“Digital skills composition and adult learning: Human capital in the knowledge economy”

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DIGITAL SKILLS COMPOSITION AND ADULT LEARNING: HUMAN CAPITAL IN THE KNOWLEDGE ECONOMY

Abstract

Digital skills anchor human-capital policy in the European knowledge economy, yet which dimension of digitalization moves lifelong learning remains unsettled, as connectivity, specialist, and training targets are pursued in parallel. The paper aims to quantify the association between the workforce's digital-skills composition and adult participation in learning across 31 European economies, using an unbalanced annual 2015–2023 panel supplemented by DigComp 2.0 waves for 2021–2025, and to draw implications for human-capital policy. Both are estimated with two-way fixed-effects models, lagged regressors, country-clustered standard errors, and an audit of survey redesigns. The lagged employment share of ICT specialists is positively associated with adult learning: a one-percentage-point increase corresponds to a 1.38-point gain ($\beta = 1.379$, $p = 0.034$; wild-cluster bootstrap $p = 0.077$). Diffusion-type measures – daily internet use and online health-information seeking – show no such link ($\beta = 0.021$ and 0.025), although the specialist coefficient is not statistically distinguishable from theirs. The coefficient stays positive across estimators and samples (0.44–1.76) and significant in the pre-break 2015–2020 window ($\beta = 1.536$, $p = 0.024$); a formal test does not reject constancy across the 2021 EU-LFS redesign. Direct skills measures point the same way but stay insignificant ($\beta = 0.042$–0.121); unemployment is counter-cyclical ($\beta = 0.583$–0.694). The evidence is consistent with rebalancing policy from connectivity targets towards workforce skills composition – a margin unmeasured in Ukraine, Kazakhstan, and Armenia – with specialist pipelines as the instrument most directly supported and enterprise ICT training as a possible, not yet established, channel.

Keywords human capital, knowledge economy, digital skills, lifelong learning, adult learning, ICT specialists, panel data

JEL Classification C23, I28, J24, O33

INTRODUCTION

In the European knowledge economy, digital transformation has moved skills to the center of policy. The Digital Decade policy program commits the EU to two headline human-capital targets for 2030: at least 80% of adults with at least basic digital skills and 20 million employed ICT specialists (European Commission, 2024; Eurostat, 2024). Actual figures remain far from both benchmarks. In 2023, only 55.6% of EU citizens aged 16-74 possessed at least basic digital skills, compared with 53.9% in 2021, and the European Commission considers the current pace of progress insufficient to achieve the 2030 target (European Commission, 2024). The specialist side of the gap is equally stark: in 2024 the EU employed 10.3 million ICT specialists, accounting for approximately 5% of total employment and representing only slightly more than one-half of the 2030 target (Eurostat, 2024; Council of the European Union, 2025). Because digital skills increasingly condition employability, productivity, and access to services, these shortfalls translate directly into risks for the accumulation and effective utilization of human capital in the digital economy.

The skills targets are mirrored on the learning side. Under the European Pillar of Social Rights Action Plan, endorsed at the 2021 Porto Social Summit, at least 60% of adults should participate in training every year by 2030 (European Commission, 2021; Council of the European Union, 2022). The Adult Education Survey indicates that 46.6% of adults aged 25-64 participated in education and training during the previous twelve months in 2022 (Eurostat, 2025). The shorter-term indicator used in this paper – participation during the last four weeks – averages 11.4% across the economies studied here, and the two measures are not interchangeable: the four-week series tracks the frequency of learning episodes rather than annual coverage. The policy response has therefore concentrated on financing and entitlements, ranging from individual learning accounts to national skills strategies (Council of the European Union, 2022). What remains empirically unsettled is whether adult participation in lifelong learning is more strongly associated with digital diffusion and connectivity or with the digital composition of the workforce itself.

What has been missing is a design that separates these dimensions of digitalization from one another, accounts for the 2021 change in the measurement framework of the EU Labour Force Survey, and exploits the newly available DigComp-based indicators of digital skills. The mechanism is organizational – specialists diffuse knowledge to co-workers and raise firms' absorptive capacity – though this is argued here rather than tested. This paper addresses these gaps for 31 European economies. It separates composition-type digitalization (the share of ICT specialists in employment) from diffusion-type digitalization (daily internet use and online health-information seeking, which capture how widely digital tools are used rather than whether infrastructure is available), finding the specialist share to be the only one of these dimensions individually associated with adult participation in lifelong learning. It audits the 2021 redesign and series breaks, verifying results on the pre-2021 subsample, and adds DigComp-aligned indicators.

1. LITERATURE REVIEW

Preserving and renewing human capital has become a central concern of knowledge-economy policy in Europe, and a substantial body of work treats human capital as a foundational driver of growth that must continually adapt to sectoral and technological change (Vakhovych et al., 2015; Ladonko et al., 2022). In this literature, digitalization is cast as a macro-level catalyst whose returns depend on institutional maturity and national economic capacity, shaping outcomes from public-sector efficiency to energy equity (Valaskova et al., 2025; Kirichok et al., 2025). A parallel strand frames education and intellectual-capital preservation as anti-crisis instruments, most visibly in Ukraine, where migration and instability deplete the national stock of skills and generate structural labor-market imbalances (Yeremenko, 2026; Zahorodnia et al., 2026b). Yet the same accounts caution that headline digital metrics do not translate mechanically into labor-market gains, since even sophisticated indices of technological vibrancy leave short-run employment structures largely unmoved (Kuzior et al., 2025). This ambiguity motivates a closer look at which specific dimension of digitalization actually bears on skills formation.

A second line of literature turns from the macro landscape to the workforce composition, arguing that digital transformation compels a fundamental rethinking of skills, organizational structure, and the intensity of employees' intellectual activity (Bashynska et al., 2023). Service-sector evidence from outside Europe similarly shows that deliberate adjustment of workforce skills, rather than technology adoption as such, predicts firms' dynamic capability in the post-pandemic work environment (Zayed et al., 2022). Firm-level evidence consistently identifies digital culture, leadership, and organizational readiness as proximate drivers of performance in digitalizing sectors, from insurance to the ICT industry itself (Al-Smadi, 2025; Grigoryan et al., 2025). Complementary studies show that the payoff to technological adoption is mediated by digital-service quality and employee engagement rather than by hardware as such, and that gains in internal efficiency need not carry over to client-facing outcomes (Nafei et al., 2025; Hernik et al., 2025); indeed, poorly balanced transformation can even depress the customer experience (H. Saifi & F. Saifi, 2025). The same emphasis surfaces in analyses of banking-ecosystem digitalization and knowledge-based absorptive capacity, where the accent falls on the human and organizational complement to technology (Abbasova et al., 2025). Work on enterprise digitalization and financial constraints adds that specialist-intensive transformation is uneven across firms and regions, from Ukraine and Kazakhstan to Thai SMEs (Zakharkina et al., 2025; Sartamorn et al., 2025), while cautions about meritocratic HR systems warn that composition effects can reproduce inequality if left unmonitored (Ly & Mujtaba, 2025). Sector-level workforce analyses make the point concretely: the age and gender composition of employment, and the wage differentials attached to it, vary systematically across branches of activity and become visible only where such structures are measured directly (Miniailo et al., 2023). Universities, in turn, are increasingly tasked with closing the resulting innovation-skills gap through digital learning (Prokopenko et al., 2024a). These accounts also point to a mechanism through which a denser stock of ICT specialists may raise adult learning more broadly: specialists diffuse tacit and codified knowledge to co-workers, strengthen the absorptive capacity and digital culture of the organizations that employ them, and create technological complementarities that make continued training worthwhile, so that specialist density plausibly operates through workplace knowledge spillovers and peer effects rather than through individual up-skilling alone.

If composition matters, so does the distinction between access to technology and the capacity to use it. A recurrent finding is that a digital divide persists across European, Western-Balkan, and other regions and was sharpened by the pandemic, exposing deficits in both infrastructure and capacity for remote engagement, and that the returns to digital infrastructure are themselves distributed asymmetrically across the income distribution (Kovac et al., 2024; Tutar et al., 2025). Crucially, this literature stresses that hardware access does not equate to proficiency, so that credible measurement of digital capacity requires multidimensional indices that separate connectivity from specialized skill (Kolupaieva & Tiesheva, 2023). The same asymmetry operates inside firms: the migration of financial reporting to digital formats such as XBRL and SAF-T turns analysis and audit into competence-intensive tasks that connectivity alone does not equip organizations to perform (Shygun et al., 2023). Indicator-level analysis of the DESI 2024 data makes the same point for the EU, reporting that member states differ markedly in digital skills and in network infrastructure, and that the two dimensions need to be developed in tandem rather than treated as substitutes (Obelovska et al., 2025). Applied studies of educational technology reinforce the point, showing that tools such as BIM, virtual reality, and AI-assisted instruction modernize competencies only where they are matched by pedagogical and organizational change (Volkotrubova & Yu, 2026; Omelyanenko et al., 2020). Related work on digital entrepreneurial intention and AI-supported teacher education extends the argument to the demand side of skills formation (Maulid Adha et al., 2026; Prokopenko et al., 2024b). Taken together, these strands imply that the skills composition of the workforce, not the spread of connectivity, is the more informative object of study.

A fourth strand of the literature addresses the learning side directly. Retraining and lifelong learning are presented as vital instruments for preserving human resources during crises, most acutely in Ukraine, where wartime disruption threatens the national stock of skills, and for aligning provision with the shifting expectations of younger cohorts (Yeremenko, 2026; Kozová et al., 2024). Dual-education models that combine study with workplace training are singled out as the mechanism most likely to raise qualifications and match them to employer needs (Davlikanova, 2025; Sigdel et al., 2026); the same logic has been applied to the public workforce, where digital competencies are written directly into civil-service recruitment and training frameworks (Sabdenov et al., 2025). However, evidence is not uniformly optimistic: formal training reliably improves access to employment but does not always translate into broader social protection, and its quality is frequently compromised where quality-management standards are weak (Aid et al., 2024; Zahorodnia et al., 2026a). Studies of active labor-market policy and financial-capability programs reach a similar verdict, treating training as effective but contin-

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gent on design and context (Jarząbek & Stolarska-Szeląg, 2024; Rawat & Joshi, 2025). Where continuity was tested most severely, during the pandemic, the rapid shift to e-learning preserved instruction but exposed the limits of remote delivery for practice-based competencies (Sułkowski et al., 2024; Śliwa et al., 2021), and even cultural and heritage settings are now being reframed as sites of lifelong learning (Oe et al., 2026).

None of these mechanisms, however, operates uniformly across Europe. Human capital converges across the continent's regions, but slowly and unevenly, with persistent asymmetries between Western and Central-Eastern European regions and part of the gap traced to the education-migration-labor-market chain that shapes sectoral and occupational composition (Dańska-Borsiak, 2023; Mukhtarova et al., 2024). Institutions appear to determine how much of any policy impulse survives these differences, with the competitiveness gains from EU Structural Funds and the labor-market effects of active policy both proving institutionally mediated (Blazhko et al., 2026; Knapińska & Woźniak-Jasińska, 2024). Regulatory alignment and compliance are themselves treated as channels through which European integration supports sustainable economic development and business growth, which places candidate economies on a different institutional trajectory from incumbent member states (Ivanenko & Muraviov, 2026). Comparative evidence on audit practice within the Union points the same way, documenting that shared European reporting requirements are implemented with markedly different national profiles (Bezverkhyi et al., 2025). Cluster analyses reinforce the point from another angle: the distance between advanced and transitional economies turns out to be as much institutional as material, and countries lagging on one human-capital margin may converge on another (Saher et al., 2025; Kyzenko et al., 2026). Evidence on productivity and firm performance, whether across European economies or within individual firms, likewise points to bounded, context-dependent returns rather than uniform ones (Lyeonov et al., 2025; Shafitranata et al., 2026). For the present analysis, such heterogeneity is not a nuisance to be assumed away: a country panel disciplined by measurement audits absorbs stable differences through fixed effects, and its cluster-based sub-samples ask directly whether the composition–learning association extends beyond the digital frontier.

Methodologically, the panel literature converges on two-way fixed- and random-effects frameworks, supplemented by robust or Driscoll-Kraay standard errors and, where dynamics matter, GMM estimators to test human-capital convergence (Kuzior et al., 2025; Dańska-Borsiak, 2023). A recognized limitation of within-country fixed effects is a loss of power when identification rests on temporal variation alone, and several authors combine such models with additional tools – from fuzzy-set evaluation to economic-mathematical optimization – to strengthen inference (Sitnicki et al., 2021; Bashynska et al., 2023). Against this backdrop, three gaps stand out. First, macro-level scholarship tends to treat digital access as a monolith, rarely separating the specialist composition of the workforce from mere connectivity when explaining adult learning. Second, cross-country comparisons seldom confront the measurement breaks introduced by survey redesigns, leaving time-series comparability in doubt. Third, the direct skills measures of the DigComp framework have only recently accumulated enough waves for panel use. The present study addresses these gaps by distinguishing composition-type from diffusion-type digitalization across 31 European economies, by auditing the 2021 and 2024 series breaks, and by bringing DigComp 2.0 skills waves into a robust panel design. In doing so, it locates the association between digital human capital and lifelong learning specifically in the participation flow rather than in the stock of attainment or in productivity, and shows that this association is shared, in direction, across digitally leading and lagging clusters.

The study aims to quantify the association between the digital-skills composition of the workforce and adult participation in lifelong learning across 31 European economies, using an annual 2015–2023 panel supplemented by DigComp 2.0 waves for 2021–2025, and to derive implications for human capital policy in the knowledge economy – including policy hypotheses for economies such as Ukraine, Kazakhstan, and Armenia, which lie outside the estimation sample and pursue knowledge-economy agendas without producing the specialist and adult-learning statistics this analysis relies on.

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2. METHOD

2.1. Data, sample, and variables

The empirical analysis combines three sources. Country-year indicators of human capital and digitalization are drawn from Eurostat: adult participation in learning (dataset trng_lfse_01; population aged 25-64, participation in education and training during the last four weeks), tertiary educational attainment of the 25-34 age group (edat_lfse_03), real labor productivity per person employed (nama_10_lp_ulc; index, 2015 = 100), the share of ICT specialists in total employment (isoc_sks_itspt), daily internet users (isoc_ci_ifp_fu), enterprises providing ICT training to their personnel (isoc_ske_ittn2), individuals seeking health information online (isoc_ci_ac_i), individuals interacting with public authorities online (isoc_ciegi_ac), the harmonised unemployment rate (une_rt_a), and individuals' overall digital skills under the DigComp 2.0 framework (isoc_sk_dskl_i21) – the shares with at least basic and with above-basic skills. Macroeconomic controls come from the World Bank World Development Indicators – GDP per capita in constant 2015 US dollars (NY.GDP.PCAP.KD), GDP growth, and inflation – and institutional controls from the Worldwide Governance Indicators (government effectiveness; regulatory quality is used in robustness checks). Table 1 summarizes definitions, roles, and sources. Adult participation in learning is the dependent variable because it is the only annual, cross-nationally harmonized learning measure available for the full panel. It is not, however, the indicator behind the 2030 European Pillar of Social Rights target: that target refers to participation over the previous twelve months, monitored through the Adult Education Survey and, since 2022, through biennial EU Labor Force Survey modules, whereas trng_lfse_01 records participation during the last four weeks and is reported by Eurostat as recent participation. The four-week series is used here for its annual frequency and complete panel coverage, so the estimates speak to the frequency of learning episodes rather than to

Table 1. Variable definitions and sources

Variable	Role	Definition	Source (dataset / code)
Adult participation in learning (AL)	Dependent (flow)	Population 25-64 in education/training, last 4 weeks, %	Eurostat trng_lfse_01
Tertiary attainment	Dependent (alt.)	Population 25-34 with ISCED 5-8, %	Eurostat edat_lfse_03
Labour productivity	Dependent (alt.)	Real labour productivity per person, index 2015 = 100 (ln)	Eurostat nama_10_lp_ulc
ICT specialists	Digital proxy (headline)	ICT specialists, % of total employment	Eurostat isoc_sks_itspt
Daily internet users	Digital proxy	Individuals using the internet daily, %	Eurostat isoc_ci_ifp_fu
Enterprise ICT training	Digital proxy	Enterprises (10+ empl., all sectors excl. financial) providing ICT training, %	Eurostat isoc_ske_ittn2
Health information online	Digital proxy	Individuals seeking health information online, %	Eurostat isoc_ci_ac_i (I_IHIF)
Digital skills: at least basic / above basic	Skills (Track B)	DigComp 2.0 overall digital skills, % of individuals	Eurostat isoc_sk_dskl_i21
Digital skills (former vintage)	Context only	Overall digital skills under the earlier (2015) measurement framework, % of individuals; 2015–2019 vintage, not pooled with the DigComp 2.0 series	Eurostat isoc_sk_dskl_i
GDP per capita	Control	Constant 2015 USD (ln in estimation)	WDI NY.GDP.PCAP.KD
Unemployment rate	Control	15-74, % of active population	Eurostat une_rt_a
Government effectiveness	Control	WGI estimate	World Bank WGI
Regulatory quality	Control (robustness)	WGI estimate	World Bank WGI
GDP growth; inflation	Descriptive only	Annual %, reported in Table 2; not in the estimated models	WDI NY.GDP.MKTP.KD.ZG; FP.CPI.TOTL.ZG
E-government use	Descriptive only	Individuals interacting with public authorities online, %; series ends 2021	Eurostat isoc_ciegi_ac (I_IUGOV12)

Note: All Eurostat series are extracted via the SDMX-CSV dissemination API in the harmonized country-year long format; country codes are mapped to ISO3. Expected signs: the digitalization proxies, log GDP per capita, and government effectiveness are expected to enter positively. The sign on unemployment is ambiguous a priori: weaker labor demand depresses training, while the falling opportunity cost of time in a downturn raises it.

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the 60% annual-participation target itself. Twelve-month measures are biennial or six-yearly and leave too few periods for a fixed-effects design; expenditure measures are not harmonized, and employer-provided training already enters as a digitalization proxy.

The estimation sample comprises 31 European economies: the EU-27, Norway, and three candidate economies (North Macedonia, Serbia, and Türkiye). A country enters the sample if it has at least seven of the nine years of complete data on the core variable set over 2015–2023; Iceland, Switzerland, Montenegro, Bosnia and Herzegovina, Albania, Kosovo, and the United Kingdom do not meet this criterion. Two estimation windows are used. Track A is an annual, unbalanced panel over 2015–2023 with up to 279 country-year observations; imbalance reflects the biennial enterprise ICT survey and occasional single-year gaps in individual series. The window closes in 2023 for two reasons fixed prior to estimation: the Worldwide Governance Indicators end in 2023, and six of the 31 countries, including Belgium, Germany, and France, carry officially flagged breaks in the dependent-variable series in 2024. Track B covers the three available DigComp 2.0 skills waves (2021, 2023, and 2025; 92-93 observations, as North Macedonia lacks the 2023 wave). The frame is set by data availability rather than by geography: the Eurostat ICT and labor-force instruments supplying the core variables do not cover Ukraine, Kazakhstan, or Armenia, so these economies cannot enter the estimation sample even though the policy questions motivating the analysis apply to them directly.

2.2. Empirical strategy

The baseline specification is a two-way fixed-effects model:

$$AL_{it} = \beta D_{i,t-1} + \gamma'X_{i,t-1} + \alpha_i + \delta_t + \varepsilon_{it}, \qquad (1)$$

where AL_{it} is adult participation in learning in country i and year t; $D_{i,t-1}$ is one of four digitalization proxies (ICT specialists, daily internet users, enterprise ICT training, and online health-information seeking), entered one at a time in view of their mutual correlations of 0.53-0.78; $X_{i,t-1}$ collects the controls (log GDP per capita, the unemployment rate, and government effectiveness); a_i and δ_t are country and year fixed effects; and standard errors are clustered at the country level (CRV1, 31 clusters). All right-hand-side variables are lagged one year to mitigate simultaneity; identification is discussed in Section 3.3. The same specification, with the dependent variable replaced by tertiary attainment or log labor productivity, traces the association along the human-capital chain; the restricted samples behind Table 5 use the identical specification.

The fixed-effects choice is tested against the random-effects alternative,

$$AL_{it} = \beta D_{i,t-1} + \gamma'X_{i,t-1} + \delta_t + \mu_i + \varepsilon_{it}, \qquad (2)$$

where μ_i is a country-specific random component, via the Hausman contrast on the slope coefficients. As a further check on timing, and on sensitivity to measurement error in the regressor, the model is also estimated over differences of variable length k:

$$\Delta_k AL_{it} = \beta_k \Delta_k D_{i,t-1} + \gamma'\Delta_k X_{i,t-1} + \delta_t + \Delta_k \varepsilon_{it}, \qquad (3)$$

where $\Delta_k z_{it} \equiv z_{it} - z_{i,t-k}$. Setting $k = 1$ recovers the conventional first-difference check; tracing the estimate over $k = 1, \ldots, 8$ exploits the errors-in-variables logic of panels (Griliches & Hausman, 1986), under which classical measurement error attenuates the coefficient the more, the shorter the difference. Direct measures of digital competencies are examined in two complementary designs. Across the three DigComp 2.0 waves, the specification is

$$\begin{aligned} AL_{it} &= \beta S_{it} + \gamma_1 \ln GDPpc_{i,t-1} \\ &+ \gamma_2 U_{it} + \alpha_i + \delta_t + \varepsilon_{it}, \end{aligned} \qquad (4)$$

where S_{it} is the share of individuals with at least basic (alternatively, above-basic) digital skills; the income control enters with a lag so that the 2025 wave, for which contemporaneous WDI data are not yet available, is retained. Long differences over 2021–2025 are estimated in the cross-section of the 31 countries with heteroskedasticity-robust (HC1) standard errors:

$$\begin{aligned} \Delta AL_i &= \beta_0 + \beta \Delta S_i + \gamma_1 \Delta U_i \\ &+ \gamma_2 \Delta \ln GDPpc_{i,t-1} + u_i, \end{aligned} \qquad (5)$$

In (5), Δz_i denotes the 2021–2025 change; the income term carries the $(t-1)$ subscript because it is differenced over the corresponding lagged window (one year behind each wave endpoint, i.e., 2020 and 2024), consistent with the lag in (4), so that the 2025 wave is retained.

Country heterogeneity is examined by clustering countries on their standardized average 2021–2025 digital profiles (at-least-basic and above-basic skills, ICT specialists, and daily internet use) with the k-means algorithm, which minimizes the within-cluster sum of squared distances:

$$\min_{\{C_k\},\{\mu_k\}} \sum_{k=1}^{K} \sum_{i \in C_k} \| z_i - \mu_k \|^2, \qquad (6)$$

where z_i is the standardized profile of country i, C_k and μ_k are the clusters and their centroids, and the number of clusters K is selected by the silhouette criterion; the baseline specification is then re-estimated within each cluster. Two auxiliary tests address comparisons the design invites but the baseline does not deliver. To compare the digitalization proxies rather than assess them one at a time, each proxy is rescaled by its sample standard deviation and the four equations are estimated jointly on stacked data with errors clustered by country; this reproduces the separate coefficients exactly while supplying the cross-equation covariance needed to test their equality by Wald statistics. To test stability across the 2021 IESS redesign, the baseline is augmented with interactions between the regressors and an indicator for 2021 onwards, which nests the sub-window comparison as a Chow test; a variant additionally allows the post-2021 level shift to differ by country. Wild-cluster bootstrap p-values, imposing the null and using Rademacher weights, or Webb weights where a sub-sample has fewer than twelve clusters, with 19,999 replications, supplement cluster-robust inference for the headline coefficient and for the cluster-specific estimates, since asymptotic cluster-robust inference loses reliability when clusters are few. Diagnostics comprise the Hausman test, the Breusch–Pagan test for heteroskedasticity, variance inflation factors, and the CD test for cross-sectional dependence, which remains valid at small T (Pesaran, 2021). Measurement quality is audited through Eurostat observation flags, which identify the 2021 redesign of the EU Labour Force Survey (IESS) and the 2024 series breaks that define the sub-window analysis.

All data analyzed in this study derive from publicly available official sources and can be obtained without restriction. Indicators of human capital, digitalization and lifelong learning (adult participation in learning, tertiary educational attainment, real labor productivity, the share of ICT specialists in employment, daily internet use, enterprise-provided ICT training, online health-information seeking, e-government use, and overall digital skills under the DigComp 2.0 framework, together with the harmonized unemployment rate) are from Eurostat (Eurostat, 2026), retrieved through the SDMX-CSV dissemination API. Macroeconomic and structural controls (GDP per capita in constant 2015 US dollars, GDP growth, inflation, and internet users) are from the World Development Indicators (World Bank, 2026a). Institutional quality is measured by the government-effectiveness and regulatory-quality estimates from the Worldwide Governance Indicators (World Bank, 2026b), whose series extend through 2023. All series were harmonized into an unbalanced country–year panel keyed on ISO3 codes; no new primary data were created.

3. RESULTS

3.1. Descriptive statistics

Table 2 reports descriptive statistics for the two estimation windows. In the annual panel (Track A: 31 European economies, 2015–2023), adult participation in learning averages 11.4% of the population aged 25-64 (SD = 7.99), ranging from 0.9% to 38.8%, which indicates substantial variation both across countries and over time. The share of ICT specialists in total employment averages 4.1% (SD = 1.48; range 1.2-8.7%), whereas daily internet use is already widespread (mean 76.8%, SD = 12.05). In the skills window (Track B: waves 2021, 2023, and 2025), 56.9% of individuals on average possess at least basic overall digital skills under the DigComp 2.0 framework (SD = 14.29), but only 28.5% report above-basic skills, with wide cross-country dispersion (6.1-55.6%). The contrast between near-universal connectivity and far-from-universal skills motivates the distinction, maintained throughout the analysis, between composition-type and diffusion-type measures of digitalization. The second label is preferred to ac-

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Table 2. Descriptive statistics

Variable	N	Mean	SD	Min	Max
Panel A. Annual panel (Track A): 31 European economies, 2015–2023					
Adult participation in learning, 25–64 (%)	279	11.38	7.99	0.90	38.80
Tertiary educational attainment, 25–34 (%)	279	41.91	9.16	22.50	63.00
Real labour productivity (index, 2015 = 100)	270	104.96	7.51	89.77	134.19
ICT specialists (% of total employment)	279	4.12	1.48	1.20	8.70
Daily internet users (%)	275	76.83	12.05	36.63	97.67
Enterprises providing ICT training (%)	203	10.10	4.76	2.06	24.25
Seeking health information online (%)	275	54.18	12.09	23.89	82.62
Interaction with public authorities online (%)	215	54.67	21.16	8.73	92.25
GDP per capita (constant 2015 USD)	279	32,011.85	23,491.48	5,262.69	110,873.19
GDP growth (annual %)	279	2.78	3.86	−10.94	24.62
Inflation (annual %)	279	3.71	6.61	−2.10	72.31
Unemployment rate, 15–74 (%)	279	7.82	4.39	2.00	26.10
Government effectiveness (WGI)	279	0.94	0.63	−0.29	2.02
Regulatory quality (WGI)	279	1.05	0.55	−0.24	2.04
Panel B. Digital-skills waves (Track B): 2021, 2023, 2025					
At least basic digital skills (DigComp 2.0, %)	92	56.93	14.29	27.73	83.61
Above basic digital skills (DigComp 2.0, %)	92	28.47	12.51	6.14	55.61
Adult participation in learning, 25–64 (%)	93	13.63	8.36	1.40	38.80
ICT specialists (% of total employment)	93	4.87	1.56	1.40	8.90
Daily internet users (%)	92	86.45	6.87	67.42	99.29
Seeking health information online (%)	92	60.32	10.92	36.02	82.62

Note: Both panels are computed on the 31-country estimation sample; in Panel B, N reflects the waves available per country (North Macedonia lacks the 2023 wave). Panel A reports contemporaneous levels within the 2015–2023 window, whereas the models in Table 3 enter each digitalization proxy at t − 1, so the effective window shifts to 2014–2022 and N need not coincide with Panel A whenever a survey round is missing at either end. The enterprise ICT survey ran in 2014 but not in 2021 or 2023, so N rises from 203 here to 232 in Table 3, column (3); online health-information seeking has no 2014 round while 2023 was collected, so N falls from 275 here to 245 in column (4). The specialist share and daily internet use have complete rounds at both ends and are unaffected. Sources: Eurostat; World Bank WDI and WGI.

cess: daily internet use approximates the extent of connectivity, but online health-information seeking reflects how individuals use the internet and is shaped by demographic structure and health-system context as much as by availability, so it is read as an indicator of usage intensity rather than of infrastructure access. The panel contains no measure of infrastructure access as such, which bounds what the comparison below can establish. Pairwise correlations are reported in Appendix B: the two governance indicators correlate at 0.92 and the Eurostat and WDI internet-use measures at 0.94, which motivates the parsimonious control set, while the four digitalization proxies correlate at 0.53-0.78 and are therefore entered one at a time.

3.2. Baseline fixed-effects estimates

Table 3 presents two-way fixed-effects estimates of the baseline specification: four parallel models, each entering one digitalization proxy lagged by one year, with the logarithm of GDP per capita, the unemployment rate, and government effectiveness (all at t–1) as controls, country and year fixed effects, and standard errors clustered by country (31 clusters). The lagged share of ICT specialists carries a positive and statistically significant coefficient: a one-percentage-point increase is associated with a 1.38 percentage-point increase in adult learning participation ($\beta = 1.379$, $p = 0.034$, N = 279), or roughly one-eighth of the sample mean of 11.4%. Daily internet use ($\beta = 0.021$, SE = 0.065) and online health-information seeking ($\beta = 0.025$, SE = 0.031) yield estimates close to zero, and enterprise-provided ICT training is positive but short of conventional significance ($\beta = 0.145$, $p = 0.125$; N = 232, reflecting the biennial enterprise survey). The Hausman test rejects the random-effects specification ($\chi^2(4) = 11.94$, $p = 0.018$), supporting the fixed-effects choice; the Breusch–Pagan test rejects homoskedasticity (LM = 18.03, $p = 0.001$), consistent with the use of clustered standard errors. The Pesaran CD statistic on the residuals is –1.60 ($p = 0.110$), so cross-sectional independence

Table 3. Digitalization proxies and adult participation in learning: baseline fixed-effects estimates

Regressor	(1)	(2)	(3)	(4)
ICT specialists (t − 1)	1.379*			
	(0.621)			
Daily internet users (t − 1)		0.021		
		(0.065)		
Enterprises providing ICT training (t − 1)			0.145	
			(0.092)	
Seeking health information online (t − 1)				0.025
				(0.031)
ln GDP per capita (t − 1)	7.160	5.428	8.582	3.643
	(4.300)	(4.077)	(4.576)	(5.137)
Unemployment (t − 1)	0.075	0.072	0.119	0.095
	(0.105)	(0.118)	(0.141)	(0.132)
Government effectiveness (t − 1)	0.271	0.277	0.569	0.110
	(1.667)	(2.002)	(1.853)	(1.822)
Country FE / Year FE	Yes / Yes	Yes / Yes	Yes / Yes	Yes / Yes
Observations	279	275	232	245
R^2	0.956	0.956	0.959	0.959

Note: Dependent variable – adult participation in learning, 25–64 (%). Standard errors clustered by country (CRV1) in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001.

is not rejected once year fixed effects are included; estimated without them, the same model yields CD = 7.91 (p < 0.001). Variance inflation factors in the baseline set remain below 5, whereas entering two governance dimensions jointly raises them to 8.97 and 7.29.

Whether the specialist share matters more than the diffusion measures is a distinct question from whether it is individually significant, and it requires the coefficients to be compared directly. Because the proxies are measured on different scales, each is rescaled by its sample standard deviation, and the four equations of Table 3 are estimated jointly on stacked data, which reproduces the reported coefficients exactly while supplying the cross-equation covariance; the significance of each individual coefficient is as reported in Table 3. A one-standard-deviation increase corresponds to 1.99 points of adult learning for the specialist share, against 0.69 for enterprise ICT training, 0.31 for online health-information seeking, and 0.27 for daily internet use. The ordering matches the composition reading, but none of the differences is statistically determined: the specialist share exceeds daily internet use by 1.71 points (SE = 1.45, p = 0.245) and online health-information seeking by 1.68 points (SE = 1.03, p = 0.115), and equality across the four proxies is not rejected (F(3, 30) = 0.89, p = 0.457). Rescaling by within-country standard deviations, the variation that identifies the coefficients under fixed effects leaves this unchanged (F(3, 30) = 0.37, p = 0.773). Entered jointly in a single equation despite their mutual correlation, the specialist share remains the only proxy significant at the 5% level (2.33 points per standard deviation, SE = 0.96, p = 0.021, N = 245), while daily internet use and health-information seeking are jointly indistinguishable from zero (F(2, 30) = 1.92, p = 0.164). The specialist share is therefore the only dimension of digitalization carrying a detectable signal, but the analysis does not establish that its association is larger than that of the diffusion measures.

3.3. Digital skills wave and long differences

Table 4 turns to the direct measures of digital competencies. Within-country estimates over the three DigComp 2.0 waves are positive but statistically insignificant for both at-least-basic (β = 0.042, SE = 0.058) and above-basic skills (β = 0.073, p = 0.210; N = 92); with three waves and slow-moving skill stocks, the within-country variation available for identification is small. Long differences over 2021–2025 across 31 countries are of the same sign (Δ at-least-basic: β = 0.121, p =

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Table 4. Digital skills and adult participation in learning: wave panel and long differences

Panel A. FE across waves 2021/2023/2025	(1)	(2)
At least basic digital skills (t)	0.042 (0.058)	
Above basic digital skills (t)		0.073 (0.057)
ln GDP per capita (t − 1)	8.602 (7.856)	9.635 (7.944)
Unemployment (t)	0.583** (0.190)	0.585** (0.183)
Country FE / Year FE	Yes / Yes	Yes / Yes
Observations	92	92
R^2	0.977	0.978
Panel B. Long differences 2021 → 2025 (N = 31)	(1)	(2)
Δ At least basic digital skills	0.121 (0.114)	
Δ Above basic digital skills		0.106 (0.090)
Δ Unemployment	0.609* (0.292)	0.694** (0.236)
Δ ln GDP per capita (t − 1)	12.160 (7.118)	14.473 (7.202)
Intercept	1.575 (1.189)	1.464 (1.138)
R^2	0.186	0.186

Note: Panel A – two-way FE on the 31-country sample, SE clustered by country. Panel B – cross-section of 2021–2025 changes, heteroskedasticity-robust (HC1) SE. * p < 0.05, ** p < 0.01.

0.300; Δ above-basic: β = 0.106, p = 0.249). In this window, contemporaneous unemployment enters positively and significantly, both in the wave panel (β = 0.583, p < 0.01) and in long differences (β = 0.609, p = 0.047; β = 0.694, p = 0.007), whereas the lagged unemployment term in the annual panel is small and insignificant.

3.4. Robustness of the headline estimate

Table 5 summarizes the behavior of the headline coefficient across estimators, samples, and windows; Appendix C full leave-one-out diagnostics are reported in . The coefficient is positive across the checks that retain adult learning as the dependent variable, with a single exception – the post-break-only window 2021–2023 (row 10, β = −0.345) – and ranges from 0.44 to 1.76, reaching statistical significance in the majority: under random effects (β = 1.759, p = 0.011), after replacing government effectiveness with regulatory quality (β = 1.459, p = 0.019), when the sample is restricted to the EU-27 (β = 1.556, p = 0.040), and in the pre-break window 2015–2020 (β = 1.536, p = 0.024, N = 186), which predates the 2021 EU Labour Force Survey redesign (IESS) that generated documented series breaks in 36 country-series of the dependent variable. Because inference rests on 31 clusters, wild-cluster bootstrap p-values (restricted, Rademacher weights, 19,999 replications) are also computed for the specifications that reach significance under cluster-robust inference: 0.077 for the baseline, 0.055 with regulatory quality as the institutional control, 0.079 for the EU-27, and 0.061 for the pre-break window. Under this more conservative inference, the headline association is significant at the 10% rather than the 5% level; its sign and magnitude are unaffected, and the attenuation evidence in Table C4 indicates that the within estimate understates rather than overstates the association. Smaller estimates appear in first differences (β = 0.446, p = 0.350), on the common sample dictated by the biennial enterprise survey (β = 0.959, SE = 0.720, N = 201), when Türkiye is excluded (β = 1.281, p = 0.070), and, most markedly, when 2024 is added to the window (β = 0.462, p = 0.457). The 2024 change coincides with documented measurement breaks: six of the 31 countries, including Belgium, Germany, and France, carry official series breaks in 2024, and mean participation rose by 1.50 percentage points in flagged countries against 0.84 in the remaining ones. Leave-one-year-out estimates within 2015–2023 range from 1.058 to 1.726, and leave-one-country-out estimates from 1.082 to 1.752, with significance falling to the 10% level when Sweden or Estonia is excluded. Excluding 2020 or 2021 individually leaves the estimate essentially unchanged (β = 1.479 and 1.541).

The contrast between the pre- and post-redesign windows is tested formally rather than inferred from the two sub-samples. Interacting the lagged specialist share with an indicator for 2021 onwards, the level shift being absorbed by the year effects,

Table 5. Robustness of the headline estimate (ICT specialists, t–1 → adult learning)

Specification / sample	β (ICT specialists, t–1)	SE	N
Panel A. Level coefficient on the lagged share of ICT specialists			
(1) Baseline FE, 2015–2023	1.379*	0.621	279
(2) Random effects	1.759*	0.646	279
(3) First differences	0.446	0.470	279
(4) Common sample (biennial grid)	0.959	0.720	201
(5) Regulatory quality replaces government effectiveness	1.459*	0.591	279
(6) EU-27 only	1.556*	0.720	243
(7) Excluding Türkiye	1.281	0.682	270
(8) Window 2015–2024	0.462	0.613	310
(9) Pre-break window 2015–2020	1.536*	0.648	186
(10) Post-break window 2021–2023	−0.345	1.309	93
(11) Leave-one-year-out, 2015–2023 (range of β)	1.058…1.726	–	–
(12) Leave-one-country-out, 2015–2023 (range of β)	1.082…1.752	–	–
(13) Dependent variable: tertiary attainment, 25–34	0.864	0.863	279
(14) Dependent variable: ln labor productivity	0.001	0.008	270
Panel B. Structural-break tests (coefficient on the interaction with post-2021)			
(15) Interaction: ICT specialists × post-2021, country and year FE	0.228	0.304	279
(16) Interaction, country-specific post-2021 level shifts	−1.963	1.253	279

Note: Significance stars are as reported by the estimation output (* $p < 0.05$). Row (2): random effects with SE clustered by country; Hausman $\chi2(4) = 11.94$, $p = 0.018$. Row (3): first differences with year FE and country-clustered SE. Rows (11)–(12): full results in Appendix C. Rows (13)–(14): baseline specification with the dependent variable replaced. Panel B reports coefficients on the interaction of the lagged specialist share with an indicator for 2021 onwards, not level coefficients: row (15) with country and year fixed effects, row (16) additionally allowing country-specific post-2021 level shifts. The joint constancy test across all four regressors gives $F(4, 30) = 0.22$, $p = 0.926$.

leaves the interaction far from significance (β = 0.228, SE = 0.304, p = 0.460; Panel B, row 15), and interacting all four regressors with the same indicator does not reject parameter constancy either ($F(4, 30) = 0.22$, $p = 0.926$). Allowing the post-2021 level shift to differ by country, which is the form a country-specific measurement change would take, reverses the sign of the interaction but still leaves it insignificant (β = −1.963, SE = 1.253, p = 0.128; row 16). The estimated change in slope is therefore not robust in sign to how country effects are handled, and in no specification does it reach conventional significance. The sub-sample contrast in rows (9) and (10) reflects the precision available in a three-year window: the post-break estimate carries a standard error of 1.309, so −0.345 is not statistically distinguishable from the pre-break estimate of 1.536 (z = 1.29, p = 0.198). With 31 clusters, these tests have limited power, so the evidence is non-rejection of constancy rather than proof of it, and identification continues to rest primarily on the pre-2021 window, as set out in Section 3.3.

Table C4 traces the estimate over differences of length k = 1, …, 8: it rises steadily with k, from 0.446, matches the level estimate at k = 4 (β = 1.490, p = 0.032), and reaches roughly 2.3–2.4 by k = 6-8; the same rising profile holds on the 2015–2020 window (0.302 at k = 1 to 2.768 at k = 5). As k lengthens, the sample falls from 279 to 92 observations, and the identifying variation shifts increasingly between countries. Finally, with the dependent variable replaced, the same lagged regressor shows no significant association with tertiary attainment (β = 0.864, SE = 0.863) or with log labor productivity (β = 0.001, SE = 0.008).

3.5. Country heterogeneity

To examine country-level heterogeneity, countries are clustered on their average 2021–2025 digital profiles (at-least-basic and above-basic skills, ICT specialists, and daily internet use) using k-means; the silhouette criterion selects two clusters (average silhouette width 0.445, against 0.416 for three, 0.332 for four, and 0.354 for five). Table 6 reports the profiles. A digital frontrunner group of 11 economies (Belgium, Denmark, Estonia, Finland, Ireland, Luxembourg, Malta, the Netherlands, Norway, Spain, and Sweden) combines 69.9% at-least-basic skills with 6.3% ICT specialists, whereas the follower group of 20 economies – which includes Germany, France, and Austria alongside Central-Eastern and Southern European countries

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Table 6. Digital-profile clusters (means over 2021–2025) and sub-sample estimates

Cluster	n	At least basic (%)	Above basic (%)	ICT spec. (%)	Daily internet (%)	β ICT spec. (t−1)	N	Wild bootstrap p
Followers (0)	20	49.4	21.5	4.1	82.9	1.058 (0.644)	180	0.105
Frontrunners (1)	11	69.9	40.5	6.3	92.8	1.637 (1.281)	99	0.273

Note: k-means on standardized profiles; silhouette = 0.445 for K = 2. Sub-sample β from the baseline specification with country and year FE, SE clustered by country. The country list is in Appendix A. Wild-cluster bootstrap p-values impose the null and use 19,999 replications, with Webb weights for the 11-cluster group and Rademacher weights for the 20-cluster group.

and the candidate economies – averages 49.4% and 4.1%, respectively. Sub-sample estimates of the baseline specification are positive in both groups: β = 1.058 (SE = 0.644, N = 180) among followers and β = 1.637 (SE = 1.281, N = 99) among frontrunners. Neither is significant at conventional levels, and the two sub-sample coefficients are not statistically distinguishable from each other. Because the frontrunner group contains only 11 countries, conventional cluster-robust inference has limited precision there, so wild-cluster bootstrap p-values are reported alongside in Table 6 (restricted bootstrap with Webb weights for the 11-cluster group and Rademacher weights for the 20-cluster group, 19,999 replications): 0.273 for frontrunners and 0.105 for followers. Neither coefficient is distinguishable from zero under either form of inference, so the common positive sign is a descriptive consistency across groups rather than evidence of a common underlying mechanism. Appendix A lists cluster membership together with EU membership status.

4. DISCUSSION

The central result of this study, namely that the specialist composition of the workforce is the only dimension of digitalization individually associated with adult participation in learning, while diffusion measures are not, sits comfortably within a literature that has grown skeptical of access-based accounts of digital human capital. Aggregate indicators of technological "vibrancy" have been shown to leave short-run employment structures largely unmoved (Kuzior et al., 2025), and cross-country evidence attributes competitiveness to asymmetries in how technology is used rather than to connectivity per se (Kolupaieva & Tiesheva, 2023). The observation that near-universal internet use carries little additional signal for learning is consistent, too, with pandemic-era findings that access to online instruction did not protect practice-based performance (Śliwa et al., 2021). The composition-diffusion distinction thus resolves, at the macro level, a tension that firm- and individual-level studies had already begun to expose. That the composition and diffusion coefficients cannot be formally separated at this sample size tempers the strength of the claim without altering its direction.

The study's more qualified results also find echoes in the literature. The confinement of the association to the participation flow, without a corresponding response in tertiary attainment or labor productivity, aligns with evidence that education exerts only a bounded direct effect on labor-market structure (Mukhtarova et al., 2024) and that productivity responds to context-dependent factors rather than to human-capital measures uniformly (Lyeonov et al., 2025). The counter-cyclical pattern, whereby higher unemployment coincides with greater learning, resonates with the broader finding that active labor-market policy improves outcomes when institutionally supported (Knapińska & Woźniak-Jasińska, 2024), and invites reading recessions as potential windows for reskilling rather than as purely scarring episodes. The shared direction of the association across digitally leading and lagging clusters is, in turn, compatible with evidence of human-capital convergence spanning Western and Central-Eastern European regions (Dańska-Borsiak, 2023). It does not, however, show that the mechanism linking specialist density to adult learning operates beyond the frontier: neither cluster-specific coefficient is significant, the frontrunner group holds only 11 countries, and wild-cluster bootstrap inference confirms the imprecision, so the common sign is a descriptive consistency rather than evidence of a common underlying mechanism.

How the headline estimate behaves across checks is itself informative. Excluding 2020 or 2021 individually leaves it essentially unchanged (β = 1.479 and 1.541), so the association does not hinge on any single pandemic year, and residual cross-sectional dependence is absent once year effects are included (CD = –1.60, p = 0.110, against 7.91 without them). More informative still is the first-difference attenuation: under classical measurement error in a survey-based regressor, the noise in a k-year difference is invariant to k while the underlying signal accumulates, so attenuation should recede as differences lengthen (Griliches & Hausman, 1986). The estimate traced in Table C4 rises steadily with k, from 0.446 at k = 1, through the level estimate at k = 4 (β = 1.490, p = 0.032), to around 2.3-2.4 by k = 6-8, and the same profile holds on the undisturbed 2015–2020 window (0.302 rising to 2.768). Because the sample contracts as k grows and long differences lean increasingly on between-country variation, which the fixed-effects design is meant to remove, the pattern is best treated as consistent with measurement error attenuating the within estimate rather than as an independent estimate of a larger effect; if anything, it suggests the headline coefficient is a lower bound. These boundaries also delimit where the evidence travels. The composition–diffusion distinction is decision-relevant wherever digitalization advances while its returns stay constrained by institutional capacity and a thin specialist workforce – the configuration documented for Ukraine and Kazakhstan and for Armenia's ICT sector (Zakharkina et al., 2025; Grigoryan et al., 2025). Two things follow. The instrument most directly supported here, the specialist pipeline, does not depend on EU membership and is therefore a plausible candidate for transfer, but whether it bears the same relationship to adult learning in economies outside the sample is a hypothesis the present design cannot test; enterprise-provided training is a less secure candidate still, given its insignificant baseline coefficient; and none of these economies produces harmonized statistics on specialist density or adult learning, so the decisive margin is the one they cannot yet monitor (Ivanenko & Muraviov, 2026). Read against this body of work, the contribution of the analysis is less the discovery of a novel effect than the disciplined separation of composition from diffusion, together with an explicit measurement audit that anchors identification in the pre-redesign period, an adjustment the comparative literature has rarely made.

The principal limitation is identification. Digitalization and learning participation are plausibly jointly determined, and lagging every regressor by one year attenuates but does not remove this simultaneity. Slow-moving confounders – education and vocational-training systems, innovation policy, labor-market institutions – are absorbed by the country fixed effects, and common shocks, including COVID-19 and the diffusion of AI, by the year effects; time-varying country-specific confounders remain outside the design. Learning participation is persistent, yet a lagged dependent variable is not added: at nine years per country the Nickell bias would be of the same order as the coefficient of interest (Nickell, 1981). Dynamic-panel GMM is not pursued either – not because the time dimension is short, which is what such estimators were built for, but because the cross-section is: with 31 countries the instrument count cannot be held safely below N and the Hansen test loses power (Roodman, 2009), and the 2021 breaks contaminate the lagged levels that would serve as internal instruments. No credible external instrument exists at the country-year level, so all estimates are conditional associations. Measurement adds a further caveat: the 2021 IESS redesign of the EU Labour Force Survey generated officially flagged series breaks in 36 country-series of the dependent variable and in the ICT-specialist series; year fixed effects absorb common shifts but not country-specific measurement changes, which is why identification relies primarily on the pre-2021 period and the headline estimate is verified on the 2015–2020 window. Statistical power constrains the skills-side evidence: several key series are short or biennial, namely the enterprise ICT training survey and the three DigComp 2.0 waves, so the corresponding estimates are underpowered and are read as compatible with, rather than confirming, the annual-panel results, and the two digital-skills vintages (2015–2019 and 2021–2025) follow different measurement frameworks and are never pooled into a single coefficient. With three waves and slow-moving skill stocks, the within-country variation available for identification is small, so the insignificance of the direct-skills coefficients reflects limited power rather than evidence against an association; the point estimates are uniformly positive. The country-level design bounds the scope of the conclusions: heterogeneity across regions and socio-demographic groups within countries lies outside its reach, and Ukraine, not covered by the Eurostat ICT surveys, cannot enter the panel, so the transfer of the European evidence to the Ukrainian policy context is discussed, rather than estimated, in the concluding sections.

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CONCLUSION

This study sets out to establish whether the digital-skills composition of the workforce is linked to adult participation in lifelong learning in Europe, and what this implies for human capital policy in the knowledge economy. The evidence base is an unbalanced panel of 31 European economies over 2015–2023, complemented by three DigComp 2.0 skills waves (2021–2025), estimated with two-way fixed-effects models with lagged regressors and country-clustered standard errors, with an extensive robustness and measurement audit.

A one-percentage-point increase in the lagged share of ICT specialists in employment is associated with a 1.38-point rise in adult learning participation, whereas diffusion-type measures – daily internet use and online health-information seeking – show no such association. Compared directly, however, the four coefficients are not statistically distinguishable, so the specialist share is best described as the only dimension carrying a detectable signal rather than as the demonstrably larger one. The estimate is positive and moderate in magnitude across estimators and samples (0.44-1.76) and holds in the pre-break window 2015–2020, so it does not appear to be driven by the 2021 EU-LFS redesign, and a formal interaction test does not reject constancy of the coefficient across it. Under wild-cluster bootstrap inference, which is more conservative with 31 clusters, the baseline and pre-break estimates are significant at the 10% rather than the 5% level. Direct skills measures over three waves point in the same positive direction but remain insignificant, reflecting limited within-country variation at short horizons. The association is confined to the participation flow (tertiary attainment and labor productivity do not respond at this horizon), points in a shared direction across digital-frontrunner and follower clusters, and coexists with a counter-cyclical pattern whereby higher unemployment coincides with greater learning participation.

For governments, the results are consistent with rebalancing digital human-capital policy from connectivity towards composition, though they do not by themselves establish that such a rebalancing would raise participation. With internet use already near-universal in Europe, further connectivity targets carry no measurable signal for lifelong learning in these data. Expanding the pipeline of ICT specialists (through STEM capacity, reskilling into ICT occupations, and incentives for enterprise-provided ICT training) is the direction most consistent with the estimates, subject to two qualifications: enterprise ICT training is not significant in the baseline specification, and the mechanism through which specialist density would raise participation, namely knowledge diffusion to co-workers and greater absorptive capacity in firms, is argued here rather than tested. These recommendations are therefore offered as consistent with the evidence, not as established by it.

The counter-cyclical pattern further suggests embedding learning entitlements in labor-market policy, so downturns become reskilling windows, not scarring episodes. Future micro-level work, linking training decisions to workplace specialist density and testing interactions with income, institutions, and innovation, would sharpen what a country panel can only approximate. For economies outside the panel – among them Ukraine, where post-war recovery strategies place human capital at the center, together with Kazakhstan and Armenia – the European evidence can motivate policy hypotheses but does not establish that the same relationship holds there: these economies are absent from the estimation sample, and nothing in the estimates confirms that prioritizing specialist density, still less enterprise training, would be associated with adult learning in the same way. What the evidence does support is a caution against connectivity-only targets and a prior step: building the specialist and adult-learning statistics without which the hypothesis cannot be tested and the margin cannot be monitored.

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APPENDIX A

Table A1. Estimation sample: EU membership and digital-profile cluster

Country	ISO3	EU member	Digital-profile cluster
Austria	AUT	Yes	Follower
Belgium	BEL	Yes	Frontrunner
Bulgaria	BGR	Yes	Follower
Croatia	HRV	Yes	Follower
Cyprus	CYP	Yes	Follower
Czechia	CZE	Yes	Follower
Denmark	DNK	Yes	Frontrunner
Estonia	EST	Yes	Frontrunner
Finland	FIN	Yes	Frontrunner
France	FRA	Yes	Follower
Germany	DEU	Yes	Follower
Greece	GRC	Yes	Follower
Hungary	HUN	Yes	Follower
Ireland	IRL	Yes	Frontrunner
Italy	ITA	Yes	Follower
Latvia	LVA	Yes	Follower
Lithuania	LTU	Yes	Follower
Luxembourg	LUX	Yes	Frontrunner
Malta	MLT	Yes	Frontrunner
Netherlands	NLD	Yes	Frontrunner
North Macedonia	MKD	No	Follower
Norway	NOR	No	Frontrunner
Poland	POL	Yes	Follower
Portugal	PRT	Yes	Follower
Romania	ROU	Yes	Follower
Serbia	SRB	No	Follower
Slovakia	SVK	Yes	Follower
Slovenia	SVN	Yes	Follower
Spain	ESP	Yes	Frontrunner
Sweden	SWE	Yes	Frontrunner
Türkiye	TUR	No	Follower

Note: Clusters from k-means on standardized average 2021–2025 digital profiles (Table 5); silhouette selects K = 2.

APPENDIX B

Table B1. Pairwise correlations, Track A (31 economies, 2015–2023)

Indicator	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Adult learning	1.00													
2. ICT specialists	0.81	1.00												
3. Daily internet users	0.69	0.77	1.00											
4. Unemployment	−0.28	−0.48	−0.44	1.00										
5. GDP per capita	0.61	0.67	0.63	−0.32	1.00									
6. Government effectiveness	0.78	0.75	0.63	−0.40	0.76	1.00								
7. Tertiary attainment	0.45	0.50	0.54	−0.16	0.61	0.58	1.00							
8. Labour productivity (index)	−0.20	−0.03	0.07	−0.28	−0.22	−0.33	−0.08	1.00						
9. Enterprise ICT training	0.53	0.61	0.58	−0.26	0.71	0.71	0.34	−0.29	1.00					
10. Health information online	0.63	0.61	0.78	−0.31	0.42	0.60	0.44	−0.03	0.53	1.00				
11. E-government use	0.80	0.74	0.83	−0.34	0.62	0.82	0.58	−0.11	0.56	0.77	1.00			
12. GDP growth	−0.09	−0.09	−0.10	−0.05	−0.01	−0.10	0.07	0.28	−0.05	−0.04	−0.05	1.00		
13. Inflation	−0.04	−0.08	0.18	−0.09	−0.13	−0.26	−0.04	0.56	−0.10	0.08	0.01	0.09	1.00	
14. Regulatory quality	0.71	0.78	0.61	−0.47	0.74	0.92	0.55	−0.23	0.60	0.50	0.76	−0.09	−0.28	1.00

Note: Lower triangle shown; N up to 279. Labor productivity is an index (2015 = 100), so its pooled correlations partly reflect cross-country catch-up dynamics; estimation uses only its within-country log variation. The governance pair (6, 14) correlates at 0.92 and the digitalization proxies (2, 3, 9, 10) at 0.53–0.78, motivating single-entry specifications; the WDI internet-use measure (r = 0.94 with variable 3) is excluded from the models. The strong bivariate correlations of adult learning with ICT specialists (0.81) and e-government use (0.80) are dependent-variable–regressor associations rather than collinearity among regressors; variance inflation factors for the estimated models are reported in Section 4.2 and remain below 5.

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APPENDIX C

Influence diagnostics for the headline estimate (β on ICT specialists, t–1; baseline specification). Sub-window estimates are reported in Table 5, rows (9)–(10).

Table C1. Leave-one-year-out, main window 2015–2023

Excluded year	β	SE	Excluded year	β	SE
2015	1.058	0.657	2020	1.479	0.653
2016	1.214	0.671	2021	1.541	0.688
2017	1.338	0.627	2022	1.117	0.619
2018	1.426	0.617	2023	1.726	0.555
2019	1.573	0.653			

Table C2. Leave-one-year-out, extended window 2015–2024

Excluded year	β	SE	Excluded year	β	SE
2015	0.127	0.584	2020	0.535	0.637
2016	0.282	0.610	2021	0.542	0.669
2017	0.380	0.623	2022	0.218	0.641
2018	0.464	0.610	2023	0.388	0.571
2019	0.578	0.636	2024	1.379	0.621

Note: In the extended window only the exclusion of 2024 restores the baseline estimate, consistent with the officially flagged 2024 series breaks discussed in the text.

Table C3. Leave-one-country-out, main window 2015–2023 (sorted by β)

Excl.	β	SE	Excl.	β	SE
SWE	1.082	0.637	FRA	1.393	0.623
EST	1.104	0.676	LVA	1.396	0.644
NLD	1.130	0.629	POL	1.399	0.624
TUR	1.281	0.682	BEL	1.414	0.635
BGR	1.294	0.637	HRV	1.418	0.627
FIN	1.304	0.625	DEU	1.419	0.621
SVK	1.312	0.642	MLT	1.434	0.637
GRC	1.322	0.628	SRB	1.438	0.629
CZE	1.333	0.620	LTU	1.448	0.666
HUN	1.338	0.644	ROU	1.456	0.619
DNK	1.351	0.607	ESP	1.462	0.618
PRT	1.356	0.650	ITA	1.488	0.623
AUT	1.361	0.626	MKD	1.508	0.628
SVN	1.363	0.633	IRL	1.576	0.615
NOR	1.372	0.624	LUX	1.752	0.550
CYP	1.386	0.646			

Note: β ranges 1.082-1.752 and is positive in all 31 exclusions; excluding Sweden or Estonia lowers significance to the 10% level.

Table C4. Estimates by differencing length (Griliches-Hausman diagnostic)

k	β (Δ ICT specialists, t–1)	SE	p	N
Panel A. Main window 2015–2023				
1	0.446	0.470	0.350	279
2	0.488	0.432	0.267	278
3	1.036	0.631	0.111	247
4	1.490	0.661	0.032	216
5	2.020	0.646	0.004	185
6	2.419	0.683	0.001	154
7	2.309	0.822	0.009	123
8	2.268	0.861	0.013	92

Table C4 (cont.). Estimates by differencing length (Griliches-Hausman diagnostic)

k	β (Δ ICT specialists, t–1)	SE	p	N
Panel B. Pre-break window 2015–2020				
1	0.302	0.393	0.448	186
2	1.084	0.490	0.035	185
3	1.833	0.801	0.029	154
4	2.168	0.803	0.011	123
5	2.768	0.979	0.008	92

Note: Dependent variable – the k-year change in adult participation in learning; regressors are the corresponding k-year changes, lagged one year. Year fixed effects; standard errors clustered by country (31 clusters). k = 1 in Panel A corresponds to Table 5, row (3). The estimation sample shrinks as k lengthens. Source: authors' calculations.

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