Journal of
Theoretical and Applied Electronic Commerce Research

MDPI

Article

Consumer Online Shopping Adoption and Small and Medium-Sized Enterprises' E-Sales Participation in the European Union: A Longitudinal Alignment Analysis, 2015–2024

Ovidiu-Aurel Ghiuță

The Entrepreneurship Center, Business Incubation Laboratory, Faculty of Food Engineering, Ștefan cel Mare University of Suceava, 720229 Suceava, Romania; ovidiughiuta@fia.usv.ro

Abstract

Buying online has become ordinary behaviour for European consumers, yet selling online remains a minority practice among small and medium-sized enterprises (SMEs). This study asks whether the two have developed in step within countries. Using Eurostat household and enterprise ICT survey data for the EU-27 from 2015 to 2024 (268 complete country-years), we measure their alignment as a within-year standardised difference. The measure captures relative cross-country positioning between two adoption indicators; it does not quantify unmet demand or transaction volumes. Cross-country dispersion in consumer adoption fell steeply and significantly (SD trend −0.91 per year, $R^2 = 0.96$; country-clustered bootstrap 95% CI [−1.25, −0.51]), while dispersion in SME participation drifted upward only slightly and not significantly, and the annual correlation between the two edged down from 0.539 to 0.415 without reaching significance under the same bootstrap. SME-leading and consumer-leading observations retained their category in roughly 90% of year-to-year transitions (Cohen's $\kappa = 0.74$), whereas the aligned category showed lower retention. Consistent with this, the within-country association between the two series is small and statistically non-significant in both the two-way fixed-effects specification ($\beta = 0.116$, $p = 0.112$, $N = 268$) and the first-difference specification ($\beta = 0.091$, $p = 0.055$, $N = 239$), and it remains weak across nine robustness specifications and under wild cluster bootstrap and equivalence testing. It is, however, stronger when member states are weighted by economic size; this changes the estimand and does not establish heterogeneity by economy size. Consumer adoption is therefore an incomplete indicator of national e-commerce development, and the two dimensions warrant separate monitoring.

Keywords: e-commerce development; SME digitalisation; online shopping adoption; consumer–SME alignment; EU-27; panel data; two-way fixed effects; Eurostat; digital policy

Academic Editor: Ting Chi

Received: 8 August 2026
Revised: 7 September 2026
Accepted: 8 September 2026
Published: 10 September 2026

1. Introduction

In Denmark, 37.9% of small and medium-sized enterprises (SMEs) with 10 to 249 persons employed reported electronic sales worth at least 1% of turnover in 2023, which is the highest share in the Union, based on the Eurostat data used throughout this study. Danish consumers, by contrast, are among the most active online shoppers in the Union. Denmark is not unusual in this respect, and the pattern is not confined to the Nordic states: across the EU-27, the share of adults who buy online has risen faster and further than the share of firms that sell online. These two indicators of the same national e-commerce ecosystem have not moved together.

This divergence is easy to observe and easy to leave unexamined. Research on national e-commerce development has tended to pick a side. Consumer-facing work treats online purchasing as the outcome of interest and explains it with income, education, skills, and trust. Enterprise-facing work treats firm adoption as the outcome and explains it with technological, organisational and environmental conditions. Both traditions are productive, and both leave the relationship between the two sides unmeasured. A country where nine adults in ten shop online and one firm in eight sells online is described by the first literature as digitally advanced and by the second as digitally lagging. Neither description is wrong. Neither is complete.

The distance between the two curves is not merely descriptive. A high consumer adoption rate does not reveal whether SMEs participate in electronic selling to a comparable relative extent. Where a composite measure combines the two sides, a country whose consumers have moved far ahead of its firms will score higher than its enterprise side alone would indicate, and whether progress on one side is accompanied by progress on the other within countries has not been tested directly. The present indicators cannot identify which sellers serve consumer demand; they can only show how consumer adoption and SME e-sales participation are positioned relative to one another within the EU distribution.

1.1. What Existing Work Establishes, and What It Leaves Open

Country-level e-commerce research in Europe has converged on heterogeneity as its central finding. The European digital divide is not one-dimensional: an individual-level dimension of access and use is separable from an organisational dimension covering firms and governments [1]—conceptually the same consumer–firm distinction pursued here, though established by clustering countries in cross-section rather than by tracking the distance between the dimensions over time. That heterogeneity is spatially structured and durable, appearing as persistent regional diversity in European e-retailing [2] and as a North–South gradient in enterprise digital readiness that has proved to be lasting [3]. A parallel panel of literature has moved from describing this variation to accounting for it, adding determinants across different observation windows and estimators, including cointegration approaches for the EU-27 [4–6]. What this body of work establishes is that countries differ and differ systematically on each side taken separately.

Two studies come closer, and the way they treat the two sides is instructive. Where household online purchasing and business online sales are brought into a single European analysis (with pandemic-year controls in one case and in a fixed- and random-effects panel built on business turnover from digital sales in the other), the two series function as alternative indicators of a single latent construct called e-commerce development [7,8]. Consumer adoption and SME participation are measured jointly, then summed or substituted rather than differenced. That choice is coherent on its own terms, but it forecloses a question rather than answering it: once the two sides are collapsed into one scale, the distance between them, its sign, whether it persists, and whether it narrows cannot be the object of study.

The closest theoretical precedent is older. Ho et al. [9] built a growth-theory model of European B2C e-commerce in which an Internet-based selling technology adoption function sits alongside demand-side factors, making the co-evolution of the two sides explicit at the level of mechanism. Country-level adoption theory has since been extended with institutional and cultural factors, carrying forward the demand–pull, supply–push framing of earlier Southern European work [10]. The mechanism is therefore available in the literature; what none of this work provides is a longitudinal measure of the asymmetry itself.

1.2. What This Study Does Differently

We measure the distance rather than the levels. For each country-year, household online purchasing adoption and SME e-sales participation are standardised across the EU-27 within the year, and the difference between the two standardised scores becomes the unit of analysis. A positive value indicates that consumer adoption is relatively further advanced than SME e-sales participation; a negative value the reverse; and values near zero indicate relative alignment. The panel permits within-country co-development to be estimated directly from the two raw indicators, while the alignment gap supports the analysis of relative country configurations, their persistence, and cross-country co-alignment. This allows for three questions that levels cannot answer: whether the two sides move together within countries, whether particular configurations persist, and whether the countries have become more alike in how their two sides relate.

This design is deliberately distinct from cross-sectional country screening. Bresfelean, Comes, and Pop-Nistor [11] recently used Eurostat and World Bank indicators to build a 2023 portfolio for comparing EU-27 cross-border e-commerce markets, reducing enterprise e-sales penetration, digital financial participation, and logistics capability to a single execution condition axis and positioning countries for market-entry evaluation. That study answers where a firm might consider expanding. The present one asks whether two domestic development processes are synchronised, uses a panel rather than a single vintage, keeps the two sides separate rather than combining them, and speaks to domestic commercialisation policy rather than foreign market selection. One indicator, enterprise e-sales penetration, overlaps; it is the only harmonised European measure of firm-level e-sales participation, and its analytical role differs: a longitudinal outcome here, a component of a composite screening axis there.

A closer methodological relative appeared in this journal more recently. Păun et al. [12] estimated two-way fixed-effects models of e-commerce adoption for eleven Central and Eastern European member states over 2014–2023, using harmonised Eurostat and international data. Their dependent variable was adoption itself, and their explanatory interest was payment infrastructure. We share the estimator, the data provider, and much of the window, and we differ in the construct: their outcome was a level, ours is a relation between two levels.

The closest study of all is that by Cetulean et al. [13], who cover almost identical ground, that is, an EU-27 panel over 2015–2023, a two-way fixed-effects design on harmonised Eurostat indicators, and a headline result that will recur below. The within-country effect of digital capability on e-commerce turnover is, in their words, small and statistically fragile, while turnover is strongly persistent within countries. Where they compress many indicators into a single digital maturity index and relate it to one supply-side outcome, we keep the two indicators apart and make the distance between them the object of study. This difference is not cosmetic. A maturity index cannot show that households converged while firms did not, because it dissolves the two sides into one scale; it cannot yield a consumer–SME persistence typology because it does not separate the two sides; and it cannot track whether the two sides are drifting into looser correspondence because it never pairs them. The three findings this paper turns on—the divergence in dispersion, the typology of persistent configurations, and the decade-long decline in the cross-country correlation between the sides—are precisely what a single-index design leaves unseen.

1.3. Research Questions and Contributions

One clarification frames everything that follows. The asymmetry studied here is a measure of relative cross-country positioning between two adoption indicators; it does not quantify unmet demand, the matching of domestic buyers to domestic sellers, or the

volume of e-commerce transactions. A country is labelled consumer-leading when its consumer adoption stands higher against the EU distribution than its SME participation does, and nothing stronger is claimed.

RQ1 asks how household online purchasing adoption and SME e-sales participation developed across the EU-27 over the study period; this query is answered on raw levels. RQ2 asks whether within-country changes in the two series are associated and whether any association survives first-differencing and country-specific trends. RQ3 asks which member states display persistent consumer-leading, relatively aligned, or SME-leading configurations and how stable those positions are. RQ4 asks, separately, whether cross-country dispersion in each series converged or diverged over the period and whether the annual correlation between them strengthened or weakened.

Conceptually, the study defines the consumer–SME alignment gap as a country-level object with its own dynamics, distinct from either side's level and from any composite of the two. The empirical contribution is longitudinal EU-27 evidence on whether the two sides co-develop within countries, with a persistence typology attached. A methodological point emerges alongside: the dispersion of a within-year standardised difference is an exact function of the annual cross-country correlation between its components, which constrains the convergence claims such measures can support. That applies to any relative index built this way, not only to the one used here.

2. Conceptual Background

2.1. Complementary Innovations Need Not Diffuse Together

At the transactional level, online purchasing and online selling are complementary processes: the value of adopting on one side rises with adoption on the other. The national indicators used here are broader aggregates and do not form a directly matched buyer–seller pair, as Section 2.4 sets out. The complementarity structure has a specific literature, and it does not predict synchrony.

Where two innovations are useless in isolation, their diffusion is coupled but not symmetric. Evidence on supermarket scanners and UPC symbols shows two-way co-diffusion effects that are stronger than each innovation's own diffusion effect, that run unequally in the two directions, and that differ across market segments [14]; the same logic of linked adoption of complementary technologies has been formalised more generally [15]. The asymmetry is the point. Adoption on one side accelerates adoption on the other, but not equally in both directions and not at the same rate everywhere. Rogers' account of diffusion [16] supplies the underlying S-curve logic, and the network-externality tradition explains why an installed base on one side changes the returns to adoption on the other.

Applied to national e-commerce, this yields the study's central expectation and its central caution. Consumer adoption and SME e-sales participation may be positively associated within countries, because developments on either side can raise the returns to participation on the other. The strength, timing, and direction of adjustment are not theoretically determinate at country level. However, the association need not be tight, the adjustment need not be symmetric, and countries may settle into configurations where one side runs persistently ahead.

The European case sharpens that expectation. Consumer adoption faces low switching costs, is served by a small number of large platforms with continent-wide reach, and can therefore rise without any change in domestic SME selling: a consumer in Bucharest can shop as easily from Berlin as from down the street. SME adoption requires internal capability, fixed investment, and process change, and it is bounded by firm size and sector composition. Asymmetric adjustment is thus the expected pattern rather than an anomaly.

2.2. Why Asymmetry Might Persist Rather than Close

Co-diffusion explains why the two sides should be related. It does not explain why a gap would endure for a decade. To explain this phenomenon, we draw on institutional complementarities.

The comparative capitalism tradition [17] argues that institutional arrangements in different spheres reinforce one another, so that configurations are stable and reform in one sphere has muted effects unless matched elsewhere. Empirical testing across political economies supports the stronger implication: interaction effects across institutional subspheres must be taken into account if the economic impact of reform in any single sphere is to be assessed accurately [18]. Transposed to e-commerce, the implication is that consumer adoption and SME e-sales participation depend on partly different institutional supports (consumer payment infrastructure, delivery networks, and digital skills on one side and access to finance, managerial capability, firm size structure, and sectoral composition on the other). Where those supports have developed unevenly, the asymmetry may persist rather than close quickly.

This is a lens rather than an established result. The complementarities literature has been applied extensively to labour markets, corporate governance, and innovation systems and only sparingly to digital commerce; recent work in this journal has begun to use complementarity language for country-level digitalisation and performance [19]. In its current statement for innovation policy, the framework holds that outcomes depend on matching policy to a country's dominant mode of coordination and that several viable institutional paths coexist rather than there being one best practice [20]—exactly the reasoning that would let consumer- and firm-side digital development settle into different, nationally path-dependent configurations. This reasoning motivates the persistence analysis in RQ3, and we treat any finding of durable configurations as consistent with, not confirmation of, the institutional account.

2.3. What Country Aggregates Can and Cannot Carry

Much of the SME e-commerce literature is firm-level, built on technology–organisation–environment reasoning, technology acceptance models, and their extensions. Those frameworks explain adoption decisions inside firms. They do not license inferences about countries, and country-level associations need not hold within firms, which is the ecological fallacy Robinson identified for correlations between aggregates and individual behaviour [21].

We take two precautions. TOE reasoning is used only as descriptive vocabulary for why SME e-sales participation may lag, never as a country-level causal model, and no result is generalised to individual consumers or individual firms. The cost of that restraint is real: the mechanisms behind any asymmetry we measure remain outside the design, recoverable only with linked firm and consumer data. What it buys is claims proportionate to aggregate observational evidence.

2.4. What the Two Indicators Jointly Represent

One qualification on scope is needed before the empirical design. The two indicators are not the demand and supply sides of a single national transaction market; they are distinct adoption measures drawn from different populations, and we treat their standardised difference as a descriptive measure of relative cross-country positioning rather than as a market imbalance. Consumers may buy from large domestic retailers or from sellers abroad; the enterprise series covers business-to-business as well as business-to-consumer sales through web and EDI channels and excludes both microenterprises and large firms. What the two series jointly describe is the relative standing of a country's consumer adoption

and its SME selling participation within the EU distribution—two dimensions of a national e-commerce ecosystem, not a matched buyer–seller pair. Every quantity below is to be read in that light.

2.5. From the Literature Gap to the Research Questions

The literature establishes that European e-commerce development is heterogeneous [1–3], that both consumer- and firm-side indicators respond to institutional and socio-economic conditions [5,10], and that the two sides can be measured jointly from official statistics [7,8]. It has not established whether they develop in step within countries. The questions stated in Section 1.3 follow from that gap. Given an observational design, diffusion-descriptive theory, and no theoretically determinate prediction about the sign of asymmetric adjustment, we pose research questions rather than hypotheses. Formal hypotheses would imply a directional theory that the literature does not supply.

3. Materials and Methods

3.1. Design

The study uses an unbalanced country-year panel of the 27 EU member states over economic reference years 2015–2024. The United Kingdom is excluded throughout, and the present EU-27 composition is applied to the whole window so that country membership does not change with the period. EU aggregates are never treated as countries. The design is observational, and no causal interpretation is offered for any estimate.

3.2. Indicators and Sources

All variables come from Eurostat. Two series carry the analysis.

Household online purchasing adoption is the share of individuals aged 16–74 who ordered or purchased goods or services online in the previous 12 months. Eurostat maintains this measure across two datasets: the series ending in 2019 and Internet purchases by individuals (2020 onwards) which begins in 2020. Section 3.4 documents how the two are handled. The reference metadata for ICT usage in households and by individuals [22] confirms annual collection since 2002 for individuals aged 16–74.

SME e-sales participation is the share of enterprises with 10–249 persons employed whose electronic sales amount to at least 1% of total turnover. The enterprise ICT survey excludes enterprises with fewer than ten persons employed, so the measure covers small and medium-sized enterprises in the Eurostat sense and not microenterprises; we describe it accordingly throughout, never as "all SMEs". Sectoral coverage follows NACE Rev. 2 sections C–N excluding K; activities outside this scope therefore include agriculture, forestry and fishing, mining and quarrying, financial and insurance activities, public administration, education, health, and other services.

One secondary household indicator supports the robustness analysis: the share of individuals purchasing online in the previous three months. This is used as an alternative definition of consumer adoption.

The primary specification includes no additional time-varying covariates. The objective is to estimate a descriptive within-country association rather than a causal parameter, while country and year fixed effects absorb time-invariant national differences and common annual shocks. Candidate covariates were considered and set aside for the reasons given below. Digital skills indicators are excluded because their methodology changed around 2021 and the earlier and later series are not comparable; the Digital Intensity Index is excluded because its composition varies across survey years.

3.3. Temporal Alignment

Enterprise and household observations do not refer to the same period as published. The reference metadata for ICT usage in enterprises states that total turnover used in the survey refers to the previous calendar year, as do the other questions on e-commerce sales, and that data refer either to the survey period or, where the model questionnaire specifies, to the calendar year prior to it [23]. Enterprise e-commerce variables published under survey year t therefore describe sales in year $t - 1$.

We align all enterprise series to their economic reference year by subtracting one from the published survey year and report household observations at their survey year. The convention is applied uniformly and is recorded for every observation in the panel, so the effect of the alignment can be inspected directly in the data.

3.4. Series Breaks and Panel Construction

Both surveys were affected by regulatory change during the window, and neither break can be assumed innocuous.

On the enterprise side, Regulation (EU) 2019/2152 on European business statistics [24] and its implementing regulation [25] applied from reference year 2021, changing sectoral scope and the set of collected variables. The technical specifications for the ICT usage and e-commerce topic are renewed annually by implementing regulation, so module content varies year to year, and the presence of any given question must be verified rather than assumed. On the household side, Regulation (EU) 2019/1700 [26] established the framework for European social statistics from 2021 and coincides with the split between the two household e-commerce datasets from 2020.

Every observation retains its original quality and break flags in separate fields, and no flagged value is silently overwritten. Where the household series is spliced, each observation carries the identifier of its source dataset, and a regime indicator distinguishes pre-2020 from 2020-onwards observations; Section 4.6 tests whether the estimated association differs between the pre-2020 and 2020-onwards source periods. Unavailable values are treated as missing rather than zero, and the primary analysis does not interpolate. Coverage is reported in full, with 27 countries and 268 complete country-year observations, or 99.3% of the 270-cell grid, with a complete missingness table by country, year, and variable presented in the Supplementary Materials.

3.5. Constructing the Asymmetry

Let D_{it} denote consumer adoption and C_{it} denote SME e-sales participation in country i and year t. The two are percentages of different populations with different denominators, so their raw difference has no interpretation as unmet demand or as a supply deficit, and we never use it that way. What is comparable is relative position. Within each year, both variables are standardised across the member states with complete observations in each year, i.e., 27 countries in eight years and 26 in 2019 and 2020:

$$z^{D}{}_{it} = (D_{it} - \overline{D}_t)/\sigma^{D}{}_t,\ z^{C}{}_{it} = (C_{it} - \overline{C}_t)/\sigma^{C}{}_t. \quad (1)$$

The asymmetry is their difference:

$$A_{it} = z^{D}{}_{it} - z^{C}{}_{it}. \quad (2)$$

Positive values indicate that consumer adoption is relatively further advanced than SME e-sales participation and negative values indicate the reverse. Standardisation places the two indicators on a common statistical scale but does not, by itself, make them measures of the same underlying market quantity: they are drawn from different populations and dif-

ferent economic domains, and the gap is therefore not an estimate of excess demand or unmet supply. What the standardised difference does capture is each country's position on one indicator relative to its position on the other, expressed in cross-country standard-deviation units. A country with a large positive value ranks higher, among EU member states, on consumer adoption than on SME participation; a large negative value indicates the opposite ranking. The construct is thus explicitly relational and distributional—being a comparison of relative standing across two adoption dimensions—rather than a within-market balance of buyers against sellers. It is meaningful to the extent that the two indicators both act as a proxy for national e-commerce development and that their relative movement informs whether the two dimensions advance together, which is the question this study addresses.

How this measure can be read depends on three features of its construction, which we state here rather than in Section 7.

Relative position is all it captures. A country's classification can change without any change in its own behaviour, if other member states move. This is intrinsic to within-year standardisation, and we disclose it alongside every typology result.

Standardising within year also removes the annual mean and variance by construction, so A_{it} carries no information about EU-wide development. RQ1 is answered on raw levels for that reason, and the asymmetry is reserved for relative position.

The third feature is less obvious and more consequential. Because both terms in (2) have unit variance within year, the cross-country variance of the asymmetry is

$$\mathrm{Var}(A_t) = 2(1 - \varrho_t), \text{ so } \mathrm{SD}(A_t) = \sqrt{(2(1 - \varrho_t))}, \tag{3}$$

where ϱ_t is the within-year cross-country correlation between D and C. Falling dispersion of the asymmetry and rising correlation between the two sides are the same fact. We therefore report ϱ_t alongside $\mathrm{SD}(A_t)$ throughout, frame any narrowing as increasing cross-country co-alignment rather than as convergence in levels, and test convergence in levels separately on the raw series. For the same reason, we do not run β-convergence regressions on the asymmetry: with the annual cross-sectional mean pinned at zero, regressing change on initial level returns a negative coefficient almost mechanically (the dynamic form of Galton's fallacy identified by Quah [27] and Friedman [28]). Dispersion claims depend on σ-convergence [29].

3.6. Estimation

RQ2 is addressed with a two-way fixed-effects specification,

$$C_{it} = \beta D_{it} + \alpha_i + \lambda_t + \varepsilon_{it}, \tag{4}$$

where α_i absorbs time-invariant country characteristics and λ_t absorbs shocks common to all member states in a given year, including the pandemic and the regulatory changes of Section 3.4. β is interpreted as a within-country association, not an effect.

Both series trend upward in nearly every member state, and with a time dimension of roughly a decade, panel unit-root tests are too underpowered to be informative. Year effects absorb the common trend but not country-specific drift, so (4) alone cannot separate co-movement from shared upward motion. We therefore estimate a first-difference specification as a short-run sensitivity check,

$$\Delta C_{it} = \beta \Delta D_{it} + \lambda_t + u_{it}, \tag{5}$$

and a third specification adding country-specific linear trends to (4). The baseline and first-difference specifications are estimated as the primary analysis, and the country-specific-

trend specification as part of the robustness analysis. The first-difference specification examines short-run year-to-year co-movement, while the levels specification captures medium-term within-country co-development; a difference between them would indicate that the two should be distinguished rather than that either is spurious. A null result is reported as such and would itself answer RQ2.

Inference follows the small-cluster literature. With 27 clusters and a short time dimension, cluster-robust standard errors can over-reject: Cameron, Gelbach and Miller [30] document over-rejection with 5 to 30 clusters and show that a bootstrap-t procedure restores nominal size where standard methods reject at twice the intended rate. We therefore report country-clustered robust standard errors as the primary inference and, given the small cluster count, supplement them with the bias-reduced CR2 variance with a t(26) reference and a wild cluster bootstrap-t (Rademacher weights, null imposed, 9999 replications) implemented in R and reported in Section 4.3. These small-sample corrections confirm the primary inference rather than overturn it. Driscoll–Kraay standard errors are not used either: they depend on large-T asymptotics [31] and are unreliable at this panel length. Cross-sectional dependence is expected given common European shocks and is addressed through year effects. Year fixed effects absorb shocks common to all countries, although they do not eliminate every possible form of residual cross-sectional dependence. A Pesaran CD test on the two-way fixed-effects residuals indicates that residual cross-sectional dependence is weak and at the margin of conventional significance (mean pairwise residual correlation = -0.03; CD = -1.92, $p = 0.055$). The dependence is small and, if anything, negative, but its possible presence is why inference relies on country-clustered and wild cluster bootstrap procedures rather than on treating year effects as fully sufficient.

Because both variables are proportions and several member states approach the upper range of consumer adoption, ceiling effects may attenuate the within-country association mechanically. As a functional-form sensitivity check, both shares are logit-transformed and analysed in a linear fixed-effects specification. Temporal ordering is examined only descriptively. With ten annual observations per country, lagged specifications carry no Granger-type inferential content; we therefore report a pooled within-country cross-correlation and a distributed-lag specification in Section 4.4 as exploratory description only, without any causal ordering claim.

3.7. Typology, Persistence, and Robustness

Configurations are defined by a tolerance band around zero. Because SD(A_t) equals $\sqrt{(2(1-\varrho_t))}$ rather than one and varies by year, the reference standard deviation must be fixed in advance: we use the pooled standard deviation across all country-years and prespecify bands of ± 0.25 and ± 0.50 pooled standard deviations, reporting both. Thresholds were fixed before the classification was inspected. Persistence is summarised by a year-on-year transition matrix, where concentration on the diagonal indicates persistence of the relative configurations over the observation window.

The robustness programme addresses specific concerns rather than accumulating tests. It comprises an alternative definition of consumer adoption (three-month rather than twelve-month purchasing); exclusion of the pandemic years 2020 and 2021; country-specific linear trends added to the levels specification; a functional-form check on logit-transformed shares; the pre-2020 against 2020-onwards source–period interaction; exclusion of three contrasting member states in turn; and the two tolerance bands for the configuration typology. The programme also includes small-sample corrections (CR2 and the wild cluster bootstrap-t), an equivalence (TOST) test, population- and enterprise-weighted specifications, an expanded threshold sensitivity grid from 0.10 to 1.00 SD, continuous persistence measures, and country-clustered bootstrap inference for the dispersion and

co-alignment trends. Each is reported in Sections 4.3–4.6, with the supporting detail in Tables A2–A4.

3.8. Reproducibility

The workflow is deliberately simple and is documented so that it can be reproduced with standard tools from the stated Eurostat datasets, Excel processing steps, and SPSS 27 specifications. Eurostat series were downloaded in spreadsheet format, with the dataset code, extraction date and indicator definition recorded for each; raw downloads are preserved unaltered. Harmonisation, the survey-year to reference-year alignment, flag handling, and the missingness audit were carried out in Excel, and the resulting panel was analysed in IBM SPSS Statistics using the specifications set out in Section 3.6. The small-sample and weighted analyses introduced in revision—the wild cluster bootstrap-t, the CR2 correction, the equivalence test, the country-clustered bootstrap for the dispersion and persistence trends, and the population- and enterprise-weighted specifications—were carried out in R. The wild cluster bootstrap-t uses Rademacher weights, whereas the country-clustered bootstraps for the trend and persistence analyses resample countries with replacement; all resampling procedures use a fixed random seed, so that the reported estimates reproduce exactly, and the scripts, the weighted panel, and the two Eurostat weighting series are deposited with the minimal dataset so that every analysis introduced in revision can be reproduced from the deposited scripts. Cluster-robust inference is obtained through generalised estimating equations with an independent working correlation and a robust sandwich covariance, which yields country-level robust standard errors under that working-correlation structure. The estimates for every model reported here, with their standard errors, confidence intervals, and sample sizes, are collected in Table A1 in Appendix A. The raw Eurostat downloads, the harmonised panel, its documentation, the R scripts for the small-sample and weighted analyses, the weighted panel, and the two Eurostat weighting series are deposited as a minimal dataset with this article, so that the construction of every variable can be traced from the published spreadsheets to the analysis file. The original SPSS output files are included in the deposit and document the procedures, variables, model specifications and numerical results. The additional analyses introduced during revision were implemented in R, and the corresponding executable scripts are deposited with the data package.

4. Results

4.1. Coverage

The panel holds 270 country-year cells, of which 268 carry both a consumer adoption and an SME participation value. Two are incomplete: Greece has no SME e-sales figure for 2019, and France has no household purchasing figure for 2020. Neither series was interpolated, and both gaps are carried as missing through every model. The three-month consumer adoption series, added as a robustness measure, covers 269 cells across the full window. Three sample sizes recur below and are worth fixing here: 270 is the full country-year grid (27×10); 269 is either single series on its own, missing one cell; and 268 is the count with both series present, which is the base for every model that uses the two together and for the alignment gap. First-difference models drop one further year per country and so run on 239.

Consumer adoption carries five break flags and SME participation three; the enterprise series also carries one low-reliability flag. All flags are retained in dedicated variables and documented in the metadata audit; no flagged observation is recoded or removed from the primary analysis. In total, 9 of the 270 country-years carry a break (b) or low-reliability (u) flag on one of the two indicators: breaks for Germany (2021), Ireland (2021),

Latvia (2016), Luxembourg (2018) and Sweden (2016) on the consumer series and for France (2021), the Netherlands (2024) and Sweden (2022) on the enterprise series, together with the low-reliability flag for Greece (2019). Excluding all nine leaves the main levels coefficient essentially unchanged (0.111 versus 0.116, $p = 0.175$), so the finding does not rest on flagged observations. Table 1 lists the concepts and sources; Table 2 reports coverage and descriptive statistics.

Table 1. Concepts, indicators, and sources.

Concept	Indicator	Population	Source Dataset	Years
Consumer adoption	Purchased online in the last 12 months (%)	Individuals 16–74	isoc_ec_ibuy (2015–2019); isoc_ec_ib20 (2020–2024)	2015–2024
SME e-sales participation	E-commerce sales $\geq 1\%$ of turnover (%)	Enterprises, 10–249 persons employed	isoc_ec_esels (survey year t $\rightarrow$ reference year $t-1$)	2015–2024
Three-month adoption (robustness)	Purchased online in the last 3 months (%)	Individuals 16–74	isoc_ec_ibuy; isoc_ec_ib20	2015–2024

Note: the enterprise series covers NACE Rev. 2 sections C to N excluding K (manufacturing, utilities, construction, trade, transport, accommodation and food service, information and communication, real estate, professional and administrative activities), so activities outside the survey include agriculture, forestry and fishing, mining and quarrying, financial and insurance activities, public administration, education, health and other services; microenterprises (fewer than 10 persons employed) are outside the survey. Enterprise e-commerce variables published under survey year t refer to sales in year $t-1$ and are aligned accordingly. All series were extracted on 5 August 2026; the Eurostat "last update of data" stamps of the extracted versions were 16 June 2024 (isoc_ec_ibuy), 17 April 2026 (isoc_ec_ib20) and 15 June 2026 (isoc_ec_esels). SME: small and medium-sized enterprises. Source: own processing.

Table 2. Panel coverage and descriptive statistics, pooled 2015–2024.

Variable	*N*	Mean	SD	Min	Max	Complete (%)
Consumer adoption (%)	269	59.78	18.22	10.83	94.70	99.6
SME e-sales participation (%)	269	19.61	7.73	5.19	37.90	99.6
Three-month adoption (%)	269	48.75	18.10	7.63	86.66	99.6
Alignment gap (z-difference)	268	0.00	0.995	−2.52	2.87	99.3

Note: the two incomplete cells are Greece (SME participation, 2019) and France (consumer adoption, 2020). No value is interpolated. Source: own processing.

4.2. Development of the Two Series (RQ1)

The two series grew at very different rates (Figure 1a; Table 3). Consumer online purchasing rose from a cross-country mean of 47.1% in 2015 to 72.1% in 2024, representing a gain of 25 percentage points. SME e-sales participation moved from 16.0% to 22.1% over the same decade, a gain of 6 percentage points. The two figures rest on different populations and denominators and are not directly comparable as magnitudes.

The dispersion of the two series moved in opposite directions, and this is the more telling pattern. Cross-country variation in consumer adoption collapsed: its standard deviation fell from 19.08 to 11.91, a decline of 0.91 points per year that a simple time trend fits almost perfectly ($p < 0.001$, $R^2 = 0.96$). Member states that lagged in 2015—Romania at 22%, Bulgaria at 31%—closed much of the distance to the leaders, so the household distribution tightened year after year. SME participation did not show comparable convergence. Its cross-country standard deviation rose slightly, from 6.75 to 7.37 (+0.13 per year), though this upward drift was not significant under the country-clustered bootstrap (Table A3); no statistically reliable convergence in SME participation was evident, and if anything, dispersion drifted upward. Member states are becoming more alike in the share of consumers purchasing online. They are not becoming similarly alike in the share of SMEs participating

in e-sales. Škare et al. [32], linking DESI to the joint European Commission–ECB enterprise survey, identify managerial capability and the supply of skilled labour—resources that develop slowly and remain unevenly distributed across member states—as constraints on the firm side. Constraints of that kind may help to explain why the SME participation distribution stays wide while the consumer adoption one closes.

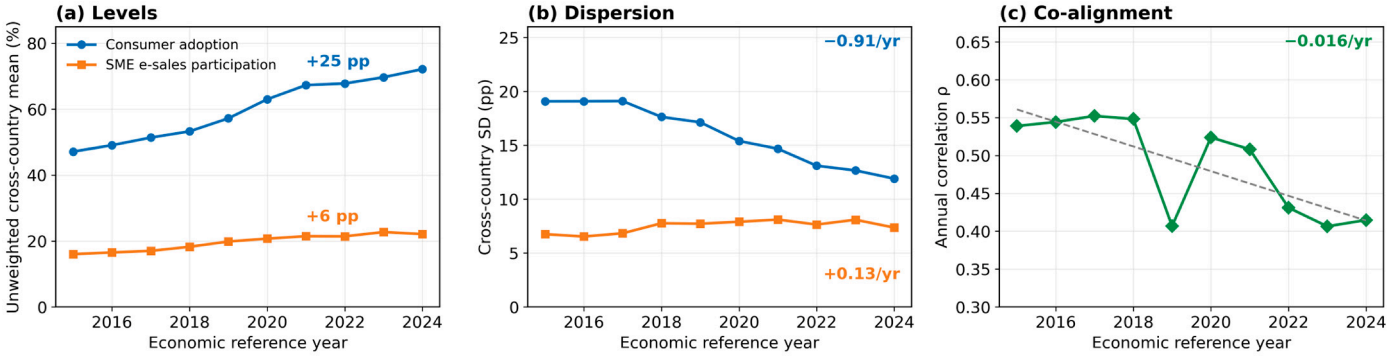

Figure 1. Development of the two series across the EU-27, 2015–2024: (**a**) unweighted cross-country mean levels; (**b**) cross-country standard deviation of each series; (**c**) annual cross-country correlation between them, with a fitted linear trend shown as a dashed line; the trend line is a descriptive summary over ten annual values, not a causal model. Panel (**a**) shows the different growth paths of the two raw indicators from which the alignment gap $A_{it} = zD_{it} - zC_{it}$ is formed, panel (**b**) the convergence of consumer adoption against the absence of comparable convergence in SME participation, and panel (**c**) the decline in co-alignment, which is not monotonic. Source: own processing.

Table 3. Annual EU-27 means and cross-country dispersion of the two series.

Year	Consumer Adoption Mean (%)	Consumer Adoption SD	SME Participation Mean (%)	SME Participation SD	ϱ (Consumer Adoption, SME Participation)	SD of Gap	*N*
2015	47.13	19.08	16.02	6.75	0.539	0.960	27
2016	49.06	19.09	16.53	6.54	0.544	0.955	27
2017	51.38	19.10	17.01	6.83	0.552	0.946	27
2018	53.27	17.63	18.25	7.77	0.548	0.951	27
2019	57.21	17.13	19.86	7.73	0.407	1.089	26
2020	63.00	15.39	20.72	7.90	0.524	0.976	26
2021	67.29	14.68	21.46	8.11	0.508	0.992	27
2022	67.78	13.10	21.42	7.65	0.431	1.067	27
2023	69.67	12.67	22.72	8.09	0.406	1.090	27
2024	72.13	11.91	22.11	7.37	0.415	1.082	27

Note: annual means and SDs use all available observations for each individual series; the reported *N* is the complete-case count used for the annual correlation and the alignment-gap dispersion. Means and SDs are on raw percentages; ϱ is the annual cross-country Pearson correlation on complete cases; SD of the gap equals $\sqrt{(2(1-\varrho))}$ by construction (Equation (3)). Linear trends: consumer adoption SD −0.910/year ($p < 0.001$, $R^2 = 0.96$); SME participation SD +0.131/year ($R^2 = 0.48$); gap SD +0.016/year ($R^2 = 0.60$). These are ordinary least squares fits to ten annual points; inferential conclusions about the trends depend on the country-clustered bootstrap intervals in Table A3, under which only the consumer adoption trend is significant. Source: own processing.

The convergence in consumer adoption we observe is consistent with Stângaciu et al. [33], who report that the pandemic-era expansion of EU e-commerce coincided with a narrowing of disparities among member states. Their territorial convergence finding is told with consumer indicators; the value of separating the two measures is that the observed convergence is confined to consumer adoption and has no clear counterpart on the firm side.

Pooled across all country-years, consumer adoption and SME participation correlate at 0.55 (Spearman 0.51, $N = 268$). This is a moderate pooled association, and it frames the two questions the rest of the analysis turns on: whether it reflects the two sides moving together within countries over time and whether it is stable across the decade.

4.3. Within-Country Association (RQ2)

Table 4 reports the primary levels specification alongside the first-difference sensitivity check. Both isolate within-country variation, and both return a small positive coefficient that falls short of conventional significance. Because the co-development question is conceptually symmetric whereas the fixed-effects specification is directional, we also estimated the reverse specification, with consumer adoption as the dependent variable. The coefficient is 0.364 ($p = 0.151$), leading to the same substantive conclusion of a positive but imprecisely estimated within-country association. As direction-neutral summaries, the two-way within-country correlation is 0.205, and the correlation of consecutive-year within-country changes is 0.15. The weak within-country co-movement therefore does not depend materially on which indicator is designated as the dependent variable.

Table 4. Within-country association between consumer adoption and SME e-sales participation.

	Baseline Two-Way Fixed Effects	First-Difference Sensitivity
Coefficient on adoption (Δ consumer adoption)	0.116	0.091
Robust SE (clustered on country)	0.073	0.047
95% Wald CI	[−0.027, 0.259]	[−0.002, 0.184]
Wald χ^2 (df = 1)	2.53	3.69
p	0.112	0.055
Country fixed effects	yes	absorbed by differencing
Year fixed effects	yes	yes
Estimation	GEE, independent working correlation, robust covariance	GEE, independent working correlation, robust covariance
Observations	268	239
Countries (clusters)	27	27

Note: the dependent variable is SME e-sales participation in the baseline specification and its first difference in the sensitivity specification; the predictor is consumer adoption and its first difference, respectively. Coefficients are within-country associations, not causal effects. Both confidence intervals include zero. With 27 country clusters, conventional robust covariance estimates may give imperfect finite-sample inference, so the Wald tests should be read cautiously. Source: own processing.

In levels, with country and year fixed effects, a one-point rise in consumer adoption is associated with a 0.116-point change in SME e-sales participation (SE 0.073, 95% CI [−0.027, 0.259], $p = 0.11$). The confidence interval spans zero. In first differences, where the year-to-year change in SME e-sales participation is regressed on the corresponding change in consumer adoption, the coefficient is 0.091 (SE 0.047, 95% CI [−0.002, 0.184], $p = 0.055$)—the same sign, the same rough magnitude, and again an interval whose lower bound touches zero.

The contrast with the pooled correlation of 0.55 is the point. A substantial part of that pooled association reflects persistent differences between countries: countries that rank high on consumer adoption also rank high on SME e-sales participation, because both track a country's broader digital and economic development. Once that between-country ranking is absorbed by the fixed effects, the estimated within-country association is small and imprecisely estimated. The estimate is sensitive, however, to the weighting assigned to member states. Because the unweighted specification treats each member state as one observation—appropriate for a question about convergence across jurisdictions, each

of which sets policy independently—it gives Luxembourg the same weight as Germany. Weighting instead toward larger economies, by national population aged 15–74 or by the number of enterprises with 10–249 persons employed, raises the levels association to 0.248 ($p = 0.002$) and 0.260 ($p < 0.001$), respectively (Table A3). The first-difference association remains small and non-significant under every weighting. Weighting changes the estimand rather than testing a mechanism: it allows a few large economies to dominate, so the larger weighted coefficient does not by itself establish that the association is stronger in bigger member states. We therefore report the weighted models only as sensitivity analyses and do not interpret the contrast as evidence of heterogeneity by economy size; the unweighted specification, which treats each member state as one jurisdiction, remains the primary one. On this evidence, consumer adoption is not robustly associated with contemporaneous changes in SME e-sales participation. The two specifications agree, which matters more than either p-value: a result that survives first-differencing but stays marginal in levels, or vice versa, would invite a specification-dependent reading. Here neither model provides statistically precise evidence of a within-country association, and both point the same way.

Inference rests on country-clustered robust standard errors from a GEE with an independent working correlation. With 27 clusters, conventional robust covariance estimates may provide imperfect finite-sample inference [30], so we supplement the Wald tests with two small-sample corrections: the bias-reduced CR2 variance with a t(26) reference and a wild cluster bootstrap-t that imposes the null with Rademacher weights over 9999 replications. Both are reported in Table A2. These procedures differ in their variance estimators and reference distributions and therefore need not yield identical p-values; the primary GEE sandwich inference in Table 4 remains the reference, with CR2 and the bootstrap reported as small-sample robustness checks. They confirm and, if anything, strengthen the baseline reading: the levels' p-value is 0.145 under CR2 and 0.218 under the bootstrap, and the first-difference p-value rises from 0.055 to 0.097. The GEE sandwich p-value of 0.112 in Table 4 and the 0.148 obtained under conventional cluster-robust (CR1) standard errors in Table A2 apply to the identical point estimate of 0.116 and differ only in the variance estimator; the small-sample corrections (CR2, wild cluster bootstrap) are the appropriate reference for finite-sample inference with 27 clusters. Two single-country exclusions that came closest to significance under conventional inference are also non-significant under the bootstrap. Two one-sided tests (TOSTs) for equivalence further support equivalence within ± 0.5 and ± 0.3 percentage-point bounds ($p < 0.001$ and $p = 0.015$, respectively). The equivalence tests use country-clustered standard errors (HC1) with a t(26) reference. These bounds are anchored to the SME participation scale rather than chosen arbitrarily: a coefficient of 0.3 to 0.5 would imply a 3-to-5-percentage-point change in SME participation for a 10-point change in consumer adoption, equivalent to roughly 15 to 26% of the pooled SME participation mean of 19.6%.

4.4. Configurations and Persistence (RQ3)

Classifying each country-year by its alignment gap makes the imbalance concrete. Under the ± 0.25 SD band, 117 of 268 observations (43.7%) are SME-leading—the firm side relatively ahead—and 99 (36.9%) are consumer-leading, with only 52 (19.4%) falling inside the aligned band. Four observations in five sit off-centre. Widening the band to ± 0.50 SD moves more cases into the aligned category, as it must, but the picture holds: 32.1% are SME-leading, 29.5% consumer-leading, and 38.4% aligned. The split between the two leading directions stays near-symmetric under both bands, so both directional categories remain substantial under the wider tolerance band.

The substantive pattern is not materially sensitive to the particular standardisation used. Reconstructing it from within-year percentile ranks or from robust standardized

scores using the median and the median absolute deviation reproduces almost exactly the same series (correlations of 0.97 and 0.98 with the z-score gap). The three-category configuration is correspondingly stable: the same country-year is assigned the same category in 83–87% of cases across constructions (Cohen's $\kappa = 0.74$ and 0.79). The main levels coefficient is also not driven by extreme values; trimming the most extreme 5% of gaps leaves it small and non-significant (0.08, $p = 0.31$).

One caution on reading these labels: the gap is relative, standardised within each year across the 27 states, so "SME-leading" does not mean a country's firms sell more than its consumers buy. The two percentages have different populations and different denominators—individuals aged 16–74 on one side and enterprises with 10–249 persons employed on the other—and cannot be compared as magnitudes at all. The label means only that the country's SME side stands higher against the EU distribution than its consumer side does. A country can shift category because its neighbours moved, not because it did.

Persistence is high. Year-on-year, 200 of 239 transitions (83.7%) stay in the same configuration, against 36.5% expected from the marginal distributions alone; Cohen's κ of 0.74 places this in the range conventionally read as substantial agreement beyond chance, with a 95% country-clustered bootstrap interval of [0.60, 0.83] that stays well clear of zero. Because slow-moving national indicators can generate high year-to-year retention mechanically, we benchmarked the observed persistence using joint circular shifts of the panel. The same temporal offset was applied to every country, preserving the cross-sectional configuration within each shifted year while changing the temporal cut point. Across the nine non-trivial rotations, retention averages 0.793 (range 0.782–0.799) and κ averages 0.673 (range 0.657–0.685). The observed values (0.837 and 0.743) are the largest among the ten admissible temporal rotations. Because only ten joint temporal rotations are possible, the exact randomisation p-value is 0.10 for both statistics. The configuration persistence is thus somewhat greater than a null preserving both temporal and cross-sectional structure would typically produce, but the margin is modest, and much of the observed stability reflects the joint inertia of the two slow-moving series rather than a distinct and durable cross-country ordering. This persistence does not depend on the ± 0.25 SD threshold: across bands from 0.10 to 1.00 SD, κ remains between 0.68 and 0.74 (Table A4, Panel B), and it is equally visible in threshold-free measures. The within-country first-order autocorrelation of the gap is 0.46, the between-country share of its variance is 0.85, and the year-to-year rank correlation of countries on the gap is 0.93 (Table A4, Panel A). The temporal ordering of the two series can also be read descriptively. Upon pooling the within-country demeaned series, the cross-correlation between consumer adoption at year t and SME participation at year t + k peaks at k = 0 (r = 0.68), with near-symmetric values one year on either side (0.66 at k = −1, 0.58 at k = +1); a distributed-lag specification tells the same story, with the contemporaneous term significant (0.13, $p = 0.04$) and the one-year lag not (−0.03, $p = 0.68$). At annual frequency, the co-movement is therefore essentially contemporaneous, with no detectable lead of one dimension over the other. This is exploratory description, not a causal-ordering test. Broken out by state, the two imbalanced configurations are the most stable (Table 5): an SME-leading country-year is followed by another SME-leading year 90.3% of the time (country-clustered bootstrap 95% CI 83.3–95.7%) and a consumer-leading year by another consumer-leading year 90.8% of the time (95% CI 81.2–96.7%). Direct transitions between the two extremes number 3 in 239 (1.3%). Relative alignment shows the lowest retention: 57.1% of aligned years are followed by another aligned year (95% CI 41.9–69.2%), an interval that does not overlap those of the two outer categories. This difference should not be over-read, because the classification geometry works against the middle category; the aligned band is bounded on both sides and sits at the centre of the

distribution, so its members are closer to a boundary than those of the two open-ended outer categories and have more ways to leave it.

Table 5. Year-on-year transition matrix of alignment configurations (±0.25 SD band).

From\To	SME-Leading	Relatively Aligned	Consumer-Leading	Row Total	Stayed (%)
SME-leading	93	8	2	103	90.3
Relatively aligned	13	28	8	49	57.1
Consumer-leading	1	7	79	87	90.8
Column total	107	43	89	239	83.7

Note: Configurations follow the alignment gap $A_{it} = zD_{it} - zC_{it}$, where positive values are consumer-leading and negative values SME-leading. There are 239 usable year-on-year transitions; 200 (83.7%) remain in the same configuration. Agreement expected from the marginal distributions alone is 36.5%, giving Cohen's $\kappa = 0.74$, showing substantial agreement beyond chance. There are three direct transitions between the two leading configurations (1.3%). Classification uses the pooled SD of the alignment gap (0.995); the ±0.50 SD band yields 86 SME-leading, 103 aligned, and 79 consumer-leading observations, with the same qualitative pattern. Country-clustered bootstrap 95% confidence intervals for the retention probabilities (2000 replications, countries resampled with replacement) show SME-leading 83.3–95.7%; relatively aligned 41.9–69.2%; and consumer-leading 81.2–96.7%. Source: own processing.

Grouping countries according to the configuration implied by their mean alignment gap yields three classes, and this grouping is not a new construction; it applies the same ±0.25 SD bands set in Section 3.7, evaluated on each country's mean gap. Thirteen member states fall on the SME-leading side (Lithuania, Croatia, Ireland, Portugal, Malta, Romania, Spain, Belgium, Cyprus, Bulgaria, Greece, Denmark, Czechia), nine on the consumer-leading side (Latvia, Finland, Poland, Estonia, Germany, Slovakia, the Netherlands, France, Luxembourg), and five sit inside the aligned band (Sweden, Italy, Slovenia, Hungary, Austria). No separation test is reported for these classes: they are defined by cutting the mean gap at the pre-set bands, so testing whether the resulting groups differ on the mean gap would be circular. What can be checked independently is agreement with a different criterion, and the mean-gap classification matches the unique modal annual configuration wherever one exists, resolving the ties for Czechia (5/0/5) and Sweden (4/4/2). Figure 2 displays the underlying country-year pattern.

These are classes read off the pre-specified bands at country level, not groups recovered by a clustering procedure, and nothing in what follows depends on their boundaries.

Table 6 gives the full country picture, and it shows how unevenly the positions are held. Nine member states never leave one configuration across the whole decade; Croatia, Cyprus, Lithuania and Portugal are SME-leading in all ten years, while Estonia, Luxembourg, the Netherlands, Poland and Slovakia are consumer-leading in all ten. France is consumer-leading in all nine of its observed years. Romania and Spain are SME-leading in nine years of ten, and Germany is consumer-leading in eight. Others are far less settled: Sweden splits into four SME-leading years, four aligned, and two consumer-leading, and Czechia divides evenly between the two extremes. Hungary and Italy sit mostly in the aligned band. Therefore, the persistence reported above is not uniform across states—a minority of countries account for most of the stability, and a handful move repeatedly.

Romania's position illustrates how the relative measure must be read and is worth spelling out because it inverts the intuitive picture. Romanian consumers rank near the bottom of the EU adoption distribution, and Romanian SMEs also rank low on e-sales participation; the classification is SME-leading because the consumer side sits further below the EU norm than the firm side does. It is a statement about relative standing within the EU distribution, not about either side being strong in absolute terms. The same caution applies to every label in the table.

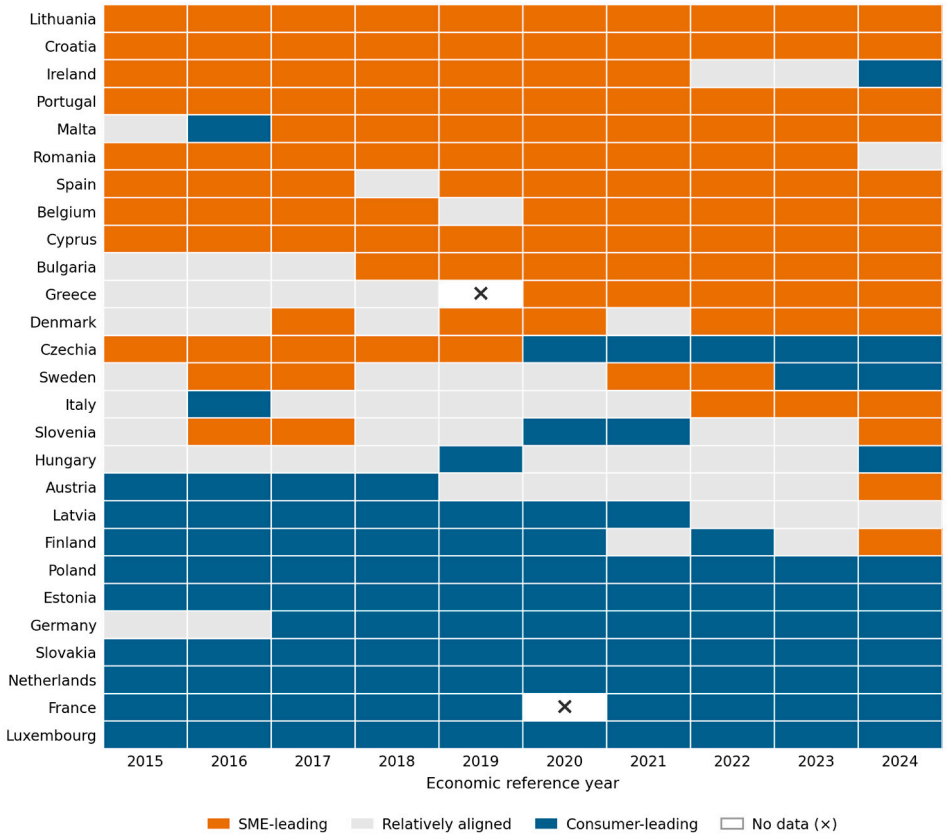

Figure 2. Alignment configuration by country and year, 2015–2024. Countries are ordered by their mean alignment gap, from most SME-leading at the top to most consumer-leading at the bottom. Configurations use the ±0.25 SD band around zero, applied to the alignment gap $A_{it} = zD_{it} - zC_{it}$, so positive values are consumer-leading and negative values SME-leading. Unbroken horizontal bands indicate countries that hold one position throughout; the two cells marked × indicate the missing observations discussed in Section 4.1 (Greece 2019, France 2020). Source: own processing.

Table 6. Country profiles: years in each configuration, 2015–2024 (±0.25 SD band).

Country	Mean Gap	SME-Leading	Relatively Aligned	Consumer-Leading	Dominant	Share (%)	Switches
Lithuania	−1.71	10	0	0	SME-leading	100	0
Croatia	−1.50	10	0	0	SME-leading	100	0
Ireland	−0.88	7	2	1	SME-leading	70	2
Portugal	−0.82	10	0	0	SME-leading	100	0
Malta	−0.73	8	1	1	SME-leading	80	2
Romania	−0.65	9	1	0	SME-leading	90	1
Spain	−0.63	9	1	0	SME-leading	90	2
Belgium	−0.60	9	1	0	SME-leading	90	2
Cyprus	−0.57	10	0	0	SME-leading	100	0
Bulgaria	−0.55	7	3	0	SME-leading	70	1
Greece	−0.33	5	4	0	SME-leading	56	1
Denmark	−0.30	6	4	0	SME-leading	60	5
Czechia	−0.26	5	0	5	SME-leading	50	1
Sweden	−0.14	4	4	2	Relatively aligned	40	4
Italy	−0.09	3	6	1	Relatively aligned	60	3
Slovenia	0.01	3	5	2	Relatively aligned	50	5
Hungary	0.20	0	8	2	Relatively aligned	80	3
Austria	0.21	1	5	4	Relatively aligned	50	2
Latvia	0.43	0	3	7	Consumer-leading	70	1
Finland	0.45	1	2	7	Consumer-leading	70	4

Table 6. *Cont.*

Country	Mean Gap	SME-Leading	Relatively Aligned	Consumer-Leading	Dominant	Share (%)	Switches
Poland	0.60	0	0	10	Consumer-leading	100	0
Estonia	0.66	0	0	10	Consumer-leading	100	0
Germany	0.93	0	2	8	Consumer-leading	80	1
Slovakia	1.15	0	0	10	Consumer-leading	100	0
Netherlands	1.35	0	0	10	Consumer-leading	100	0
France	1.38	0	0	9	Consumer-leading	100	0
Luxembourg	2.49	0	0	10	Consumer-leading	100	0

Note: Countries ordered by mean alignment gap, from most SME-leading to most consumer-leading. A positive gap indicates a consumer-leading position. "Dominant" is the class assigned by the country's mean gap against the ±0.25 SD bands. This classification coincides with the unique modal annual configuration wherever one exists; for Czechia (5/0/5) and Sweden (4/4/2), where the modal configuration is tied, the mean gap resolves the assignment. "Share" is the percentage of observed years in the modal configuration; "switches" counts changes of configuration between consecutive observed years. France has nine observed years (2020 consumer adoption missing) and Greece nine (2019 SME participation missing). Labels describe relative standing within the annual EU distribution, not absolute levels on either side. Source: own processing.

4.5. Dispersion and Co-Alignment (RQ4)

Because the gap is standardised within year on complete cases, its cross-country dispersion is governed exactly by the annual correlation between the two sides: $SD(gap) = \sqrt{(2(1 - \varrho))}$. The two are the same fact viewed from two angles, and reporting them together keeps the convergence question honest.

The annual correlation fell over the decade from 0.539 in 2015 to 0.415 in 2024 (Figure 1c). A linear trend on the 10 annual values has a negative slope (−0.016 per year), but inference on 10 annual points estimated from 27 countries is fragile: a country-clustered bootstrap that re-estimates each year's correlation places the 95% interval on the slope at [−0.049, 0.013], so the decline is not statistically distinguishable from no trend (Table A3). By the identity, the dispersion of the gap rose in step, from 0.96 to 1.08, and carries the same uncertainty. The point estimate is robust to which year is dropped (leave-one-year-out slopes range from −0.020 to −0.024), so the direction is stable even though it is imprecisely estimated. The decline is a trend rather than a monotonic slide; Table 3 shows the correlation dropping in 2019, recovering through 2020 and 2021, then falling again. The pattern is not confined to a single pandemic-year change; the downward trend includes interruptions and partial recoveries.

Set beside the raw-series trends from Section 4.2, three separate facts line up. Cross-country dispersion in consumer adoption fell sharply and significantly (−0.91 per year, R^2 = 0.96; bootstrap 95% CI [−1.25, −0.51]). Dispersion in SME e-sales participation drifted upward, but only slightly and not significantly (+0.13 per year; bootstrap 95% CI [−0.06, +0.33]). Moreover, independently of both, the annual correlation between the series edged down without reaching significance. The first two do not mathematically imply the third: a correlation is invariant to separate rescaling of its components, so both dispersions could have moved exactly as they did while the correlation held constant. What ties the third fact to the gap is construction rather than causation—because the gap is a difference in within-year standardised scores, its dispersion rises precisely as the correlation falls. Taken together and read at the level of statistical confidence each trend supports, the three describe consumers growing significantly more alike in their online buying, firms showing no significant change in the spread of whether they sell online, and a co-alignment between the two that edges down but not decisively. Only the consumer-side convergence is firmly established; the other two are directional descriptions consistent with it. The convergence observed over the Digital Single Market period is, on these data, a

convergence in consumer adoption with no counterpart in SME participation. Whether the single-market programme caused the household convergence is beyond what a descriptive panel can establish; what the data show is that the observed convergence is confined to consumer adoption and has no clear counterpart on the firm side.

This reading sits within a small recent study on digital convergence in Europe. Aumeboonsuke [34], applying sigma and beta convergence to five composite digital indices, which finds the EU converging more consistently than comparator regions across the 2010–2024 window (a demand-and-infrastructure convergence that our household series echoes). What that single-index work cannot register is that the convergence is one-sided; pairing the two constructs, as we do, exposes a divergence inside a single economy that composite-index convergence studies are built not to see. Bruno et al. [35] make the complementary point at finer resolution, showing that national digital aggregates mask substantial within-country divides. Their caution reinforces ours (Section 3.5): a country-level correlation is a statement about national distributions, not about the firms and consumers inside any one country.

4.6. Robustness

Table 7 collects the robustness programme. The question it addresses is narrow: is the weak, marginal within-country association of Section 4.3 sensitive to the definition of consumer adoption, to functional form, to sample or to specification? Across the reported specifications, the estimate remains small and positive and does not reach conventional significance. The separate-period estimates reported below are descriptive checks of temporal stability, interpreted through the interaction test rather than through differences in their individual p-values.

Table 7. Robustness of the within-country association (RQ2).

Check	What Changes	Coefficient	95% CI	p	Conclusion
Baseline two-way fixed effects	—	0.116	[−0.027, 0.259]	0.112	small, positive, not significant
First-difference sensitivity	levels → Δ	0.091	[−0.002, 0.184]	0.055	sign and inference unchanged
Three-month adoption	12-month → 3-month window	0.048	[−0.127, 0.222]	0.593	smaller estimate; inference unchanged
Exclude pandemic	drop 2020–2021	0.112	[−0.039, 0.263]	0.147	near-identical estimate
Country-specific trends	add country × linear year	0.126	[−0.019, 0.271]	0.090	sign and inference unchanged
Logit shares	levels → logit of proportions	0.177	[−0.043, 0.397]	0.114	inference unchanged; scale differs
Source–period interaction	pre-2020 vs. 2020-onwards slope	0.031	[−0.051, 0.114]	0.453	no difference between source periods
Leave out Germany	drop the largest economy	0.081	[−0.069, 0.230]	0.290	inference unchanged
Leave out Romania	drop the lowest-adoption state	0.141	[−0.011, 0.294]	0.068	inference unchanged
Leave out Denmark	drop a high-participation state	0.133	[−0.010, 0.277]	0.069	inference unchanged

Note: The dependent variable is SME e-sales participation, or its logit or first difference. All level specifications include country and year fixed effects; first-difference models include year fixed effects, with time-invariant country effects removed by differencing. Standard errors are clustered by country throughout. The logit specification is a linear fixed-effects sensitivity check on logit-transformed shares, not a fractional-response model in the sense of Papke and Wooldridge [36], so its coefficient is not directly comparable in magnitude with the percentage-point estimates. Across the level and first-difference specifications, the estimate stays between 0.05 and 0.14 and reaches significance in none. In the source–period interaction, the 2020-onwards series is the reference category, so the reported coefficient is the differential for the pre-2020 series relative to it. Taking the pre-2020 series as the reference instead yields the 2020-onwards differential, −0.031, with a 95% confidence interval of [−0.114, 0.051]; the two parameterisations are exact negatives and share the same magnitude, interval width, and p-value. Source: own processing.

The country-specific trend specification remains positive (0.126, $p = 0.09$); the coefficient does not reach the 0.05 threshold, so the substantive interpretation is unchanged, although the association is better described as consistently weak than as a clean zero. Across the specifications measured in percentage points, the estimate stays between 0.05 and 0.14; that stability, not any single row, is the result. The three-month consumer adoption measure returns the weakest coefficient of all (0.048, $p = 0.59$); defining consumer adoption by recent rather than annual purchasing does not strengthen the link to SME e-sales participation, which warns against any reading in which the null is an artifact of the twelve-month window. The source–period interaction, testing whether the association differs between the pre-2020 and 2020-onwards household series, is itself insignificant (interaction coefficient 0.03, $p = 0.45$), so we find no evidence that the estimated association differs across the two source periods; this test does not, however, establish full metric comparability across the series break. The three selected-country exclusions move the coefficient between 0.08 (without Germany) and 0.14 (without Romania) without crossing into significance. These are illustrative checks on contrasting states, not the full leave-one-country-out series over all 27 members. A more direct pre-/post-pandemic test addresses whether the consumer–SME relationship itself shifted around COVID-19, rather than merely across the data-source break. Adding an interaction between consumer adoption and a post-2020 indicator to the two-way fixed-effects specification leaves the interaction insignificant (-0.03, $p = 0.50$), and estimating the association separately on the two sub-periods gives 0.176 ($p = 0.08$) for 2015–2019 and 0.121 ($p = 0.01$) for 2020–2024, both small and close to one another. We therefore find no evidence that the within-country relationship changed across the pandemic break; the data do not support a distinct pandemic-era regime. A further check addresses the survey-year to reference-year realignment of the enterprise series. Re-estimating the baseline with each enterprise value kept at its original published survey year, rather than shifted to the reference year it describes, gives a within-country association of 0.128 ($p = 0.12$), which is close to the 0.116 of the aligned specification. This result is therefore not an artefact of the temporal realignment.

Two further properties hold by construction rather than by test and are noted for completeness. Both directional configurations remain substantial under the ± 0.25 and ± 0.50 SD bands, although the category shares change as expected when the tolerance band is widened: the ± 0.25 and ± 0.50 SD bands give the same qualitative split (Section 4.4). The alignment measure is scaled so that its annual mean is exactly zero and the correlation–dispersion identity of Equation (3) holds to machine precision, which no estimation choice can disturb.

5. Discussion

Over the decade, European households converged significantly in online buying, while SME e-sales participation showed no comparable evidence of convergence. The annual cross-country co-alignment of the two indicators also edged downward, although this trend remains statistically uncertain, and within countries, the two series co-developed only weakly. This finding argues against the intuition that progress in consumer adoption carries SME participation with it.

The within-country result is central to the interpretation. A pooled correlation of 0.55 between consumer adoption and SME e-sales participation looks, at first glance, like two processes moving together. A substantial part of it reflects persistent differences between countries: those ranking high on consumer adoption tend also to rank high on SME e-sales participation, which is a pattern consistent with broader differences in digital and economic development that are not measured here, and the cross-section does the rest. After country and year fixed effects are included, the estimated within-country association

is small and imprecise (0.116 in levels and 0.091 in differences, with neither reaching significance). This result is compatible with asynchronous co-diffusion, though it provides no direct evidence of the underlying mechanism. Complementary innovations diffuse with asymmetric and imperfectly synchronised timing [14]: the two sides raise each other's returns without moving in lockstep. Consumer adoption in Europe faces low switching costs and is served by platforms with continental reach, so a country's consumers may advance without equivalent domestic SME e-sales development. Selling participation is bounded by internal capability, fixed investment, and firm-size structure and moves slowly. The weak within-country coupling is compatible with asynchronous co-diffusion rather than an anomaly to be explained away.

If the two sides were merely slow to synchronise, the imbalance would be expected to erode as laggards caught up. The observed configurations show little tendency to erode over the ten-year window. An SME-leading or consumer-leading observation is followed by the same category in roughly 90% of transitions; direct reversals between the two are rare, occurring in only 3 of 239 transitions, and the aligned category shows lower retention. The configurations are persistent over the observed decade; whether that persistence reflects institutional structure or simply the slow movement of the underlying indicators cannot be resolved here. The institutional complementarities account [17,18] does provide a reason: consumer adoption and SME e-sales participation depend partly on different foundations (consumer payment rails, delivery networks and digital skills on one side and access to finance, managerial capability, and sectoral composition on the other), and where those foundations developed unevenly, the relative position remains persistent over the observation window. That interaction effects across institutional spheres must be taken together to gauge the impact of change in any one sphere [18] maps directly onto the persistence observed here. The complementarities literature has rarely focused on digital commerce; the durability of these configurations suggests it should.

One qualification on the persistence figures is due, and it argues against reading them too triumphantly. The alignment gap is a standardised level, and levels are autocorrelated almost by definition: a country near the top of the EU distribution this year is very likely to be near the top next year, whether or not any structural mechanism holds it there. Some of the 90% year-to-year persistence is therefore mechanical, being the ordinary inertia of a slow-moving national indicator rather than evidence of institutional lock-in. The configurations are persistent over the observation window, although part of this persistence may reflect serial dependence on slow-moving national indicators together with the geometry of the classification thresholds. The results are consistent with persistent national differences but do not identify the mechanisms sustaining them.

The two studies that come nearest measured the same two sides but combined them. Roszko-Wójtowicz et al. [7,8] treat household purchasing and business online sales as alternative indicators of a single "e-commerce development" construct, summing or substituting them. That move is defensible if the two track one latent process—but our fixed-effects results do not show robust within-country co-movement, and our persistence results show the configurations remain persistent over the observation window. A composite that averages the two sides discards precisely the signal that turns out to matter: the sign and durability of the distance between them. The relational construct is not a refinement of the joint one. It measures something the joint one cannot see.

Against cross-sectional country screening [11], the longitudinal frame changes what can be claimed. A single-year portfolio can rank countries on execution conditions; it cannot tell whether a country's position is a fixed feature or a passing phase. Clustering the EU-27 on 2021 e-commerce indicators yields five groups shaped by education, poverty risk, and household internet access [37], creating a clean cross-sectional map but a snapshot

that is silent on whether a country's cluster membership is stable or fleeting. The transition matrix speaks to this factor: the two outer configurations show higher retention than the aligned category partly because of the geometry of the classification bands. The closest methodological relative in this journal, Păun et al.'s [12] two-way fixed-effects panel of e-commerce adoption in Central and Eastern Europe, shares our estimator and much of our window but keeps adoption itself as the outcome. The step taken here is to make the relation between two adoption processes the outcome, and it is that step that surfaces the divergence.

The result that most demands positioning is Cetulean et al.'s [13], because their design is so close to ours and their central number so similar: they found a small, fragile within-country association and strong within-country persistence on an EU-27 Eurostat panel across nearly the same years. The similarity between the estimates is consistent with both studies finding weak short-run within-country relationships in closely related EU panels. The present analysis adds a relational measure, a country-year typology, and evidence of the persistence of relative configurations; it does not independently validate the mechanisms underlying either study. What the present design adds is a description of how the two sides are positioned relative to one another, how persistent those positions are over the observed decade, and that their annual cross-country correlation edged downward rather than upward, although the trend is imprecisely estimated.

Their finding also affects measurement practice. The findings caution against treating consumer adoption as a substitute for SME e-sales participation in assessments of national e-commerce development. Where the two are combined into a single measure, a state whose consumers have converged on the EU frontier while its firms have not will score higher than its enterprise side alone would indicate. The two dimensions should be reported separately.

Three caveats bound the reading. The within-country coefficients are positive, not zero, and marginal rather than decisively null; with 27 clusters, the finite-sample properties of the robust covariance estimator are uncertain, so a modest true association could sit inside these intervals, though the wild cluster bootstrap and CR2 corrections reported in Section 4.3 and Table A2 confirm that the association remains small and imprecise under small-sample inference. Ceiling effects may attenuate the association where consumer adoption already approaches saturation, which describes the Nordic and Benelux states by the end of the window. Moreover, the alignment measure is relative, so category membership shifts with the whole distribution, not only with a country's own behaviour (a property disclosed with every typology result and not to be overread as movement).

6. Practical and Policy Implications

The policy inference follows from the persistence result, and it is a modest one. Because the contemporaneous within-country association is positive but imprecisely estimated, while the relative configurations are persistent and the imbalanced configurations are stable, strong consumer adoption should not be treated as evidence that SME e-sales participation will improve automatically. Policy monitoring should therefore report the consumer and enterprise dimensions separately rather than combining them into a single e-commerce development score, and progress on one should not be read as progress on the other. What this design cannot say is where the constraint lies: nothing here measures firm capability, access to finance, logistics or skills, so the results support separate measurement rather than any particular instrument or allocation of funds.

None of this speaks to where a firm should expand or which market to enter. The design is domestic and observational, and the implications stay within domestic commercialisation policy.

7. Limitations

The evidence is aggregate and observational, which bounds every claim. Country-level associations do not license inferences about individual consumers or firms [21]; no estimate here identifies a causal effect; and the mechanisms behind the measured asymmetry sit outside the design, recoverable only with linked firm- and consumer-level data.

Measurement imposes the sharper limits. The two series have different populations and denominators (with individuals aged 16–74 on one side and enterprises with 10–249 persons employed on the other), so their relationship is one of relative standing, and no quantity here estimates unmet demand, market failure, or lost sales. Consumer adoption counts purchases from any seller anywhere, so a country can post high adoption alongside low domestic SME participation with no inconsistency: its consumers may be served by large domestic retailers or by sellers abroad. Restricting consumer adoption to purchases from national sellers would address this directly, but the retrieved Eurostat seller origin breakdown does not supply a national-seller rate on the all-individuals base required, so no such measure could be constructed. Future research using harmonised seller-origin data could distinguish purchases served by domestic and by foreign sellers, which is the single change that would most sharpen the construct. The channel-specific indicators available in the Eurostat series, covering sales through an own website and through a marketplace, do not provide an independent validation of the alignment gap: they are closely related to its SME participation component and are not conditioned on enterprises that already sell online. A defensible channel measure would need that conditioning, which the retrieved series do not supply.

Microenterprises, being the majority of European firms by count, are excluded from the enterprise survey entirely, and the 1% turnover threshold treats a firm with marginal online sales and one selling predominantly online as equivalent.

Both surveys changed during the window—the enterprise instrument under Regulation (EU) 2019/2152 [24] from reference year 2021 and the household instrument under Regulation (EU) 2019/1700 [26]—and the household series is spliced from two datasets at 2020. Breaks are flagged, retained and testable through the source–period interaction, but a spliced series remains a spliced series. The analysis begins in 2015 because the enterprise e-sales series aligned to economic reference year is not available for 2014 in the extracted dataset; recovering that year would require the survey-year 2015 enterprise vintage.

Two properties of the alignment measure warrant restating. It carries no information about EU-wide trends, since within-year standardisation removes the common mean, so all level trend statements rest on the raw series. Moreover, its dispersion is, by construction, an exact function of the annual correlation between its components, so the RQ4 dispersion results speak to cross-country co-alignment, not to convergence in levels, which is tested separately and directly on the raw series.

A dynamic specification with a lagged dependent variable would let short-run adjustment be separated from the long-run relationship. It is not estimated here: with ten years and 27 units, difference- or system-GMM estimators sit in the region where Nickell bias and weak, proliferating instruments trade off against each other with no comfortable resolution, and the estimates would be less trustworthy than the static within-country results they were meant to refine. The panel is too short for dynamic panel methods to add more than they cost. Inference at 27 clusters is likewise uncertain in finite samples: cluster-robust standard errors can over-reject at this cluster count [30], which is why the CR2 correction and the wild cluster bootstrap-t are reported alongside them in Section 4.3. Both confirm the primary inference that the marginal p-values remain non-significant, so the finding is not an artefact of the standard-error estimator, though a modest true association could still sit within the confidence intervals. Linked microdata would recover the mechanisms;

category-level or expenditure data would move the consumer measure closer to volume; parcel-level and payment indicators would sharpen the firm measure. Each is a direction that the present public data design cannot reach.

8. Conclusions

Across the EU-27 from 2015 to 2024, household online purchasing and SME e-sales participation followed different longitudinal patterns. Cross-country dispersion in consumer adoption fell sharply, while SME participation dispersion showed only a modest, statistically uncertain upward drift (RQ1, RQ4). The within-country association was positive but imprecisely estimated in the level specification, and the short-run year-to-year association likewise fell short of conventional significance in first differences (RQ2). The distinction matters: between countries, the two sides are moderately associated in the pooled data, because both track national digital and economic development; within countries, over time, the association is weak and imprecisely estimated. The pooled correlation of 0.55 is therefore a statement about which countries sit where and not about one side pulling the other along. The consumer–SME alignment configurations were persistent over the observation window. SME-leading and consumer-leading observations retained their category in about 90% of year-to-year transitions; reversals between the extremes were rare, in 3 of 239 transitions; and the aligned category showed lower retention than the two outer categories, although this difference was partly shaped by the geometry of the classification bands (RQ3). The annual cross-country correlation between the two indicators edged down from 0.54 in 2015 to 0.42 in 2024, although the corresponding trend was not statistically distinguishable from zero under the country-clustered bootstrap (RQ4). Consumer adoption is therefore an incomplete measure of national e-commerce development, and the two sides warrant separate monitoring rather than the assumption that progress on one carries the other. Over the decade, member states became markedly more alike in consumer online shopping adoption, while there is no comparable evidence of convergence in SME e-sales participation.

Supplementary Materials: The following supporting information can be downloaded at https://www.mdpi.com/article/10.3390/jtaer21090321/s1, A minimal dataset is deposited with this article, containing the three Eurostat source spreadsheets as downloaded, the harmonised country-year panel in SPSS format, the missingness audit, and a readme documenting variable construction and the survey year to reference year alignment. The deposit also includes the original SPSS output files for the main analyses, the two Eurostat weighting series, the weighted panel, and the R scripts for the small-sample and weighted analyses.

Funding: This research received no external funding.

Institutional Review Board Statement: Not applicable.

Informed Consent Statement: Not applicable.

Data Availability Statement: All data underlying this study are publicly available from Eurostat. Table 1 gives the dataset codes, indicators, reference populations and extraction dates; the indicator codes, units, enterprise size class and sectoral selection needed to repeat the extraction are recorded with the panel. A minimal dataset is deposited with this article. It contains the three Eurostat spreadsheets exactly as downloaded, the harmonised country-year panel as an SPSS data file with all derived variables and value labels, a completeness audit reporting every series by country and by year with the Eurostat quality flags, and a readme documenting the sheet used for each series, the temporal alignment, the construction of every derived variable, and the mapping from variables to the reported tables and figures. The source datasets were downloaded from the Eurostat data browser on 5 August 2026: isoc_ec_ibuy (https://ec.europa.eu/eurostat/databrowser/view/isoc_ec_ibuy/default/table, accessed on 5 August 2026) and isoc_ec_ib20 (https://ec.europa.eu/

eurostat/databrowser/view/isoc_ec_ib20/default/table, accessed on 5 August 2026) for consumer online purchasing and isoc_ec_esels (https://ec.europa.eu/eurostat/databrowser/view/isoc_ec_esels/default/table, accessed on 5 August 2026) for enterprise e-commerce sales. The complete reproducibility package—the Eurostat source spreadsheets, the harmonised panel, the weighting series, the original SPSS output files, and the R scripts (including those for the small-sample and bootstrap inference, the weighting, the reverse-model and symmetric-correlation checks, the Pesaran CD test, the gap-validation, the flag sensitivity, and the persistence benchmark)—is archived at https://doi.org/10.5281/zenodo.22647855. Where resampling is used, fixed random seeds are specified in the deposited R scripts. The reproducibility package includes the original Eurostat source files, the harmonised panel, the weighting data, the original SPSS output files for the main analyses, and executable R scripts for the additional analyses introduced during revision.

Acknowledgments: During the preparation of this manuscript, the author used Claude Opus 4.8 (Anthropic) for language editing, for assistance with the organization of the SPSS dataset and derived variables, for generating the plotting code used to render Figures 1 and 2 from author results, and for assistance in implementing in R the small-sample and weighting procedures reported in the paper. ChatGPT (GPT-5.6 Sol, OpenAI) was additionally used for language editing. All study design decisions, the choice of analyses, statistical analyses in IBM SPSS Statistics 27 and R version 4.6.1, interpretation of results, verification of outputs, and final manuscript revisions were performed by the author, who takes full responsibility for the content of the publication.

Conflicts of Interest: The author declares no conflict of interest.

Appendix A

Coefficient estimates for the main and robustness specifications, with the estimator details needed to read them, are reported in Table A1 in Appendix A, which also contains the small-cluster in-ference (Table A2), the population- and enterprise-weighted sensitivity (Table A3), and the ro-bustness of the persistence result (Table A4).

Table A1. Coefficient estimates, standard errors, confidence intervals, *p*-values and sample sizes for the main and robustness specifications.

Model	Specification	*N*	Coefficient	SE	95% CI	*p*	Reported in
M1	Baseline two-way fixed effects (country + year), cluster-robust	268	0.116	0.073	[−0.027, 0.259]	0.112	Table 4
M2	First-difference sensitivity + year FE, cluster-robust	239	0.091	0.047	[−0.002, 0.184]	0.055	Table 4
R1	Alternative adoption definition (3-month window)	268	0.048	0.089	[−0.127, 0.222]	0.593	Table 7
R2	Pandemic years 2020–2021 excluded	215	0.112	0.077	[−0.039, 0.263]	0.147	Table 7
R3	Country-specific linear trends added	268	0.126	0.074	[−0.019, 0.271]	0.09	Table 7
R4	Logit-transformed shares (functional form)	268	0.177	0.112	[−0.043, 0.397]	0.114	Table 7
R5	Source–period interaction (pre-2020 vs. 2020+)	268	0.031	0.042	[−0.051, 0.114]	0.453	Table 7/Section 4.6
R6	Germany excluded	258	0.081	0.076	[−0.069, 0.23]	0.29	Table 7
R7	Romania excluded	258	0.141	0.078	[−0.011, 0.294]	0.068	Table 7
R8	Denmark excluded	258	0.133	0.073	[−0.01, 0.277]	0.069	Table 7
D1	OLS trend, annual SD of consumer adoption on year	10	−0.91	0.064	—	<0.001	Table 3
D2	OLS trend, annual SD of SME participation on year	10	0.131	0.049	—	0.027	Table 3

Table A1. *Cont.*

Model	Specification	*N*	Coefficient	SE	95% CI	*p*	Reported in
D3	OLS trend, annual SD of alignment gap on year	10	0.016	0.005	—	0.009	Table 3
D4	OLS trend, annual correlation on year	10	−0.016	0.005	—	0.009	Table 3/Figure 1c

Note: M denotes the main models, R the robustness checks, and D the dispersion trends. Coefficients are expressed in percentage points unless noted and give the estimated within-country association between consumer adoption and SME e-sales participation; they are not interpreted as causal effects. For the M and R models, the estimator is generalised estimating equations with a normal distribution, identity link, independent working correlation and robust sandwich covariance with country as the subject variable, which yields country-clustered robust standard errors; all level specifications include country and year fixed effects, while M2 includes year fixed effects only, with time-invariant country effects being removed by differencing. This generalised-estimating-equations implementation with country and year indicators yields the same baseline point estimate as the conventional two-way fixed-effects (within) estimator—both give a baseline coefficient of 0.116. With 27 country clusters, conventional robust covariance estimates may give imperfect finite-sample inference, and Wald tests should be read cautiously; the CR2 correction and the wild cluster bootstrap-t reported in Table A2 confirm the results under small-sample inference. The R4 coefficient is on the logit scale and is not directly comparable in magnitude with the percentage-point estimates, R4 being a functional-form sensitivity check rather than a fractional-response model. R5 reports the coefficient on the interaction between consumer adoption and the household source–period indicator, with the 2020-onwards series as the reference category; the main effect is collinear with the year fixed effects and is therefore not estimated separately. R6 to R8 exclude one member state at a time and are selected-country exclusion checks on three contrasting member states, not the full leave-one-country-out series over all 27 members. The D models are ordinary least squares regressions on ten annual values, one per year, country-clustered bootstrap confidence intervals for these trends are reported in Table A3. Source: own processing.

Table A2. Small-cluster inference for the within-country association.

Specification	Coefficient	CR1 *p*	CR2 *p* (df = 26)	Wild Bootstrap-t *p*
M1 baseline (levels)	0.116	0.148	0.145	0.218
95% CI			[−0.043, 0.275]	[−0.085, 0.319]
M2 first differences	0.091	0.066	0.078	0.097
95% CI			[−0.011, 0.193]	[−0.019, 0.201]
Leave out Romania	0.141	0.068	—	0.161
Leave out Denmark	0.133	0.069	—	0.170

The two-way fixed-effects estimator has 27 country clusters. CR1 is the conventional cluster-robust *p*-value; CR2 is the bias-reduced variance with a t(26) reference distribution; the wild cluster bootstrap-t imposes the null, uses Rademacher weights and B = 9999 replications. Confidence intervals for M1 and M2 are the CR2 and bootstrap intervals, respectively. Under either small-cluster correction, the association remains small and does not reach conventional significance; the first-difference *p*-value rises from 0.055 to 0.097. Two contrasting single-country exclusions that were closest to significance under CR1 are also non-significant under the bootstrap. Source: own processing.

Table A3. Trend inference and population- and enterprise-weighted sensitivity.

Quantity	Unweighted/Estimate	Population-Weighted	Enterprise-Weighted
Panel A. Weighted main models and trends			
M1 coefficient (levels)	0.116	0.248	0.260
p	0.148	0.002	<0.001
M2 coefficient (first diff.)	0.091	0.068	0.075
p	0.066	0.203	0.105
D1 SD-of-consumer trend	−0.910	−1.059	−1.100
D4 correlation trend	−0.016	−0.038	−0.036

Table A3. *Cont.*

Quantity	Unweighted/Estimate	Population-Weighted	Enterprise-Weighted
Panel B. Country-clustered bootstrap inference for the unweighted trends (95% CI)			
Consumer adoption SD trend	[−1.25, −0.51]		
SME participation SD trend	[−0.06, 0.33]		
Correlation trend (ϱ)	[−0.049, 0.013]		
Correlation trend, leave-one-year-out	−0.024 to −0.020		

Note: Panel A gives the unweighted estimates alongside population- and enterprise-weighted versions; each member state is one observation in the unweighted specification. Population weights use Eurostat population aged 15–74 (dataset demo_pjangroup); enterprise weights use the count of enterprises with 10–249 persons employed (dataset sbs_sc_ovw), averaged over 2021–2024 because the size-class series is discontinuous in 2021 under Regulation (EU) 2019/2152 [24]. Both weighting series were extracted on 2 September 2026. Panel B reports 95% confidence intervals from a country-clustered bootstrap that resamples countries with replacement and re-estimates each annual statistic (5000 replications); only the consumer adoption dispersion trend excludes zero. Source: own processing.

Table A4. Robustness of the persistence result.

Panel A. Continuous Persistence Measures	
Continuous Persistence Measure	**Value**
Within-country lag-1 autocorrelation of the gap	0.462 (median 0.465)
Between-country share of gap variance (ICC)	0.848
Year-to-year rank persistence (Spearman)	0.930
Cohen's κ, ±0.25 SD band (95% CI)	0.743 [0.600, 0.826]
Category retention, ±0.25 SD band (95% CI)	0.837 [0.751, 0.895]

Panel B. Sensitivity of Retention and κ to the Tolerance Band		
Tolerance Band (SD)	**Retention**	**Cohen's κ**
0.10	0.820	0.696
0.20	0.820	0.712
0.25	0.837	0.743
0.30	0.833	0.743
0.40	0.828	0.742
0.50	0.803	0.703
0.75	0.820	0.680
1.00	0.866	0.713

Panel A reports three threshold-free persistence measures—the lag-1 autocorrelation, the between-country variance share, and year-to-year rank persistence of the alignment gap, together with Cohen's κ and overall category retention for the baseline ±0.25 SD band and the latter two with 95% country-clustered bootstrap intervals (3000 replications). Panel B varies the band that defines the three configurations from 0.10 to 1.00 SD; κ stays between 0.68 and 0.74 throughout, so the persistence result does not depend on the ±0.25 SD threshold. Source: own processing.

References

1. Cruz-Jesus, F.; Oliveira, T.; Bacao, F. Digital Divide across the European Union. *Inf. Manag.* **2012**, *49*, 278–291. [CrossRef]
2. Sadowski, A.; Lewandowska-Gwarda, K.; Pisarek-Bartoszewska, R.; Engelseth, P. A Longitudinal Study of E-Commerce Diversity in Europe. *Electron. Commer. Res.* **2021**, *21*, 169–194. [CrossRef]
3. Hunady, J.; Pisár, P.; Vugec, D.S.; Bach, M.P. Digital Transformation in European Union: North Is Leading, and South Is Lagging Behind. *Int. J. Inf. Syst. Proj. Manag.* **2022**, *10*, 39–56. [CrossRef]
4. Lucero Ortiz, A.; Rodríguez, J.C.; Gómez, M. E-Commerce Development in Europe: A Panel Data Analysis 2003–2017. *E+M Ekon. Manag.* **2020**, *23*, 89–101. [CrossRef]
5. Bădîrcea, R.M.; Manta, A.G.; Florea, N.M.; Popescu, J.; Manta, F.L.; Puiu, S. E-Commerce and the Factors Affecting Its Development in the Age of Digital Technology: Empirical Evidence at EU–27 Level. *Sustainability* **2022**, *14*, 101. [CrossRef]

6. Bucevska, V.; Bucevska, J. E-Commerce and Factors Affecting Its Development: Empirical Evidence from Europe. *Ekon. Pregl.* **2025**, *76*, 210–224. [CrossRef]
7. Roszko-Wójtowicz, E.; Sharma, G.D.; Dańska-Borsiak, B.; Grzelak, M.M. Innovation-Driven E-Commerce Growth in the EU: An Empirical Study of the Propensity for Online Purchases and Sustainable Consumption. *Sustainability* **2024**, *16*, 1563. [CrossRef]
8. Roszko-Wójtowicz, E.; Deep Sharma, G.; Dańska-Borsiak, B.; Grzelak, M.M. Social Capital as a Driver of E-Commerce Development: Evidence from European Panel Data Analysis. *Equilib. Q. J. Econ. Econ. Policy* **2025**, *20*, 681–712. [CrossRef]
9. Ho, S.-C.; Kauffman, R.J.; Liang, T.-P. A Growth Theory Perspective on B2C E-Commerce Growth in Europe: An Exploratory Study. *Electron. Commer. Res. Appl.* **2007**, *6*, 237–259. [CrossRef]
10. Ahluwalia, P.; Merhi, M.I. Understanding Country Level Adoption of E-Commerce: A Theoretical Model Including Technological, Institutional, and Cultural Factors. *J. Glob. Inf. Manag.* **2020**, *28*, 1–22. [CrossRef]
11. Bresfelean, V.P.; Comes, C.-A.; Pop-Nistor, P. A Portfolio-First Public-Data Framework for EU-27 Cross-Border E-Commerce Market-Entry Screening. *J. Theor. Appl. Electron. Commer. Res.* **2026**, *21*, 239. [CrossRef]
12. Păun, C.A.; Păun, N.; Păun, D. Digital Payment Infrastructure and E-Commerce Adoption in Central and Eastern Europe: A Panel Data Analysis. *J. Theor. Appl. Electron. Commer. Res.* **2026**, *21*, 152. [CrossRef]
13. Cetulean, M.; Bodislav, D.A.; Georgescu, R.I.; Moroianu, N.; Popa, R.A.; Papuc, C.M. When Does Digital Maturity Become a Systemic Advantage? Modelling E-Commerce Behaviour and Competitiveness in Europe. *Systems* **2026**, *14*, 118. [CrossRef]
14. Bucklin, L.P.; Sengupta, S. The Co-Diffusion of Complementary Innovations: Supermarket Scanners and UPC Symbols. *J. Prod. Innov. Manag.* **1993**, *10*, 148–160. [CrossRef]
15. Smith, M.Y. A Model of Linked Adoption of Complementary Technologies. *Econ. Innov. New Technol.* **2004**, *13*, 91–99. [CrossRef]
16. Rogers, E.M. *Diffusion of Innovations*, 5th ed.; Free Press: New York, NY, USA, 2003.
17. Hall, P.A.; Soskice, D. *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*; Oxford University Press: Oxford, UK, 2001.
18. Hall, P.A.; Gingerich, D.W. Varieties of Capitalism and Institutional Complementarities in the Political Economy: An Empirical Analysis. *Br. J. Political Sci.* **2009**, *39*, 449–482. [CrossRef]
19. Paun, D.; Paun, C.A.; Paun, N. The Digitalization–Performance Nexus in the European Union: A Country-Level Analysis of Heterogeneity and Complementarities. *J. Theor. Appl. Electron. Commer. Res.* **2025**, *20*, 274. [CrossRef]
20. May, C.; Schedelik, M. Comparative Capitalism and Innovation Policy: Complementarities and Comparative Institutional Advantage. *J. Econ. Policy Reform* **2021**, *24*, 456–471. [CrossRef]
21. Robinson, W.S. Ecological Correlations and the Behavior of Individuals. *Am. Sociol. Rev.* **1950**, *15*, 351–357. [CrossRef]
22. Eurostat. ICT Usage in Households and by Individuals (isoc_i)—Reference Metadata (ESMS). Available online: https://ec.europa.eu/eurostat/cache/metadata/en/isoc_i_esms.htm (accessed on 5 August 2026).
23. Eurostat. ICT Usage in Enterprises (isoc_e)—Reference Metadata (ESMS). Available online: https://ec.europa.eu/eurostat/cache/metadata/en/isoc_e_esms.htm (accessed on 5 August 2026).
24. European Parliament; Council of the European Union. Regulation (EU) 2019/2152 of 27 November 2019 on European Business Statistics. *Off. J. Eur. Union* **2019**, *L327*, 1–35.
25. European Commission. Commission Implementing Regulation (EU) 2020/1197 of 30 July 2020 Laying Down Technical Specifications and Arrangements Pursuant to Regulation (EU) 2019/2152. *Off. J. Eur. Union* **2020**, *L271*, 1–170.
26. European Parliament; Council of the European Union. Regulation (EU) 2019/1700 of 10 October 2019 Establishing a Common Framework for European Statistics Relating to Persons and Households. *Off. J. Eur. Union* **2019**, *L261*, 1–32.
27. Quah, D. Galton's Fallacy and Tests of the Convergence Hypothesis. *Scand. J. Econ.* **1993**, *95*, 427–443. [CrossRef]
28. Friedman, M. Do Old Fallacies Ever Die? *J. Econ. Lit.* **1992**, *30*, 2129–2132.
29. Sala-i-Martin, X. The Classical Approach to Convergence Analysis. *Econ. J.* **1996**, *106*, 1019–1036. [CrossRef]
30. Cameron, A.C.; Gelbach, J.B.; Miller, D.L. Bootstrap-Based Improvements for Inference with Clustered Errors. *Rev. Econ. Stat.* **2008**, *90*, 414–427. [CrossRef]
31. Hoechle, D. Robust Standard Errors for Panel Regressions with Cross-Sectional Dependence. *Stata J.* **2007**, *7*, 281–312. [CrossRef]
32. Škare, M.; de Obesso, M.M.; Ribeiro-Navarrete, S. Digital Transformation and European Small and Medium Enterprises (SMEs): A Comparative Study Using Digital Economy and Society Index Data. *Int. J. Inf. Manag.* **2023**, *68*, 102594. [CrossRef]
33. Stângaciu, A.; Țimiraș, L.; Zaiț, L.; Nichifor, B.; Danu, M.; Harja, E. The EU E-Commerce Market in a Pandemic Context—Linking Demographic Factors and Territorial Convergence. *J. Bus. Econ. Manag.* **2024**, *25*, 21–46. [CrossRef]
34. Aumeboonsuke, V. Digital Convergence and Divergence in EU and ASEAN Economies. *Economics* **2025**, *13*, 153–177. [CrossRef]
35. Bruno, G.; Diglio, A.; Piccolo, C.; Pipicelli, E. A Reduced Composite Indicator for Digital Divide Measurement at the Regional Level: An Application to the Digital Economy and Society Index (DESI). *Technol. Forecast. Soc. Change* **2023**, *190*, 122461. [CrossRef]

36. Papke, L.E.; Wooldridge, J.M. Econometric Methods for Fractional Response Variables with an Application to 401(k) Plan Participation Rates. *J. Appl. Econom.* **1996**, *11*, 619–632. [CrossRef]
37. Gogonea, R.M.; Moraru, L.C.; Bodislav, D.A.; Păunescu, L.M.; Vlăsceanu, C.F. Similarities and Disparities of e-Commerce in the European Union in the Post-Pandemic Period. *J. Theor. Appl. Electron. Commer. Res.* **2024**, *19*, 340–361. [CrossRef]