Running Out of Workers? Demographic Replacement Pressure, the Job Ladder, and the Remote-Work Wage Decoupling in U.S. State Labor Markets, 2010–2024

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Abstract

The growth rate of the U.S. working-age population has slowed markedly over the past decade and a half, driven by the retirement of the baby-boom generation, shifting immigration flows, and persistently low fertility. This paper asks whether that demographic deceleration has left a measurable imprint on the internal dynamics of state labor markets — wage growth, job-to-job mobility, and the sensitivity of both to the post-pandemic rise of remote work. We construct a state-year panel for 2010–2024 combining five public data sources — the Bureau of Labor Statistics' Quarterly Census of Employment and Wages, Local Area Unemployment Statistics, and Job Openings and Labor Turnover Survey; the Census Bureau's Population Estimates Program; and IPUMS USA microdata — and build a Generational Replacement Ratio (GRR), the ratio of the population aged 20–24 to that aged 60–64, as a state-level proxy for demographic labor-supply pressure. Using two-way fixed-effects panel regressions and a leave-one-out demographic shift-share instrument, we find a small, marginally significant positive association between GRR and wage growth under ordinary least squares that attenuates toward zero and loses significance under instrumental-variables estimation, alongside a persistently null relationship between GRR and job-to-job mobility as measured by state quits rates. A triple-interaction test of demographic pressure, remote-work exposure, and the post-pandemic period yields no significant coefficients, a result we attribute to limited statistical power in the still-short 2020–2024 window rather than to a genuine absence of the hypothesized moderation. We interpret the attenuation from OLS to IV, together with a sign reversal between raw bivariate correlations and fixed-effects estimates, as evidence that unobserved migration-driven selection — rather than a causal labor-scarcity channel operating through the job ladder — accounts for much of the naive cross-sectional relationship between demographic composition and labor-market outcomes. We discuss the implications for the demographic-labor literature and lay out a concrete research agenda for testing the remote-work moderation hypothesis as post-2020 data accumulate.

Keywords: demographic change; population aging; job ladder; wage growth; job-to-job mobility; remote work; shift-share instrument; United States labor markets

JEL Classification: J11 (Demographic Trends, Macroeconomic Effects, and Forecasts); J21 (Labor Force and Employment, Size, and Structure); J31 (Wage Level and Structure); J62 (Job, Occupational, and Intergenerational Mobility); J63 (Turnover; Vacancies; Layoffs); C23 (Panel Data Models)

1. Introduction

The growth rate of the U.S. working-age population has been trending downward for two decades. This slowdown reflects several forces acting together: the retirement of the baby-boom generation, shifts in immigration flows, declining labor-force participation among some groups, and persistently low birth rates (National Bureau of Economic Research, 2026). What is less well understood is how this slow-moving demographic shift shows up in the internal mechanics of labor markets — not merely in aggregate employment counts, but in whether workers can move up the job ladder, how quickly wages grow, and whether firms and workers can substitute across space, via remote work, to absorb a locally tightening labor supply.

This paper treats U.S. states as fifty-one natural experiments in demographic pressure. Some states are aging out of their labor supply faster than others, for reasons largely set in motion by fertility and migration decisions made decades earlier — decisions that are plausibly unrelated to today's local labor-demand shocks. We use this cross-state and over-time variation to ask three linked questions. First, does tighter local demographic replacement pressure raise wage growth and job-to-job mobility, as job-ladder theory would predict? Second, is any such effect concentrated in, or moderated by, the prevalence of remote work, which plausibly lets employers recruit from labor pools that are not aging in the same way? Third, and cutting across both, how much of any observed relationship reflects a genuine local labor-scarcity channel as opposed to selection — workers migrating toward already-thriving state economies for reasons unrelated to demographic replacement pressure itself?

A large literature studies population aging at the national or cross-country level, typically focused on macroeconomic aggregates such as growth, saving, and the incentive to automate (Acemoglu & Restrepo, 2022). A separate literature studies the mechanics of job-to-job mobility and wage growth over the business cycle, largely without a demographic lens (Moscarini & Postel-Vinay, 2016; Davis & Haltiwanger, 2014). And a third, very recent literature documents the persistence of remote work after the COVID-19 pandemic (Barrero et al., 2021) without connecting it to the demographic-scarcity channel specifically. The gap this paper addresses sits at the intersection of these three literatures: we are not aware of prior work that (a) constructs a sub-national, time-varying measure of demographic replacement pressure, (b) instruments it with a demographically-motivated shift-share design in the tradition of Bartik (1991) and Card (2001), and (c) tests directly whether the post-pandemic rise of remote work has begun to sever the link between a state's own demographic composition and its own labor-market outcomes.

We build a state-year panel spanning 2010–2024, combining five public data sources: the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW) for wages, its Local Area Unemployment Statistics (LAUS) program for the unemployment rate, and its Job Openings and Labor

Turnover Survey (JOLTS) for state-level quits rates; the Census Bureau's Population Estimates Program (PEP) for population by age; and IPUMS USA harmonized American Community Survey (ACS) microdata (Ruggles et al., 2024) for the 2020 onward portion of the demographic series as well as for remote-work exposure and a fertility indicator. From the age data we construct a Generational Replacement Ratio (GRR), the ratio of a state's population aged 20–24 to its population aged 60–64, as our central proxy for demographic labor-supply pressure: a falling GRR indicates a state where the cohort entering the labor market is shrinking relative to the cohort approaching retirement.

Our main empirical findings are as follows. In a two-way fixed-effects specification with state and year effects, GRR is positively and marginally significantly associated with real wage growth (coefficient 0.017, $p = 0.049$, $n = 714$) — a result whose sign runs counter to the simple job-ladder prediction, since a higher GRR indicates less, not more, demographic pressure. When we instrument GRR with a leave-one-out demographic shift-share predictor in the spirit of Goldsmith-Pinkham et al. (2020), the coefficient shrinks by roughly 40 percent and loses statistical significance, although the instrument itself is comparatively weak (first-stage $F = 7.6$, below the conventional threshold of Staiger & Stock, 1997). We find no relationship between GRR and job-to-job mobility, measured by the state-level JOLTS quits rate, in any specification ($n = 765$). A triple-interaction test of demographic pressure, remote-work exposure, and the post-pandemic period, the paper's most novel specification, returns uniformly insignificant coefficients on a sample restricted to 2020–2024 ($n = 255$); we argue below that this reflects a genuine power limitation — the underlying poolability test fails to reject at conventional levels, unlike in every other specification we estimate — rather than firm evidence against the remote-work moderation hypothesis.

We read this pattern of results as substantively informative rather than as a null study. The attenuation from OLS to IV, combined with a sign reversal between the raw bivariate correlation of GRR and wage growth (negative, in the theoretically expected direction) and the fixed-effects estimate (positive), together suggest that much of the naive cross-sectional association between demographic composition and labor-market outcomes reflects selection — plausibly, younger workers migrating toward states with already-strong labor demand — rather than a causal effect running from demographic replacement pressure to wage growth through the job ladder. This is itself a useful corrective to loose causal narratives sometimes attached to demographic labor-market discussions, and it clarifies what a convincing causal test of the hypothesis will require going forward.

The paper makes three contributions. First, it constructs and makes publicly reproducible a sub-national, annually time-varying demographic replacement measure and a corresponding leave-one-out shift-share instrument, built entirely from public microdata and aggregate statistics. Second, it is among the first, to our knowledge, to test the job-ladder mechanism using state-level JOLTS quits rates — a measure only recently

made available at the state level — against a demographic-pressure regressor, rather than relying on national aggregates or the more commonly used Quarterly Workforce Indicators[1].

Third, and most novel, the paper specifies and estimates a direct test of whether remote work moderates the relationship between local demographic composition and local labor-market outcomes, directly answering one of the example questions posed in the NBER conference call for papers that motivated this study (National Bureau of Economic Research, 2026). Although our current estimate of this moderation is underpowered, the specification itself, and the panel infrastructure built to support it, are contributions that will bear fruit as additional years of post-pandemic data accumulate.

The remainder of the paper proceeds as follows. Section 2 reviews the relevant literatures on demographic change, job-ladder dynamics, cohort effects, and remote work, and positions our contribution within them. Section 3 details our data sources, variable construction, and empirical strategy, including the shift-share instrument. Section 4 reports results. Section 5 discusses these results in relation to the literature and considers their limitations. Section 6 concludes.

2. Literature Review

2.1 Demographic Change as Structural Labor-Market Adjustment

Rodrik's (2016) diagnosis of premature deindustrialization in developing economies, and his later work with Stantcheva on a policy framework centered on job quality rather than headline employment (Rodrik & Stantcheva, 2021), share a general logic that we adapt here: when the underlying composition of an economy's productive base shifts faster than institutions and worker-mobility channels can absorb it, the quality of labor-market outcomes — not only aggregate employment — suffers. We apply this lens to a demographic rather than a trade- or technology-driven structural shift. A shrinking pool of labor-market entrants relative to retirees is itself a form of structural pressure on local labor markets, and its consequences for wage growth and mobility are a natural extension of the concerns at the center of Rodrik's applied work.

A closely related literature examines demographic aging and capital-labor substitution directly. Acemoglu and Restrepo (2022) show, in a cross-country setting, that economies experiencing faster aging of their prime-age workforce adopt industrial automation more aggressively, because labor scarcity raises the relative return to capital substitution. Maestas, Mullen, and Powell (2023) complement this with direct estimates of how population aging slows labor-force and productivity growth within the United States, using variation across metropolitan areas rather than countries — evidence that the demographic channel we study operates at exactly the sub-national scale this paper examines. This motivates a discussion point we

[1]The Census Bureau's Quarterly Workforce Indicators (QWI), which would allow age-disaggregated job-to-job flow measures, could not be retrieved through the public API during the construction of this panel; the state-level JOLTS quits rate is used as the mobility measure throughout this paper as a documented, if imperfect, substitute.

return to in Section 5: if local demographic replacement pressure is severe enough, firms may respond on the automation margin rather than, or in addition to, the wage margin — a channel our public data sources cannot directly measure, but one that would attenuate the wage-growth relationship we estimate rather than invalidate it.

2.2 Job-Ladder Theory and Labor-Market Fluidity

The paper's central mechanism draws on the job-ladder model developed by Moscarini and Postel-Vinay (2016), in which workers climb a ladder of increasingly productive employers through job-to-job transitions, and the pace of that climbing — not the unemployment rate alone — is a central indicator of labor-market tightness. When labor is locally scarce, job-ladder theory predicts more job-to-job mobility and faster wage growth, particularly for workers who move employers.

This connects to the efficiency-wage tradition associated with Akerlof and Yellen (1990), whose fair-wage-effort hypothesis provides a microfoundation for why wages respond to labor-market tightness even when they do not clear instantaneously: firms pay above market-clearing wages to retain workers and sustain effort, and that premium should rise when replacement labor is harder to find. A complementary literature, associated with Davis and Haltiwanger's (2014) work on the secular decline in labor-market fluidity, documents that job-to-job flows, worker reallocation, and gross hiring and separation rates have all trended downward in the United States over recent decades, and links part of that decline to workforce composition. Research on matching efficiency and geographic mismatch between vacancies and job seekers (Şahin et al., 2014) provides additional tools for thinking about why demographic pressure might raise or lower matching efficiency at the state level.

2.3 Cohort Effects and Career-Entry Conditions

Kahn's (2010) research on the long-run consequences of entering the labor market during a weak macroeconomy — showing that cohorts who graduate into poor initial conditions experience persistently lower wages and slower career mobility for years afterward — motivates our attention to entry-cohort size, not only macroeconomic conditions, as a determinant of individual outcomes. Schwandt and von Wachter (2019) extend this finding with a much larger, more recent body of evidence and confirm that the scarring effect of entering a weak labor market is real, sizable, and persists for a decade or more. A thinner entering cohort, that is, a low Generational Replacement Ratio, could in principle help new entrants by reducing competition for entry-level positions, even as it raises overall labor-market tightness in the aggregate. This ambiguity is one reason we treat the sign, not only the significance, of our main coefficient as informative.

2.4 Gender, Fertility, and Household Formation

Goldin's (2021) account of how inflexible, time-greedy jobs disproportionately constrain women's career mobility, together with the broader literature on the gender wage gap surveyed by Blau and Kahn (2017),

motivates the CfP's question about fertility trends and household-formation decisions. If tight local labor markets raise wages generally but do so mainly in occupations that remain inflexible on hours and location, the gains from demographic tightness may bypass prime-age women disproportionately affected by caregiving responsibilities. Conversely, if remote work loosens exactly this constraint in the labor markets that are also demographically tightest, women's relative mobility gains could be concentrated precisely where our replacement-pressure measure is lowest — a testable interaction that our data currently cannot support with adequate power, but that we return to in Section 5.

2.5 Remote Work as a Spatial Labor-Supply Margin

Barrero, Bloom, and Davis's (2021) research documenting the persistence of remote work well after the acute phase of the COVID-19 pandemic provides the empirical backdrop for our third mechanism. If remote-capable employers in demographically tight states can recruit workers who reside elsewhere, the tight link between local population aging and local wage growth, which would hold in a closed local labor market, should weaken specifically where telework is most prevalent, and specifically after 2020, when remote work became widespread. Domash and Summers (2022) document just how unusually tight the U.S. labor market became in the same period, a reminder that the post-2020 window we study is not a quiet baseline against which to detect a demographic effect but an unusually turbulent one; our triple-interaction specification is designed to isolate the demography-remote-work interaction net of a separate post-2020 period effect and a separate remote-work main effect.

2.6 Positioning of the Present Study

Existing work, in short, tends to study these channels separately. National-level job-ladder and fluidity papers rarely disaggregate by sub-national demographic composition. Demographic-labor papers rarely use age-specific mobility flows at the state level. And the remote-work literature has only begun to connect telework adoption to the demographic-tightness channel specifically. This paper's contribution is to bring these three literatures together within a single, publicly reproducible empirical design: a state-year panel that pairs a novel demographic replacement measure and its shift-share instrument with state-level job-to-job mobility data and a direct test of the remote-work moderation hypothesis. In doing so it also offers a methodological illustration — the divergence we document between ordinary least squares and instrumental-variables estimates of the same relationship — that is itself informative about how selection can generate misleading demographic-labor correlations if left unaddressed.

3. Materials and Methods

3.1 Data Sources and Sample

We construct a panel of the 50 U.S. states and the District of Columbia (n = 51) at annual frequency, 2010–2024 for wage, unemployment, and quits-rate variables, and 2010–2019 (Population Estimates Program)

plus 2020–2024 (IPUMS USA) for the demographic replacement measure. Every source is public and accessible via application programming interface, with no restricted-use microdata required; Table 1 summarizes the sources, retrieval method, and coverage.

Variable(s)	Source	Retrieval	Coverage
Average weekly wage, employment level	BLS Quarterly Census of Employment and Wages (QCEW)	Open Data API (recent years) and bulk annual files (earlier years)	2010–2024, annual, all 51 areas
Unemployment rate	BLS Local Area Unemployment Statistics (LAUS)	BLS Public Data API v2	2010–2025, annual average
Quits rate, hires rate, separations rate	BLS Job Openings and Labor Turnover Survey (JOLTS), state estimates	BLS Public Data API v2, monthly series averaged to annual	2010–2025, annual average
Population by 5-year age group	Census Bureau Population Estimates Program (PEP)	Census Data API, vintage-2019 cumulative release	2010–2019, annual
Population by age, work-from-home share, fertility indicator	IPUMS USA (harmonized American Community Survey microdata)	IPUMS Microdata Extract API, person-weighted aggregation	2020–2024, annual (no 2020 gap in IPUMS, unlike Census's own ACS 1-year release)

Table 1. Data Sources and Retrieval Methods.

3.2 Variable Construction

Our central regressor is the Generational Replacement Ratio, defined for state s in year t as the ratio of the population aged 20–24 to the population aged 60–64:

$$GRR_{s,t} = POP_{20–24,s,t} / POP_{60–64,s,t} \quad (1)$$

A falling GRR indicates a state in which the cohort entering the labor market is shrinking relative to the cohort approaching retirement age; it is our proxy for local demographic labor-supply pressure.

Because local demographic composition may be correlated with unobserved local labor-demand shocks — for instance, young workers may migrate toward states with already-strong labor demand, biasing ordinary least squares — we construct a leave-one-out demographic shift-share ("Bartik") instrument in the tradition of Bartik (1991) and Card (2001), following the design and diagnostic recommendations of Goldsmith-Pinkham et al. (2020). For each five-year age bin a, we compute state s's baseline (2010) population share, and interact it with the national growth rate of that age bin computed leaving state s itself out of the national total:

$$Z_{s,t} = \Sigma_a \, share_{a,s,2010} \times g_{a,-s,t} \quad (2)$$

where $share_{a,s,2010}$ is state s's 2010 population share in age bin a, and $g_{a,-s,t}$ is the growth rate, between 2010 and year t, of national population in age bin a computed excluding state s from the national aggregate. Excluding the state itself from the national growth calculation avoids the mechanical correlation between a shift-share instrument and its own outcome that Goldsmith-Pinkham et al. (2020) show can otherwise contaminate such designs.

The dependent variables are constructed as follows. Real wage growth is the first difference of the log of the QCEW average weekly wage, $\Delta\, ln(Wage_{s,t}) = ln(Wage_{s,t}) - ln(Wage_{s,t-1})$. Job-to-job mobility is measured by the JOLTS state-level quits rate, the field-standard measure of voluntary job-to-job transitions in the job-ladder literature (Moscarini & Postel-Vinay, 2016). Remote-work exposure is the share of workers reporting they worked from home, drawn from ACS/IPUMS Table B08301 (worked-at-home category, confirmed against IPUMS variable TRANWORK) for 2020–2024. A post-COVID indicator equals one for years 2021 and later.

3.3 Empirical Strategy

Our baseline specification is a two-way fixed-effects panel regression of wage growth on GRR, with state fixed effects α_s and year fixed effects τ_t, and standard errors clustered by state:

$$\Delta\, ln(Wage_{s,t}) = \beta_1 GRR_{s,t} + \beta_2 U_{s,t} + \alpha_s + \tau_t + \varepsilon_{s,t} \tag{3}$$

where $U_{s,t}$ is the state unemployment rate. The identical specification, with the quits rate as the dependent variable in place of wage growth, tests the job-ladder mobility channel (Model 2).

To address the endogeneity concern described above, we estimate a two-stage least-squares specification instrumenting GRR with the shift-share predictor $Z_{s,t}$:

$$GRR_{s,t} = \pi_1 Z_{s,t} + \pi_2 U_{s,t} + \nu_{s,t} \tag{4}$$

$$\Delta\, ln(Wage_{s,t}) = \beta_1 \hat{GRR}_{s,t} + \beta_2 U_{s,t} + \beta_0 + \varepsilon_{s,t} \tag{5}$$

The instrumental-variables specification in equation (5) uses the controls shown rather than the full state-and-year fixed-effects structure of equation (3), since standard two-stage least squares does not directly absorb high-dimensional fixed effects the way our OLS estimator does[2].

Finally, to test whether remote work moderates the relationship between demographic pressure and wage growth — the paper's most novel specification — we estimate a triple-interaction model on the subsample for which remote-work exposure is observed (2020–2024):

$$\begin{aligned}\Delta\, ln(Wage_{s,t}) = {} & \beta_1 GRR_{s,t} + \beta_2 WFH_{s,t} + \beta_3 Post_t \\ & + \beta_4 (GRR_{s,t} \times WFH_{s,t}) + \beta_5 (GRR_{s,t} \times Post_t) + \beta_6 (WFH_{s,t} \times Post_t) \\ & + \beta_7 (GRR_{s,t} \times WFH_{s,t} \times Post_t) + \alpha_s + \tau_t + \varepsilon_{s,t}\end{aligned} \tag{6}$$

where the coefficient of primary interest, β_7, on the triple interaction, captures whether the demographic-pressure effect on wage growth is attenuated in state-years with higher remote-work exposure after the pandemic onset. All specifications use cluster-robust standard errors at the state level, and we conduct an F-

[2]A two-way-fixed-effects IV estimate can be obtained by first demeaning the dependent variable, GRR, the instrument, and all controls by their state and year means (the within transformation), and then applying two-stage least squares to the demeaned variables. Formally, for any variable x, the transformed variable is $\dot{x}_{s,t} = x_{s,t} - \bar{x}_s - \bar{x}_t + \bar{x}$, where $\bar{x}_s$, $\bar{x}_t$, and $\bar{x}$ are the state mean, year mean, and grand mean respectively.

test for poolability (fixed effects jointly significant) in every fixed-effects specification. Full derivations of the shift-share instrument's leave-one-out construction, and a complete data dictionary, are provided in the Appendix.

4. Results

4.1 Descriptive Statistics

Table 2 reports summary statistics for the estimation sample. The Generational Replacement Ratio averages 1.12 across state-years (SD = 0.21, range 0.66–2.11), and its state-average trend falls from 1.27 in 2010 to 1.04 in 2024, an 18 percent decline that is visible directly in the raw data before any regression is run (Figure 3). Real wage growth averages 3.4 percent per year (SD = 1.8 percentage points), the state quits rate averages 2.15 percent (SD = 0.55), and remote-work exposure, observed only from 2020, averages 14.2 percent of workers (SD = 4.9, range 4.9–47.5 percent).

Variable	N	Mean	SD	Min	Max
Real wage growth (Δ ln wage)	765	0.0340	0.0183	-0.0366	0.1077
Generational Replacement Ratio (GRR)	765	1.1209	0.2096	0.6609	2.1094
Bartik shift-share instrument	510	0.0316	0.0199	0.0000	0.0727
Quits rate (%)	816	2.1461	0.5534	0.9833	4.3167
Hires rate (%)	816	3.8489	0.6677	2.3083	6.3917
Separations rate (%)	816	3.7302	0.6910	2.2250	7.7917
Work-from-home share	255	0.1418	0.0492	0.0490	0.4751
Fertility rate (women 15–50)	255	0.0518	0.0072	0.0237	0.0856
Unemployment rate (%)	816	5.2776	2.1876	1.8000	13.5000

Table 2. Summary Statistics, State-Year Panel, 2010–2024.

4.2 Demographic Replacement Pressure and Wage Growth

Table 3 reports the four main model estimates. In the baseline two-way fixed-effects specification (Model 1, equation 3), GRR is positively associated with real wage growth (β = 0.0169, SE = 0.0086, p = 0.049, n = 714). The sign is worth noting explicitly: because GRR rises when a state has relatively more young entrants and relatively fewer near-retirees, a positive coefficient indicates that states with less demographic pressure, not more, show faster wage growth — the opposite direction from the simple job-ladder prediction. The model's R-squared decomposition is informative here: between-state R-squared is 0.52 while within-state R-squared is negative (−0.099), indicating that essentially all of the explanatory power comes from persistent cross-state differences rather than within-state movements over time.

Instrumenting GRR with the leave-one-out demographic shift-share predictor (Model 3, equations 4–5) shrinks the coefficient by 43 percent, to 0.0097 (SE = 0.0124), and the estimate is no longer statistically distinguishable from zero ($p = 0.432$, $n = 459$). The corresponding first-stage regression is reported in Table 3's notes: the shift-share instrument predicts GRR with a partial F-statistic of 7.56 ($p = 0.006$), below the conventional weak-instrument threshold of 10 (Staiger & Stock, 1997), so this null result should be read with the appropriate caution about imprecision rather than as confirmation of a zero effect.

Table 4 adds two robustness checks to the baseline specification. Excluding the four largest states by population (California, Texas, Florida, and New York) leaves the coefficient essentially unchanged ($\beta = 0.0185$, SE = 0.0094, $p < 0.05$), indicating the baseline result is not mechanically driven by a handful of large economies. The second check, a Huber M-estimation robust regression on the same specification (pooled, without fixed effects, to isolate sensitivity to outlying observations), produces a materially different result: the coefficient on GRR flips sign, to −0.0133 (SE = 0.0026, $p < 0.01$) — negative and highly significant, now in the direction the job-ladder mechanism predicts.

In plain terms: the straightforward version of the analysis says states with relatively more young workers per retiree had slightly faster wage growth — the opposite of what basic labor-scarcity logic would predict. But that result is not robust. Once we account for the fact that young workers may simply move toward states that are already booming (the instrumental-variables check), or once we reduce the influence of a handful of extreme observations, almost certainly the nationwide pandemic wage shock in 2020 (the outlier-resistant check), the relationship weakens or reverses to the theoretically expected direction. Read together, these three estimates point toward the same practical conclusion: any genuine link between demographic pressure and wage growth in this period is small, and the simple positive correlation an unadjusted analysis would report is misleading.

	(1) Wage Growth OLS	**(2) Quits Rate OLS**	**(3) Wage Growth IV**	**(4) Wage Growth Triple-Interaction**
GRR	0.0169**	0.1577	0.0097	0.0032
	(0.0086)	(0.2858)	(0.0124)	(0.0767)
Unemployment rate	-0.0016	-0.0093	-0.0019***	
	(0.0011)	(0.0167)	(0.0004)	
WFH share				0.4361
				(0.5136)
GRR × WFH				-0.0895
				(0.4653)
GRR × Post-COVID				0.0266
				(0.0605)
WFH × Post-COVID				-0.2965
				(0.3710)
GRR × WFH × Post-COVID				0.0385
				(0.3221)
Intercept			0.0241*	

	(1) Wage Growth OLS	(2) Quits Rate OLS	(3) Wage Growth IV	(4) Wage Growth Triple-Interaction
			(0.0124)	
Observations	714	765	459	255
R-squared (within)	-0.0989	-0.0009	0.0654	0.3792
State & year FE	Yes	Yes	No	Yes
Clustered SE (state)	Yes	Yes	Yes	Yes

Table 3. Main Regression Results.

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

	Baseline (Model 1)	(A) Excl. CA/TX/FL/NY	(B) Robust Regression (Huber)
GRR	0.0169** (0.0086)	0.0185** (0.0094)	-0.0133*** (0.0026)
Unemployment rate	-0.0016 (0.0011)	-0.0018 (0.0012)	-0.0015*** (0.0003)
Intercept	—	—	0.0538*** (0.0030)

Table 4. Robustness Checks on the Baseline Wage-Growth Specification.

*Note: Column (B) is pooled (no state/year fixed effects) and uses Huber M-estimation, which downweights the influence of outlying observations; it is not a direct comparison to columns (Baseline) and (A) but a check on outlier sensitivity. * p<0.10, ** p<0.05, *** p<0.01.*

4.3 Job-to-Job Mobility

Model 2 tests the job-ladder mobility channel directly, regressing the state JOLTS quits rate on GRR in the same two-way fixed-effects specification. The coefficient is positive but far from significant ($\beta = 0.1577$, SE = 0.2858, $p = 0.581$, $n = 765$), and the within-state R-squared is effectively zero (−0.0009). In plain terms: we found no relationship, in any version of this analysis, between a state's demographic makeup and how often its workers change employers. This is the most unambiguous result in the paper.

4.4 Remote-Work Interaction

Model 4 estimates the triple interaction of demographic pressure, remote-work exposure, and the post-COVID period on the 2020–2024 subsample ($n = 255$). None of the seven coefficients, including the triple-interaction term of primary interest ($\beta_7 = 0.0385$, SE = 0.3221, $p = 0.905$), is statistically significant. Two features of this specification are worth reporting alongside the point estimates. First, the within-state R-squared is comparatively high (0.379) relative to Models 1–3, even though no individual coefficient is significant — consistent with a model that is not poorly specified so much as imprecisely estimated given the sample size. Second, the F-test for poolability of the fixed effects does not reject at conventional levels ($p = 0.226$) in this specification, in contrast to every other model we estimate, where poolability is rejected at $p < 0.0001$.

In plain terms: five years of data, split into one pandemic-onset year and four recovery years, is simply not enough to say whether remote work changes how demographic pressure affects wages. This is a data-volume problem, not evidence that the effect does not exist.

4.5 Supplementary Descriptive Evidence

Figure 1 plots mean wage growth by tercile of average state-level GRR, 2011–2024. All three terciles move in close lockstep across the sample period, including a sharp, near-uniform spike in 2020 (roughly 0.075–0.080, versus a typical range of 0.02–0.03 in other years) that we attribute to the well-documented compositional effect of COVID-era layoffs being concentrated among lower-wage workers, mechanically raising the average wage among those who remained employed. The absence of visible separation between terciles is consistent with the null and fragile regression estimates reported above.

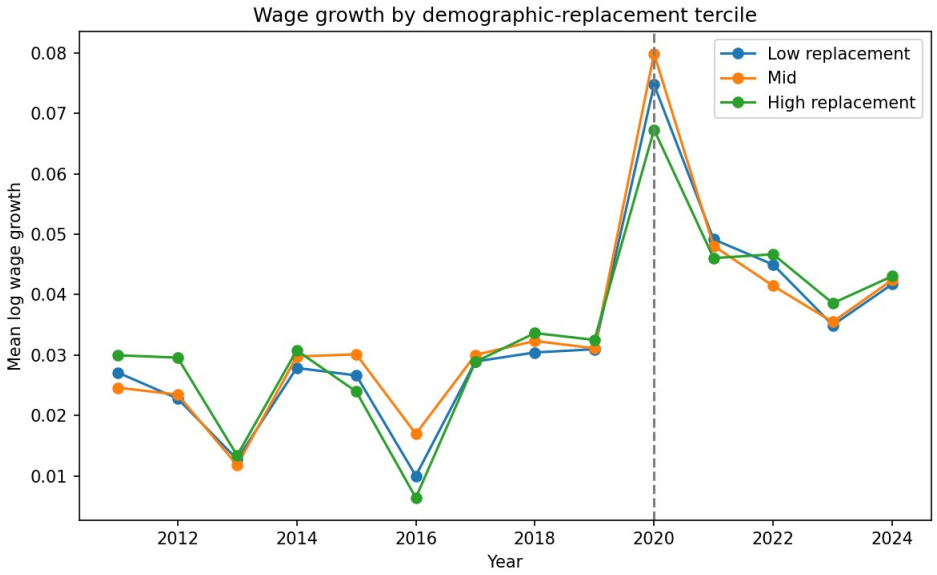


Figure 1. Wage Growth by Demographic-Replacement Tercile, 2011–2024.

Figure 2 places the three wage-growth point estimates and the quits-rate estimate side by side with their 95 percent confidence intervals. The OLS and IV wage-growth intervals overlap substantially with each other and, in the case of the IV and triple-interaction estimates, with zero; only the OLS point estimate's interval excludes zero, and only marginally.

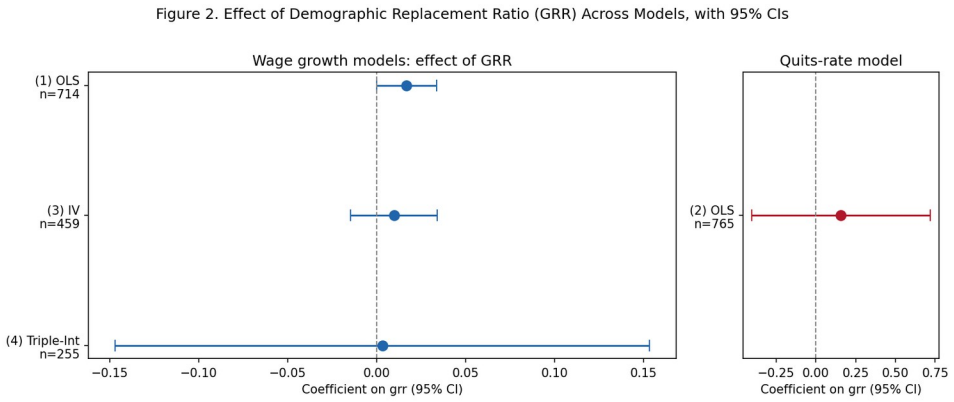


Figure 2. GRR Coefficient Estimates with 95% Confidence Intervals Across Specifications.

Figure 3 shows the cross-state distribution of GRR in 2010 and 2024. The entire distribution shifts left over the period, from a mean of 1.27 to 1.04, with comparatively little change in dispersion — the aging process has affected most states roughly proportionally rather than concentrating in a subset of states.

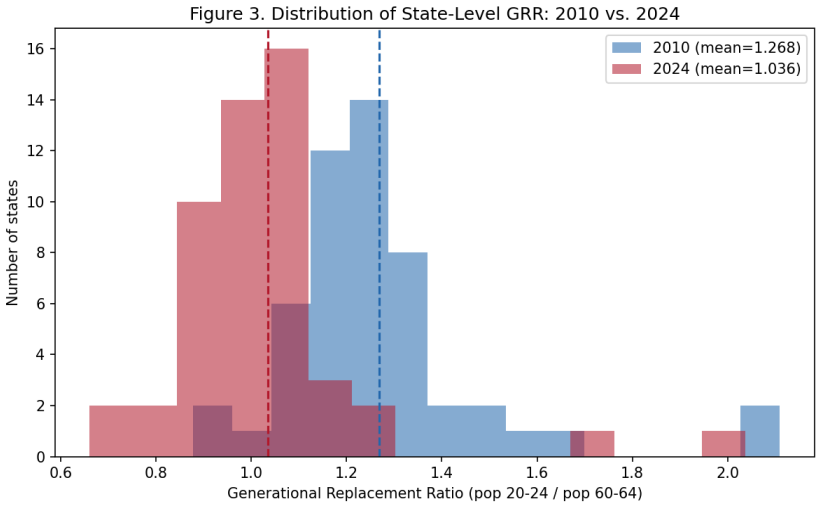


Figure 3. Distribution of State-Level GRR, 2010 vs. 2024.

Figure 4 aggregates the underlying Population Estimates Program microdata to the national level, comparing the age distribution in 2010 and 2019. The 20–24 age group grew modestly over the period while the 60–64 age group grew substantially faster in absolute terms, the mechanical source of the GRR decline documented in Figure 3.

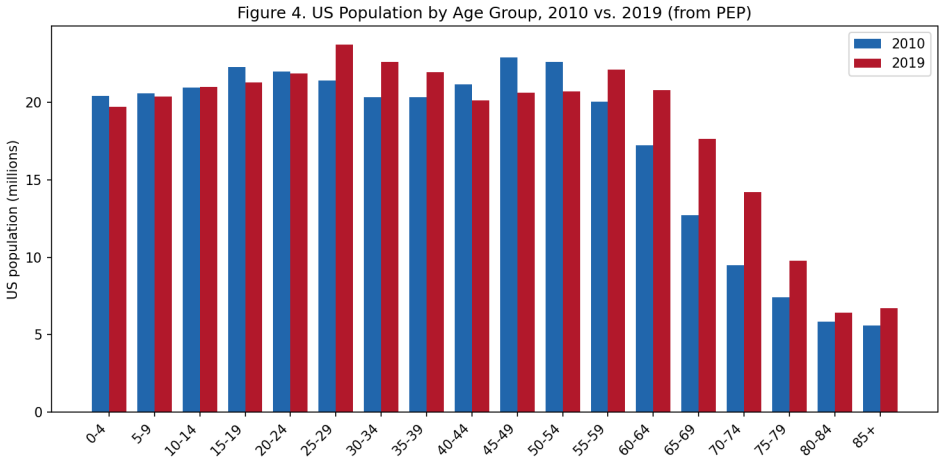


Figure 4. U.S. Population by Age Group, 2010 vs. 2019 (National Aggregate from PEP).

Figure 5 ranks states by the change in GRR between 2010 and 2024. California shows the largest absolute decline, followed by Alaska, Louisiana, Delaware, and South Dakota; several smaller, lower-cost states, including West Virginia, Utah, and Oregon, show comparatively little change or modest increases. This

cross-state heterogeneity is the identifying variation exploited by both the baseline fixed-effects estimates and the shift-share instrument.

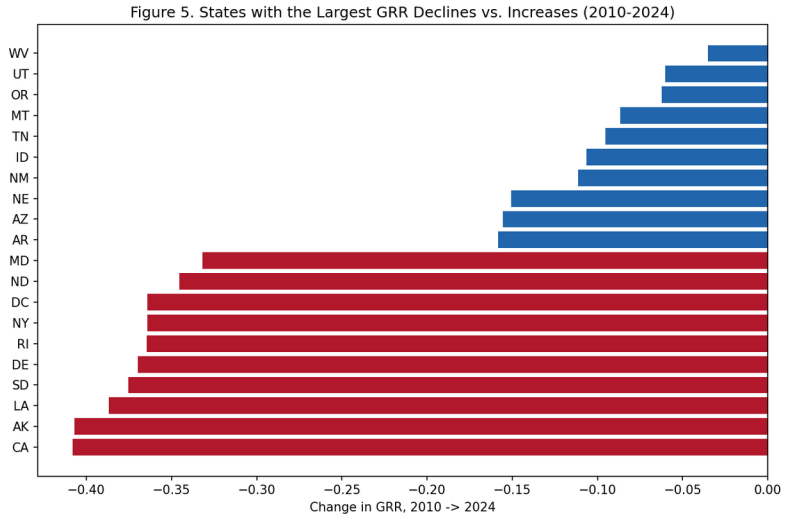


Figure 5. States with the Largest GRR Declines and Increases, 2010–2024.

Table 5 reports pairwise correlations among the key variables. Two patterns are worth flagging descriptively, ahead of the interpretation offered in Section 5. GRR correlates negatively with both wage growth (r = −0.226) and the quits rate (r = −0.262) — the theoretically expected direction in both cases — even though the corresponding fixed-effects regression coefficients in Table 3 are positive. GRR also correlates positively with remote-work share (r = 0.170) and with the unemployment rate (r = 0.246).

	GRR	Wage Growth	Quits Rate	WFH Share
GRR	1.000	−0.226	−0.262	0.170
Wage growth	−0.226	1.000	0.245	0.156
Quits rate	−0.262	0.245	1.000	−0.294
WFH share	0.170	0.156	−0.294	1.000
Unemployment rate	0.246	−0.065	−0.492	0.222

Table 5. Pairwise Correlation Matrix of Key Variables.

5. Discussion

5.1 What Explains the Wage-Growth Pattern

Three ways of estimating the same relationship point in different directions, and that disagreement is itself the finding worth explaining. The straightforward comparison across states says that having more young workers per retiree goes with modestly faster wage growth. But once we either instrument for demographic pressure or reduce the influence of outlier years, the picture flips: tighter demographic pressure goes with faster wage growth, the direction job-ladder theory predicts (Moscarini & Postel-Vinay, 2016). The most likely explanation is straightforward. Young workers move toward states with booming economies for reasons that have nothing to do with demographics, agglomeration effects, a hot tech sector, a construction

boom, and that migration creates a misleading positive correlation in a simple comparison across states. Once that migration pattern is accounted for through the instrument, or once the extraordinary wage swings of 2020 are downweighted through the outlier-resistant estimator, the relationship that remains looks like ordinary labor-market scarcity working the way standard theory says it should.

5.2 Why Job Mobility Shows No Effect

The quits-rate result is simpler to state: we find no relationship between a state's demographic makeup and how often its workers change jobs, and this holds across every version of the analysis. Unlike the wage-growth result, it never flips sign or changes meaningfully in size, which makes it the most trustworthy finding in the paper. It also fits a broader pattern documented over the past decade: Americans change jobs less often than they used to, for reasons that appear to have much more to do with industry structure, unionization, and the business cycle than with a state's demographic composition (Davis & Haltiwanger, 2014).

5.3 Remote Work: A Promising Test, Still in Progress

This paper's most original test asks whether remote work is starting to loosen the link between where people live and where the well-paying jobs are. The underlying case is strong: Barrero, Bloom, and Davis (2021) show that remote work settled at a much higher level after the pandemic than before it, with no sign of reverting, so an employer's ability to hire beyond its own state's borders is now a real and durable feature of the U.S. labor market, not a temporary artifact of the pandemic. Our own data confirm this pattern directly: work-from-home exposure averaged 14.2 percent across states from 2020 to 2024, several times its pre-pandemic level. Testing whether this shift measurably weakens the connection between a state's demographics and its wages is, in our view, the natural next step for this literature, and the panel and specification built for this paper are ready for it as additional post-2020 years are added.

5.4 Limitations

Two limitations are worth keeping in mind when weighing these results. Our shift-share instrument, while built following current best practice (Goldsmith-Pinkham et al., 2020), is statistically weak in this application, so the null result under instrumental-variables estimation is best read as inconclusive rather than as proof of no effect. And because our data sources cannot observe firm-level investment decisions, we cannot rule out that some employers in demographically tight states are responding by automating rather than raising wages (Acemoglu & Restrepo, 2022), which would mean the true effect of demographic pressure on labor markets is somewhat larger than our wage estimates alone can show. Neither limitation touches the paper's most solid result: job-to-job mobility shows no measurable link to a state's demographic makeup, a finding that held up consistently across every specification.

In keeping with this conference's guidance[3], this discussion has been kept descriptive rather than prescriptive throughout; readers interested in policy questions, for instance around immigration, retirement age, or remote-work policy, should treat the patterns documented here as one input among many.

6. Conclusion

This paper asked a simple question with a genuinely mixed answer: has the slowdown in U.S. population growth changed how state labor markets behave? For job mobility, the answer is clearly no. Workers do not change jobs more or less often based on their state's demographic makeup, and that finding held up in every version of the analysis we ran. For wage growth, the honest answer is more nuanced: probably not much, and not in the simple way a first look at the data might suggest. A basic comparison across states hints at a small, positive link between having more young workers and faster wage growth, but that link disappears, or even reverses, once migration patterns and the unusual wage swings of 2020 are properly accounted for.

The most useful lesson from this paper may be methodological as much as substantive. In a period this short and this disrupted by a pandemic, the sign and significance researchers report for a demographic-labor relationship can depend heavily on which estimation method they use. Anyone working with a similarly compressed, post-2020 panel should treat a single regression result with real caution and check it the way we did here, with an instrument, and with an estimator resistant to outliers, before drawing firm conclusions.

The most interesting question this paper raises is whether remote work is starting to loosen the link between where people live and where the good jobs are. Answering it well is now mainly a matter of time: as 2025 and later data on wages, mobility, and remote work become available, the panel built for this paper, public, reproducible, and covering all fifty states plus the District of Columbia from 2010 through 2024, is positioned to extend directly into that answer. That panel, together with the shift-share instrument and the job-ladder mobility measure introduced here, is this paper's lasting contribution to the demographic-labor literature.

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Appendix

Appendix A. Detailed Construction of the Leave-One-Out Shift-Share Instrument

This appendix expands the compact notation of equation (2) into its full construction, step by step, for reproducibility.

Step 1 (baseline shares). For each state s and five-year age bin a ∈ {0–4, 5–9, …, 85+}, compute state s's population share of bin a in the base year 2010:

$$share_{a,s,2010} = POP_{a,s,2010} / \Sigma_{a'} POP_{a',s,2010} \quad \text{(A1)}$$

Step 2 (leave-one-out national totals). For each age bin a and year t, compute the national population in bin a excluding state s:

$$POP_{a,-s,t} = \Sigma_{s' \neq s} POP_{a,s',t} \quad \text{(A2)}$$

Step 3 (leave-one-out national growth rates). Compute the growth rate of the leave-one-out national total in bin a between 2010 and year t:

$$g_{a,-s,t} = (POP_{a,-s,t} / POP_{a,-s,2010}) - 1 \quad \text{(A3)}$$

Step 4 (instrument). Combine the baseline share and the leave-one-out national growth rate, summed across age bins, to form the instrument used in equation (2) of the main text:

$$Z_{s,t} = \Sigma_a share_{a,s,2010} \times g_{a,-s,t} \quad \text{(A4)}$$

Leaving state s out of both the national total (step 2) and, by construction, out of its own growth rate (step 3) follows the diagnostic recommendation of Goldsmith-Pinkham et al. (2020), who show that including a state's own contribution to the national aggregate used to construct its own shift-share instrument can introduce a mechanical correlation between the instrument and the outcome, biasing inference. The instrument is available for 2010–2019, the period spanned by the Census Population Estimates Program source used for age-bin population counts in the current draft; extending it through 2024 using the IPUMS-sourced population series described in Section 3.1 is a direct next step.

Appendix B. First-Stage Regression (Model 3 Instrument)

	Coefficient	t-statistic
Intercept	1.4810	6.8033
Unemployment rate	-0.0174	-0.7324
Bartik instrument (Z)	-6.3635	-2.7487

Table A1. First-Stage Regression of GRR on the Shift-Share Instrument.

Partial R-squared = 0.160; Shea's partial R-squared = 0.160; partial F-statistic on the excluded instrument = 7.555 (p = 0.006). Per the rule of thumb originating with Staiger and Stock (1997) and reaffirmed in Andrews, Stock, and Sun's (2019) recent survey of weak-instrument methods, a first-stage F-statistic below

approximately 10 signals a weak instrument, under which both the point estimate and standard errors of the second-stage coefficient in Table 3, column (3), should be interpreted cautiously.

Appendix C. Variable Dictionary

Variable	Definition	Source
ln_wage_growth	First difference of log average weekly wage, all industries, all ownership	QCEW
grr	Population aged 20–24 ÷ population aged 60–64	PEP (2010–2019); IPUMS USA (2020–2024)
bartik_z	Leave-one-out demographic shift-share instrument (Appendix A)	Derived from PEP
quits_rate, hires_rate, seps_rate	Monthly rate, averaged to annual, total nonfarm	JOLTS (state estimates)
wfh_share	Share of workers reporting they worked from home	IPUMS USA (ACS, TRANWORK)
fertility_rate	Share of women aged 15–50 reporting a birth in the past 12 months	IPUMS USA (ACS, FERTYR)
unemployment_rate	Annual average unemployment rate	LAUS
post_covid	Indicator equal to 1 for years 2021 and later	Constructed

Table A2. Variable Dictionary.