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

Suresh Naidu
Nikita Raheja
Niharika Singh
Aaron Sojourner

September 2026

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 3.2 percentage point increase in membership, concentrated among workers experiencing the largest wage gains. We provide evidence that this reflects gift-exchange, rather than income or price effects: only wage variation driven by the contract, rather than other sources, increases membership and a change in the marginal dues rate does not change membership. A randomized outreach experiment further shows that union contact increases membership only for workers experiencing large wage gains, consistent with a gift-exchange mechanism. Embedding these effects in a model of union public good financing, we show that while gift-exchange allows the union to partly internalize non-members' interests, our estimated marginal value of union funds exceeds the dues rate, suggesting that free-riding constrains the benefits unions can deliver.

*Naidu and Raheja (Columbia University); Singh (London School of Economics and Political Science); Sojourner (Upjohn Institute). We are grateful to Tyler Frank, Carrie Salmia, and Philip Cryan for sharing their knowledge and expertise on the homecare sector in Minnesota. We thank Amanda Kreider, Michael Reich for helpful comments. Allison Hainstock provided excellent research assistance. IRB approval was received by Columbia University.

1 Introduction

In many collective bargaining systems, negotiated wages and working conditions extend to every worker in the industry, regardless of individual union membership. These systems, common in Europe and Latin America, are increasingly discussed in the U.S. as a response to declining union density and persistent high inequality (Farber et al., 2021; Dube, 2026).[1] While these models expand union coverage, their 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 in these settings remains an open question.

This paper examines the role of social preferences in sustaining union membership in voluntary settings. Workers may respond to union-provided 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 rather than reciprocity per se (Ostrom, 1999).

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. This setting closely resembles European sectoral bargaining: the union negotiates contracts that apply to all covered workers regardless of membership, and—following *Harris v. Quinn* (2014)—cannot collect fees from non-members, making free-riding individually rational. In 2023, the union negotiated a large increase in the sectoral wage floor from $15.25 to $19 per hour, a 25% increase, to be implemented from January 1, 2024. Importantly, the wage increase was fully funded by increases in the state Medicaid budget, leaving little scope for reductions in hours or employment, unlike other wage increases.

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

[2] Several European countries mitigate free-riding through selective incentives: Scandinavian unions manage access to state-subsidized unemployment insurance via the Ghent system, while German unions historically restricted works council participation to members (Jäger et al., 2025). In the U.S., by contrast, the duty of fair representation and other features of labor law generally prevent unions from conditioning collectively-bargained benefits on membership.

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 determinants 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. We have three main findings.

First, using an event-study difference-in-differences design, we show that workers experiencing large wage increases—those moving from the previous minimum of $15.25 to the new minimum of $19—are significantly more likely to become union members, relative to workers already earning above the new wage 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 3.2 percentage points by 2024—a 31% increase relative to the pre-period membership rate of 10.3%. This pattern suggests that workers respond to both anticipated and realized contract-driven wage gains.

Second, we show that price and income effects cannot explain the rise in membership. A regression kink design exploiting a cap in the dues schedule, where the marginal cost goes from 3% of earnings to 0%, finds no membership response at the kink. To rule out income effects, we employ a control function approach, which shows that only wage variation driven by the increase in the wage floor increases membership. Wage variation arising from other sources (e.g., experience, acuity of care, or spillovers) does not generate similar responses. Together, these results rule out price and income effects as a driver of membership.

Third, we provide direct experimental evidence that the attribution of wage gains to the union is central to the membership response. In a randomized messaging experiment, higher union contact substantially increases engagement with the union, but converts into membership only among workers who experienced large wage gains, with little effect on workers who were already above the new wage floor. This heterogeneity in response underpins the gift-exchange mechanism, and is not consistent with other explanations such as reduced information frictions, increased salience of the union, or altruism towards the union.

We also examine employment in the covered sector. Among incumbents, for the extensive margin, we find that treated workers become significantly less likely to work in the covered sector following the 2023 contract announcement. This gap emerges immediately after contract announcement, a full year before the wage increase is implemented, and continues afterwards as well. This differential exit raises a natural concern for our membership estimates: if treated workers who leave the covered sector are systematically different in ways correlated with their gift-exchange propensity from those who stay, our DID estimates could be biased. We address this concern in two ways. First, we provide estimates restricting to workers observed continuously throughout the panel. These balanced-sample effects are of similar magnitude to the unbalanced sample, which lets worker composition change over time, indicating that our results are not an artifact of differential attrition. Second, we bound our membership estimates under alternative assumptions about the membership status of workers who exit the sample, and find that our estimates are robust to these bounds. Conditional on participation, we find small increases in hours worked after contract announcement

and then small declines after contract implementation, suggesting the wage increase did not substantially depress hours among stayers. Given the muted effect on hours, earnings conditional on participation rise significantly after the wage increase.

Separately, we provide evidence that the extensive margin decline likely reflects reallocation of clients and workers toward agencies operating in the uncovered sector, which faces the same Medicaid reimbursement rate but is not bound by the union contract, rather than genuine exit from home care work. As the participation decline occurs even prior to the actual wage increase, the anticipatory timing points away from a response to realized labor costs and toward a forward-looking response to agency incentives. We also show that aggregate personal care aide (PCA) employment grows over the same period, consistent with reallocation of workers across home care models.

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. Without gift-exchange, the 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. Even so, voluntary dues collection falls short of the public good provision that could be sustained with mandatory dues collection. Using our estimated gift-exchange and dues elasticities, alongside external estimates on the efficacy of lobbying expenditures, we find that the marginal value of union funds is substantially larger than the current dues rate, suggesting under-provision of public goods in voluntary membership settings. These results highlight a central tension in sectoral bargaining systems: while free-riding is partially mitigated through social preferences, it cannot fully substitute for mandatory dues collection and limits union power.

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, demonstrating the clear limits of social preferences in overcoming the free-rider problem. Foy (2026) provides complementary evidence in a right-to-work setting among Wisconsin teachers, showing that individual membership decisions respond to beliefs about coworkers' membership rates, consistent with a social-custom mechanism.

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 (DellaVigna et al., 2022; Fehr and Charness, 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 workers are willing to 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 foreign labor supply shapes the availability and quality of long-term care workforce (e.g., Shen, 2024; Gandhi et al., 2024; Kreider and Werner, 2025). 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), roughly doubling over the last decade and now exceeding 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). In this section, we first 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 traditional agency-based programs and 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.

models. Only care workers under the CD model are represented by Service Employees International Union (SEIU) Healthcare Minnesota & Iowa. We estimate that approximately 30% of the home care workforce in Minnesota is covered by union representation.

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—that is, 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.

Within the consumer-directed programs, wage-setting differs across program types. Under PCA Choice, wages are closely tied to the reimbursement rates negotiated between the state and the union, resulting in hourly wages that are anchored to a contractual minimum with variation determined by acuity of care and experience. In contrast, under budget-based models—Community Support Grants (CSG) or Consumer-Directed Community Support (CDCS)—households are allocated an individualized budget and determine 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 union wage floors funded by the state Medicaid budget likely also put upward pressure on wages paid by 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 choose to pay. Using claims data obtained via a public records request from the Minnesota Department of Human Services, we find that 83% of agencies operated both the CD and Agency models simultaneously; among those that operate the PCA Choice model at all, 98% also offer work under the Agency model.[4] This dual-track structure highlights that private agencies may adjust their labor allocation across models in response to union-negotiated rate increases.

2.2 Collective bargaining and dues structure

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 SEIU at two-year intervals from 2015-2025. Over this period, the contractual minimum wage has increased from $10.75 to

[4]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.

$20 per hour. In addition, these contracts provide paid-time off (PTO), training and retention stipends, holiday pay for all eligible workers, and, most recently, an experience-based wage scale.

Following a 2014 U.S. Supreme Court decision (*Harris v. Quinn*), home care workers are not required to pay agency fees to support collective bargaining. However, those 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. Given the 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).

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 SEIU Healthcare 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, resignation.[5] After processing and cleaning the raw data provided in Excel files, we obtain a sample of approximately 4.1 million records at the worker-pay period level from 79,042 unique workers from 2018–2024.[6] Figure 1 shows the evolution of the wage floors and the quarterly total of union-eligible workers and union members from January 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.[7] The hourly wage rate is defined separately as the average 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.

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.

[5] 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 resigned within 2 years of joining.

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

[7] This normalization avoids a mechanical pattern in monthly totals: because 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.

2.4 Descriptive patterns

Using the full panel from 2018–24, Figure 2 documents the baseline cross-sectional relationships between monthly worker-level outcomes and voluntary union membership, controlling for year-month fixed effects. Higher hourly wage rates in a given month exhibit a statistically significant negative association with union membership (slope= -0.002, SE= 0.0003), indicating that voluntary membership is slightly concentrated among lower-wage tiers. Conversely, Panel (b) shows a strong positive gradient between monthly hours worked and union membership (slope= 0.0003,SE= 0.00029). Workers with higher monthly intensive-margin labor supply are substantially more likely to be union members, with membership rates rising from under 10% among low-hour workers (less than 50 hours per month) to about 15% for those working full-time monthly schedules (more than 150 hours per month).

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 $a_i \in [0,1]$. The realized wage is $W_i = \max(MinWage, w_i)$, earnings are $y_i = W_i l_i$, and dues are $\tau(y) = \min(0.03\,y,\, 36)$ per pay period (piecewise linear in earnings). Workers have a utility function $U(c,l)$ over consumption and leisure. A warm-glow taste ω_U and a gift-exchange parameter ω_G (constant across workers) govern the non-pecuniary value of membership.

Indirect utility from membership and non-membership is

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

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

Let $A(W_i)$ be the threshold salience a_i at which $V_1^i = V_0^i$. 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. Conditional on participation, the membership share is

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

Comparative statics. 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.

Social preferences alone are not enough to deliver positive union membership. If social preferences only entail gift-exchange, with no warm-glow motive $\omega_U = 0$, but also no binding floor ($W_i = w_i$), membership is also zero.

A higher minimum wage moves s through three channels in $A(w_i)$: (i) gift exchange via $\omega_G(W_i - w_i)$ (+); (ii) an income effect through U_c (+ under diminishing marginal utility of consumption); and (iii) a dues channel through $\tau(y^*)$ (−, and zero whenever dues bind at the cap). With quasi-linear U and capped dues, only the gift-exchange channel operates. Raising the dues rate or the cap increases $\tau(y^*)$ and lowers s.

Let T index an experimental intervention that strengthens attribution, so that $F(a \mid w_i, T)$ is decreasing in T for all (a, w_i) (first-order stochastic dominance). Then $ds/dT > 0$. The cross-partial $d^2s/(dT\, dMinWage)$ is unsigned absent further structure on the joint distribution of (a_i, w_i).[8]

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 negotiations. This setting offers two features that make it well-suited to studying how membership responds to wage changes. First, this wage increase was the largest the union had secured since 2014: the contract raised the minimum wage from $15.25 to $19.00 per hour for all union-eligible workers, effective January 1, 2024 (see Figure 1. Second, the timeline separates the announcement of the wage increase from its implementation, allowing us to distinguish membership responses to anticipated gains from responses to realized gains. A tentative agreement outlining the new wage schedule was reached on January 6, 2023, and the contract was formally ratified the following month, in February 2023, nearly a full year before the higher wages took effect.

Sample: We define our estimation sample using employment in the pre-period: the three months prior to the contract agreement in January 2023. A total of 29,500 workers worked at all during this time, approximately 80% of all workers active during 2022. For these workers, we construct a monthly panel spanning August 2022 through December 2024.[9] We present results using two versions of this panel. In the unbalanced panel, the sample composition can vary month to month as workers who did not supply any hours have missing wage and membership information. In the balanced panel, we restrict to those who worked at least once per quarter throughout the full panel window from 2022q4 to 2024q4 (N=18,125), ensuring a

[8] A sufficient condition for $\frac{d^2s}{(dT\, dMinWage)} \geq 0$ is non-decreasing density $f_a(a|w,T) \geq 0$ for all w, T along with a monotone likelihood ratio so that $f(a|w,T_1) > f(a|w,T_0)$ for $T_1 > T_0$.

[9] A separate, smaller wage increase took effect in July 2022, raising the wage floor from $14.40 to $15.25 per hour. As a robustness check, we also verify that our results are consistent when using the full 2022 calendar year to define the pre-period sample.

consistent set of workers are observed in every period and assessing whether our results are sensitive to sample attrition.

Specifications: Using this sample, we first present a binary treatment specification, as in equation 5. 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, while comparison workers are already paid at or above the new floor and so experience no mechanical wage change from the contract itself.[10] The treatment group constitutes 28% of the estimation sample, and the comparison group 16%.

$$Y_{it} = \sum_{k=-5}^{24} \beta_k AtMinWage_{2022,i} \cdot \mathbb{1}[t - t_w = k] + \gamma_i + \delta_t + \varepsilon_{it} \tag{5}$$

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 five months before (k=-5) to twenty-four months after (k=24). 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 $\Delta Wage_i = max(0, MinWage_{2024} - Wage_{2022,i})$, 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 proportional to how far below the new floor their pre-period wage was. This allows us to use variation from the full sample, including workers 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 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 is muted and increases sharply only at the time of implementation in January 2024. We observe a modest rise in the month preceding imple-

[10] Comparison-group workers, though above the new contract minimum, could still experience spillover wage effects, attenuating our estimates. As a robustness check, we present our main results using a higher wage threshold of \$20 and \$21 for the comparison group (see Appendix C.1.2).

mentation and a substantial, discrete jump at and after the wage increase. The steady decline in the effect in 2024 suggests spillover increases for the comparison group over time.

The pooled DiD estimates in Table 2 reflect this timing: the 2023 estimate is small ($0.06 per hour, column 1) though statistically significant, while the 2024 estimate shows an average, significant increase in the wage rate of $2 per hour. Results are similar using the continuous specification, which shows a dose-response relationship: each additional dollar of the pre-period wage gap raises the 2024 wage rate by $0.52 (column 2). These patterns hold across both unbalanced and balanced samples.

4.3 Union Membership and Dues

Unlike wage rates, Figure 3b shows that union membership responds immediately following the announcement of the new contract minimum wage, *before* any wage gains are realized. Membership continues to rise gradually in the months following implementation of the wage increase. This figure includes both members and non-members from the pre-period.

Table 2, columns 3–4, present the average pooled estimates. The 2023 estimate is already 1.1 p.p., with the 2024 estimate rising further to 3.2 p.p. (Panel A, column 3)—a 31% increase relative to the pre-period mean. The continuous specification shows the dose-response pattern: each additional dollar of the pre-period wage gap raises membership by 0.7 p.p. (column 4). Appendix Table C.1.1 presents results restricting to non-members only and finds similar effects.

Dues payments follow a similar trajectory to membership, though the response builds more slowly (see Figure 3c). The 2023 estimate is not statistically significant, while the 2024 estimate rises to $0.49 per pay period in the binary specification (Panel A, column 5)— a 26% increase relative to the pre-period mean—and $0.20 per dollar of the pre-period wage gap in the continuous specification (column 6), a 10% increase relative to the pre-period mean.

One concern with the unbalanced panel is that if treated workers have differential exit rates from the sector (as we show in the next section), this would bias our estimates. Reassuringly, the balanced panel shows similar effects (Panel B, columns 3–6), suggesting our results are not purely driven by compositional differences in the sample over time.

4.4 Employment in the Covered Sector

4.4.1 Participation, hours, and earnings

Figure 4 and Table 3 present our event-study and pooled estimates for employment, hours worked per pay period, and earnings per pay period, respectively.

Panel (a) of Figure 4 shows that treated workers become steadily less likely to remain active in the covered sector following the contract announcement, with the extensive-margin gap widening further after implementation in January 2024. In the unbalanced sample, treated workers are 6.7 p.p. less likely to be employed in the covered sector in 2023 (column 1) and

15.7 p.p. less likely in 2024, with a similar pattern in the continuous specification (column 2). When we restrict to workers with continuous participation, this pattern reverses during the announcement period, and treated workers are instead 2 p.p. more likely to be employed in 2023 (column 1), before the gap turns negative and near zero in 2024. This differential attrition could reflect either genuine exit from home care work or reallocation toward the uncovered Agency sector, a distinction we return to in section 4.4.2.

Conditional on participation in the covered sector, treated workers see a modest increase in hours per pay period following the announcement (Panel A, column 3). This reverses after the implementation of the wage increase, where treated individuals work one less hour per pay period. The pattern is similar in the continuous specification and for the balanced sample. Given the small effect on hours and large increase in wages, earnings per pay period increase significantly (column 5); effects are positive but attenuated in the continuous specification (column 6).

Together, these results suggest that the main margin of adjustment following an increase in the wage floor is on the participation side, rather than hours conditional on employment. Given that the wage floor increase was largely funded through state Medicaid reimbursements increases following a legislative process, we now turn to understanding the extensive margin decline in the covered sector.

4.4.2 Interpreting exits

Differential exits between the treated and comparison groups are consistent with treated workers either switching to the uncovered sector or leaving home care altogether. As we do not observe workers in the uncovered sector in our administrative data, we start by using aggregate labor market data from the Quarterly Workforce Indicators (QWI) to distinguish between these possibilities. Figure A.2 shows that total employment among Minnesota PCAs grew steadily over our main analysis period, rising from approximately 87,000 workers in 2023Q1 to over 104,000 by 2025Q1.[11] If treated workers were losing home care work in response to the wage increase, given that the union workforce is roughly 30% of the total workforce, we would expect this to show up as a slowdown in aggregate PCA employment growth around the same period, as long as entry rates did not rise enough to fully offset these losses.

This aggregate increase combined with exit from the covered sector is instead consistent with the institutional features described in Section 2.1: because the state pays the same Medicaid reimbursement rate for services performed under the CD and Agency models, but only CD-model wages are bound by the union contract, FIs and agencies that operate both structures face incentives to steer clients and workers toward the Agency model once the CD

[11] This figure combines total quarterly employment 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.

wage floor rises, retaining the difference between the reimbursement rate and the lower wage they pay. Under this interpretation, the participation decline we observe reflects a (labor) demand-side reallocation across models within the home care sector rather than a genuine reduction in labor supply.

To probe the possibility of this strategic behavior, we assess how the likelihood of participation in the covered sector changes by pre-period FI size.[12] The intuition is that larger FIs have more to gain by reallocating workers from the CD to the Agency model, and may also be more likely to have the administrative infrastructure to actually shift workers and clients across models. We split the continuous DID sample by whether a worker's pre-period FI had 100 or more workers versus fewer than 100.[13]

Appendix C.3, Panel (b), shows that the employment decline in the covered sector is concentrated almost entirely among workers from large FIs.[14] Among these workers, the effect turns negative shortly after the contract announcement and grows following the implementation of the wage increase. In contrast, among workers at small FIs in Panel (a), point estimates are centered near zero throughout the event window, with no discernible break at either the announcement or implementation date. Combined with the aggregate increase in PCA employment, this evidence is consistent with the employment decline reflecting worker reallocation across models rather than exits from the home care sector altogether.

4.4.3 Bounding membership estimates

Given the differential exit we document above, we construct a lower bound on our membership estimates for 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.

In our analysis sample, 95.4% of workers who supply labor in the pre-period are observed working again at some point in 2023 or 2024. For these workers, we use administrative union sign-up and drop records to reconstruct membership status in months without reported payroll data. We code workers as non-members before their sign-up date, if any. If a drop date is subsequently observed, we code them as non-members afterward. Otherwise we do not allow membership to lapse, since drop events are rare—only 3.6% of workers who ever sign a union membership card drop their membership within two years. For the remaining 4.6% of the analysis sample who never reappear in the payroll data after 2022, we adopt a conservative bounding assumption and code them as non-members for the remainder of the post-period.

Figure 5 presents the results from our bounding exercise. The pooled difference-in-

[12] As an estimated 98% of agencies offering PCA Choice also operate the Agency model, the option to reallocate workers across models is available to nearly all providers, motivating our focus on FI size rather than dual vs single-track agencies.

[13] We restrict the sample to workers who were associated with a single FI in the pre-period, dropping the 0.4% of workers associated with more than one FI.

[14] 94% of workers are associated with 20 large FIs, whereas the remainder work at 97 small FIs.

difference estimate for 2023 is 0.5 p.p. and the 2024 estimate is 2.1 p.p. (compared to 1.1 p.p. in 2023 and 3.2 p.p. in 2024 for our main specification). This represents a 20% increase relative to the pre-period mean. In the continuous specification, each additional dollar of the pre-period wage gap raises membership by 0.4 p.p. (compared to 0.7 p.p. in our main specification). These results are all significant at the 1% level.

5 Could income effects explain the membership response?

A natural explanation for the positive effects on membership is that workers experiencing larger wage gains are more likely to join because higher income increases their willingness to pay dues (or lowers the threshold level of attribution α_i required to join). This reflects a standard income effect rather than social preferences.

However, our results suggest that income effects alone cannot account for the observed patterns. As shown in Figure 3, membership rises *before* any wage gains are realized. Specifically, membership increases after the *announcement* of the wage increase and continues to rise through the actual implementation of the wage increase. While this could reflect workers responding to anticipated or permanent income effects, it is not consistent with a pure current income effect.

To more directly test this explanation, we note that income effects could operate through two channels. The first is a direct earnings channel: higher wages raise incomes, lowering the effective burden of dues. If this channel drives membership, workers should respond similarly to wage variation regardless of its source. To assess this, we implement a control function approach in Section 5.1 to distinguish membership responses to contract-driven *wage floor* changes from those arising through other sources of wage variation (e.g. experience, acuity of care, spillovers). In contrast, reciprocity predicts stronger responses to wage increases that are clearly linked to union activity, since workers can only reciprocate benefits they attribute to the union.[15] The second channel is a price channel: for workers below the dues cap, higher earnings push them toward the threshold where the marginal cost of membership falls from 3% of earnings to zero, making membership cheaper as a share of income. To assess this channel, we exploit the kink in the dues schedule using a regression kink design in Section 5.2 to assess the sensitivity of membership to dues.

5.1 Does source of wage variation matter? A control function approach

The control function approach is implemented in two steps, using the sample as described in Section 4.1. First, we run the following first-stage regression:

$$\log(wage_{it}) = \mu Post_t \times (MinWage_{2024} - Wage_{2022,i}) + \gamma_i + \delta_t + \epsilon_{it} \tag{6}$$

[15] That is, while spillovers across the wage distribution from the increase in the contractual minimum wage are driven by the union, we assume these are harder to attribute to the union than the wage floor increase itself.

The instrument is the interaction of a *Post* indicator, defined as 1 for all months starting in January 2024, with each worker's pre-period wage gap to the new minimum wage floor, set to zero for workers already earning at or above $19. We include worker and year-month fixed effects. The residuals from this regression capture wage variation not driven by the wage floor increase. Standard errors are clustered at the worker level.

In the second-stage regression, we directly include these *residuals* from the first-stage regression and estimate the following:

$$membership_{it} = \beta \log(wage_{it}) + \lambda \hat{\epsilon}_{it} + \gamma_i + \delta_t + \psi_{it} \tag{7}$$

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).[16]Under an income effects explanation, we would therefore expect λ to be positive and significant, as variation driven by non-contract elements should also increase willingness to become a member.

Results: Table 4 presents control function estimates of the effect of log wages on union membership, progressively adding the first-stage residual and its interaction with log wages across three specifications. Column 1 reports the OLS estimate: a 10 log-point wage increase is associated with a 0.26 percentage point rise in the probability of union membership.

Columns 2 and 3 introduce the control function correction. Including the first-stage residual—wage changes not driven by the minimum wage instrument Z—raises the log-wage coefficient sharply, while the residual itself enters with a significant negative coefficient of a similar magnitude. This pattern suggests that non-complier wage growth, captured by the residual, is negatively associated with membership conditional on total wages, indicating that OLS was biased. Column 3 further adds the interaction between the residual and log wages to allow for heterogeneous responses across the two sources of wage change (Chay and Greenstone, 2005), but the interaction term is small and statistically insignificant, suggesting that the linear control function is sufficient and that the membership response to contract-driven wages does not differ meaningfully from the average effect identified in column 2.

5.2 Price effects: Costs of membership

If membership is sensitive to the cost of dues, then higher wages should increase joining by reducing the effective price of membership (Barth et al., 2025). In our setting, the kink in the dues schedule—where the marginal cost of membership falls from 3% of earnings to zero at gross earnings of $1200 per pay period—provides a natural opportunity to test this channel. We exploit this feature of the dues schedule to estimate the price sensitivity of membership

[16]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.

using a regression kink design.

We use data at the pay-period level from 2018–2024, restricting the sample to 14-day pay periods to ensure a consistent earnings window for the running variable.[17] Figure 6, Panel (a), confirms that the dues schedule is as specified for union members: dues rise linearly at 3% of gross earnings up to $1,200 per pay period, at which point they flatten at exactly $36, producing a clear kink in the dues-earnings relationship.

The regression kink design identifies the price sensitivity of membership from a change in the slope of membership at the dues cap. If workers respond to this reduction in marginal cost at the kink, we may expect the relationship between earnings and membership to be steeper at the kink. We estimate this change in slope using a local-linear regression, estimated separately on each side of the threshold (Calonico et al., 2014). Standard errors are clustered at the worker level.

The validity of the RKD approach requires that workers do not sort around the earnings cap of $1200. We do observe a spike at $1,220, which appears to be driven by full-time workers bunching at the 2022 wage floor ($15.25 $\times$ 80 hours = $1,220). To ensure this does not drive our estimates, we show robustness to excluding pay-periods where this spike is unusually pronounced.

We find no evidence of a change in the slope of membership at the dues kink, as shown in Figure 6, Panel (c). The conventional estimate at the MSE-optimal bandwidth is small and statistically indistinguishable from zero. The implied dues elasticity is near zero, in contrast to negative elasticities from Scandinavian countries (Barth et al., 2025). Results remain similar across other specifications and bandwidth choices. These findings suggest limited price sensitivity of union membership with respect to dues in this setting.

Overall, the results in Sections 5.1 and 5.2 rule out income effects as the main driver of the membership response we documented earlier. 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

Even though workers can free-ride, we observe a positive membership response to contract-driven wage gains. If workers benefit from the union's efforts, they may feel an obligation to support the institution responsible for those benefits—a form of gift exchange in the spirit of Akerlof (1982). Under this interpretation, membership is not driven solely by private considerations, but instead reflects social preferences: workers reciprocate material benefits even when free-riding is individually optimal.

Our model also generates a specific prediction that distinguishes *reciprocity* from other social preferences: workers with larger wage gains should be more willing to reciprocate than those with smaller or no wage gains. This heterogeneity in the membership response

[17] Approximately 95% of records are of this length; deviations arise from delays in hours submission or other idiosyncratic reporting reasons.

is neither predicted by a "warm glow" explanation—which derives utility from the act of membership itself, independently of what the union did for the worker—nor by altruism toward the union, which implies a membership response that does not vary with own wage gains. We therefore use this wage heterogeneity alongside experimental variation in the salience of the union to directly test for gift-exchange.

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. An ideal experiment would have varied attribution directly—for instance, by randomly varying whether workers were informed that the union was responsible for the wage increase. However, this was not feasible given that the union was delivering the messages.[18]

We instead designed the experiment to vary the salience of the union's role by randomizing the intensity of outreach around the wage increase, focusing on whether workers received one or two contacts, which we refer to as the "High Salience" condition.[19] 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). About half the sample was contacted before the wage increase and the rest after the wage increase; all specifications include an indicator for whether contacts were delivered before or after the wage increase to account for timing variation across groups.[20]

Repeated contact could activate reciprocity, but could also operate through reduced information frictions (e.g., how to join the union), or greater attention or salience toward union membership. While average effects on membership cannot adequately separate these mechanisms, we focus on heterogeneity in treatment effects by wage gains. Such heterogeneity helps distinguish reciprocity from these alternative channels and is the primary focus of our analysis.

Randomization was conducted at the individual level, stratified by (i) program type (either the PCA choice or the Budget model) and (ii) the FI that processed worker payments. 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).[21] Messages varied by program type

[18] Credibly varying attribution would have required sending some messages that either downplayed or omitted the union's role. Such messages were not possible given the union's objectives.

[19] Individuals were randomly assigned to one of four groups: Group 1 would receive a single contact before the wage increase; Group 2 would receive two contacts before the wage increase; Group 3 would receive a single contact after the wage increase; and Group 4 would receive two contacts after the wage increase.

[20] Fully disaggregated results by timing and contact frequency are reported in Appendix Table C.2, Columns 1–2.

[21] 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.

to account for the differences in underlying funding structures. In the analysis, we restrict the sample to those who had contactable mobile phone numbers, yielding a sample of 21,353 individuals.

In addition to membership status from the pay records, we observe engagement statistics at the worker level by method of contact—e.g., whether someone replied to a text or registered for a Zoom information session. We use this engagement information to understand how individuals reacted—positively or negatively—to being contacted by the union. We restrict attention to whether an individual joined the union before June 2025, as the union had resumed further contact with the population after this time.

6.2 Results

Table 6, columns 1 and 2 show that being in the High Salience group—i.e., receiving at least two contacts from the union—has no significant effect on membership on average, but increases membership significantly among workers with a larger wage gap between their current pay and the new wage floor.

We next assess whether these effects are driven by positive engagement with union organizers, either through text responses or sign-ups for Zoom information sessions. This type of engagement may be the mechanism through which salience translates into membership.[22] Higher salience substantially increases positive engagement with the union (columns 3–4). Furthermore, it is only among workers who both engaged and stood to gain from the wage floor that membership increases: the IV estimates in column 5 show a significant interaction between engagement and the wage gap, while the main effect of engagement remains insignificant.[23] The latter finding suggests that this engagement does not simply proxy for pre-existing pro-union sentiment. If it did, we would expect engaged workers to join regardless of their wage gap. Instead, these results suggest that repeated contact raises union salience and drives engagement, but that active engagement converts into membership only when workers gain from the wage floor increase.

7 Discussion

7.1 Alternative explanations

Forward-looking investments: An alternative explanation is that a worker joins the union as a forward-looking investment: membership strengthens the union, which in turn may deliver larger future wage gains. However, since future contract gains are also non-excludable, this mechanism faces the same free-rider problem—an individual worker can always benefit from a stronger union without paying dues.

[22] We exclude from this engagement measure any activity that is negatively coded, such as requests to stop further contact.

[23] These effects are similar if we omit Zoom registrations from the engagement measures. See Appendix Table C.2, columns 3–5.

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 attachment with total hours worked in 2022.[24] We then estimate heterogeneity in the membership response by splitting the sample at 928 hours in 2022, the median of the hours distribution among the same binary DID sample as in Section 4.1. We find no evidence that the membership response differs between high- and low-attachment workers following the contract announcement or the subsequent wage increase (see Appendix C.5), suggesting that forward-looking investment is unlikely to be a primary driver of membership.

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 control workers—those already earning above the new wage floor who received no direct wage gain from the contract. If peer effects operate independently of own wage gains, control workers in FIs with a greater share of treated minimum wage workers, or a stronger pre-existing union culture, should become more likely to join as they observe peers joining or are influenced by norms around union membership at their FI. We proxy peer exposure using each control 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. We find no evidence of positive peer contagion: among control workers, a higher share of pre-existing union members at the FI does not significantly predict post-contract membership, and a higher share of treated peers exhibits a marginally significant *negative* correlation (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. These fixed effects absorb all time-varying peer behavior (or other shocks) within each FI, so the remaining identifying variation comes from differences in own wage gains between workers at the same FI in the same month. While adding FI-month fixed effects attenuates the point estimate, the 2024 treatment effect remains positive and statistically significant (see Appendix C.5.2). This confirms that even as FI-level environment may account for part of the overall response, own wage gains still drive workers' decision to join the union.

[24] The correlation between hours worked in 2022 and 2024 is 0.64, suggesting that hours worked is a persistent measure of attachment to the sector.

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 workforce N. The union puts a positive weight α 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 economics are similar to the public goods problem of a local government (or a government facing endogenous immigration), where the composition of taxes and spending endogenously alters the objective function of the government by attracting new citizens and voters. Unions will provide public goods in order to attract reciprocating potential members, as well as benefiting both members and non-members, financed by dues that may deter dues-averse workers. As such, the union solves a variant of the optimal public goods finance problem (Atkinson and Stern, 1974), but with endogenous membership, akin to a local government facing immigration in response to policy choices:

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

subject to

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

We can write the solution to the union's optimization problem as:

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

Observe that without gift exchange (i.e. if $\epsilon_{s,w} = 0$), there is a strictly lower level of public goods provision, regardless of the weight that the union puts on non-members. 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 would implement the sectoral optimum, given by:

$$\underbrace{\epsilon_{w,G}}_{\text{Effect of G on w}} = \underbrace{\tau}_{\text{Membership Revenue Base}}$$

which corresponds to the social planner's solution for the optimal financing of a productive public good, as in (Atkinson and Stern, 1974).

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 union objective side of the ledger, we estimate gift-exchange $\epsilon_{s,w} \approx 1 - 1.5$. Estimates of $\epsilon_{w,G}$ are necessarily speculative. As an illustrative example, we take the elasticity of bill enactment with respect to lobbying expenditures of 0.13, estimated by Moshary and Slattery (2024).[25]

On the union cost side, our RKD point estimates suggest 0 elasticity ($\epsilon_{s,\tau} = 0.0003$), implying that membership is essentially completely inelastic with respect to the dues rate. Thus the marginal cost of union funds is simply the dues rate, τ. Under a natural baseline case of $\alpha = 0$, we have that the marginal value of union funds is around 8.6.

9 Conclusion

This paper studies the determinants of voluntary union membership in a high-coverage environment, where free-riding is individually optimal. In our setting—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 relationship operates through reciprocity rather than income effects. These results imply that the gift-exchange margin gives the union meaningful incentives to internalize the interests of non-members—by raising wages broadly, the union can cultivate future membership. While this is noteworthy, the gift-exchange motivation falls well short of the first-best outcome in a framework of optimal public good financing. Our findings shed light on the potential limitations of sectoral bargaining approaches in building union density without policies that aim to mitigate free-riding.

[25]Other estimates include 0.29 (Kang 2016), and 0.2 to 0.3 (De Figueiredo and Silverman 2006).

References

Akerlof, G. A. (1982). Labor contracts as partial gift exchange. *The quarterly journal of economics*, 97(4):543–569.

Andreoni, J. and Payne, A. A. (2013). Charitable giving. In *Handbook of public economics*, volume 5, pages 1–50. Elsevier.

Atkinson, A. B. and Stern, N. H. (1974). Pigou, taxation and public goods. *The Review of economic studies*, 41(1):119–128.

Barth, E., Bryson, A., and Dale-Olsen, H. (2025). Turning non-members into members: Do public subsidies to union membership matter? *Journal of Economic Behavior & Organization*, 229:106855.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Chay, K. Y. and Greenstone, M. (2005). Does air quality matter? evidence from the housing market. *Journal of political Economy*, 113(2):376–424.

DellaVigna, S., List, J. A., Malmendier, U., and Rao, G. (2022). Estimating social preferences and gift exchange at work. *American Economic Review*, 112(3):1038–1074.

Dube, A. (2026). *The Wage Standard: What's Wrong in the Labor Market and How to Fix It*. Penguin Random House, New York.

Farber, H. S., Herbst, D., Kuziemko, I., and Naidu, S. (2021). Unions and inequality over the twentieth century: New evidence from survey data. *The Quarterly Journal of Economics*, 136(3):1325–1385.

Fehr, E. and Charness, G. (2025). Social preferences: fundamental characteristics and economic consequences. *Journal of Economic Literature*, 63(2):440–514.

Finan, F. and Schechter, L. (2012). Vote-buying and reciprocity. *Econometrica*, 80(2):863–881.

Foy, M. (2026). Experimental evidence on the complementarity of union membership.

Gandhi, A. D., Olenski, A., Ruffini, K., and Shen, K. (2024). Alleviating worker shortages through targeted subsidies: Evidence from incentive payments in healthcare. *Review of Economics and Statistics*, pages 1–31.

Gottlieb, J. D., Mahoney, N., Rinz, K., and Udalova, V. (2025). The rise of healthcare jobs. Technical report, National Bureau of Economic Research.

Jäger, S., Naidu, S., and Schoefer, B. (2025). Collective bargaining, unions, and the wage structure: An international perspective. In *Handbook of Labor Economics*, volume 6, pages 229–372. Elsevier.

Kreider, A. and Werner, R. (2025). Immigration enforcement, the supply of home care workers, and access to long-term care: evidence from secure communities. *SSRN*.

Minnesota Department of Human Services (2025). PCA and CFSS tiered rates and wage floors. Minnesota Department of Human Services. Accessed: 2026-07-30.

Moshary, S. and Slattery, C. (2024). Consolidation and political influence in the auto retail industry. *Available at SSRN 4832068*.

Nesterak, M. (2024). Gov. Tim Walz highlights 31% pay increase for unionized home care workers. Accessed: 2026-07-30.

Ostrom, E. (1999). Coping with tragedies of the commons. *Annual review of political science*, 2(1):493–535.

Shen, K. (2024). Who benefits from public financing of home-based long term care? evidence from medicaid. *Journal of Public Economics*, 236:105151.

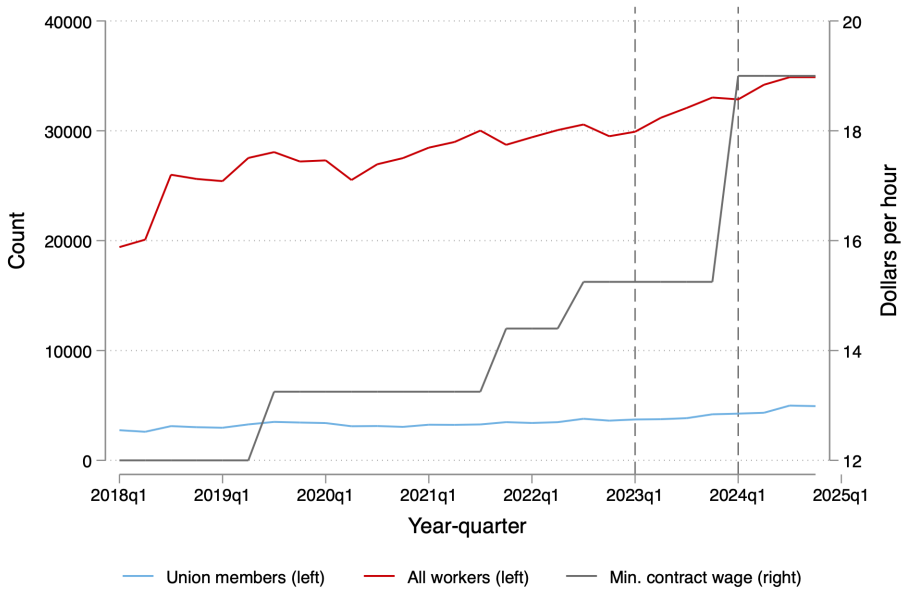
UC Berkeley Labor Center (2023). Analysis of the potential impacts of statewide or regional collective bargaining for in-home supportive services providers. Accessed: 2026-04-16.

U.S. Bureau of Labor Statistics (2025a). Fastest growing occupations, 2024–2034 projections. https://www.bls.gov/emp/tables/fastest-growing-occupations.htm. Employment Projections program; Accessed: 2026-04-06.

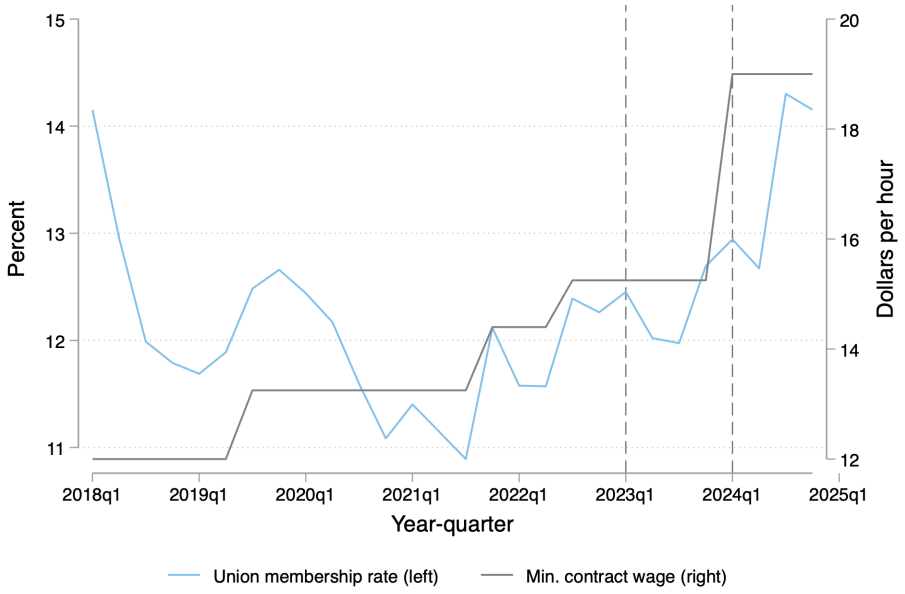
U.S. Bureau of Labor Statistics (2025b). Occupational employment and wage statistics (oews): Home health and personal care aides (31-1120. https://data.bls.gov/oes/#/industry/000000. Accessed: 2026-04-06.

Figure 1: Trends in the Home Care Union Sector (2018–24)

(a) Number of workers and union members



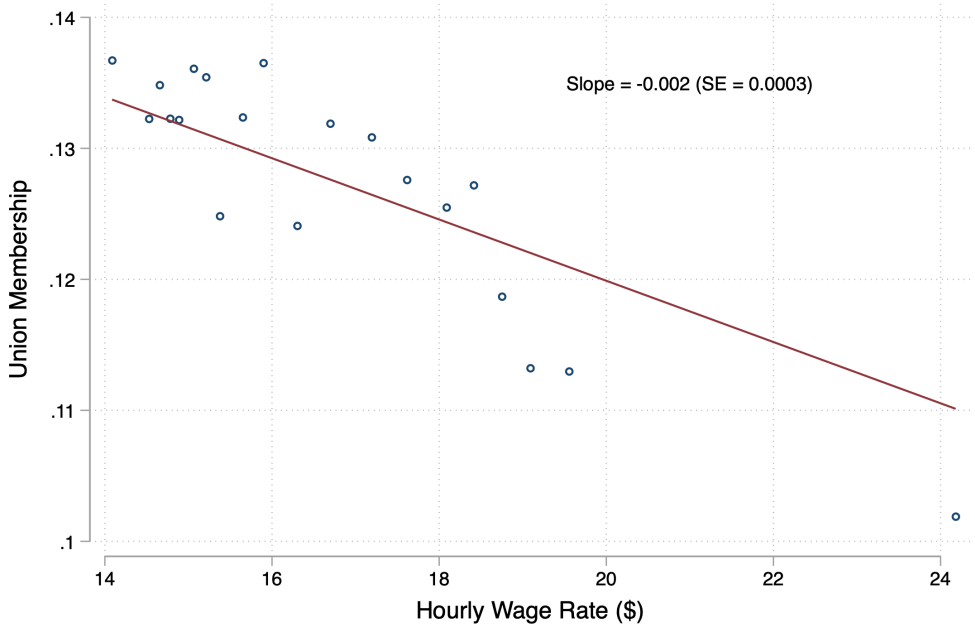
(b) Membership Rate



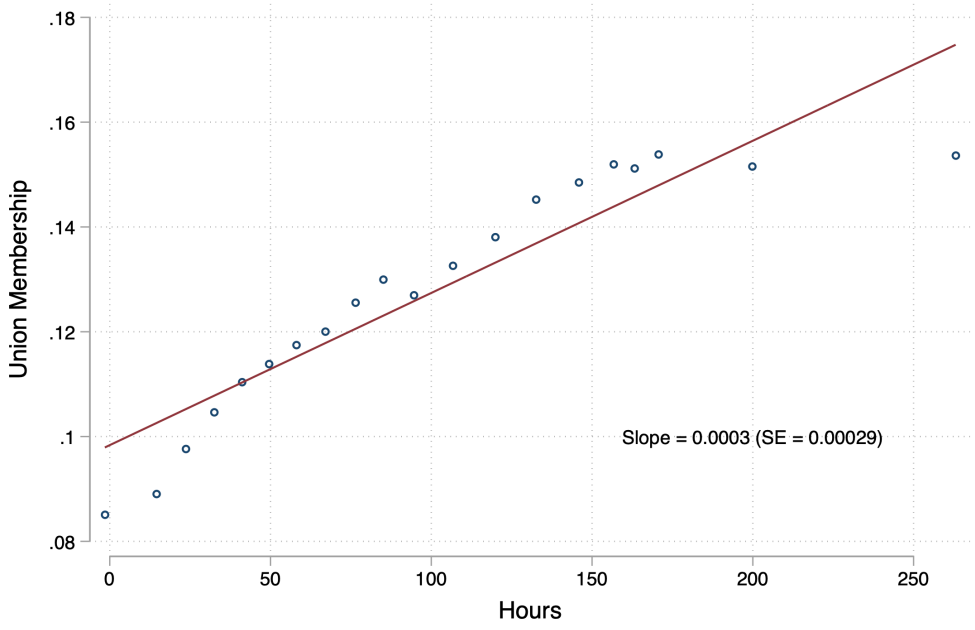
Notes: This figure shows quarterly trends in the number of union-eligible workers and union members, as well as the contract minimum wage, from 2018q1 to 2024q4. The right axis reports the contractual wage floor (dollars per hour). In panel (a), the left axis measures the count of workers and members. 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



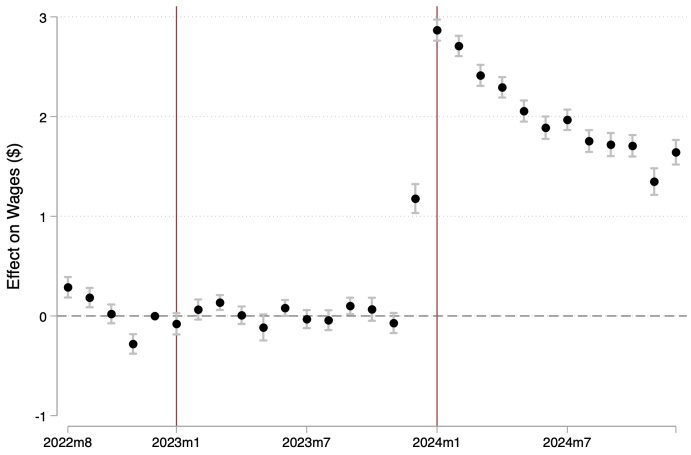
(b) Membership and hours



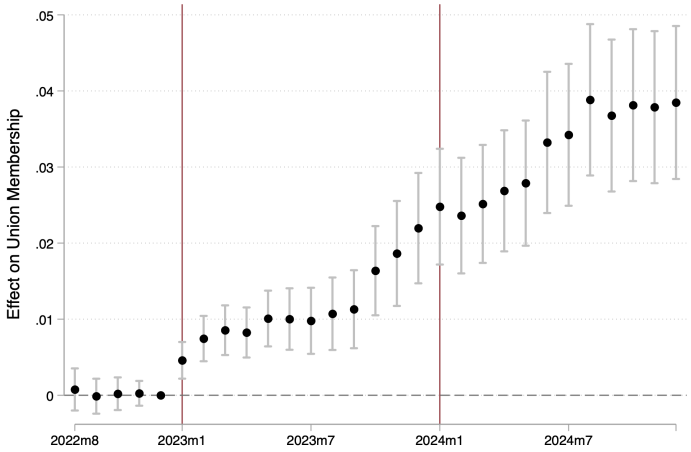
Notes: This figure shows binned scatterplots of the relationship between union membership and key worker-level variables. Panel (a) plots union membership against the hourly wage rate; Panel (b) plots union membership against hours worked. The reported slope coefficient and standard error come from a linear regression of membership status on the corresponding variable, controlling for year-month fixed effects; standard errors are clustered at the worker level. Sample includes all worker-months in the administrative panel, 2018–24.

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

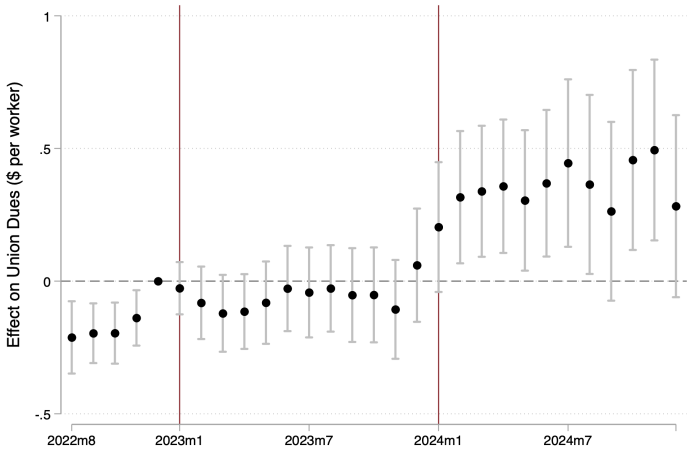
(a) Wage rate



(b) Union membership



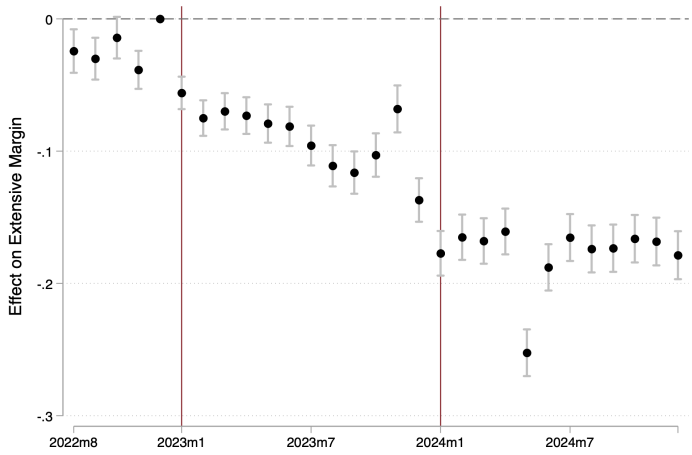
(c) Dues paid ($ per worker)



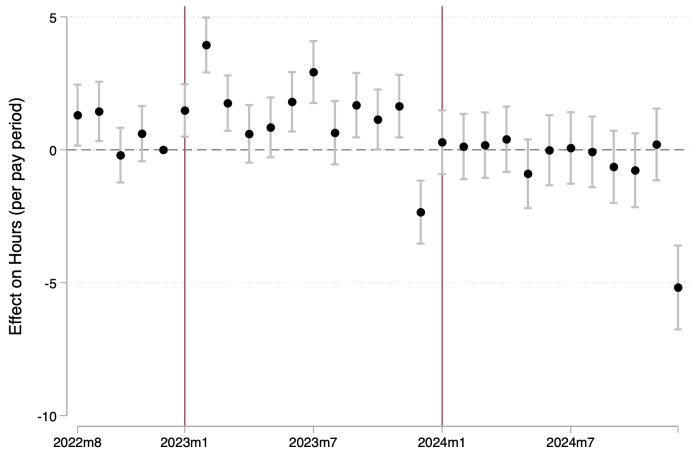
Notes: This figure reports difference-in-differences estimates as per equation 5. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. Sample is restricted to those who worked at least 1 hour in the three months prior to January 2023 ('the pre-period'). Panel (a) shows the effects on the hourly wage rate. Panels (b) and (c) show the effects on union membership and actual dues paid per pay period ($ per worker), conditional on working in the month. Estimates are normalized relative to 2022m12.

Figure 4: Difference-in-differences Estimates: Labor market outcomes

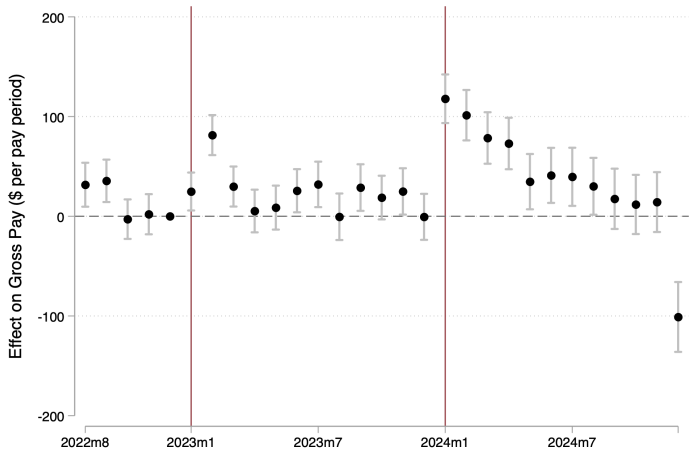
(a) Participation



(b) Hours per pay period (conditional)

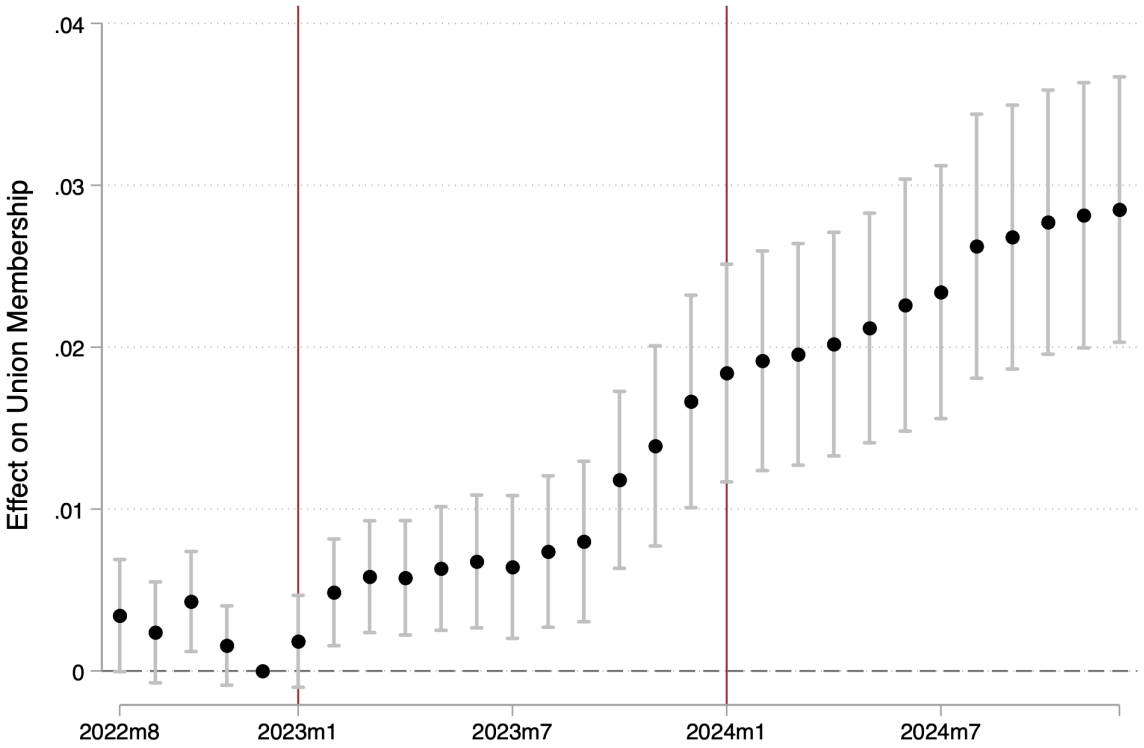


(c) Earnings per pay period (conditional)



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. Sample is restricted to those who worked at least 1 hour in the three months prior to January 2023 ('the pre-period'). Panel (a) shows the effects on participation in the covered sector, defined as supplying at least 1 hour of labor in each month. Panels (b) and (c) show the effects hours and gross earnings per pay period, conditional on participation. Estimates are normalized relative to 2022m12.

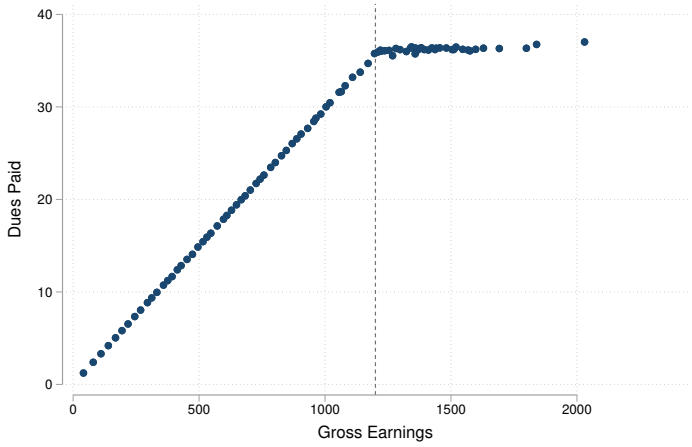
Figure 5: Bounded union membership estimates



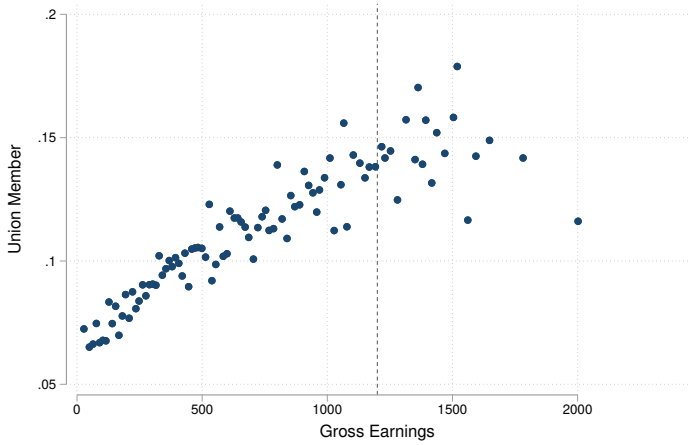
Notes: This figure reports difference-in-differences estimates as per equation 5. The two red lines denote the announcement (2023m1) and the implementation (2024m1) of the wage floor increase in the 2023–25 contract. The coefficients represent a lower bound on union membership for non-union members in 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 .5 p.p. and the 2024 estimate is 2.1 p.p. In the continuous specification, each additional dollar of the pre-period wage gap raises membership by 0.4 p.p.

Figure 6: Sensitivity of membership to dues rate

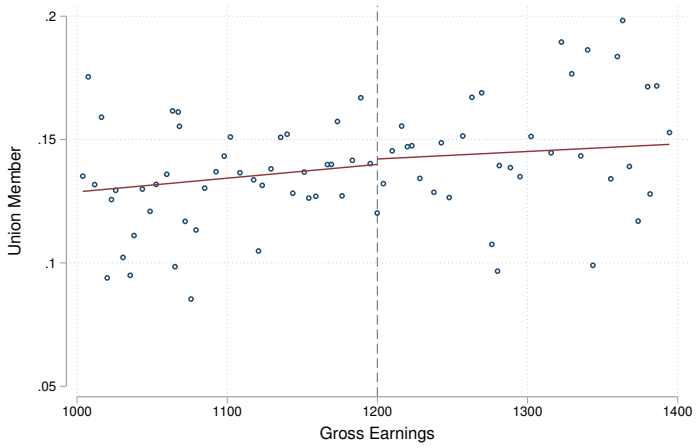
(a) Kink in dues schedule as a function of gross wages



(b) Membership as a function of gross earnings



(c) Membership and earnings at the kink



Notes: This figure documents the relationship between dues, earnings, and membership.Panel (a) plots dues paid against gross earnings, showing that dues rise linearly with earnings up to a kink at approximately $1,200 (dashed line), beyond which dues are flat and no longer increase with earnings. Panel (b) plots union membership against gross earnings over the same range. Panel (c) zooms in on the region immediately around the dues kink ($1,000–$1,400 in gross earnings) and shows no discernible change in the slope of the membership-earnings relationship at the kink.

Table 1: Summary statistics

	(1) Analysis Sample	(2) Binary DiD Sample
Wages (dollars)	17.23 (2.97)	17.39 (3.76)
Gross pay (dollars per pay period)	913.86 (641.81)	891.08 (675.44)
Extensive margin	0.89 (0.31)	0.86 (0.34)
Intensive margin (hours per pay period)	52.73 (35.02)	50.44 (34.73)
Union membership	0.13 (0.33)	0.12 (0.32)
Union dues (dollars per month)	3.86 (15.27)	3.30 (14.21)
Union dues (dollars per pay period)	2.03 (7.85)	1.72 (7.19)
Observations	88,500	39,591
Unique Workers	29,500	13,197

Notes: This table shows summary statistics for key variables during October–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) restricts to workers used in the binary difference-in-differences specification. 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	0.061**	0.024***	0.011***	0.003***	0.090	0.039***
	(0.027)	(0.005)	(0.002)	(0.000)	(0.064)	(0.014)
Treatment × Post2024	2.003***	0.520***	0.032***	0.007***	0.494***	0.203***
	(0.040)	(0.008)	(0.004)	(0.001)	(0.129)	(0.028)
Pre-period mean	20.975	17.215	0.103	0.128	1.904	2.067
Observations	272,000	642,236	272,000	642,236	272,000	642,236
Panel B: Balanced sample						
Treatment × Post2023	0.038	0.019***	0.010***	0.003***	0.095	0.051***
	(0.027)	(0.005)	(0.002)	(0.001)	(0.080)	(0.018)
Treatment × Post2024	1.886***	0.496***	0.026***	0.007***	0.492***	0.225***
	(0.044)	(0.009)	(0.005)	(0.001)	(0.150)	(0.033)
Pre-period mean	20.782	17.426	0.106	0.138	2.136	2.381
Observations	201,410	498,417	201,410	498,417	201,410	498,417

Notes: This table shows difference-in-difference estimates for hourly wage rate and union membership variables at the worker-month level. The dependent variables are: hourly wage rate (columns 1–2); indicator for union membership (columns 3–4); and average dues paid ($) per pay period per worker in the 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. 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 (October–December 2022). 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 3: Effects on Participation, Hours, and Earnings

	Employed		Hours (cond.)		Earnings (cond.)	
	(1) Binary	(2) Cont.	(3) Binary	(4) Cont.	(5) Binary	(6) Cont.
Panel A: Unbalanced sample						
Treatment × Post2023	-0.067***	-0.016***	0.774***	0.119**	11.299**	1.394
	(0.005)	(0.001)	(0.286)	(0.058)	(5.586)	(1.079)
Treatment × Post2024	-0.157***	-0.042***	-1.063***	-0.553***	28.427***	3.149*
	(0.007)	(0.001)	(0.403)	(0.083)	(8.757)	(1.731)
Pre-period mean	0.878	0.873	59.837	53.132	1241.291	919.997
Observations	382,713	855,500	272,000	642,236	272,000	642,236
Panel B: Balanced sample						
Treatment × Post2023	0.020***	0.005***	1.096***	0.136**	13.186**	1.217
	(0.004)	(0.001)	(0.322)	(0.065)	(6.260)	(1.198)
Treatment × Post2024	-0.003	-0.004***	-0.824*	-0.510***	22.915**	2.396
	(0.004)	(0.001)	(0.431)	(0.088)	(9.365)	(1.852)
Pre-period mean	0.933	0.930	64.420	56.363	1333.699	990.554
Observations	213,063	525,625	201,410	498,417	201,410	498,417

Notes: This table shows difference-in-difference estimates for labor market participation and gross earnings at the worker-month level. The dependent variables are: an indicator for positive hours worked in the covered sector in that month (columns 1–2); total hours per pay period conditional on participation (columns 3–4); and gross pay per pay period conditional on participation (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

	(1) Is union member	(2) Is union member	(3) Is union member
Log wages	0.026***	0.165***	0.165***
	(0.006)	(0.022)	(0.022)
FS Residual		-0.164***	-0.161***
		(0.023)	(0.024)
FS Residual × Log wages			-0.007
			(0.009)
Observations	633206	631656	631656

Standard errors clustered at the individual level

Worker and month fixed effects included

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

Table 5: Regression Kink Design Estimates ($\times$ 100)

Panel A: MSE-Optimal Bandwidth	
RKD Estimate	-0.005
	(0.006)
Permutation test p-value	0.576
Panel B: Bias-Corrected	
RKD Estimate	-0.003
	(0.010)
Panel C: Bandwidth Sensitivity	
BW $\times$ 0.5	-0.017
	(0.016)
BW $\times$ 0.75	-0.006
	(0.008)
BW $\times$ 1.5	-0.001
	(0.003)
BW $\times$ 2.0	-0.003
	(0.002)
Bandwidth (left, right)	(254.8, 254.8)
Eff. N (left, right)	(341035, 324554)
Kernel	Triangular
Order local poly.	1

Notes: RKD estimates of the change in slope of Union Member (X 100) at the kink point 1200 dollars per pay period. Standard errors in parentheses (conventional and permutation test in Panel A, robust in Panel B). Panel C uses conventional estimates at alternative bandwidths.

Table 6: Experimental effects

	Membership		Positive Engagement		IV: Membership
	(1)	(2)	(3)	(4)	(5)
High Salience	0.002	-0.000	0.034***	0.033***	
	(0.002)	(0.001)	(0.003)	(0.006)	
High Salience × Wage Gap		0.001**		0.001	
		(0.001)		(0.002)	
Positive Engagement					-0.004
					(0.029)
Positive Engagement × Wage Gap					0.031**
					(0.015)
N	21353	21329	21353	21329	21329
Omitted Group Mean	0.009	0.009	0.009	0.009	

Notes: This table reports experimental effects on membership. The "High Salience" group received at least two contacts from the union during the experiment. Columns 1–2 report OLS estimates of the effect of High Salience on union membership. Columns 3–4 report OLS estimates of the effect of High Salience on positive engagement, defined as replying to a union text message or registering for a Zoom information session. Column 5 reports IV estimates of the effect of engagement on membership, instrumenting for engagement and its interaction with the wage gap using High Salience and its interaction with the wage gap. The wage gap is defined as the difference between a worker's current wage and the new wage floor; it is set to zero for workers already earning at or above the floor. All specifications include an indicator for whether contacts were delivered after the wage increase. The omitted group is the low salience (single contact) condition. The sample in columns 2, 4, and 5 is restricted to workers with available wage data. Specifications include strata fixed effects and standard errors are clustered by FI. * p<0.10, ** p<0.05, *** p<0.01.

ONLINE APPENDIX

Why Join the Union?

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

Table of Contents

A Details on Context and Data **1**

A.1 Administrative data cleaning procedures 1

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

B Experimental Details **3**

B.1 Design overview 3

B.2 Example text messages 3

C Additional Results **4**

C.1 DID Estimates 4

C.1.1 Effects on Union Membership and Dues (Non-Members) 4

C.1.2 Effects on Wages, Union Membership and Dues (Alternative Controls) . 5

C.2 Experimental Results: Robustness 6

C.3 Employment effects by pre-period FI size 7

C.4 Membership effects by hours 8

C.5 Evidence on peer effects 9

C.5.1 Peer Effects on Membership Among Control Workers 9

C.5.2 Membership Effects with FI × Month Fixed Effects 9

Appendix A Details on Context and Data

Appendix A.1 Administrative data cleaning procedures

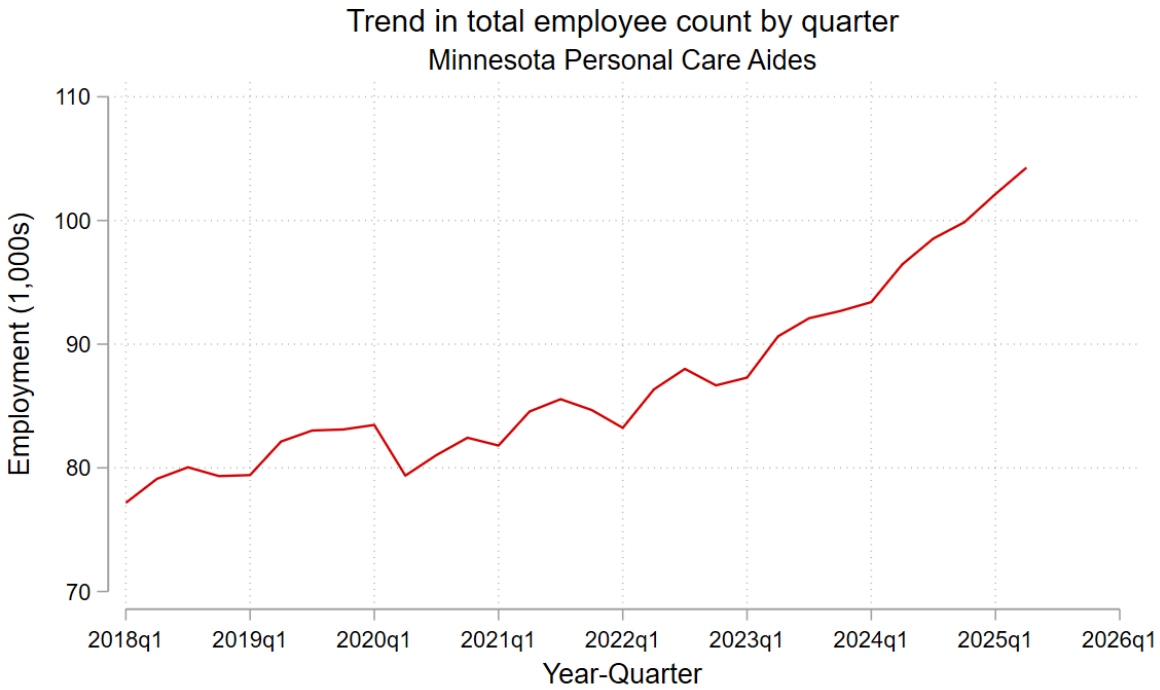
We construct our sample from administrative payroll records for Minnesota home care workers, submitted by fiscal intermediaries to our union data partner . We extracted worker-level pay records and provider metadata from approximately 103,000 Excel files covering 2018–24 (38,000 of which cover 2022–24), 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 4.9 million worker pay-period records (2.4 million for 2022–24), covering 81,561 unique workers (57,483 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 impute hours, wages, and gross pay when missing ($\sim$ 13,000 observations) and drop records where these cannot be imputed ($\sim$ 5,000 observations). We drop observations where reported wages are below the contract minimum wage or reported gross pay differs by more than one dollar from manually calculated gross pay ($\sim$ 118,000 observations). We drop all records for workers who ever report working more than 600 hours in a month or with more than 6 pay records in a month (about 390,000 observations).

This yields a final cleaned sample of approximately 4.1 million worker–pay-period records (2 million for 2022–24), covering 79,042 workers (55,128 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 B Experimental Details

Appendix B.1 Design overview

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

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

In Groups 1 and 2, the first text message omitted the membership link due to an implementation error; subsequent messages, where required, included the correct link. This issue affects the content of the initial outreach but not the timing or number of contacts. While Groups 1 and 2 received emails where available, Groups 3 and 4 did not and were instead offered Zoom information sessions as an option in the text messages.

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] @ SEIU Home Care Workers & Clients United

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] @ SEIU Healthcare United Home Care Workers & Clients. Activate your Union membership here! [Link]

Appendix C Additional Results

Appendix C.1 DID Estimates

Appendix C.1.1 Effects on Union Membership and Dues (Non-Members)

	Membership		Dues	
	(1) Binary	(2) Cont.	(3) Binary	(4) Cont.
Panel A: Unbalanced sample				
Treatment × Post2023	0.012***	0.003***	0.057	0.030***
	(0.002)	(0.000)	(0.043)	(0.010)
Treatment × Post2024	0.037***	0.009***	0.421***	0.150***
	(0.005)	(0.001)	(0.102)	(0.023)
Pre-period mean	0.000	0.000	0.064	0.087
Observations	239,331	559,173	239,331	559,173
Panel B: Balanced sample				
Treatment × Post2023	0.012***	0.003***	0.086	0.041***
	(0.003)	(0.001)	(0.055)	(0.012)
Treatment × Post2024	0.032***	0.008***	0.465***	0.169***
	(0.005)	(0.001)	(0.121)	(0.027)
Pre-period mean	0.000	0.000	0.085	0.104
Observations	176,131	430,974	176,131	430,974

Notes: This table shows difference-in-difference estimates for union membership variables, restricting to *non-members* from the pre-period. The dependent variables are: indicator for union membership (columns 1–2); and average dues paid ($) per pay period per worker in the month (columns 3–4). 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), respectively, relative to the pre-period (October–December 2022). Regressions include worker and month fixed effects. Standard errors are clustered at the worker level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C.1.2 Effects on Wages, Union Membership and Dues (Alternative Controls)

	(1) $\geq 19 (baseline)$	(2) ≥ 20	(3) ≥ 21
Panel A: Wage			
Treatment × Post2023	0.061**	0.008	0.055
	(0.027)	(0.055)	(0.085)
Treatment × Post2024	2.003***	2.129***	2.126***
	(0.040)	(0.077)	(0.118)
Pre-period mean	20.975	22.748	24.833
Observations	272,000	207,402	186,470
Panel B: Membership			
Treatment × Post2023	0.011***	0.012***	0.014***
	(0.002)	(0.002)	(0.003)
Treatment × Post2024	0.032***	0.034***	0.034***
	(0.004)	(0.005)	(0.007)
Pre-period mean	0.103	0.106	0.109
Observations	272,000	207,402	186,470
Panel C: Dues			
Treatment × Post2023	0.090	0.243***	0.301***
	(0.064)	(0.079)	(0.090)
Treatment × Post2024	0.494***	0.731***	0.795***
	(0.129)	(0.162)	(0.219)
Pre-period mean	1.904	1.942	2.099
Observations	272,000	207,402	186,470

This table reports difference-in-differences estimates of the effect of the union contract on hourly wages (Panel A), union membership (Panel B), and dues paid (Panel C) at the worker-month level, using alternative wage thresholds to define the comparison group. Column 1 reproduces the baseline specification, in which the comparison group consists of workers earning at or above $19 per hour in the pre-period; columns 2 and 3 raise this threshold to $20 and $21, respectively. Regressions include worker and month fixed effects. Standard errors, clustered at the worker level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

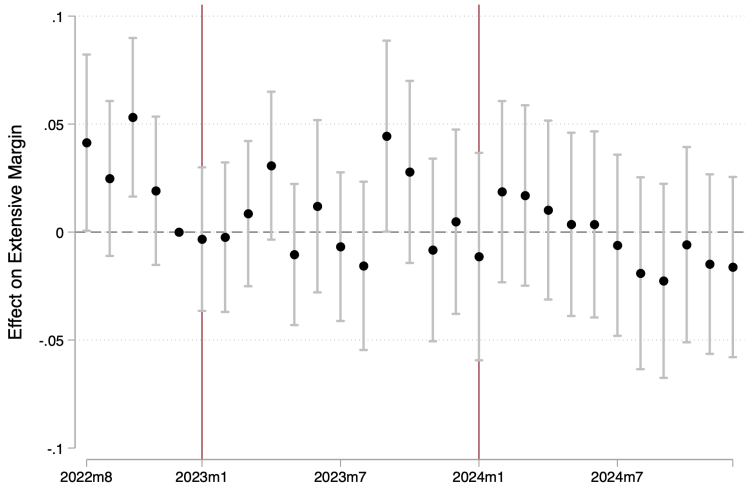
Appendix C.2 Experimental Results: Robustness

	Membership		Text Engagement		IV: Membership
	(1)	(2)	(3)	(4)	(5)
Before, 2 contacts	0.003	-0.001			
	(0.003)	(0.002)			
After, 1 contact	0.001	-0.001			
	(0.002)	(0.003)			
After, 2 contacts	0.003	-0.001			
	(0.002)	(0.003)			
Before, 2 contacts × Wage Gap		0.002***			
		(0.001)			
After, 1 contact × Wage Gap		0.001			
		(0.001)			
After, 2 contacts × Wage Gap		0.002*			
		(0.001)			
Multiple Contact			0.033***	0.029***	
			(0.003)	(0.003)	
Multiple contact × Wage Gap				0.002*	
				(0.001)	
Text Engagement					-0.002
					(0.033)
Text Engagement × Wage Gap					0.030**
					(0.014)
N	21353	21329	21353	21329	21329
Omitted Group Mean	0.009	0.009	0.009	0.009	

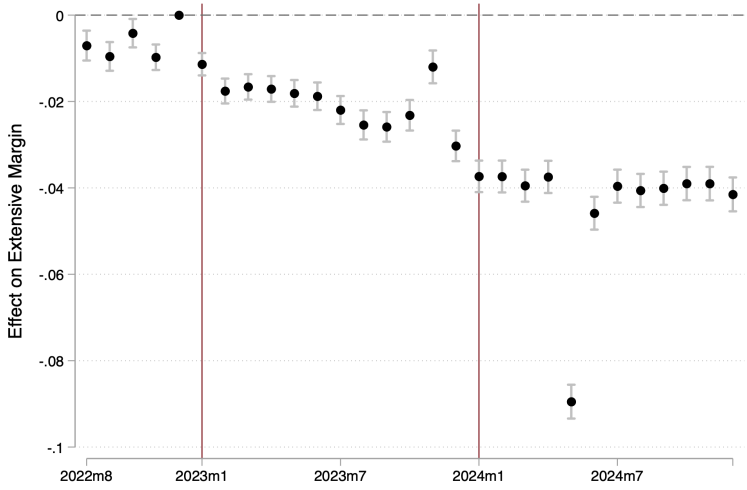
Notes: This table reports experimental effects on membership. Workers were randomly assigned to receive contacts from the union before or after the wage increase, with either one or two contacts. Columns 1–2 report OLS estimates of the effect of each treatment condition (Before, 2 contacts; After, 1 contact; After, 2 contacts) on union membership, with column 2 interacting each condition separately with the wage gap. Columns 3–4 report OLS estimates of the effect of receiving multiple contacts on positive text engagement only, with column 4 additionally interacting this with the wage gap. Column 5 reports IV estimates of the effect of the positive text engagement on membership, instrumenting for engagement and its interaction with the wage gap using multiple contact and its interaction with the wage gap. The wage gap is defined as the difference between a worker's baseline wage and the new wage floor; it is set to zero for workers already earning at or above the floor. The omitted group is the low salience (single contact) condition. The sample in columns 2, 4, and 5 is restricted to workers with available wage data. Specifications include strata fixed effects and standard errors are clustered by FI. * p<0.10, ** p<0.05, *** p<0.01.

Appendix C.3 Employment effects by pre-period FI size

(a) Small FIs

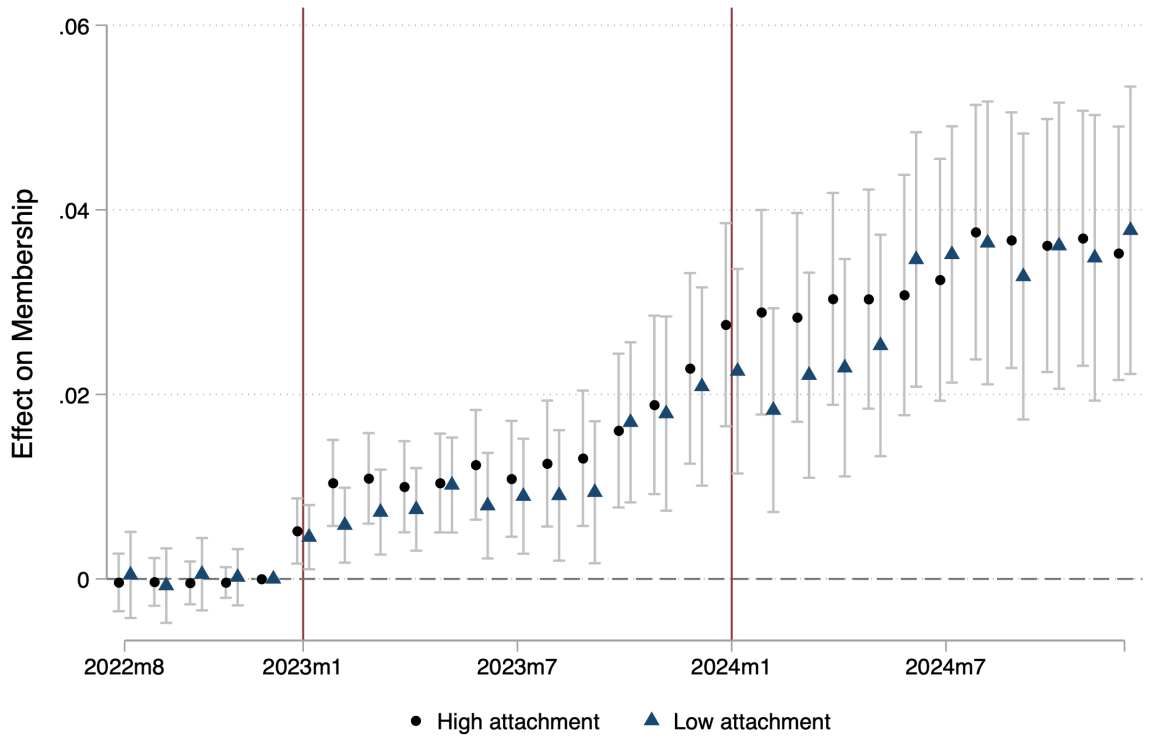


(b) Large FIs



Notes: This figure reports DID estimates of the employment effect in the covered sector, splitting the sample by pre-period FI size. Panel (a) restricts to workers whose pre-period FI had fewer than 100 workers (97 FIs and 1,809 workers); Panel (b) restricts to workers whose pre-period FI had 100 or more workers (20 FIs and 27,509 workers). Sample is restricted to workers who were associated with a single FI in the pre-period. Standard errors are clustered at worker level.

Appendix C.4 Membership effects by hours



Notes: This figure reports event-study DID estimates of the effect of the contract on union membership, separately for workers with high versus low past attachment. Attachment is proxied by hours worked in 2022, with workers split into "High attachment" and "Low attachment" groups at the median of the 2022 hours distribution among the binary DID sample. The two red lines mark the contract announcement (January 2023) and implementation of the new wage floor (January 2024). Standard errors are clustered at the worker level.

Appendix C.5 Evidence on peer effects

Appendix C.5.1 Peer Effects on Membership Among Control Workers

	Membership		
	(1) Member Share	(2) Treat Share	(3) Both
Post2023 × FI Member Share	0.015		0.020
	(0.022)		(0.021)
Post2024 × FI Member Share	0.020		0.023
	(0.115)		(0.085)
Post2023 × FI Treat Share		-0.015	-0.015
		(0.009)	(0.009)
Post2024 × FI Treat Share		-0.057*	-0.058*
		(0.033)	(0.032)
Observations	109,085	109,085	109,085

Notes: This table considers peer effects on union membership among control workers, defined as those already earning at or above the new $19 wage floor in the preperiod. FI Treat Share is the leave-one-out share of workers at a control worker's pre-period employer who were treated (minimum wage) workers; FI Member Share is the analogous leave-one-out share of pre-period union members. Both measures are fixed at their pre-period value and held constant across each worker's panel. Column 1 includes only the FI Member Share interactions; column 2 includes only the FI Treat Share interactions; column 3 includes both together. All specifications include worker fixed effects and calendar-month fixed effects, restricted to control workers. Standard errors are clustered at the pre-period employer level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix C.5.2 Membership Effects with FI × Month Fixed Effects

	Membership	
	(1)	(2)
Treatment Intensity × Post2023	0.003***	0.001
	(0.000)	(0.000)
Treatment Intensity × Post2024	0.007***	0.002**
	(0.001)	(0.001)
FI × Month FE	No	Yes
Observations	642,236	641,769

Notes: This table tests whether individual wage gains predict union membership independently of the peer environment at a worker's fiscal intermediary (FI). Treatment Intensity is the continuous pre-period wage gap, defined as the difference between a worker's current wage and the new wage floor, set to zero for workers already earning at or above the floor. Column 1 reports the baseline continuous specification with worker and calendar-month fixed effects. Column 2 adds fiscal intermediary × month fixed effects, which absorb all time-varying peer behavior within each FI, so the remaining identifying variation comes from differences in own wage gains between workers at the same FI in the same month. Standard errors are clustered at the worker level * p<0.10, ** p<0.05, *** p<0.01.