The Workforce Consequences of Ending Mandatory Union Fees: Evidence from *Janus v. AFSCME*[*]

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Abstract

Public-sector unions shape how state and local governments staff and size their workforces, yet clean shocks to union power are rare. The 2018 ruling by the U.S. Supreme Court in *Janus v. AFSCME* provided one, eliminating mandatory agency fees in the 21 states that had previously permitted them. I use a difference-in-differences design comparing affected states with right-to-work states and draw on two complementary datasets: three waves of the Census of Governments (CoG) and a balanced 2011–2025 annual panel that combines the CoG with the Annual Survey of Public Employment and Payroll. I find that *Janus* reduced part-time employment by 6.2 to 8.4 percent while leaving full-time employment unchanged, thereby raising the full-time share of the workforce by 1.13 to 1.75 percentage points. A simple model rationalizes this pattern: because union power sustained the part-time periphery, weakening it shifts the adjustment onto the least-protected positions rather than the full-time core. Per-worker earnings are unchanged for full-time and part-time workers alike, in both datasets. I additionally confirm the earnings null using individual-level CPS and state-level QCEW data.

Keywords: Public-sector unions; Union membership; Collective bargaining; Agency fees; *Janus*; Free-riding

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1 Introduction

Public-sector unions represent millions of state and local government workers and shape how those workforces are sized, staffed, and compensated. Yet how much of that influence depends on union bargaining power has been difficult to study, because clean shocks to union bargaining power are rare — until the 2018 U.S. Supreme Court ruling in *Janus v. AFSCME* delivered an unusually large one. Overturning four decades of precedent, the Court held that public-sector workers could no longer be compelled to pay agency fees as a condition of employment, immediately making union financial support voluntary in the 21 states where it had previously been mandatory. In this paper, I examine how a contraction in union bargaining power reshapes the public-sector workforce — its size, its full-time/part-time composition, and the compensation it receives.

The labor market effects of *Janus* are ambiguous ex ante. If union bargaining power falls, wages, employment, or both could decline. However, public-sector wage schedules are embedded in multi-year collective bargaining agreements, civil-service pay scales, and political commitments that insulate wages from short-run adjustment (Freeman, 1986). By contrast, adjustments on the employment margin — and particularly on part-time employment, through hiring restraint, attrition, and the non-renewal of seasonal positions — may be more politically feasible when union capacity to resist them weakens (Poterba and Rueben, 1995; Scorsone and Plerhoples, 2010). This asymmetry motivates focusing on employment composition as a key margin of adjustment.

This institutional account departs from the standard neoclassical prediction (Farber, 1986), under which weaker unions both lower compensation and raise employment. Rather than the expansion of cheap, flexible part-time work that a cost-minimizing employer would choose, it points to adjustment on the employment margin, and, within employment, to an ordering that reflects whom unions protect. Because a union's membership and leadership are predominantly drawn from its full-time staff, with part-time workers only about half as likely to be unionized (5.4 versus 11.6 percent in 2018, BLS, 2019), a loss in union leverage results in the employment adjustments falling on the least-protected employees rather than on the full-time core.

Union capacity, moreover, is at least as much political as it is contractual. Public employees are a sizable and well-organized share of the local electorate, and they turn out at high rates in the low-salience contests that decide local office (Anzia, 2011, 2013; Sieg and Wang, 2013). Unions thus raise the political cost of restructuring the workforce, but only where officials answer to voters; because *Janus* lowered that cost, its effects should concentrate in general-purpose governments and remain muted in single-function special districts. I examine these predictions in Section 5.

Using a difference-in-differences (DiD) design that compares non-right-to-work (non-RTW, treated) and right-to-work (RTW, control) states, and drawing on two complementary datasets — three waves of the quinquennial Census of Governments (CoG) for 2012, 2017, and 2022, and a balanced 2011–2025 annual panel that combines the CoG with the Annual Survey of Public Employment and Payroll (ASPEP) — I document two main results. First, *Janus* reduced part-time employment by 6.2 to 8.4 percent, with no statistically or economically meaningful change in full-time employment, thereby raising the full-time share by 1.13 to 1.75 percentage points, with magnitudes depending on the dataset and specification. Using the annual frequency of the spliced panel to conduct an event study, I confirm the absence of a pre-trend for these outcome variables. The second finding is a null result on per-worker earnings, for both full-time and part-time employees. Although some specifications indicate a small *increase* after *Janus* for overall earnings, an earnings decomposition shows that this increase is driven almost entirely by composition — the loss of lower-paid part-time workers — and not by rising category-specific pay.

I confirm both the employment and the earnings results using two additional approaches. First, to isolate the effect of *Janus* from broader local shocks, I compare pairs of bordering states in which one had adopted RTW laws (e.g., Iowa) and its neighbor had not (e.g., Minnesota). Second, using the Census Finance Survey, I construct matched difference-in-differences estimators that condition on pre-treatment fiscal stress and liquidity.

Examining heterogeneity by government type offers clues as to why part-time employment fell but full-time employment did not. The decline is largest in general-purpose local governments — counties and municipalities — and statistically indistinguishable from zero in special districts, which deliver a single service and are funded largely through user charges rather than general taxation. This pattern is consistent with a political economy channel: by eroding unions' electoral capacity, *Janus* likely lowered the political cost to elected officials of workforce restructuring.

I validate the null result on earnings using BLS Quarterly Census of Employment and Wages (QCEW) state-level data (2011–2024) as well as 13 waves of the Current Population Survey (CPS) from 2011–2024 (excluding 2018). Individual-level data on worker characteristics in the CPS lets me account for possible variation in workforce composition that aggregate CoG data may mask. Using the CPS, I also find no change in whether public employers offer health insurance or a pension after *Janus*.

This paper contributes to the literature on public-sector labor market institutions in three ways. First, it identifies an employment-composition margin of adjustment to a major shock to union bargaining power, distinct from the compensation (Frandsen, 2016; Brunner and Ju, 2019)

or membership (Lyon, 2023) margins on which prior work has concentrated. Studies of the closest comparable shock — Wisconsin's Act 10 — have concentrated on teachers: their pay (Biasi, 2021) and retention (Biasi, 2024), their sorting across districts (Biasi, Fu, and Stromme, Forthcoming), and their students' achievement (Baron, 2018). None examines the size or full-time/part-time composition of the overall public-sector workforce. Second, it shows that public-sector union power affects staffing choices even when wages are sticky, consistent with Freeman's (1986) account of wage rigidity in public-sector unions. Third, it provides evidence consistent with a political economy channel: the employment effects concentrate in general-purpose governments and are indistinguishable from zero in single-function special districts. That the contrast runs along the general-purpose/single-function line is what one would expect if mandatory agency fees mattered in part by sustaining the electoral leverage public-sector unions exert over elected officials — the membership they mobilize as volunteers, canvassers, and voters (Flavin and Hartney, 2015; Feigenbaum et al., 2018; Carreri et al., 2025).

The most closely related study is Bagchi (2026), which uses administrative payroll data from 400 state and local governments to trace the membership and dues-paying response to *Janus*. The present paper addresses a different margin — the size and full-time/part-time composition of the workforce — across the near-universe of governments affected, and it pairs the employment analysis with a systematic treatment of earnings using event studies, multiple independent datasets, and a Lemieux (2006) decomposition. Workforce size and composition are outcomes the membership data cannot speak to, so the two analyses are complements rather than substitutes.

The paper proceeds as follows. Section 2 provides institutional background on *Janus* and public-sector collective bargaining. Section 3 describes the data and sample construction. Section 4 outlines the empirical strategy. Section 5 reports the results, including event studies, employment and earnings regressions, and heterogeneity by government type. Section 6 concludes.

2 Institutional Landscape: The *Janus* Decision and Public-Sector Collective Bargaining

While the National Labor Relations Act of 1935 governs private-sector bargaining, the rights of public-sector employees in state and local government are determined by state law. Prior to *Janus*, public-sector union law was governed by *Abood v. Detroit Board of Education* (431 U.S. 209, 1977), which held that states could constitutionally require non-union public employees to pay agency fees — also called "fair share" fees — to cover the union's costs of collective bargaining and contract

administration. As of June 2018, 21 states that had not enacted RTW laws permitted such fees, typically set at about 85 percent of full union dues (Murphy, 2019).[1]

On June 27, 2018, a divided Supreme Court overruled *Abood* in *Janus v. AFSCME*. The Court's five Republican appointees held, over the dissent of its four Democratic appointees, that mandatory agency fees constituted compelled speech in violation of the First Amendment. The decision took immediate effect, rendering mandatory agency fee arrangements in public-sector contracts unconstitutional. However, because the ruling left state labor law unaffected, unions retained exclusive representation rights. They thus remained obligated to represent every worker in the bargaining unit, non-members included, but lost the agency fees that had funded that representation. Additionally, full members could now drop their membership.

2.1 A Framework for the Composition Adjustment

Because public-sector pay is relatively inflexible — set by multi-year contracts and civil-service scales — a weaker union tends to push adjustment onto the employment margin rather than onto pay. Yet a natural question is why that adjustment fell on part-time positions while sparing the full-time core. The following framework, developed formally in Appendix A, points to the political rather than the budgetary logic of public employment. Consider an elected public employer who chooses how many full-time and part-time workers to employ, taking wages as fixed. Rather than minimizing cost, the employer maximizes net electoral support. The two workforces play different political roles. Full-time employees are tenured *insiders*: an organized, high-turnout bloc whose votes the employer can reliably count on. Part-time employees, in contrast, are *outsiders* who carry little independent political weight; their relevance to the employer exists solely through the union, whose political power B is what *Janus* reduced.

This structure yields two results. First, full-time employment depends little on B — the insider bloc delivers its votes with or without the union — so it barely moves when union power falls. Second, because the peripheral workforce's entire political value runs through the union, the response of part-time employment to a fall in B carries the sign of a single model primitive, θ, the direction in which the union deploys its power on the part-time margin. If the union had been *sustaining* part-time employment as a shock-absorbing buffer for its insiders ($\theta > 0$), weakening it

[1]These states are: Alaska, California, Connecticut, Delaware, Hawaii, Illinois, Maine, Maryland, Massachusetts, Minnesota, Montana, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, and Washington. Because most other states already prohibited mandatory union payments through RTW laws, *Janus* had no direct impact there. I exclude Colorado as its Labor Peace Act required a 75 percent "super-majority" to authorize agency fees; consequently, most local and all state agencies in Colorado operated as open shops prior to 2018.

causes part-time employment to fall; if the union had instead been *restraining* such employment because part-time employees compete with insiders ($\theta < 0$), then part-time employment rises. The framework turns the part-time direction into a sign test of θ; a decline implies $\theta > 0$, the buffer case.

This mechanism reframes a familiar intuition. The expectation that a weaker union should yield *more* part-time work — because employers find it cheap — presumes a cost-minimizing employer. That assumption fits the private sector better than the public sector. Here, union power is what made the peripheral workforce valuable; once *Janus* weakened it, the electorally expendable part-time positions were cut while the full-time core was preserved.

3 Data and Sample Construction

First dataset: the three-wave Census of Governments. The Census of Governments (CoG) is a quinquennial census of the roughly 90,000 U.S. state and local governments. The 2012 and 2017 waves give two clean pre-*Janus* observations and the 2022 wave a post-*Janus* observation four years after the ruling, after the initial adjustment period passed. As a census, the CoG preserves the full size distribution of governments — including the many small units that make up most of the universe — but its three observations per unit constrain dynamic pre-trend testing.

Second dataset: a balanced annual CoG–ASPEP panel. To add annual frequency, I combine the three CoG waves with the Annual Survey of Public Employment and Payroll (ASPEP), a yearly survey of a subset of governments, into a balanced 2011–2025 panel. Limiting to units observed in all 15 years yields about 3,800 governments per year, with 8 pre- and 7 post-*Janus* years each.[2] The annual structure supports the event study and dynamic identification the census cannot.

Estimation sample. I exclude states with ambiguous agency-fee status before *Janus* (Colorado, Missouri, and Wisconsin), leaving 47 states.[3] I also exclude townships given that full-time employment averages 31 workers per township in affected non-RTW states but fewer than 2 in RTW states, indicating that the label captures very different tiers of government depending on history and tradition. Because the log specification requires positive full- and part-time employment, units lacking either are dropped, leaving 35,822 CoG units in 2017. For the annual panel, the balance requirement further reduces the sample to about 1 in 10 units present in the CoG, tilted toward larger, more full-time-intensive governments: the median employer in the CoG–ASPEP panel is

[2]The March payroll reference month for the 2018 survey precedes the June 2018 ruling; thus, 2018 counts here as pre-*Janus*.

[3]Dropping states that adopted RTW laws during the 2010s (such as Michigan) leaves the results unchanged.

over 10 times larger (602.5 vs. 58 employees) and has a higher full-time share (0.82 vs. 0.69). The two datasets therefore complement each other — the CoG for its census coverage, the balanced panel for the annual frequency enabling dynamic identification — and I report results from both datasets throughout.

Outcomes. For both employment and earnings, I first separately examine full-time workers and part-time workers, and then the overall workforce. I also report the full-time share of employment as a compact summary of the relative shift between the two groups. I include summary statistics split by treatment status in Table A.1 and split by government type in Tables A.2 and A.3.

4 Empirical Strategy

The baseline estimating equation for examining the impact of *Janus* is:

$$y_{it} = \alpha_i + \gamma_t + \beta(\text{Treated}_s \times \text{Post}_t) + \varepsilon_{it} \tag{4.1}$$

where y_{it} is an outcome for government unit i in year t; α_i are unit fixed effects, γ_t are year fixed effects, Treated_s equals one for units in states with mandatory agency fees pre-*Janus*, and Post_t equals one for years after 2018. The coefficient of interest is β. I augment the baseline specification with state-specific linear trends to allow for the possibility that treated and control states were on different growth trajectories pre-treatment. This concern is first-order because agency-fee states tend to be Democratic-leaning, with higher unionization, higher per-employee spending, and potentially different long-run trends in public-sector employment and compensation. I cluster standard errors by state throughout.[4] Regressions are unweighted: the object of interest is the staffing response of the typical affected government — the unit at which the hiring decision and the political economy mechanism operate — not the aggregate change in public employment.

For the balanced annual CoG–ASPEP panel, I also plot the results from an event-study specification:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k \neq 2018} \beta_k \big(\text{Treated}_s \times \mathbf{1}[t = k]\big) + \varepsilon_{it} \tag{4.2}$$

with 2018 as the omitted reference year. The pre-period coefficients (β_k, $k < 2018$) serve as a direct test of the parallel trends assumption: If treated and control units were on similar trajectories before the ruling, these coefficients should be statistically indistinguishable from zero.

[4] Because the ruling took effect across all affected states simultaneously, with employers implementing it within a single pay cycle (Bagchi, 2026, Table A.2), the heterogeneous-timing concerns prominent in the recent DiD literature (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020) do not apply here.

To cross-validate the earnings results at the individual level, I estimate DiD models using the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), restricting to public-sector workers and using individual-level data on annual earnings and hourly wages. I also examine changes to the provision of employer-provided health insurance and pension coverage as total compensation could adjust on the benefits margin even if wages are rigid. The CPS data allows me to control for characteristics such as age, education, and gender, beyond state and year fixed effects.

5 Results

5.1 Overall Changes to Employment, Earnings, and the Full-Time Share

Table 1 reports estimates for all outcomes involving employment and earnings — separately for full-time, part-time, and all workers — in addition to the full-time share of employment. Panel A presents results from the CoG three-wave panel and Panel B from the balanced annual CoG–ASPEP panel. In each instance, I first present results obtained using conventional TWFE (Panels A1 and B1), followed by a specification that includes state-specific pre-trends (Panels A2 and B2).

The results are broadly consistent across both datasets and empirical approaches. Full-time employment is unchanged, with coefficients hovering around zero and statistically insignificant throughout. In contrast, there is a reduction in part-time employment, with magnitudes ranging from 6.2 percent (in the CoG–ASPEP panel with TWFE) to 8.4 percent (in the three-wave CoG with TWFE and state-specific trends). With full-time employment flat and part-time employment falling, the full-time share rises by 1.13 to 1.75 percentage points, with the increase being statistically significant at the 1 percent level throughout. Moreover, total employment drops — by about 2.6 to 2.8 percent in the three-wave CoG and about 1.4 to 1.8 percent in the annual sample, although only the former estimates are statistically significant. The somewhat smaller impacts in the annual panel likely reflect averaging over years in which the initial adjustment was still underway; the three-wave CoG estimate, by contrast, is based on 2022, by which point much of the adjustment had already taken place. These employment patterns reflect a net contraction of the part-time periphery rather than a reclassification of part-time positions into full-time ones: as I show in Table A.4, governments that entered the period with a larger part-time share do not register a larger full-time increase.

Turning to earnings, the coefficient is indistinguishable from zero for both full-time and part-

time per-worker earnings throughout. Overall earnings appear to hold steady in the annual panel, while the CoG three-wave panel suggests a small, and statistically significant, increase of about 1.7 percent. Although a rise in earnings following the loss of union leverage is surprising, as I show in Appendix B, a Lemieux (2006) decomposition confirms that this increase reflects the mechanical elevation of the overall average caused by the exit of lower-paid part-time workers and *not* by rising category-specific pay.

5.2 Identification

A possible concern with the estimates in Table 1 is that the post-2018 patterns reflect differential pre-*Janus* trends rather than the ruling itself. The annual panel can test for parallel trends directly: it runs from 2011 to 2025, with *Janus* at the midpoint.[5] Figure 1 plots event-study coefficients for log full-time employment, log part-time employment, and the full-time employment share.

Through 2017 the treated and control series move together — the pre-period coefficients are small and statistically indistinguishable from zero for all three outcomes — so the later movements do not extend a pre-existing trend.[6] The series then separate on the part-time margin alone, with full-time employment never diverging: the part-time coefficients remain small in 2019 and 2020, turn sharply negative in 2021, and persist thereafter. A divergence confined to the part-time margin is what a compositional effect would produce, and the next subsection shows that the 2021 onset is consistent with a lagged adjustment to *Janus* rather than the pandemic. This lag reflects hiring restraint, attrition, the non-renewal of seasonal positions, and the two-to-three-year contract cycle, under which pre-ruling agreements would have expired by 2020–2021, freeing employers to restructure staffing. The part-time decline attenuates in 2025, but that year is statistically indistinguishable from 2021–2024.

5.3 Distinguishing *Janus* from the Pandemic

A second concern is that the post-*Janus* period coincides with the COVID-19 pandemic, and the part-time decline I attribute to *Janus* simply reflects pandemic-era disruptions. Six pieces of evidence argue against that reading. The first five concern the public sector: the timing of the decline,

[5] The three-wave panel, with only two pre-periods, cannot itself test for parallel trends; nonetheless, a wave-specific placebo locates the full compositional shift in 2022 rather than in a pre-existing 2012–2017 trend.

[6] A falsification exercise reinforces this conclusion: assigning a placebo treatment date of 2015 to the pre-period yields precise zeros for both part-time employment (0.6 percent, s.e. 1.9) and the full-time share (0.1 percentage points, s.e. 0.3).

its insensitivity to local pandemic policy, its independence from the COVID relief governments received, its presence in services that never closed, and its incidence across types of government; the sixth turns to the private sector.

First, a pandemic channel predicts that the part-time decline should weaken once the pandemic years are removed. It does not. As Table 2 illustrates, dropping 2021, or both 2021 and 2022, leaves the decline essentially intact (5.6 and 5.3 percent). Furthermore, restricting the post-*Janus* period to the post-acute years 2023–2025 deepens it to 7.4 percent — the opposite of what a pandemic explanation would predict.

Second, the effect does not track the intensity of local pandemic policy. Splitting treated states at the median of the Oxford lockdown-stringency index, the part-time decline appears in both halves of both datasets, and the two halves are statistically indistinguishable. If anything, the decline is smaller where restrictions were tighter — the opposite of what a pandemic-driven mechanism would predict.[7]

Third, the result is not an artifact of pandemic aid. Controlling directly for the federal COVID relief each government received per capita, interacted with the post-period, leaves the part-time decline and the full-time share increase essentially unchanged in both datasets. Moreover, a pandemic account operating through fiscal stress would predict the largest part-time reductions among the most constrained governments; instead, as I show in Table A.5, the part-time decline does not scale with pre-period fiscal stress: it appears across the stress distribution and, in the annual panel, is statistically absent among the most stressed.

Fourth, the part-time decline is not limited to services that the pandemic shut down. Within treated states — holding lockdown policy, pandemic relief, and local labor-market conditions largely fixed — the part-time decline in policing is as large as the decline in services that were actually disrupted during the pandemic. Because policing stayed open throughout, a closure-based account cannot explain it.

Fifth, as I show in Subsection 5.5, the employment response is strongest in general-purpose governments — counties and municipalities — and weakest in single-function special districts, even as special districts entered the period employing part-time workers in comparable proportion to municipalities. A pandemic common to every local government in a state would not single out general-purpose governments; however, the fact that only they adjusted — and that the pre-*Janus* framework underwrote union political influence there — is what the proposed mechanism predicts.

[7] A triple-difference interacting Treated × Post with a standardized state lockdown-stringency index yields null or opposite-signed results, making pandemic confounding an unlikely explanation.

Sixth and finally, the private sector — which the pandemic hit at least as hard — displays the opposite profile. Applying the baseline event-study specification to private-sector outcomes from the CPS-ASEC, Figure A.1 shows a sharp part-time decline confined to 2020, the acute pandemic year, that recovers by 2021 — precisely when the public-sector decline begins. The placebo therefore does double duty: it confirms that the design detects a pandemic-driven composition shock where one in fact occurred, and it establishes what such a shock looks like — immediate and transitory. The public-sector adjustment, absent in 2020 and persistent through 2025, matches that signature in neither timing nor persistence.

Taken together, these patterns are difficult to reconcile with a pandemic explanation.

5.4 Robustness Checks: Confirming Null on FT Employment, Decline in PT Employment, and Rise in the FT Share

Table 3 presents robustness checks that confirm the null effect of *Janus* on full-time employment, the decline in part-time employment, and the rise in the full-time share on both the CoG three-wave panel and the CoG–ASPEP annual panel; Table A.6 reports the corresponding per-worker earnings results.[8]

Border discontinuity. I restrict the sample to government units in states that share a border with at least one non-agency-fee state, absorbing state-border-pair fixed effects to compare treated-state governments directly to their neighbors across the state line. The results are consistent with the full-panel findings: part-time employment falls by between 6.3 and 8.9 percent, the full-time share rises, and the full-time employment estimate remains statistically indistinguishable from zero.[9] The border estimates therefore weigh against broad regional shocks, though with only 23 state clusters I treat them as suggestive.

Matched difference-in-differences. I match treated and control units on two pre-treatment fiscal variables using nearest-neighbor Mahalanobis matching with exact matching on government type: long-term debt per employee, proxying long-run solvency (Gorina, Maher, and Joffe, 2018), and cash and deposits per employee, proxying cash solvency (Scorsone and Plerhoples, 2010). Both capture pre-treatment fiscal conditions on which treated and control governments could differ and that could independently bear on employment, making them natural covariates to balance. Be-

[8] Applying these designs to per-worker earnings reproduces the null: the full-time and part-time earnings estimates are indistinguishable from zero across both estimators and datasets.

[9] Because the border sample contains only 23 state clusters — too few for reliable asymptotic inference (Cameron, Gelbach, and Miller, 2008) — inference for the border panels uses the Webb wild-cluster bootstrap, under which the part-time employment and full-time share estimates remain significant at the 10 percent level (p-values = 0.054 and 0.062, respectively).

cause covariate matching absorbs pre-existing differences in fiscal conditions but does not guarantee parallel trends in outcomes, I estimate the matched DiD under the same two specifications used throughout: conventional TWFE, and TWFE augmented with state-specific pre-trends.

The results are consistent across both datasets: the decline in part-time employment is about 6 to 8 percent, the increase in full-time share is about 1 percentage point, and the full-time employment estimate is indistinguishable from zero throughout.

Robustness to functional form. Replacing the log transformation with the inverse hyperbolic sine (IHS) — which retains zero-employment observations — in Table A.7 yields estimates that are virtually identical to the baseline. Those results confirm that the decline in part-time employment is not driven by the exclusion of zeros from the log specification; instead, they suggest that treated governments reduced PT headcount rather than eliminating those positions.

Robustness to sample selection concerns: (a) Restricting the CoG–ASPEP panel to units observed in all 15 years could select a particular set of governments. Table A.8 progressively lowers the inclusion threshold from 15 years to as few as 8, nearly doubling the sample size. The results are essentially unchanged throughout: the part-time decline stays near 6 percent, the full-time share increase near 1 percentage point, while the full-time employment estimate is indistinguishable from zero in all panels.

(b) A second concern is that the balanced annual panel — which retains about 1 in 10 census units and tilts toward larger employers — is too selected to recover the effects identified in the representative census. Table A.9 shows otherwise. Restricting the balanced-panel units to the three census waves reproduces the full-census estimates almost exactly (a part-time decline of 7.9 percent in Panel B1 versus 7.5 percent in Panel A1; a full-time share gain of 1.5 versus 1.4 points). These selected units are also statistically indistinguishable from the non-selected remainder of the census (Panel C1); the modest gap between the two datasets therefore reflects frequency — how many years enter the estimation — rather than selection into the balanced panel.

Influential states. Finally, I drop each of the 21 treated states individually and then every possible pair (for example, dropping California and Illinois simultaneously) and find that the result is not driven by any single state or pair. As Figure A.2 shows, the part-time employment and full-time share coefficients stay clustered around the full-sample estimate, and their statistical significance is durable. The full-time share coefficient is significant at the 5 percent level (or better) in all 231 leave-one-out and leave-two-out samples, in both datasets. The part-time-employment coefficient is significant at the 5 percent level in all 231 samples in the CoG, and it stays significant at the 10 percent level in the large majority of the annual panel samples (199 of 231).

5.5 Government-Type Heterogeneity and the Political Economy Channel

Having shown the average effect to be robust, I now turn to how it varies across types of government, since that variation bears on its interpretation. Table 4 reports the baseline specification estimated separately by government type, and the results reveal a pattern consistent with a political economy channel.[10]

Among the various forms of local government, counties show the largest and most precisely estimated response. Part-time employment falls by about 16.0 percent in the CoG three-wave panel and about 13.1 percent in the balanced annual panel, full-time employment is unchanged in both, and the full-time share rises in step — by about 1.6 percentage points in the CoG panel and 1.2 percentage points in the annual panel. Municipalities show a weaker version of the same pattern: part-time employment falls by about 5.7 percent in the CoG panel and 10.2 percent in the annual panel, but the full-time share rises only modestly — about 0.6 and 1.0 percentage points — and falls short of conventional significance in either dataset.

School districts present a more mixed picture. In the CoG three-wave panel the full-time share rises by a statistically significant 1.9 percentage points, though the accompanying part-time decline of about 7.3 percent is imprecisely estimated and not itself significant. In the balanced annual panel neither estimate is statistically distinguishable from zero. However, that panel retains fewer than 5 percent of treated CoG districts, so I rely on the CoG three-wave estimates here.

The employment response is weakest in special districts. These entities — water and sewer authorities, housing authorities, transit agencies — deliver a single service and are frequently financed through user charges rather than general taxation, and across all specifications and both datasets their part-time employment and full-time share coefficients are statistically indistinguishable from zero, although imprecisely estimated.[11] This is not for want of a part-time margin to cut: Table A.2 shows special districts entered the post-*Janus* period with a mean full-time share of 0.59, nearly identical to municipalities at 0.57, so they employed part-time workers in comparable proportion yet show no similar decline.[12],[13] That the response is concentrated in

[10] Figure A.3 plots the event studies for the full-time share for all four types. Table A.10 presents corresponding results with TWFE and state-specific trends.

[11] The special-district estimated impact on part-time employment is imprecise; its 95 percent confidence interval, [−8.3, +3.5] percent, rules out a county-sized decline (−16.0 percent) but not a smaller, municipality-sized one — so the sharpest contrast is with counties, where the differential is both large and statistically significant.

[12] Table A.3 tells a similar story for the annual panel; special districts are at 0.79 full-time versus 0.78 for municipalities.

[13] Elected and appointed special-district boards are roughly equally common in the treated states, and neither shows a compositional shift; in the three-wave Census panel each shows a smaller part-time response than general-purpose governments, and the two are statistically indistinguishable at the 5 percent level.

general-purpose governments — even within the same local labor markets — is consistent with a theory in which agency fees mattered partly because they funded the political capacity that disciplined elected officials there (Flavin and Hartney, 2015; Feigenbaum et al., 2018; Carreri et al., 2025).[14]

5.6 Individual-Level Evidence from the CPS-ASEC

Having established the employment composition effect, I now corroborate the null earnings result with worker-level data. The CoG and ASPEP report only establishment averages, which a compositional shift toward full-time work could mechanically inflate; the CPS-ASEC instead records earnings for individual workers, and is immune to that concern. I estimate difference-in-differences models on the CPS-ASEC, restricting the sample to state and local government workers.

The wage outcomes I choose are the log of annual earnings and log of hourly wages, each measured either as-is or winsorized at the 5th and 95th percentiles. I present results from a parsimonious specification that includes no controls and then progressively introduce individual-level demographic controls — a strength of this source over the CoG or the ASPEP. As shown in Panels A and B of Table A.12, the "Treated x Post" coefficient is small and consistently indistinguishable from zero across all earnings and wage measures and specifications.[15]

The CPS also captures non-wage benefits — such as pensions and health insurance coverage, both substantially larger in the public sector than in the private (Gittleman and Pierce, 2012). The non-wage results reinforce this conclusion: linear probability model (LPM) and probit estimates for whether health insurance coverage and pension benefits are offered are indistinguishable from zero, as shown in Panels C and D of Table A.12.

A final, external check uses BLS QCEW state-level data (2011–2024). Panel B of Table A.13 shows that average annual pay in treated states does not diverge from control states after *Janus* in any specification, consistent with the CPS null result. Public employment (Panel A) moves little: the state-government estimate is insignificant throughout, and the local-government estimate is insignificant with TWFE, attaining significance only at the 10 percent level once state-specific trends are added. Because the QCEW does not separate full- from part-time workers, it cannot speak to the composition margin, which continues to rest on the CoG and CoG–ASPEP panels.

[14] A cut by union strength points the same way: the part-time decline is concentrated among treated states with below-median 2017 public-sector union density and is smaller, and less precisely estimated, among higher-density states, as shown in Table A.11. The gradient is consistent with stronger unions being better able to resist the part-time adjustment.

[15] Adding occupation fixed effects leaves the point estimates unchanged, so the null is not driven by occupational composition. In the winsorized, fully controlled column, the CI rules out earnings changes larger than about 3 percent.

6 Discussion and Conclusions

The *Janus v. AFSCME* ruling was the most significant shock to public-sector labor relations in over four decades, affecting state and local governments in 21 states that had previously permitted mandatory agency fees. Using data from the Census of Governments and ASPEP, I find that the elimination of these fees resulted in changes to employment composition rather than in broad headcount cuts or changes to compensation. Across both datasets, I find that governments in affected states reduced part-time employment by about 6 to 8 percent, while full-time employment was unchanged. These results are robust to border and matched designs and are not an artifact of the overlapping COVID-19 pandemic.

On earnings, I do not observe any statistically significant change for either full-time or part-time workers. A rise in per-worker earnings observed for the overall sample is almost entirely explained by the compositional effect of losing lower-paid part-time workers. I confirm the null earnings results using the CPS, which allows individual-level demographic controls, as well as state-level QCEW data.

The government-type pattern speaks to the possible channels that underpin these results. The part-time decline is concentrated in general-purpose governments — counties and municipalities — and weaker, although imprecisely estimated, in single-function special districts. This contrast suggests that agency fees may have mattered partly through the political capacity they provided to unions in dealing with elected officials. The *Janus* ruling, by making mandatory agency fees illegal and allowing employees to opt out of paying dues altogether, likely reduced the ability of unions to influence employment outcomes in settings where their political influence had been greatest.

Two qualifications merit emphasis. First, the design identifies the joint effect of agency-fee elimination and the broader institutional response — by unions, by newly empowered employees, and by state governments that often made opting out harder — and it cannot attribute the compositional shift to any single mechanism. Because several of these responses blunted it, the estimates likely represent a lower bound on the effect of an unmitigated loss of agency-fee financing. Second, the estimates reflect a medium-run, rather than a long-run, response since RTW effects can take up to a decade to materialize (Lyon, 2023). Subject to those qualifications, the evidence establishes that public-sector union power affects more than wages: it shapes the composition of public workforces, particularly the FT-PT margin in general-purpose governments. The results suggest that the consequences of *Janus* extend well beyond dues revenue, reshaping not just union finances but the structure of the public workforces unions have long helped organize.

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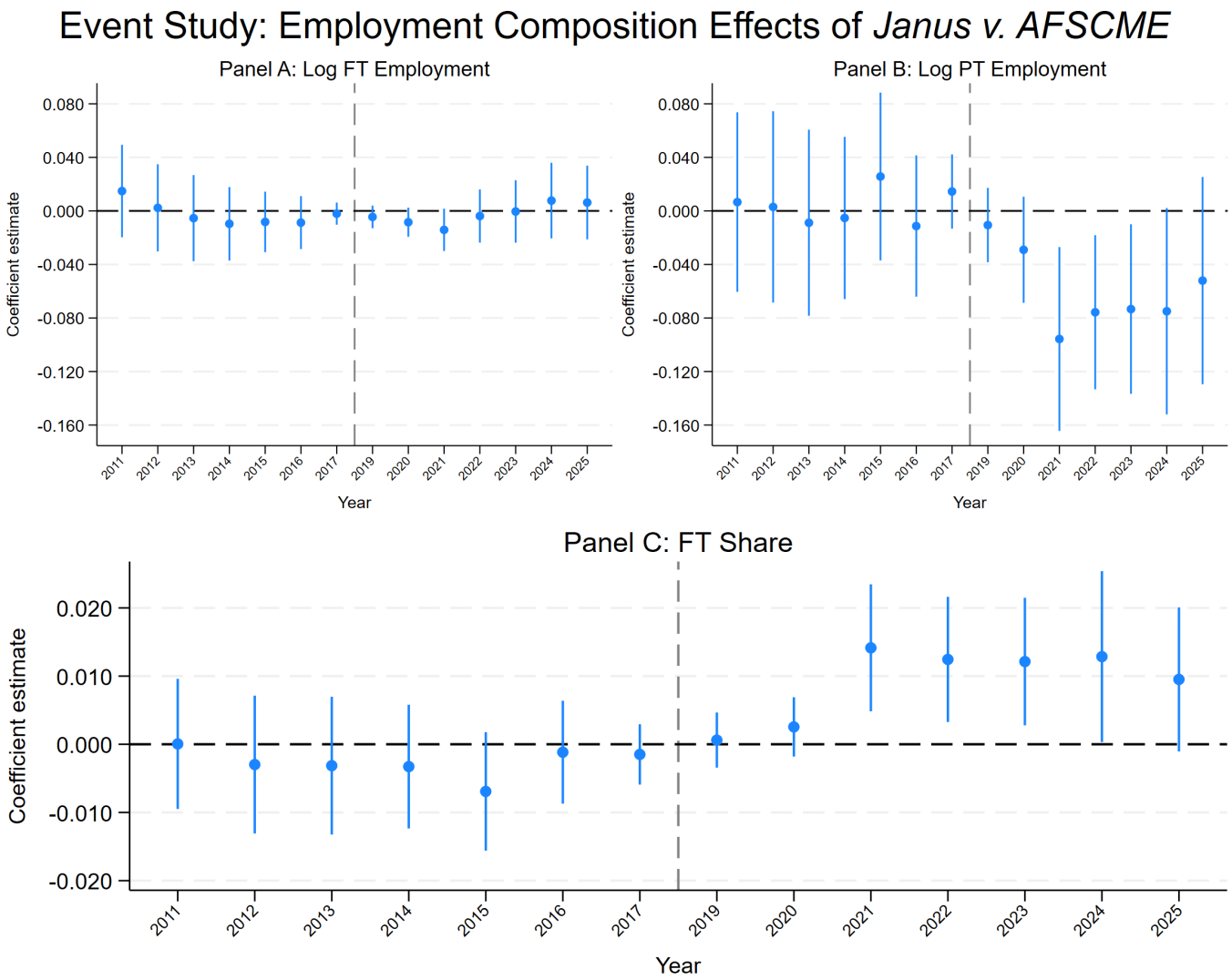
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Figure 1: Event study plot of the effects of *Janus* on FT employment, PT employment, and FT share



Notes: The dependent variables are the natural logarithm of full-time employment (Panel A: Log FT Employment), the natural logarithm of part-time employment (Panel B: Log PT Employment), and the share of full-time workers in total employment (Panel C: FT Share). The sample is the CoG–ASPEP balanced annual panel (2011–2025), restricted to government units observed in all 15 years; townships are excluded. All regressions are the baseline plain two-way fixed-effects specification, including government-unit and year fixed effects, with no state-specific trends. Each point is an event-study coefficient relative to the 2018 base year (the pre-*Janus* reference year, with the vertical dashed line marking the June 2018 ruling), with 95 percent confidence intervals; standard errors are clustered by state. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1. Regressions are unweighted.

Table 1: Baseline estimates of the impact of *Janus* on employment and earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	FT Empl.	PT Empl.	Tot Empl.	FT Earnings	PT Earnings	Overall Earnings	FT Share
Panel A1: CoG three-wave; TWFE							
Treated x Post	-0.0015	-0.0754***	-0.0282*	0.0008	-0.0041	0.0174*	0.0140***
	(0.0165)	(0.0227)	(0.0155)	(0.0100)	(0.0111)	(0.0090)	(0.0039)
Panel A2: CoG three-wave; TWFE + State-specific trends							
Treated x Post	0.0088	-0.0838***	-0.0262***	-0.0049	0.0051	0.0178**	0.0175***
	(0.0119)	(0.0175)	(0.0093)	(0.0075)	(0.0101)	(0.0078)	(0.0038)
Number of observations	102241	102241	102241	102241	102241	102241	102241
Number of clusters	47	47	47	47	47	47	47
Panel B1: CoG–ASPEP balanced annual; TWFE							
Treated x Post	-0.0004	-0.0617*	-0.0183	-0.0014	0.0226	0.0100	0.0115***
	(0.0172)	(0.0312)	(0.0149)	(0.0091)	(0.0201)	(0.0098)	(0.0042)
Panel B2: CoG–ASPEP balanced annual; TWFE + State-specific trends							
Treated x Post	0.0044	-0.0635**	-0.0137	-0.0044	0.0209	0.0081	0.0113***
	(0.0117)	(0.0263)	(0.0101)	(0.0076)	(0.0181)	(0.0083)	(0.0040)
Number of observations	55029	55029	55029	55029	55029	55029	55029
Number of clusters	47	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), part-time employment (PT Empl.), and total employment (Tot Empl.); the natural logarithm of annualized full-time payroll per worker (FT Earnings), part-time payroll per worker (PT Earnings), and overall payroll per worker (Overall Earnings); and the share of full-time workers in total employment (FT Share). The treatment indicator equals one if the unit is located in a state that had a mandatory agency-fee law in place at the time of the *Janus* ruling (June 2018) and zero otherwise; Colorado, Missouri, and Wisconsin are excluded because of their ambiguous agency-fee status during the sample period. The CoG three-wave rows use the Census of Governments three-wave panel (survey years 2012, 2017, and 2022); Treated x Post equals one in 2022 for treated units. The CoG–ASPEP rows use the balanced annual panel (2011–2025); Treated x Post equals one in 2019 and later for treated units. Both panels exclude townships. The regression sample in each panel is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification. All regressions include government-unit and year fixed effects; Panels A1 and B1 are the baseline two-way fixed-effects specification; Panels A2 and B2 add state-specific pre-period linear trends. Regressions are unweighted.

Table 2: Distinguishing *Janus* from the pandemic: Robustness of the main results to leave-out timing and lockdown stringency

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	FT Share	FT Empl.	PT Empl.	FT Share
Panel A. Timing — Leaving out the pandemic years (using the CoG–ASPEP panel)						
R0 Baseline full sample	-0.0004	-0.0617*	0.0115***	0.0044	-0.0635**	0.0113***
(2011–2025)	(0.0172)	(0.0312)	(0.0042)	(0.0117)	(0.0263)	(0.0040)
R1 Excluding 2021	0.0016	-0.0563*	0.0108**	0.0064	-0.0579**	0.0106**
	(0.0174)	(0.0327)	(0.0044)	(0.0120)	(0.0272)	(0.0041)
R2 Excluding 2021 and 2022	0.0022	-0.0527	0.0100**	0.0070	-0.0539*	0.0098**
	(0.0173)	(0.0327)	(0.0044)	(0.0120)	(0.0271)	(0.0040)
R3 Post-recovery only	0.0062	-0.0744*	0.0141**	0.0112	-0.0755**	0.0139**
(2023–2025)	(0.0208)	(0.0404)	(0.0057)	(0.0157)	(0.0362)	(0.0055)
Panel B1. Pandemic exposure — Treated x Post within subsample (using the CoG–ASPEP panel)						
R4.1 Below-median-stringency	-0.0180	-0.0873**	0.0116**	-0.0050	-0.0724*	0.0108*
treated states	(0.0181)	(0.0407)	(0.0054)	(0.0122)	(0.0376)	(0.0054)
R4.2 Above-median-stringency	0.0248	-0.0252	0.0114**	0.0180	-0.0508*	0.0122***
treated states	(0.0233)	(0.0299)	(0.0055)	(0.0164)	(0.0251)	(0.0040)
Panel B2. Pandemic exposure — Treated x Post within subsample (using the three-wave CoG panel)						
R4.1 Below-median-stringency	-0.0099	-0.0950***	0.0157***	0.0040	-0.0903***	0.0175***
treated states	(0.0168)	(0.0241)	(0.0046)	(0.0132)	(0.0206)	(0.0048)
R4.2 Above-median-stringency	0.0126	-0.0426*	0.0111***	0.0169	-0.0729***	0.0174***
treated states	(0.0251)	(0.0240)	(0.0037)	(0.0159)	(0.0192)	(0.0035)
State-specific pre-trends	No	No	No	Yes	Yes	Yes

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), the natural logarithm of part-time employment (PT Empl.), and the share of full-time workers in total employment (FT Share). Each of these three outcomes appears twice: columns 1-3 report the plain two-way fixed-effects (baseline) specification and columns 4-6 add state-specific pre-period linear trends. Panels A and B1 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on); Panel B2 uses the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022). All regressions include government-unit and year fixed effects. Panel A is a leave-out-timing robustness check on the CoG–ASPEP panel, reporting in turn the full-panel baseline, the panel excluding 2021, the panel excluding 2021 and 2022, and a post-recovery window restricted to pre-period years and 2023–2025. Panels B1 and B2 split treated states at the state-collapsed median of the 2020–2021 OxCGRT lockdown-stringency index into below-median and above-median exposure subsamples, with control states retained in both halves. Each panel is a separate sample, and the regression sample in each is the set of observations with non-missing values for the three core outcomes. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table 3: Border and matched difference-in-differences robustness: employment and the full-time share

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	FT Share	FT Empl.	PT Empl.	FT Share
Panel A1: Using a border-state matching approach (three-wave CoG)						
Treated x Post	-0.0134	-0.0678*	0.0096*	-0.0020	-0.0628**	0.0120**
	(0.0247)	(0.0352)	(0.0051)	(0.0180)	(0.0256)	(0.0049)
Number of observations	65385	65385	65385	65385	65385	65385
Number of clusters	23	23	23	23	23	23
Panel A2: Using log of long-term debt and cash reserves per employee for matching (three-wave CoG)						
Treated x Post	-0.0151	-0.0693***	0.0108***	-0.0059	-0.0757***	0.0134***
	(0.0151)	(0.0243)	(0.0037)	(0.0102)	(0.0176)	(0.0035)
Number of observations	53429	53429	53429	53429	53429	53429
Number of clusters	47	47	47	47	47	47
Panel B1: Using a border-state matching approach (CoG–ASPEP)						
Treated x Post	-0.0120	-0.0839*	0.0111*	-0.0077	-0.0887*	0.0126*
	(0.0234)	(0.0432)	(0.0065)	(0.0145)	(0.0468)	(0.0075)
Number of observations	6336	6336	6336	6336	6336	6336
Number of clusters	23	23	23	23	23	23
Panel B2: Using log of long-term debt and cash reserves per employee for matching (CoG–ASPEP)						
Treated x Post	-0.0093	-0.0627	0.0104**	-0.0054	-0.0727**	0.0111**
	(0.0182)	(0.0383)	(0.0048)	(0.0123)	(0.0314)	(0.0043)
Number of observations	32867	32867	32867	32867	32867	32867
Number of clusters	47	47	47	47	47	47
State-specific pre-trends	No	No	No	Yes	Yes	Yes

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), part-time employment (PT Empl.), and the share of full-time workers in total employment (FT Share). Panels A1 and A2 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panels B1 and B2 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the plain-TWFE columns (columns 1-3) are the baseline and the state-trends columns (columns 4-6) add state-specific pre-period linear trends. Panels A1 and B1 estimate a state-border-pair difference-in-differences among government units in border states — treated states adjacent to a right-to-work control state, and the control states they adjoin — adding state-border-pair segment fixed effects. Because cluster-robust inference is not identified across the 23 state clusters once state trends are included, all six border columns report standard errors inverted from a Webb wild-cluster bootstrap (999 replications; seed 12345), with significance levels based on the bootstrap p-value. Panels A2 and B2 estimate a matched difference-in-differences using 1-nearest-neighbor Mahalanobis matching exact on government type jointly on 2017 long-term debt per employee and cash reserves per employee; standard errors are clustered by state. The regression sample in each panel is the set of observations with non-missing values for all three outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table 4: Difference-in-differences estimates of the impact of *Janus* on employment using TWFE — Split by type of government

	Census of Governments			CoG–ASPEP		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	FT Share	FT Empl.	PT Empl.	FT Share
Panel A: Counties						
Treated x Post	-0.0114	-0.1597***	0.0161***	-0.0298	-0.1313***	0.0121**
	(0.0169)	(0.0356)	(0.0043)	(0.0227)	(0.0446)	(0.0046)
Number of observations	8171	8171	8171	11304	11304	11304
Number of clusters	45	45	45	45	45	45
Panel B: Municipalities						
Treated x Post	-0.0300*	-0.0565*	0.0059	-0.0202	-0.1015**	0.0098
	(0.0159)	(0.0290)	(0.0060)	(0.0168)	(0.0498)	(0.0060)
Number of observations	37453	37453	37453	20309	20309	20309
Number of clusters	47	47	47	47	47	47
Panel C: School districts						
Treated x Post	0.0057	-0.0732	0.0186***	0.0249	0.0623	0.0118
	(0.0187)	(0.0510)	(0.0054)	(0.0211)	(0.0914)	(0.0108)
Number of observations	33621	33621	33621	11462	11462	11462
Number of clusters	44	44	44	41	41	41
Panel D: Special districts						
Treated x Post	0.0163	-0.0244	0.0064	0.0262	-0.0267	0.0086
	(0.0232)	(0.0293)	(0.0060)	(0.0253)	(0.0388)	(0.0068)
Number of observations	22855	22855	22855	11249	11249	11249
Number of clusters	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), the natural logarithm of part-time employment (PT Empl.), and the share of full-time workers in total employment (FT Share). Columns 1-3 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); columns 4-6 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; every column is the baseline plain two-way fixed-effects specification with no state-specific trends. Each panel reports a separate government type, estimated on its own subsample: Panel A counties, Panel B municipalities, Panel C independent school districts, and Panel D special districts. The regression sample in each category-by-dataset cell is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1; state governments and townships are excluded. Regressions are unweighted.

Online Appendix

A Political Economy of the Full-Time and Part-Time Adjustment

This Appendix develops the framework summarized in Section 2.1. The framework rationalizes why the employment adjustment that followed *Janus* fell on the part-time rather than the full-time margin. It also isolates the condition under which a weaker union reduces rather than expands part-time employment, and generates the testable predictions examined in the body of the paper. The central modeling choice is to treat the public employer not as a cost-minimizing firm but as an office-motivated politician, a distinction that proves essential: a cost minimizer facing fixed wages and a weakened union would expand cheap, flexible part-time labor, whereas an elected official who values the votes of the career workforce would not necessarily do so.

Environment

Consider an elected public employer — a mayor, council, or governing board — who chooses the size of two workforces in a single period: a stock of full-time employees F and a stock of part-time employees P, both non-negative. Two institutionally grounded features of the setting shape the model. First, full-time employees are *insiders*: they hold tenured, civil-service-protected positions, they are overwhelmingly inside the bargaining unit, and they constitute an organized, high-turnout political bloc. Part-time employees are *outsiders*: they are the peripheral workforce, are less likely to be unionized or even eligible for the bargaining unit, and carry little independent political weight.[16] Second, pay is fixed over the relevant horizon by multi-year collective bargaining agreements and civil-service salary scales, so the employer adjusts headcount, not pay.

Employing workers is costly, and that cost falls on current resources: wages are paid from today's revenue and, unlike deferred obligations such as pension liabilities, those costs cannot be shifted onto future taxpayers (Novy-Marx and Rauh, 2011; Glaeser and Ponzetto, 2014). Financing the payroll is increasingly costly at the margin. The rising marginal cost of public funds implies that each additional position is financed from a more distortionary tax base than the last and accordingly the politician is cognizant of headcount (Ballard, Shoven, and Whalley, 1985; Hendren and Sprung-Keyser, 2020). The model thus preserves a basic public-finance tension between win-

[16] This core/periphery distinction has an empirical counterpart in the membership response to *Janus* itself: Bagchi (2026) finds that dues-paying membership held roughly flat among teachers, police, and firefighters — career occupations with strong professional stakes in representation — while it fell among administrative and social-service staff, the lower-attachment, more peripheral groups that overlap with the part-time workforce.

ning the votes of public-sector workers by employing them and losing votes of others by taxing them to pay for those costs.

The Politician's Objective

Let B denote the *political* power of the public-sector union — its capacity to mobilize voters and campaign for favored candidates. This is the object that *Janus* reduced, by ending the mandatory agency fees that financed it and by letting union members opt out if they chose to *without* losing coverage under collective bargaining. Public-sector workers are a politically consequential constituency: they turn out at higher rates than the general electorate (Johnson and Libecap, 1991) and can swing low-turnout local elections through both their votes and their unions' campaign support (Sieg and Wang, 2013). The politician therefore chooses F and P to maximize net electoral support:

$$S(F,P) = a\ln F + \lambda B\ln F + \theta B\ln P + cF + ckP - (\tfrac{1}{2}w_F F^2 + \tfrac{1}{2}w_P P^2).$$

Each term in the objective function is a political object. The term $a\ln F$ is the *autonomous* electoral value of the full-time bloc; because full-time employees are tenured and vote regardless of the union's strength, this value is independent of B. It is also concave so the marginal political return to an additional career employee diminishes. The term $\lambda B\ln F$ is the union's marginal amplification of full-time support — the incremental mobilization the union supplies on behalf of a workforce that would largely turn out anyway — with $\lambda \geq 0$ assumed small.[17]

The third term, $\theta B\ln P$, is the *entire* first-order political value of the part-time workforce. Outsiders have no autonomous electoral weight; their political relevance to the politician exists only insofar as the union, with power B, makes it so. The parameter θ is the model's central primitive: it is the direction in which the union aims its political muscle on the peripheral margin. If $\theta > 0$, the union protects the periphery, rewarding the politician with support for keeping part-timers on the payroll; if $\theta < 0$, the union instead works to suppress part-time positions, as they compete with its core full-time members. The *Janus* decision is modeled as an exogenous fall in B, from a pre-decision level B_0 to a lower post-decision level B_1.

$cF + ckP$ is the value voters place on the public services the workforce delivers — for example, police officers on patrol, classrooms staffed with teachers, recreational facilities open for longer hours — where the constant c is a full-timer's service value and $k \in (0,1)$ scales a part-timer's below it. Lastly, the term $(\tfrac{1}{2}w_F F^2 + \tfrac{1}{2}w_P P^2)$ represents the convex payroll cost. The wage rates

[17] An alternative and functionally equivalent formulation simply drops λ; all results below continue to hold.

w_F and w_P are fixed by collective bargaining and held constant by assumption; the cost is convex, though, rather than linear in headcount as each additional position must be financed from an increasingly distortionary tax base — the rising marginal cost of public funds.

The objective function is constructed to be separable in the number of full-time (F) and part-time workers (P). Separability is partly a tractability assumption and partly substantive: full-time and part-time positions each carry their own payroll, and the two groups reach the politician through distinct channels — full-time workers directly via their own votes (in addition to any union-mediated influence), whereas part-time workers affect political support only through their public-sector union. The assumption is a simplification rather than a substantive restriction; I return to its robustness and the supporting evidence in the Discussion below.

Equilibrium

The Hessian of S is diagonal, with $S_{FF} = -(a + \lambda B)/F^2 - w_F < 0$ and $S_{PP} = -\theta B/P^2 - w_P$, and a zero cross-partial $S_{FP} = 0$. The program is therefore strictly concave wherever an interior part-time optimum exists, and it has a unique interior maximum. The first-order conditions are

$$\frac{a + \lambda B}{F} + c - w_F F = 0, \tag{A.1}$$

$$\frac{\theta B}{P} + ck - w_P P = 0, \tag{A.2}$$

each a single quadratic in its own variable. Taking the economically relevant positive root of each,

$$F^* = \frac{c + \sqrt{c^2 + 4w_F(a + \lambda B)}}{2w_F}, \tag{A.3}$$

$$P^* = \frac{ck + \sqrt{(ck)^2 + 4w_P\theta B}}{2w_P}. \tag{A.4}$$

Wages enter only as fixed parameters and are not chosen, so earnings are invariant by design.

The Main Result

Proposition. At the interior optimum:

(i) full-time employment is weakly increasing in union power, and is *identically invariant* to union power ($dF^*/dB = 0$ for every B) when $\lambda = 0$;

(ii) part-time employment is globally strictly monotone in union power, its response carrying the sign of θ at every interior B.

Consequently, any fall in union power — of any magnitude — changes part-time employment by $-\operatorname{sign}(\theta)$: if $\theta > 0$, part-time employment falls and the full-time share rises; if $\theta < 0$, part-time employment rises and the full-time share falls. The two regimes meet at the crossover $\theta = 0$, and the regime $\theta < 0$ is non-empty and interior for $\theta \in \big(-(ck)^2/(4w_P B),\, 0\big)$.

Proof. Each first-order condition involves only its own headcount, so the two solve independently for the positive roots F^* and P^* stated above. Strict concavity, established by the diagonal Hessian with $S_{FF} < 0$ and $S_{PP} < 0$ at the upper root in both regimes, confirms these are maxima. Differentiating F^* and P^* with respect to B yields

$$\frac{dF^*}{dB} = \frac{\lambda}{\sqrt{c^2 + 4w_F(a + \lambda B)}} \geq 0, \tag{A.5}$$

$$\frac{dP^*}{dB} = \frac{\theta}{\sqrt{(ck)^2 + 4w_P \theta B}}. \tag{A.6}$$

Both square roots are strictly positive on the interior — the full-time root always, and the part-time root wherever the discriminant $(ck)^2 + 4w_P\theta B > 0$, which rearranges to $\theta > -(ck)^2/(4w_P B)$ and defines the interior region. Hence the sign of dF^*/dB equals the sign of λ, and is zero when $\lambda = 0$, while the sign of dP^*/dB equals the sign of θ at every interior B. Because θ is the sole sign-carrier and the denominator is positive throughout, the statement is global rather than local, holding for the full empirical fall in B, which yields the two predictions below.

Comparative Statics and Empirical Predictions

I model the *Janus* decision as resulting in a reduction in the union strength parameter, B. Recalling that θ is the model's central primitive that determines how the union aims its political muscle on the peripheral part-time margin, for $\theta > 0$, the proposition yields two predictions.

Prediction 1 — Impact on Part-time employment: The fall in union power reduces part-time employment, the model's counterpart to the estimated decline of roughly 6 to 8 percent.

Prediction 2 — Impact on Full-time employment: Full-time employment is essentially unchanged — exactly so when $\lambda = 0$, and negligibly responsive for small λ. Thus, the full-time share rises mechanically as part-time contracts and full-time employment stays flat.

The Direction of Union Power

The parameter θ governs how union power maps into part-time employment. If $\theta > 0$, union power confers political value on the periphery — the term $\theta B \ln P$ rewards the politician for keeping part-

timers employed, so union strength sustains those positions; a fall in B withdraws the reward, and because part-timers carry no autonomous votes, the politician sheds them. If $\theta < 0$, part-time positions instead threaten insiders — competing for their jobs or undercutting their terms — so the union suppresses them, and a fall in B raises part-time employment. This second case corresponds, in spirit, to the standard expectation that a weaker union yields more contingent work — the prediction of a cost-minimizing employer, and more generally of any setting in which profit-maximizing motives are first-order and political economy is second-order.

The sign of θ is not arbitrary; it follows from insider-outsider primitives (Lindbeck and Snower, 1988; Saint-Paul, 2000). If the union acts for its insider members and the workforce faces shocks that force cuts, seniority and civil-service protections imply part-time positions are cut first, so a larger part-time stock P shields insiders by absorbing adjustment that would otherwise reach them — a buffer worth $\beta \ln P$ to insiders ($\beta > 0$). As in standard interest-group politics (Becker, 1983; Grossman and Helpman, 1994), the union backs the incumbent in proportion to the benefit its staffing delivers to insiders — requiring both the union's clout B and a member stake worth rewarding — so its backing on the part-time margin is $\beta B \ln P$, exactly $\theta B \ln P$, giving $\theta = \beta$. Thus $\theta > 0$ is a consequence of the layoff ordering — part-time first — which makes the periphery a buffer insiders rationally defend (Farber, 1986). By contrast, $\theta < 0$ arises when part-timers instead substitute for insiders. The sign of θ is the buffer value net of the substitution threat.

Both regimes are admissible under the same primitives, so the direction of the effect is an empirical matter. The observed decline in part-time employment after *Janus* corresponds to $\theta > 0$: union power sustained the periphery, and weakening it reduced part-time employment rather than expanding it — identifying the buffer motive as the dominant one in the public sector.

Discussion

Three features of the model warrant comment. First, the micro-foundation above derives the direction of θ from the seniority ordering of layoffs but takes the strength of the buffer motive, and the union's objective more broadly, as given rather than deriving it from a fully specified bargaining game. Second, the model's near-zero full-time response is more an assumption than a result. By taking insider political support to be largely independent of union strength (exactly so at $\lambda = 0$) — an independence I motivate on civil-service and seniority grounds rather than derive — the model effectively builds full-time stability in by construction. I therefore do not lean on the observed full-time stability as independent evidence for the mechanism; the part-time decline, not

the full-time null, is what the model is meant to explain. Third, the separability of the first-order conditions is innocuous: modest substitution between full-time and part-time labor leaves the sign of the part-time response and the stability of full-time employment intact, and the data show little substitution in any case. A triple-difference specification interacting the post-*Janus* period with pre-period part-time intensity finds the full-time response small and insignificant in both samples (Table A.4); units more reliant on part-time labor *did not* convert those positions into full-time ones, so the decline reflects a net contraction of the periphery rather than a substitution toward the core.

B Earnings Decomposition

This Appendix decomposes the small positive post-*Janus* change in per-worker earnings into a component due to the shifting full-time/part-time composition of the workforce and a component due to changes in category-specific pay, following Lemieux (2006).

The CoG three-wave panel results show a small positive post-*Janus* increase in total per-worker annual earnings of about 1.7 percent, the all-employee per-worker earnings estimate first reported in Table 1. An increase in earnings is surprising; if *Janus* weakened union bargaining power as surmised, one would expect downward pressure on earnings rather than earnings growth. Table A.14 resolves this tension by showing that the increase is a mechanical composition effect driven by the loss of part-time employees rather than a genuine wage gain.

I construct two counterfactual earnings series following Lemieux (2006). The composition counterfactual holds the earnings per full-time and per part-time employee fixed at their 2017 (pre-*Janus*) levels and asks how much average per-worker annual earnings would change solely from the post-*Janus* shift in the full-time/part-time employment mix. The wage counterfactual, by contrast, holds the 2017 full-time/part-time employment mix fixed and asks how much average per-worker earnings would change solely from changes in category-specific earnings. Because part-time employees earn substantially less than full-time employees,[18] a shift toward full-time employment would mechanically raise the per-worker average earnings even if no individual's earnings changed.

The decomposition in Table A.14 confirms the composition channel as the operative mechanism. The decomposition is estimated on a sample locked to the earnings outcomes; the actual

[18] In the CoG three-wave estimation sample, mean annual earnings for part-time employees ($12,231) are approximately 24 percent of those for full-time employees ($50,740); in the annual panel the corresponding figures are $15,708 and $57,956 (about 27 percent).

increase in total per-worker earnings on that sample is about 1.0 percent in the CoG three-wave panel, modestly below the 1.7 percent all-outcome estimate, but the composition attribution is unchanged. In the pooled sample of all government types (Panel A), the composition counterfactual in the CoG three-wave panel is positive and precisely estimated at about 1.4 percent, while the wage counterfactual is economically small and indistinguishable from zero. The balanced annual panel tells the same story: the composition counterfactual is positive and statistically significant at about 1.0 percent, while the wage counterfactual is again small and insignificant. Thus, the positive earnings result that appears in Table 1 reflects the mechanical elevation of the per-worker average caused by the relative contraction of the part-time workforce and not by rising category-specific pay.

The decomposition by government type reinforces this interpretation. For counties and municipalities, the composition counterfactual is positive and statistically significant in both datasets (though mostly at the 10 percent level) — ranging from about 0.9 to 1.5 percent — while the wage counterfactual is uniformly small and insignificant. For special districts, neither counterfactual is statistically significant in either dataset, consistent with the null employment results for that type of government.

School districts offer somewhat mixed results. The composition counterfactual is significant in the CoG three-wave panel (about 2.3 percent) but falls short of significance in the balanced annual panel (about 1.6 percent), broadly consistent with the shift documented for counties and municipalities. The wage counterfactual, however, is positive and significant in the annual panel (a large +5.5 percent, $p<0.01$), but an event study, available on request, shows that this reflects pre-existing convergence: treated districts had relatively lower wages in the early 2010s, and that gradual catching-up continues smoothly through the post-*Janus* period without a visible break at 2019. The wage estimate is therefore best read as long-run convergence — potentially compounded by the national teacher shortage (Sutcher, Darling-Hammond, and Carver-Thomas, 2016; García and Weiss, 2019) — and not a *Janus* effect. Because the wage counterfactual is statistically significant, the convergence reading is provisional for this one estimate: I cannot fully rule out a *Janus* effect on school-district wages, though the event study points to pre-existing convergence rather than a break at 2019. The composition channel, which has flat pre-trends and is unaffected by this confound, points to the same FT/PT reallocation seen elsewhere.

C Additional Robustness Checks: Substitution, Fiscal Stress, and Union Density

This section collects three supplementary checks referenced in the main text: a direct test for part-time to full-time substitution, an examination of whether the part-time decline tracks pre-period fiscal stress, and a split by pre-period public-sector union density.

Table A.4 reports a triple-difference test of whether the decline in part-time employment reflects substitution toward full-time work rather than a net reduction in headcount. I interact Treated x Post with the z-scored 2017 part-time share of each unit, so the interaction measures whether governments that entered the period with more part-time positions experienced larger full-time gains. Substitution would imply a positive full-time-employment interaction. The estimated interaction is small and statistically insignificant in both panels (–0.013 in the Census of Governments and 0.009 in the annual panel), while total employment declines. Part-time positions were therefore shed rather than converted into full-time ones.

Table A.5 estimates the treatment effect within quartiles of pre-period fiscal stress, where stress is the negative of standardized log cash and deposits per employee measured in 2017, so that higher values denote greater illiquidity. If pandemic-era general-fund stress drove the part-time reductions, the decline should be largest in the most-stressed governments. It is not. In the Census of Governments the part-time decline appears in every quartile and is, if anything, largest in the middle of the distribution rather than at the top — about 5.0 percent in the least-stressed quartile, 10.3 percent in the third, and 7.6 percent among the most stressed — and a continuous specification finds that its interaction with fiscal stress is itself insignificant. In the annual panel the decline ranges from about 8.5 to 11.4 percent across the lower three quartiles and is statistically absent among the most stressed (0.6 percent). This pattern is difficult to reconcile with a fiscal-stress mechanism.

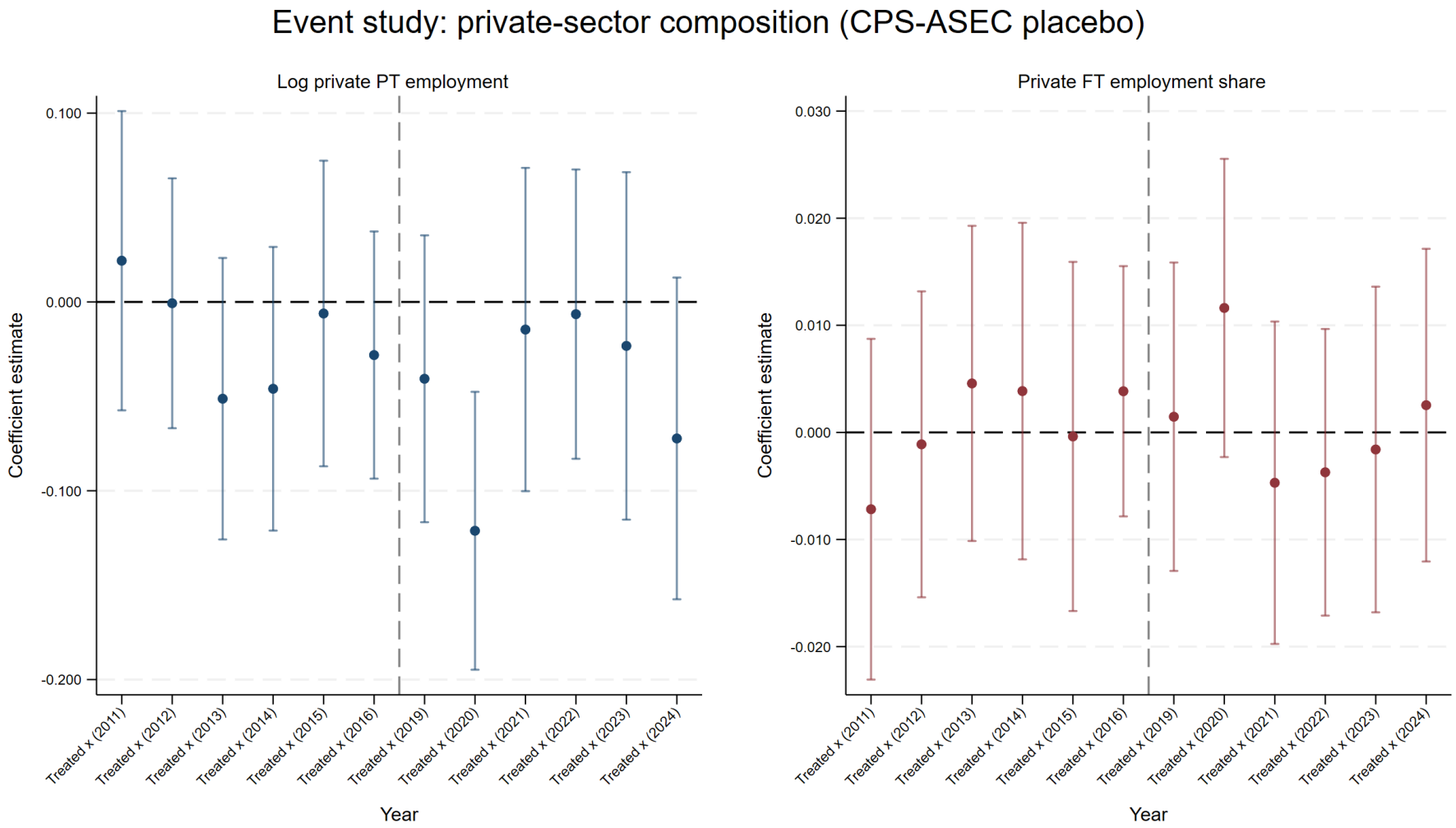
Finally, Table A.11 splits treated states at the median of 2017 public-sector union density, measured from the CPS, and compares each group to the full set of RTW control states. In the Census of Governments, the part-time decline is markedly larger among lower-density treated states (–10.6 and –9.3 percent under the two-way and trend-augmented specifications) than among higher-density treated states (–6.1 and –7.9 percent). The annual panel shows the same ordering: a decline of 8.3 and 7.1 percent among lower-density states versus 4.9 and 5.9 percent among higher-density states, with the coefficients in the higher-density states either statistically insignif-

icant or significant only at the 10 percent level.[19] This ordering is consistent with stronger unions being better able to resist the part-time adjustment, and with the mechanical incidence of the ruling, since lower-density unions relied more heavily on agency-fee revenue from non-members and were therefore more exposed to the loss of mandatory fees. Because the contrast between the two groups is directional rather than precisely estimated, and because it rests on the CPS density measure, I treat it as suggestive rather than dispositive.[20]

[19]The full-time share rises across both groups.

[20]This contrast relies on the CPS measure of public-sector union density, which Bagchi (2026, Appendix A) shows is structurally unable to detect the post-*Janus* shock, for three reasons. First, the bulk of the decline came from agency-fee payers ending their compulsory payments — but these workers had always reported as non-members in the CPS, so their shift from fee-payer to non-payer never registers as a loss of membership (this channel alone accounts for essentially the entire 2019 decline and at least three-quarters of the cumulative decline through 2021). Second, among full members who do stop paying, the reporting biases run the other way: respondents reluctant to admit free-riding, or who conflate bargaining-unit coverage with dues-paying membership, keep reporting as members, so the CPS overstates post-*Janus* membership. Third, the CPS's well-documented baseline misclassification of union status (about 2.7 percent; Card, 1996) is large enough to absorb the modest voluntary attrition that remains. Together with the small state-level samples underlying any state density estimate, these features make the CPS a poor proxy for agency-fee exposure; I therefore use it only to rank treated states by pre-period density, not as a precise measure.

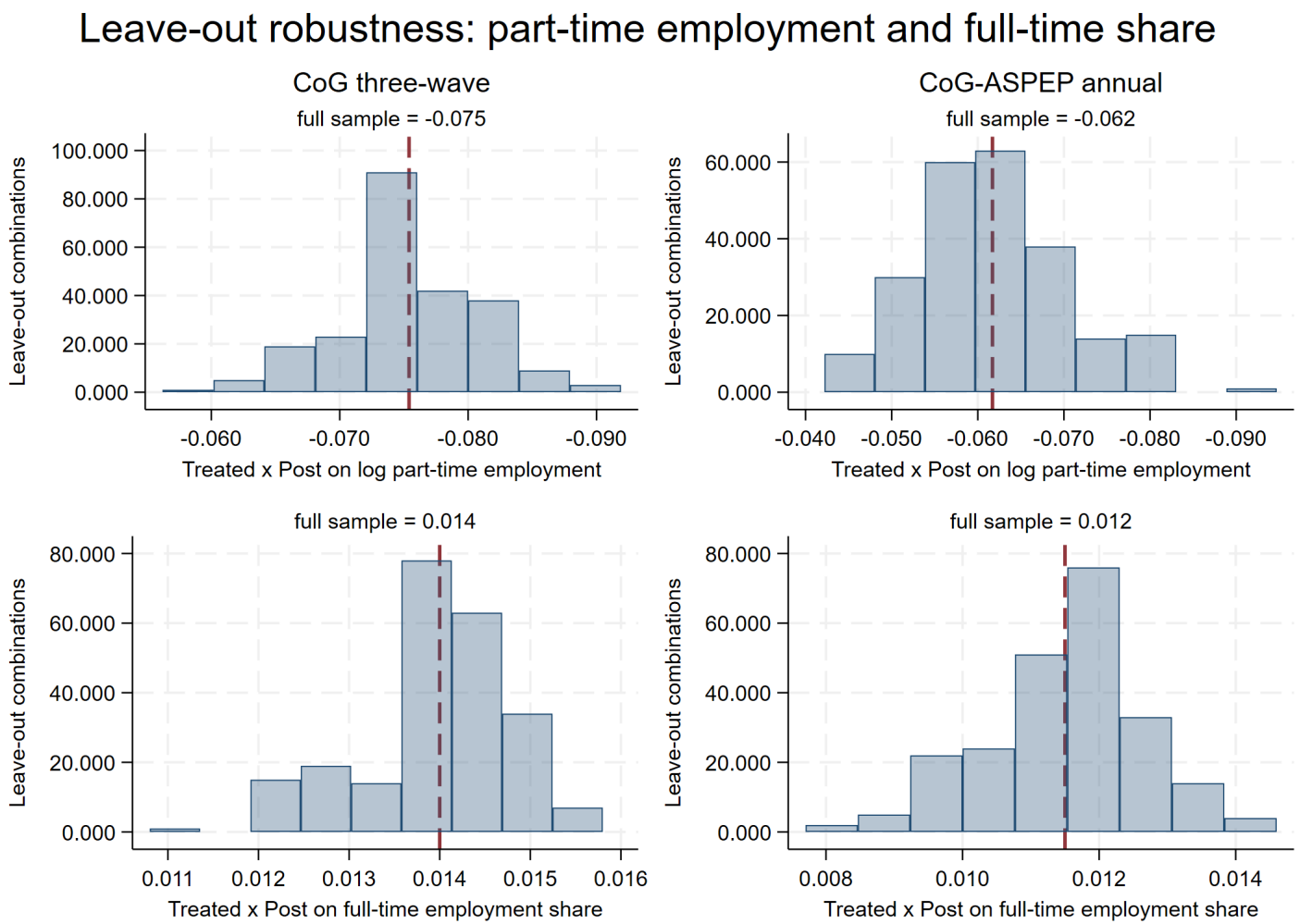
Figure A.1: Event study: Private-sector composition (CPS-ASEC placebo)



CPS-ASEC private-sector workers. 47 states (excluding CO, MO, WI); base year 2017 (2018 not sampled). State and year FE; 95% CI; SEs clustered by state.

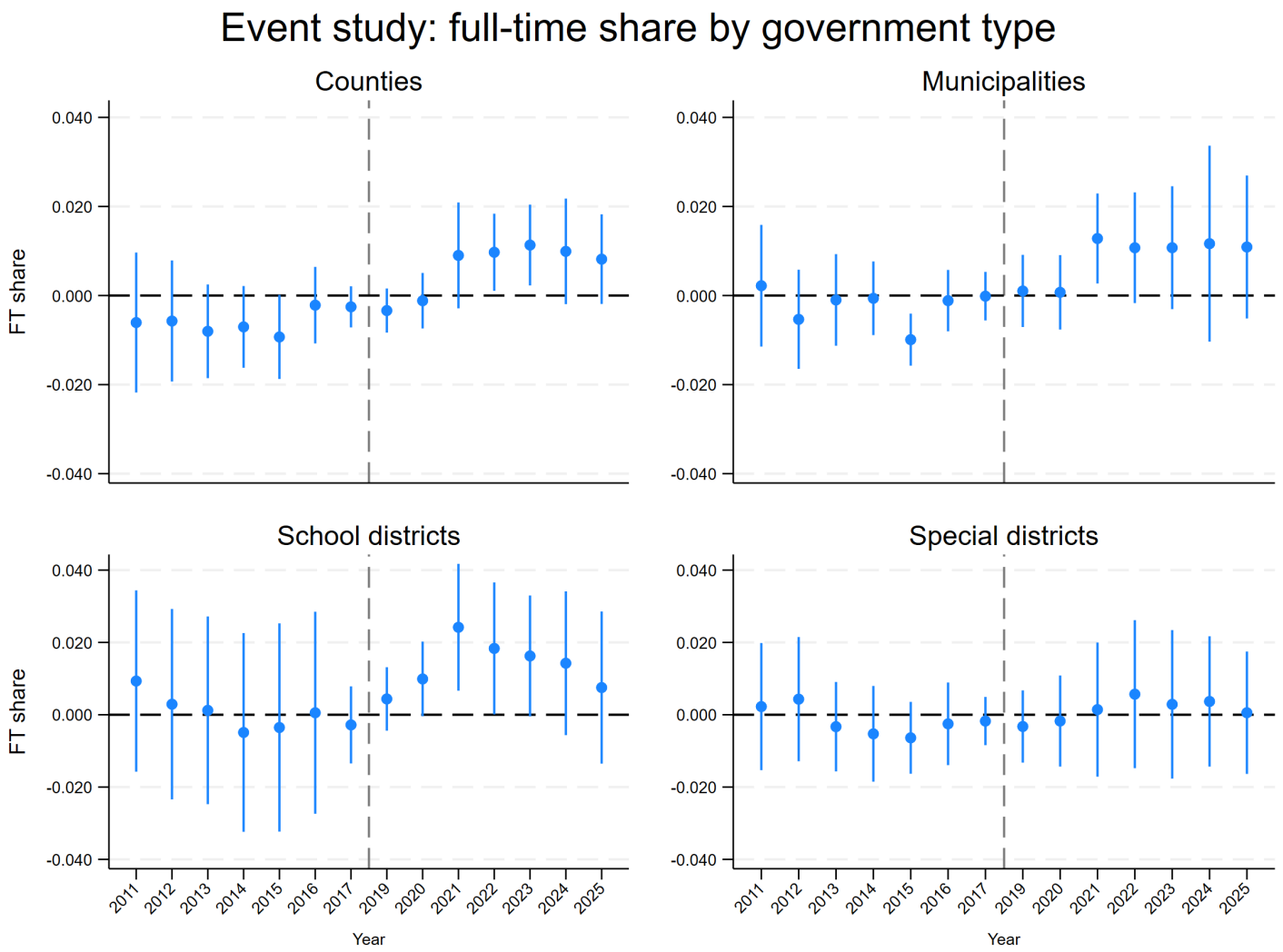
Notes: The dependent variables are the natural logarithm of private-sector part-time employment (Log Private PT Employment) and the private-sector full-time employment share (Private FT Share). The sample is built from the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), restricted to private-sector workers and aggregated to state-year cells, covering 47 states. All regressions are the baseline plain two-way fixed-effects specification, including state and year fixed effects, with no state-specific trends. Because the CPS-ASEC sample excludes 2018, each point is an event-study coefficient relative to the 2017 base year (the last pre-*Janus* wave), with 95 percent confidence intervals; standard errors are clustered by state. Part-time employment shows a significant dip in 2020 alone (coefficient −0.121, $p<0.01$); no other point in either panel is significant at the 5 percent level. The dashed vertical line marks June 2018 (*Janus* decision). The horizontal dashed line marks zero. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1. Regressions are unweighted.

Figure A.2: Leave-one-out and leave-two-out robustness of the part-time employment and full-time share estimates



Notes: The dependent variables are the natural logarithm of part-time employment (PT Empl.) and the share of full-time workers in total employment (FT Share), one per row. Each panel plots the distribution of the Treated x Post coefficient across 231 leave-out re-estimations — every leave-one-out (21) and leave-two-out (210); the full-sample estimate is the dashed line. The top row reports part-time employment and the bottom row the full-time share; the left column uses the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022) and the right column uses the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). In the part-time panels the horizontal axis is reversed so that larger declines appear on the right. The baseline specification includes government-unit and year fixed effects (plain two-way fixed effects), with no state-specific trends; standard errors are clustered by state. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Figure A.3: Event study: Full-time employment share by government type



Notes: The dependent variable is the share of full-time workers in total employment (FT Share), estimated separately for each of four government types: counties, municipalities, independent school districts, and special districts (one panel each). The sample is the CoG–ASPEP balanced annual panel (2011–2025), with 2018 as the omitted base year; pre-period coefficients (2011–2017) test for parallel pre-trends and post-period coefficients (2019–2025) measure the differential change in treated relative to control states following the *Janus* ruling. All regressions are the baseline plain two-way fixed-effects specification, including government-unit and year fixed effects, with no state-specific trends. Each point is an event-study coefficient relative to the 2018 base year, with 95 percent confidence intervals; standard errors are clustered by state. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.1: Summary statistics for the estimation sample by treatment status — CoG three-wave and CoG–ASPEP balanced panels

	Mean	Median	SD	N
Panel A: Treated states; CoG three-wave survey (2012, 2017, and 2022)				
Full-time employment	368.8	38	5440.7	16984
Part-time employment	132.9	20	1843.9	16984
Total employment	501.7	64	7023.8	16984
Full-time annual earnings	58727	56245	19721	16984
Part-time annual earnings	13720	12190	8996	16984
Overall earnings	41424	40370	18951	16984
Share of full-time employment	0.61	0.66	0.24	16984
Panel B: Control states; CoG three-wave survey (2012, 2017, and 2022)				
Full-time employment	372.4	38	3581.3	18838
Part-time employment	107.0	12	1382.9	18838
Total employment	479.5	54	4901.6	18838
Full-time annual earnings	43539	42095	12756	18838
Part-time annual earnings	10889	9774	6931	18838
Overall earnings	32466	32474	12933	18838
Share of full-time employment	0.67	0.73	0.24	18838
Panel C: Treated states; CoG–ASPEP balanced annual panel (2011–2025)				
Full-time employment	3093.2	518	18584.2	1421
Part-time employment	979.6	127	6300.8	1421
Total employment	4072.9	673	23978.2	1421
Full-time annual earnings	68326	65919	18030	1421
Part-time annual earnings	18264	16019	11078	1421
Overall earnings	55656	53277	17139	1421
Share of full-time employment	0.76	0.79	0.17	1421
Panel D: Control states; CoG–ASPEP balanced annual panel (2011–2025)				
Full-time employment	2301.2	435	10137.6	2249
Part-time employment	645.7	77	3954.5	2249
Total employment	2946.9	538	13915.7	2249
Full-time annual earnings	51404	49751	11638	2249
Part-time annual earnings	14093	12392	7630	2249
Overall earnings	43672	42422	11599	2249
Share of full-time employment	0.80	0.84	0.15	2249

Notes: Cells report the mean, median, and standard deviation (Mean, Median, SD), together with the number of observations (N), of full-time employment, part-time employment, and total employment (in persons); full-time, part-time, and overall annual earnings per worker (in dollars); and the share of full-time workers in total employment (FT Share, full-time over total employment). Panels A and B use the CoG three-wave panel (survey years 2012, 2017, and 2022) and Panels C and D use the CoG–ASPEP balanced annual panel (2011–2025), each evaluated in the 2017 cross-section, with the common estimation sample locked on the full-time share specification and intersected across all seven outcomes. Panels A and C report treated units (in states that permitted agency fees before Janus) and Panels B and D report control units (in right-to-work states); each panel pools all government types, including state governments. The sample covers 47 states; the exclusion of Colorado, Missouri, and Wisconsin and of townships follows Table 1.

Table A.2: Summary statistics for the estimation sample for the CoG three-wave survey (2012, 2017, and 2022)

	Mean	Median	SD	N
Panel A: Overall sample				
Full-time employment	370.7	38.0	4558.4	35822
Part-time employment	119.3	15.0	1618.0	35822
Total employment	490.0	58.0	6002.0	35822
Full-time annual earnings	50740	47685	18096	35822
Part-time annual earnings	12231	10758	8101	35822
Overall earnings	36713	35370	16680	35822
Share of full-time employment	0.64	0.69	0.24	35822
Panel B: Counties				
Full-time employment	854.7	198.5	3225.5	2732
Part-time employment	172.9	38.0	593.0	2732
Total employment	1027.6	242.0	3679.5	2732
Full-time annual earnings	44719	43099	11317	2732
Part-time annual earnings	13849	12510	6757	2732
Overall earnings	38879	37354	10952	2732
Share of full-time employment	0.81	0.83	0.12	2732
Panel C: Municipalities				
Full-time employment	173.7	15.0	3921.2	12763
Part-time employment	38.5	12.0	342.4	12763
Total employment	212.2	30.0	4253.1	12763
Full-time annual earnings	49670	46776	18951	12763
Part-time annual earnings	9986	8857	6754	12763
Overall earnings	32970	30842	17891	12763
Share of full-time employment	0.57	0.60	0.26	12763
Panel D: School Districts				
Full-time employment	397.6	145.0	1246.7	11541
Part-time employment	140.9	43.0	442.8	11541
Total employment	538.6	198.0	1583.3	11541
Full-time annual earnings	52013	48859	15645	11541
Part-time annual earnings	13701	12276	7726	11541
Overall earnings	40621	38491	11983	11541
Share of full-time employment	0.72	0.75	0.17	11541
Panel E: Special Districts				
Full-time employment	57.6	7.0	465.0	8739
Part-time employment	20.9	4.0	142.2	8739
Total employment	78.5	13.0	590.9	8739
Full-time annual earnings	52444	48972	20865	8739
Part-time annual earnings	13047	11082	9873	8739
Overall earnings	36279	32512	20067	8739
Share of full-time employment	0.59	0.62	0.26	8739

Notes: Cells report the mean, median, and standard deviation (Mean, Median, SD), together with the number of observations (N), of full-time employment, part-time employment, and total employment (in persons); full-time, part-time, and overall annual earnings per worker (in dollars); and the share of full-time workers in total employment (FT Share, full-time over total employment). The sample is the CoG three-wave panel (survey years 2012, 2017, and 2022), evaluated in the 2017 cross-section, with the common estimation sample locked on the full-time share specification and intersected across all seven outcomes. Panel A reports the overall sample (including state governments) and Panels B through E report counties, municipalities, school districts, and special districts, respectively. The sample covers 47 states; the exclusion of Colorado, Missouri, and Wisconsin and of townships follows Table 1.

Table A.3: Summary statistics for the estimation sample for the CoG–ASPEP balanced annual panel (2011–2025)

	Mean	Median	SD	N
Panel A: Overall sample				
Full-time employment	2607.9	468.5	14028.0	3670
Part-time employment	775.0	93.0	4997.3	3670
Total employment	3382.9	602.5	18478.9	3670
Full-time annual earnings	57956	54621	16637	3670
Part-time annual earnings	15708	13633	9343	3670
Overall earnings	48312	46013	15173	3670
Share of full-time employment	0.78	0.82	0.16	3670
Panel B: Counties				
Full-time employment	2421.7	894.0	5821.1	757
Part-time employment	455.3	118.0	1060.1	757
Total employment	2877.0	993.0	6608.1	757
Full-time annual earnings	52664	50976	12132	757
Part-time annual earnings	17209	15555	8164	757
Overall earnings	46974	45459	11262	757
Share of full-time employment	0.84	0.85	0.10	757
Panel C: Municipalities				
Full-time employment	1276.2	292.0	12001.1	1350
Part-time employment	197.9	64.0	1035.1	1350
Total employment	1474.1	369.0	13008.8	1350
Full-time annual earnings	61609	59342	16186	1350
Part-time annual earnings	12309	11089	6406	1350
Overall earnings	50456	49176	14751	1350
Share of full-time employment	0.78	0.82	0.15	1350
Panel D: School Districts				
Full-time employment	2666.0	1555.0	4063.8	769
Part-time employment	845.5	495.0	1429.1	769
Total employment	3511.5	2264.0	5070.7	769
Full-time annual earnings	55488	51327	15919	769
Part-time annual earnings	15746	14672	7190	769
Overall earnings	43365	41125	10147	769
Share of full-time employment	0.73	0.77	0.17	769
Panel E: Special Districts				
Full-time employment	440.4	134.0	1456.9	747
Part-time employment	109.2	22.0	441.7	747
Total employment	549.6	184.0	1851.8	747
Full-time annual earnings	58986	54819	20276	747
Part-time annual earnings	20313	16808	13740	747
Overall earnings	50893	47695	21416	747
Share of full-time employment	0.79	0.85	0.20	747

Notes: Cells report the mean, median, and standard deviation (Mean, Median, SD), together with the number of observations (N), of full-time employment, part-time employment, and total employment (in persons); full-time, part-time, and overall annualized payroll per worker (in dollars); and the share of full-time workers in total employment (FT Share). The sample is the CoG–ASPEP balanced annual panel (2011–2025), evaluated in the 2017 cross-section, with the common estimation sample locked on the full-time share specification and intersected across all seven outcomes. Panel A reports the overall sample (including state governments) and Panels B through E report counties, municipalities, school districts, and special districts, respectively. The sample covers 47 states; the exclusion of Colorado, Missouri, and Wisconsin and of townships follows Table 1.

Table A.4: Full-Time/Part-Time substitution test: Triple-difference estimates by pre-period part-time share

	(1)	(2)	(3)	(4)
Dependent variable	FT Empl.	Tot Empl.	FT Empl.	Tot Empl.
Panel A: Census of Governments three-wave panel				
Treated x Post	-0.0109	-0.0237	-0.0011	-0.0204**
	(0.0191)	(0.0148)	(0.0152)	(0.0094)
Treated x Post x pre-period	-0.0129	-0.0175	-0.0121	-0.0185*
part-time share	(0.0080)	(0.0120)	(0.0078)	(0.0108)
Number of observations	123695	123695	123695	123695
Number of clusters	47	47	47	47
Panel B: CoG–ASPEP balanced annual panel				
Treated x Post	-0.0024	-0.0107	0.0046	-0.0059
	(0.0164)	(0.0151)	(0.0114)	(0.0106)
Treated x Post x pre-period	0.0093	0.0066	0.0103	0.0055
part-time share	(0.0078)	(0.0114)	(0.0070)	(0.0106)
Number of observations	58414	58414	58414	58414
Number of clusters	47	47	47	47
State-specific pre-trends	No	No	Yes	Yes

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.) and the natural logarithm of total employment (Tot Empl.). Panel A uses the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panel B uses the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the plain-TWFE columns (columns 1 and 2) are the baseline and the state-trends columns (columns 3 and 4) add state-specific pre-period linear trends. The specification is a triple difference that interacts Treated x Post with the 2017 part-time share of the unit, standardized to mean zero and unit standard deviation across treated units (per SD); the reported terms are Treated x Post and Treated x Post x pre-period part-time share, while the post-by-share main interaction is included but not reported. Substitution of full-time for part-time positions would imply a positive coefficient on the part-time-share interaction in the full-time-employment columns; a coefficient indistinguishable from zero indicates that the part-time buffer is shed rather than replaced by full-time hiring. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.5: Part-Time decline by pre-period fiscal stress: Within-quartile difference-in-differences

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	Tot Empl.	FT Empl.	PT Empl.	Tot Empl.
Panel A: Census of Governments three-wave panel						
Q1 (least stressed)	-0.0142	-0.0496*	-0.0209	-0.0096	-0.0680**	-0.0249**
	(0.0131)	(0.0293)	(0.0132)	(0.0102)	(0.0316)	(0.0123)
Q2	-0.0175	-0.0810***	-0.0353**	-0.0064	-0.0879***	-0.0315***
	(0.0139)	(0.0270)	(0.0168)	(0.0107)	(0.0257)	(0.0114)
Q3	-0.0111	-0.1033***	-0.0434**	0.0049	-0.1070***	-0.0376***
	(0.0174)	(0.0312)	(0.0187)	(0.0133)	(0.0246)	(0.0127)
Q4 (most stressed)	0.0154	-0.0757**	-0.0250	0.0216	-0.0852***	-0.0254*
	(0.0196)	(0.0362)	(0.0184)	(0.0148)	(0.0293)	(0.0128)
Number of observations	92676	92676	92676	92676	92676	92676
Number of clusters	47	47	47	47	47	47
Panel B: CoG–ASPEP balanced annual panel						
Q1 (least stressed)	-0.0277	-0.0874**	-0.0377**	-0.0264*	-0.1121**	-0.0379***
	(0.0189)	(0.0409)	(0.0174)	(0.0145)	(0.0463)	(0.0136)
Q2	-0.0165	-0.1144**	-0.0325	-0.0063	-0.1136***	-0.0228
	(0.0177)	(0.0442)	(0.0205)	(0.0137)	(0.0414)	(0.0160)
Q3	-0.0132	-0.0852**	-0.0284	-0.0035	-0.0793**	-0.0204
	(0.0192)	(0.0388)	(0.0172)	(0.0153)	(0.0369)	(0.0138)
Q4 (most stressed)	0.0421	-0.0056	0.0060	0.0407**	-0.0008	0.0076
	(0.0261)	(0.0598)	(0.0201)	(0.0191)	(0.0530)	(0.0159)
Number of observations	52889	52889	52889	52889	52889	52889
Number of clusters	47	47	47	47	47	47
State-specific pre-trends	No	No	No	Yes	Yes	Yes

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), part-time employment (PT Empl.), and total employment (Tot Empl.). Panel A uses the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panel B uses the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the plain-TWFE columns (columns 1-3) are the baseline and the state-trends columns (columns 4-6) add state-specific pre-period linear trends. The table reports a within-quartile difference-in-differences in which each cell is the treated-versus-control Treated x Post effect estimated within the indicated quartile of pre-period fiscal stress; stress is the negative of the standardized log of cash and deposits per employee measured in 2017 (higher values denote greater illiquidity), quartile cutoffs are taken among treated governments, and Q1 (Q4) denotes the least (most) stressed governments. The post-by-quartile terms are included but not reported. A common estimation sample is locked across the three outcomes using the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.6: Border and matched difference-in-differences robustness: per-worker earnings

	(1)	(2)	(3)	(4)
Dependent variable	FT Earnings	PT Earnings	FT Earnings	PT Earnings
Panel A1: Using a border-state matching approach (three-wave CoG)				
Treated x Post	0.0160	-0.0155	0.0097	-0.0089
	(0.0154)	(0.0137)	(0.0105)	(0.0122)
Number of observations	65385	65385	65385	65385
Number of clusters	23	23	23	23
Panel A2: Using log of long-term debt and cash reserves per employee for matching (three-wave CoG)				
Treated x Post	0.0072	-0.0078	0.0010	-0.0047
	(0.0112)	(0.0118)	(0.0084)	(0.0118)
Number of observations	53429	53429	53429	53429
Number of clusters	47	47	47	47
Panel B1: Using a border-state matching approach (CoG–ASPEP balanced annual panel)				
Treated x Post	0.0077	0.0342	0.0046	0.0338
	(0.0120)	(0.0297)	(0.0102)	(0.0259)
Number of observations	6336	6336	6336	6336
Number of clusters	23	23	23	23
Panel B2: Using log of long-term debt and cash reserves per employee for matching (CoG–ASPEP panel)				
Treated x Post	0.0015	0.0248	-0.0013	0.0209
	(0.0096)	(0.0263)	(0.0081)	(0.0241)
Number of observations	32867	32867	32867	32867
Number of clusters	47	47	47	47
State-specific pre-trends	No	No	Yes	Yes

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of annualized full-time payroll per worker (FT Earnings) and part-time payroll per worker (PT Earnings); columns 1 and 3 report FT Earnings and columns 2 and 4 report PT Earnings. Panels A1 and A2 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panels B1 and B2 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the plain-TWFE columns (columns 1-2) are the baseline and the state-trends columns (columns 3-4) add state-specific pre-period linear trends. This table is the earnings companion to Table 3: Panels A1 and B1 use the state-border-pair design restricting to government units in border states paired across the treated/control state line and adding state-border-pair segment fixed effects; Panels A2 and B2 use 1-nearest-neighbor Mahalanobis matched difference-in-differences exact on government type jointly on 2017 long-term debt and cash reserves per employee. For the border panels (A1 and B1) inference uses the Webb wild-cluster bootstrap (999 replications; seed 12345) and the reported standard error is inverted from that bootstrap test; the matched panels (A2 and B2) cluster standard errors by state. The regression sample in each panel is the set of observations with non-missing values for both earnings outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.7: Robustness check: Estimates of the impact of *Janus* on employment and earnings using IHS transformation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	FT Empl.	PT Empl.	Tot Empl.	FT Earnings	PT Earnings	Overall Earnings	FT Share
Panel A1: Using the three-wave Census of Governments (CoG) with TWFE							
Treated x Post	-0.0066	-0.0722***	-0.0274	0.0008	-0.0041	0.0174*	0.0091**
	(0.0156)	(0.0224)	(0.0177)	(0.0100)	(0.0111)	(0.0090)	(0.0035)
Panel A2: Using the three-wave Census of Governments (CoG) with TWFE and state-specific pre-trends							
Treated x Post	0.0016	-0.0715***	-0.0246**	-0.0049	0.0051	0.0178**	0.0112***
	(0.0115)	(0.0160)	(0.0112)	(0.0075)	(0.0101)	(0.0078)	(0.0035)
Number of observations	152060	152060	152060	102241	102241	102241	152060
Number of clusters	47	47	47	47	47	47	47
Panel B1: Using CoG–ASPEP balanced annual panel (2011–2025) with TWFE							
Treated x Post	-0.0000	-0.0633*	-0.0157	-0.0014	0.0224	0.0100	0.0100**
	(0.0172)	(0.0358)	(0.0144)	(0.0091)	(0.0201)	(0.0098)	(0.0041)
Panel B2: Using CoG–ASPEP balanced annual panel (2011–2025) with TWFE and state-specific pre-trends							
Treated x Post	0.0054	-0.0634**	-0.0119	-0.0044	0.0211	0.0081	0.0105***
	(0.0116)	(0.0277)	(0.0099)	(0.0076)	(0.0180)	(0.0083)	(0.0039)
Number of observations	58431	58431	58431	55029	55029	55029	58431
Number of clusters	47	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the inverse hyperbolic sine (IHS) transformation of full-time employment (FT Empl.), part-time employment (PT Empl.), and total employment (Tot Empl.); and the IHS transformation of annualized full-time payroll per worker (FT Earnings), part-time payroll per worker (PT Earnings), and overall payroll per worker (Overall Earnings); and the share of full-time workers in total employment (FT Share). Panels A1 and A2 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panels B1 and B2 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; Panels A1 and B1 are the baseline plain two-way fixed-effects specification, and Panels A2 and B2 add state-specific pre-period linear trends. The IHS transformation, $IHS(x) = ln(x + sqrt(x^2 + 1))$, is defined at zero and therefore retains the zero-employment observations that the log specification drops; the full-time share is likewise defined for these units, since it requires only positive total employment. As a result — and unlike the other tables, where the sample is common to all columns — observation counts differ by column within each panel. The three employment columns and the full-time share column include these zero-employment units, giving the larger counts (152,060 in the Census of Governments panel and 58,431 in the annual panel); per-worker earnings are undefined at zero headcount, so the three earnings columns are estimated on the positive-employment sample used elsewhere in the paper — identical to Table 1 — giving the smaller counts (102,241 and 55,029). Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.8: Varying the criteria for inclusion in the CoG–ASPEP panel

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	FT Share	FT Empl.	PT Empl.	FT Share
Panel A. Units present in all 15 of 15 years (units = 3829; obs = 55029)						
Treated x Post	-0.0004	-0.0617*	0.0115***	0.0044	-0.0635**	0.0113***
	(0.0172)	(0.0312)	(0.0042)	(0.0117)	(0.0263)	(0.0040)
Panel B. Units present in at least 14 of 15 years (units = 3845; obs = 55196)						
Treated x Post	-0.0008	-0.0600*	0.0112**	0.0044	-0.0622**	0.0112***
	(0.0172)	(0.0309)	(0.0042)	(0.0116)	(0.0262)	(0.0039)
Panel C. Units present in at least 13 of 15 years (units = 4517; obs = 63400)						
Treated x Post	-0.0022	-0.0550*	0.0103**	0.0026	-0.0581**	0.0103***
	(0.0174)	(0.0297)	(0.0040)	(0.0119)	(0.0233)	(0.0035)
Panel D. Units present in at least 12 of 15 years (units = 5208; obs = 71162)						
Treated x Post	0.0020	-0.0500*	0.0099**	0.0038	-0.0567**	0.0103***
	(0.0170)	(0.0289)	(0.0039)	(0.0117)	(0.0235)	(0.0036)
Panel E. Units present in at least 11 of 15 years (units = 5801; obs = 77297)						
Treated x Post	0.0015	-0.0542*	0.0106**	0.0028	-0.0605**	0.0105***
	(0.0174)	(0.0308)	(0.0042)	(0.0118)	(0.0237)	(0.0037)
Panel F. Units present in at least 10 of 15 years (units = 6596; obs = 84578)						
Treated x Post	0.0036	-0.0547*	0.0109**	0.0047	-0.0630***	0.0111***
	(0.0175)	(0.0304)	(0.0041)	(0.0119)	(0.0233)	(0.0037)
Panel G. Units present in at least 9 of 15 years (units = 8018; obs = 96675)						
Treated x Post	0.0094	-0.0504	0.0118***	0.0069	-0.0620***	0.0119***
	(0.0188)	(0.0302)	(0.0040)	(0.0130)	(0.0223)	(0.0037)
Panel H. Units present in at least 8 of 15 years (units = 8920; obs = 103408)						
Treated x Post	0.0111	-0.0469	0.0116***	0.0078	-0.0605***	0.0118***
	(0.0182)	(0.0303)	(0.0040)	(0.0126)	(0.0218)	(0.0035)

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), the natural logarithm of part-time employment (PT Empl.), and the share of full-time workers in total employment (FT Share). All regressions use the CoG–ASPEP annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the plain-TWFE columns (columns 1-3) are the baseline and the state-trends columns (columns 4-6) add state-specific pre-period linear trends. Each panel relaxes the balanced-panel inclusion requirement to units present in at least k of the 15 years, with Panel A ($k = 15$) the baseline balanced sample and successive panels lowering k to 8; within each panel the regression sample is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.9: Reconciling the census and the balanced CoG–ASPEP panel under common three-wave timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	FT Empl.	PT Empl.	Tot Empl.	FT Earnings	PT Earnings	Overall Earnings	FT Share
Panel A1: CoG three-wave; TWFE (N = 102241)							
Treated x Post	-0.0015	-0.0754***	-0.0282*	0.0008	-0.0041	0.0174*	0.0140***
	(0.0165)	(0.0227)	(0.0155)	(0.0100)	(0.0111)	(0.0090)	(0.0039)
Panel A2: CoG three-wave; TWFE + State-specific trends (N = 102241)							
Treated x Post	0.0088	-0.0838***	-0.0262***	-0.0049	0.0051	0.0178**	0.0175***
	(0.0119)	(0.0175)	(0.0093)	(0.0075)	(0.0101)	(0.0078)	(0.0038)
Panel B1: CoG–ASPEP balanced (three waves only); TWFE (N = 10888)							
Treated x Post	-0.0042	-0.0790**	-0.0273*	-0.0041	0.0328	0.0120	0.0147***
	(0.0170)	(0.0327)	(0.0155)	(0.0090)	(0.0208)	(0.0098)	(0.0046)
Panel B2: CoG–ASPEP balanced (three waves only); TWFE + State-specific trends (N = 10888)							
Treated x Post	-0.0024	-0.0850**	-0.0244**	-0.0072	0.0346*	0.0095	0.0139***
	(0.0114)	(0.0328)	(0.0119)	(0.0074)	(0.0196)	(0.0086)	(0.0050)
Panel C1: Rest of CoG (units not in the balanced panel); TWFE (N = 91353)							
Treated x Post	-0.0016	-0.0775***	-0.0286*	0.0017	-0.0091	0.0179*	0.0140***
	(0.0168)	(0.0236)	(0.0159)	(0.0105)	(0.0118)	(0.0094)	(0.0040)
Panel C2: Rest of CoG (units not in the balanced panel); TWFE + State-specific trends (N = 91353)							
Treated x Post	0.0096	-0.0863***	-0.0271***	-0.0044	0.0015	0.0190**	0.0181***
	(0.0124)	(0.0182)	(0.0095)	(0.0079)	(0.0106)	(0.0082)	(0.0040)
Panel D1: CoG–ASPEP balanced annual; TWFE (N = 55029)							
Treated x Post	-0.0004	-0.0617*	-0.0183	-0.0014	0.0226	0.0100	0.0115***
	(0.0172)	(0.0312)	(0.0149)	(0.0091)	(0.0201)	(0.0098)	(0.0042)
Panel D2: CoG–ASPEP balanced annual; TWFE + State-specific trends (N = 55029)							
Treated x Post	0.0044	-0.0635**	-0.0137	-0.0044	0.0209	0.0081	0.0113***
	(0.0117)	(0.0263)	(0.0101)	(0.0076)	(0.0181)	(0.0083)	(0.0040)

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), part-time employment (PT Empl.), and total employment (Tot Empl.); the natural logarithm of annualized full-time payroll per worker (FT Earnings), part-time payroll per worker (PT Earnings), and overall payroll per worker (Overall Earnings); and the share of full-time workers in total employment (FT Share). The table reconciles the two datasets under a common three-wave timing. Panels A1 through C2 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022): Panels A1 and A2 are the full CoG three-wave panel; Panels B1 and B2 restrict the CoG–ASPEP balanced units to the three census waves (the selected units under census timing); and Panels C1 and C2 are the rest of CoG (census units never retained in the balanced panel). Panels D1 and D2 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; the odd-numbered panels (A1, B1, C1, and D1) are the baseline plain two-way fixed-effects specification, and the even-numbered panels (A2, B2, C2, and D2) add state-specific pre-period linear trends. The structure decomposes the gap between the two datasets: holding timing fixed, A1 versus B1 isolates sample selection and C1 provides the non-selected census complement, while holding the sample fixed, B1 versus D1 isolates frequency. Within each panel the regression sample is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.10: Difference-in-differences estimates of the impact of *Janus* on employment using TWFE + State-specific trends — Split by type of government

	Census of Governments			CoG–ASPEP		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	FT Empl.	PT Empl.	FT Share	FT Empl.	PT Empl.	FT Share
Panel A: Counties						
Treated x Post	-0.0075	-0.1280***	0.0127***	-0.0192	-0.0854**	0.0079**
	(0.0115)	(0.0298)	(0.0038)	(0.0151)	(0.0332)	(0.0039)
Number of observations	8171	8171	8171	11304	11304	11304
Number of clusters	45	45	45	45	45	45
Panel B: Municipalities						
Treated x Post	-0.0020	-0.0656**	0.0132**	-0.0032	-0.0899*	0.0106*
	(0.0140)	(0.0285)	(0.0060)	(0.0152)	(0.0503)	(0.0062)
Number of observations	37453	37453	37453	20309	20309	20309
Number of clusters	47	47	47	47	47	47
Panel C: School Districts						
Treated x Post	0.0036	-0.0687***	0.0168***	0.0171	0.0073	0.0142
	(0.0120)	(0.0246)	(0.0041)	(0.0145)	(0.0594)	(0.0086)
Number of observations	33621	33621	33621	11462	11462	11462
Number of clusters	44	44	44	41	41	41
Panel D: Special Districts						
Treated x Post	0.0136	-0.0391	0.0088	0.0210	-0.0486	0.0090
	(0.0191)	(0.0263)	(0.0056)	(0.0177)	(0.0430)	(0.0067)
Number of observations	22855	22855	22855	11249	11249	11249
Number of clusters	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of full-time employment (FT Empl.), the natural logarithm of part-time employment (PT Empl.), and the share of full-time workers in total employment (FT Share). Columns 1-3 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); columns 4-6 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; every column adds state-specific pre-period linear trends, so the table reports the trends-augmented specification only and has no plain two-way fixed-effects columns. Each panel reports a separate government type, estimated on its own subsample: Panel A counties, Panel B municipalities, Panel C independent school districts, and Panel D special districts. The regression sample in each category-by-dataset cell is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1; state governments and townships are excluded. Regressions are unweighted.

Table A.11: Heterogeneity by pre-period union density: Treated non-RTW states versus RTW control states

	(1)	(2)
Dependent variable	PT Empl.	FT Share
Panel A: Census of Governments three-wave panel		
Panel A1: Lower-density treated states		
(a) Two-way fixed effects	-0.1061***	0.0130***
	(0.0200)	(0.0037)
(b) Adding state pre-trends	-0.0931***	0.0148***
	(0.0170)	(0.0051)
Number of observations	69259	69259
Number of clusters	37	37
Panel A2: Higher-density treated states		
(a) Two-way fixed effects	-0.0611**	0.0144***
	(0.0268)	(0.0045)
(b) Adding state pre-trends	-0.0794***	0.0187***
	(0.0210)	(0.0042)
Number of observations	86665	86665
Number of clusters	36	36
Panel B: CoG–ASPEP balanced annual panel		
Panel B1: Lower-density treated states		
(a) Two-way fixed effects	-0.0830*	0.0080
	(0.0442)	(0.0050)
(b) Adding state pre-trends	-0.0705**	0.0074*
	(0.0320)	(0.0039)
Number of observations	41882	41882
Number of clusters	37	37
Panel B2: Higher-density treated states		
(a) Two-way fixed effects	-0.0487	0.0137**
	(0.0349)	(0.0057)
(b) Adding state pre-trends	-0.0592*	0.0138**
	(0.0342)	(0.0054)
Number of observations	46864	46864
Number of clusters	36	36

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Notes: The dependent variables are the natural logarithm of part-time employment (PT Empl.) and the share of full-time workers in total employment (FT Share). Panel A uses the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); Panel B uses the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; within each density block, the row labeled (a) Two-way fixed effects is the baseline plain two-way fixed-effects specification and the row labeled (b) Adding state pre-trends adds state-specific pre-period linear trends. Each panel reports a genuine subsample split on pre-period public-sector union density: treated states are partitioned at the median of 2017 public-sector union density among treated states into lower-density and higher-density groups, and each group is compared against the common control group of all right-to-work states. The regression sample is the set of observations with non-missing values for all seven outcomes, determined by the full-time share specification, so that all-part-time units do not enter even though full-time employment is not a displayed outcome. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.

Table A.12: The impact of *Janus* on earnings, hourly wages, and benefits using the CPS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable – Earnings (as-is in cols. (1) – (3)); winsorized (in cols. (4) – (6))						
Treated x Post	0.0197	0.0138	0.0122	0.0163	0.0111	0.0098
	(0.0138)	(0.0142)	(0.0120)	(0.0111)	(0.0109)	(0.0093)
Number of observations	98787	98787	98787	98787	98787	98787
Number of clusters	47	47	47	47	47	47
Panel B: Dependent variable – Wages (as-is in cols. (1) – (3)); winsorized (in cols. (4) – (6))						
Treated x Post	0.0089	0.0052	0.0050	0.0102	0.0068	0.0066
	(0.0109)	(0.0107)	(0.0105)	(0.0091)	(0.0087)	(0.0086)
Number of observations	98789	98789	98789	98789	98789	98789
Number of clusters	47	47	47	47	47	47
Panel C: Dependent variable – Health insurance coverage (LPM in cols. (1) – (3); probit in cols. (4) – (6))						
Treated x Post	0.0022	0.0011	0.0014	0.0046	0.0058	0.0105
	(0.0124)	(0.0126)	(0.0120)	(0.0430)	(0.0446)	(0.0461)
Number of observations	101521	101521	101521	101521	101521	101521
Number of clusters	47	47	47	47	47	47
Panel D: Dependent variable – Pension offered (LPM in cols. (1) – (3); probit in cols. (4) – (6))						
Treated x Post	0.0182	0.0172	0.0174	0.0423	0.0388	0.0386
	(0.0123)	(0.0116)	(0.0121)	(0.0355)	(0.0351)	(0.0368)
Number of observations	101521	101521	101521	101521	101521	101521
Number of clusters	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variables are the natural logarithm of annual earnings (Panel A) and the natural logarithm of the hourly wage (Panel B), and two indicators for non-wage compensation — whether the employer offers health insurance coverage (Panel C) and whether a pension is offered (Panel D). The data are individual-level records from the Current Population Survey Annual Social and Economic Supplement (CPS-ASEC), restricted to public-sector (state or local government) workers, with Treated x Post equal to one for workers in treated states from 2019 on. All regressions include state and year fixed effects; every column is the baseline plain two-way fixed-effects specification with no state-specific trends. Within each set of coefficients, three covariate specifications are reported: no controls (columns 1 and 4), with demographic controls (columns 2 and 5), and with demographic controls plus a full-time indicator (columns 3 and 6). Demographic controls are female, education dummies, age, and age squared. In Panels A and B, columns 1-3 use the outcome as-is and columns 4-6 winsorize it at the 5th and 95th percentiles; in Panels C and D, columns 1-3 report linear-probability estimates (LPM) and columns 4-6 report probit estimates. The regression sample within each panel is the set of observations non-missing for the outcome and all covariates, determined by the full-covariate specification. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1. Regressions are unweighted.

Table A.13: Public-Sector Employment and Earnings: Evidence from the BLS Quarterly Census of Employment and Wages (QCEW)

	(1)	(2)	(3)	(4)
Panel A: Log public-sector employment				
Dependent variable	Employment at the state level		Employment at the local level	
Treated x Post	-0.0259	-0.0139	-0.0106	-0.0133*
	(0.0220)	(0.0141)	(0.0129)	(0.0077)
Number of observations	658	658	658	658
Number of clusters	47	47	47	47
Panel B: Log average annual pay				
	Earnings of state workers		Earnings of local workers	
Treated x Post	0.0272	0.0226	0.0019	-0.0047
	(0.0201)	(0.0177)	(0.0117)	(0.0103)
Number of observations	658	658	658	658
Number of clusters	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.
Notes: The dependent variable is the natural logarithm of annual average public-sector employment (Panel A) and the natural logarithm of average annual pay, computed as the average weekly wage times 52 (Panel B); within each panel, columns 1 and 2 cover state government (Employment/Earnings at the state level) and columns 3 and 4 cover local government (Employment/Earnings at the local level). Estimates come from a state-year panel built from the BLS Quarterly Census of Employment and Wages (QCEW) annual singlefile releases for 2011–2024, with Treated x Post equal to one from 2019 on. All regressions include state and year fixed effects; the plain-TWFE columns (columns 1 and 3) are the baseline and the state-trends columns (columns 2 and 4) add state-specific pre-period linear trends. For each year the statewide ownership-total record (aggregation level 51, industry code 10) is retained separately for state government (ownership code 2) and local government (ownership code 3), the only aggregation level unsuppressed for all states; observations with zero reported employment or wages are dropped. Treatment status and the exclusion of Colorado, Missouri, and Wisconsin follow Table 1. Regressions are unweighted.

Table A.14: Decomposition of earnings increase — compositional counterfactual (holding wages steady) and wage counterfactual (holding composition steady)

	(1)	(2)	(3)	(4)	(5)	(6)
	Actual change in earnings	Composition effect	Wages effect	Actual change in earnings	Composition effect	Wages effect
	Panel A1: Overall sample – CoG dataset			Panel A2: Overall sample – CoG–ASPEP dataset		
Treated x Post	0.0100	0.0143***	-0.0031	0.0072	0.0095**	-0.0003
	(0.0102)	(0.0050)	(0.0082)	(0.0101)	(0.0040)	(0.0091)
Number of observations	151130	151130	151130	58426	58426	58426
Number of clusters	47	47	47	47	47	47
	Panel B1: Counties – CoG dataset			Panel B2: Counties – CoG–ASPEP dataset		
Treated x Post	0.0077	0.0151***	-0.0019	-0.0009	0.0087*	-0.0103
	(0.0099)	(0.0040)	(0.0083)	(0.0120)	(0.0047)	(0.0108)
Number of observations	8340	8340	8340	11415	11415	11415
Number of clusters	45	45	45	45	45	45
	Panel C1: Municipalities – CoG dataset			Panel C2: Municipalities – CoG–ASPEP dataset		
Treated x Post	-0.0008	0.0117*	-0.0027	-0.0101	0.0109*	-0.0169
	(0.0194)	(0.0068)	(0.0164)	(0.0123)	(0.0059)	(0.0145)
Number of observations	50997	50997	50997	20580	20580	20580
Number of clusters	47	47	47	47	47	47
	Panel D1: School districts – CoG dataset			Panel D2: School districts – CoG–ASPEP dataset		
Treated x Post	0.0310**	0.0226***	0.0098	0.0673***	0.0155	0.0545***
	(0.0142)	(0.0050)	(0.0142)	(0.0197)	(0.0094)	(0.0182)
Number of observations	36782	36782	36782	11669	11669	11669
Number of clusters	44	44	44	41	41	41
	Panel E1: Special districts – CoG dataset			Panel E2: Special districts – CoG–ASPEP dataset		
Treated x Post	0.0301	0.0079	0.0141	-0.0125	0.0002	-0.0124
	(0.0205)	(0.0084)	(0.0181)	(0.0137)	(0.0070)	(0.0121)
Number of observations	54870	54870	54870	14057	14057	14057
Number of clusters	47	47	47	47	47	47

Standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Notes: The dependent variable in every column is the natural logarithm of total annualized payroll per worker; each set of three columns decomposes the post-*Janus* change in this outcome following Lemieux (2006) into the actual change in earnings ("Actual change in earnings"), a composition counterfactual that holds per-worker wages fixed at their 2017 levels while the full-time/part-time mix varies ("Composition effect"), and a wage counterfactual that holds the 2017 full-time/part-time mix fixed while wages vary ("Wages effect"). Columns 1-3 use the CoG three-wave panel (survey years 2012, 2017, and 2022; Treated x Post equals one in 2022); columns 4-6 use the CoG–ASPEP balanced annual panel (2011–2025; Treated x Post equals one from 2019 on). All regressions include government-unit and year fixed effects; every column is the baseline plain two-way fixed-effects specification with no state-specific trends. Panels A1 and A2 report the overall sample and Panels B1/B2, C1/C2, D1/D2, and E1/E2 report counties, municipalities, school districts, and special districts separately, with the suffix denoting the dataset (1 = CoG, columns 1-3; 2 = CoG–ASPEP, columns 4-6); within each panel the regression sample is the set of observations with non-missing values for all three decomposition outcomes, determined by the total-earnings specification. Treatment status and the exclusion of Colorado, Missouri, Wisconsin, and townships follow Table 1. Regressions are unweighted.