Labor Adjustment Costs in French Viticulture: Evidence from Trade-Cost Shocks

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Abstract

We study labor adjustment on French wine farms using RICA/FADN panel data (2003–2024). Higher sales predict a higher probability of adjusting temporary and salaried employment. Organic-adopting farms respond less strongly, with a sales elasticity of temporary labor roughly three-quarters of the full-sample estimate. Instrumenting farm sales with pre-determined regional export exposure to foreign demand shocks supports a causal interpretation for the wine sector.

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1 Introduction

Viticulture is among the most pesticide- and fungicide-intensive agricultural activities.[1] Reducing chemical inputs is therefore a central environmental and public-health objective in wine-producing regions.[2] Yet, despite sustained growth in consumer demand for organic wine, the supply response appears limited and has recently stalled: while organic wine sales grew by 9% in 2023 and 8% in 2024, the area under organic vineyards declined by 4% in 2024, and the organic share of French vineyard area fell from 22% to 21% (Agence Bio, 2024). Why?

In this paper, we argue that one explanation for this sluggish expansion lies in the high fixed costs of adjusting labor in organic viticulture. Pest and weed management without synthetic inputs, compliance and record-keeping requirements, and the organizational adaptations required by alternative practices all impose additional burdens on an already labor-intensive production system (Crowder and Reganold, 2015; Carneiro et al., 2024). These frictions may make the organic workforce quasi-fixed in the sense of Oi (1962)—a factor whose adjustment involves fixed and sunk costs that create inertia even when market conditions are favorable.

We study the French wine sector over 2003–2024 using farm-level microdata from the French Farm Accountancy Data Network (RICA/FADN), distinguishing conventional, quality-wine (PDO/PGI), and organic producers. Two stylised facts motivate the analysis. First, year-to-year labor changes display a pronounced mass near zero—a large share of farm-year observations involve changes so small as to be economically negligible. Second, this inertia coexists with substantial volatility in wine sales. The disconnect between volatile sales and inert labor is consistent with large adjustment costs, as Cooper and Willis (2009) document in manufacturing—a finding we bring to agriculture using farm-level panel data.

Empirically, we model labor adjustment as a discrete outcome—the probability that a farm changes its workforce by more than a *de minimis* threshold—and estimate fixed-effects linear probability models. The probability of adjusting temporary and salaried employment rises significantly with sales for the full sample and quality-wine farms. For organic-adopting farms—defined as those that receive the organic production subsidy at any point during the panel, tracked over the full 2003–2024 period—the binary indicator is insignificant, but a continuous

[1] With approximately 15 to 20 chemical treatments per year, the wine sector ranks below apple and peach production but well above all other arboricultural crops (Simonovici et al., 2019; Cretin and Triquenot, 2018).

[2] Although not specifically applied to the wine sector, several studies document the detrimental effects of fungicides and pesticides on health; see among others Chatzimichael et al. (2021) and Calzada et al. (2023).

specification (log temporary labor) reveals a significant sales elasticity roughly three-quarters of the full-sample estimate. Outsourcing provides an additional adjustment margin for all farm types, and especially for organic-adopting farms, whose outsourcing response to shocks is roughly twice the full-sample estimate—consistent with substitution between external service providers and on-farm workforce adjustment.

To address endogeneity, we instrument farm sales using a structural gravity approach based on the trade-flow tetrads of Caliendo and Parro (2015), exploiting variation in bilateral trade costs between non-French wine exporters and France's destination markets, with pre-determined regional export exposure. The instrument has a shift-share structure and passes its key validity test.[3] The causal effect of demand on temporary labor adjustment is significant and robust across a wide range of *de minimis* thresholds.[4]

These findings shed light on the organic conversion puzzle. The IV confirms that demand causally raises the probability of adjusting temporary labor. Organic-adopting farms respond to demand—the continuous specification shows a significant elasticity—but less strongly than conventional producers. We cannot determine whether this weaker response reflects the organic production mode itself or selection of inherently less responsive farms into organic conversion.

Our analysis contributes to the quasi-fixed labor tradition (Oi, 1962; Hamermesh, 1989; Hamermesh, 2017) and the non-convex adjustment-cost literature (Abel and Eberly, 1993; Cooper and Haltiwanger, 2006; Cooper and Willis, 2009) by providing farm-level causal evidence in a high-value perennial crop. While prior agricultural work has examined capital adjustment costs (Pietola and Myers, 2000; Gardebroek, 2004), we focus on labor, where the quasi-fixity stems from human capital and organizational frictions rather than financial constraints. Our analysis also speaks to the growing literature on how agricultural labor responds to shocks (Hornbeck and Naidu, 2014; Charlton and Taylor, 2016; Emerick, 2018) and to the empirical trade literature on trade shocks and labor markets (Autor et al., 2013; Malgouyres, 2016), shifting the focus from regional employment to within-farm adjustment and its dependence on production mode.

The remainder of the paper is organized as follows. Section 2 presents stylised facts, the adjustment-cost framework, and the data. Section 3 introduces the baseline specification and OLS evidence. Section 4 examines outsourcing. Section 5 develops the IV strategy and presents causal evidence. Section 6 concludes.

[3]The instrument follows Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022); a share exogeneity test fails to reject the null that the 2007 regional export shares are uncorrelated with differential outcome trends.

[4]The Anderson–Rubin confidence interval excludes zero under region-level clustering, and the Webb (2023) wild cluster bootstrap independently supports reduced-form significance.

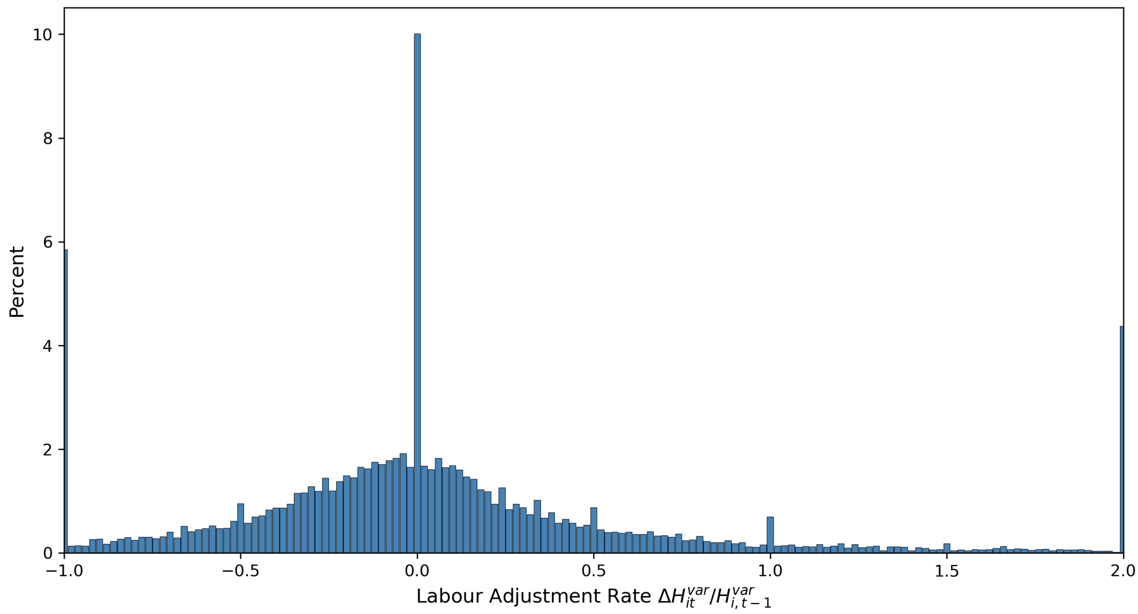
2 Background

2.1 Stylised facts: the sales–labor gap

A basic assumption in microeconomic textbooks is that labor adjusts instantaneously to minimize costs. In practice, a long literature beginning with Clark (1923) has emphasized that labor is often quasi-fixed (e.g., Bouchet et al., 1989; Moschini, 1989). In viticulture, quasi-fixity is arguably even stronger: grapevines are perennial, many tasks require plot-specific knowledge, and production under quality schemes adds rigidity through rules on practices and yields. For organic production, these frictions are amplified further through pest management without synthetic inputs, compliance requirements, and organizational adaptation (Carneiro et al., 2024).

These mechanisms are visible in two descriptive patterns that, taken together, reveal substantial adjustment costs.

Fact 1: Labor changes cluster near zero, with secondary spikes at large adjustments. Figure 1 plots the distribution of signed relative year-to-year changes in temporary labor, $\Delta H_{it}^{var}/H_{i,t-1}^{var}$, separating expansion (positive) from contraction (negative). Three features stand out. First, a pronounced spike near zero: about 10% of farm-year observations involve changes below 1% in absolute value, which for a farm employing two annual work units corresponds to roughly 32 hours over a year. These are economically negligible changes that our *de minimis* threshold filters out. Second, secondary concentrations of mass appear at large positive and large negative changes—roughly 16% of observations involve expansions exceeding 50%, and 14% involve contractions of comparable magnitude. This bimodal pattern—inaction interspersed with occasional large adjustments—is a signature of fixed adjustment costs in the investment literature (Cooper and Haltiwanger, 2006; Caballero and Engel, 1993). It is consistent with the "piecewise quadratic" cost structure that Cooper and Willis (2009) find sufficient to match aggregate labor moments in US manufacturing: the fixed component generates the inaction region, while the convex component governs the size of adjustments once the farm decides to act. Third, the expansion spike is sharper than the contraction spike. This asymmetry reflects the nature of temporary agricultural labor: positive changes correspond to hiring additional seasonal workers, while negative changes reflect fewer hires the following season rather than active dismissals—seasonal contracts simply expire at the end of the campaign. The adjustment margin is therefore fundamentally a hiring margin, operating through the volume of new contracts rather than through separations. Appendix A shows that quality-wine and organic-adopting farms display comparable distributions.



Note: Distribution of signed relative year-to-year changes in temporary labor, $\Delta H_{it}^{var} / H_{i,t-1}^{var}$. Farm-years with zero lagged temporary labor ($H_{i,t-1}^{var} = 0$) are excluded from this figure (the relative change is undefined); these observations are handled by the absolute-change criterion in the regression indicator. Sample: all wine farms, RICA/FADN 2003–2024, survey-weighted. Bin width: 2 percentage points.

Figure 1: Temporary labor adjustment rate distribution

Fact 2: Sales are volatile. Wine farm sales follow a pronounced cyclical pattern: they grew during the early-2000s phase of "hyper-globalisation" (Subramanian, Kessler, et al., 2013), dropped sharply during the financial crisis, rose again with the expansion of Chinese demand, and then weakened with the subsequent slowdown in that market (Candau et al., 2016; Bazen and Cardebat, 2022). More recently, additional disruptions—including adverse weather (e.g., in 2017, with early frosts and hail) and trade policy tensions affecting access to the US market (Zhang et al., 2021)—coincided with another downturn. Quality-wine farms (Protected Designation of Origin / Protected Geographical Indication, hereafter PDO/PGI) and organic-adopting farms experience comparable volatility. Despite the rapid expansion of the organic wine sector—which grew from 9% to nearly 22% of French vineyard area between 2012 and 2023, reaching 171,000 hectares and over 12,000 producers (Agence Bio, 2024)—this sales volatility, combined with the labor inertia documented in Fact 1, is all the more striking given that organic viticulture is among the most labor-intensive forms of farming.

If adjustment were costless, the workforce would track sales far more closely. The gap between volatile sales and inert labor is consistent with substantial adjustment costs.

2.2 Conceptual framework

We interpret these patterns through the lens of non-convex adjustment costs, following the lumpy investment logic of Abel and Eberly (1993) and Cooper and Haltiwanger (2006), adapted to labor and cost minimisation. When a farm changes labor from $H_{i,t-1}$ to H_{it}, it incurs

$$\Psi_i(H_{it}, H_{i,t-1}) = c_i\,\mathbf{1}\{H_{it} \neq H_{i,t-1}\} + \frac{\phi}{2}(H_{it} - H_{i,t-1})^2, \tag{1}$$

where $c_i \geq 0$ is a farm-specific fixed cost and $\phi \geq 0$ governs convex adjustment costs. The fixed component generates an inaction region: small shocks are absorbed without changing H_{it}, while sufficiently large shocks trigger discrete adjustments. Figure 1 confirms this structure: the distribution of labor changes exhibits a pronounced spike at zero alongside secondary concentrations at large positive and negative changes—the bimodal pattern predicted by fixed adjustment costs (Caballero and Engel, 1993).

A key implication concerns state dependence. If the fixed cost depends on market conditions—$c_i(S_{it})$ decreasing in sales S_{it}—then the inaction region narrows when sales are strong and widens when sales decline. Strong demand facilitates hiring and training, lowering the threshold for expanding the seasonal workforce; weak demand raises the relative cost of adjusting, so farms are more likely to remain inert. In the context of temporary agricultural labor, "inaction" does not mean physical inaction but inertia in the hiring strategy: re-hiring the usual volume of seasonal workers is the administrative default, while changing the recruitment target involves organizational costs (search, training, scheduling). When demand weakens, some farms do reduce hiring—and those that do trigger the adjustment indicator—but the positive $\hat{\beta}$ implies that the probability of *any* change (up or down) is higher when sales are strong than when they are weak. The model therefore predicts that the probability of changing the workforce covaries with sales, a prediction we test in Section 3 and exploit causally in Section 5.

2.3 Data

We use farm-level panel data from the French RICA/FADN, an annual survey that collects detailed accounting information for a representative sample of farms. We focus on wine producers over 2003–2024 and use extrapolation weights in all estimates. The panel extends to 2001 for the construction of lagged variables.

Labor is measured in annual work units (UTA). We use total labor H_{it}^{tot}, permanent (salaried and family) labor H_{it}^{fix}, and temporary labor $H_{it}^{var} = \max\{H_{it}^{tot} - H_{it}^{fix}, 0\}$. We also consider salaried labor H_{it}^{sal}, which includes both permanent employees and seasonal workers but excludes unpaid family labor; this measure captures the workforce margin subject to market hiring and separation costs. We define an adjustment indicator equal to one when the absolute relative change exceeds a *de minimis* threshold. Our baseline uses 1%.[5]

Market conditions are proxied by lagged log deflated sales. The control is lagged capital ($\ln K_{i,t-1}$). We estimate models for the full wine sample, quality-wine farms (PDO/PGI), and organic-adopting farms—defined as farms that receive the organic production subsidy (SBVBIO) at any point during the sample period, tracked over the full 2003–2024 panel. All monetary variables are deflated using INSEE agricultural price indices. Appendix B provides variable definitions and survey-weighted descriptive statistics (Table 6). The estimation sample is smaller than the descriptive sample because lagging loses the first observation per farm and singleton farms are dropped from the fixed-effects estimator.

3 Baseline Specification and OLS Evidence

3.1 Empirical specification

We estimate a two-way fixed-effects linear probability model:

$$A_{it} = \alpha_i + \mu_t + \beta \ln S_{i,t-1} + \delta \ln K_{i,t-1} + u_{it}, \tag{2}$$

where A_{it} equals one if farm i adjusts temporary labor in year t (exits the *de minimis* band), $\ln K_{i,t-1}$ is lagged capital, and u_{it} is the error term. Farm fixed effects α_i absorb time-invariant heterogeneity; year fixed effects μ_t absorb common

[5]For temporary and salaried labor, which have a large mass at zero, we complement the relative criterion with an absolute threshold: an observation is classified as inaction when either the absolute change is below a *de minimis* level or, when the lagged level is positive, the relative change is below 1%. The absolute threshold is 0.02 UTA for temporary labor and 0.05 UTA for salaried labor—the higher salaried threshold reflects the coarser measurement of this aggregate. For total and permanent labor, only the 1% relative threshold applies.

shocks. The coefficient β is identified from within-farm sales variation net of aggregate time shocks. Standard errors are clustered at the farm level. We estimate equation (2) separately for temporary labor—seasonal workers and short-term contracts—and for salaried labor, which adds permanent employees to the seasonal component but excludes family workers. The comparison allows us to assess whether demand-driven adjustment operates through the flexible seasonal margin alone or extends to the broader hired workforce. Total and permanent labor results are in Appendix C.

The 1% *de minimis* threshold is not a structural inaction band estimated from a dynamic optimisation problem (Caballero and Engel, 1993)—it is a measurement-robust filter that separates economically meaningful workforce changes from noise in the accounting data. Two features of the RICA labor measure make such a filter necessary: labor in UTA is constructed from annual accounting information and may be mechanically insensitive to small within-year reallocations, and reporting and rounding practices can generate small apparent changes even when the underlying organisation is unchanged.

3.2 Baseline results

Table 1 reports estimates of equation (2). For both temporary and salaried labor, $\hat{\beta}$ is positive and significant at the 1% level for the full sample and quality-wine farms, with comparable point estimates across the two margins. For organic-adopting farms, the coefficient is small and insignificant on both margins in the binary specification.

Since A_{it} is a binary indicator, the LPM coefficients represent percentage-point changes in the adjustment probability. A coefficient of 0.069 means that a 10% increase in sales predicts a 0.69 percentage-point higher adjustment probability, off a baseline rate of 70%—a modest but precisely estimated effect. This coefficient is stable across alternative control sets: adding chemical inputs and twice-lagged labor leaves the estimate in the range 0.058–0.067 (Table 11 in Appendix E). The within R-squared is low (0.004), consistent with adjustment decisions depending heavily on idiosyncratic factors (weather, vintage, local labor availability) beyond aggregate sales. Since $\hat{\beta} > 0$, higher sales predict a higher probability of changing the temporary workforce. In practice, the adjustment margin is predominantly a hiring margin: farms expand by recruiting additional seasonal workers when demand is strong, and contract passively by hiring fewer workers the following campaign when demand weakens. Because seasonal and fixed-term contracts (CDD) expire at the end of each campaign, downward adjustment does not involve dismissals—it is the natural consequence of not renewing expiring contracts. This holds for both temporary and salaried labor.

	Temporary			Salaried		
	All	Quality	Bio-adopt.	All	Quality	Bio-adopt.
Farm sales ($\ln S_{i,t-1}$)	0.069***	0.080***	0.031	0.068***	0.079***	-0.015
	(0.016)	(0.019)	(0.042)	(0.017)	(0.021)	(0.042)
Capital ($\ln K_{i,t-1}$)	0.004	-0.004	0.046	0.013	0.006	0.068*
	(0.013)	(0.014)	(0.032)	(0.011)	(0.011)	(0.035)
R-squared (within)	0.004	0.004	0.003	0.003	0.004	0.004
Observations	20,997	18,211	2,825	20,997	18,211	2,825
Farms	2,851	2,481	307	2,851	2,481	307

Note: Two-way fixed-effects linear probability model estimated by WLS with RICA survey weights. Sample: wine farms, 2003–2024. Dependent variable: indicator equal to one if absolute relative labor change exceeds 1%. Columns 1–3: temporary labor (seasonal and short-term); columns 4–6: salaried labor (all hired workers, excluding unpaid family). Control: $\ln K_{i,t-1}$. Farm and year FE. Standard errors clustered at the farm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 1: OLS FE-LPM: probability of adjusting labor

Organic-adopting farms and the continuous specification. The binary indicator is underpowered for the organic-adopting subsample: the minimum detectable effect at 80% power is 0.12, which exceeds the full-sample coefficient (0.069). The binary indicator also discards information about the magnitude of adjustment. Table 2 presents the continuous specification, replacing the binary indicator with log temporary labor. The sales elasticity is 0.292 for all farms and 0.306 for quality-wine farms, both significant at the 1% level. For organic-adopting farms, the elasticity is 0.225 ($p < 0.05$)—roughly three-quarters of the full-sample elasticity. Organic-adopting farms do adjust temporary labor in response to sales, but less strongly than conventional farms.

	All	Quality	Bio-adopters
Farm sales ($\ln S_{i,t-1}$)	0.292***	0.306***	0.225**
	(0.040)	(0.044)	(0.094)
Capital ($\ln K_{i,t-1}$)	0.096***	0.076***	0.103
	(0.027)	(0.028)	(0.063)
R-squared (within)	0.773	0.781	0.767
Observations	15,922	14,038	2,329
Farms	2,036	1,802	240

Note: Two-way FE (farm + year) estimated by WLS with RICA survey weights. Dependent variable: $\ln H_{it}^{var}$ (log temporary labor in UTA). Farm-years with zero temporary labor excluded. Control: $\ln K_{i,t-1}$. Farm-clustered SEs. Sample: wine farms, 2003–2024. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: OLS: log temporary labor on sales

3.3 Diagnostic tests

Although the binary LPM is insignificant for organic-adopting farms, the continuous specification (Table 2) shows a significant elasticity. To probe whether the weaker organic-adopting response reflects compositional artifacts, we augment the baseline with interaction terms:

$$A_{it} = \alpha_i + \mu_t + \beta \ln S_{i,t-1} + \boldsymbol{\theta}' \mathbf{W}_{it} + \delta \ln K_{i,t-1} + u_{it}. \tag{3}$$

Organic exposure. Because the organic subsidy identifier (SBVBIO) is available only from 2015, year fixed effects may absorb the sales shock if it is common to all organic-adopting producers in the post-2015 period. We set $\mathbf{W}_{it} = \big(\text{Exp}_i^{org} \times \ln S_{i,t-1},\ \text{Exp}_i^{org} \times \ln S_{t-1}^{org},\ \text{Exp}_i^{org} \times T_t\big)$, where Exp_i^{org} equals one if the farm is organic-adopting, $\ln S_{t-1}^{org}$ is the weighted mean of log-sales among organic-adopting farms, and T_t is a linear time trend. The first interaction allows organic-adopting farms their own idiosyncratic sales slope rather than forcing them to share the full-sample $\hat{\beta}$. The organic-specific sales slope is negative but insignificant (-0.038, $p = 0.42$ for temporary labor), confirming the weaker response documented in the subsample analysis. The organic demand-state interaction is negative and marginally significant for temporary labor (Table 3, column 1), meaning that organic-adopting farms are less likely to adjust when organic sales are high—if anything reinforcing the weaker organic response rather than explaining it away. The trend interaction and both salaried-labor interactions (column 3) are insignificant.

Organizational rigidity. Organic-adopting farms disproportionately adopt co-operative legal forms (GAEC/EARL) whose labor is structurally rigid. We set $\mathbf{W}_{it} = \left(\text{Coop}_i \times \ln S_{i,t-1},\ \text{Large}_i \times \ln S_{i,t-1}\right)$.[6] For temporary labor, neither interaction is significant (column 2). For salaried labor (column 4), the cooperative interaction is negative and marginally significant, and the large-farm interaction is negative and significant, indicating that both cooperative and corporate farms adjust salaried employment less in response to sales than individual farms—consistent with the greater structural rigidity of family-based cooperative labor and the administrative inertia of larger corporate structures. Results for total and permanent labor (Table 8 in Appendix C) are qualitatively similar.

	Temporary		Salaried	
	Org. exp.	Legal form	Org. exp.	Legal form
Farm sales ($\ln S_{i,t-1}$)	0.073***	0.081***	0.077***	0.102***
	(0.017)	(0.022)	(0.019)	(0.027)
Organic × $\ln S_{i,t-1}$	-0.035		-0.082*	
	(0.047)		(0.046)	
Organic × sales state	-0.289*		-0.199	
	(0.161)		(0.157)	
Organic × trend	-0.002		-0.003	
	(0.004)		(0.004)	
Cooperative × $\ln S_{i,t-1}$		-0.032		-0.068*
		(0.033)		(0.037)
Large farm × $\ln S_{i,t-1}$		-0.015		-0.084**
		(0.050)		(0.039)
Capital ($\ln K_{i,t-1}$)	0.004	0.005	0.013	0.014
	(0.013)	(0.013)	(0.011)	(0.011)
Observations	20,997	20,997	20,997	20,997
Farms	2,851	2,851	2,851	2,851

Note: Two-way FE-LPM (3) estimated by WLS with RICA survey weights. Sample: wine farms, 2003–2024. Dependent variable: adjustment indicator (1% threshold). Columns 1–2: temporary labor; columns 3–4: salaried labor. Each pair augments the baseline with a different $\mathbf{W}_{it}$. Control: $\ln K_{i,t-1}$. Farm and year FE. Farm-clustered SEs. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

Table 3: Diagnostic tests for the organic null

Two further tests analyse the organic-adopting subsample. First, interacting sales with farm size (total UTA at first panel observation) within the organic-

[6] In the sample, 1,452 farms are classified as individual, 991 as cooperative, and 407 as large-scale corporate.

adopting subsample yields an insignificant interaction, ruling out scale effects as a driver of the weaker response. Second, among the 211 farms that converted to organic during the sample, the sales–adjustment gradient does not change after conversion (the interaction between sales and a post-conversion indicator is -0.002, $p > 0.50$), suggesting that conversion does not itself reduce adjustment—farms that convert were already less responsive beforehand.

These exercises suggest that the weaker organic-adopting response is not driven by year effects, legal form, farm scale, or a causal effect of organic conversion. We cannot determine whether it reflects the production mode itself or selection of inherently less responsive farms into organic conversion. OLS estimates may also be biased by joint determination of sales and labor choices. Before turning to instrumental variables, we first examine outsourcing as an alternative adjustment margin.

4 Outsourcing as an Alternative Adjustment Margin

The preceding sections show that conventional farms adjust on-farm employment more strongly than organic-adopting farms, whose binary adjustment indicator is insignificant though the continuous specification reveals a significant response. Does the weaker on-farm adjustment of bio-adopters reflect higher internal labor costs, or does adjustment partly occur through a margin that our labor measures do not capture? One such margin is outsourcing: farms may purchase contract work from external providers rather than expanding their workforce. Agricultural outsourcing has grown rapidly in France: by 2016, 56% of farms used external providers, a 40% increase over the prior decade (Nguyen et al., 2022). Dupraz and Latruffe (2015) show that hired labor and contract work are substitutes on French farms.

4.1 Empirical strategy

We replace the adjustment indicator with log outsourcing expenditure, excluding farm-years with zero contract expenditure (approximately 18% of observations), and retain the same OLS specification and controls. The dependent variable measures farm i's expenditure on purchased crop work and services: payments to agricultural contracting firm (called *Entreprise de Travaux Agricoles*) and cooperative machinery pools (CUMAs) for vineyard tasks such as spraying, pruning, and mechanical harvesting.[7]

[7] The variable corresponds to compte 6051 of the French chart of agricultural accounts, which records crop-specific contract work. For viticulture farms with no livestock, the crop sub-account

4.2 Results

Table 4 reports the OLS and PPML estimates. Outsourcing expenditure is positively and significantly associated with sales for all three farm categories. The PPML specification, which incorporates the 18% of farm-years with zero contract expenditure, yields larger semi-elasticities throughout. For organic-adopting farms, the outsourcing response is particularly strong: the PPML semi-elasticity (0.588) is roughly twice the full-sample estimate (0.264), suggesting that bio-adopters rely more heavily on external service providers when demand changes. Combined with the weaker on-farm employment elasticity documented in Table 2, this pattern is consistent with organic-adopting farms substituting outsourcing for internal labor adjustment. Section 5 provides causal evidence on these margins for the pooled sample.

	OLS (log)			PPML (levels)		
	All	Quality	Bio-adopt.	All	Quality	Bio-adopt.
Farm sales ($\ln S_{i,t-1}$)	0.178***	0.217***	0.251**	0.259***	0.306***	0.589***
	(0.033)	(0.038)	(0.114)	(0.040)	(0.047)	(0.161)
Capital ($\ln K_{i,t-1}$)	0.073**	0.070**	0.240***	0.056*	0.056*	0.099
	(0.029)	(0.031)	(0.085)	(0.031)	(0.033)	(0.135)
Observations	17,238	14,828	2,311	20,133	17,458	2,738
Farms	2,288	1,990	259	2,494	2,172	273

Note: Two-way FE (farm + year). Columns 1–3 (OLS): dependent variable is ln(Outsourcing); farm-years with zero contract expenditure excluded. Columns 4–6 (PPML): dependent variable is spending in outsourcing in levels (euros); zeros included. PPML coefficients are semi-elasticities. Control: $\ln K_{i,t-1}$. Farm-clustered SEs. Sample: wine farms, 2003–2024. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

Table 4: Outsourcing: OLS and PPML estimates

5 Causal Evidence from Structural Gravity

5.1 Instrument

To address the endogeneity of farm sales, we construct an instrumental variable based on the structural gravity tetrads of Caliendo and Parro (2015). Under the

captures essentially all production-related outsourcing.

gravity model, the product of bilateral trade flows along a triad of countries (n, i, h) cancels out all multilateral resistance terms, leaving only bilateral trade costs:

$$\frac{X_{ni}\, X_{ih}\, X_{hn}}{X_{nh}\, X_{hi}\, X_{in}} = \left(\frac{\kappa_{ni}}{\kappa_{in}} \frac{\kappa_{ih}}{\kappa_{hi}} \frac{\kappa_{hn}}{\kappa_{nh}} \right)^{-\theta}, \tag{4}$$

where X_{ni} denotes bilateral trade flows, κ_{ni} bilateral trade costs, and θ the trade elasticity (Caliendo and Parro, 2015, eq. 21). We adapt this to our setting by assigning the roles $n = r$ (French wine region), $i = d$ (export destination), h (reference country), and freezing all flows involving France at 2007—the last year before the sequence of global trade disruptions (financial crisis, European debt crisis, and subsequent protectionist episodes) that generate the instrument's time variation—while allowing the non-French flows to vary over time. This yields a region-destination-reference-year tetrad:

$$T_{r,d,h,t} = \ln \left(\frac{X^0_{r,d}\, X_{d,h,t}\, X^0_{h,F}}{X^0_{d,F}\, X_{h,d,t}\, X^0_{F,h}} \right), \tag{5}$$

where F denotes France, superscript 0 denotes the base year (2007), and subscript t denotes current-year flows from BACI (HS 220421). France is excluded as an exporter from the BACI sample, so the time-varying components $X_{d,h,t}$ and $X_{h,d,t}$ reflect trade between non-French countries only. The instrument for farm i in region r aggregates over all destinations d and averages over a set $\mathcal{H}$ of reference countries (the 25 largest wine-trading nations excluding France and the destinations themselves):

$$Z_{r(i),t} = \frac{1}{|\mathcal{H}|} \sum_{h \in \mathcal{H}} \sum_{d} T_{r,d,h,t}\,. \tag{6}$$

The instrument enters the regression lagged one period: $Z_{r(i),t-1}$ instruments for $\ln S_{i,t-1}$ in equation (2). Because the tetrad is log-linear, its time-invariant components (all 2007 flows) are absorbed by farm fixed effects. The surviving time variation comes from the non-French bilateral flows $X_{d,h,t}$ and $X_{h,d,t}$, summed over region-specific destination sets $\mathcal{D}_r$. Since regions differ in their 2007 export composition—of 33 destinations in the union, only 5 are common to all fifteen regions—the instrument varies across regions even after absorbing year fixed effects. Appendix F provides a formal proof that the identifying variation is the interaction between region-specific destination baskets and destination-specific bilateral trade shocks, which is functionally a shift-share design (Appendix D illustrates the cross-regional dispersion in sales growth that underpins this variation).

Identification. The exclusion restriction requires that the non-French bilateral trade flows embedded in Z affect farm labor adjustment only through their effect on farm sales, conditional on farm and year fixed effects. Three potential threats merit discussion. First, correlated global supply shocks could simultaneously shift non-French trade and French farm labor decisions; year fixed effects absorb shocks common to all regions, so identification relies on differential regional exposure to the same set of bilateral trade changes. Second, trade diversion—if non-French bilateral flows directly affected French export competitiveness—could violate exclusion; however, France is excluded as an exporter from the BACI sample, so the instrument captures trade between third countries that does not directly compete with French wine. Third, the 2007 regional export composition could correlate with region-specific labor market trends; the share exogeneity test in Section 5.2 (Figure 2) finds no evidence of such differential trends. As a further placebo, Appendix E shows that the instrument does not predict permanent labor adjustment, which is dominated by family workers and should not respond to short-run demand shocks.

5.2 Results

The 2SLS system is:

$$\ln S_{i,t-1} = \alpha_i^{(1)} + \mu_t^{(1)} + \pi Z_{r(i),t-1} + \delta^{(1)} \ln K_{i,t-1} + v_{it}, \tag{7}$$

$$A_{it} = \alpha_i^{(2)} + \mu_t^{(2)} + \beta \widehat{\ln S}_{i,t-1} + \delta^{(2)} \ln K_{i,t-1} + \varepsilon_{it}, \tag{8}$$

where π is the first-stage coefficient and β the causal effect of interest. The outsourcing column replaces A_{it} with $\ln \text{Outsourcing}_{it}$ (log crop contract expenditure). Table 5 reports the estimates for both outcomes.

Inference strategy. The instrument varies at the region-by-year level, while the estimation sample contains farm-level observations. We report both farm-clustered and region-clustered standard errors. As Abadie et al. (2023) emphasise, the appropriate variance lies between these two extremes: the RICA covers all fifteen wine-producing regions ($q_k = 1$ in their notation), so the between-cluster sampling variance vanishes, but the instrument's region-level assignment warrants accounting for within-region correlation. Rather than choosing between the two, we rely on two inference procedures that are valid regardless of the clustering level: the Webb (2023) wild cluster bootstrap (WCR) and the Anderson–Rubin (AR) confidence interval, which is robust to both weak instruments and few clusters.

First stage and reduced form. The first stage (Panel A) is significant under both clustering levels and survives the WCR bootstrap; the first-stage F-statistic is 52.3 under farm clustering and 10.7 under region clustering, the latter exceeding the Staiger and Stock (1997) rule-of-thumb threshold of 10.[8] The reduced form (Panel B) directly tests whether the instrument predicts the outcome. For temporary labor adjustment, the WCR p-value is 0.028—significant at the 5% level. For outsourcing, the WCR p-value is 0.088—significant at the 10% level.

Causal effect. Panel C reports the 2SLS point estimate under farm-level clustering and the AR 95% confidence interval under region clustering. For temporary adjustment, the AR interval is $[0.07,\ 1.24]$. The lower bound is strictly positive, so the interval excludes a zero effect at the 5% level; it also contains the OLS estimate, consistent with a positive causal effect of demand on the probability of adjusting temporary labor. The interval is wide, reflecting the limited number of RICA regions, so while the sign is firmly identified, the magnitude remains imprecise. For outsourcing, the AR interval is $[-7.20,\ 0.08]$, which includes zero—the outsourcing result is therefore suggestive but not statistically significant at conventional levels. The 2SLS point estimate for outsourcing is negative, reversing the OLS sign; given the wide AR interval that includes zero, the outsourcing IV estimate is uninformative about the sign or magnitude of the causal effect and may reflect LATE heterogeneity or imprecision from the limited number of clusters.

[8]The formal Stock and Yogo (2005) critical value for 10% maximal IV size is 16.38 in an exactly identified model, which the region-clustered F does not pass. This borderline instrument strength is precisely why we rely on the AR confidence interval, which is valid regardless of instrument strength. With a single endogenous variable and a single instrument, the effective F-statistic of Olea and Pflueger (2013) coincides with the conventional robust F.

	Temporary adjustment (1)	Outsourcing (2)
Panel A: First stage (dep. var.: $\ln S_{i,t-1}$)		
$Z_{\text{tetrad},t-1}$	-0.0185^{***}	-0.0214^{***}
	(0.0026)	(0.0028)
	$[0.0057]^{***}$	$[0.0059]^{***}$
WCR p-value	0.004	0.004
First-stage F	52.3 / 10.7	59.6 / 13.1
Panel B: Reduced form		
$Z_{\text{tetrad},t-1}$	-0.0062^{***}	0.0324^{***}
	(0.0021)	(0.0060)
	$[0.0021]^{***}$	$[0.0163]^{**}$
WCR p-value	0.028	0.088
Panel C: Second stage (2SLS)		
$\ln S_{i,t-1}$	0.338^{***}	-1.513^{***}
	(0.117)	(0.376)
AR 95% CI	$[0.07,\ 1.24]$	$[-7.19,\ 0.07]$
Panel D: OLS		
$\ln S_{i,t-1}$	0.069^{***}	0.178^{***}
	(0.015)	(0.033)
Observations	20,640	17,238
Farms	2,494	2,288
Farm FE	Yes	Yes
Year FE	Yes	Yes
Controls	$\ln K_{t-1}$	$\ln K_{t-1}$

Note: Panels A–C report IV estimates using the Caliendo and Parro (2015) trade-flow tetrad as instrument, with pre-determined (2007) French regional wine export exposure and contemporaneous non-French bilateral wine trade from BACI. Standard errors in parentheses are clustered at the farm level; square brackets report region-clustered standard errors ($G = 15$). Stars on brackets indicate significance under region clustering. WCR denotes the Webb (2023) six-point wild cluster bootstrap p-value under region-level clustering. First-stage F reported under farm / region clustering. Panel C reports the farm-clustered 2SLS point estimate and the Anderson–Rubin 95% confidence interval under region clustering, which is robust to weak instruments. Sample: wine farms, 2003–2024. Control: $\ln K_{i,t-1}$. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

Table 5: Instrumental variable estimation: trade-flow tetrads

The IV estimation sample is slightly smaller than the OLS sample (20,634 vs.

20,990 for temporary adjustment) because the IV estimator drops singleton fixed-effect groups; the outsourcing column has 17,234 observations because farm-years with zero contract expenditure are excluded from the log specification.[9]

The IV point estimate for temporary adjustment (0.34) is roughly five times the OLS estimate (0.07). Two channels likely account for this gap. First, classical measurement error in lagged deflated sales attenuates the OLS coefficient toward zero; IV corrects this attenuation. Second, the instrument identifies a local average treatment effect for farms in regions with high export exposure, which are plausibly more responsive to demand shocks than the average farm—consistent with a larger LATE than ATE. The AR confidence interval $[0.07,\ 1.24]$ contains the OLS estimate near its lower bound, so the data are consistent with either a modest attenuation-corrected effect or a substantially larger LATE; the current sample cannot distinguish between the two.

Share exogeneity. The instrument has a shift-share structure: cross-sectional variation comes from pre-determined 2007 regional wine export exposure, while time variation comes from non-French bilateral trade flows. Under the Goldsmith-Pinkham et al. (2020) interpretation, identification requires that the 2007 export shares are uncorrelated with region-specific outcome trends. We test this by interacting a standardised measure of total 2007 regional wine exports—the pure cross-sectional component of the instrument—with year dummies, controlling for farm and year fixed effects.[10]

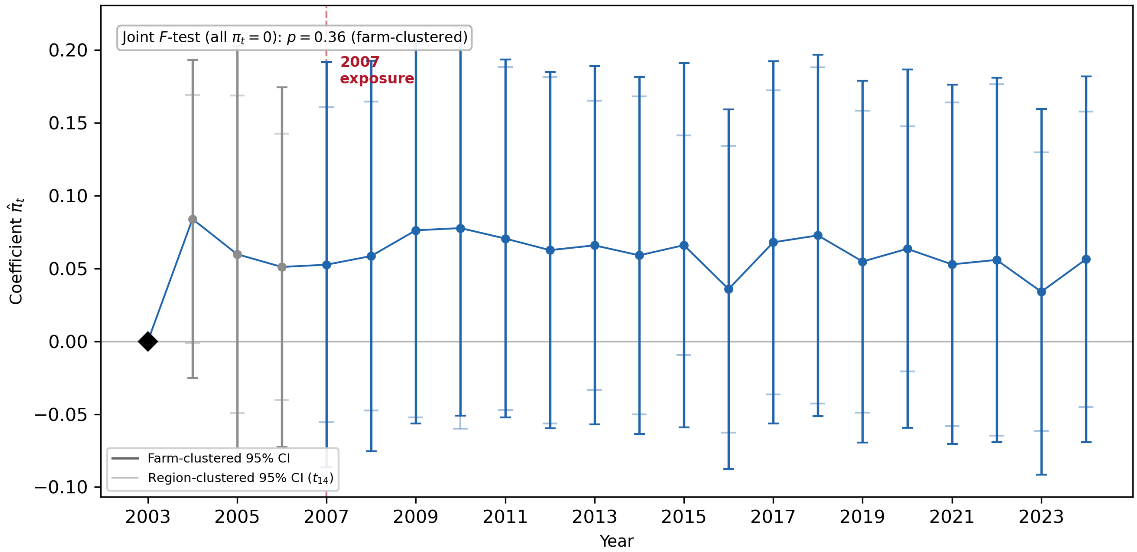
Figure 2 reports the coefficients. Under the null of share exogeneity, all coefficients should be zero: the exposure measure, stripped of time-varying shocks, should have no predictive power for the outcome conditional on farm and year fixed effects. The results are consistent with this null: all 21 coefficients are positive and approximately constant (0.04–0.08). Since the base year is 2003, the persistent positive values indicate that high-export regions have slightly higher adjustment rates throughout the sample—but crucially, this differential is stable over time with no structural break around 2007 and no differential trend, which is what the test requires. The joint F-test under farm-level clustering fails to reject the null that all coefficients are zero ($p = 0.36$).[11]

[9] Panel D reports the OLS benchmark estimated on the IV sample for comparability.

[10] Under the alternative Borusyak et al. (2022) interpretation, identification requires exogeneity of the shocks (non-French bilateral trade flows), which is plausible since France is excluded as exporter. The two perspectives are complementary; we test the more demanding one.

[11] The joint test under region-level clustering is numerically infeasible because the number of interaction parameters (21) exceeds the number of clusters (15), making the cluster-robust variance–covariance matrix near-singular. Following Abadie et al. (2023), the farm-clustered test is informative since both clustering levels bracket the appropriate variance.

The 2SLS coefficient is also robust to the choice of *de minimis* threshold: Appendix E shows that the IV estimate remains positive and stable (0.25–0.34) across thresholds ranging from 0.5% to 15%.



Note: Each point is the coefficient on $\text{Exposure}_r^{2007} \times \mathbf{1}\{\text{year} = t\}$ from a regression of the temporary labor adjustment indicator on the full set of interactions, $\ln K_{i,t-1}$, farm FE, and year FE, with WLS weights. Exposure_r^{2007} is the standardised total 2007 wine export value of region r from French customs data. Base year: 2003. Inner bars: farm-clustered 95% CI; outer bars: region-clustered 95% CI (using t_{14} critical values). Under the null of share exogeneity, all coefficients should be zero.

Figure 2: Share exogeneity test: 2007 regional export exposure $\times$ year dummies

Results are also consistent when using 2004 rather than 2007 as the base year for regional export exposure: the IV point estimate is 0.214 (significant at the 10% level) and the first-stage F-statistic under region clustering is 14.5, providing an informal overidentification check. Adding region-specific linear trends to the specification leaves the reduced form significant ($\hat{\gamma} = -0.0077$, $p < 0.01$) and the IV point estimate positive and significant ($\hat{\beta} = 0.816$, $p < 0.05$); the sign and significance are preserved though the magnitude increases, consistent with limited precision given fifteen clusters. Appendix E further shows that the result holds when using log temporary labor as a continuous dependent variable (Table 10), confirming that the finding is not an artifact of the *de minimis* threshold.

Taken together with the OLS evidence of Sections 3 and 4, the IV results support a causal interpretation: demand shocks raise the probability that farms adjust temporary employment. The AR confidence interval, which is the most robust diagnostic in this setting, excludes zero for temporary adjustment regardless of the clustering assumption.

6 Conclusion

Labor is among the most difficult inputs to reallocate in agriculture. We measure how French wine producers navigate adjustment frictions when demand changes, using farm-level panel data over two decades.

For conventional and quality-wine farms, the OLS evidence is clear: higher sales predict a higher probability of adjusting both temporary and salaried employment. The same farms increase outsourcing expenditure when revenues rise. An instrumental variable based on the structural gravity tetrads of Caliendo and Parro (2015) supports a causal interpretation: the Anderson–Rubin 95% confidence interval under region-level clustering is [0.07, 1.24], excluding zero, and the wild cluster bootstrap independently supports reduced-form significance.

Organic-adopting farms—those that receive the organic production subsidy at any point during the panel—tell a more nuanced story. While the binary adjustment indicator is insignificant in this subsample, the continuous specification reveals a significant sales elasticity of temporary labor roughly three-quarters of the full-sample estimate. Organic-adopting farms do respond to demand, but less strongly. We cannot determine whether this weaker response reflects the organic production mode itself or self-selection of inherently less responsive farms into organic conversion: conversion tracking shows that farms that switched to organic were already less responsive before converting.

Several limitations should be noted. As Abadie et al. (2023) argue, the appropriate inferential uncertainty for our instrument lies between farm-level and region-level clustering, since all fifteen RICA regions are observed. Wild cluster bootstrap and Anderson–Rubin inference—both valid across this range—support significance for temporary employment. The outsourcing IV result is suggestive but borderline under the most conservative inference.

These findings have implications for the organic conversion puzzle. If organic-adopting farms adjust labor less strongly—whether because of production-mode frictions or because less flexible farms self-select into organic—then policies promoting organic conversion should account for the labor adjustment constraints these farms face, rather than targeting only output prices or certification subsidies.

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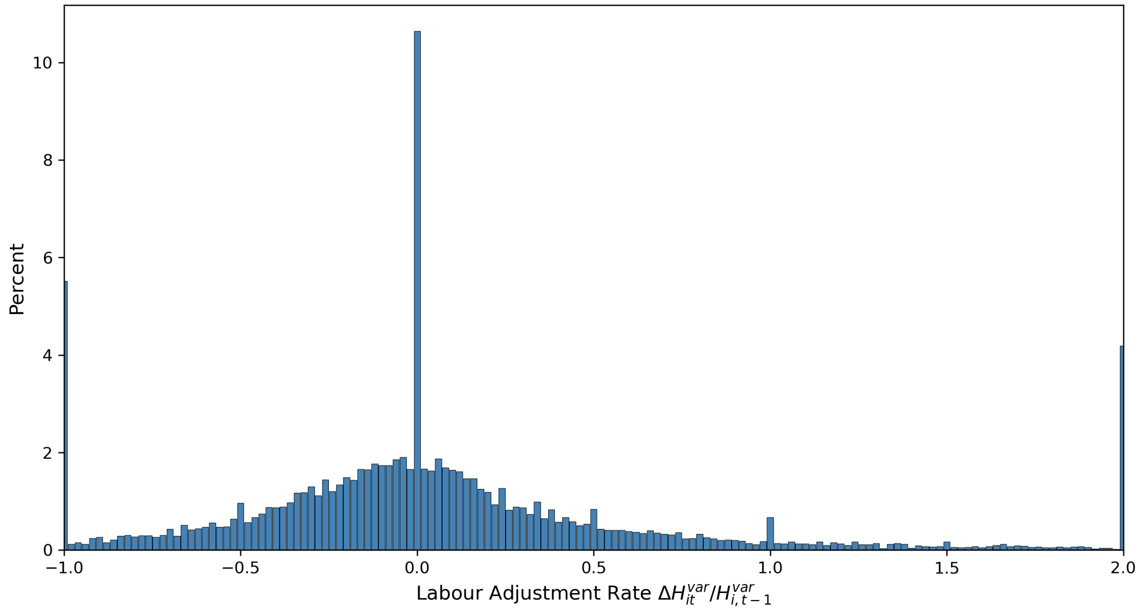
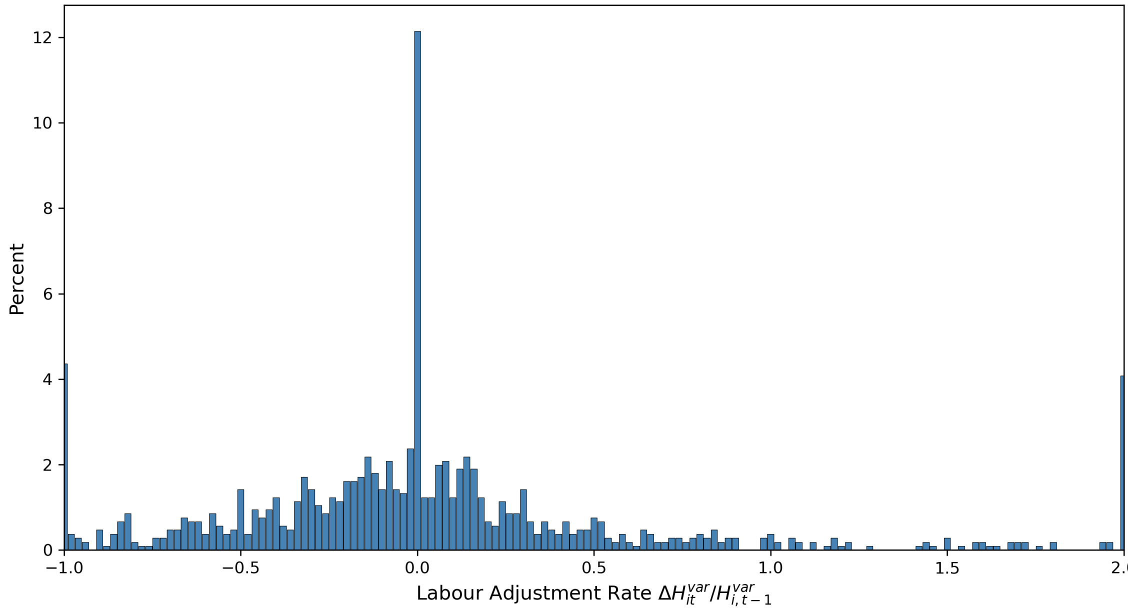
A Labor-change distributions by segment

Figures 3 and 4 replicate the temporary labor adjustment rate distribution following Cooper and Haltiwanger (2006) separately for quality-wine farms and organic-adopting farms. Both exhibit the same concentration near zero and the secondary mass at large adjustments as in the pooled sample (Figure 1).



Note: Same variable and construction as Figure 1, restricted to quality-wine farms (PDO/PGI). RICA/FADN 2003–2024, survey-weighted.

Figure 3: Temporary labor adjustment rate: quality-wine farms



Note: Same variable and construction as Figure 1, restricted to organic-adopting farms (years with SBVBIO > 0). RICA/FADN 2015–2024, survey-weighted.

Figure 4: Temporary labor adjustment rate: organic-adopting farms

B Variable definitions

Labor is measured in annual work units (UTA). Total labor includes all farm workers; permanent labor covers the farm operator, family members, and permanent employees; temporary labor (total minus permanent) captures seasonal workers and short-term contracts. Sales are deflated farm revenue in constant euros. The control variable is lagged log capital (total farm capital from RICA accounts). All monetary variables are deflated using INSEE agricultural price indices; variable names and construction details are documented in the replication code.

Quality-wine farms are those classified under the PDO/PGI wine orientation. Organic-adopting farms ("bio-adopters") are classified as such if they receive the organic production subsidy at any point during the sample period. This time-invariant classification tracks these farms over the full panel (2003–2024), including years before they adopted organic production. The regional distribution of bio-adopters in the RICA closely tracks the independently measured organic vineyard share from CASD administrative data (cross-sectional correlation across 15 regions: $r = 0.95$), suggesting that the classification identifies a population already engaged in organic production for much of the panel.

Descriptive statistics

	All farms		Quality		Bio-adopt.	
	Mean	SD	Mean	SD	Mean	SD
Total labour (UTA)	3	2	3	3	4	4
Permanent labour (UTA)	2	2	2	2	3	3
Temporary labour (UTA)	1	1	1	1	1	1
Sales (€, deflated)	237,691	282,985	246,500	291,484	293,680	377,090
Chemical inputs (€, deflated)	13,304	16,464	12,091	14,782	14,305	20,922
Capital (€, deflated)	261,023	370,804	267,716	389,268	296,159	406,218
General subcontracting (compte 611)	3,962	11,849	3,410	10,228	3,895	11,535
Crop contract work (compte 6051)	10,221	19,243	10,022	18,916	14,070	27,090
Adjustment indicator (total)	0.698	0.459	0.707	0.455	0.750	0.433
Adjustment indicator (permanent)	0.324	0.468	0.329	0.470	0.354	0.478
Adjustment indicator (temporary)	0.665	0.472	0.678	0.467	0.723	0.448
Adjustment indicator (salaried)	0.579	0.494	0.593	0.491	0.659	0.474
Observations	26,796		22,721		3,249	
Farms	3,872		3,282		353	

Note: Sample: wine farms, RICA/FADN 2003–2024. Survey-weighted means and standard deviations using RICA extrapolation weights. All monetary variables in constant euros, deflated using INSEE agricultural price indices. Adjustment indicators equal one when absolute relative change exceeds 1%.

Table 6: Descriptive statistics (survey-weighted)

C Total and permanent labor results

	Total			Permanent		
	All	Quality	Bio-adopt.	All	Quality	Bio-adopt.
Farm sales ($\ln S_{i,t-1}$)	0.058***	0.069***	0.008	0.045***	0.046**	-0.006
	(0.016)	(0.019)	(0.040)	(0.015)	(0.018)	(0.055)
Capital ($\ln K_{i,t-1}$)	-0.001	-0.008	0.015	0.015	0.008	-0.001
	(0.013)	(0.014)	(0.030)	(0.011)	(0.011)	(0.035)
R-squared (within)	0.002	0.003	0.000	0.002	0.002	0.000
Observations	20,997	18,211	2,825	20,997	18,211	2,825
Farms	2,851	2,481	307	2,851	2,481	307

Note: Same specification as Table 1. Columns 1–3: total labor; columns 4–6: permanent labor. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: OLS FE-LPM: total and permanent labor

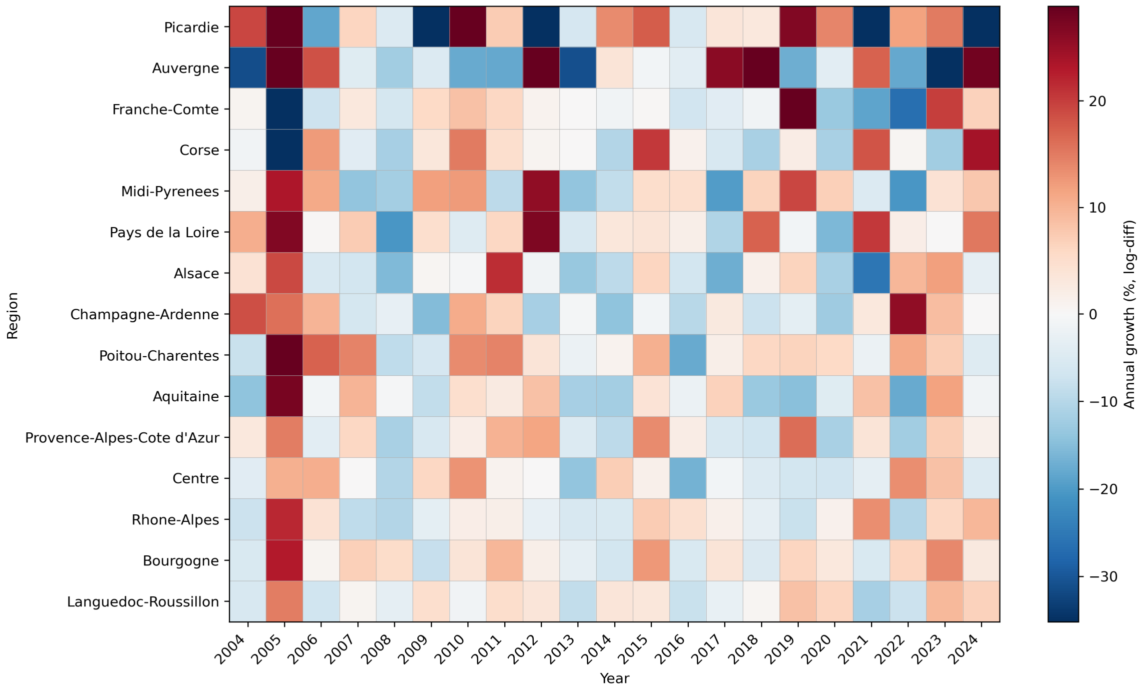
	Organic exposure			Legal form		
	Total	Temp.	Perm.	Total	Temp.	Perm.
Farm sales ($\ln S_{i,t-1}$)	0.064***	0.073***	0.051***	0.062***	0.081***	0.054***
	(0.017)	(0.017)	(0.015)	(0.022)	(0.022)	(0.017)
Organic × $\ln S_{i,t-1}$	-0.055	-0.035	-0.063			
	(0.043)	(0.047)	(0.055)			
Organic × sales state	-0.281**	-0.289*	0.060			
	(0.137)	(0.161)	(0.132)			
Organic × trend	-0.000	-0.002	0.004			
	(0.004)	(0.004)	(0.004)			
Cooperative × $\ln S_{i,t-1}$				-0.019	-0.032	-0.003
				(0.033)	(0.033)	(0.027)
Large farm × $\ln S_{i,t-1}$				0.015	-0.015	-0.049
				(0.052)	(0.050)	(0.056)
Capital ($\ln K_{i,t-1}$)	-0.001	0.004	0.015	-0.000	0.005	0.015
	(0.013)	(0.013)	(0.010)	(0.013)	(0.013)	(0.010)
Observations	20,997	20,997	20,997	20,997	20,997	20,997
Farms	2,851	2,851	2,851	2,851	2,851	2,851

Note: Same specifications as Table 3. Farm-clustered SEs. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Falsification tests: total and permanent labor

D Regional sales variation

Figure 5 displays annual real sales growth by region and year. The heatmap reveals broad comovement across regions in certain years (e.g., negative in 2008, 2013, 2017, 2020; positive in 2005, 2015), reflecting aggregate demand shocks, alongside substantial cross-regional dispersion within the same year, which reveals heterogeneous exposure to export markets.

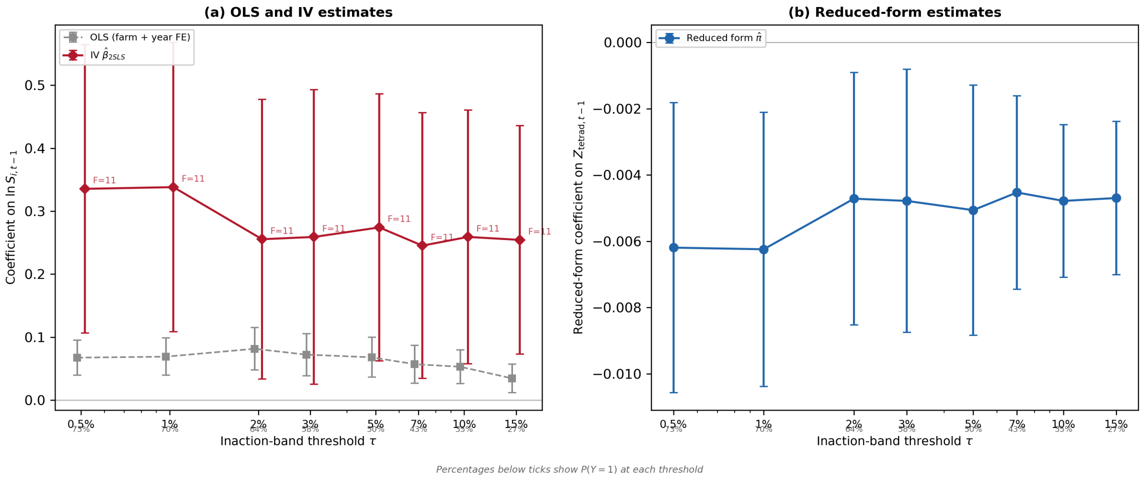


Note: Annual real sales growth (log-difference) by RICA region. Source: RICA/FADN 2004–2024, survey-weighted.

Figure 5: Wine sales growth at the regional level

E Threshold robustness

Figure 6 and Table 9 show that the main results are robust to the choice of *de minimis* threshold. For each threshold $\tau \in \{0.5\%, 1\%, 2\%, 3\%, 5\%, 7\%, 10\%, 15\%\}$, we re-estimate the OLS, 2SLS, and reduced-form specifications using the same sample, controls, and fixed effects as in Table 5. The adjustment indicator is reconstructed at each threshold following the dual criterion described in Section 3. The OLS coefficient is positive and significant at all thresholds. The IV coefficient remains positive and stable (0.25–0.34), and the reduced-form coefficient is negative throughout. The first-stage F-statistic under region clustering is 10.7 at every threshold, above the Staiger and Stock (1997) rule-of-thumb threshold of 10.



Note: Panel (a): OLS (farm-clustered SE, farm + year FE) and 2SLS (farm-clustered SE) point estimates with 95% CI. Panel (b): reduced-form coefficient on $Z_{\text{tetrad},t-1}$ (region-clustered SE) with 95% CI. F-statistics computed under region clustering ($G = 15$). Percentages below ticks show $P(Y = 1)$ at each threshold. $N = 20{,}634$ at all thresholds.

Figure 6: Coefficient stability across *de minimis* thresholds

τ	$P(Y=1)$	$\hat{\beta}_{\text{OLS}}$	$\hat{\gamma}_{\text{RF}}$	$\hat{\beta}_{\text{IV}}$
0.5		(0.014)	(0.0022)	(0.117)
1		(0.015)	(0.0021)	(0.117)
2		(0.017)	(0.0019)	(0.113)
3		(0.017)	(0.0020)	(0.119)
5		(0.016)	(0.0019)	(0.108)
7		(0.015)	(0.0015)	(0.108)
10		(0.014)	(0.0012)	(0.103)
15		(0.012)	(0.0012)	(0.092)

Note: Each row reports estimates at a different *de minimis* threshold τ for the temporary labor adjustment indicator. OLS and IV: farm + year FE, farm-clustered SEs. Reduced form: region-clustered SEs ($G = 15$). Control: $\ln K_{i,t-1}$. First-stage F-statistic (region-clustered) is 10.7 at all thresholds. $N = 20{,}640$ throughout. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Threshold robustness: OLS, reduced form, and IV estimates

Continuous dependent variable. Table 10 replaces the binary adjustment indicator with log temporary labor (excluding farm-years with zero temporary labor). The OLS elasticity is 0.292 for all farms and 0.306 for quality-wine farms, both significant at the 1% level. The IV estimate is 1.732, with an AR 95% confidence interval of $[0.82, \ 4.98]$ that excludes zero. The LPM remains our primary specification because the 1% threshold filters measurement noise—for a farm employing two annual work units, a change below 1% corresponds to roughly 32 hours, below the precision of the UTA accounting measure—but the continuous specification confirms that the direction and significance are robust to functional form.

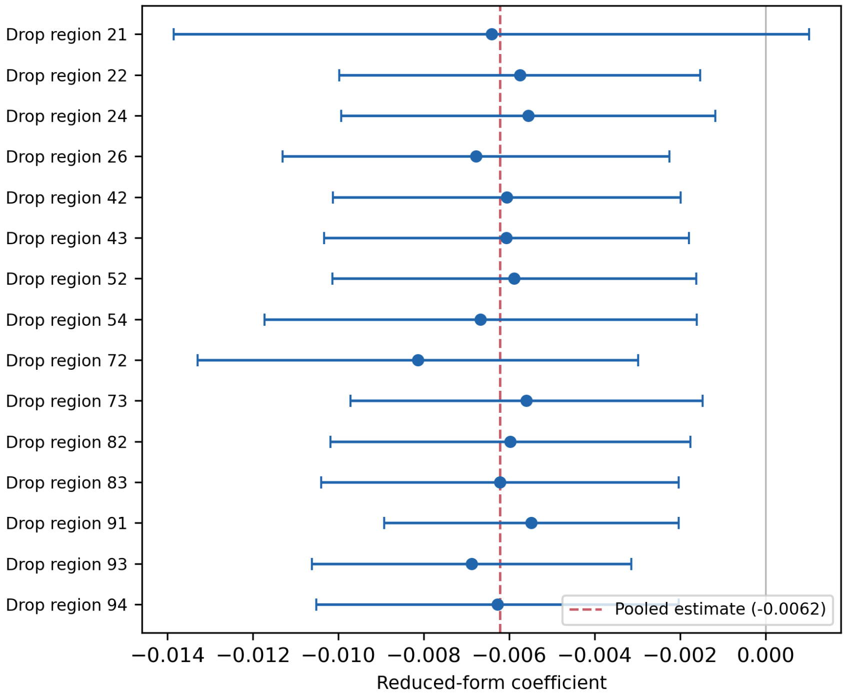
	OLS (All)	OLS (Quality)	IV (All)
$\ln S_{i,t-1}$	0.292***	0.306***	1.731***
	(0.040)	(0.044)	(0.435)
AR 95% CI			[0.83, 4.96]
Observations	15,922	14,038	15,922
Farm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: Dependent variable: $\ln H_{it}^{var}$ (log temporary labor in UTA). Farm-years with zero temporary labor excluded. Farm and year FE. Control: $\ln K_{i,t-1}$. OLS: farm-clustered SEs. IV: farm-clustered SE for point estimate; AR CI under region clustering ($G = 15$). Sample: wine farms, 2003–2024. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

Table 10: Continuous dependent variable: log temporary labor

Placebo: permanent labor. As a further falsification test, we estimate the IV reduced form using permanent labor adjustment as the outcome. Permanent labor is dominated by family workers, who are structurally tied to the farm and should not respond to short-run demand shocks. The reduced-form coefficient is -0.0022 (SE $= 0.0016$, $p = 0.19$ under region clustering), confirming that the instrument does not predict permanent labor adjustment. By contrast, the temporary labor reduced form estimated on the same sample is -0.0063 ($p = 0.01$), three times larger and significant—consistent with the instrument operating exclusively through the flexible labor margin.

Leave-one-out-region stability. Figure 7 shows the reduced-form coefficient estimated after dropping each of the fifteen regions in turn. The coefficient is negative in all fifteen specifications, ranging from -0.0055 to -0.0082, tightly clustered around the pooled estimate (-0.0062). The sign is robust to dropping any single region, though precision decreases when Region 21 (Champagne) is excluded—its large sample and strong export exposure contribute disproportionately to the first-stage variation.



Note: Each point is the reduced-form coefficient on $Z_{\text{tetrad},t-1}$ estimated after dropping one region. Specification: farm FE + year FE, $\ln K_{i,t-1}$ control, region-clustered SE ($G = 14$), WLS with survey weights. Dashed line: pooled estimate. Horizontal bars: 95% CI.

Figure 7: Leave-one-out-region: reduced-form coefficient stability

Sensitivity to the control set. Table 11 reports the OLS coefficient on lagged log sales under five specifications with progressively richer control sets, all estimated on a common sample. Column (1) includes only farm and year fixed effects; column (2) adds lagged log capital (the baseline); column (3) adds a deflated pesticide-fertilizer index; column (4) adds twice-lagged total labor, capturing state dependence in the adjustment process; column (5) includes all three controls. The sales coefficient ranges from 0.058 to 0.067, remaining highly significant throughout. This stability is consistent with limited omitted variable bias in the baseline specification (Oster, 2019). We do not include these additional controls in the IV specification because they may respond to the same demand shock the instrument captures, making them potential mediators rather than confounders (**AngristPischke2009**).

	(1) FE only	(2) + Capital	(3) + Chemicals	(4) + Lagged labor	(5) All three
$\ln S_{i,t-1}$	0.067***	0.066***	0.059***	0.060***	0.054***
	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)
Observations	18,534	18,534	18,534	18,534	18,534
Farm + year FE	Yes	Yes	Yes	Yes	Yes

Note: Dependent variable: adjustment indicator (temporary labor, 1% threshold). All columns estimated on the common sample where all controls are non-missing. Farm + year FE. Farm-clustered SEs. WLS with survey weights. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 11: OLS sensitivity to control set

F Identification of the tetrad instrument under two-way fixed effects

The tetrad instrument for region r in year t is

$$Z_{r,t} = \frac{1}{|\mathcal{H}|} \sum_{h \in \mathcal{H}} \sum_{d \in \mathcal{D}_r} T_{r,d,h,t} \,,$$

where $\mathcal{D}_r$ denotes the set of destinations to which region r exported wine in 2007 and

$$T_{r,d,h,t} = \ln X^0_{r,d} + \ln X_{d,h,t} + \ln X^0_{h,F} - \ln X^0_{d,F} - \ln X_{h,d,t} - \ln X^0_{F,h} \,.$$

Grouping terms:

$$Z_{r,t} = \underbrace{\frac{1}{|\mathcal{H}|} \sum_{h} \sum_{d \in \mathcal{D}_r} \left[\ln X^0_{r,d} + \ln X^0_{h,F} - \ln X^0_{d,F} - \ln X^0_{F,h}\right]}_{C_r \text{ (time-invariant, absorbed by farm FE)}} + \underbrace{\frac{1}{|\mathcal{H}|} \sum_{h} \sum_{d \in \mathcal{D}_r} \left[\ln X_{d,h,t} - \ln X_{h,d,t}\right]}_{V_{r,t} \text{ (time-varying)}}$$

After absorbing farm fixed effects, only $V_{r,t}$ remains. Define the bilateral asymmetry shock $g_{dh,t} \equiv \ln X_{d,h,t} - \ln X_{h,d,t}$. Then

$$V_{r,t} = \frac{1}{|\mathcal{H}|} \sum_{h} \sum_{d \in \mathcal{D}_r} g_{dh,t} \,.$$

If $\mathcal{D}_r = \mathcal{D}$ for all r (all regions export to the same set of destinations), then $V_{r,t}$ does not vary across r and is absorbed by year fixed effects. *This is the additive separability concern.*

In our data, however, $\mathcal{D}_r$ differs substantially across regions. Of the 33 destinations in the union, only 5 are common to all fifteen regions (Switzerland, Germany, Spain, Japan, United States). The pairwise symmetric difference $|\mathcal{D}_r \triangle \mathcal{D}_{r'}|$ ranges from 0 to 24, with a mean of 7.2.

Because the destination sets differ, the within-transformed instrument

$$\tilde{V}_{r,t} \equiv V_{r,t} - \bar{V}_{\cdot,t} = \frac{1}{|\mathcal{H}|} \sum_{h} \sum_{d \in \mathcal{D}_r} g_{dh,t} \; - \; \frac{1}{R} \sum_{r'} \frac{1}{|\mathcal{H}|} \sum_{h} \sum_{d \in \mathcal{D}_{r'}} g_{dh,t}$$

is nonzero whenever the bilateral asymmetry shocks $g_{dh,t}$ differ across destinations that belong to some regions' baskets but not others. The identifying variation is the interaction between *which* destinations enter each region's sum (the exposure margin, determined in 2007) and *how* those destinations' bilateral trade evolves over time (the shock margin). This is functionally a shift-share design in which the implicit "shares" are the 2007 export composition and the "shocks" are the destination-specific bilateral trade asymmetries, averaged over reference countries.