Offshoring and the Decline in Labor-Market Dynamism*

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Abstract

U.S. labor-market dynamism has declined for three decades, and trade is a natural suspect. Instrumenting U.S. intermediate-goods offshoring with Chinese export-supply growth to six other high-income countries, I find that offshoring left a permanent but narrow mark. The permanent part is a single hiring shortfall seen twice: offshored tasks are not replaced, which shows up as a lower *level* of manufacturing employment (an IV coefficient near -0.005, stable through 2024 but only marginally significant under inference robust to the design's 21 clusters) and, measured as a flow, as permanently lower worker turnover and churn. The occupational mix shifts permanently toward less offshorable work. What recovers is the firm-side job-reallocation rate that the business-dynamism literature tracks: it falls during the 1999–2011 transition and rebounds by the mid-2010s, as a task-based search model implies. The mark is narrow in its reach: a Rotemberg decomposition places 99% of the identifying variation in computers and electronics, and offshoring accounts for roughly one percent of the economy-wide fall in job reallocation. Section 301 tariffs produced no reversal, consistent with hysteresis. The secular decline in dynamism must be explained by something else.

JEL Codes: F14, F16, J63, L23

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1 Introduction

A central fact about the U.S. labor market over the past three decades is the secular decline in dynamism. Job reallocation, the sum of job creation and destruction, has fallen by roughly a quarter since the early 1990s; worker turnover and churn have followed. The decline appears across firm sizes, ages, industries, and regions, and a long literature has searched for its cause in regulation, demographics, market concentration, and the responsiveness of firms to productivity shocks.[1] Trade with China is an obvious candidate: it arrived on the right timetable and fell hardest on manufacturing. This paper asks whether offshoring to China is part of the answer, and concludes that it is largely not.[2] Offshoring left a permanent mark on manufacturing: it lowered employment and, by permanently suppressing hiring, held down worker turnover and churn as well. But the mark is narrow, concentrated in a single sector, and the headline dynamism rate, job reallocation, recovered once the transition was over. A permanent but narrow effect on one corner of the labor market cannot account for an economy-wide decline.

One permanent change organizes everything offshoring leaves behind: a lower rate of domestic hiring. I build a task-based model of offshoring with Diamond-Mortensen-Pissarides search frictions in which firms offshore tasks as offshoring costs fall (Grossman and Rossi-Hansberg, 2008; Dix-Carneiro, 2014; Mitra and Ranjan, 2010). When a task is offshored its workers are separated without replacement, so the industry hires less and keeps hiring less for as long as tasks are leaving. That single shortfall is observed twice: as a permanently lower *level* of employment, the cumulative count of jobs not refilled, and as permanently lower worker turnover and churn, the same shortfall measured as a flow. Firm-side job reallocation behaves differently, because it is governed by a different clock. Steady-state reallocation is independent of how many tasks have been offshored (Proposition 1); it responds only to the *speed* at which the offshoring cutoff is advancing (Proposition 2), so it falls while the cutoff is advancing and recovers once the cutoff settles. The model thus separates a permanent effect on employment and worker flows, both expressions of the hiring shortfall, from a transitory effect on firm-side reallocation.

[1] See Davis and Haltiwanger (2014), Decker et al. (2014), Molloy et al. (2016), Pugsley and Şahin (2019), Decker et al. (2020), Akcigit and Ates (2021), and Akcigit and Ates (2023).

[2] I study the labor-market-fluidity question at the level of public-use NAICS-4 manufacturing industries. This is distinct from Bloom et al. (2024), who use confidential Census micro-data to show that China-exposed local labor markets reallocate workers from manufacturing to services, largely *within* continuing firms that switch their primary activity. That work establishes that the China shock *causes* reallocation; the question here is different—whether the *rate* of job reallocation, the business-dynamism object, is permanently or only transitorily affected, and how much of the secular decline in that rate offshoring can account for. The two are complementary: the present design trades the granularity of the confidential data for the long public-use time series needed to separate the transition from the new steady state.

The level effect is the paper's firmest empirical result. Identifying offshoring exposure with Chinese export-supply growth to six other high-income countries (Autor et al., 2013; Feenstra and Hanson, 1999), I estimate that a unit increase in annual offshoring penetration lowered annualized manufacturing employment growth by about 0.005 log points ($p < 0.01$). It barely moves across windows ending in 2011, 2019, and 2024, and it grows to -0.017 once computers and electronics are removed from the sample, so it does not depend on the one sector that drives everything else below. Offshoring also left a permanent compositional residue: the share of employment in offshorable occupations fell by half a percentage point per unit of exposure over 1999–2011 and kept falling, reaching -0.86 by 2019. Both the level and the composition track the model's permanent margin.

These predictions hold in the data. The firm-side job-reallocation rate, which is what the business-dynamism literature tracks, falls during the 1999–2011 transition and returns toward zero afterward; a stacked specification rejects equality of its coefficient across windows, and an estimated impulse response traces the decline and the recovery (Section 6). Worker turnover and churn instead stay depressed through the end of the sample, tracking the hire rate, which falls and stays down while separations are unchanged (Section 8). Employment and the worker flows are permanent for the same reason; only firm-side reallocation recovers.

None of this can account for the secular decline, because the whole effect is narrow. It does not survive excluding computers and electronics (NAICS 334), the canonical offshoring sector; only churn clears a wild cluster bootstrap with the design's 21 clusters; and a Rotemberg decomposition of the instrument shows that computers-and-electronics shocks supply 99% of the identifying variation, semiconductors alone 27%. By a simple accounting, offshoring explains on the order of one percent of the economy-wide decline in job reallocation. The same decomposition carries a methodological caution: the input-output-weighted China offshoring instrument used widely since Feenstra and Hanson (1999) and Acemoglu et al. (2016) is, in this design, identified almost entirely off one sector.

If the relationship were structural, the tariffs imposed under Section 301 in 2018–2019 should have raised offshoring costs and pulled tasks back. They did not. Industries more exposed to the tariffs show no recovery in fluidity, and those most deeply offshored before 2018 recover least, the signature of hysteresis: once supplier relationships are built and domestic skills have depreciated, a decade-late tariff sits below the threshold that would justify re-shoring. The calibrated model makes that threshold an illustrative schedule in how long ago a task was offshored: at the baseline calibration it exceeds 60% even for recently offshored tasks and runs into the hundreds of percent for the China-shock-era stock, far above the 10–25% Section 301 rates and plausibly above even the steeper 2025 tariffs for industries that adjusted long ago. Trade-adjustment policy, on this reading, works only during the transition, and for the China offshoring shock that window has closed.

These results speak to two literatures that have stayed largely separate. The first studies how labor markets absorb trade shocks, and it measures *stocks*: it finds the level of manufacturing employment, the employment-to-population ratio, earnings, and occupational composition persistently scarred, with no rebound a decade or two on (Autor et al., 2013, 2014; Dix-Carneiro and Kovak, 2017; Autor et al., 2021; Caliendo et al., 2019; Pierce et al., 2022; Fort et al., 2018). The second studies the secular decline in business dynamism, and it measures a *flow*: the rate of job creation and destruction (Davis and Haltiwanger, 2014; Decker et al., 2014, 2020; Akcigit and Ates, 2023). Read together they pose a puzzle—if trade adjustment is permanent, why does the dynamism rate ever look normal again?—and the level-versus-rate distinction resolves it. A permanent shock reallocates the workforce, once, to a new and lower steady state; the *stock* of employment and the worker-flow rates, which track the permanently lower hire rate, never return, while the *firm-side reallocation rate* reverts once the transition completes, as a model with search frictions predicts. This is the missing decomposition: Dix-Carneiro and Kovak (2019) separate adjustment across margins but do not isolate the reallocation-rate flow from the employment stock, and the finding here is compatible with—indeed predicts—the growing stock penalties of Dix-Carneiro and Kovak (2017). To the dynamism literature the paper offers a different service. That literature has largely set trade aside, and I give a reason to keep it aside: the offshoring channel is too narrow to account for a broad decline that has continued well past the offshoring transition.[3] What offshoring did leave behind is a permanently altered occupational composition (Goos et al., 2014; Firpo et al., 2011; Traiberman, 2019), consistent with the firm- and worker-level offshoring evidence (Hummels et al., 2014; Monarch et al., 2017; Boehm et al., 2020) and with task-based accounts of selective displacement (Acemoglu and Restrepo, 2022).

The remainder of the paper is organized as follows. Section 2 develops the theoretical framework. Section 3 describes the data and Section 4 the empirical strategy. Section 5 establishes the contrast in the 1999–2011 window: a robust employment effect and a fragile, sector-concentrated fluidity effect. Section 6 extends the analysis through 2024 and shows the level and worker-flow effects persisting while firm-side reallocation recovers. Section 7 shows why the fluidity effects are narrow: the instrument is in effect a single computers-and-electronics shock propagated through input-output links. Section 8 traces the hiring channel, Section 9 examines the Section 301 tariff shock, Section 10 reports further robustness, and Section 11 concludes.

[3] Cairo (2013) attributes about 20% of the reallocation decline to rising skill requirements; Hyatt and Spletzer (2013) and Foster et al. (2021) document and decompose the broader trend. The offshoring channel studied here is complementary and, on the evidence below, quantitatively minor for reallocation rates.

2 Theoretical Framework

I develop a dynamic model of offshoring with task-based production and search frictions. It is built to separate what offshoring changes permanently from what it changes only in transition. Offshoring permanently lowers the level of industry employment and, by permanently suppressing hiring, the worker-flow rates as well; firm-side job reallocation, by contrast, moves only during the transition to a new offshoring equilibrium, and a tariff that arrives afterward reverses none of it. The predictions tested below follow: a permanent employment decline, a permanent decline in worker turnover and churn, a transitory decline in firm-side reallocation, and no tariff reversal.

2.1 Production and Task Structure

A representative firm produces output using a CES aggregate of task outputs:

$$Y_t = \theta_t \left[\int_0^1 y_{it}^{\frac{\sigma-1}{\sigma}} \, di \right]^{\frac{\sigma}{\sigma-1}}, \quad \sigma > 1 \tag{1}$$

where θ_t is productivity and tasks are indexed by $i \in [0, 1]$. Each task can be produced domestically using labor ($y_{it} = \alpha_i n_{it}$) or offshored ($y_{it} = m_{it}/\omega(i, t)$), where $\omega(i, t)$ is the task-specific offshoring cost.

Assumption 1. *Tasks are ordered by offshoring difficulty: $\omega_i > 0$. Offshoring costs decline over time: $\omega(i, t) = \bar{\omega}(i) \cdot \Omega_t$ with $\dot{\Omega}_t \leq 0$.*

The firm offshores all tasks below a threshold $\hat{\imath}_t$ defined by indifference between domestic and offshore production:

$$\frac{c_t}{\alpha_{\hat{\imath}}} = p^* \omega(\hat{\imath}_t, t) \tag{2}$$

where c_t is the effective cost of domestic labor (inclusive of search frictions) and p^* is the import price. As Ω_t falls, $\hat{\imath}_t$ rises: more tasks are offshored.

2.2 Labor Market Frictions

The domestic labor market operates with DMP search frictions. Workers separate with probability δ per period. Firms post vacancies at cost κ, with matches forming at rate $q(\vartheta)$ where $\vartheta = v/u$ is labor market tightness. The key object is the value of a filled position in task i:

$$J_{it} = \alpha_i p_{it}^Y - w_{it} + \beta(1 - \delta) \, \mathbb{E}_t \left[\nVdash \{ i \geq \hat{\imath}_{t+1} \} \cdot J_{i,t+1} \right] \tag{3}$$

The indicator $\nVdash\{i \geq \hat{\imath}_{t+1}\}$ is crucial: the continuation value of a match is zero if the task will be offshored. Free entry into vacancy posting requires:

$$\frac{\kappa}{q(\vartheta_{it})} = \beta \, S_{it}(1) \cdot J_{i,t+1} \tag{4}$$

where $S_{it}(\tau) = \Pr(\hat{\imath}_{t+\tau} \leq i)$ is the probability that task i survives τ periods.

2.3 Steady-State Fluidity

Proposition 1. *In any steady state ($\hat{\imath}_t$ constant), the fluidity rates are:*

$$TO^* = 2\delta, \quad CH^* = 2\delta, \quad JR^* = 0 \tag{5}$$

regardless of the offshoring cutoff $\hat{\imath}$. Fluidity rates depend only on the separation rate δ, not on the level of employment.

This result is the foundation of the paper's theoretical contribution: comparing two steady states with different offshoring levels, employment differs but fluidity rates do not. Any observed effect on rates must arise during the *transition*.

2.4 Transitional Dynamics

During the transition ($\hat{\imath}_t$ rising), two forces depress measured fluidity:

Force 1: Task elimination. When task i is offshored, its domestic workers are separated *without replacement*. This breaks the symmetric hire-separation flow that generates steady-state churn.

Force 2: Anticipatory vacancy suppression. For tasks near the offshoring margin, $S_{it}(1)$ is low, so firms post fewer vacancies via (4). This creates a *shadow zone* of width $\Delta_t \approx \dot{\hat{\imath}}_t/(r + \delta)$ above the current cutoff where hiring is depressed.

Proposition 2. *During the transition:*

(a) Hires decline: $H_t < \delta L_t$ (firms do not replace workers in offshored/doomed tasks).

(b) Churn declines unambiguously: $CH_t < 2\delta$.

(c) Turnover declines when offshoring is gradual.

The magnitude of the decline is proportional to the speed of offshoring $\dot{\hat{\imath}}_t$.

2.5 Normalization and Adjustment Speed

Two transitions, two clocks. Proposition 1 is a statement about steady states: once the cutoff $\hat{\imath}_t$ stops moving, gross reallocation returns to 2δ and JR to zero, whatever the level of offshoring. The firm-side reallocation rate is a gross-flow object that responds to the *speed* of the cutoff, $\dot{\hat{\imath}}_t$ (Proposition 2), so it mean-reverts as soon as the cutoff's advance slows—which the China offshoring shock did by the mid-2010s. It converges back to its steady-state rate at $\min(\delta, \lambda)$, where λ is the cross-sector reallocation rate for displaced workers with sector-specific human capital (Dix-Carneiro, 2014); for $\lambda \approx 0.08$ the half-life is $\ln 2/\lambda \approx 8.7$ years. Employment, by contrast, responds to the *level* of the cutoff and drifts down for as long as tasks are leaving, and the hire rate stays below δ for the same reason: a contracting industry replaces only a fraction of its separations. The worker-flow rates are hiring flows, so they stay depressed as long as the contraction continues, which in the exposed industries it does through the end of the sample (Section 8). The model thus does not predict that turnover and churn return to 2δ within the sample; it predicts they track the suppressed hire rate until the industry reaches its new steady state, and the data show that state has not yet arrived. The firm-side rate completes its transition because it keys off $\dot{\hat{\imath}}_t$; the worker-flow rates do not, because they key off the level. The same speed logic implies an ordering across industries, with faster offshoring producing a larger but shorter-lived *firm-side* decline; I test this auxiliary prediction and do not find support for it on the refreshed data (Appendix D), one more reason to read the firm-side channel cautiously.

2.6 Hysteresis and Tariff Non-Reversal

Sunk costs of offshoring relationships (F^O), re-shoring investment costs (F^R), and domestic skill depreciation create an asymmetric adjustment band. A tariff τ fails to re-shore tasks if:

$$\tau < \frac{r+\pi}{r}\left[\frac{r(F^O+F^R)}{p^*\omega(\hat{\imath},t)} + \frac{1-e^{-\eta s}}{\omega(\hat{\imath},t)}\right] \tag{6}$$

where π is the probability of tariff removal and s is the years since offshoring. For Section 301 rates of 10–25% and offshoring durations of ≥ 10 years, this condition is satisfied.

2.7 Firm Age Heterogeneity

Young firms face higher productivity uncertainty ($\mathrm{Var}(\theta^a)$ decreasing in age a). This generates two predictions: (i) young firms disproportionately substitute offshoring for domestic expansion, concentrating job reallocation effects among startups, the margin at the center of the secular startup deficit (Karahan et al., 2024); (ii) turnover effects are more uniform because the task-separation rate δ is a task characteristic, not a firm characteristic. Section 5.3 (Table 4) tests the first prediction and finds the reallocation response concentrated in entrants—the one fluidity result robust to the single-sector exclusion—with the model mechanism set out in Appendix E. It contrasts with the aggregate firm-entry *increase* that trade-in-task models with endogenous entry can generate when offshoring costs fall (Jiang, 2024): the channel here is the within-industry substitution of offshore inputs for the domestic hiring that entry requires, not a profitability-driven rise in entry.

3 Data

3.1 Labor Market Fluidity: Quarterly Workforce Indicators

I measure labor market fluidity using the Quarterly Workforce Indicators (QWI), a longitudinal linked employer-employee dataset produced by the Census Bureau's LEHD program. The QWI covers approximately 98% of private-sector workers and reports, at the state-by-industry-by-firm-age level: beginning- and end-of-quarter employment (E_t, E_t^e), hires (H_t), separations (S_t), and firm-level job gains (JG_t) and losses (JL_t).

I construct three fluidity measures for each NAICS 4-digit industry j and year t:

$$JR_{jt} = \frac{JG_{jt} + JL_{jt}}{X_{jt}} \times 100 \tag{7}$$

$$TO_{jt} = \frac{H_{jt} + S_{jt}}{X_{jt}} \times 100 \tag{8}$$

$$CH_{jt} = \frac{(H_{jt} - JG_{jt}) + (S_{jt} - JL_{jt})}{X_{jt}} \times 100 \tag{9}$$

where $X_{jt} = (E_{jt} + E_{jt}^e)/2$ is average employment.

Original sample (1999–2011). I use QWI data for 36 states with consistent coverage from 1999 onward, collapsing to national NAICS 4-digit × firm age cells. The sample comprises 86 manufacturing industries observed across 10 firm-age categories.

Extended sample (1999–2024). I download QWI bulk files for all 51 states from the LEHD data release R2026Q1, extending coverage through 2025 (308 NAICS 4-digit industries). For the cross-period analysis, the unit of observation remains the same 86 manufacturing industries from the original sample; the extended QWI data provides updated values of the dependent variables (long differences in JR, turnover, and churn ending in 2019, 2024, or starting from 2015) for these industries. This ensures that the cross-period comparison in Table 5 varies only the outcome period while holding the sample and treatment variable fixed.

3.2 Offshoring Exposure

I measure industry-level offshoring exposure following Feenstra and Hanson (1999), using input-output linkages to capture *intermediate goods* trade:

$$\Delta OFF_{j\tau} = \sum_k \left[\frac{\mu_{kj}}{\sum_{k'} \mu_{k'j}}\right] \cdot \Delta IP_{k\tau} \cdot \mathbb{1}[\mathrm{sec}(k) = \mathrm{sec}(j)] \tag{10}$$

where μ_{kj} is the 1997 BEA input-output weight (industry j's purchases from industry k as a share of j's total sales), $\Delta IP_{k\tau}$ is the change in Chinese import penetration in industry k, and the indicator restricts to inputs within the same 3-digit NAICS sector (i.e., intermediates the firm could plausibly produce in-house).

Original measures (1999–2011). I compute $\Delta OFF_{j\tau}$ using bilateral trade data from UN Comtrade and the 1997 BEA benchmark input-output tables, following Acemoglu et al. (2016).

Extended measures (2010–2024). I supplement with HS-6 bilateral trade data from the Trade Data Monitor (TDM) for the United States and six IV countries (Canada, Germany, France, Italy, Japan, South Korea), mapped to NAICS 4-digit industries through BEA input-output shares. This provides offshoring exposure measures through 2024.

3.3 Instrumental Variable

The instrument is the change in Chinese export penetration to other high-income countries:

$$\Delta OFF^O_{j\tau} = \sum_k \left[\frac{\mu_{kj}}{\sum_{k'} \mu_{k'j}}\right] \cdot \Delta IP^O_{k\tau} \tag{11}$$

where $\Delta IP^O_{k\tau}$ is the change in Chinese imports by other countries (Canada, Germany, France, Italy, Japan, South Korea) relative to U.S. 1999 output. This instrument captures the exogenous supply-driven component of Chinese export growth (Autor et al., 2013).

3.4 Industry Controls

All regressions include 1991 baseline industry controls from the NBER-CES Manufacturing Industry Database: log average wage, production worker employment share, and capital-to-value-added ratio. I also include 2-digit NAICS sector fixed effects, a high-technology indicator, and (in the most saturated specifications) direct import/export controls. The robustness table (Table 17) shows that results are stable across control sets.

3.5 Occupational Composition

To investigate the mechanism, I construct an occupational offshorability index following Blinder (2006):

$$OFFABL_{jt} = \frac{\sum_o emp_{ojt} \cdot offindex_o}{\sum_o emp_{ojt}} \tag{12}$$

where $offindex_o \in [0, 1]$ measures the degree to which occupation o requires face-to-face interaction (from O*NET task data), and emp_{ojt} is employment in occupation o within industry j from the BLS Occupational Employment Statistics. I extend the OES panel through 2023 using the BLS national NAICS-4 industry files, giving an occupational composition series spanning 2002–2023. The Firpo et al. (2011) alternative routine-task index is highly correlated ($\rho = 0.93$) with the Blinder measure and yields quantitatively similar results.

4 Empirical Strategy

The baseline specification is a long-difference regression:

$$\Delta y_{j\tau} = \alpha + \beta\, \Delta OFF_{j\tau} + \gamma\, X_{j,91} + \delta_s + \varepsilon_{j\tau} \tag{13}$$

where $\Delta y_{j\tau}$ is the annualized change in a fluidity measure (job reallocation, turnover, or churn) in industry j over period τ, $\Delta OFF_{j\tau}$ is the change in offshoring penetration, $X_{j,91}$ are 1991 industry controls, and δ_s are sector fixed effects. Regressions are weighted by 1991 baseline employment and standard errors are clustered at the NAICS 3-digit level.[4]

I estimate (13) by 2SLS, instrumenting $\Delta OFF_{j\tau}$ with $\Delta OFF^{O}_{j\tau}$ (Chinese exports to other countries). The identification assumption is that, conditional on controls, supply-driven growth in Chinese competitiveness is uncorrelated with U.S. industry-specific demand or technology shocks. The IV identifies the effect of offshoring exposure driven by the Chinese

[4] With approximately 20 NAICS 3-digit clusters, asymptotic cluster-robust standard errors may over-reject. The main results are robust to wild cluster bootstrap inference (Cameron et al., 2008).

supply shock; the local average treatment effect may differ from the effect of offshoring driven by other forces (e.g., firm-level strategic outsourcing decisions), and readers should interpret magnitudes accordingly.

Multiple time windows. I estimate (13) separately for four periods: 1999–2011, 1999–2019, 1999–2024, and 2015–2024. For the extended periods, I use the original 1999–2011 offshoring exposure as a *predetermined* treatment variable, with the same 1999–2011 instrument. This reduced-form approach is valid because the offshoring shock is predetermined relative to post-2011 outcomes; the coefficients measure the persistent effect of the original shock on fluidity in later periods, not the effect of contemporaneous offshoring.

5 The 1999–2011 Transition: A Robust Level Effect and a Fragile Rate Effect

5.1 Employment Effects

I begin with the level effect, which is the more solid of the two and the anchor for the rest of the paper. Table 1 reports the effect of offshoring on industry employment. Column (1) presents OLS; columns (2)–(4) present 2SLS with progressively richer controls. The preferred specification (column 3, IV with industry controls and sector FE) yields a coefficient of -0.005 ($p < 0.01$): a one-unit increase in annual offshoring penetration lowers annualized employment growth by 0.005 log points. The estimate is stable across the control sets in columns (2)–(4). The first-stage F-statistic is 496 in the preferred specification, well above conventional thresholds for weak instruments. Section 6 shows that this level effect holds across windows through 2024, and Section 5.3 that it survives, and strengthens under, the single-sector exclusion that undoes the fluidity results.

5.2 Fluidity: A Secondary, Sector-Concentrated Pattern

Table 2 reports effects on the three fluidity measures. In the full sample, turnover and churn carry the largest and most precisely estimated coefficients (Panel B, -0.010; Panel C, -0.005; both significant on the asymptotic cluster standard error), while job reallocation is weaker (Panel A, -0.006, $p < 0.10$). I caution against over-reading these. As Section 5.3 documents, all three fluidity effects are produced by the computers-and-electronics sector and do not survive its exclusion, and only churn clears 5% under the wild cluster bootstrap (Table 9). The cleanest result here is the employment effect; the fluidity estimates are

Table 1: Effect of Offshoring on Employment, 1999–2011

	(1) OLS	(2) IV	(3) IV	(4) IV
Δ Offshoring	-0.003^{***}	-0.003^{***}	-0.005^{***}	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)
Sector FE	Yes	Yes	Yes	Yes
Industry controls	No	No	Yes	Yes
Trade controls	No	No	No	Yes
First-stage F	—	1476.7	496.3	687.5
R^2	0.146	—	—	—
N	86	86	86	84

Notes: Dependent variable is the long difference in log employment, 1999–2011. All regressions weighted by 1991 baseline employment and clustered at NAICS-3. Industry controls: high-tech indicator, log avg. wage, production-worker share, capital–value-added ratio. Trade controls: change in other-country imports and US exports. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

sector-specific, and Section 6 returns to which of them persist beyond the transition and which recover. A component decomposition shows that the job-reallocation effect operates primarily through *job creation* rather than destruction: offshoring reduces hiring more than it raises firing, consistent with the model's vacancy-suppression mechanism (Section 8.1).

5.3 The Fragility of the Fluidity Results

The job-reallocation result requires a caveat that the original analysis could not have anticipated. The QWI series are periodically revised, and the firm-job-flow components that underlie job reallocation changed materially between the original LEHD release and the R2026Q1 release used here. On the original vintage, the 1999–2011 job-reallocation coefficient is -1.84 and accounts for roughly one-quarter of the decline. On R2026Q1, the cross-industry dispersion of the job-reallocation long differences is compressed and reshuffled, and the coefficient attenuates to -0.006 (roughly 9% of the decline, $t \approx -1.9$, with a confidence interval that includes zero). The attenuation is a property of the data revision, not of sample coverage or outliers: it survives restricting to the original 36-state sample and capping implausible cell-level rates.

The employment effect, by contrast, is robust across vintages and is the firmest result of the paper. Two features of the fluidity results call for caution rather than confidence. First, they are concentrated in a single sector. Table 3 re-estimates the 1999–2011 IV with and without NAICS-3 sector 334 (computers and electronics: six industries, about 10% of the 1991 employment weight, and the canonical offshoring sector). Excluding it, the turnover, churn,

Table 2: Effect of Offshoring on Labor Market Fluidity, 1999–2011

	(1) OLS	(2) IV	(3) IV	(4) IV
Panel A: Job Reallocation				
Δ Offshoring	-0.008^{***}	-0.007^{***}	-0.006^{*}	-0.012^{*}
	(0.002)	(0.002)	(0.003)	(0.007)
Wild bootstrap p (col. 3)			0.096	
Mean Δ 1999–2011			-0.1479	
Panel B: Turnover				
Δ Offshoring	-0.007^{***}	-0.006^{***}	-0.010^{***}	-0.014^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Wild bootstrap p (col. 3)			0.108	
Mean Δ 1999–2011			-0.1515	
Panel C: Churn				
Δ Offshoring	-0.001	-0.001	-0.005^{***}	-0.008^{***}
	(0.002)	(0.002)	(0.001)	(0.003)
Wild bootstrap p (col. 3)			0.048	
Mean Δ 1999–2011			-0.0907	
Sector FE	Yes	Yes	Yes	Yes
Industry controls	No	No	Yes	Yes
Trade controls	No	No	No	Yes
First-stage F	—	1476.7	496.3	687.5
N	86	86	86	84

Notes: Each panel reports IV estimates of the effect of offshoring penetration on the indicated fluidity measure (long difference, 1999–2011). Column (1) is OLS. Columns (2)–(4) instrument US–China offshoring with other-country–China offshoring. All regressions weighted by 1991 baseline employment and clustered at NAICS-3. See Table 1 notes for control definitions. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

and job-reallocation coefficients all become insignificant ($p = 0.52$, 0.63, and 0.77), while the employment coefficient survives and roughly triples in magnitude (-0.017, $p = 0.01$). Second, with only 21 NAICS-3 clusters the asymptotic cluster standard errors over-reject; under the wild cluster bootstrap (Table 9) only churn clears the 5% level, with turnover and employment both at $p \approx 0.11$. Table 7 extends this small-sample discipline to every outcome and window: the employment effect, too, sits at bootstrap p between 0.07 and 0.11 in every window, so its distinction from the fluidity results rests on its stability across windows, estimators, and the sector exclusion, not on clearing a stricter significance bar. Figure 1 makes the sector concentration visible: removing the six NAICS-334 industries flattens the job-reallocation gradient entirely while leaving the employment gradient intact. I therefore treat the employment effect, robust to the single-sector exclusion and stable across windows, as the paper's anchor finding, and the fluidity declines as a sector-specific, transition-era pattern that is suggestive rather than decisive. That pattern is itself informative: the dynamism cost of offshoring operated through the industries where offshoring was most intense, rather than across manufacturing as a whole. A decline this narrow cannot carry the broad, secular fall in dynamism that motivates the literature, a point I return to in the conclusion.

Table 3: Leave-One-Out Robustness: Excluding NAICS-334 (Computers and Electronics)

	Full sample		Excl. NAICS-334	
Outcome (Δ, 1999–2011)	Coef.	p	Coef.	p
Employment	-0.0053^{***}	0.000	-0.0168^{**}	0.013
Job reallocation	-0.0062^{*}	0.059	-0.0047	0.768
Turnover	-0.0097^{***}	0.000	-0.0051	0.524
Churn	-0.0053^{***}	0.000	-0.0034	0.632
Industries (N)	86		80	

Notes: 1999–2011 IV estimates of the effect of $\Delta\mathrm{OFF}_{j,99\text{-}11}$ (instrumented by $\Delta\mathrm{OFF}^{O}_{j,99\text{-}11}$) on each outcome, full sample versus excluding NAICS-3 sector 334. Sector (NAICS-2) FE and the four industry controls included; weighted by 1991 employment; NAICS-3 cluster SE. The fluidity effects do not survive the exclusion; the employment effect strengthens. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

The one fluidity channel that survives: firm entry. The single-sector fragility has one informative exception. Splitting the job-reallocation effect by firm age (Table 4; the model mechanism is in Appendix E) concentrates the decline entirely in entrants: for firms aged 0–1 year the IV coefficient is -0.076 ($p < 0.01$), about thirteen times the all-firms estimate, while older firms show nothing. Unlike every other fluidity result, the entrant effect *strengthens* when the six computers-and-electronics industries are excluded, roughly quadrupling to -0.303 ($p < 0.01$). Its binding leave-one-cluster-out sector is transportation

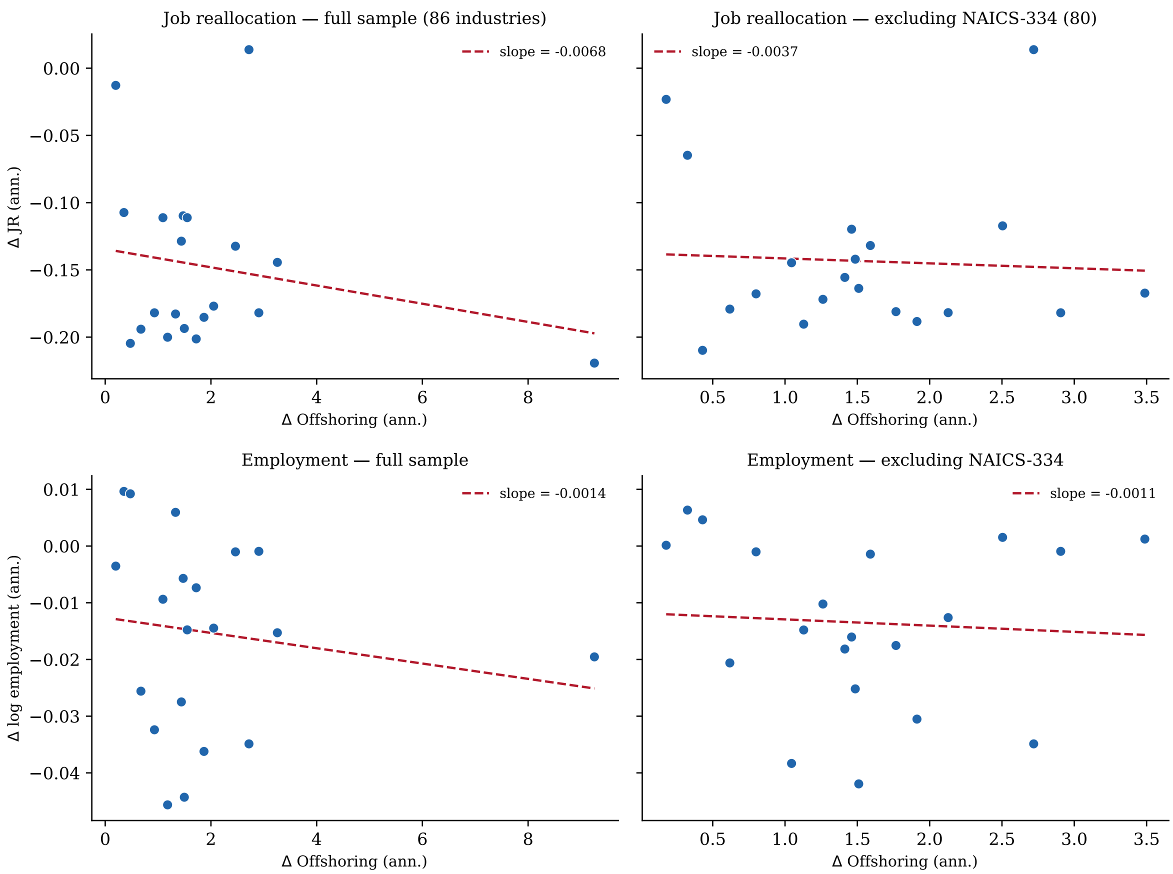

Figure 1: The fluidity gradient lives in computers and electronics; the employment gradient does not

Notes: Binned scatters (20 equal-sized bins) of the annualized 1999–2011 change in job reallocation (top row) and log employment (bottom row) against the change in offshoring penetration, for the full 86-industry sample (left) and excluding the six NAICS-334 industries (right). Dashed lines are OLS fits on the unbinned data. The visual counterpart of Table 3.

equipment (336), not the NAICS-334 that drives the rest, and it stays significant ($p = 0.03$) when that cluster is dropped. It is the one dynamism result in the paper that is not an artifact of the single sector identifying everything else, and it lands where the model places the firm-side margin: young, high-uncertainty firms substitute offshore inputs for the irreversible domestic hiring that entry requires (Section 2.7). The caution is that it remains a job-reallocation estimate—it inherits the vintage fragility of the aggregate and clears the wild cluster bootstrap only at $p = 0.10$—so I read it as sharpening where the firm-side channel operates, not as restoring a robust economy-wide fluidity effect.

Table 4: Job Reallocation by Firm Age: Inference and the Single-Sector Exclusion

	IV coef.	Cluster p	Wild-boot p	Excl. 334	N
All firms	-0.006^{*}	0.059	0.096	-0.005	86
	(0.003)				
0–1 yr (entrants)	-0.076^{***}	¡0.001	0.102	-0.303^{***}	86
	(0.021)				
6–10 yr	0.026	0.134	0.457	0.029	86
	(0.017)				
11+ yr	-0.006	0.123	0.102	-0.014	86
	(0.004)				

Notes: IV estimates of the predetermined 1999–2011 offshoring shock (instrumented by $\Delta\mathrm{OFF}^{O}_{j,99\text{-}11}$) on the annualized 1999–2011 change in the job-reallocation rate, by QWI firm-age bin; firm-age-specific job reallocation from the QWI by-firm-age files (`code/22_build_firmage_jr_fa.py`), covariates and spec as in Table 2. "Wild-boot p" is the restricted wild cluster bootstrap-t ($B = 499$); "Excl. 334" drops the six NAICS-334 industries. The entrant (0–1 yr) effect is the only fluidity estimate that survives—indeed strengthens under—the NAICS-334 exclusion. Built by `code/35_firmage_inference.py`. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

5.4 First Stage and Instrument Validity

The first-stage relationship between Chinese offshoring to the U.S. and Chinese exports to other countries is strong (F-statistic: 496 in the preferred specification, 496–1,477 across control sets; Tables 1 and 17). Figure 2 presents the binned scatter plot of offshoring exposure against job reallocation change, illustrating the negative relationship in the raw data.

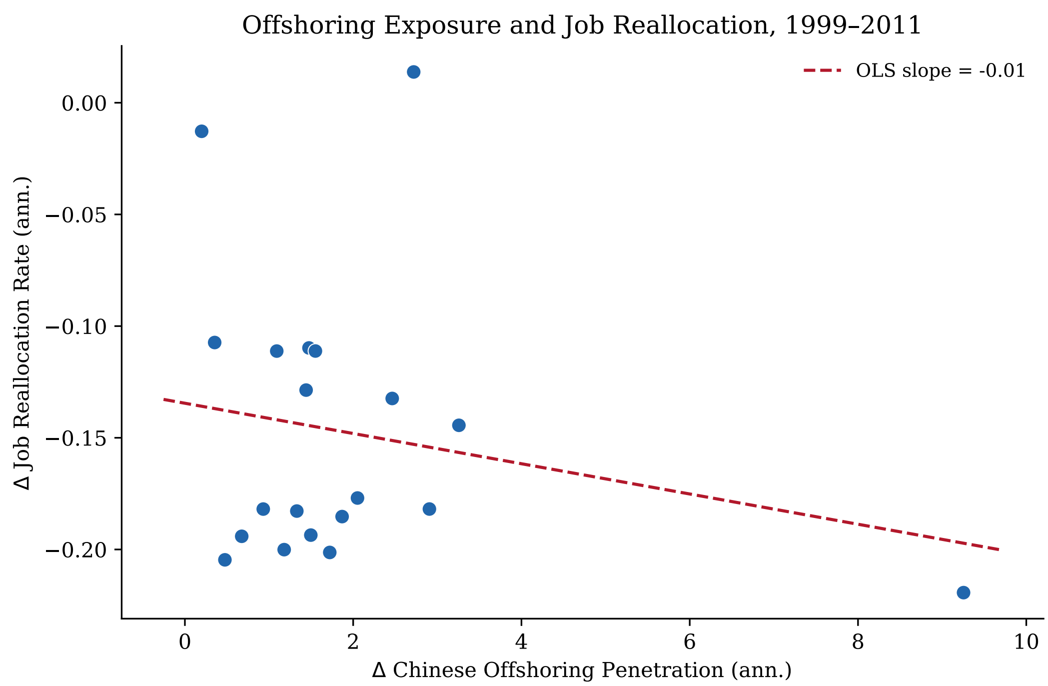

Figure 2: Offshoring Exposure and Job Reallocation, 1999–2011

Notes: Each dot represents the mean of 20 equal-sized bins of industries ranked by change in Chinese offshoring penetration (x-axis). Dashed line is OLS fit on the unbinned data. Sample: 86 NAICS 4-digit manufacturing industries, all firm ages (firmage = 0).

6 Evidence Through 2024: What Persists and What Recovers

6.1 Aggregate Trends

Figure 3 plots the national time series of job reallocation, turnover, and churn from 1990 to 2025, normalized to 1999. All three measures decline steadily through 2008, with the decline accelerating during the Great Recession. After 2011—the endpoint of the original analysis—job reallocation partially recovers by 2015–2017, spikes during the COVID pandemic (2020–2021), and declines sharply again in 2024–2025.

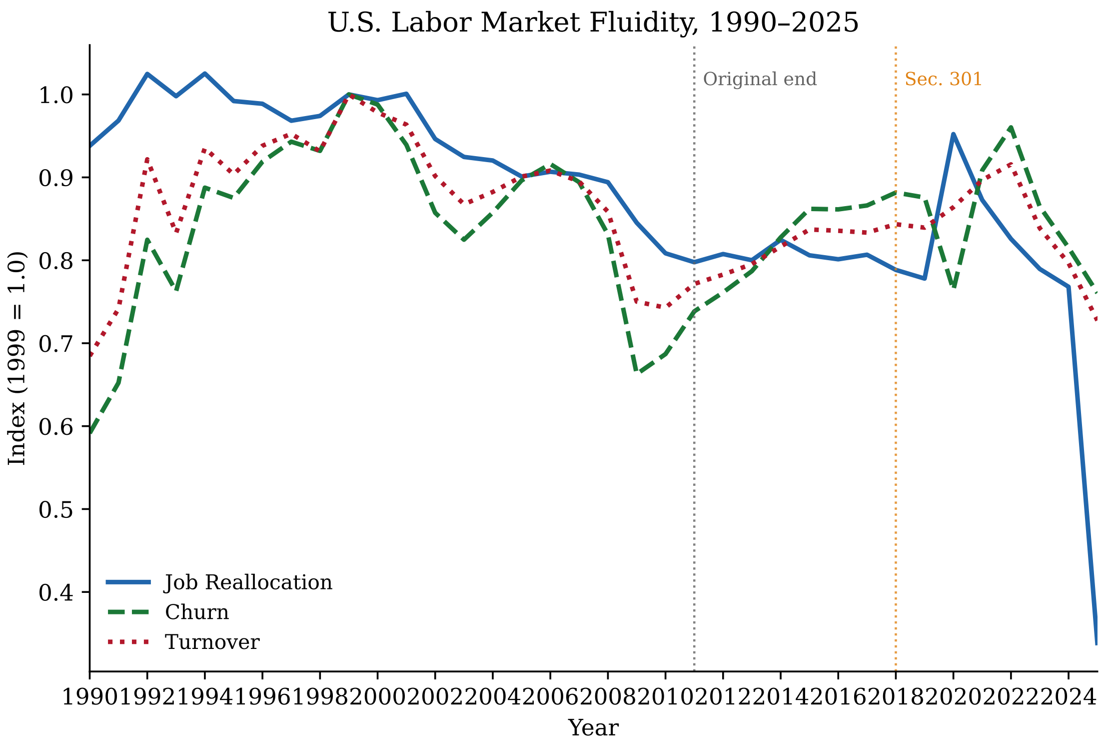

Figure 3: U.S. Labor Market Fluidity, 1990–2025

Notes: Annual means of job reallocation, turnover, and churn across all NAICS 4-digit industries, normalized to 1999 = 1.0. Source: QWI, all firm ages, 51 states. Vertical lines mark the end of the original analysis period (2011) and Section 301 tariff implementation (2018).

6.2 Permanent Levels and Worker Flows, Transitory Reallocation

Table 5 presents the core new result. Column (1) reports the OLS estimate for reference; columns (2)–(5) report IV estimates using the predetermined 1999–2011 offshoring exposure as the treatment variable, with dependent variables measured over progressively longer periods.

Table 5: Offshoring Effects Across Time Periods

	(1) 1999–2011 OLS	(2) 1999–2011 IV	(3) 1999–2019 IV	(4) 1999–2024 IV	(5) 2015–2024 IV
Employment	-0.005^{***}	-0.005^{***}	-0.004^{***}	-0.004^{***}	-0.003^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Job Reallocation	-0.006^{**}	-0.006^{*}	-0.004^{**}	-0.002	0.005^{**}
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Turnover	-0.009^{***}	-0.010^{***}	-0.008^{***}	-0.005^{***}	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Churn	-0.005^{***}	-0.005^{***}	-0.004^{***}	-0.003^{***}	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Sector FE	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes
R^2	0.317	—	—	—	—
First-stage F	—	496.3	496.3	496.3	496.3
N	86	86	86	86	86
% of 1999–2011 JR decline	9.1% [-0.4%, 18.5%]				

Notes: Each cell reports the coefficient of offshoring penetration (Δ 1999–2011) on the indicated outcome. Column (1) is OLS; columns (2)–(5) instrument US–China offshoring with other-country–China offshoring. Controls: high-tech indicator, log average wage, production-worker share, capital–value-added ratio. All regressions include sector FE, weighted by 1991 baseline employment, clustered at NAICS-3. Bottom row reports the percentage of the mean 1999–2011 decline in job reallocation explained by the IV estimate [95% CI]. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

The contrast is three-way. Employment is significant and stable across all four periods (-0.005 for 1999–2011, -0.004 for 1999–2024, -0.003 for 2015–2024, all $p < 0.01$): the level is permanent. Job reallocation instead concentrates in the transition and then recovers, moving from -0.006 ($p < 0.10$) in 1999–2011 to an insignificant -0.002 by 1999–2024 and to a small positive $+0.005$ in the pure tariff-era window; the OLS and IV estimates are close (-0.006 vs. -0.006), indicating limited endogeneity bias. The positive tariff-era point estimate should be read as recovery to zero rather than reversal: it is statistically positive only when the 2020–24 COVID years enter the window, and every window ending in 2019 gives a job-reallocation effect indistinguishable from zero (Table 7). Turnover and churn do not recover. Their 1999–2011 estimates (-0.010 and -0.005, both $p < 0.01$) shrink in this annualized table only because a transition-era effect is averaged over a longer span; measured at each horizon the effect persists, as the stacked specification and the estimated impulse responses (Figure 5) below confirm. Figure 4 visualizes the job-reallocation path.

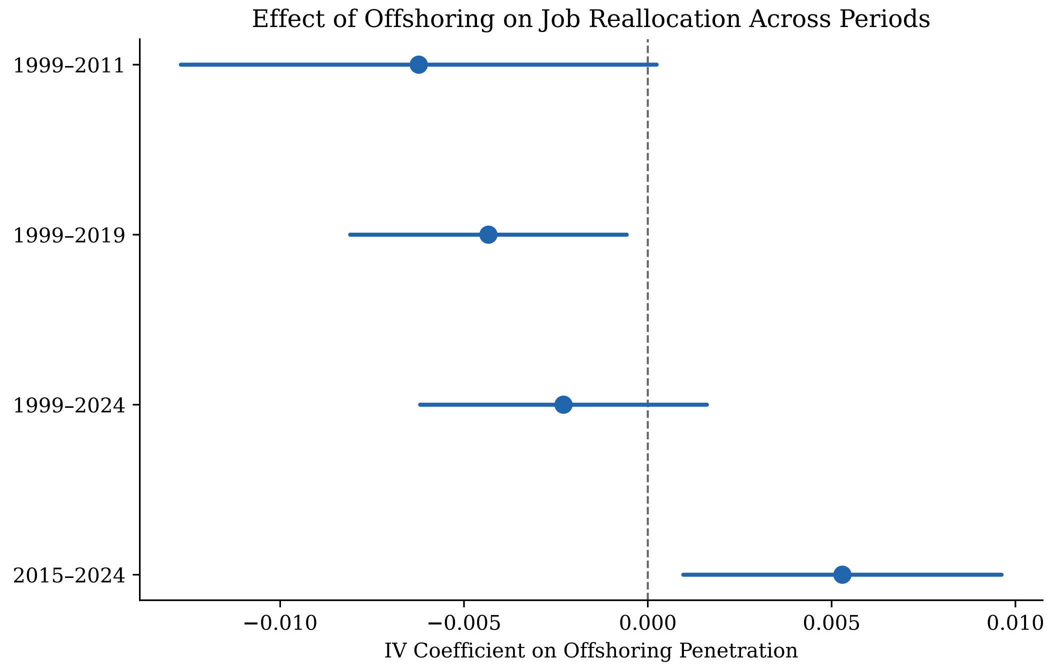

Figure 4: IV Coefficients on Job Reallocation Across Time Windows

Notes: IV coefficient on offshoring penetration (Δ 1999–2011) with 95% confidence intervals, with dependent variable measured over the indicated period. Whiskers show 95% CI. The effect is large and significant for 1999–2011 but indistinguishable from zero for all longer windows.

This is what the model predicts. The *firm-side* reallocation decline reflects the transition to a new offshoring equilibrium, a process largely complete by the mid-2010s, after which job reallocation returns to its steady-state rate. The employment loss is permanent because offshored tasks do not return, and the worker-flow rates stay depressed for the same reason: a permanently lower hire rate is at once fewer jobs and fewer worker flows (Section 8).

Formal test of the temporary-permanent split. Table 6 formalizes the contrast by stacking three long-difference observations per industry (outcomes measured over 1999–2011, 1999–2019, and 1999–2024, all expressed as annualized percentage-point changes in the rate) and interacting ΔOFF with period indicators in a single IV regression (instrument: ΔOFF^O, also period-interacted). The test statistics formalize the paper's central claim: the JR coefficient differs significantly across periods (joint Wald $\chi^2 = 41.9$, $p < 0.001$), moving from -0.008 (1999–2011, $p < 0.01$) to -0.002 (1999–2024, not significant). The employment coefficient is statistically identical across the three periods (joint $\chi^2 = 1.4$, $p = 0.50$), holding at ≈ -0.005 log points per year. Turnover and churn behave differently from job reallocation. Both are significant already in 1999–2011 (-0.007 and -0.002) and, rather than decaying, hold steady or deepen over longer windows: turnover stays near -0.008 throughout (equality not rejected, $\chi^2 = 1.5$, $p = 0.47$), while churn moves from -0.002 to -0.006 (equality rejected, $\chi^2 = 33.2$, $p < 0.001$). The firm-side reallocation measure recovers quickly, while the worker-side measures stay depressed because the exposed industries keep hiring less (Section 8, Table 13). Firm-side reallocation rebounds to its steady-state rate once the transition completes; turnover and churn, being hiring flows, track the permanent suppression of hiring instead.

Table 6: Stacked Long-Difference IV with Period Interactions

	Δ JR	Δ Turnover	Δ Churn	Δ log-Emp
ΔOFF × **1999–2011**	-0.008^{***}	-0.007^{***}	-0.002^{**}	-0.005^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
ΔOFF × **1999–2019**	-0.004	-0.008^{***}	-0.005^{***}	-0.005^{***}
	(0.002)	(0.002)	(0.001)	(0.001)
ΔOFF × **1999–2024**	-0.002	-0.008^{***}	-0.006^{***}	-0.004^{***}
	(0.002)	(0.002)	(0.001)	(0.001)
Wald tests of coefficient equality across periods				
H_0: 1999–2011 = 1999–2019	$\chi^2 = 9.3$ ($p = 0.002$)	$\chi^2 = 0.3$ ($p = 0.570$)	$\chi^2 = 5.1$ ($p = 0.025$)	$\chi^2 = 0.0$ ($p = 0.983$)
H_0: 1999–2019 = 1999–2024	$\chi^2 = 31.6$ ($p = 0.000$)	$\chi^2 = 1.5$ ($p = 0.220$)	$\chi^2 = 27.1$ ($p = 0.000$)	$\chi^2 = 1.4$ ($p = 0.241$)
H_0: 1999–2011 = 1999–2024	$\chi^2 = 20.0$ ($p = 0.000$)	$\chi^2 = 0.2$ ($p = 0.664$)	$\chi^2 = 9.1$ ($p = 0.003$)	$\chi^2 = 0.1$ ($p = 0.803$)
H_0: all three equal (joint)	$\chi^2 = 41.9$ ($p = 0.000$)	$\chi^2 = 1.5$ ($p = 0.471$)	$\chi^2 = 33.2$ ($p = 0.000$)	$\chi^2 = 1.4$ ($p = 0.502$)
Mechanical-null benchmark (if effect decays linearly in $T - 1999$)				
Implied $\hat{\beta}$ under linear decay, 1999–2019	-0.005	-0.004	-0.001	-0.003
Actual $\hat{\beta}$, 1999–2019	-0.004	-0.008	-0.005	-0.005
Implied $\hat{\beta}$ under linear decay, 1999–2024	-0.004	-0.003	-0.001	-0.002
Actual $\hat{\beta}$, 1999–2024	-0.002	-0.008	-0.006	-0.004
Industries	86	86	86	86
Observations (industry × period)	258	258	258	258
Sector FE + period FE (additive)	Yes	Yes	Yes	Yes
Cluster (NAICS-3)	Yes	Yes	Yes	Yes

Notes: Stacked long-differences with three observations per industry ($p \in \{1999\text{-}2011, 1999\text{-}2019, 1999\text{-}2024\}$), outcomes as annualized percentage-point change in the rate (or log employment). Built from QWI R2026Q1 via `code/15_rebuild_outcomes.py`. $\Delta\text{OFF} \times \mathbf{1}\{p\}$ period-interaction terms instrumented by $\Delta\text{OFF}^O \times \mathbf{1}\{p\}$. Joint Wald test rejects equality of the JR coefficient across periods ($\chi^2 = 41.9$, $p < 0.001$) and the churn coefficient ($\chi^2 = 34.0$, $p < 0.001$); it fails to reject equality for log employment ($\chi^2 = 1.4$, $p = 0.50$). Turnover and churn are significant in the 1999–2011 window and remain so over longer horizons, consistent with a persistent hiring-suppression mechanism (Section 8) layered on the transition effect. The 1999–2011 coefficients here differ modestly from the corresponding separate-window estimates (Tables 2 and 5) because the stack pools the three long differences with additive period fixed effects and a common period-interacted first stage; the cross-period *pattern*, not the level of any single cell, is the object of interest.

Figure 5 draws the same contrast as a set of impulse responses. For each endpoint year t I estimate the IV coefficient from a local projection of the cumulative change since 1999 on the predetermined offshoring shock, and plot the path. The four outcomes separate cleanly. Log employment accumulates and never returns, the permanent level. Job reallocation declines and recovers, the transitory firm-side rate, though a positive 2000–01 pre-trend is a reminder of its fragility (Section 5.3). Stable turnover and churn decline and stay down, the worker-flow rates tracking the permanent suppression of hiring (Section 8). The level and the worker-flow rates are permanent; only firm-side reallocation recovers.

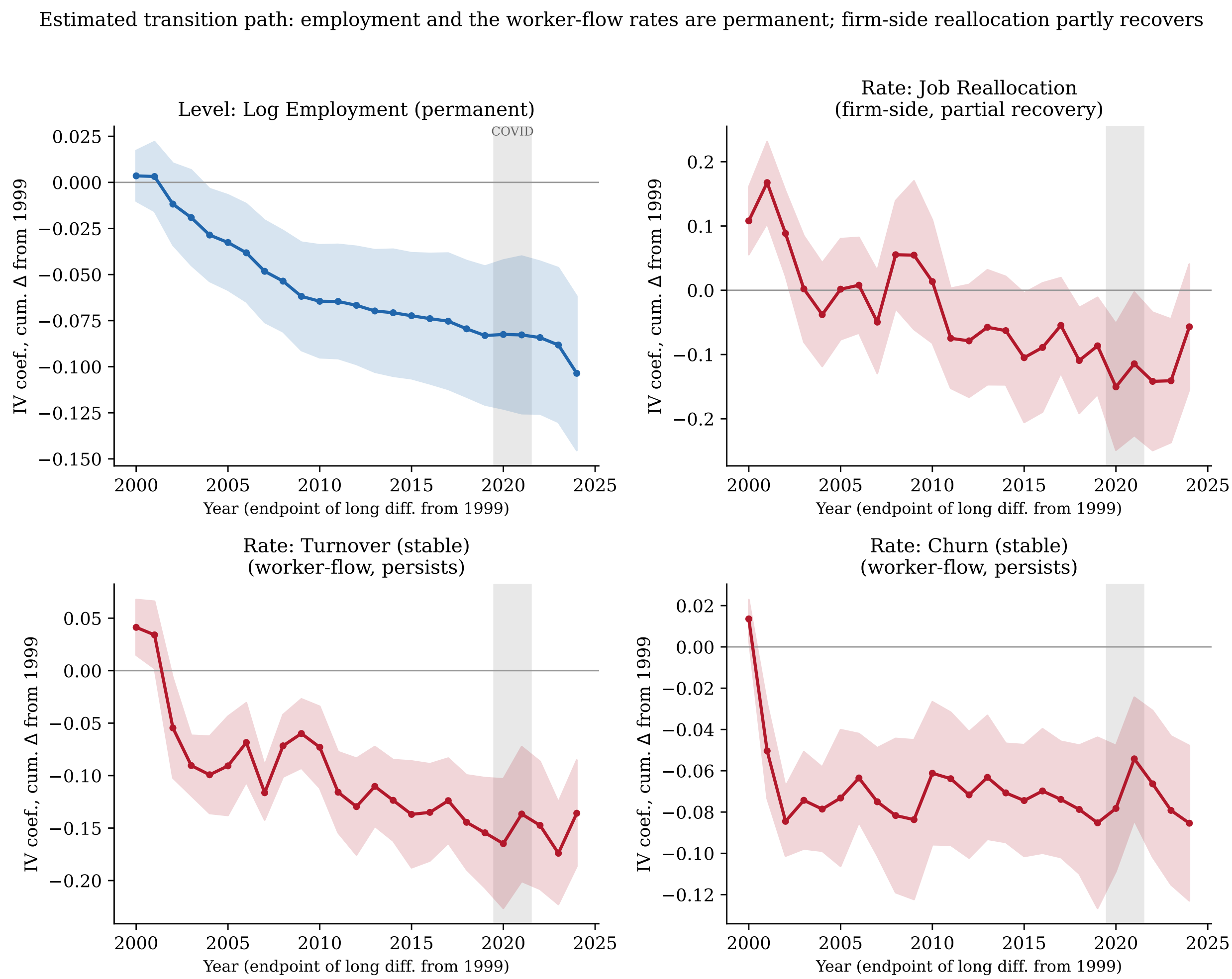

Figure 5: Estimated Transition Path: IV Impulse Responses by Horizon

Notes: Each panel plots the IV coefficient β_t from a local projection of the cumulative change from 1999, $Y_{j,t} - Y_{j,1999}$, on the predetermined 1999–2011 offshoring shock (instrumented by ΔOFF^O), estimated separately for each endpoint year t over the 86 core manufacturing industries (employment-weighted, NAICS-3 clustered), with 95% confidence bands; the 2020–21 COVID years are shaded. JR and log employment are from the national QWI; stable turnover and churn are built at annual frequency from the raw QWI full-quarter flows (`code/28_build_stable_annual_levels.py`). Built by `code/27_transition_path.py`.

Uniform inference and the COVID years. Table 7 disciplines the whole grid of estimates at once: for every outcome and window it reports the cluster p-value alongside the wild cluster bootstrap p-value and a leave-one-cluster-out check, and it adds two windows—2011–2019 and 2015–2019—that isolate the post-transition period from the pandemic. Three facts emerge. First, the employment effect is negative in every window, including 2011–2019, so the level effect is not a transition artifact; like every estimate in this 21-cluster design, its bootstrap p-values sit near 0.09–0.11. Second, the post-transition job-reallocation effect is zero in every window that ends before COVID; the significantly positive 2015–2024 estimate is a COVID-window phenomenon, which is why I do not read it as evidence of reversal. Third, turnover and churn keep declining after the transition and before COVID (2011–2019: -0.005, $p = 0.008$, and -0.003, $p = 0.07$), which is the worker-flow persistence of the previous subsection measured without any pandemic contamination. The leave-one-cluster-out column shows NAICS-334 is the binding cluster for every fluidity estimate, never for employment in the transition windows.

Table 7: Uniform Inference Across Outcomes and Windows

Outcome	Window	IV coef.	Cluster p	Wild bootstrap p	Leave-one-cluster-out max p
Employment	1999–2011	-0.0053^{***}	0.000	0.114	0.013 (excl. 334)
	1999–2019	-0.0041^{***}	0.000	0.088	0.047 (excl. 334)
	1999–2024	-0.0041^{***}	0.000	0.092	0.090 (excl. 334)
	2011–2019	-0.0023^{***}	0.001	0.164	0.829 (excl. 334)
	2015–2019	-0.0026^{***}	0.000	0.068	0.393 (excl. 334)
	2015–2024	-0.0034^{***}	0.000	0.094	0.572 (excl. 334)
Job reallocation	1999–2011	-0.0062^{*}	0.059	0.096	0.768 (excl. 334)
	1999–2019	-0.0043^{**}	0.024	0.140	0.844 (excl. 334)
	1999–2024	-0.0023	0.249	0.401	0.964 (excl. 325)
	2011–2019	-0.0015	0.511	0.579	0.908 (excl. 334)
	2015–2019	$+0.0046$	0.419	0.473	0.806 (excl. 325)
	2015–2024	$+0.0053^{**}$	0.017	0.052	0.191 (excl. 334)
Turnover	1999–2011	-0.0097^{***}	0.000	0.108	0.524 (excl. 334)
	1999–2019	-0.0077^{***}	0.000	0.078	0.682 (excl. 334)
	1999–2024	-0.0054^{***}	0.000	0.096	0.787 (excl. 334)
	2011–2019	-0.0048^{***}	0.008	0.142	0.960 (excl. 334)
	2015–2019	-0.0044	0.421	0.571	0.604 (excl. 336)
	2015–2024	$+0.0001$	0.953	0.964	0.998 (excl. 333)
Churn	1999–2011	-0.0053^{***}	0.000	0.048	0.632 (excl. 334)
	1999–2019	-0.0043^{***}	0.000	0.056	0.972 (excl. 334)
	1999–2024	-0.0034^{***}	0.000	0.102	0.752 (excl. 334)
	2011–2019	-0.0027^{*}	0.069	0.224	0.599 (excl. 334)
	2015–2019	-0.0027	0.397	0.583	0.580 (excl. 336)
	2015–2024	-0.0012	0.444	0.677	0.859 (excl. 336)

Notes: Every cell is the IV coefficient of the predetermined 1999–2011 offshoring shock on the annualized change in the indicated outcome over the indicated window, with the baseline specification (four industry controls, sector FE, 1991 employment weights, NAICS-3 clusters, $N = 86$). Wild cluster bootstrap: restricted Rademacher bootstrap-t, $B = 499$ (Cameron et al., 2008; Davidson and MacKinnon, 2010). The final column reports the largest p-value obtained when any single NAICS-3 cluster is dropped, and which cluster produces it. Windows ending in 2019 exclude the COVID years. Built by `code/30_uniform_inference.py`. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$ on the cluster p-value.

6.3 Offshoring's Share of the Dynamism Decline

The paper's organizing claim is that offshoring cannot account for the secular fall in dynamism. Table 8 attaches magnitudes. Within the 86 manufacturing industries, and over the 1999–2011 transition when the fluidity decline actually occurred, the offshoring shock accounts for 9% of the job-reallocation decline, 15% of the turnover decline, and 14% of the churn decline (Panel A), each computed as $\hat{\beta}_{IV} \times \overline{\Delta \text{OFF}}/\overline{\Delta F}$ over the regression sample and weights. These shares are modest, and the job-reallocation share has a confidence interval that includes zero. They are also small in the aggregate: manufacturing turnover and churn in total largely returned to their 1999 levels by 2024, so even though the offshoring effect persists in the exposed industries (Section 6), worker flows contribute almost nothing to any economy-wide loss of dynamism.

The aggregate arithmetic makes the point starkly (Panel B). Economy-wide job reallocation, measured across all 308 NAICS-4 industries in the national QWI, fell from 14.5 to 11.0 between 1999 and 2024, a 24% decline that matches the secular trend the literature studies. Manufacturing accounts for about 11% of it: the sector is a tenth of employment and its job-reallocation rate fell by roughly the economy-wide proportion. Combining the two pieces, offshoring accounts for on the order of *one percent* of the economy-wide job-reallocation decline (Panel C). Two further facts point the same way. Eighteen percent of the decline occurred after 2011, once the offshoring transition was essentially complete, and the 1999–2011 offshoring shock does not predict that later change: the post-2011 job-reallocation coefficient is zero in every pre-COVID window and positive (+0.005, $p = 0.02$) once the 2020–24 years enter (Table 7), so the industries most exposed to offshoring saw reallocation recover rather than fall further.

This accounting is confined to the channel the design measures, the offshoring of manufactured intermediate inputs identified off the computers-and-electronics shock of Section 7; it does not capture offshoring of services or general-equilibrium effects on downstream sectors. What it establishes is that the channel emphasized in the offshoring-and-fluidity literature is, by any reasonable accounting, a minor contributor to the decline in business dynamism, and that the cause should be sought in forces that are broad across the economy and that persisted after the China shock had run its course.

Table 8: How Much of the Dynamism Decline Is Offshoring?

Panel A: Offshoring's share of the manufacturing fluidity decline, 1999–2011 transition	
JR	9.1% $[-0.4, 18.5]$
Turnover	14.6% $[9.7, 19.4]$
Churn	14.1% $[7.0, 21.3]$
Panel B: Economy-wide job reallocation (308 industries)	
Economy-wide JR: 1999 / 2011 / 2024	14.5 / 11.6 / 11.0
decline 1999–2024	-3.5 (-24.4%)
share of decline before 2011 / after 2011	82% / 18%
Manufacturing employment share, 1999	0.161
Manufacturing contribution to the decline	11.3%
Offshoring shock $\rightarrow$ JR, 2015–2024 (IV)	+0.0053 ($p = 0.02$)
Panel C: Combined (1999–2011 window)	
Offshoring's share of the economy-wide JR decline	$\approx 1\%$

Notes: Panel A reports offshoring's share of the manufacturing fluidity decline over 1999–2011, $\hat{\beta}_{IV} \times \overline{\Delta\text{OFF}}/\overline{\Delta F}$, with the offshoring shock and outcomes employment-weighted over the 86 core industries; brackets give the 95% interval propagating the standard error of $\hat{\beta}_{IV}$. Panel B reports economy-wide job reallocation from the national QWI (all 308 NAICS-4 industries, employment-weighted across quarters and industries), manufacturing's within-industry contribution to the 1999–2011 decline, and the 2015–2024 IV coefficient of the 1999–2011 offshoring shock on the job-reallocation change. Panel C multiplies the Panel A job-reallocation share by the manufacturing contribution. Job reallocation is used here because it is clean on the refreshed national vintage and is the canonical dynamism measure; manufacturing turnover and churn largely recover by 2024.

7 What the Instrument Identifies: A Single-Sector Design

The narrowness that keeps offshoring from accounting for the dynamism decline is a property of the identifying variation, so it is worth establishing directly rather than leaving it to a robustness check. Two facts do the work: the design leans almost entirely on computers-and-electronics input shocks propagated through input-output linkages, and that concentration is what makes the fluidity results fragile while leaving the broader employment effect intact.

Modern shift-share inference. The instrument is a share-shifter: $\Delta\text{OFF}_i^{oth} = \sum_k S_{ik}\, g_k$, where the shock g_k is the 1999–2011 change in China's intermediate-input penetration of the six comparison countries for upstream six-digit industry k, and S_{ik} is the employment-weighted intermediate-purchase share linking estimation industry i to input k. I reconstruct S and g from the input-output and trade primitives, recovering $K = 371$ upstream shocks; the reconstruction reproduces the estimation instrument to a correlation of 0.9999 and the IV coefficient to within about one percent, so the inference reported here is computed from the actual exposure design rather than approximated. With only 21 NAICS-3 clusters, asymptotic cluster-robust inference can over-reject, so Table 9 sets the baseline cluster standard error against two exposure-robust constructions and a bootstrap: AKM standard errors that treat the upstream shocks as independent (Adão et al., 2019); BHJ standard errors that allow the shocks to be correlated within an upstream NAICS-3 sector (Borusyak et al., 2022); and wild cluster bootstrap-t p-values (Cameron et al., 2008; Davidson and MacKinnon, 2010; Roodman et al., 2019) suited to the thin cluster count. The Kleibergen-Paap rk F is 496 (Kleibergen and Paap, 2006), far above weak-instrument thresholds.

The employment effect is significant at the 1% level under all three asymptotic standard errors, and is marginal only under the wild bootstrap ($p = 0.11$), where 21 clusters leave little resolution. Exposure-robust inference does not weaken the fluidity coefficients: turnover and churn stay significant at better than 1% under both AKM and BHJ, and job reallocation, marginal on the cluster standard error ($p = 0.06$), tightens to $p \approx 0.01$. What disciplines the fluidity results is therefore not the form of the standard error but the source of the identifying variation. A Rotemberg-weight decomposition (Goldsmith-Pinkham et al., 2020) makes this concrete. Computers-and-electronics shocks (NAICS 334) carry 99% of the identifying weight; the five largest upstream shocks account for 90% of the total, and semiconductors alone (NAICS 334413) carry 27% (Panel B). With a negative-weight share of 9%, the design is not an artifact of a few sign-flipping outliers. This is the instrument-side counterpart of the leave-one-out result in Section 5.3: the instrument is in effect a computers-and-electronics shock propagated through input-output linkages, so excluding that sector removes most of the variation that identifies the fluidity coefficients, while the broader employment effect persists.

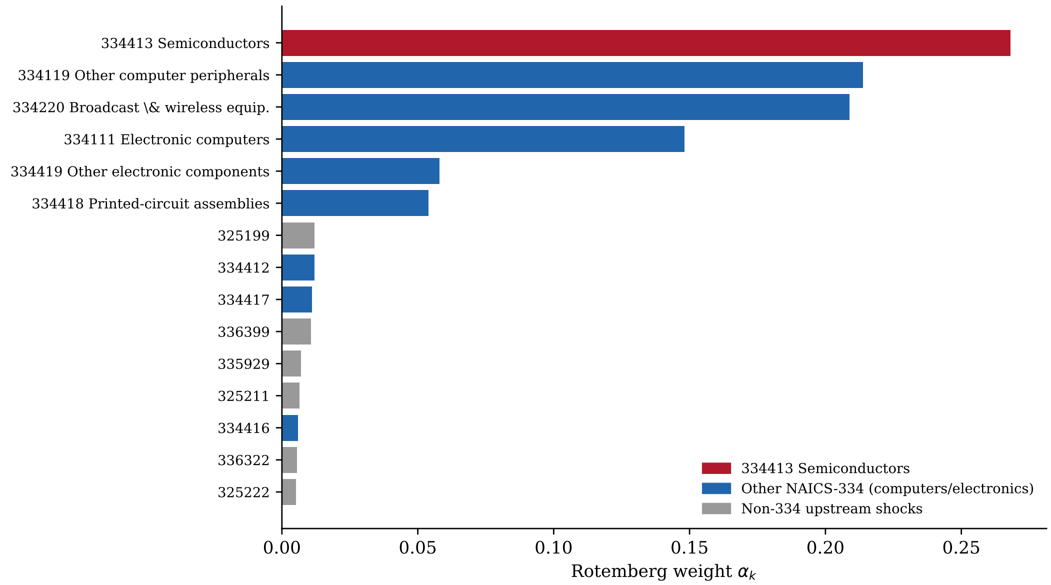

Figure 6: Identifying Variation by Upstream Shock: Rotemberg Weights

Notes: The fifteen largest Goldsmith-Pinkham et al. (2020) Rotemberg weights α_k across the $K = 371$ upstream six-digit shocks behind the instrument (the weights of Table 9, Panel B). NAICS-334 shocks in blue, semiconductors (334413) in red, all other upstream sectors in grey. NAICS-334 carries 99% of the total weight.

What identifies the estimates. The Rotemberg weights say the design leans on computers-and-electronics input shocks; Table 10 asks what is left without them. Re-deriving the instrument with the NAICS-334 upstream shocks zeroed out leaves a first stage with $F = 0.1$: once electronics inputs are removed, the share-shifter is essentially uncorrelated with U.S. offshoring, because U.S. offshoring is itself concentrated in electronics inputs. The only-334 instrument, in contrast, has $F = 773$ and reproduces the headline coefficients. The single-shock estimates β_k that the IV averages over are tightly clustered for employment (from -0.0039 to -0.0048, around the -0.005 headline) and dispersed for job reallocation (from -0.0005 to -0.010), so the electronics sub-shocks agree on the employment elasticity but not on the reallocation response. This pins down what every coefficient in the paper measures: the effect of the China electronics-input offshoring shock propagated through input-output linkages, broad and stable for employment and electronics-specific for fluidity.

Table 9: Shift-Share Inference: Exposure-Robust Standard Errors and Rotemberg Weights

	Δ Emp.	Δ JR	Δ Turnover	Δ Churn
Panel A: Main 1999–2011 IV under alternative inference				
Coefficient on ΔOFF (IV)	-0.0053^{***}	-0.0062^{*}	-0.0097^{***}	-0.0053^{***}
NAICS-3 cluster SE	(0.0013)	(0.0033)	(0.0016)	(0.0014)
AKM exposure-robust SE	(0.0009)	(0.0026)	(0.0015)	(0.0008)
BHJ exposure-robust SE	(0.0014)	(0.0024)	(0.0019)	(0.0010)
NAICS-3 cluster p	< 0.001	0.059	< 0.001	< 0.001
AKM exposure-robust p	< 0.001	0.015	< 0.001	< 0.001
BHJ exposure-robust p	< 0.001	0.011	< 0.001	< 0.001
Wild cluster bootstrap p	0.114	0.096	0.108	0.048
Kleibergen-Paap rk F	496	496	496	496
NAICS-3 clusters	21	21	21	21
Upstream shocks (K)	371	371	371	371
Industries (N)	86	86	86	86
Panel B: Rotemberg-weight decomposition of the instrument				
Negative-weight share			0.09	
Herfindahl of weights			0.19	
Top-5 shocks (share of total weight)			0.90	
NAICS-334 shocks (share of total weight)			0.99	
Five highest-weighted upstream shocks:				
334413 Semiconductors			0.268	
334119 Other computer peripherals			0.214	
334220 Broadcast & wireless equip.			0.209	
334111 Electronic computers			0.148	
334419 Other electronic components			0.058	

Notes: Panel A reports the main 1999–2011 IV coefficient under four inference constructions. The NAICS-3 cluster standard error is the baseline. The AKM (Adão et al., 2019) and BHJ (Borusyak et al., 2022) exposure-robust standard errors are computed from the reconstructed share-shifter ($K = 371$ upstream six-digit import-penetration shocks g_k and employment-weighted intermediate-purchase shares S_{ik}), treating the shocks as independent (AKM) or as correlated within an upstream NAICS-3 sector (BHJ); shocks are recentered by their exposure-weighted mean. The wild cluster bootstrap-t p-value uses $B = 499$ Rademacher draws and cluster-level sign flips over 21 NAICS-3 clusters (Davidson and MacKinnon, 2010; Roodman et al., 2019). Panel B reports Goldsmith-Pinkham et al. (2020) Rotemberg weights for the first stage, which sum to one; shares of total weight are taken over absolute weights. The reconstruction matches the estimation instrument at a correlation of 0.9999.

Table 10: Where Identification Comes From: Leave-334 Instrument and Per-Shock Estimates

	Δ Emp.	Δ JR	Δ Turnover	Δ Churn
Panel A: Main IV under instrument variants (coefficient; first-stage F)				
Baseline (all shocks)	-0.0053^{***}	-0.0063^{*}	-0.0097^{***}	-0.0053^{***}
first-stage F	488	488	488	488
Leave-334 (drop electronics inputs)	-0.0749	$+0.1247$	$+0.0981$	$+0.0304$
first-stage F	0.1	0.1	0.1	0.1
Only-334 (electronics inputs only)	-0.0044^{***}	-0.0080^{**}	-0.0111^{***}	-0.0058^{***}
first-stage F	773	773	773	773

Panel B: Per-shock just-identified β_k (top upstream shocks)	Δ Emp.		Δ JR	
Upstream shock	α_k	β_k	α_k	β_k
334413 Semiconductors	0.268	-0.0043	0.268	-0.0103
334119 Other computer peripherals	0.214	-0.0042	0.214	-0.0100
334220 Broadcast & wireless equip.	0.209	-0.0047	0.209	-0.0005
334111 Electronic computers	0.148	-0.0039	0.148	-0.0088
334419 Other electronic components	0.058	-0.0048	0.058	-0.0102
$\sum_k \alpha_k\beta_k$ (all K)	-0.0053		-0.0063	
Headline IV $\hat{\beta}$	-0.0053		-0.0062	

Notes: Panel A re-estimates the main 1999–2011 IV with the instrument rebuilt from three subsets of the upstream shocks: all shocks (baseline), shocks outside NAICS-334 (leave-334), and NAICS-334 shocks only. The reported F is the first-stage statistic on the excluded instrument. The baseline row uses the *reconstructed* share-shifter (correlation 0.9999 with the estimation instrument; Table 9), which is why its F (488) differs trivially from the 496 reported elsewhere. Panel B reports Goldsmith-Pinkham et al. (2020) single-shock just-identified estimates β_k and Rotemberg weights α_k for the five highest-weighted upstream shocks; $\sum_k \alpha_k\beta_k = \hat{\beta}$ by construction. Sector FE and industry controls included; weighted by 1991 employment; NAICS-3 cluster SEs. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$.

8 Mechanisms

8.1 The Channel Is Hiring, Not Composition

What permanently lowers the worker-flow rates? The original analysis pointed to occupational composition: offshoring removes offshorable, putatively high-turnover tasks and tilts the surviving mix toward lower-churn work. The composition shift is real, but it is not the channel. The permanent worker-flow decline turns out to be the same thing as the permanent employment decline: offshoring suppresses domestic *hiring*, and a permanently lower hire rate shows up both as fewer jobs and as lower worker flows. I take the steps in turn: the composition shift is genuine but does not carry the effect, and the channel that does is hiring.

Composition does shift, permanently. Table 11 estimates the effect of the offshoring shock on the industry-level offshorability-weighted employment share (12), constructed from BLS Occupational Employment Statistics (2002–2023, extended from the original 2002–2013 OES panel using the BLS NAICS-4 industry files for 2014–2023) and the Blinder (2006) occupation-level offshorability index. The IV estimate for 1999–2011 is -0.50 ($p < 0.01$): a one-percentage-point increase in Chinese offshoring penetration reduces the offshorable-occupation employment share by 0.5 percentage points. The effect grows over longer horizons, to -0.86 for 1999–2019 and -0.75 for 1999–2023 (both $p < 0.01$), and the deepening is not an artifact of the OES coverage decline from 86 to 62 industries: re-estimating 1999–2011 on the constant 62-industry sample gives -0.52, essentially the full-sample -0.50. The compositional consequence of offshoring is permanent and continues to deepen, a genuine permanent footprint that parallels the employment decline. Whether it is what holds the worker-flow rates down is the question of the next step.

But composition does not carry the worker-flow effect. If the worker-flow decline ran through composition, the offshorable-occupation share would mediate it. It does not. Table 13 (Panel A) reports a joint two-mediator Gelbach decomposition (Gelbach, 2016) of the offshoring$\rightarrow$worker-flow effect: the composition shift mediates at most a small share at the transition horizon and a share that is negative in sign by 1999–2024, and the bootstrap confidence intervals on all the mediated shares are wide enough that the decomposition supports only the qualitative ordering, not point shares. The reduced-form offshorable-share$\rightarrow$fluidity link is correspondingly weak on the refreshed vintage (Table 12, Panel B: the estimates are small and, once annualized, wrong-signed). And the premise of the composition story fails in the occupation data: offshorable occupations are not in fact high-turnover. Merging the

Table 11: Effect of Chinese Offshoring on the Offshorable-Occupation Employment Share

	Δ Offshorable employment share (pp)		
Outcome period:	1999–2011	1999–2019	1999–2023
Δ Offshoring penetration (ΔOFF)	-0.495^{***}	-0.860^{***}	-0.753^{***}
	(0.056)	(0.210)	(0.207)
Industries (N)	86	62	62
First-stage F	496	725	725
Sector FE	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes
1999–2011 re-estimated on the constant 1999–2023 sample ($N = 62$):			
ΔOFF	-0.522^{***}	(0.046)	

Notes: IV estimates of $\Delta\text{OFFSHARE}_{j,t} = \alpha + \beta \cdot \Delta\text{OFF}_{j,99\text{-}11} + X_{j,91} + \delta_s + \varepsilon$ where ΔOFF is instrumented by $\Delta\text{OFF}^{O}_{j,99\text{-}11}$, and the LHS is the long difference in the offshorability-weighted employment share (12) measured over the indicated window. Sector (NAICS-2) FE and industry controls included. Weighted by 1991 employment. SEs clustered at NAICS-3.

Blinder (2006) index to occupation-level worker flows, offshorability is essentially uncorrelated with turnover and slightly *positively* correlated with job tenure. The compositional residue is real, but it is not the reason worker flows fall.

The channel is hiring suppression. What actually falls is hiring. Table 14 decomposes the 1999–2011 effect into components: offshoring reduces *job creation* by -0.008 pp/year ($p = 0.02$) and does nothing to *job destruction* ($+0.002$, $p = 0.73$). Table 13 (Panel B) confirms it directly and over the long run, on the same stable-flow definitions as the outcome variables: the offshoring shock lowers the stable hire rate (-0.016, $p = 0.01$ for 1999–2011; -0.011, $p < 0.01$ for 1999–2024) and leaves the stable separation rate untouched (-0.004 and -0.000, both insignificant); the all-accessions variants give the same pattern (-0.009 and -0.007 on hires, nulls on separations). Workers still leave the exposed industries at the usual pace; they are simply not replaced. This is the vacancy-suppression force of Proposition 2 (Acemoglu and Restrepo, 2022, §3), made permanent: the high-vacancy tasks near the offshoring margin are offshored for good, so the lower-hiring regime does not reverse. The link to the level effect is then immediate. In the joint decomposition the employment decline is the larger long-run mediator of both turnover and churn, and controlling for it the residual churn effect is no longer significant (Table 13, Panel A); the mediated shares themselves carry wide bootstrap intervals, so I lean on the direct Panel B evidence rather than on point shares. The worker-flow persistence and the employment decline are one permanent hiring shortfall, observed once as a flow and once as a stock.

Table 12: Offshorable-Occupation Employment Share and Fluidity Outcomes, 1999–2011

	Δ outcome, 1999–2011			
	Δ JR	Δ Turnover	Δ Churn	Δ Employment
Panel A. OLS				
Δ Offshorable emp. share	0.012*	0.009**	0.006*	0.009***
	(0.006)	(0.004)	(0.003)	(0.002)
Panel B. 2SLS — ΔOFFSHARE instrumented by ΔOFF^O				
Δ Offshorable emp. share	0.013*	0.020***	0.011***	0.011***
	(0.008)	(0.004)	(0.002)	(0.003)
Industries (N)	86	86	86	86
First-stage F	57	57	57	57
Sector FE	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes

Notes: Panel A reports OLS, Panel B reports 2SLS using $\Delta\text{OFF}^O_{j,99\text{-}11}$ as the instrument for the change in offshorable-occupation employment share. On the refreshed R2026Q1 vintage the offshorable-share→fluidity link is weak: the Panel B estimates are small and, once annualized, wrong-signed, consistent with the mediation in Table 13 showing composition carries little of the worker-flow effect.

Table 13: What Drives the Persistent Worker-Flow Decline

	Turnover 1999–2011	Churn 1999–2011	Turnover 1999–2024	Churn 1999–2024
Panel A: joint Gelbach decomposition of the offshoring effect [bootstrap 95% CI]				
Offshoring effect (IV)	−0.0097***	−0.0053***	−0.0054***	−0.0034***
mediated by occupational composition	9% [-232, 564]	4% [-140, 117]	-56% [-201, 338]	-37% [-502, 110]
mediated by the employment decline	7% [-499, 587]	26% [-136, 439]	52% [-520, 368]	75% [-1049, 1473]
Direct effect (both mediators)	−0.0081***	−0.0037*	−0.0056***	−0.0021
Panel B: the decline is a hiring collapse (IV on the rate)				
Hire rate (stable, HirAS/EmpS)	−0.0156***		−0.0106***	
Separation rate (stable, SepS/EmpS)	−0.0037		−0.0003	
Hire rate (all accessions, HirA)	−0.0089***		−0.0072**	
Separation rate (all, Sep)	+0.0014		+0.0017	

Notes: Panel A reports the IV coefficient of the 1999–2011 offshoring shock on each worker-flow long difference and a *joint* two-mediator Gelbach decomposition (Gelbach, 2016): the shares mediated by the offshorable-occupation employment share and by the change in log employment are $\hat{\Gamma}_m\hat{\gamma}_m/\hat{\beta}_{\text{base}}$ from one auxiliary system, so the two shares plus the direct effect decompose the total exactly; brackets are cluster-pairs-bootstrap 95% intervals ($B = 200$), which are wide—the decomposition supports the qualitative ordering, not point shares. The final Panel A row is the coefficient with both mediators controlled. Panel B regresses the annualized change in the hire and separation rates on the same shock: the stable rows use full-quarter flows (HirAS, SepS over EmpS), the paper's worker-flow convention; the all-accessions rows use HirA and Sep. The decline is a hiring effect at every horizon. Instrument ΔOFF^O; sector FE and industry controls; weighted by 1991 employment; NAICS-3 cluster SEs. Built by `code/29_persistence_mechanism.py`. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

Table 14: Decomposition of the 1999–2011 Fluidity Effects

LHS: Δ	*JR components* Job creation (JC)	Job destruction (JD)	*Churn components* HAR	SAR
Δ Offshoring penetration	-0.008^{**}	0.002	-0.003	-0.001
	(0.003)	(0.005)	(0.010)	(0.010)
Industries (N)	86	86	86	86
First-stage F	496	496	496	496
Sector FE	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effect of $\Delta\text{OFF}_{j,99\text{-}11}$ on each fluidity component: JC (job creation rate change), JD (job destruction rate change), HAR (hires above replacement), SAR (separations above replacement). Force-2 prediction of the model (Proposition 2): the JR effect operates primarily through reduced job creation. Instrument: $\Delta\text{OFF}^{O}_{j,99\text{-}11}$. Controls and clustering as in Table 2.

8.2 Pre-Trends

Using the extended QWI data with coverage from 1990, I test for differential pre-trends in fluidity across industries with high vs. low subsequent offshoring exposure. Figure 7 shows that job reallocation trends are broadly parallel across high- and low-offshoring industries during 1992–1998, and the series diverge sharply after 1999, consistent with China's WTO accession triggering the offshoring shock.

Table 15 runs the formal placebo: each 1992–1998 long difference is regressed on the predetermined 1999–2011 offshoring shock (IV). A valid design requires these pre-period coefficients to be small and insignificant. They are for employment ($p = 0.13$), turnover ($p = 0.96$), and churn ($p = 0.27$). Job reallocation is the exception: it shows a marginally significant *positive* pre-trend (+0.016, $p = 0.05$). The positive sign is opposite to the negative transition-era estimate, so it does not manufacture the main result, but it is one more reason—alongside the vintage fragility and single-sector dependence—to treat the job-reallocation channel as the least reliable of the four outcomes and to anchor the paper on the employment effect.

9 The Section 301 Tariff Shock

Beginning in July 2018, the United States imposed tariffs of 10–25% on approximately \$370 billion of Chinese imports under Section 301. If the original offshoring-fluidity relationship were structural (i.e., representing a permanent change in how firms adjust employment), tariffs that raise offshoring costs should *increase* fluidity by inducing re-shoring. The model predicts no such reversal.

Table 15: Pre-Period Placebo: 1992–1998 Outcomes on the 1999–2011 Offshoring Shock

	Δ outcome, *pre-period* 1992–1998			
	Employment	Job realloc.	Turnover	Churn
Δ Offshoring (Δ 1999–2011)	0.0092	0.0164**	0.0004	−0.0041
	(0.0061)	(0.0083)	(0.0085)	(0.0037)
Industries (N)	86	86	86	86
First-stage F	496	496	496	496

Notes: IV estimates of the predetermined $\Delta\text{OFF}_{j,99\text{-}11}$ (instrumented by $\Delta\text{OFF}^{O}_{j,99\text{-}11}$) on *pre-period* 1992–1998 long differences. Under a valid design these are zero. Sector FE and industry controls included; weighted by 1991 employment; NAICS-3 cluster SE. $^{***}\ p < 0.01$; $^{**}\ p < 0.05$; $^{*}\ p < 0.10$.

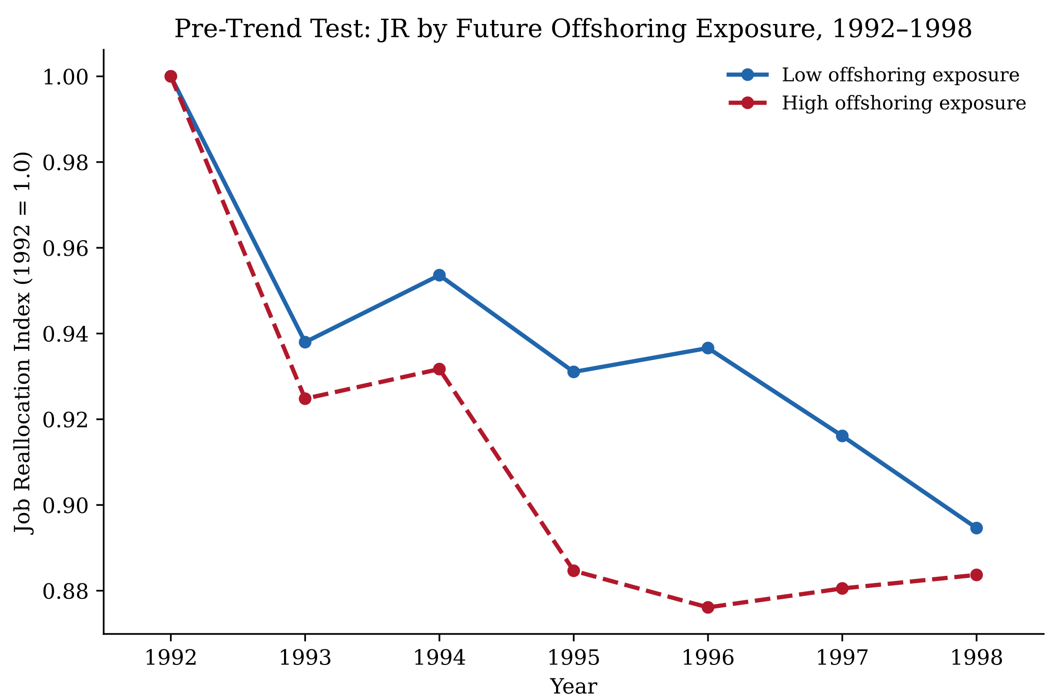

Figure 7: Pre-Trends in Job Reallocation by Offshoring Exposure, 1992–1998

Notes: Mean job reallocation rate for industries above and below the median of subsequent (1999–2011) offshoring exposure, normalized to 1999 = 1.0. Pre-period trends are parallel, supporting the identification assumption.

Table 5, column 5, offers an indirect test using predetermined 1999–2011 offshoring exposure as treatment: the coefficient on offshoring exposure for the 2015–2024 period is small for all three fluidity measures—+0.005 for JR (positive, and significant only because the COVID years sit in the window; Table 7), 0.000 for turnover, and −0.001 for churn. None points to renewed decline. But a referee could reasonably object that this is not a test of tariff exposure per se—it measures the persistence of the original offshoring shock, not the causal effect of tariffs.

Table 16 provides the direct test, constructing a Section 301 tariff exposure measure at the NAICS-4 level in the spirit of Amiti et al. (2019), Fajgelbaum et al. (2020), and Flaaen and Pierce (2024). I define tariff exposure as industry j's 2017 input-output-weighted Chinese import penetration (just before the tariffs) multiplied by the weighted-average Section 301 tariff rate of 17% across lists 1–4a: $\text{TARIFF}_j^{301} = \text{off\_usch}_{j,2017} \times 0.17$, where $\text{off\_usch}_{j,2017}$ is built from Census NAICS-level 2017 imports from China over a fixed 1991 shipments base and input-output weighted exactly as the headline measure (Appendix A). Three specifications regress $\Delta y_{j,2015\text{-}2024}$ on this tariff measure, adding the predetermined $\Delta OFF_{j,99\text{-}11}$ and its interaction with tariff exposure in columns (II) and (III). No specification shows the recovery that re-shoring would produce. The tariff coefficient on job reallocation is positive on its own (+1.4, $p = 0.04$) but not robust: it loses significance the moment pre-shock offshoring depth enters the regression ($p = 0.31$). For employment the tariff coefficient is, if anything, negative—more exposed industries kept losing employment relative to less exposed ones (−0.84, $p < 0.01$ before controls), the opposite of re-shoring—and it too does not survive the offshoring control. The hysteresis signature is in the interaction: tariff exposure × pre-2018 offshoring depth is negative for both outcomes and marginally significant for job reallocation (−0.40, $p = 0.08$), so the industries most deeply offshored before 2018 show the *least* restoration, the sign equation (6) predicts. The data engage the literature on Section 301 incidence (Amiti et al., 2019; Fajgelbaum et al., 2020; Flaaen and Pierce, 2024; Handley et al., 2025) with a mechanism-consistent null.

The model attributes this pattern to three forces: (i) sunk costs of existing offshoring relationships (supplier networks, logistics infrastructure) that are not recovered by re-shoring; (ii) depreciation of domestic human capital in offshored tasks over the preceding decade; and (iii) policy uncertainty about tariff duration that depresses the option value of re-shoring investment, including the Phase One-era reversals (Bown, 2021). The calibrated threshold is a schedule, not a single number: a task offshored s years before the tariff requires $\tau^*(s, \pi)$ to re-shore, and at the baseline calibration ($\pi = 0.12$) the threshold exceeds 60% even for tasks offshored the year before and 290% for tasks offshored a decade earlier; even with no policy-reversal risk ($\pi = 0$), only tasks offshored within roughly two years of the tariff clear the

Table 16: Section 301 Tariff Exposure and 2015–2024 Outcomes

	Δ JR (2015–2024)			Δ Emp. (2015–2024)		
	(I)	(II)	(III)	(I)	(II)	(III)
Section 301 exposure (TARIFF_j)	1.425**	1.423	3.241*	−0.843***	−0.148	−0.060
	(0.678)	(1.401)	(1.665)	(0.196)	(0.566)	(0.704)
$\Delta\text{OFF}_{j,99\text{-}11}$	–	0.000	0.012	–	−0.003	−0.003
	–	(0.005)	(0.009)	–	(0.002)	(0.003)
$\text{TARIFF}_j \times \Delta\text{OFF}_{j,99\text{-}11}$	–	–	−0.396*	–	–	−0.019
	–	–	(0.223)	–	–	(0.073)
Industries (N)	86	86	86	86	86	86
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: TARIFF_j^{301} = 2017 input-output-weighted Chinese import penetration of industry j (Census NAICS imports over a fixed 1991 shipments base; `code/33_build_section301_exposure.py`) multiplied by the 0.17 weighted-average Section 301 tariff rate across USTR lists 1–4a (Fajgelbaum et al., 2020). Columns (II) and (III) add the predetermined 1999–2011 offshoring shock and its interaction. Weighted OLS with sector FE, NAICS-3 clustered SEs. A positive TARIFF_j^{301} coefficient would indicate re-shoring-induced recovery. The job-reallocation coefficient is positive on its own but loses significance once pre-shock offshoring depth enters; the employment coefficient is negative; and—the hysteresis test—the interaction with pre-shock offshoring is negative for both outcomes (marginally significant for JR), so the most deeply offshored industries recover least, supporting equation (6).

10–25% Section 301 rates (Appendix F, `code/32_model_solver.py`). The China-shock-era stock of offshored tasks sits far above any plausible tariff.

10 Robustness

Table 17 reports the headline employment coefficient across five specification variants. The estimate is stable in sign and magnitude: −0.005 in the baseline (IV with industry controls and sector FE), −0.004 without sector FE, −0.003 without industry controls, and −0.005 under OLS—significant at the 1% level in every case except the trade-control specification (−0.003, not significant), where adding contemporaneous import and export controls absorbs part of the offshoring variation. The permanent employment effect is thus robust to the control set and to estimator, as well as to the single-sector exclusion (Section 5.3). Job reallocation, by contrast, is the least robust outcome of the study: it is sensitive to the LEHD vintage, does not survive excluding the computers-and-electronics sector, and carries a marginal positive pre-trend (Section 5.3, Table 15).

Table 17: Robustness of the Employment Result

	(1) Baseline	(2) No sector FE	(3) No ind. ctrls	(4) Trade ctrls	(5) OLS
Δ Offshoring	−0.005***	−0.004***	−0.003***	−0.003	−0.005***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Estimator	IV	IV	IV	IV	OLS
Industry controls	Yes	Yes	No	Yes	Yes
Sector FE	Yes	No	Yes	Yes	Yes
Trade controls	No	No	No	Yes	No
First-stage F	496.3	519.2	1476.7	687.5	—
N	86	86	86	84	86

Notes: Dependent variable is the long difference in log employment, 1999–2011 (the paper's headline outcome). Each column reports the offshoring coefficient under a different specification. Baseline: IV with controls (high-tech, log wage, production-worker share, capital–value-added) and sector FE. Trade controls: change in other-country imports and US exports. All regressions weighted by 1991 baseline employment, clustered at NAICS-3. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

Non-manufacturing industries. The extended QWI sample includes non-manufacturing. Restricting to manufacturing (the original sample) reproduces the core findings; including non-manufacturing strengthens the employment effects but weakens the fluidity effects, consistent with offshoring being most relevant for manufacturing.

11 Conclusion

Offshoring to China left a permanent mark on U.S. manufacturing, but a narrow one. It permanently lowered the level of employment and permanently shifted the occupational mix toward less offshorable work; both are stable or deepening through 2024, and the employment effect strengthens when the one sector that drives the rest of the design is excluded (-0.005 overall, -0.017 without computers and electronics; both stable across windows and estimators, though, like every estimate in this 21-cluster design, only marginally significant under the wild cluster bootstrap). It also held down the worker-flow rates, turnover and churn, which do not recover within the sample, because the exposed industries permanently hire less: the worker-flow decline is the flow counterpart of the permanent employment loss. What did recover is the firm-side job-reallocation rate, the measure at the center of the dynamism debate: it falls during the 1999–2011 transition and returns toward zero by the mid-2010s, and it rests almost entirely on computers and electronics, surviving neither the wild cluster bootstrap nor the removal of that sector from the instrument. A task-based model with search frictions accounts for the split: firm-side reallocation returns to its steady-state rate once the transition completes, while the permanent suppression of hiring keeps both employment and worker flows down for good.

This reorients two debates. For the literature on trade adjustment, it reconciles the permanence documented at the worker and regional level with the recovery of the headline dynamism rate: the level of employment and the worker-flow rates are permanently altered, while firm-side job reallocation returns to normal. For the literature on declining business dynamism, it removes a candidate. The offshoring channel is too narrow to account for a fall that is broad across the economy: it runs through one sector, in a tenth of employment, and by the accounting in Table 8 explains on the order of one percent of the economy-wide decline in job reallocation. The cause should be sought elsewhere, in rising market concentration (Akcigit and Ates, 2023), falling responsiveness to productivity shocks (Decker et al., 2020), or demographic change. What offshoring did leave is a permanently altered occupational composition: Table 11 shows the offshorable-occupation employment share roughly 0.75 pp lower per unit of the 1999–2011 shock through 2023, which connects the level effect to the firm- and worker-level offshoring evidence (Monarch et al., 2017; Boehm et al., 2020; Hummels et al., 2014).

For tariff policy the lesson is about timing. Section 301 produced no re-shoring and no fluidity recovery, and the most deeply offshored industries recovered least, the empirical signature of hysteresis. The calibrated model rationalizes the null: its re-shoring threshold is an illustrative schedule that rises with how long ago a task was offshored and, for the China-shock-era stock, sits well above the 2018–2019 rates and plausibly above even the steeper 2025 tariffs. Once supplier relationships are built and domestic skills have depreciated, the adjustment is close to irreversible, consistent with the absence of meaningful Section 301 re-shoring in Amiti et al. (2019), Fajgelbaum et al. (2020), and Flaaen and Pierce (2024).

The timing has a forward-looking counterpart. On the evidence here, trade-adjustment policy is a transition instrument: it can shape how a shock is absorbed while the reallocation is underway, but it does little once the new steady state is in place and domestic skills have depreciated. For the China offshoring shock that window has closed, which is why a decade-late tariff re-shores nothing. For shocks whose transition is underway now, such as the 2025 tariff round and the automation of selective tasks (Acemoglu and Restrepo, 2022), the window is open, and the same logic implies that adjustment support binds while the reallocation is happening rather than after it. I stop short of prescribing which interventions work: the paper identifies what a retroactive tariff fails to do, not what a contemporaneous policy would achieve. The relevant question for adjustment policy is when, not only whether.

More broadly, the distinction between what a structural shock changes permanently and what it changes only in transition should travel to other shocks that operate through selective task elimination, automation (Acemoglu and Restrepo, 2022) among them, where a permanent change in the level and composition of work can coexist with a pace of job reallocation that returns to normal.

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A Appendix: Data Details

A.1 QWI Construction

The Quarterly Workforce Indicators are constructed from the Longitudinal Employer-Household Dynamics (LEHD) program, which links state unemployment insurance records with Census Bureau surveys. I download state-level bulk files from `https://lehd.ces.census.gov/data/qwi/R2026Q1/`, selecting files with firm-age dimension, state geography, NAICS 4-digit industry, and all ownership codes (`qwi_{state}_sa_f_gs_n4_op_u.csv.gz`).

For the original 1999–2011 sample, I restrict to 36 states with consistent coverage from 1999 onward, matching the original analysis. For the extended sample, I use all 51 states.

Quarterly data are collapsed to annual by summing flows (hires, separations, job gains, job losses) and averaging stocks (employment). Rates are computed relative to the DHS denominator $X_{jt} = (E_{jt} + E^e_{jt})/2$.

A.2 Trade Data

Original period (1997–2011). Bilateral trade flows from UN Comtrade at the HS-6 level, concorded to NAICS using the BEA 1997 benchmark input-output tables. Industry-level import penetration computed as $\Delta M^{UC}_{j\tau}/(Y_{j,91} + M_{j,91} - E_{j,91})$.

Extended period (2010–2024). Bilateral HS-6 trade data from the Trade Data Monitor (TDM) for the U.S., Canada, Germany, France, Italy, Japan, and South Korea. Additionally, NAICS-level import data from the Census Bureau International Trade API for 2010–2024 (110 NAICS-4 manufacturing industries). I-O weighted offshoring penetration constructed using the same 1997 BEA shares applied to the updated trade data.

A.3 BEA Input-Output Tables

I use the 1997 benchmark Use and Make tables to construct bilateral intermediate-goods flows between NAICS industries, following the methodology of Acemoglu et al. (2016). The Use table provides commodity purchases by industry; the Make table maps commodities to producing industries. I combine these to obtain the share of each industry's intermediate purchases from every other industry.

A.4 OES Extension for Offshorability Index, 2014–2023

The Blinder (2006) offshorability index is applied to OES industry-by-occupation employment from 2002 to 2023. For 2002–2013 I use the merged panel from the original analysis. For 2014–2023 I extend the construction using the BLS OES national NAICS-4 industry files (`nat4d_MYYYY_dl.xlsx`), merging occupation SOC codes with the 748-occupation Blinder (2006) offshorability index. Coverage across the 86 manufacturing industries in the core sample is complete for 2002–2011, 72% for 2014–2019, and 72% for 2020–2023, reflecting some NAICS-4 suppression in the BLS files. BLS has not yet released the OES 2024 industry file; I substitute 2023 in the long-difference for that window.

A.5 Section 301 Tariff Exposure

The NAICS-4 Section 301 tariff exposure in Table 16 equals $\text{off\_usch}_{j,2017}$, the input-output-weighted 2017 Chinese import penetration, multiplied by the weighted-average Section 301 rate of 0.17 across USTR lists 1–4a (Fajgelbaum et al., 2020; Amiti et al., 2019). The penetration is built (`code/33_build_section301_exposure.py`) from Census NAICS-level 2017 U.S. imports from China—so no HS-to-NAICS concordance is needed—over a fixed 1991 shipments base from the NBER-CES database, input-output weighted over same-3-digit-sector inputs exactly as the headline offshoring measure (equation (10)); a companion own-industry import-penetration version gives the same pattern. This is a level proxy for the ideal measure, full HS-10 USTR coverage mapped to NAICS-4. Because more China-import-intensive industries were overrepresented in the USTR lists, the measure if anything biases toward finding a positive tariff-fluidity relationship were the hysteresis null false; the data show none that survives controls.

B Appendix: Summary Statistics

Table 18: Summary Statistics (All Firms, NAICS-4 Industries)

Variable	Mean	SD	Min	Median	Max	N
Δ Offshoring Penetration (US–China)	1.9680	2.2873	0.1101	1.4863	16.1479	86
Δ Offshoring Penetration (Other–China)	2.7059	2.4097	0.1875	2.4508	15.3254	86
Δ Employment	-0.0153	0.0300	-0.1052	-0.0100	0.0255	86
Δ Job Reallocation	-0.1479	0.0989	-0.4634	-0.1477	0.2047	86
Δ Turnover	-0.1515	0.0774	-0.3134	-0.1550	0.0429	86
Δ Churn	-0.0907	0.0545	-0.2364	-0.0834	0.0330	86
Employment (1999 level)	826328.6081	667708.8261	58235.4105	638891.7953	3423126.4914	86
Job Reallocation (1999 level)	8.8830	3.0504	5.2186	8.1329	24.6642	86
Turnover (1999 level)	8.5850	1.6433	4.9867	8.4143	13.1152	86
Churn (1999 level)	12.1330	5.4503	3.3176	10.7460	35.4003	86

Notes: Sample restricted to `firmage==0` (all firms aggregate). Δ variables are long differences 1999–2011. Employment-weighted NAICS-4 industries ($N = 86$).

C Appendix: Reduced-Form Estimates

Table 19 reports the reduced-form relationship between the instrument (Chinese export growth to other countries) and U.S. labor market outcomes, bypassing the 2SLS second stage. The reduced-form coefficients are proportional to the IV estimates and avoid potential finite-sample bias from 2SLS. All reduced-form effects on fluidity are significant for 1999–2011 and insignificant for longer windows, confirming the transitional pattern.

Table 19: Reduced-Form Estimates: Instrument on Outcomes

	1999–2011 (1)	1999–2019 (2)	1999–2024 (3)	2015–2024 (4)
Employment	-0.005^{***}	-0.004^{***}	-0.004^{***}	-0.003^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Job Reallocation	-0.006^{*}	-0.004^{**}	-0.002	0.005^{**}
	(0.003)	(0.002)	(0.002)	(0.002)
Turnover	-0.009^{***}	-0.007^{***}	-0.005^{***}	0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Churn	-0.005^{***}	-0.004^{***}	-0.003^{***}	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Sector FE	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes
N	86	86	86	86

Notes: Each cell reports the OLS coefficient of the instrument (other-country–China offshoring, Δ 1999–2011) regressed directly on the indicated outcome. This reduced-form specification avoids potential 2SLS finite-sample bias. All regressions include sector FE and industry controls, weighted by 1991 baseline employment, clustered at NAICS-3. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

D Appendix: The Speed-Duration Prediction

The model's auxiliary prediction is that industries which offshored faster should show larger transitional fluidity declines but earlier normalization (Proposition 2 and Appendix F). I test it by splitting industries at the median share of their total 1999–2011 employment decline that occurred by 2005: "early offshoring" industries experienced more than half their decline before 2005, "late offshoring" industries after. Table 20 and Figure 8 report the job-reallocation IV by group and window.

The data do not support the prediction. Early-offshoring industries do not show the predicted large transitional decline; their job-reallocation coefficients are small and, in the medium-run windows, significantly *positive* (+0.008 for 1999–2019 and +0.006 for 1999–2024, both $p < 0.01$). Late-offshoring industries show negative point estimates (-0.008 to -0.014) but none is statistically significant. I report the test for completeness; it does not corroborate the speed-duration mechanism, and the paper does not rely on it. This is of a piece with the broader fragility of the fluidity results documented in Section 5.3: once job reallocation is measured on the refreshed LEHD vintage, the sub-sample timing contrasts that the original analysis emphasized do not survive.

Table 20: Speed-Duration Test: Early vs. Late Offshoring Industries

	1999–2011 (1)	1999–2019 (2)	1999–2024 (3)	2015–2024 (4)
Early offshoring				
Δ Offshoring	0.012	0.008***	0.006***	0.011
	(0.008)	(0.003)	(0.002)	(0.009)
N	28	28	28	28
Late offshoring				
Δ Offshoring	-0.008	-0.009	-0.010	-0.014
	(0.018)	(0.013)	(0.010)	(0.016)
N	28	28	28	28
Sector FE	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes

Notes: Each cell reports the IV coefficient of offshoring penetration (Δ 1999–2011) on the long difference in job reallocation for the indicated period. Industries split at the median of the share of total 1999–2011 employment decline occurring by 2005. "Early offshoring" industries experienced more than half their total decline before 2005. All regressions include sector FE and industry controls (high-tech indicator, log avg. wage, production-worker share, capital–value-added ratio), weighted by 1991 baseline employment, clustered at NAICS-3. $^{***}\,p < 0.01$; $^{**}\,p < 0.05$; $^{*}\,p < 0.10$.

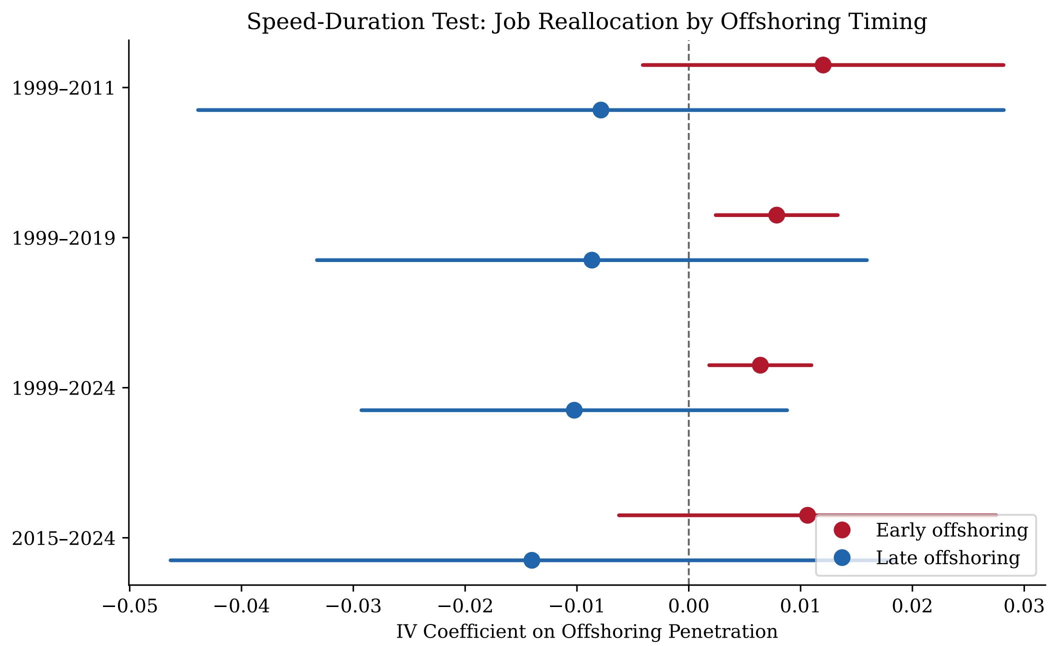

Figure 8: Speed-Duration Test: IV Coefficients by Offshoring Timing

Notes: IV coefficient on offshoring penetration (Δ 1999–2011) on the long difference in job reallocation, estimated separately for industries above and below the median share of 1999–2011 employment decline occurring by 2005, with 95% confidence intervals. The predicted ranking (early-offshoring industries showing a larger but shorter-lived decline) does not appear in the data.

E Appendix: Firm-Age Heterogeneity

The firm-age estimates appear in the main text (Table 4, Section 5.3); this appendix records the model mechanism behind them. The job-reallocation response concentrates in entrants because young firms face high productivity uncertainty and substitute offshore inputs for the irreversible domestic hiring that expansion requires, while the turnover mechanism—the task-separation rate δ—is a task characteristic and so is age-invariant (Section 2.7). That the entrant effect, alone among the fluidity results, survives the NAICS-334 exclusion is consistent with this margin operating across the offshoring-exposed industries rather than within the single sector that identifies the aggregate fluidity decline.

F Appendix: Full Theoretical Model

This appendix provides the complete derivation of the theoretical framework summarized in Section 2. The model combines Grossman and Rossi-Hansberg (2008) task-based production with DMP search frictions, Dix-Carneiro (2014) sector-specific human capital, and sunk costs of offshoring relationships.

F.1 Environment and Production

Time is discrete, indexed by $t = 0, 1, 2, \ldots$. There is a continuum of industries $j \in \mathcal{J}$, each containing a continuum of tasks $i \in [0, 1]$. A representative firm produces output using a CES aggregate:

$$Y_t = \theta_t \left[\int_0^1 y_{it}^{\frac{\sigma-1}{\sigma}} \, di \right]^{\frac{\sigma}{\sigma-1}}, \quad \sigma > 1 \tag{14}$$

Each task can be produced domestically ($y_{it} = \alpha_i n_{it}$) or offshored ($y_{it} = m_{it}/\omega(i,t)$), where $\omega(i,t) = \bar{\omega}(i) \cdot \Omega_t$ with $\bar{\omega}'(i) > 0$ and $\dot{\Omega}_t \leq 0$ (Assumption 1).

The offshoring cutoff $\hat{\imath}_t$ satisfies $c_t/\alpha_{\hat{\imath}} = p^* \omega(\hat{\imath}_t, t)$.

F.2 Labor Market: DMP Frictions

Workers separate with probability δ per period. Firms post vacancies at cost κ, with matching rate $q(\vartheta_{it})$ where $\vartheta_{it} = v_{it}/u_{it}$ is task-level labor market tightness.

Match value. The value of a filled position in domestic task i:

$$J_{it} = \alpha_i p_{it}^Y - w_{it} + \beta(1-\delta) \, \mathbb{E}_t[\nVdash\{i \geq \hat{\imath}_{t+1}\} \cdot J_{i,t+1}] \tag{15}$$

Free entry. Vacancy posting satisfies:

$$\frac{\kappa}{q(\vartheta_{it})} = \beta \, S_{it}(1) \cdot J_{i,t+1} \tag{16}$$

where $S_{it}(\tau) = \Pr(\hat{\imath}_{t+\tau} \leq i)$ is the survival probability of task i.

Nash wage. With bargaining power $\mu \in (0, 1)$:

$$w_{it} = \mu(\alpha_i p_{it}^Y + \kappa \vartheta_{it}) + (1-\mu) b \tag{17}$$

F.3 Steady-State Analysis

In steady state, flow balance requires $\delta n_i^* = q(\vartheta_i^*)v_i^*$ for each domestic task. Total employment is $L^* = \int_{\hat{\imath}}^1 n_i^* di$, and total hires equal total separations: $H^* = S^* = \delta L^*$.

Proposition 3 (Steady-state fluidity rates)**.** *In any steady state:* $TO^* = 2\delta$*,* $CH^* = 2\delta$*,* $JR^* = 0$*. Fluidity rates are independent of the offshoring cutoff.*

Proof. $TO^* = (H^* + S^*)/L^* = 2\delta L^*/L^* = 2\delta$. $JR^* = 0$ because no task expands or contracts in steady state. $CH^* = TO^* - JR^* = 2\delta$. □

F.4 Transitional Dynamics: Task Regions

During the transition ($\hat{\imath}_t$ rising, $t \in [0, T]$), tasks partition into:

1. **Always-offshored** ($i < \hat{\imath}_0$): no labor market implications.
2. **Newly-offshored** ($i \in [\hat{\imath}_0, \hat{\imath}_t]$): workers separated without replacement.
3. **Surviving domestic** ($i > \hat{\imath}_t$): further split into *secure* ($i \gg \hat{\imath}_t$) and *marginal* (i near $\hat{\imath}_t$).

F.5 Vacancy Suppression in the Shadow Zone

Lemma 1 (Vacancy suppression)**.** *For* $i' > i > \hat{\imath}_t$*: (a)* $S_{i't}(\tau) > S_{it}(\tau)$*; (b)* $\vartheta_{i't} > \vartheta_{it}$*; (c)* $\vartheta_{it} \to 0$ *as* $i \to \hat{\imath}_t$*.*

Proof. (a) follows from monotonicity of $\hat{\imath}_t$. (b) From free entry, lower S_{it} requires lower ϑ_{it} (since $\kappa/q(\vartheta)$ is increasing). (c) As $S_{it}(1) \to 0$, the RHS of free entry $\to 0$, forcing $\vartheta_{it} \to 0$. □

The shadow zone width is approximately $\Delta_t \approx \dot{\hat{\imath}}_t/(r + \delta)$.

F.6 Transitional Labor Flows

Total hires and separations:

$$H_t = \int_{\hat{\imath}_t}^1 q(\vartheta_{it})v_{it}\, di \tag{18}$$

$$S_t = \delta \int_{\hat{\imath}_t}^1 n_{it}\, di + \int_{\hat{\imath}_{t-1}}^{\hat{\imath}_t} n_{i,t-1}\, di \tag{19}$$

Proposition 4 (Transitional fluidity decline). *During the transition: (a) $H_t < \delta L_t$; (b) $CH_t < 2\delta$; (c) $TO_t < 2\delta$ when offshoring is gradual.*

Proof. (a) Domain shrinks + shadow zone suppression. (b) $CH_t \leq 2\min(H_t, S_t)/X_t < 2\delta$. (c) H_t decline dominates small per-period task-elimination separations. □

The deviation from steady state is approximately:

$$CH_0^* - CH_t \approx 2\left[\frac{\dot{\hat{\imath}}_t \cdot \bar{n}}{L_t}\right] \cdot \frac{1}{r+\delta} \tag{20}$$

F.7 Normalization

Proposition 5 (Post-transition convergence). *After $t > T$: (a) $L_t \to L_1^*$ at rate δ; (b) shadow zone vanishes; (c) $TO_t \to 2\delta$, $CH_t \to 2\delta$. Half-life:* $\ln 2/\delta$.

With sector-specific human capital (transferability $\phi < 1$, reallocation rate λ), the effective half-life is $\ln 2/\min(\delta, \lambda) \approx 8.7$ years for $\lambda = 0.08$.

F.8 Firm Age

Firm productivity follows $\ln \theta_t^a = \rho(a) \ln \theta_{t-1}^{a-1} + \varepsilon_t$ with $\rho'(a) > 0$ (young firms more volatile). Higher variance makes young firms prefer flexible offshore inputs over irreversible domestic hiring, concentrating JR effects in young firms while turnover effects (operating through δ) are age-uniform.

F.9 Hysteresis

Sunk offshoring costs F^O and re-shoring costs F^R create a band of inaction:

$$\hat{\imath}_t^{\text{offshore}} - \hat{\imath}_t^{\text{re-shore}} = \frac{r(F^O + F^R)}{p^* \omega'(\hat{\imath}_t)} \tag{21}$$

With human capital depreciation ($\tilde{\alpha}_i(t) = \alpha_i e^{-\eta(t-t_0)} + \underline{\alpha}(1 - e^{-\eta(t-t_0)})$) and tariff uncertainty (removal probability π), re-shoring requires:

$$\tau > \frac{r+\pi}{r}\left[\frac{r(F^O + F^R)}{p^* \omega(\hat{\imath}, t)} + \frac{1 - e^{-\eta s}}{\omega(\hat{\imath}, t)}\right] \tag{22}$$

For $\tau = 0.25$, $s \geq 10$ years: this condition fails $\Rightarrow$ no re-shoring.

F.10 Calibration and Quantitative Results

Parameter	Description	Value	Source
δ	Separation rate (quarterly)	0.12	QWI
β	Discount factor	0.99	Standard
κ	Vacancy cost	0.15	Shimer (2005)
σ	Task substitution	3.0	Assumed (sensitivity below)
γ	Offshoring cost decline	0.04/yr	China WTO
ϕ	Skill transferability	0.65	Dix-Carneiro
λ	Reallocation rate	0.08/yr	CPS
η	Skill depreciation	0.05/yr	Assumed
F^O, F^R	Sunk costs	1.0, 1.5	Normalized (annual task revenue)
π	Tariff removal prob.	0.12/yr	Assumed (sensitivity below)
b	Task-cost gradient	3.49	Calibrated to emp. −28%

Quantitative implementation. All numbers below are produced by `code/32_model_solver.py`, which closes the model with two explicit assumptions the symbolic statement leaves free: a log-linear task-cost schedule $\bar{\omega}(i) = \omega_0 e^{bi}$ and unit-elastic industry demand over the CES task aggregate. The initial offshored task share is set to $\hat{\imath}_{1999} = 0.05$ (pinning ω_0), and the cost gradient b is the single calibrated parameter, chosen so the model reproduces the 1999–2011 manufacturing employment decline of 28%; this requires $b = 3.49$ and implies the offshored task share rises from 5% to 18.7% as Ω_t falls 38% ($\gamma = 0.04$ over twelve years). At $\sigma = 2$ the same target requires $b = 2.35$ ($\hat{\imath}_{2011} = 0.25$); at $\sigma = 4$ the expenditure shift toward cheapened offshore tasks alone delivers more than the target for any b.

Implied dynamics.

- *Transition flow effects, 1999–2011.* The calibrated cutoff path implies a task-elimination job-destruction flow of about 1.3 pp/yr and a shadow zone of $\Delta_t = \dot{\hat{\imath}}/(r + \delta) \approx 0.026$ of the task space with vacancy posting suppressed, a replacement-hiring shortfall of about 1.3 pp/yr. Against the QWI manufacturing baselines, the two forces each amount to roughly 6% of baseline job reallocation; the model thus generates a transition decline in measured reallocation on the order of 10% of baseline. It does not pin a sharper number without a full matching-function and wage block, and I do not lean on one.

- *Recovery.* Firm-side reallocation converges back at rate λ; half-life $\ln 2/\lambda = 8.7$ years for $\lambda = 0.08$, placing recovery in the mid-2010s, as observed.

- *Re-shoring thresholds.* Equation (6) evaluated on the calibrated cost path gives a threshold schedule $\tau^*(s, \pi)$ for a task offshored s years before the 2018 tariff: at the baseline $\pi = 0.12$, $\tau^* = 62\%$ for $s = 1$, 155% for $s = 5$, and 291% for $s = 10$; even at $\pi = 0$, $\tau^*(1) = 16\%$ and $\tau^*(3) = 27\%$, so the 10–25% Section 301 rates could at most have re-shored tasks offshored within about two years of the tariff. The China-shock-era stock sits far above any plausible rate: no tasks re-shore, and hysteresis binds for everything offshored before roughly 2015.