Tying Reimbursement to Worker Pay: Wage Mandates and Workforce Adjustment in Long-Term Care

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Abstract

Long-term care systems face a common dilemma: low wages hinder recruitment and retention, but mandating higher pay may raise provider costs and reduce formal-care supply. We study a novel German payment rule that, beginning in 2022, conditioned providers' eligibility for public long-term care insurance reimbursement on paying collectively agreed wages or meeting state- and qualification-specific customary-pay benchmarks. Its bite varied with benchmarks and pre-reform wages, generating sharply different, predetermined exposure. We link newly collected benchmark data to administrative employer–employee records covering more than 500,000 nurses and 17,000 nursing-home and home-care establishments. A 10-log-point larger pre-reform benchmark gap predicts 2.3 percent greater growth in daily earnings during 2022–24 and 2.9 percent among skilled full-time nurses. No comparable gradient appears in pre-reform placebo growth or among hospital nurses. Counties with larger gaps show no detectable decline in regular nursing employment or establishment counts; at the establishment level, greater exposure is associated with lower full-time shares and less reliance on workers new to the establishment. Competitor-market exposure strongly predicts local outside-wage growth, with suggestive evidence of modest additional pass-through. Thus, tying public reimbursement to worker pay can raise earnings in low-wage care without a detectable immediate contraction in the formal-care workforce, while changing how providers organize employment.

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1 Introduction

Long-term care systems face an increasingly binding workforce constraint. Population aging is raising demand for formal care just as providers in many countries struggle to recruit and retain workers. The constraint is especially acute because long-term care (LTC) is produced locally and is highly labor intensive: expanding nursing-home or home-care services requires workers to provide hands-on care. Yet LTC jobs are demanding, predominantly held by women, and often pay less than clinically related jobs in hospitals. Low pay may contribute to turnover, vacancies, and unstable care teams, with consequences for both workers and care recipients (Shen et al., 2023; Ruffini, 2024). Raising pay could therefore improve recruitment and retention. But the cost does not disappear. If regulated reimbursement fails to accommodate higher labor costs, providers may reduce staffing, reorganize employment, restrict service provision, or exit; if costs are passed through, they must instead be borne by social insurance, taxpayers, or care recipients. Wage policy in LTC is therefore also payment policy.

Public payers have used several instruments to address this problem. U.S. Medicaid programs, for example, have adopted wage-pass-through provisions, payroll subsidies, and payments tied to staffing. Existing evidence shows that wage pass-throughs can increase direct-care staffing (Feng et al., 2010; Foster and Lee, 2015), that staffing-contingent payments can expand employment when unrestricted rate increases do not (Gandhi et al., 2024), and that payroll subsidies may raise staffing while changing which residents providers admit (Hegland, 2025). A related literature uses statutory minimum-wage reforms to study the LTC sector. These policies raise earnings and often produce limited employment responses in care homes (Machin et al., 2003; Vadean and Allan, 2021); in U.S. nursing homes, higher minimum wages also increase worker retention and improve resident outcomes (Ruffini, 2024). Together, these studies show that both the level and the design of public support for care labor matter. Much less is known about a different instrument: conditioning a provider's participation in a publicly regulated care market directly on the wages it pays. Nor is it clear whether such a rule affects only providers whose own wages fall below the standard or also propagates through the local outside options of workers.

This paper studies such a policy. Germany's 2022 Healthcare Development Act (*Gesundheitsver-*

sorgungsweiterentwicklungsgesetz, GVWG) linked participation in the statutory LTC insurance market to worker remuneration. Beginning on September 1, 2022, both residential and home-care providers could remain eligible for reimbursement only if they satisfied one of several wage requirements. Providers bound by a collective agreement or a church-specific wage regime had to pay accordingly. Providers outside these regimes could orient their pay to an applicable collective agreement or ensure that average remuneration within each qualification group met at least the customary regional level (*regional übliches Entlohnungsniveau*, rüE). These customary-pay benchmarks are calculated from collectively agreed wages and vary across Germany's 16 federal states and three worker qualification groups. A companion rule made compliant wage costs recognizable in rate negotiations: collectively agreed wages could not be rejected as uneconomic, and for non-covered providers the same protection generally extended to remuneration up to 10 percent above the regional benchmark. The reform thus combined a wage condition for provider participation with a cost-recognition rule in regulated rate negotiations.

This design differs from both a conventional minimum wage and a pure provider subsidy. It did not impose a single statutory rate on all workers or transfer an unrestricted payment to all providers. Instead, a national participation rule imported collectively bargained wages into a fragmented sector with sharply different pre-reform wage practices. The same state-by-skill benchmark had little bite for establishments already paying at or above customary pay but represented a substantial shock for low-paying establishments. Moreover, providers compete for nurses in local labor markets. Even an establishment already near the benchmark may face wage pressure if the mandate causes nearby competitors to raise pay. The policy therefore allows us to ask three connected questions: Did reimbursement-contingent wage standards raise earnings where they bound? Did providers offset those gains through employment, workforce-composition, or participation responses? And did the reform reshape local wage-setting beyond the establishments most directly exposed?

We answer these questions by linking newly collected state-by-skill benchmark data to the Integrated Employment Biographies (IEB), Germany's administrative employer–employee records. The data cover the universe of employment subject to social-security contributions and allow us to follow workers across employers and sectors through 2024. Our main 2019–2024 analysis panel

contains approximately 514,000 LTC nursing workers, nearly 17,000 residential and home-care establishments, and all 400 German counties. We observe daily earnings, occupation, establishment, full-time or part-time status, and worker transitions. This scale is important in a setting in which exposure varies simultaneously across establishments, worker skills, and places.

We begin with a transparent sector-level comparison of full-time nurses in LTC and hospitals. Hospital nurses work in clinically related occupations and share many health-care labor-market shocks, but hospital employers were not directly covered by the LTC-specific rule. Because hospitals are not a mechanically untreated version of LTC, however, we use this comparison as descriptive benchmark evidence rather than as our primary source of identification. Our main design exploits heterogeneity within LTC. For each establishment-by-skill cell, we measure exposure as the difference between the relevant 2021 state-by-skill benchmark and the establishment's average wage in 2019–2021. The measure is fixed before implementation. Event-study specifications with worker fixed effects then compare wage changes across LTC jobs with larger and smaller pre-reform gaps. Because exposure is constructed from baseline wages, we also examine pre-reform placebo growth and analogous patterns among hospital nurses and workers less directly targeted by the mandate.

The first result is rapid and targeted wage catch-up. In the aggregate, median daily wages for full-time LTC nurses rise sharply relative to hospital nursing wages during the reform period, while full-time LTC employment shows weaker relative growth than hospital employment, particularly among nursing assistants. Within LTC, a 10-log-point larger pre-reform wage gap predicts a 2.3 percent larger wage increase over 2022–2024. The gradient appears in the reform period, peaks in 2023 and remains elevated in 2024, and is larger for skilled full-time nurses, for whom the corresponding estimate is 2.9 percent. The corresponding 2019–2021 placebo relationship is flat, and no analogous gradient appears among hospital nurses. These patterns indicate that the aggregate LTC wage increase was concentrated where the benchmark should have bound most strongly, rather than reflecting only a common post-pandemic shock to nursing pay. Consistent with this mechanism, the lower part of the within-state LTC wage distribution compresses, although the state-level compression relationship is noisy.

The second result is that the wage gains are not accompanied by a detectable short-run con-

traction in the formal LTC workforce. Counties with larger pre-reform gaps do not exhibit a clear relative decline in regular LTC nursing employment per older resident or in the number of active LTC establishments. More exposed establishments also do not become detectably less likely to employ regular LTC nurses after the reform. The estimates do reveal changes in how employment is organized: exposed establishments rely less on workers who are new to the establishment, and the full-time share falls relative to the part-time share. These margins warrant caution. The administrative records distinguish full-time from part-time employment but do not report hours, so a shift in employment status is not equivalent to a measured reduction in total labor input. The evidence therefore supports a deliberately bounded conclusion: through 2024, the mandate raised wages without a clear decline in regular LTC nursing employment or provider activity, while inducing some adjustment in worker flows and employment composition.

Third, the mandate changed local wage environments as well as wages at directly exposed establishments. We decompose an establishment's own wage gap into the gap facing its local competitor market and the establishment's underpayment relative to those competitors. A 10-log-point larger pre-reform gap in the leave-one-out county-by-skill competitor market predicts approximately 4.8 percent greater growth in local outside wages after the reform, conditional on the establishment's own exposure. We then instrument leave-one-out local wage growth with this competitor-market gap. The instrumental-variable point estimate implies a modest positive pass-through from policy-induced outside-wage growth to an establishment's own wages, but it is only marginally precise: the pooled coefficient is 0.094 with a standard error of 0.055. By contrast, the direct own-exposure gradient remains large and precisely estimated after controlling for instrumented outside-wage growth. The most robust interpretation is therefore direct catch-up by low-paying establishments, accompanied by a substantial policy-induced shift in competitor wages and more suggestive evidence of additional wage propagation through local outside options.

The paper makes three contributions. First, it contributes to the economics of LTC payment and workforce policy. Prior work shows that general wage floors, Medicaid wage pass-throughs, payroll subsidies, and staffing-contingent payments can affect staffing, worker retention, quality, and access (Feng et al., 2010; Foster and Lee, 2015; Gandhi et al., 2024; Hegland, 2025; Ruffini, 2024).

The GVWG provides evidence on a distinct policy architecture: reimbursement eligibility is tied to wages themselves, while compliant wage costs receive protection in regulated price negotiations. The linked employer–employee data let us trace the first-order incidence of this rule on individual earnings, worker flows, and provider employment in both residential and home-based care. This is an important step in understanding the policy's effects, even though the present analysis does not yet measure its incidence on prices, household payments, or care quality.

Second, the paper contributes to the literature on minimum wages and institutional wage setting (Cengiz et al., 2019; Dube and Lindner, 2024). The reform is neither a uniform national minimum wage nor an extension of collective bargaining in the usual sense. It creates a set of continuous, predetermined wage shocks whose magnitudes depend jointly on state-by-skill benchmarks and establishment-level pre-reform wages. This structure lets us study a central feature that treated-versus-untreated designs often obscure: a common legal rule can bind very differently across firms and places. It also lets us distinguish an establishment's direct regulatory exposure from the exposure of its local competitors. The distinction matters more broadly because reduced-form "bite" estimates may combine mechanical compliance with equilibrium wage responses transmitted through local labor markets.

Third, the paper provides cross-national evidence from a universal social-insurance setting using an administrative linkage at unusual scale. Germany differs institutionally from the United States, but it confronts the same underlying policy problem: governments finance or regulate a large share of formal LTC while providers compete for a scarce workforce. The German reform demonstrates how collective-bargaining benchmarks can be embedded in public purchasing rules and shows how that design changes the incidence of a wage standard across regions, qualifications, and providers. The results therefore complement evidence from U.S. Medicaid reforms by isolating a different lever through which public payers can influence the care workforce.

Taken together, the findings show that a reimbursement-contingent wage standard can generate substantial earnings gains in a low-wage care sector without a detectable immediate contraction in regular nursing employment. They do not imply that higher wages are costless. The GVWG explicitly facilitates recognition of wage costs in negotiated rates, so part of the incidence may

appear in social-insurance spending or household payments rather than provider employment. Nor can outcomes through 2024 settle slower-moving responses in entry, exit, service capacity, or quality. The short-run evidence instead establishes the first link in the policy chain: the mandate reached workers, bound most strongly where pre-reform pay was low, altered local wage environments, and produced observed adjustment within ongoing establishments rather than through an immediate withdrawal of formal care providers.

The remainder of the paper describes the related literature and institutional setting, presents the linked data and exposure design, and then reports the aggregate, establishment-level, local-market, distributional, and quantity results before concluding.

2 Literature Review

This paper connects three strands of research: wage floors and the propagation of wage shocks, public payment design and the long-term care workforce, and the institutions governing German long-term care. The organizing distinction is between a general wage floor, an unrestricted increase in provider reimbursement, and a payment rule that makes reimbursement eligibility conditional on worker compensation. Germany's 2022 reform combines the third instrument with protection for compliant wage costs in regulated rate negotiations. This policy architecture allows us to study both the direct incidence of the wage requirement and its transmission through local care labor markets.

2.1 Wage Floors, Spillovers, and Local Wage Setting

A large literature studies how statutory minimum wages affect earnings and employment. Recent reviews conclude that minimum wages reliably raise pay at the bottom of the distribution, while the employment effects found in many settings are modest relative to the wage gains (Dube and Lindner, 2024). Distributional approaches show that jobs paying below a new minimum tend to be replaced by jobs at or just above it, producing substantial earnings gains with limited net changes in low-wage employment (Cengiz et al., 2019). Firms may nevertheless adjust on margins that are not visible in employment stocks. For example, evidence from Hungary shows that firms pass part of

a large minimum-wage shock into prices and substitute capital for labor, with greater employment losses among firms less able to pass through costs (Harasztosi and Lindner, 2019). These findings motivate our examination of worker flows and full-time versus part-time employment in addition to establishment and employment counts.

Wage regulation can also affect workers and employers beyond those whose wages are mechanically raised. Using administrative payroll data, Gopalan et al. (2021) document wage spillovers above state minimum wages and find that employment adjustment occurs primarily through reduced hiring rather than greater separation of incumbent workers. In English residential care homes, an age-specific increase in the National Living Wage raised the wages of younger workers who were not directly covered; the spillovers occurred within firms, with no differential employment response by age (Giupponi and Machin, 2022). Evidence from health-care labor markets also points to wage-setting interactions across nearby employers. Staiger et al. (2010) find that a wage increase at U.S. Department of Veterans Affairs hospitals elicited wage responses at non-VA hospitals and estimate a low short-run labor-supply elasticity to individual hospitals. Prager and Schmitt (2021) show that concentration-increasing hospital mergers slow wage growth for workers with industry-specific skills, especially where unions are weak. Our analysis adds a different source of wage-setting variation: a common regulatory rule whose bite varies across establishments, skills, and places. By separating an establishment's own pre-reform wage gap from the exposure of its local competitors, we distinguish direct catch-up from policy-induced changes in outside wages.

2.2 Public Payment Design and the Long-Term Care Workforce

Long-term care is a particularly informative setting for wage regulation because labor is the central input and public programs finance or regulate much of provider revenue. General reimbursement increases can improve staffing, but they need not reach workers. In a structural model of Pennsylvania nursing homes, Hackmann (2019) estimates that a 10 percent increase in Medicaid reimbursement would increase skilled-nurse staffing per resident by 8.7 percent. By contrast, exploiting regional revisions to Japan's long-term care insurance fee schedule, Kondo (2019) finds no increase in establishment employment, worker earnings, or hours where payments rose. The contrast underscores

that the incidence of public spending depends on how payment is conditioned and on providers' adjustment margins.

Several studies examine policies that target care labor more directly. U.S. state Medicaid wage pass-through programs increased direct-care wages (Baughman and Smith, 2010) and certified nursing-assistant staffing (Feng et al., 2010); estimates of staffing subsidies likewise show modest staffing gains and some improvement in clinical quality (Foster and Lee, 2015). More recent work compares targeted and unrestricted payments directly. Gandhi et al. (2024) find that Medicaid payments contingent on staffing increased nursing-home employment, largely through low-wage hires, whereas a contemporaneous unrestricted rate increase did not. Hegland (2025) finds that a payroll subsidy increased staffing but reduced the Medicaid share of admissions, revealing a trade-off between workforce expansion and access for publicly insured residents. These studies are especially relevant to the present paper because they treat payment design, rather than only payment levels, as a determinant of labor-market incidence.

A related literature estimates the effects of statutory wage floors in long-term care. The introduction of the British national minimum wage generated sizable wage gains in a low-paying residential-care sector with limited evidence of employment loss (Machin et al., 2003). Later increases in England raised hourly wages and compressed the wage distribution; headcount did not fall significantly, although total weekly hours declined modestly and short-term zero-hours contracts increased (Vadean and Allan, 2021). In U.S. nursing homes, Ruffini (2024) finds that higher state minimum wages increased earnings and retention and improved several measures of resident care quality, while providers adjusted partly through prices and payer mix. Because turnover itself is associated with worse nursing-home quality (Shen et al., 2023), retention is a substantively important response even when employment stocks change little. Our setting differs from these statutory wage floors and from U.S. pass-throughs: the GVWG conditions continued participation in the statutory long-term care insurance market on remuneration and simultaneously protects compliant wage costs in rate negotiations. We therefore observe a wage-contingent participation rule rather than a general minimum wage, an unrestricted rate increase, or a subsidy tied to payroll or staffing.

2.3 German Long-Term Care and the 2022 Wage Mandate

Research on Germany emphasizes both the institutional fragmentation of elderly-care wage setting and the consequences of workforce scarcity. Drawing on several qualitative research projects, Bosch and Evans-Borchers (2026) interpret the recent policy sequence as an institutional upgrading of elderly-care work in a sector where conventional collective-bargaining coverage is weak. Using German Care Statistics and county-level vacancy data for 2007–2017, Heger et al. (2025) document that greater nursing shortages are correlated with lower nurse-to-resident ratios, shifts in skill mix, fewer residents, and lower occupancy. Their results are correlational rather than causal, but they identify an important implication for our quantity analysis: when staffing requirements and physical capacity constrain adjustment, workforce scarcity may appear in staffing intensity and utilization before it appears in provider counts.

The closest existing evidence on the GVWG itself is the official evaluation commissioned by the Federal Ministry of Health with the participation of the Federal Ministry of Labour and Social Affairs (Evans-Borchers et al., 2026). Combining establishment wage surveys with administrative, provider, price, claims, and interview evidence, the report documents a substantial post-reform rise in long-term care wages and a marked narrowing of the wage gap with hospital nursing. Its difference-in-differences analysis by reported remuneration regime finds larger wage gains among establishments that oriented toward a sectoral agreement or used freely negotiated pay than among establishments already covered by collective agreements. The report also documents that care-related prices increased more rapidly after implementation than before and than in its inflation-based counterfactual, while carefully considering contemporaneous inflation and energy-price shocks.

Our analysis is complementary to, but distinct from, that evaluation. The official report classifies establishments by their reported collective-bargaining or remuneration regime; it does not use each establishment-by-skill cell's pre-reform distance to the applicable state benchmark as treatment intensity. We construct that quantitative, predetermined exposure measure and link it to the universe of social-security employment records. This permits worker fixed-effects and placebo designs, heterogeneity by targeted worker group, analysis of worker flows and employment composition, and a decomposition of direct establishment exposure from competitor-market exposure.

Thus, the report establishes the reform's broad institutional and aggregate effects, including likely price incidence, while this paper isolates where the wage rule bound and how its effects propagated across establishments and local labor markets.

3 Institutional Background: Wage Regulation in German Long-Term Care

Germany's long-term care (LTC) sector is a publicly financed, labor-intensive service sector with persistent recruitment and retention problems. Benefits are financed through statutory long-term care insurance, while services are delivered by a mix of private, non-profit, church-affiliated, and public providers. Because providers receive public reimbursement but differ substantially in ownership, collective-bargaining status, and wage-setting practices, LTC combines strong public regulation with substantial heterogeneity in provider-level pay.

This institutional structure matters for wage regulation. Germany has historically relied on collective bargaining to set wages and compress wage dispersion, but collective-bargaining coverage has declined over time. In LTC, this decline interacted with a fragmented provider landscape. Public, non-profit, and church-affiliated providers were more likely to follow collective or collectively oriented wage schedules, while many private providers operated outside collective agreements. As a result, collectively bound providers faced higher labor costs than non-covered competitors, and collective bargaining alone could no longer ensure broad wage standards in the sector.

3.1 Pre-2022 sectoral minimum wages in long-term care

Before the GVWG reform, wage regulation in LTC relied mainly on sector-specific minimum wages. The legal basis was created through the Arbeitnehmer-Entsendegesetz (AEntG), which allowed minimum standards in long-term care to be made generally binding. The first Pflegearbeitsbedingungenverordnung came into effect in 2010 and introduced a sector-specific wage floor for long-term care workers. At the time, Germany did not yet have a general statutory minimum wage, so the LTC minimum wage was one of the early statutory wage floors in the German labor market.

These pre-2022 minimum wages were set through the Care Commission, which brings together representatives of employers and workers in the sector and recommends wage floors that can then be made binding by the Federal Ministry of Labour. Over time, the minimum wage levels increased and the regulation differentiated more clearly between worker groups, including nursing assistants with vocational training and qualified caregivers. By the early 2020s, the LTC minimum wage had become more comprehensive than in its initial form.

Despite these increases, the pre-2022 system remained limited for the purposes of wage standardization. The sectoral minimum wages were uniform or only coarsely differentiated and remained below collectively bargained wage levels. They therefore raised the wage floor but did not close the gap between providers covered by collective or church agreements and providers outside such wage regimes. This gap is central for the GVWG reform: the policy did not merely increase a statutory minimum wage, but linked reimbursement eligibility to collective or collectively oriented pay standards.

3.2 The 2022 GVWG wage mandate

The Gesetz zur Weiterentwicklung der Gesundheitsversorgung (GVWG) introduced a new wage mandate for long-term care providers. The reform became binding on September 1, 2022. From that date onward, providers participating in statutory long-term care insurance reimbursement had to satisfy wage requirements in order to remain eligible for public reimbursement.[1]

The reform gave providers several compliance options. First, a provider could be bound by a collective bargaining agreement, including church-specific labor law. Second, a provider not directly bound by such an agreement could orient its pay schedule to an existing collective agreement. Third, a provider could comply by paying wages that, on average, meet or exceed the customary regional remuneration level, the *regional übliches Entlohnungsniveau* ($rüE$). The $rüE$ benchmark is calculated from collectively agreed wages and varies by federal state and worker skill group.

This third option is central for our empirical design. For providers outside collective or church wage regimes, the $rüE$ benchmark defines a region- and skill-specific wage standard against which

[1] See §72 (3a–3f) SGB XI.

pay is assessed. Because the benchmark varies across states and skill groups, while pre-reform wages varied across establishments and local labor markets, the same national legal reform generated heterogeneous bite. Establishments that paid far below the relevant benchmark before the reform faced stronger wage pressure than establishments that already paid close to or above it.

The GVWG mandate therefore differs from a uniform national minimum wage in two ways. First, it is sector-specific: it applies to publicly reimbursed LTC providers rather than to the entire labor market. Second, it is place- and skill-specific: the relevant benchmark depends on the state and worker group. This institutional structure motivates the empirical strategy below, which measures exposure using the pre-reform distance between establishment wages and the relevant state-by-skill benchmark.

4 Data and Measurement

4.1 Administrative worker-establishment data

Our main data source is the Integrated Employment Biographies (IEB), provided by the Institute for Employment Research (IAB). The IEB are administrative labor-market records constructed from Germany's social security notification system and the Federal Employment Agency's registers.[2] They cover the universe of employment spells subject to social security contributions, as well as marginal employment, unemployment-benefit receipt, job search, and participation in active labor-market programs.[3] The linked employer–employee structure of the data allows us to follow workers across establishments and sectors and to measure wages, employment, separations, and local labor-market conditions at high frequency.

We construct an annual worker-level panel by retaining the employment spell active on June 30 of each year.[4] Our analysis begins in 2012, the first year in which the KldB 2010 occupation classification is consistently available through the end of our sample period. This classification

[2]The IEB database is maintained by the Institute for Employment Research in accordance with the German Social Code Book III. The data are subject to the confidentiality regulations outlined in Book I, Section 35, Paragraph 1 of the Social Code, with access regulated under Section 75 of Book X.

[3]The IEB exclude self-employed workers and civil servants. For employment spells, the data report establishment identifiers, industry, occupation, employment status, daily earnings, and worker characteristics.

[4]We follow the aggregation and data-cleaning procedures described by Dauth and Eppelsheimer (2020).

is necessary to distinguish geriatric nursing occupations from hospital nursing occupations and to separate nursing assistants from skilled nurses. For most analyses, we use the years 2012–2024. We use 2019–2021 to construct the exposure measures, treat 2022 as the anticipation and transition year, and treat 2023–2024 as the fully implemented period.

The broad data extract contains the complete employment histories of all workers ever observed in a health-care establishment or in a health-care nursing occupation during the analysis period. We refer to this extract as the "Health Care Sample" (HCS). It is used to construct the aggregate LTC–hospital comparisons, the main LTC exposure-based analysis sample, and leave-one-out local labor-market wage measures. Table 1 reports summary statistics for the worker-level LTC nursing sample used in the exposure-based analysis.

4.2 LTC and hospital nurse samples

We generate two related analytical samples from the HCS. The first is an aggregate LTC–hospital comparison sample. It compares nurses in long-term care to nurses in hospitals before and after the GVWG mandate. LTC nurses are identified by geriatric nursing occupations in inpatient and outpatient long-term care establishments. Hospital nurses are identified by hospital nursing occupations in hospital establishments.[5] This comparison is used for descriptive sector-level evidence. In Figure 1, for instance, we restrict the aggregate series to full-time workers because the IEB report daily earnings but do not directly report hours worked. This restriction makes the wage trends more comparable across sectors and over time.

The second sample is the main exposure-based LTC nursing sample. It consists of regular nursing workers employed in inpatient or outpatient LTC establishments. We include both nursing assistants and skilled nurses and, unless otherwise stated, both full-time and part-time regular employment. The main wage specifications therefore exploit variation within the LTC sector rather than relying on hospitals as the control group. Exposure to the GVWG mandate is measured at the establishment-by-skill level using pre-reform wages and the relevant state-by-skill benchmark. This

[5] In the main coding, LTC establishments are identified using WZ2008 codes for inpatient geriatric nursing homes and outpatient geriatric care services. The hospital comparison group is identified using hospital industries and hospital nursing occupations. The corresponding occupation codes distinguish nursing assistants from skilled nurses.

within-LTC design is the core empirical strategy because the mandate applied to LTC providers, but its bite varied sharply across establishments, skills, and local labor markets.

The distinction between the two samples is important for interpretation. The LTC–hospital comparison provides a transparent aggregate benchmark for the timing and magnitude of sector-level adjustment. The exposure-based LTC sample then asks whether wage growth was systematically larger where the mandate was more binding, and whether policy-induced local wage growth propagated across establishments within the same county-by-skill labor market.

4.3 Wage mandate ($r\ddot{u}E$) benchmarks and wage measurement

The GVWG mandate links provider reimbursement eligibility to collectively agreed or collectively oriented pay. A central object for our analysis is the regional customary remuneration benchmark, $r\ddot{u}E$ (*regional übliches Entlohnungsniveau*). The benchmark is calculated at the federal-state-by-skill level and summarizes customary collectively agreed wages in long-term care. For providers not directly bound by a collective agreement or church-specific wage regime, the $r\ddot{u}E$ benchmark provides the relevant outside standard against which remuneration is assessed. The reform therefore creates variation in mandate bite because the benchmark varies across states and skill groups, while pre-reform wages vary across establishments and local labor markets.

The IEB report daily earnings, whereas the $r\ddot{u}E$ benchmark is expressed on an hourly wage scale. To construct comparable wage measures for exposure, we infer hourly wages from daily earnings among regular full-time LTC nurses. Specifically, we convert daily earnings into an hourly wage using potential annual workdays and an assumed full-time workday of 7.8 hours:

$$w_{ijt}^{hourly} = \frac{\text{daily earnings}_{ijt} \times 365}{\text{potential workdays}_t \times 7.8}.$$

Potential workdays are taken from the IAB working-time accounts. Because hours are not directly observed in the administrative records, the conversion is used only to put pre-reform earnings on the same scale as the benchmark. Our main wage outcomes remain based on daily earnings observed in the IEB.

We construct pre-reform establishment-by-skill wages by averaging inferred hourly wages over

2019–2021. Let $\bar{w}_{js}^{pre}$ denote the resulting pre-reform wage for establishment j and skill group s. Using a three-year pre-period reduces noise from small establishment-by-skill cells and avoids using post-reform wage outcomes in the construction of exposure. To account for remaining uncertainty in inferring hourly wages from daily earnings, we align the median of inferred IEB hourly wages with the median $r\ddot{u}E$ benchmark in the pre-reform period. This normalization affects the level of measured wage shortfalls but not the identifying variation across establishments and local labor markets.

The left two panels of Figure 2 illustrate the two ingredients of mandate bite for skilled full-time LTC nurses. The state-by-skill benchmark varies discretely across federal states, while pre-reform wages vary much more continuously across local labor markets and establishments. Combining these two sources of variation generates substantial spatial heterogeneity in exposure to the GVWG mandate.

4.4 Own bite, market bite, and leave-one-out local wages

We use the signed pre-reform distance between wages and the relevant GVWG benchmark as a predetermined proxy for potential exposure to the mandate. Our baseline measure is the establishment-by-skill own benchmark gap, which we refer to as own bite:

$$\text{OwnBite}_{js} = \log\left(r\ddot{u}E_{b(j),s}^{2021}\right) - \log\left(\bar{w}_{js}^{\text{pre}}\right). \tag{1}$$

Here, $b(j)$ denotes the federal state of establishment j, s denotes the nursing skill group, and $\bar{w}_{js}^{\text{pre}}$ is the establishment-by-skill pre-reform wage averaged over 2019–2021. As described in the preceding subsection, the wage levels entering the bite measures are inferred hourly wages among regular full-time LTC nurses, placing them on the same scale as the benchmark. When the outcome sample includes part-time workers, they are assigned the exposure calculated from full-time wages in the corresponding establishment-by-skill cell.

Positive values of own bite indicate that pre-reform pay was below the benchmark, whereas negative values indicate pay above it. Larger values therefore represent greater potential exposure to the benchmark. Because providers could satisfy the GVWG requirements through different

remuneration regimes, own bite is not necessarily each establishment's literal compliance gap. It instead provides a common measure of benchmark-based policy exposure that can be constructed for all establishments. Since it is based entirely on pre-reform information, subsequent wage adjustment does not enter its construction.

The main reduced-form analysis asks whether wages rise more in establishments with larger pre-reform benchmark gaps. We estimate event-study specifications of the form

$$\log w_{ijt} = \sum_{\tau \neq 2021} \beta_\tau \left(\text{OwnBite}_{js} \times \mathbf{1}\{t = \tau\} \right) + \alpha_i + \lambda_t + X'_{ijt}\delta + \varepsilon_{ijt}. \tag{2}$$

Here, w_{ijt} is the daily wage of worker i employed in establishment j in year t, α_i denotes worker fixed effects, λ_t denotes year fixed effects, and X_{ijt} includes controls for skill, part-time status, state, and industry where they are not absorbed by the sample restriction. The coefficients β_τ measure how the wage gradient with respect to pre-reform own bite differs in year τ from the corresponding gradient in the omitted year, 2021.

The pooled specification replaces the year-specific interactions with $\text{OwnBite}_{js} \times \text{Post}_t$, where Post_t equals one from 2022 onward. Because the annual IEB observations refer to June 30, while compliance became mandatory on September 1, the 2022 estimate captures anticipation and transition during the implementation year; 2023 and 2024 constitute the fully implemented period. We refer to 2022–2024 collectively as the reform period. Figure 4 and Table 2 report the main estimates.

Own bite summarizes the focal establishment's own component of benchmark-based exposure. The mandate may also affect its wages through changes in the local labor market. If competing LTC establishments raise wages in response to the reform, establishment j may face stronger outside options for its workers and consequently raise pay even conditional on its own benchmark gap. To investigate this channel, we operationalize the local LTC competitor market at the county-by-skill level and construct leave-one-out market wages. For each establishment j, county $c(j)$, skill group s, and year t, $\bar{w}^{\text{LOO}}_{-j,c(j),s,t}$ denotes the mean wage among workers employed by other LTC establishments in the same county-by-skill market. The leave-one-out construction removes

establishment j's workers from the market measure and thereby avoids a mechanical relationship arising from including its own wages in both variables. The market analysis is conducted among skilled full-time nurses, so changes in the measure are not driven by shifts between full-time and part-time employment.

Using the pre-reform leave-one-out wage, we define market bite as

$$\text{MarketBite}_{jcs}^{\text{LOO}} = \log\left(r\ddot{u}E_{b(j),s}^{2021}\right) - \log\left(\bar{w}_{-j,c(j),s}^{\text{pre,LOO}}\right). \tag{3}$$

This measure captures the signed distance between the relevant GVWG benchmark and the pre-reform wage paid by other LTC establishments in the focal establishment's county-by-skill market. It is predetermined and varies across local markets because pre-reform wages differ across counties even within the same state-by-skill benchmark cell.

We also define relative underpayment as the difference between the pre-reform leave-one-out competitor wage and the focal establishment's own wage:

$$\text{RelUnderpay}_{jcs}^{\text{LOO}} = \log\left(\bar{w}_{-j,c(j),s}^{\text{pre,LOO}}\right) - \log\left(\bar{w}_{js}^{\text{pre}}\right). \tag{4}$$

Positive values indicate that establishment j paid less than other LTC establishments in its local market. By construction, own bite can be decomposed as

$$\text{OwnBite}_{js} = \text{MarketBite}_{jcs}^{\text{LOO}} + \text{RelUnderpay}_{jcs}^{\text{LOO}}. \tag{5}$$

This identity has a useful economic interpretation. An establishment can have a large own benchmark gap because its entire local LTC market is low-paid relative to the benchmark, because it pays less than its local competitors, or because both conditions hold.

The IV analysis uses this distinction to investigate the transmission of reform-induced changes in local competitor wages. Local wages are equilibrium outcomes: unobserved demand shocks, staffing shortages, or provider constraints may jointly affect competitors' wages and establishment j's own wages. An OLS regression of own wages on contemporaneous competitor-wage growth

therefore need not identify causal pass-through.

For notational compactness, define leave-one-out local LTC competitor-wage growth relative to its 2019–2021 pre-period mean as

$$g_{jcst}^{\mathrm{LOO}} \equiv \log\left(\bar{w}_{-j,c(j),s,t}^{\mathrm{LOO}}\right) - \log\left(\bar{w}_{-j,c(j),s}^{\mathrm{pre,LOO}}\right).$$

We instrument this measure with the predetermined bite of the local competitor market interacted with the reform-period indicator. The first stage is

$$\begin{aligned} g_{jcst}^{\mathrm{LOO}} = \pi \left(\mathrm{MarketBite}_{jcs}^{\mathrm{LOO}} \times \mathrm{Post}_t\right) \\ + \rho \left(\mathrm{OwnBite}_{js} \times \mathrm{Post}_t\right) + \alpha_i + \lambda_t + v_{ijt}. \end{aligned} \tag{6}$$

The second stage is

$$\begin{aligned} \log w_{ijt} = \beta\, \hat{g}_{jcst}^{\mathrm{LOO}} \\ + \gamma \left(\mathrm{OwnBite}_{js} \times \mathrm{Post}_t\right) + \alpha_i + \lambda_t + \varepsilon_{ijt}. \end{aligned} \tag{7}$$

Here, $\hat{g}_{jcst}^{\mathrm{LOO}}$ denotes the fitted value from Equation (6). The dynamic version replaces the reform-period interactions with separate year interactions and uses 2021 as the omitted year.

Under the exclusion restriction, β measures pass-through from policy-induced growth in local LTC competitor wages to the focal establishment's wages, conditional on its own benchmark gap. The coefficient γ captures the remaining association between the establishment's own benchmark gap and its wage growth after accounting for the estimated local competitor-wage channel. The excluded variation comes from differences in competitor-market bite across local labor markets, interacted with the timing of the reform, while the focal establishment's own bite is included directly in both stages.

This interpretation requires that, conditional on own bite, worker and year fixed effects, competitor-market bite affects the focal establishment's wage only through measured competitor-wage growth. Two features warrant a cautious interpretation. First, Equation (5) implies that, conditional on own bite, market bite is closely related to the establishment's pre-reform wage position relative to

its competitors. Second, market bite and measured competitor-wage growth share the pre-reform market-wage component, so measurement error or mean reversion could contribute to their relationship. We therefore interpret the IV estimates as suggestive evidence about local wage transmission rather than as a complete structural decomposition of the reform's wage effects. The reduced-form own-bite estimates remain the paper's primary evidence.

Figure 7 reports the dynamic first stage. Table 3 and Figure 8 report the corresponding OLS and IV pass-through estimates.

5 Aggregate Sector-Level Evidence: LTC vs. Hospitals

5.1 Normalized wage and employment trends

We first document aggregate sector-level trends by comparing full-time LTC nurses to full-time hospital nurses. Hospital nurses provide a useful benchmark because they work in clinically related occupations within the same broad health-care labor market, but their employers were not directly subject to the LTC-specific GVWG wage mandate. This comparison therefore provides a transparent first look at whether wages and employment in LTC moved differentially around the reform.

Figure 1 plots median wage and employment series for nursing assistants and skilled nurses in LTC and hospitals. Because wage and employment levels differ substantially across the two sectors, each series is normalized to 100 in 2021. The normalization focuses attention on relative changes around the reform rather than persistent level differences across sectors. We restrict the comparison to full-time workers because the IEB report daily earnings but do not directly observe hours worked.

Panels A and B show that median wages in LTC rise relative to hospital wages after 2021 for both nursing assistants and skilled nurses. The timing lines up with the passage and implementation of the GVWG mandate: the law was passed in 2021, and the wage requirements became binding in September 2022. The employment patterns in Panels C and D are more mixed. Full-time assistant employment continues to rise in LTC after 2021, but less rapidly than in hospitals; skilled full-time

LTC employment is approximately flat while hospital employment rises modestly. The aggregate comparison therefore indicates weaker relative employment growth in LTC, though not a sharp contraction in LTC employment levels. Because the sectors differ in important ways, we treat this evidence as descriptive and rely on the within-LTC exposure design for our quantity conclusions.

5.2 Limits of the aggregate comparison

While the LTC–hospital comparison is informative descriptively, it is not the main source of identification in this paper. Hospitals are a natural benchmark for LTC nursing because they employ clinically related workers and are exposed to many of the same broad health-care labor-market shocks. At the same time, hospitals are not a mechanically untreated version of LTC. The two sectors differ in wage-setting institutions, reimbursement structures, staffing constraints, and post-pandemic labor-market conditions. Aggregate LTC–hospital comparisons therefore summarize the timing and broad magnitude of sector-level adjustment, but they cannot by themselves isolate the causal effect of the GVWG mandate.

The remainder of the paper exploits the central institutional feature of the reform: the GVWG did not generate a uniform sector-wide wage shock. Instead, the same legal rule created sharply different bite across establishments, skill groups, and local labor markets depending on pre-reform wages relative to the relevant state-by-skill benchmark. This variation allows us to ask whether wage growth was systematically larger where the mandate was more binding, and whether reform-induced wage growth among local competitors passed through to an establishment's own wages. We next define these exposure measures and document the variation in benchmarks, pre-reform wages, and mandate bite that underlies the main empirical analysis.

6 Exposure to the GVWG Wage Mandate

This section documents the empirical variation underlying the exposure-based design. The GVWG mandate did not impose a single national wage floor. Instead, it tied required remuneration to state-by-skill benchmarks derived from collectively agreed wages. As a result, the bite of the mandate depends on both the level of the relevant benchmark and the pre-reform wage paid by an estab-

lishment or local labor market. We first show the spatial variation in these objects, then validate that measured bite predicts post-reform catch-up, and finally describe the exposure distribution in the main analysis sample.

6.1 Spatial variation in benchmarks, wages, and own bite

Figure 2 illustrates the construction of mandate exposure for skilled full-time LTC nurses. The top-left panel maps the 2021 $r\ddot{u}E$ benchmark. Because the benchmark is defined at the state-by-skill level, it varies discretely across federal states. The top-right panel maps pre-reform hourly wages inferred from the IEB. These wages vary more continuously across space, reflecting local labor-market conditions and differences in the wage policies of LTC providers.

The bottom panel combines these two ingredients into own bite, defined as the pre-reform log wage shortfall relative to the relevant state-by-skill benchmark, see Equation 1. Areas with higher own bite are places where pre-reform wages were further below the benchmark and where the mandate should therefore have been more binding. The figure highlights that exposure is not simply a comparison of high- and low-benchmark states. Instead, the same state-level benchmark can generate different effective bite depending on the local and establishment-level wage structure entering the reform.

This spatial heterogeneity is central to the empirical design. The reform creates a common legal obligation for LTC providers, but the implied wage pressure differs sharply across establishments, skill groups, and local labor markets. The exposure-based analysis below exploits this variation to ask whether post-reform wage growth was larger where the mandate was more binding.

6.2 Pre-reform wage gaps and post-reform catch-up

A useful check on the exposure measure is whether high-bite establishments look exactly as expected before the reform and whether this relationship changes after implementation. Figure 3 plots establishment-level own bite against mean log daily wages for skilled full-time LTC nurses. The left panel uses 2021 wages, before the mandate became binding. By construction, establishments with higher own bite have lower pre-reform wages relative to the benchmark; the fitted slope is close to

minus one.

The right panel repeats the same exercise in 2023. The relationship between own bite and wage levels is substantially flatter after implementation. The fitted slope falls in absolute value from about -1.16 in 2021 to about -0.56 in 2023. This pattern is consistent with partial catch-up: establishments that entered the reform further below the benchmark experienced larger subsequent wage increases.

This figure is descriptive rather than a full causal estimate. It nevertheless provides an important diagnostic for the design. If the measured bite of the mandate is economically meaningful, high-bite establishments should be low-wage establishments before implementation and should move closer to other establishments after the reform. Figure 3 shows precisely this pattern.

6.3 Summary statistics and exposure distribution

Table 1 summarizes the main worker-level LTC nursing sample used in the exposure-based analysis. The pre-reform period is 2019–2021, which is used to construct establishment-by-skill wages and policy exposure measures. The analysis panel extends through the post-reform years and includes regular nursing workers in inpatient and outpatient LTC establishments.

Panel A reports worker-level wage and employment characteristics. Panel B reports the key policy exposure measures: own bite, leave-one-out market bite, relative underpayment, and leave-one-out local wages. Own bite captures the establishment's direct pre-reform wage shortfall relative to the relevant state-by-skill benchmark. Leave-one-out market bite captures the corresponding shortfall for the local county-by-skill competitor market, excluding the worker's own establishment. Relative underpayment measures where the establishment sits within that local market. Together, these variables allow us to distinguish direct own-establishment exposure from the bite of the local outside-wage environment.

The exposure distribution has two important features. First, own bite varies meaningfully across establishments and skill groups, generating substantial treatment-intensity variation for the reduced-form wage analysis. Second, market bite varies less than own bite but remains economically relevant, reflecting differences in local wage levels even within state-by-skill benchmark cells. This

variation is what allows the IV analysis to ask whether reform-induced wage growth among local competitors passes through to an establishment's own wages, conditional on its own direct exposure.

Panel C reports the size of the pre-reform sample and the full analysis panel. The sample covers a large number of workers, establishments, counties, and all federal states, which allows the analysis to combine worker-level wage outcomes with establishment-level and local labor-market exposure measures. The next section uses this variation to estimate whether wages rose more in establishments where the GVWG mandate was more binding.

7 Wage Effects by Continuous Exposure

This section estimates the main reduced-form wage response to the GVWG mandate. The empirical question is whether workers in establishments with larger pre-reform wage shortfalls experienced larger wage gains after the mandate became binding. We first describe the event-study specification, then report the main exposure-gradient estimates, and finally present nonparametric and placebo evidence.

7.1 Event-study specification

The main specification is the continuous-exposure event study in Equation (2). The outcome is the log daily wage of worker i in establishment j and year t. Treatment intensity is measured by pre-reform own bite, OwnBite_{js}, defined at the establishment-by-skill level. The coefficients on the interactions between own bite and year indicators trace whether wage growth differs systematically between high- and low-bite establishments before and after the reform, with 2021 normalized to zero.

The identifying comparison is within worker over time, net of common year shocks. Worker fixed effects absorb time-invariant differences across workers, while year fixed effects absorb aggregate wage growth and macroeconomic shocks common to all workers in the sample. The event-study coefficients therefore measure whether workers attached to more exposed establishment-by-skill cells experience different wage growth around the mandate. The pooled specification replaces the year-specific interactions with $\text{Post}_t \times \text{OwnBite}_{js}$, where Post_t equals one from 2022 onward.

7.2 Main wage effects

Figure 4 shows a sharp post-reform increase in the wage gradient with respect to own bite. Before the reform, the estimated exposure gradient is small relative to the post-reform coefficients. After 2021, the coefficients rise immediately and remain elevated, indicating that wage growth was larger in establishments where pre-reform pay was further below the relevant state-by-skill benchmark.

Table 2 reports the corresponding pooled estimates and selected event-study coefficients. In the pooled specification for all LTC nurses, the coefficient on $\text{Post}_t \times \text{OwnBite}_{js}$ is 0.230 with a standard error of 0.008. Thus, a 0.10 log-point larger pre-reform wage shortfall predicts a 0.023 log-point larger post-reform wage increase. The effect is larger for skilled full-time nurses, where the pooled coefficient is 0.292. The estimates are also positive for skilled nurses overall, nursing assistants, and part-time workers, although the interpretation of part-time daily wages is more cautious because hours are not directly observed in the administrative data.

The selected event-study coefficients in Table 2 show the timing of the response. For all LTC nurses, the own-bite coefficient rises to 0.133 in 2022 and 0.262 in 2024. For skilled full-time nurses, the corresponding coefficients are 0.195 in 2022 and 0.389 in 2024. The dynamic pattern is consistent with a mandate that became binding in 2022 and generated larger wage gains where the pre-reform wage gap to the benchmark was larger.

7.3 Wage-growth binscatters

Figure 5 provides a simple nonparametric counterpart to the event-study estimates. The figure plots individual wage growth against pre-reform own bite separately for a pre-reform placebo period and the post-reform period. In the placebo period, wage growth is essentially flat in own bite. By contrast, wage growth after the reform rises smoothly with own bite.

This comparison is useful because it shows the exposure gradient directly in wage growth rather than through the full event-study specification. It also illustrates that the post-reform relationship is not driven by a single tail of the exposure distribution. The binscatter therefore reinforces the main event-study result: exposure predicts wage growth after the mandate becomes binding, but not over the placebo pre-period.

7.4 Validation and placebo exercises

Figure 6 compares the main exposure-gradient estimate to several validation and placebo exercises. The benchmark estimate for LTC nurses using establishment-level own bite is 0.230, matching the pooled estimate in Table 2. The pre-period placebo estimate among LTC nurses is close to zero, indicating that own bite did not predict comparable wage growth before implementation of the mandate.

The hospital placebo provides a sector-level check. Hospital nurses are assigned a county-level LTC exposure measure based on local pre-reform LTC wage shortfalls. The estimated post-reform gradient is not positive, suggesting that the main exposure relationship is not simply capturing a general local wage shock affecting all nursing occupations. The within-LTC validation exercise applies the establishment-level exposure measure to non-nursing occupations in LTC establishments. The estimated gradient is positive but smaller than for LTC nurses, which is consistent with some establishment-level wage adjustment or spillovers within LTC providers, while confirming that the largest response occurs among the nursing workers targeted most directly by the mandate.

Taken together, the event study, binscatter, and placebo evidence point to the same conclusion. Wages rose differentially in establishment-by-skill cells where the GVWG benchmark was more binding, and this pattern appears precisely after the reform rather than as a continuation of pre-existing wage growth trends.

8 Local Labor-Market Propagation

The reduced-form results show that wages rose more in establishments where pre-reform pay was further below the GVWG benchmark. This section asks what that exposure gradient represents economically. A place-varying wage standard can affect an establishment's wages directly, by making its own pre-reform wage schedule harder to reconcile with the mandate. But it can also operate indirectly, by raising the wages paid by nearby competitors and thereby changing local outside options for workers. We use the decomposition of own bite into local market bite and relative underpayment to distinguish these two channels.

8.1 Own bite versus local market bite

Own bite combines two distinct sources of exposure. An establishment may be far below the benchmark because the entire local labor market is low-paid relative to the relevant state-by-skill standard. Alternatively, it may be far below the benchmark because it pays less than other LTC employers in the same county-by-skill market. Equation (5) makes this distinction explicit by writing own bite as the sum of leave-one-out market bite and relative underpayment.

This decomposition is useful because the two components map into different economic mechanisms. Relative underpayment captures the establishment's position within its local labor market. Holding the local market fixed, a more underpaying establishment faces stronger direct pressure to raise wages after the reform. Market bite, by contrast, captures how strongly the mandate binds for the local outside market as a whole. If competitors in the same county-by-skill market raise wages after the reform, workers' outside options improve, and establishment j may have to raise wages even conditional on its own pre-reform wage gap.

The empirical question is therefore not only whether wages rise where own bite is high. The reduced-form estimates already show that they do. The additional question is whether reform-induced wage growth among local competitors passes through to an establishment's own wages after holding fixed its own regulatory exposure. This is the local labor-market propagation channel.

8.2 First stage: market bite predicts leave-one-out local wage growth

To isolate the local market channel, we use leave-one-out local market wage growth, defined in Section 4.4. This variable measures wage growth among other LTC establishments in the same county-by-skill market, excluding establishment j itself. The leave-one-out construction is important because it prevents the local market wage from mechanically including the establishment's own wage.

Local outside wages are equilibrium outcomes and therefore cannot simply be included as an exogenous control. Unobserved local shocks, such as changes in labor demand, staffing shortages, or provider constraints, may affect both competitors' wages and establishment j's own wages. We therefore instrument leave-one-out local market wage growth with the interaction between pre-

reform leave-one-out market bite and the post-reform period, as described in Equation (6). The instrument uses the fact that local markets further below the relevant benchmark before the reform should experience larger policy-induced outside-wage growth after implementation.

Figure 7 shows the dynamic first stage. Before the reform, market bite does not predict differential leave-one-out local wage growth. After implementation, the relationship becomes strongly positive. Table 3 reports the corresponding first-stage estimates. In the pooled 2022–2024 specification, the coefficient on $\text{MarketBite}_{jcs}^{LOO} \times \text{Post}_t$ is 0.479 with a standard error of 0.014. Thus, a 0.10 log-point larger pre-reform shortfall in the local competitor market predicts about 0.048 log points higher leave-one-out local market wage growth after the reform, conditional on own establishment exposure. The first-stage F-statistic exceeds 1,100, indicating a very strong excluded instrument.

The annual first stages show that the market-wage response builds quickly after implementation. The market-channel first-stage coefficient is 0.312 in 2022, 0.616 in 2023, and 0.574 in 2024. This pattern confirms that the GVWG mandate shifted local outside wages more strongly in county-by-skill markets where competitors' pre-reform wages were further below the relevant benchmark.

8.3 IV estimates of local outside-wage pass-through

Columns (3) and (4) of Table 3 compare OLS and IV estimates of the relationship between leave-one-out local market wage growth and own wages. The OLS association is large: the pooled coefficient on g_{jcst}^{LOO} is 0.419 with a standard error of 0.022. This association indicates that establishments located in markets where competitors' wages grow faster also experience faster own-wage growth. However, because local wage growth is endogenous, this coefficient may reflect common shocks as well as causal pass-through.

The IV estimate is smaller. In the pooled 2022–2024 specification, the coefficient on instrumented leave-one-out local market wage growth is 0.094 with a standard error of 0.055. The annual IV estimates are 0.155 in 2022, 0.087 in 2023, and 0.047 in 2024. These estimates point to positive local outside-wage pass-through, but they are substantially smaller than the OLS associations and are less precisely estimated. The evidence for market transmission is therefore suggestive rather than definitive.

At the same time, the direct own-bite coefficients remain large and stable after controlling for the local market-wage channel. In the pooled IV specification, the coefficient on $\text{OwnBite}_{js} \times \text{Post}_t$ is 0.264 with a standard error of 0.010. The annual direct own-bite coefficients are 0.142 in 2022, 0.383 in 2023, and 0.319 in 2024. These estimates are very similar across the reduced-form, OLS, and IV columns. Thus, the main wage response is not absorbed by the local market-wage channel: establishments with larger own pre-reform wage shortfalls experience larger wage gains even conditional on policy-induced local outside-wage growth.

Figure 8 summarizes these estimates visually. The left panel compares OLS and IV estimates of local outside-wage pass-through. The right panel reports the corresponding direct own-bite coefficients. The figure reinforces the main message from Table 3: local market wage growth is strongly correlated with own wage growth, but the IV pass-through estimate is more modest, while the direct own-bite gradient remains economically important.

8.4 Interpretation: direct catch-up and market transmission

The results in this section sharpen the interpretation of the continuous-exposure wage effects. The strong own-bite gradient documented in Section 7 primarily reflects direct catch-up by establishments whose pre-reform wages were further below the relevant benchmark. This direct component remains large even when we condition on leave-one-out local market wage growth and instrument that market wage growth with competitor-market bite.

At the same time, the first stage shows that the reform clearly moved local outside wages. County-by-skill markets with larger pre-reform shortfalls to the benchmark experienced larger increases in leave-one-out local wages after implementation. This is exactly what one would expect if the GVWG mandate did not only affect isolated establishments, but shifted the wage environment in local LTC labor markets.

The second-stage IV estimates provide more cautious evidence on whether these policy-induced outside-wage changes passed through to own wages. The point estimates are positive, but smaller and less precise than the OLS associations. One interpretation is that part of the local market correlation in wages reflects common shocks or shared adjustment pressures rather than causal

pass-through. Another is that the relevant pass-through margin is difficult to estimate precisely once own regulatory exposure is held fixed, because own bite and local market bite are closely related components of the same policy environment.

Overall, the evidence supports a two-channel interpretation. First, the mandate induced direct wage catch-up in exposed establishments. Second, it generated substantial wage growth in exposed local competitor markets, creating scope for local labor-market propagation. The most robust evidence is for the direct own-bite channel; the evidence for additional causal outside-wage pass-through is positive but more tentative. This distinction is important for interpreting place-varying wage standards: own-exposure designs capture where the mandate binds, but they may also bundle direct establishment catch-up with changes in the local wage environment through which workers' outside options are formed.

9 Distributional and Quantity Responses

The wage effects documented above raise two additional questions. First, did the mandate compress wage differences within benchmark regions? Second, did higher wage growth in more exposed places or establishments lead to offsetting employment, provider, or workforce-composition responses? This section examines these margins. The evidence points to substantial wage adjustment, but limited short-run quantity responses.

9.1 Within-state wage compression

The GVWG benchmark varies at the state-by-skill level. This feature gives the reform a natural distributional implication: within a state, low-wage establishments or locations should face stronger pressure to catch up to the common benchmark than high-wage establishments. The reform can therefore compress the lower part of the wage distribution within states, even if wage differences across states persist because the benchmarks themselves differ.

Figure 9 examines this implication for skilled full-time LTC nurses. The outcome is the change in the within-state $p50 - p10$ wage gap between 2021 and 2024, with positive values indicating compression of the lower half of the wage distribution. The horizontal axis measures the state-level

bite of the GVWG benchmark. Several states experience a narrowing of the lower-tail wage gap after the reform, consistent with the idea that a binding benchmark raises wages most strongly at the bottom of the within-state distribution.

At the same time, the state-level relationship is noisy. The fitted relationship is sensitive to small-state variation and does not provide precise evidence of a monotonic compression gradient across states. We therefore interpret Figure 9 as descriptive distributional evidence rather than as a separate causal estimate. Together with the establishment-level wage effects in Section 7, it suggests that the mandate raised wages where pre-reform pay was low relative to the benchmark, with some evidence of lower-tail compression within benchmark regions.

9.2 Employment responses by exposure

We next ask whether more exposed local labor markets experienced differential employment responses. Figure 10 reports county-level evidence. Panel A plots event-study coefficients from regressions that interact county-level bite with year indicators. Panel B plots employment growth from 2021 to 2024 against county-level bite. Employment is measured as regular LTC nursing employment per 1,000 residents aged 75 and older.

The figure does not show a clear post-reform employment decline in more exposed counties. The event-study estimates are noisy, and the post-reform relationship between employment growth and county-level bite is weak. Panel A of Table 4 reports corresponding pooled estimates for several county-level outcomes. The coefficient for regular LTC nurses per 1,000 residents aged 75 and older is -0.756 with a standard error of 5.397. The estimate for LTC providers per 1,000 residents aged 75 and older is 0.167 with a standard error of 0.247. These estimates provide no evidence of a large short-run decline in total regular LTC nursing employment or provider activity in more exposed counties.

There is some heterogeneity across employment categories. The coefficient for full-time nurses is negative, while the coefficient for part-time nurses is positive. This pattern is consistent with possible adjustment along the full-time/part-time margin, but the county-level estimates are imprecise and should be interpreted cautiously. Moreover, the IEB observe employment status and

daily earnings, but not actual hours worked. The main conclusion from the county-level evidence is therefore limited but important: the strong wage responses documented above are not accompanied by a clear short-run contraction in regular LTC nursing employment or provider counts.

9.3 Worker flows, skill mix, and full-time work

Table 4 also examines establishment-level adjustment margins. Panels B–D report regressions of worker-flow, workforce-composition, and establishment-activity outcomes on post-reform own bite. These specifications use establishment-year outcomes, include establishment and year fixed effects, weight by pre-period nursing employment, and cluster standard errors at the establishment level.

Panel B shows that more exposed establishments experience a lower new-to-establishment share after the reform. The coefficient is -0.074 with a standard error of 0.011. The estimated separation rate falls by 0.013, while the retention rate rises by 0.013, although these two estimates are smaller and less precise. This pattern is consistent with higher wages reducing churn or lowering reliance on new establishment entrants, but the flow outcomes should be interpreted as short-run descriptive adjustment margins rather than definitive evidence on worker retention.

Panel C examines workforce composition. The share of skilled nurses does not change meaningfully with exposure: the coefficient is -0.005 with a standard error of 0.011. The share full-time falls more noticeably, with a coefficient of -0.088 and a standard error of 0.013. Figure 11 shows the corresponding event-study and high- versus low-bite time-series evidence for skill mix and part-time work. The skill-mix patterns are modest, while the full-time/part-time margin appears more responsive. Because hours are not observed, we interpret this result as evidence of adjustment in employment status rather than evidence on total labor input.

Finally, Panel D shows no evidence that more exposed establishments are more likely to disappear from regular LTC nursing employment. If anything, the coefficient on being active with regular LTC nurses is positive. Taken together, the quantity evidence suggests that the GVWG mandate generated rapid wage adjustment with limited short-run employment contraction. The clearest non-wage adjustment appears to be along workforce-flow and full-time/part-time margins rather than through exits of establishments, declines in provider counts, or large reductions in

regular nursing employment.

10 Conclusion

This paper studies a distinctive approach to long-term care workforce policy: conditioning providers' participation in a publicly financed care market on the wages they pay. Germany's 2022 GVWG reform tied remuneration to collectively agreed wages or state- and qualification-specific customary-pay benchmarks, generating substantial variation in the pre-reform distance between establishment wages and the relevant policy benchmark. Linking these benchmarks to administrative employer–employee records, we find that wage growth during 2022–2024 increased sharply with this pre-reform gap. A 10-log-point larger gap predicts a 2.3 percent larger increase in daily earnings across LTC nurses and a 2.9 percent larger wage increase among skilled full-time nurses. Comparable gradients are absent in pre-reform placebo growth and among hospital nurses assigned local LTC exposure.

Through 2024, counties with larger benchmark gaps do not experience a detectable differential decline in regular LTC nursing employment or in the number of LTC establishments employing regular nurses. Adjustment is instead more visible within establishments: more exposed establishments exhibit a lower share of workers who are new to the establishment and a lower full-time share. Because the administrative records do not report hours, the latter finding identifies a change in employment status rather than a reduction in total labor input. The reform also substantially shifted local wage environments. Competitor-market exposure strongly predicts growth in leave-one-out local wages, although the instrumental-variable evidence of additional pass-through to an establishment's own wages is positive but only marginally precise. The most robust result remains wage catch-up among establishments that initially paid further below the benchmark.

The absence of a detectable short-run employment contraction does not imply that higher wages are costless. The GVWG facilitates recognition of compliant labor costs in negotiated reimbursement rates, so part of the incidence may fall on social insurance or care recipients rather than on employment. The present data also do not identify every provider's compliance route or measure effects on prices, service capacity, quality, and access, while the horizon through 2024 captures only relatively rapid adjustment. Subject to these limits, the evidence shows that a reimbursement-

contingent wage standard can raise earnings quickly in a low-wage care sector without an immediate withdrawal of the formal workforce. More broadly, it demonstrates why the design of wage regulation matters: a standard tied to regional wage-setting institutions produces heterogeneous shocks across providers and can alter both directly exposed establishments and the local labor markets in which they compete for workers.

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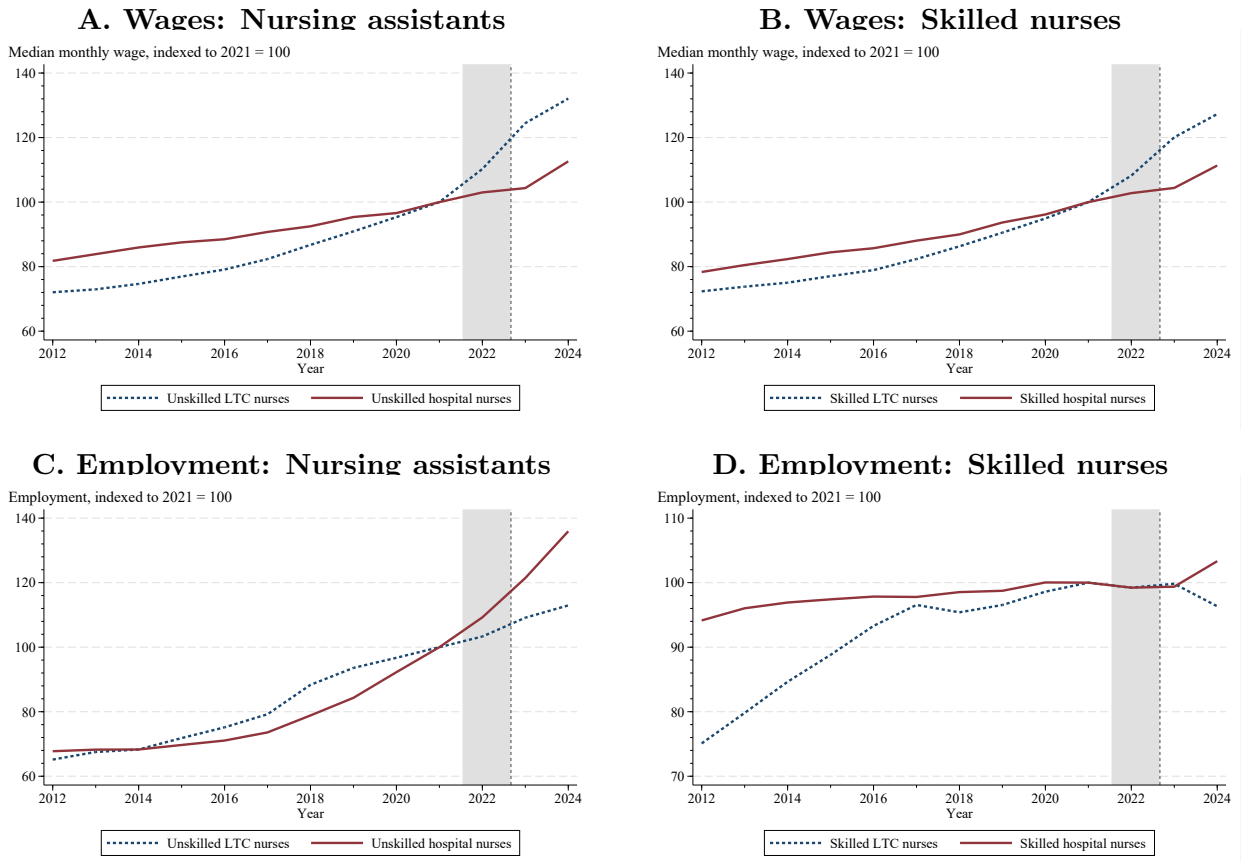
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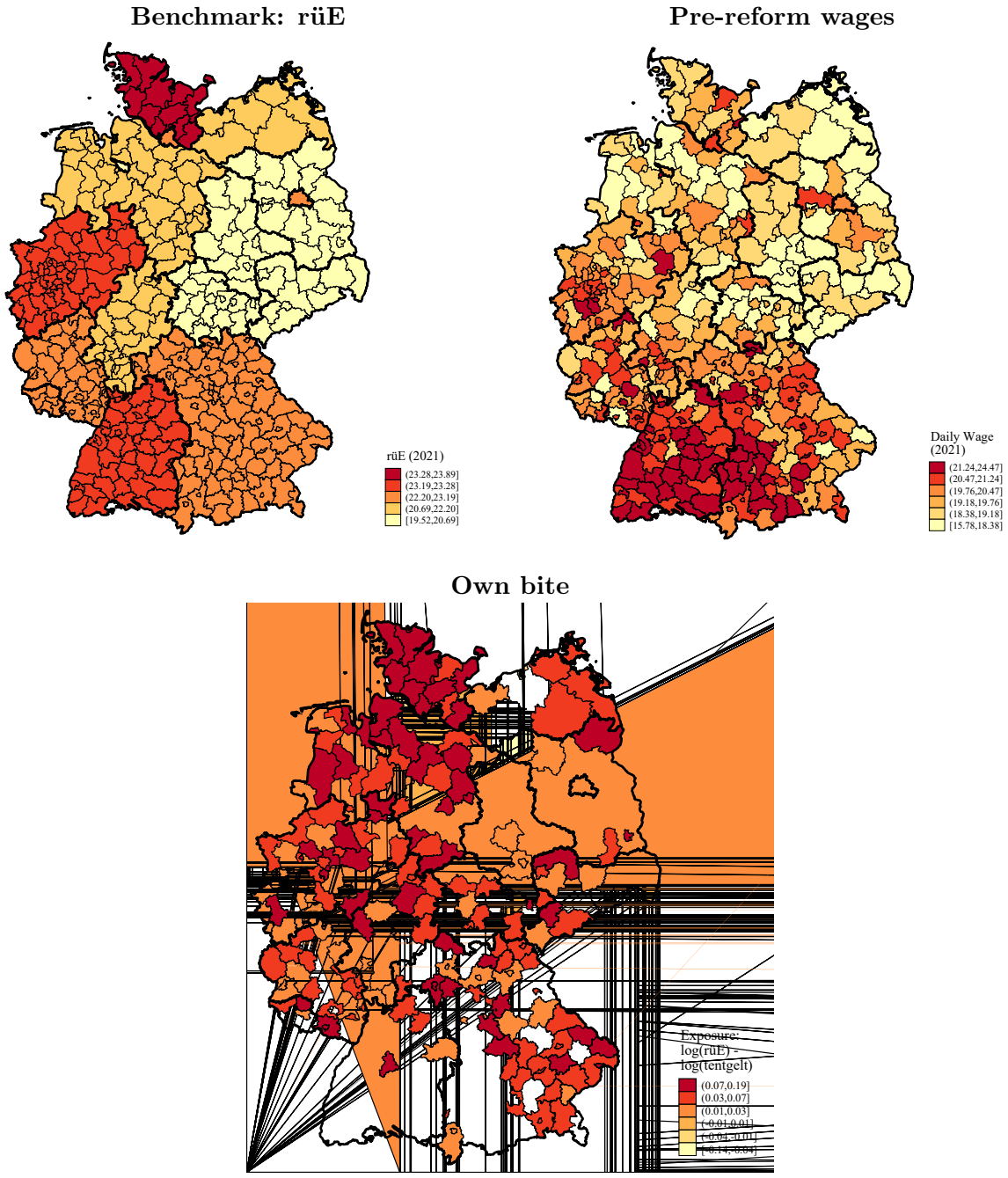
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Figure 1: Normalized Wage and Employment Trends for Full-Time LTC and Hospital Nurses



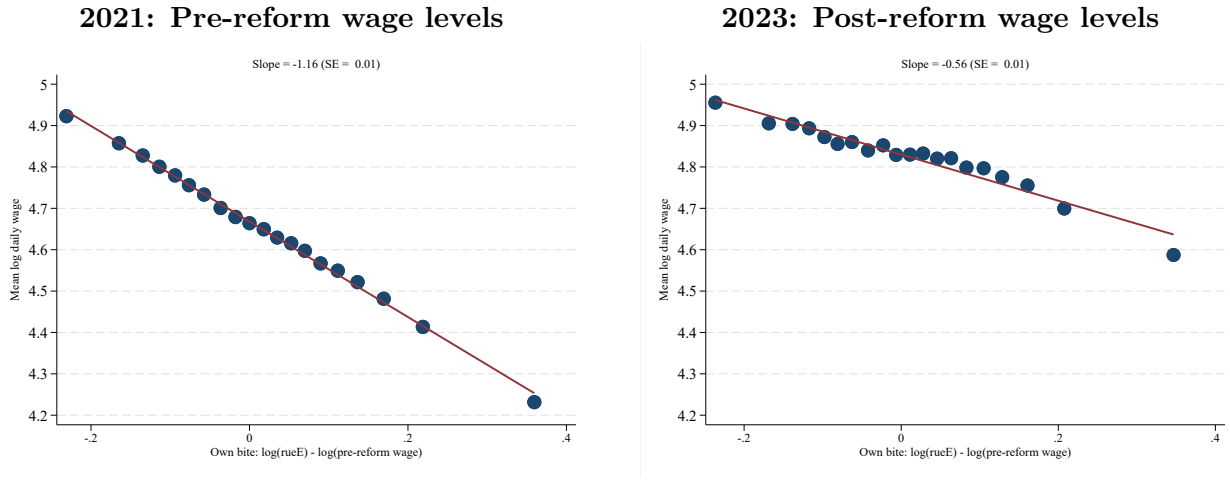
Notes: This figure plots aggregate wage and employment trends for full-time nurses in long-term care (LTC) and hospitals. All series are normalized to 100 in 2021, separately by sector and occupation group. Panels A and B show median monthly wages, constructed from daily earnings by multiplying by 30.44. Panels C and D show counts of regular full-time workers. LTC nurses are identified using geriatric nursing occupation codes; hospital nurses are identified using hospital nursing occupation codes. The vertical reference period indicates the timing of the GVWG wage mandate: the law was passed in 2021, and the mandatory wage requirements took effect in September 2022. Part-time workers are excluded because the administrative employment records do not directly report hours worked.

Figure 2: Spatial Variation in the GVWG Wage Benchmark, Pre-Reform Wages, and Mandate Bite



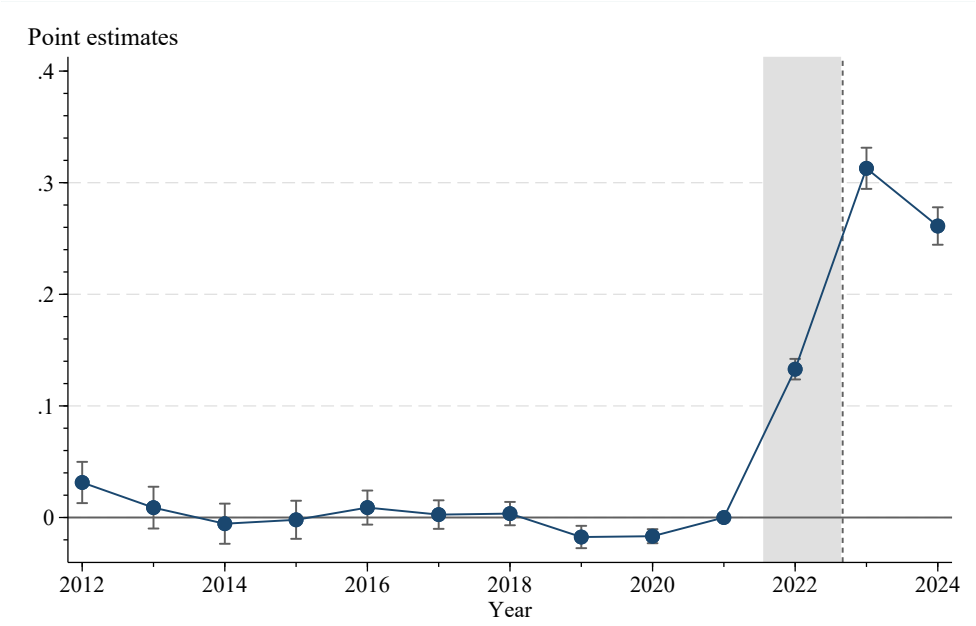
Notes: This figure summarizes the policy variation used to measure exposure to the GVWG wage mandate. The top-left panel maps the state-by-skill customary collective-bargaining wage benchmark, $r\ddot{u}E$, in 2021. The top-right panel maps pre-reform hourly wages. The bottom panel maps own bite, defined as the pre-reform log wage shortfall relative to the state-by-skill benchmark, $\log(r\ddot{u}E_{bs}^{2021}) - \log(\bar{w}_{js}^{pre})$. Maps are shown for skilled full-time LTC nurses.

Figure 3: Pre-Reform Wage Gaps and Post-Reform Catch-Up



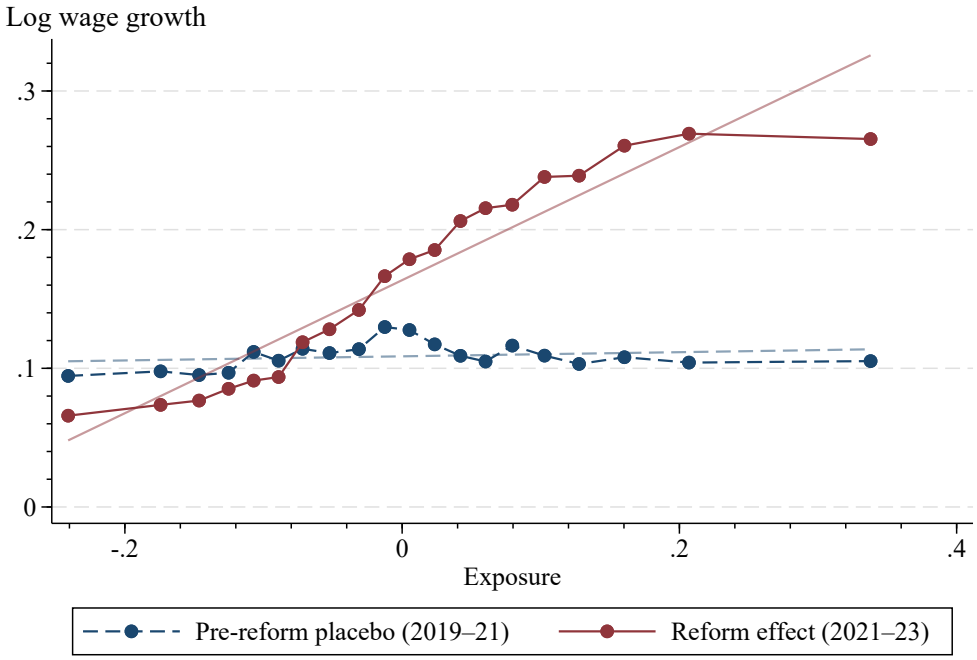
Notes: This figure plots binned scatter relationships between establishment-level own bite and mean log daily wages among skilled full-time LTC nurses. Own bite is defined as the pre-reform log wage shortfall relative to the state-by-skill $r\ddot{u}E$ benchmark. The left panel shows the relationship in 2021, before implementation of the GVWG wage mandate; the right panel shows the corresponding relationship in 2023. Each dot represents one ventile of the own-bite distribution. Fitted lines are estimated through the binned means, with bins weighted by the number of individual-year observations.

Figure 4: Event Study of Wage Responses by Pre-Reform Own Bite



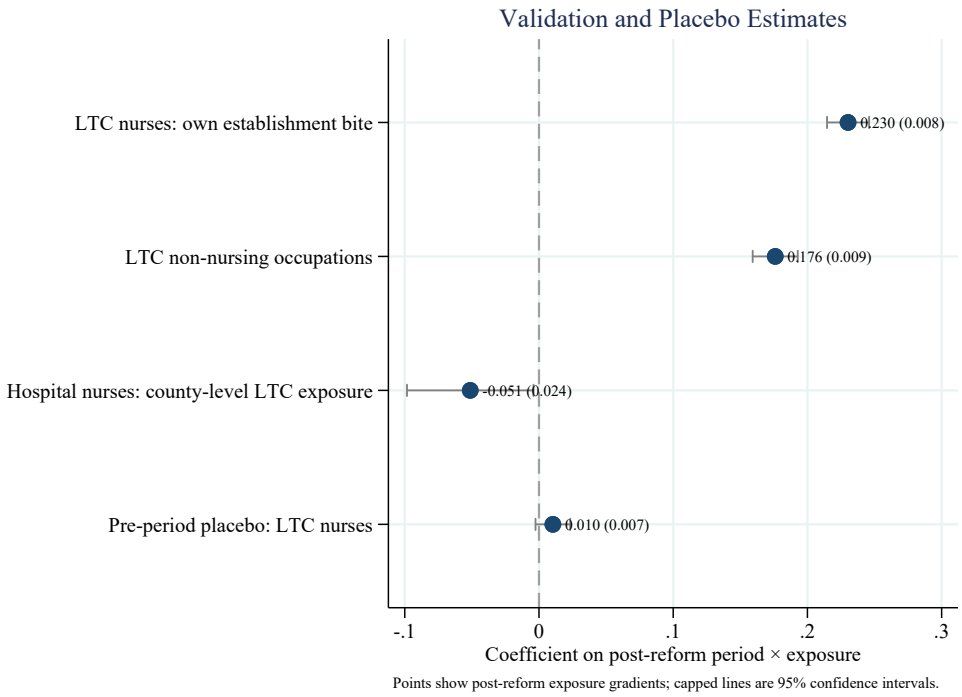
Notes: This figure reports event-study estimates of the wage response to the GVWG mandate by pre-reform own bite. The sample includes LTC workers in skilled and unskilled nursing occupations, pooling full-time and part-time employees. The outcome is the log daily wage. Own bite is defined at the establishment-by-skill level as the pre-reform log wage shortfall relative to the state-by-skill $r\ddot{u}E$ benchmark, $\log(r\ddot{u}E_{bs}^{2021}) - \log(\bar{w}_{js}^{pre})$. The plotted coefficients are estimates of β_τ from regressions of $\log w_{ijt}$ on interactions between own bite and year indicators, normalized to 2021. The specification includes worker fixed effects, year fixed effects, and controls for skill and part-time status; standard errors are clustered at the establishment level. The corresponding pooled difference-in-differences estimate is $\hat{\beta} = 0.23$ with standard error 0.008, implying that a 0.10 log-point larger pre-reform wage shortfall predicts a 0.023 log-point larger post-reform wage increase.

Figure 5: Wage Growth by Pre-Reform Own Bite Before and After the GVWG Mandate



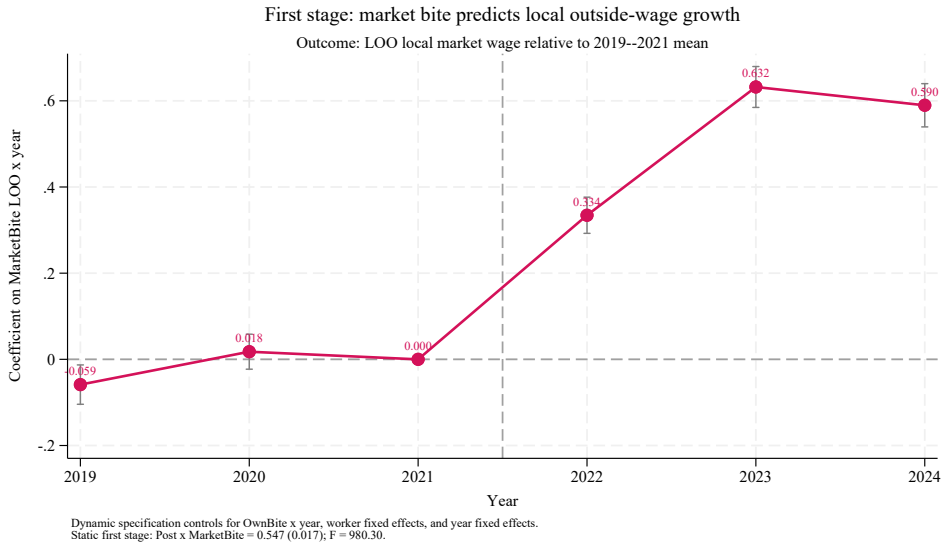
Notes: This figure plots binned scatter relationships between individual wage growth and pre-reform own bite for LTC nurses. Own bite is defined as the pre-reform log wage shortfall relative to the relevant state-by-skill $r\ddot{u}E$ benchmark. The dashed (blue) series shows the placebo relationship in the pre-reform period, while the solid (red) series shows the corresponding relationship over the post-reform period. Each dot represents one ventile of the own-bite distribution. Fitted lines are estimated from the underlying individual-level data. The figure shows that placebo wage growth is essentially flat, whereas post-reform wage growth rises smoothly with own bite, consistent with the GVWG wage mandate generating larger wage gains where the reform was more binding.

Figure 6: Validation and Placebo Estimates for the Own-Bite Wage Gradient



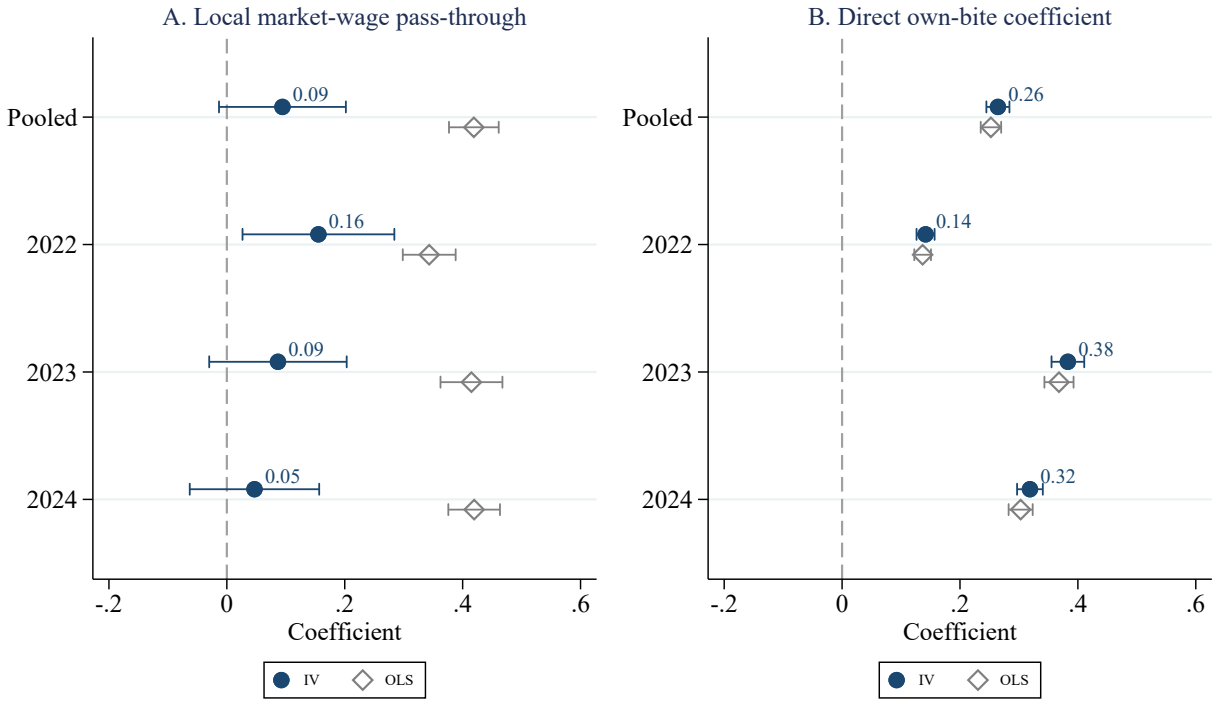
Notes: This figure compares post-reform exposure gradients across treated and placebo or validation samples. The main estimate uses LTC nurses and establishment-level own bite, defined as the pre-reform log wage shortfall relative to the relevant state-by-skill $r\ddot{u}E$ benchmark. The within-LTC validation estimate applies the same establishment-level exposure measure to non-nursing occupations in LTC establishments. The hospital placebo estimate assigns hospital nurses a county-level LTC exposure measure based on local pre-reform LTC wage shortfalls. The pre-period placebo estimates the exposure gradient among LTC nurses before implementation of the GVWG wage mandate. All specifications include worker fixed effects and year controls; standard errors are clustered at the establishment level, except for the hospital placebo, where standard errors are clustered by establishment and county. Points show coefficient estimates, and capped lines show 95 percent confidence intervals.

Figure 7: First Stage: Market Bite Predicts Leave-One-Out Local Wage Growth



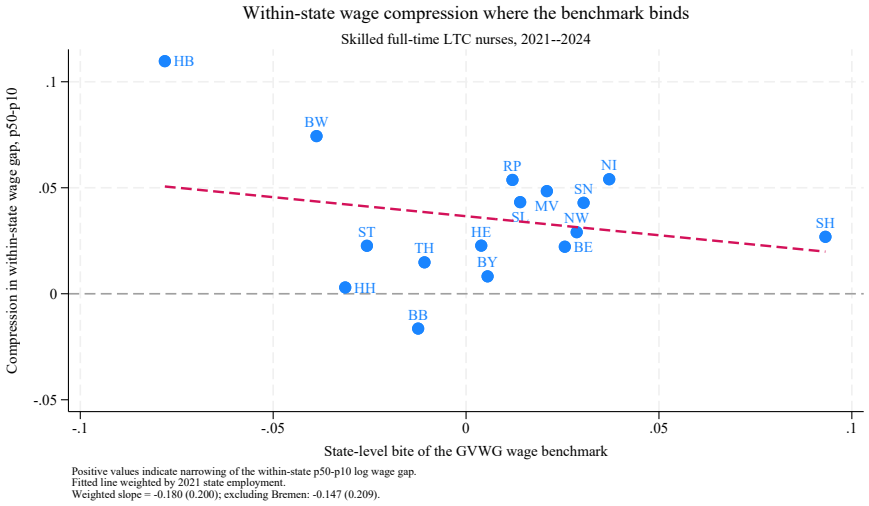
Notes: This figure reports dynamic first-stage estimates for the local market-wage channel. The outcome is the leave-one-out local market wage among other LTC establishments in the same county-by-skill market, measured relative to its 2019–2021 pre-period mean. The plotted coefficients are year-specific interactions between leave-one-out market bite and year indicators, normalized to 2021. The specification controls flexibly for the establishment's own bite interacted with year indicators and includes worker and year fixed effects. Standard errors are clustered at the establishment level. The corresponding static first stage uses $\text{Post}_t \times \text{MarketBite}_{jcs}^{LOO}$ as the excluded instrument and controls for $\text{Post}_t \times \text{OwnBite}_{js}$. The static annotation in the figure is based on the event-study plotting sample; Table 3 reports the preferred static IV specification.

Figure 8: Local Outside-Wage Pass-Through and Direct Own-Bite Effects



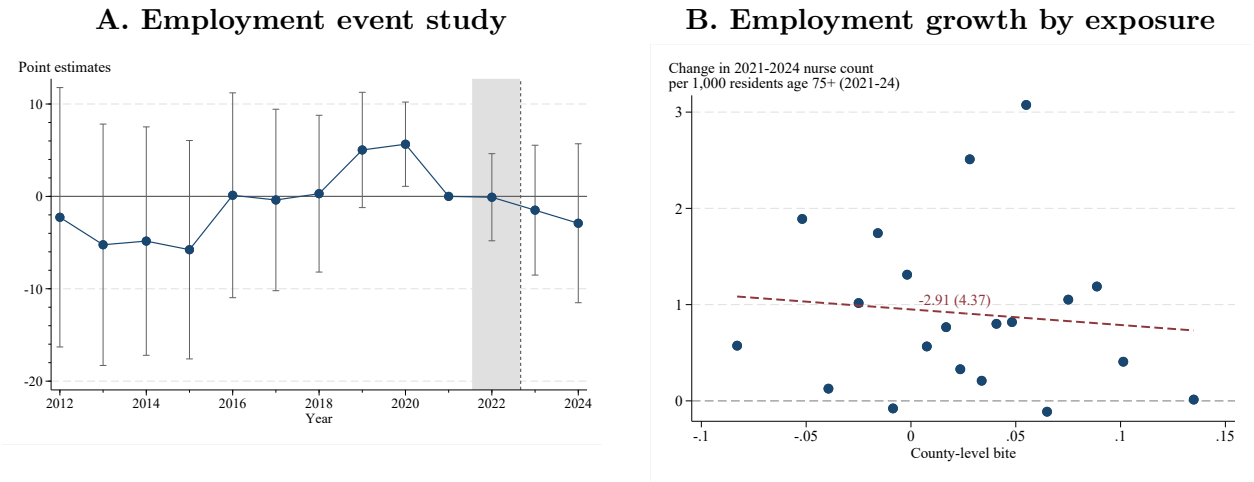
Notes: This figure reports estimates from specifications that separate local outside-wage pass-through from the direct own-establishment bite. The left panel reports coefficients on leave-one-out local market wage growth, $\Delta\bar{w}^{LOO}_{-j,c(j),s,t}$, measured among other LTC establishments in the same county-by-skill market relative to the 2019–2021 pre-period mean. In the IV specifications, this market-wage-growth variable is instrumented with $\text{MarketBite}^{LOO}_{jcs}$ interacted with the post-reform period or with post-reform year indicators. The right panel reports the coefficient on OwnBite_{js} interacted with the corresponding post period or post year from the same specifications. All specifications include worker and year fixed effects. Standard errors are clustered at the establishment level. The pooled row refers to 2022–2024; annual rows report separate estimates for 2022, 2023, and 2024.

Figure 9: Changes in Within-State Wage Dispersion by State-Level Benchmark Bite



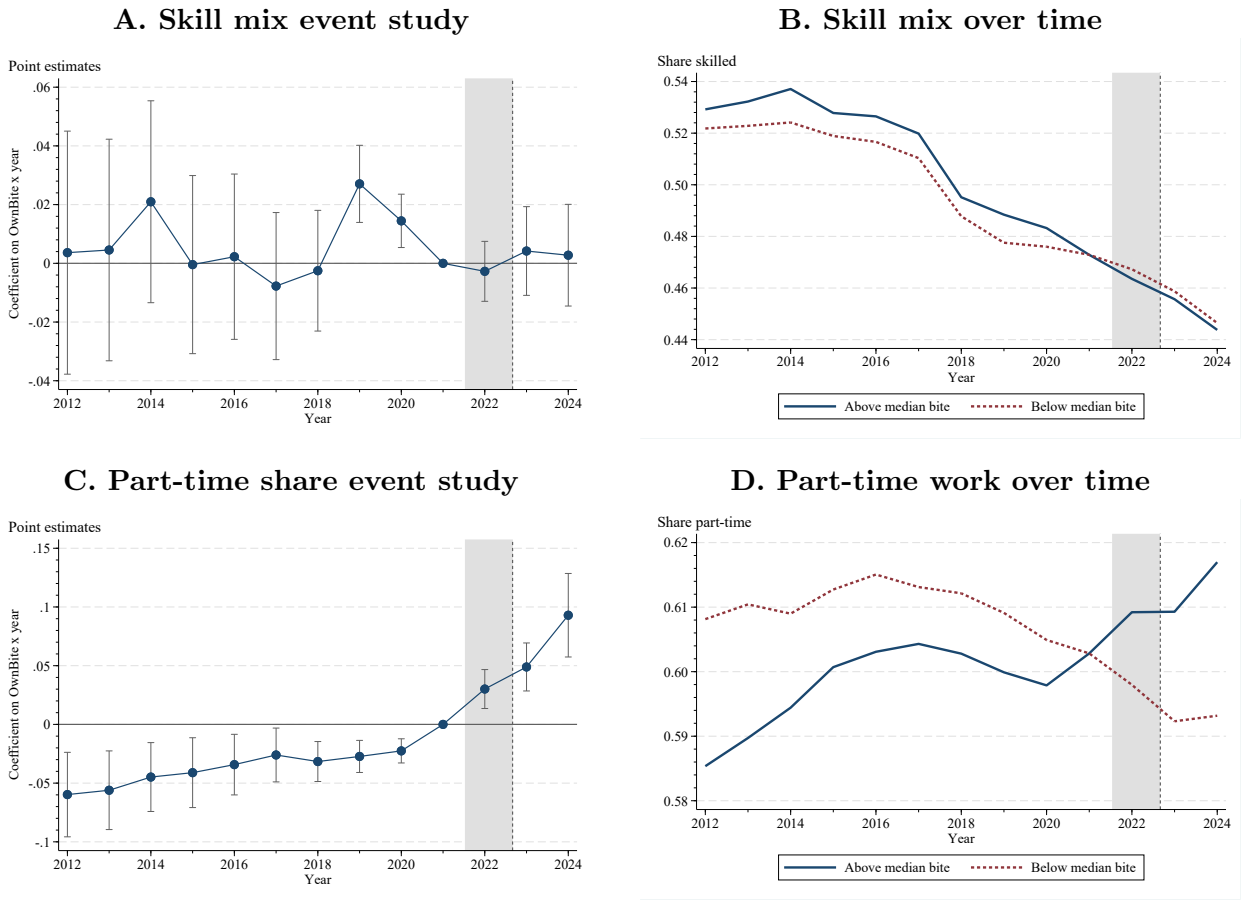
Notes: This figure relates within-state wage compression after the GVWG mandate to the bite of the wage benchmark. The outcome is the narrowing of the within-state wage spread among skilled full-time LTC nurses, measured as $(p50 - p10)_{2021} - (p50 - p10)_{2024}$ in log daily wages. Positive values therefore indicate compression of the lower half of the within-state wage distribution. The horizontal axis measures the state-level bite of the GVWG benchmark, defined using pre-reform wages relative to the relevant state-by-skill $r\ddot{u}E$ benchmark. Each dot represents one federal state. The fitted line is weighted by 2021 state employment in the skilled full-time LTC nurse sample.

Figure 10: Employment Responses by Exposure to the GVWG Wage Mandate



Notes: This figure examines whether counties more exposed to the GVWG wage mandate experienced differential employment responses. Panel A reports county-level event-study estimates, interacting county-level bite with year indicators and normalizing the coefficient in 2021 to zero. Panel B plots the relationship between employment growth from 2021 to 2024 and county-level bite. County observations are binned into ventiles. Employment is measured as regular LTC nursing employment per 1,000 residents aged 75 and older. County-level bite is defined as the pre-reform log wage shortfall relative to the relevant benchmark, measured at the county level. The event-study specification includes county and year fixed effects. Standard errors are clustered at the county level.

Figure 11: Adjustment Margins: Skill Mix and Part-Time Work



Notes: This figure reports exploratory evidence on workforce adjustment margins other than total employment. Panels A and C report establishment-level event-study estimates, interacting establishment-level own bite with year indicators and normalizing the coefficient in 2021 to zero. Panels B and D plot high- versus low-bite time series, splitting establishments at the pre-period employment-weighted median of own bite. The time-series panels are normalized so that the high- and low-bite groups have the same overall 2021 level. Own bite is the establishment-level pre-reform log wage shortfall relative to the state-by-skill *rüE* benchmark, aggregated across nursing skill groups using 2019–2021 employment weights. The event-study regressions include establishment and year fixed effects, are weighted by pre-period establishment nursing employment, and cluster standard errors at the establishment level. Composition outcomes are measured conditional on positive regular LTC nursing employment.

Table 1: Summary Statistics and Exposure to the GVWG Wage Mandate

	Mean	SD	P25	Median	P75
Panel A. Worker-level outcomes and characteristics, 2019–2021					
Daily wage	78.739	27.640	58.720	75.400	96.800
Log daily wage	4.301	0.375	4.073	4.323	4.573
Full-time	0.421	0.494	0.000	0.000	1.000
Part-time	0.579	0.494	0.000	1.000	1.000
Skilled nurse	0.499	0.500	0.000	0.000	1.000
Outpatient LTC	0.350	0.477	0.000	0.000	1.000
East Germany incl. Berlin	0.238	0.426	0.000	0.000	0.000
Age	43.641	12.172	33.000	44.000	54.000
Panel B. Policy exposure measures, 2019–2021					
Own bite	0.018	0.166	-0.096	0.013	0.119
Leave-one-out market bite	0.030	0.060	-0.007	0.028	0.068
Relative underpayment, leave-one-out	-0.012	0.165	-0.120	-0.016	0.082
Leave-one-out local wage	19.892	2.720	17.432	19.807	22.252
Workers in leave-one-out local market	1099.263	1983.117	264.000	464.000	887.000
Thin leave-one-out local market	0.000	0.012	0.000	0.000	0.000
Panel C. Sample size					
Pre-period worker-year observations, 2019–2021			814,966		
Pre-period individuals			383,446		
Pre-period establishments			16,996		
Pre-period counties			400		
Pre-period federal states			16		
Analysis worker-year observations, 2019–2024			1,610,452		
Analysis individuals			513,783		
Analysis establishments			16,996		
Analysis counties			400		
Analysis federal states			16		

Notes: This table reports summary statistics for the main worker-level LTC nursing sample used in the exposure-based analysis. Panels A and B use pre-reform worker-year observations from 2019–2021. The sample consists of regular LTC nursing workers in inpatient and outpatient LTC establishments (WZ2008 codes 87100 and 88101) and nursing occupations 82101 and 82102. Own bite is defined at the establishment-by-skill level as the pre-reform log wage shortfall relative to the relevant state-by-skill $r\ddot{u}E$ benchmark. Leave-one-out market bite is defined analogously using the county-by-skill local market wage, excluding the worker's own establishment. Relative underpayment is the difference between the leave-one-out local market wage and the establishment's own pre-reform wage. All statistics in Panels A and B are worker-year weighted. Panel C reports sample sizes for both the pre-period summary sample and the full 2019–2024 analysis panel.

Table 2: Wage Effects by Exposure to the GVWG Wage Mandate

	Dependent variable: log daily wage				
	All LTC nurses (1)	Skilled FT (2)	Skilled (3)	Unskilled (4)	Part-time (5)
$\text{Post}_t \times \text{OwnBite}_{js}$	0.230	0.292	0.257	0.187	0.200
	(0.008)	(0.012)	(0.010)	(0.011)	(0.009)
Selected event-study coefficients, normalized to 2021					
$\text{OwnBite}_{js} \times 1\{t = 2018\}$	0.003	0.052	0.000	0.008	-0.019
	(0.005)	(0.010)	(0.007)	(0.007)	(0.006)
$\text{OwnBite}_{js} \times 1\{t = 2020\}$	-0.017	-0.025	-0.024	-0.009	-0.013
	(0.003)	(0.007)	(0.005)	(0.004)	(0.004)
$\text{OwnBite}_{js} \times 1\{t = 2022\}$	0.133	0.195	0.151	0.108	0.103
	(0.005)	(0.008)	(0.006)	(0.006)	(0.006)
$\text{OwnBite}_{js} \times 1\{t = 2024\}$	0.262	0.389	0.299	0.210	0.204
	(0.009)	(0.011)	(0.010)	(0.012)	(0.010)
Worker fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes
Occupation controls	Yes	No	No	No	Yes
Part-time controls	Yes	No	Yes	Yes	No
Industry controls	Yes	Yes	Yes	Yes	Yes
Observations	2,871,585	722,499	1,492,020	1,352,546	1,643,563
Individuals	513,027	140,353	260,481	267,004	313,686
Establishments	16,931	14,889	15,074	12,047	15,651
Mean dep. var. in 2021	4.357	4.656	4.520	4.191	4.222

Notes: This table reports the results from the exposure-based wage regression in Equation 2 for LTC nursing workers, estimated for years 2012 to 2024. The dependent variable is the log daily wage. Own bite is defined at the establishment-by-skill level as the pre-reform log wage shortfall relative to the relevant state-by-skill wage benchmark. The first row reports the coefficient on $\text{Post}_t \times \text{OwnBite}_{js}$ from a pooled post-reform specification, where the post-reform period begins in 2022. The following rows report selected event-study coefficients from a specification interacting own bite with year indicators and normalizing the coefficient in 2021 to zero. Column (1) pools skilled and unskilled LTC nurses and includes both full-time and part-time workers. Column (2) restricts the sample to skilled full-time LTC nurses. Column (3) includes skilled LTC nurses, column (4) includes unskilled LTC nurses, and column (5) includes part-time LTC nurses. All specifications include worker fixed effects, year fixed effects, state controls, and industry controls; occupation and part-time controls are included where not collinear with the sample restriction. Standard errors, reported in parentheses, are clustered at the establishment level.

Table 3: Local Market-Wage Pass-Through Conditional on Own Establishment Exposure

	First stage $\Delta\bar{w}^{LOO}_{-j,c,s,t}$ (1)	Reduced form $\log w_{ijt}$ (2)	OLS $\log w_{ijt}$ (3)	IV $\log w_{ijt}$ (4)
Panel A. Pooled post-reform specification, 2022–2024				
MarketBite$^{LOO}_{jcs} \times Post_t$	0.479	0.045		
	(0.014)	(0.027)		
$\Delta\bar{w}^{LOO}_{-j,c,s,t}$			0.419	0.094
			(0.022)	(0.055)
OwnBite$_{js} \times Post_t$	-0.001	0.264	0.253	0.264
	(0.004)	(0.010)	(0.009)	(0.010)
Panel B. Annual market-channel coefficients				
2022 market channel	0.312	0.049	0.343	0.155
	(0.012)	(0.021)	(0.023)	(0.066)
2023 market channel	0.616	0.053	0.415	0.087
	(0.018)	(0.037)	(0.027)	(0.060)
2024 market channel	0.574	0.027	0.420	0.047
	(0.020)	(0.032)	(0.022)	(0.056)
Panel C. Annual direct own-bite coefficients				
OwnBite$_{js} \times 1\{t = 2022\}$	-0.000	0.142	0.137	0.142
	(0.003)	(0.008)	(0.007)	(0.008)
OwnBite$_{js} \times 1\{t = 2023\}$	-0.006	0.383	0.368	0.383
	(0.005)	(0.014)	(0.013)	(0.014)
OwnBite$_{js} \times 1\{t = 2024\}$	-0.008	0.319	0.303	0.319
	(0.006)	(0.011)	(0.010)	(0.011)
Worker fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	356,597	356,597	356,597	356,597
Individuals	93,180	93,180	93,180	93,180
Establishments	14,352	14,352	14,352	14,352
First-stage F-statistic	1111.75			1111.75
Annual first-stage F-statistics	2022: 659.33; 2023: 1219.93; 2024: 852.29			

Notes: This table reports the IV specification used to estimate local outside-wage pass-through. The sample covers skilled full-time LTC nurses observed from 2019 through 2024. The endogenous variable is leave-one-out local market wage growth, $\Delta\bar{w}^{LOO}_{-j,c(j),s,t}$, measured among other LTC establishments in the same county-by-skill market relative to the 2019–2021 pre-period mean. The excluded instrument is MarketBite$^{LOO}_{jcs} \times Post_t$, where MarketBite$^{LOO}_{jcs}$ is the pre-reform log shortfall between the state-by-skill rüE benchmark and the leave-one-out local market wage. All columns control directly for OwnBite$_{js} \times Post_t$, so the IV estimate captures the effect of policy-induced local market wage growth on own wages conditional on the establishment's own exposure to the mandate. Column (1) reports first-stage estimates. Column (2) reports reduced-form estimates of own wages on the excluded instrument. Column (3) reports OLS associations between own wages and leave-one-out local market wage growth. Column (4) reports IV second-stage estimates. Panel A reports the pooled post-reform specification. Panels B and C report annual coefficients from dynamic specifications with separate post-year market-wage-growth variables, instruments, and own-bite controls for 2022, 2023, and 2024. Annual market-channel rows report coefficients on MarketBite$^{LOO}_{jcs} \times 1\{t = \tau\}$ in columns (1)–(2) and on $\Delta\bar{w}^{LOO}_{-j,c(j),s,\tau}$ in columns (3)–(4). All specifications include worker and year fixed effects. Standard errors, reported in parentheses, are clustered at the establishment level.

Table 4: Quantity and Workforce Adjustment Responses by Exposure

	Coefficient	SE	Mean in 2021	Observations	Units
Panel A. County-level employment and providers					
Regular nurses per 1,000 75+	-0.756	(5.397)	33.634	5,200	county-year
Full-time nurses per 1,000 75+	-4.189	(2.653)	12.424	5,200	county-year
Part-time nurses per 1,000 75+	3.433	(3.768)	21.209	5,200	county-year
Skilled nurses per 1,000 75+	2.403	(2.913)	16.199	5,200	county-year
LTC providers per 1,000 75+	0.167	(0.247)	1.986	5,200	county-year
Panel B. Worker flows					
New-to-establishment share	-0.074	(0.011)	0.316	163,415	estab.-year
Separation rate	-0.013	(0.010)	0.226	159,297	estab.-year
Retention rate	0.013	(0.010)	0.774	159,297	estab.-year
Panel C. Workforce composition					
Share skilled nurses	-0.005	(0.011)	0.472	173,045	estab.-year
Share full-time	-0.088	(0.013)	0.410	173,045	estab.-year
Mean age	1.763	(0.292)	43.879	173,045	estab.-year
Panel D. Establishment activity margins					
Active with regular LTC nurses	0.039	(0.025)	0.982	220,948	estab.-year
No regular LTC nursing employment	-0.039	(0.025)	0.018	220,948	estab.-year

Notes: This table summarizes quantity and workforce adjustment responses to the GVWG wage mandate. Each row reports the coefficient on a post-reform exposure interaction from a separate regression. Panel A uses county-year outcomes and reports coefficients on $\text{Post}_t \times \text{CountyBite}_c$, where county bite is the pre-reform log wage shortfall relative to the relevant benchmark, measured at the county level. Employment and provider outcomes in Panel A are measured per 1,000 residents aged 75 and older and are estimated with county and year fixed effects; standard errors are clustered at the county level. Panels B–D use establishment-year outcomes and report coefficients on $\text{Post}_t \times \text{OwnBite}_j$, where own bite is the pre-reform establishment-level wage shortfall relative to the state-by-skill rüE benchmark, aggregated across nursing skill groups using 2019–2021 employment weights. Panels B–D include establishment and year fixed effects, are weighted by pre-period establishment nursing employment, and cluster standard errors at the establishment level. Panel B reports worker-flow outcomes based on whether workers remain at, enter, or leave the same establishment across adjacent years. Panel C reports composition outcomes conditional on positive regular LTC nursing employment. Panel D uses a balanced panel of pre-period exposed establishments and reports activity margins based on whether the establishment has any regular LTC nursing employment in a given year. The post-reform period begins in 2022.