Post2024 report the average treatment effect in the announcement year (2023) and implementation year (2024), respectively, relative to the pre-period (July–December 2022). Regressions include worker and year-month fixed effects. Standard errors are clustered at the worker level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effects on Employment in the Covered Sector

	Hours | working		Log Earnings | working		Participation	
	(1) Binary	(2) Cont.	(3) Binary	(4) Cont.	(5) Binary	(6) Cont.
Panel A: Unbalanced sample						
Treatment × Post2023	0.993***	0.183***	0.005	-0.001	-0.064***	-0.014***
	(0.278)	(0.057)	(0.006)	(0.001)	(0.005)	(0.001)
Treatment × Post2024	-1.351***	-0.548***	0.080***	0.016***	-0.142***	-0.035***
	(0.402)	(0.082)	(0.009)	(0.002)	(0.007)	(0.001)
Pre-period mean	51.599	53.635	6.493	6.549	0.782	0.808
Observations	329,442	704,301	329,442	704,300	497,730	1,006,800
Panel B: Balanced sample						
Treatment × Post2023	1.064***	0.183***	0.010*	-0.000	0.002	0.001
	(0.314)	(0.063)	(0.006)	(0.001)	(0.003)	(0.001)
Treatment × Post2024	-1.431***	-0.528***	0.082***	0.018***	-0.008***	-0.002***
	(0.435)	(0.088)	(0.009)	(0.002)	(0.003)	(0.001)
Pre-period mean	55.949	57.347	6.655	6.696	0.953	0.955
Observations	222,470	507,572	222,470	507,572	232,200	529,800

Notes: This table shows difference-in-difference estimates for hours and gross earnings at the worker-month level. The dependent variables are: total hours per pay period conditional on participation (columns 1–2); gross earnings per pay period conditional on participation (columns 3–4); and participation, i.e. supplying any hours per month (columns 5–6). Panel A uses the unbalanced sample, which includes all worker-months for workers active in the pre-period; Panel B restricts to the balanced sample of workers who worked at least once per quarter throughout the full panel window. Treatment×Post2023 and Treatment× Post2024 report the average treatment effect in the announcement year (2023) and implementation year (2024). Odd-numbered columns use the binary treatment specification, while even-numbered columns use the continuous specification. Regressions include worker and month fixed effects. Standard errors are clustered at the worker level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Control Function Estimated Effects of Wages on Membership Status

	Union member		
	(1)	(2)	(3)
Log wages	0.020***	0.161***	0.161***
	(0.005)	(0.021)	(0.021)
FS residual		-0.166***	-0.175***
		(0.022)	(0.033)
FS residual × Log wages			0.003
			(0.006)
Observations	704,301	704,301	704,301

Notes: This table reports OLS and control function estimates of the effect of log wages on union membership at the worker-month level. The sample is the same as in the continuous difference-in-differences specification, described in Section 4.1. The dependent variable is an indicator for union membership. Columns 2 and 3 include the residual from a first-stage regression of log wages on the instrument, $\text{Post}_t \times \Delta\text{Wage}_i$, where Post_t indicates months from January 2024 onward and ΔWage_i is the worker's pre-period gap to the new $19 wage floor (equation 7). Regressions include worker and year-month fixed effects. Standard errors are clustered at the worker level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Experimental effects

	Engaged (pp)			Joined (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
High salience	2.68***	2.97***	2.45***	0.29***	-0.04	0.04
	(0.31)	(0.68)	(0.49)	(0.11)	(0.19)	(0.15)
Any wage gap		-0.39			-0.00	
		(0.47)			(0.16)	
High salience × Any wage gap		-0.33			0.41*	
		(0.77)			(0.23)	
Wage gap			-0.09			-0.01
			(0.13)			(0.05)
High salience × Wage gap			0.11			0.11*
			(0.20)			(0.06)
Omitted group mean	4.31	4.28	4.28	0.50	0.50	0.50
N	21,353	21,324	21,324	21,353	21,324	21,324

Notes: This table reports experimental estimates on engagement with the union (columns 1–3) and union membership (columns 4–6). The High Salience group received two contacts from the union during the experiment; the omitted group is the Low Salience (single contact) condition. Engagement is defined as replying to a union text message or registering for a Zoom information session. We exclude from engagement negative replies asking to unsubscribe, stop or wrong number. Joined is an indicator for becoming a union member within 90 days of the initial contact in the before or after wave. Both outcomes are in percentage points. Any wage gap is an indicator for workers whose wage was below the new wage floor. The wage gap is defined as the difference between the new wage floor and a worker's baseline (Nov 2023) wage in dollars per hour; it is set to zero for workers already earning at or above the floor. All specifications include strata fixed effects and an indicator for the contact timing. Robust standard error in parantheses. * p<0.10, ** p<0.05, *** p<0.01.

ONLINE APPENDIX

Why Join the Union?

S. Naidu, N. Raheja, N. Singh, and A. Sojourner

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Appendix A Details on Context and Data

Appendix A.1 Administrative data cleaning procedures

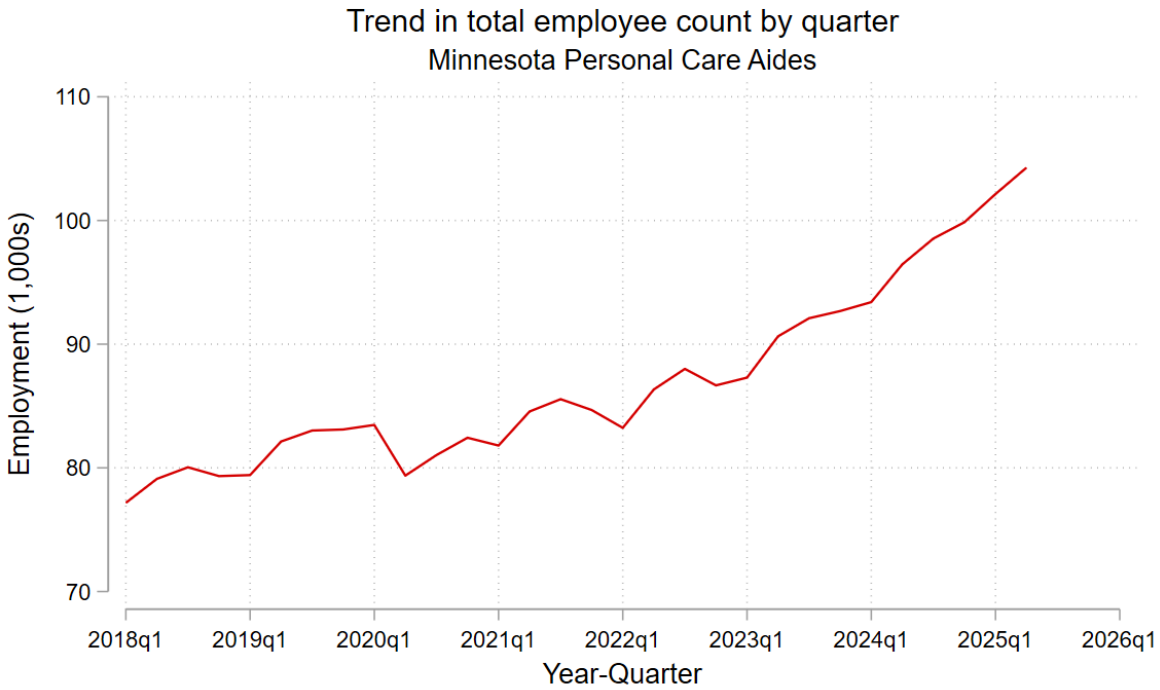
We construct our sample from administrative payroll records for Minnesota home care workers, submitted by fiscal intermediaries to our union data partner . We extracted worker-level pay records and provider metadata from approximately 103,000 Excel files covering 2018–24 (38,000 of which cover 2022–24), successfully parsing over 99 percent of submissions into a raw dataset of roughly 6.1 million observations (3.1 million for 2022–24).

After harmonizing variable definitions across providers and standardizing identifying fields, we dropped records with no worker data, missing provider information, missing identifiers, missing pay information, or missing pay-period dates. We then linked workers to external union bargaining-unit records using provider ID or other identifying information. We are able to successfully link about 98.5 percent of observations. This produces a sample of approximately 5.7 million worker pay-period records (2.8 million for 2022–24), covering 82,325 unique workers (58,270 for 2022–2024). This serves as our raw sample.

We apply some additional cleaning to deal with records with missing information and outlier values. We begin by restrict to our main analysis window between July 2018 and December 2024. We also drop any observations with where wages or pay are recorded as 0 ($\sim$ 730,000 observations). We also drop irregular records where the reported wage is below the contract minimum wage or where reported gross pay differs from hours times wage by more than \$1 ($\sim$ 149,000, observations). We remove separate overtime and holiday pay observations (344,000 records) and reconstruct our main hours and earnings variables to include overtime pay. Finally, we drop worker-months with more than 600 total hours, records with wages above \$200 per hour, and all records for workers who ever have more than 6 pay records in a single month ($\sim$ 244,000 observations).

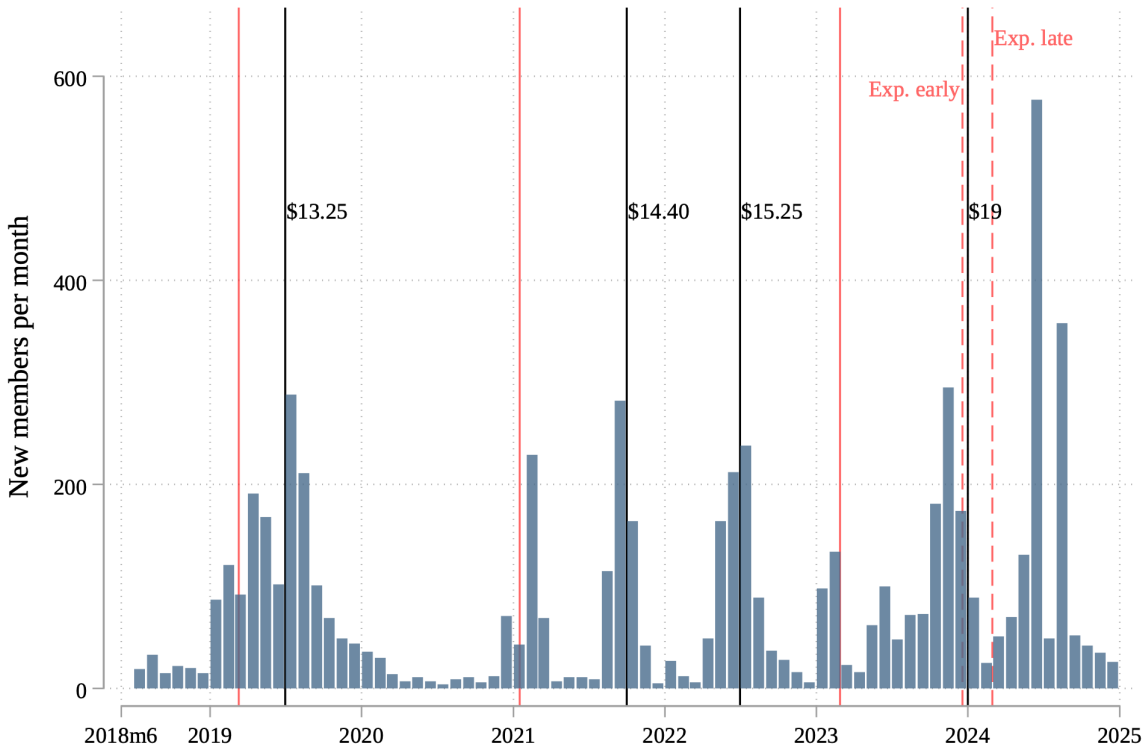
This yields a final cleaned sample of approximately 3.96 million worker–pay-period records (2 million for 2022–24), covering 77,550 workers (55,103 for 2022–24).

Appendix A.2 Total Employment Trends in Home Care - QWI data



Notes: This figure shows total quarterly employment for personal care aides in Minnesota. We combine PCA workers from all relevant 6-digit NAICS categories: 621600 (Home Health Care Services); 623300 (Continuing Care Retirement Communities and Assisted Living Facilities for the Elderly); 623900 (Other Residential Care Facilities); 624100 (Individual and Family Services); and 624300 (Vocational Rehabilitation Services). We then deflate industry-quarter employment by each industry's 2018 share of employment accounted for by PCAs, using BLS Occupational Employment and Wage Statistics (OEWS) data for Minnesota, to arrive at an implied count of PCA employment by quarter.

Appendix A.3 Membership Joins, Monthly Series 2018–2024



Notes: The figure plots the number of home care workers who joined the union in each month from July 2018 to December 2024, based on union membership records. Solid red lines mark the dates on which each contract was signed. Solid black lines mark the dates on which a new contractual wage floor took effect, labeled with the new hourly minimum. Dashed red lines mark the dates of initial contact in the experiment for the early and late waves. The spike in mid-2024 coincides with additional outreach conducted by the union after the conclusion of the experiment.

Appendix B Experimental Details

Appendix B.1 Design overview

The table below summarizes the experimental design and provide details on implementation.

Group	Week of contact	Conctact methods	Implementation issue
Before 1x	18-Dec	SMS+Email	Initial SMS missing link
Before 2x	18-Dec, 8-Jan	SMS+Email	Initial SMS missing link
After 1x	29-Feb	SMS+Zoom option	None
After 2x	29-Feb, 21-Mar	SMS+Zoom option	None

In the early wave, the first text message omitted the membership link due to an implementation error; subsequent messages included the correct link. This means that the "Before 1x" group did not receive a membership link in the first message. The early wave also received emails where available, but since less than 10% of sample had emails, this was dropped for the late wave. Instead, groups in the late wave could sign up Zoom information sessions.

Appendix B.2 Example text messages

We present examples of text messages delivered to non-union members in the randomized outreach experiment.

Initial text message to PCA Choice: Hi [first name]! Making sure you know your wage with [employer] will go to $19+/hr on Jan 1st! Union caregivers negotiating with the state have also won:

- $1000 Retention stipend for 6 months of work
- $500 Training stipend + free training
- $200 EVV stipend
- ANOTHER raise to $20-$22.50/hr next Jan

Has anyone told you about any of this yet? Or about your Union's fight for health coverage & retirement for caregivers? - [agent] @ [union]

Follow-up message to PCA Choice: Hey again, [first name]! Just checking in - have you seen your wage increase yet? Happy to answer any questions, or help navigate any issues.

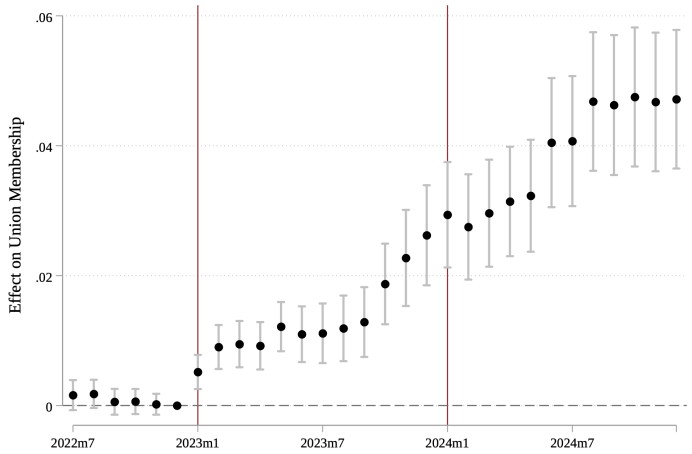
Also wondering if you think we should fight for health insurance and retirement benefits next, or would you like to see something else?

- [agent] @ [union]. Activate your Union membership here! [Link]

Appendix C Additional Results

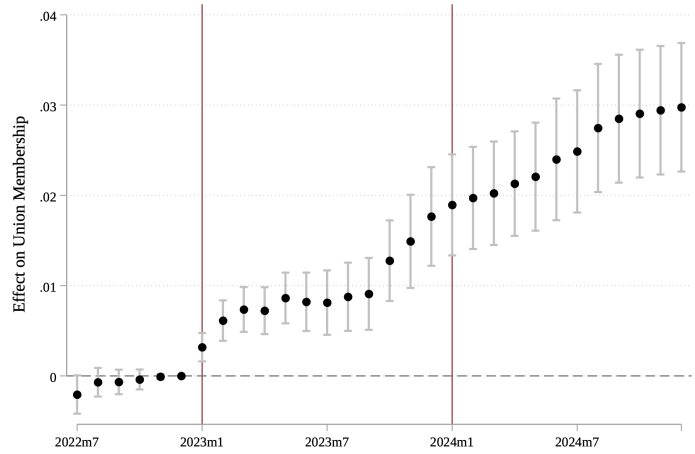
Appendix C.1 DID Estimates

Figure C.1.1: Effect on Membership among Non-Members



Notes: This figure reports DID estimates for union membership among non-members in the preperiod, as per equation 5. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. The sample is restricted to workers who supplied any hours between July–December 2022. Non-working months are coded as missing for all outcomes. Estimates are normalized relative to 2022m12.

Figure C.1.2: Bounded union membership estimates



Notes: This figure reports difference-in-differences estimates as per equation 5. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. The coefficients represent a lower bound on union membership for non-union members in the pre-period that accounts for differential patterns of attrition. Estimates are normalized relative to 2022m12. The pooled difference-in-difference estimate for 2023 is 1.0 p.p. and the 2024 estimate is 2.5 p.p, whereas in the continuous specification, each additional dollar of the pre-period wage gap raises membership by 0.5 p.p. ($p < 0.01$ for all estimates).

Table C.1.1: Pooled Effects on Membership and Dues among Non-Members

	Membership		Dues	
	(1) Binary	(2) Cont.	(3) Binary	(4) Cont.
Panel A: Unbalanced sample				
Treatment × Post2023	0.012***	0.003***	0.214***	0.047***
	(0.002)	(0.000)	(0.056)	(0.012)
Treatment × Post2024	0.038***	0.008***	0.760***	0.168***
	(0.004)	(0.001)	(0.133)	(0.028)
Pre-period mean	0.000	0.000	0.000	0.000
Observations	287,688	612,621	287,688	612,620
Panel B: Balanced sample				
Treatment × Post2023	0.011***	0.003***	0.173**	0.048***
	(0.002)	(0.001)	(0.072)	(0.015)
Treatment × Post2024	0.032***	0.008***	0.626***	0.149***
	(0.005)	(0.001)	(0.161)	(0.034)
Pre-period mean	0.000	0.000	0.000	0.000
Observations	191,667	436,208	191,667	436,208

Notes: This table reports pooled difference-in-differences estimates at the worker-month level, restricting the sample to workers who were not union members during the pre-period (July–December 2022). The dependent variables are an indicator for union membership (columns 1–2) and implied dues per pay period, computed by applying the dues schedule to gross earnings for union members (columns 3–4). Odd-numbered columns use the binary treatment, while even-numbered columns use the continuous treatment gap measure. Panel A uses the unbalanced sample; Panel B restricts to workers who worked at least once per quarter from 2022Q3 to 2024Q4. Treatment×Post2023 and Treatment×Post2024 report the average effect on treated workers in 2023 (after announcement) and 2024 (after implementation), relative to the pre-period. Regressions include worker and year-month fixed effects. Standard errors are clustered at the worker level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

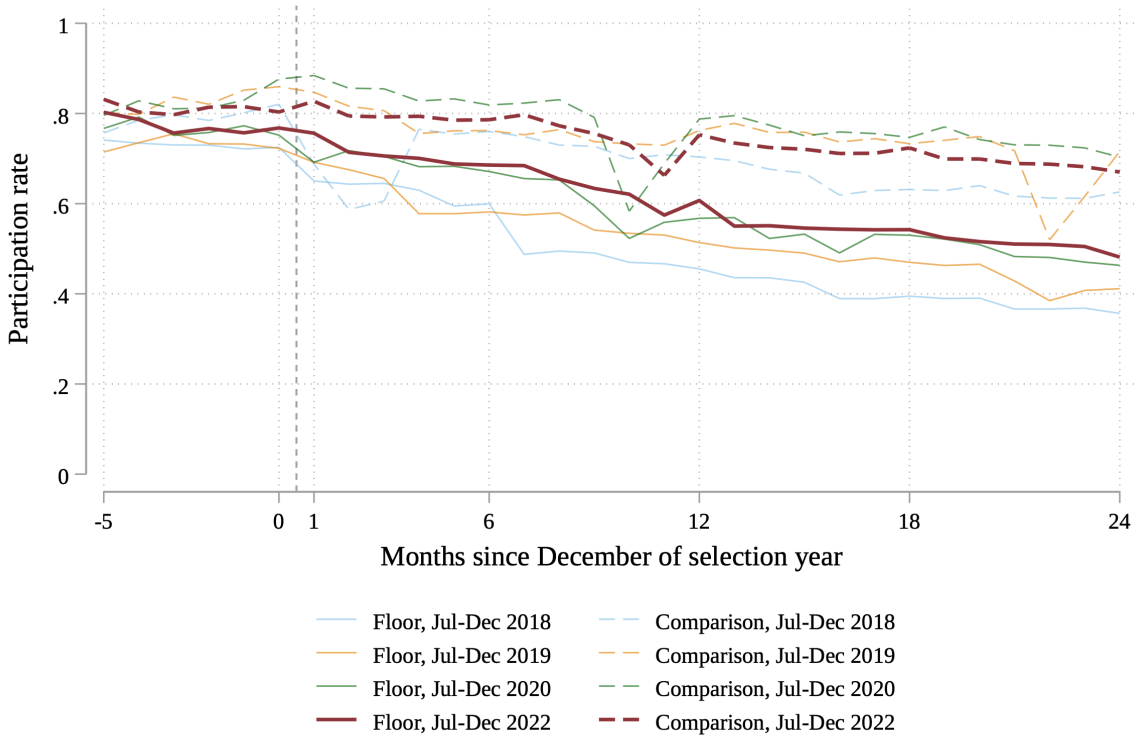
Table C.1.2: Effects on Wages, Membership, and Dues (Alternative Controls)

	(1) $\geq$19 (baseline)	(2) $\geq$20	(3) $\geq$21
Panel A: Wage			
Treatment × Post2023	-0.045*	-0.122**	-0.068
	(0.024)	(0.047)	(0.076)
Treatment × Post2024	2.016***	2.113***	2.174***
	(0.040)	(0.074)	(0.114)
Pre-period mean	16.806	16.314	16.010
Observations	329,558	273,034	252,819
Panel B: Membership			
Treatment × Post2023	0.011***	0.013***	0.016***
	(0.002)	(0.002)	(0.003)
Treatment × Post2024	0.033***	0.037***	0.039***
	(0.004)	(0.005)	(0.007)
Pre-period mean	0.127	0.132	0.135
Observations	329,442	272,918	252,704
Panel C: Dues			
Treatment × Post2023	0.229***	0.324***	0.395***
	(0.060)	(0.068)	(0.086)
Treatment × Post2024	0.994***	1.276***	1.337***
	(0.128)	(0.162)	(0.218)
Pre-period mean	3.112	3.098	3.135
Observations	329,442	272,918	252,704

Notes: This table reports pooled difference-in-differences estimates at the worker-month level using alternative wage thresholds to define the comparison group, to assess sensitivity to wage spillovers above the new floor. Column 1 reproduces the baseline specification, in which the comparison group consists of workers earning at least \$19 in the pre-period; columns 2 and 3 raise this threshold to \$20 and \$21, respectively, which reduced the comparison group from 4,348 workers to 2,060 and 1,033. The dependent variables are the hourly wage rate (Panel A), an indicator for union membership (Panel B), and implied dues per pay period (Panel C). All columns use the unbalanced sample. Treatment×Post2023 and Treatment×Post2024 report the average effect on treated workers in 2023 (after announcement) and 2024 (after implementation), relative to the pre-period. Regressions include worker and year-month fixed effects. Standard errors are clustered at the worker level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C.2 Participation Rates by Cohorts

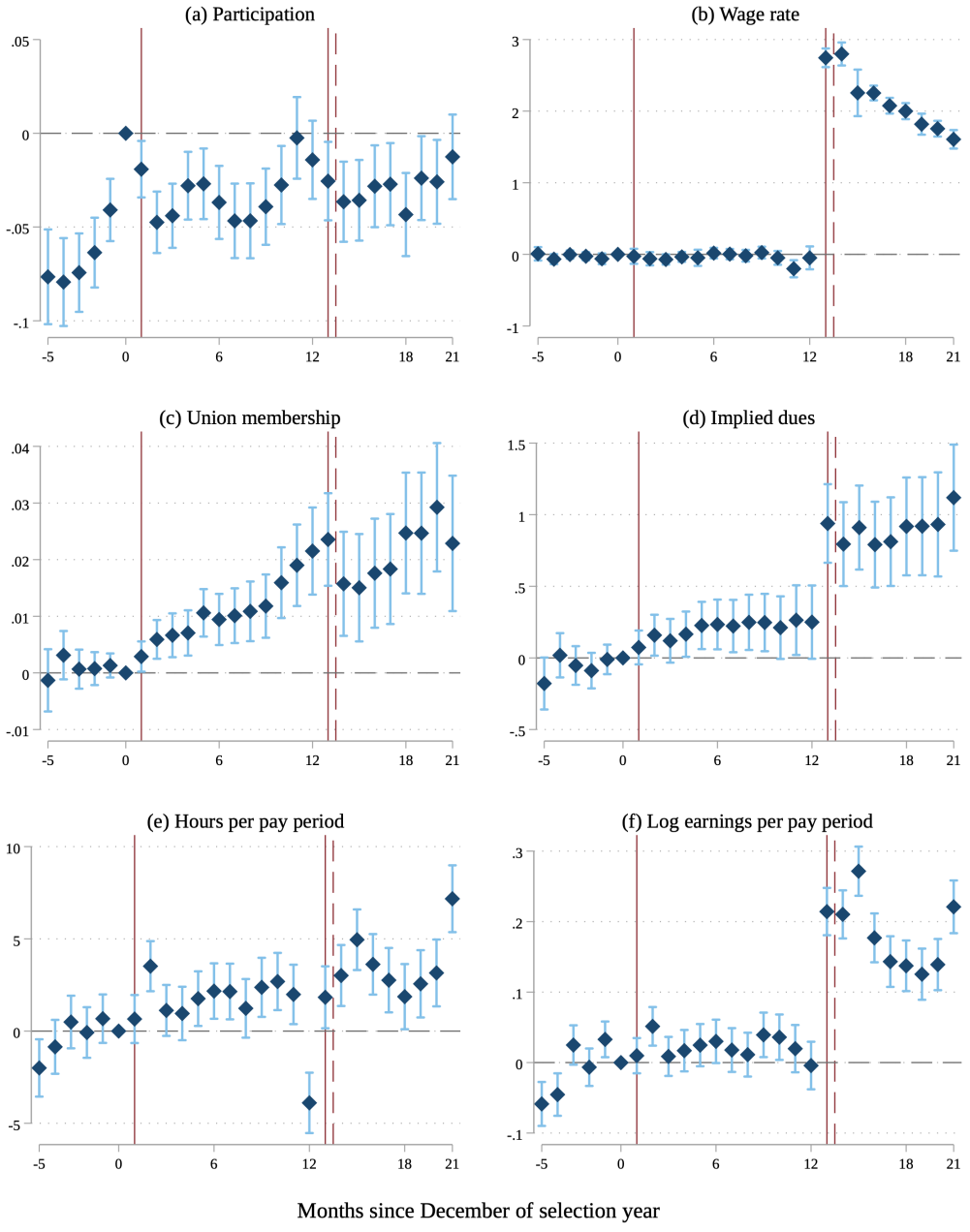
The figure below plots participation rates for floor and comparison workers in four cohorts, each constructed with the same procedure as our main sample from workers active in July–December of 2018, 2019, 2020, and 2022. The cohorts face wage floor increases at very different points in event time: month 7 for the 2018 cohort, month 22 for 2019, months 10 and 19 for 2020, and month 13 for 2022. Despite this, participation among floor workers follows a similar path in all four. It falls steadily from the end of the selection window to two years later. Participation among comparison workers also declines, but less steeply. As a result, the gap between workers at the wage floor and those comparison workers making 25% above the wage floor widens in every cohort.



Notes: This figure plots the share of workers supplying any hours in the covered sector in each month, by cohort and group. Each cohort consists of workers who supplied any hours in July–December of the indicated year. Floor workers earned exactly the prevailing wage floor during the selection window; comparison workers earned at least 125% of the floor. Event time is measured in months since December of the selection year; the vertical dashed line marks the end of the selection window. Temporary dips in some cohorts reflect missing data.

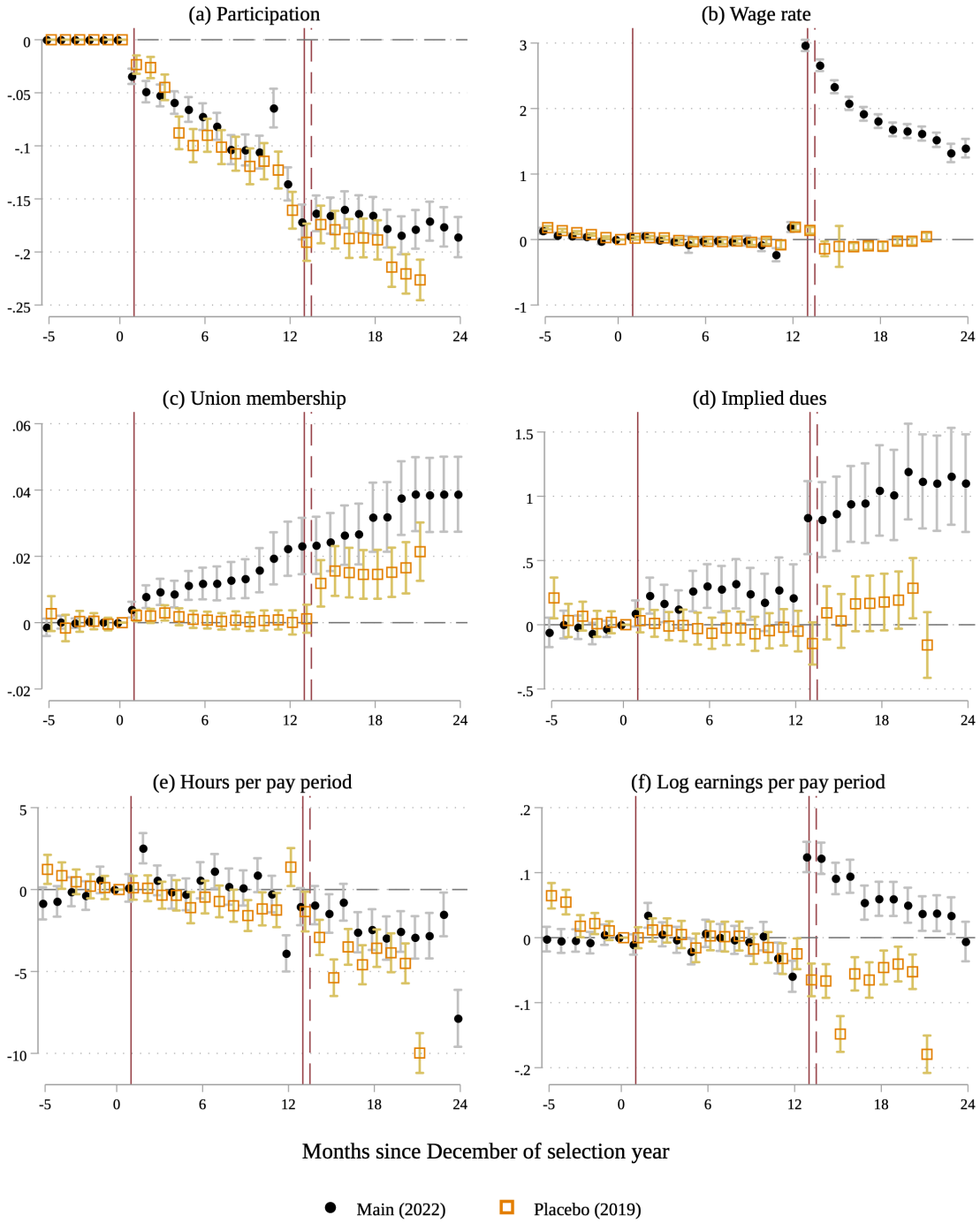
Appendix C.3 Estimation with placebo cohorts

Figure C.3.1: Triple-difference estimates, ever worked in July-Dec 2022



Notes: This figure reports triple-difference estimates of our main outcomes using equation 6, which compares the effect in the main 2022 cohort with that in the placebo 2019 cohort. Samples in both main and placebo cohorts are restricted to those ever worked in the pre-period. Event time is measured in months since December of the selection year, and estimates are normalized to h=0. Solid red lines mark the main cohort's contract announcement (h=1) and the implementation of the increase in the wage floor to $19 (h=13). The dashed red line marks the placebo cohort's 2021 contract announcement in January 2021. Standard errors are clustered at the worker level.

Figure C.3.2: DID: Main and Placebo Cohorts, worked every month in July-Dec 2022



Notes: This figure reports DID estimates separately for our main 2022 cohort and the placebo 2019 cohort. Samples in both main and placebo cohorts are restricted to those who worked every month in the pre-period. Event time is measured in months since December of the selection year, and estimates are normalized to h=0. Solid red lines mark the main cohort's contract announcement (h=1) and the implementation of the increase in the wage floor to $19 (h=13). The dashed red line marks the placebo cohort's 2021 contract announcement in January 2021. Standard errors are clustered at the worker level.

Appendix C.4 Experiment: Additional Results

Table C.4.1: Randomization Balance

	Before 1x	Before 2x	After 1x	After 2x	p-value	N
Panel A: Full sample						
Baseline wage ($)	17.18	17.13	17.14	17.18	0.610	25,900
	(2.60)	(2.57)	(2.56)	(2.52)		
Female	0.74	0.74	0.74	0.74	0.947	20,989
	(0.44)	(0.44)	(0.44)	(0.44)		
White	0.77	0.77	0.77	0.77	0.604	23,991
	(0.42)	(0.42)	(0.42)	(0.42)		
Twin Cities	0.55	0.55	0.54	0.56	0.430	23,991
	(0.50)	(0.50)	(0.50)	(0.50)		
Panel B: Analysis sample						
Baseline wage ($)	17.16	17.10	17.10	17.14	0.376	21,324
	(2.64)	(2.61)	(2.45)	(2.53)		
Female	0.75	0.75	0.74	0.75	0.558	17,140
	(0.43)	(0.43)	(0.44)	(0.43)		
White	0.76	0.75	0.76	0.75	0.422	19,698
	(0.43)	(0.43)	(0.43)	(0.43)		
Twin Cities	0.54	0.55	0.54	0.55	0.619	19,698
	(0.50)	(0.50)	(0.50)	(0.50)		

Notes: This table reports balance in baseline characteristics across the four experimental arms. Panel A includes all bargaining unit-eligible workers residing in Minnesota with available contact information who were not union members as of November 2023 (N = 25,933); whereas Panel B restricts to the analysis sample, defined as those who had mobile numbers and could receive text messages (N=21,353). Columns 1–4 report group means for workers contacted once or twice, before or after the January 2024 wage increase. The p-value is from an F-test of the joint null hypothesis that means are equal across all four groups from a regression of each characteristic on the group indicators, with strata fixed effects for program type and fiscal intermediary. N reports the number of workers with non-missing values for each characteristic. Twin Cities indicates that a worker's geocoded home address is in the seven-county Minneapolis–St. Paul metropolitan area. Gender is predicted from first names using Social Security Administration birth records, and is assigned when at least 90% of births with that name share a sex. Race is predicted from names and Census block location using Bayesian Improved Surname Geocoding (Imai et al., 2022). Observations may be lower due to missing data.

Table C.4.2: Wave-specific treatment effects

	Engaged (pp)			Joined (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
Before 2x	2.68***	1.65*	1.30*	0.45***	0.04	-0.10
	(0.44)	(0.94)	(0.68)	(0.15)	(0.32)	(0.22)
After 1x	0.26	-0.69	-0.73	0.24*	-0.18	-0.15
	(0.39)	(0.81)	(0.59)	(0.14)	(0.28)	(0.21)
After 2x	2.93***	3.59***	2.86***	0.37**	-0.29	0.03
	(0.45)	(1.02)	(0.73)	(0.14)	(0.26)	(0.22)
Before 2x × Any wage gap		1.27			0.51	
		(1.06)			(0.36)	
After 1x × Any wage gap		1.09			0.52	
		(0.92)			(0.32)	
After 2x × Any wage gap		-0.84			0.82***	
		(1.13)			(0.31)	
Before 2x × Wage gap			0.62**			0.24***
			(0.27)			(0.09)
After 1x × Wage gap			0.41*			0.18**
			(0.24)			(0.08)
After 2x × Wage gap			0.02			0.15*
			(0.28)			(0.08)
Any wage gap		-0.92			-0.26	
		(0.68)			(0.24)	
Wage gap			-0.29			-0.10*
			(0.18)			(0.06)
p: 2x = 1x, Before	0.000	0.078	0.055	0.003	0.905	0.662
p: 2x = 1x, After	0.000	0.000	0.000	0.430	0.616	0.412
p: 2x = 1x interaction, Before		0.231	0.023		0.154	0.006
p: 2x = 1x interaction, After		0.081	0.170		0.318	0.798
Omitted group mean	4.20	4.20	4.20	0.37	0.38	0.38
N	21,353	21,324	21,324	21,353	21,324	21,324

Notes: This table reports experimental estimates on engagement with the union (columns 1–3) and union membership (columns 4–6) separately by contact timing. Workers were contacted either before or after the wage increase, and received either one contact (1x) or two contacts (2x). The omitted group is workers contacted once before the wage increase (Before 1x). Engagement is defined as replying to a union text message or registering for a Zoom information session. We exclude from engagement negative replies asking to unsubscribe, stop or wrong number. Joined is an indicator for becoming a union member within 90 days of the initial contact in the before or after wave. Both outcomes are in percentage points. Any wage gap is an indicator for workers whose wage was below the new wage floor and who therefore received a contract-driven wage floor increase. The wage gap is defined as the difference between the new wage floor and a worker's baseline (Nov 2023) wage in dollars per hour; it is set to zero for workers already earning at or above the floor. Columns 2 and 5 interact each group with any raise, and columns 3 and 6 with the wage gap; the main effects of any raise and the wage gap are the slopes for the omitted group. The bottom panel reports p-values for tests that two contacts have the same effect as one within each wave (Before 2x = Before 1x; After 2x = After 1x), for the group effects and for their interactions. All specifications include strata fixed effects. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Appendix C.5 Evidence on peer effects

Table C.5.1: Peer Effects on Membership Among Control Workers

	Dep. var: Union membership					
	2022 peers			2023 peers		
	(1) Member Share	(2) Treat Share	(3) Both	(4) Member Share	(5) Treat Share	(6) Both
Post2023 × FI Member Share	0.061		0.060			
	(0.039)		(0.039)			
Post2024 × FI Member Share	0.060		0.060	-0.012		-0.011
	(0.115)		(0.113)	(0.133)		(0.134)
Post2023 × FI Treat Share		0.005	0.003			
		(0.011)	(0.011)			
Post2024 × FI Treat Share		-0.003	-0.004		0.005	0.005
		(0.058)	(0.057)		(0.047)	(0.048)
Observations	95,502	95,502	95,502	74,868	74,868	74,868
FIs	22	22	22	22	22	22

Notes: This table considers peer effects on union membership among comparison workers, defined as those already earning at or above the new $19 wage floor in the preperiod. The dependent variable is an indicator for union membership. The sample is restricted to those who worked at a single FI in the relevant window: July–December 2022 for columns 1–3 and July–December 2023 for columns 4–6. FI Treat Share is the leave-one-out share of the worker's coworkers at that FI who are in the treatment group, and FI Member Share is the corresponding share of coworkers who were union members, both measured in the same window. Treatment status is defined by pre-period wages in both cases, and shares are held fixed across each worker's panel. Columns 1–3 cover July 2022–December 2024; columns 4–6 cover 2023–2024, so the Post2024 coefficient is relative to 2023. Columns 1 and 4 include only the member share interactions, columns 2 and 5 only the treat share interactions, and columns 3 and 6 both. All specifications include worker and calendar-month fixed effects. Standard errors are clustered by FI. * p<0.10, ** p<0.05, *** p<0.01.

Table C.5.2: Membership Effects with FI × Month Fixed Effects

	Depvar: Union membership		
	(1) Baseline	(2) Current FI × Month FE	(3) 2022 FI × Month FE
Treatment × Post2023	0.002***	0.000	0.001
	(0.000)	(0.000)	(0.000)
Treatment × Post2024	0.007***	0.003***	0.002**
	(0.001)	(0.001)	(0.001)
FI × Month FE	No	Yes	Yes
Observations	701,375	700,933	700,852

Notes: This table tests whether individual wage gains predict union membership independently of the peer environment at a worker's fiscal intermediary (FI). The dependent variable is an indicator for union membership. Treatment Intensity is the continuous wage gap, ΔWage_i, defined as the difference between the new $19 wage floor and a worker's pre-period wage, set to zero for workers already earning at or above the floor. Column 1 reports the baseline continuous specification with worker and year-month fixed effects. Columns 2 and 3 add FI × month fixed effects, using each worker's current FI in that month or their pre-period FI, respectively. Standard errors clustered at the worker level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix C.6 Evidence on union efficacy

Table C.6.1: Effect on Membership by Attachment Measures in 2022

	Depvar: Union Membership	
	(1) Months	(2) Hours
Treatment × Post2023: low attachment	0.0116***	0.0105***
	(0.0039)	(0.0024)
Treatment × Post2023: high attachment	0.0117***	0.0133***
	(0.0021)	(0.0028)
Treatment × Post2024: low attachment	0.0426***	0.0313***
	(0.0084)	(0.0059)
Treatment × Post2024: high attachment	0.0292***	0.0325***
	(0.0046)	(0.0056)
p: high = low (2023)	0.984	0.440
p: high = low (2024)	0.162	0.878
Observations	329,442	329,442

Notes: This table reports binary difference-in-differences effects on union membership, estimated separately for workers with low and high attachment to the sector in 2022. In column 1, high attachment means working at least 9 months in 2022; in column 2, it means working at least 1,000 hours in 2022. Each column comes from a single regression of monthly membership on the treatment indicator interacted with Post2023 and Post2024 separately for each attachment group. All specifications include worker fixed effects and attachment-group × year-month fixed effects. The bottom rows report p-values for equal effects across attachment groups. Standard errors are clustered at the worker level. * p<0.10, ** p<0.05, *** p<0.01.

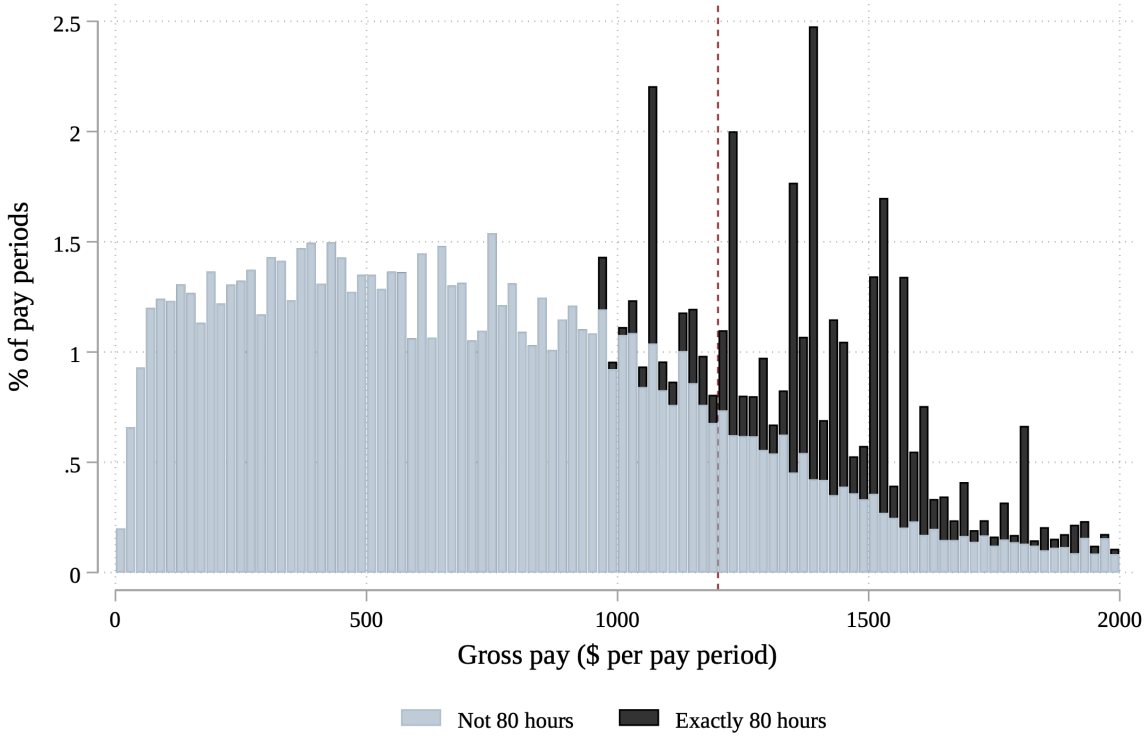
Appendix C.7 Regression Kink Design: Additional Results

Table C.7.1: RKD Estimates

Elasticity of membership w.r.t. dues	-0.53
	(0.69)
Bias-corrected elasticity	-1.01
	(1.09)
Elasticity at other bandwidths	
Bandwidth × 0.5 ($80)	-1.10
	(1.99)
Bandwidth × 0.75 ($120)	0.07
	(1.10)
Bandwidth × 1.5 ($239)	-0.19
	(0.43)
Bandwidth × 2 ($319)	-0.04
	(0.32)
Change in slope (pp per $100)	0.65
	(0.84)
Membership near the kink (%)	14.7
Bandwidth ($)	159
Pay periods (left, right)	(303,073, 300,101)

Notes: This table reports elasticity of union membership with respect to dues. We obtain the elasticity ε by multiplying the change in the slope of membership at the kink by $-1,200$ and dividing by the membership rate near the kink (14.7%), where the $36 dues cap binds. Local linear with a triangular kernel, following Calonico et al. (2014). Standard errors are clustered by worker in parentheses (conventional in Panels A and C, robust in Panel B). Panel C re-estimates at multiples of the MSE-optimal bandwidth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.7.1: Distribution of Earnings Around the Dues Cap



Notes: This figure plots the share of pay periods in $20 bins of gross earnings for all workers from July 2018 to December 2024. The dashed vertical line marks the $1,200 dues cap. Dark bars are pay periods with exactly 80 hours, and light bars are all other pay periods. The 80-hour pay periods create mass points at 80 times common wage rates, such as $1,200 (80 hours × $15) and $1,220 (80 hours × $15.25), which reflect standard full-time schedules rather than responses to the dues cap.