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Does Higher Education Expansion Raise Labour Productivity? Heterogeneous Long-Run Evidence from Transition Economies and Uzbekistan

Xurshid Zaripov [1,*], Javohir G'afforov [2], Farrux Qodirov [3], Baxtigul Aydaniyazova [4], Nigora Matmuratova [5], Juragul Manglieva [6] and Dilrabo Salayeva [7]

[1] Department of Economics, Faculty of Economics, Mamun University, Khiva 220900, Uzbekistan
[2] Department of Economics and Engineering, Faculty of Engineering and Pedagogy, Karshi International University, Karshi 180100, Uzbekistan; javohirgafforov2811@gmail.com
[3] Department of Mathematics and Applied Mathematics, Shahrisabz State Pedagogical Institute, Shahrisabz 181302, Uzbekistan; farruxqodirov0902@gmail.com
[4] Department of Economics, Faculty of Innovation Technologies and Business, University of Innovation Technologies, Khodjeyli 231300, Uzbekistan; baxtigul@uit.uz
[5] Department of Economics, RANCH University, Urgench 220106, Uzbekistan; mynigorim@gmail.com
[6] Navoi State University of Mining and Technologies, Navoi 210100, Uzbekistan; mangliyevajuragul@gmail.com
[7] Department of Economics, Faculty of Economics, Urgench State University Named After Abu Rayhan Biruni, Urgench 220100, Uzbekistan; dsalayeva3@gmail.com
* Correspondence: xolmurodov_fozil@mamunedu.uz

Abstract

Tertiary enrolment has expanded rapidly across post-socialist economies, most dramatically in Uzbekistan, where the gross enrolment ratio rose from 8.7% in 2017 to 45.8% in 2023 and 59.8% in 2025. Whether such expansion translates into higher labour productivity remains an open question. This study examines the short- and long-run relationship between tertiary enrolment and labour productivity in 24 transition economies over 2001–2023 using World Bank data. Because the series are strongly cross-sectionally dependent and heterogeneous, we estimate panel autoregressive distributed lag models with pooled mean group, mean group and dynamic fixed effects estimators on cross-sectionally demeaned data and complement them with cross-sectionally augmented estimators. The pooled mean group estimates indicate a long-run elasticity of labour productivity with respect to tertiary enrolment of about 0.10, with deviations from equilibrium corrected at about 17% per year; this estimate holds with longer lags of the regressors, in sub-periods that exclude the global financial crisis, when the sample is extended to 2024 and when Uzbekistan or Tajikistan is excluded, and panel Granger causality tests and a reverse error-correction model do not detect feedback from productivity to enrolment, although they cannot exclude it. The association is, however, strongly heterogeneous: it is positive among EU member states but negative among non-EU transition economies, the difference being statistically significant, and it is not statistically significant under fully heterogeneous common correlated effects estimators. The positive association reflects the contemporaneous co-movement of enrolment and productivity; it does not hold when enrolment is lagged to the time of graduation or when attainment is used instead of enrolment. Uzbekistan shows no statistically significant error correction towards the panel long-run relationship. Enrolment expansion alone therefore does not guarantee productivity gains; our results suggest, although the models do not test this directly, that its returns depend on the quality and labour-market relevance of higher education and on the capacity of economies to absorb graduates.

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1. Introduction

Over the past two decades, higher education systems in the post-socialist world have grown at a pace with few historical precedents. In the 24 transition economies examined in this paper, the average gross tertiary enrolment ratio rose from 38.1% in 2001 to 65.7% in 2023. The expansion has continued since: according to the latest World Development Indicators (October 2026 update), the ratio in Uzbekistan reached 56.5% in 2024 and 59.8% in 2025, and the number of university graduates in the country tripled from 70,800 in 2019 to 211,200 in 2024 (Kun.uz, 2025). Governments have treated this expansion as a central instrument of development strategy, on the premise that a more educated workforce raises productivity and income. The premise is firmly rooted in growth theory, in which human capital raises output either as a factor of production (Lucas, 1988; Mankiw et al., 1992) or by accelerating innovation and the adoption of frontier technologies (Nelson & Phelps, 1966; Romer, 1990; Benhabib & Spiegel, 1994). The macroeconomic evidence, however, is considerably less conclusive. Aggregate increases in schooling have often failed to produce the productivity gains predicted by micro-level returns to education (Pritchett, 2001), and the growth effects of education appear to depend strongly on its quality rather than its quantity (Hanushek & Woessmann, 2008, 2012). Recent evidence confirms the importance of quality: measures that combine schooling with internationally comparable learning outcomes account for a substantially larger share of cross-country income differences than years of schooling alone (Angrist et al., 2021), and for OECD economies the quality of education is estimated to be three to four times as important for productivity as its quantity (Égert et al., 2024; Hanushek & Woessmann, 2020).

Uzbekistan offers a particularly sharp illustration of this tension. It is also an analytically useful case for three reasons. First, the scale and timing of its expansion are unusual: no other economy in the sample combined two decades of very low and falling participation with a five-fold increase within six years, which provides a sharp test of whether enrolment and productivity move together. Second, Uzbekistan is the most populous economy of Central Asia and has the youngest population in the sample, so the productivity consequences of its reform matter for a large labour force. Third, the expansion was policy-driven, through quotas, new private and foreign-branch institutions and part-time and distance programmes, rather than a response to rising demand for graduates, so it allows us to ask whether supply-led expansion is reflected in productivity. After a long period of restricted access, during which the gross tertiary enrolment ratio fell from 13.3% in 2001 to below 9% in the early 2010s, the country embarked on an aggressive expansion following the reforms that began in 2017. Presidential Decree No. UP-5847 of 8 October 2019 set the goal of raising higher education coverage to more than 50% by 2030 through new public and private institutions, branches of foreign universities and regional campuses (President of the Republic of Uzbekistan, 2019). The target was approached far earlier than planned: by 2023, the gross enrolment ratio stood at 45.8%, more than five times its 2017 level, and World Bank data report 56.5% for 2024. Over the same period, GDP per person employed grew steadily, but most of this growth pre-dated the enrolment boom (Figure 1a). Whether and when the expansion of higher education will be reflected in labour productivity is therefore a question of first-order policy importance for Uzbekistan and for other economies pursuing similar strategies (Ruziev & Burkhanov, 2018). Uzbekistan is not treated differently from

the other economies in the estimation: it enters the same balanced panel and the same estimators, and its country-specific coefficients are obtained in exactly the same way as those of the other 23 economies. The emphasis on Uzbekistan concerns the interpretation of the results, and Figure 1 shows why it is warranted: in the other 23 economies, average enrolment and productivity rose together throughout the period (Figure 1b), whereas in Uzbekistan they moved in opposite directions until 2017, after which enrolment increased more than five-fold while productivity continued on its earlier trend (Figure 1a).

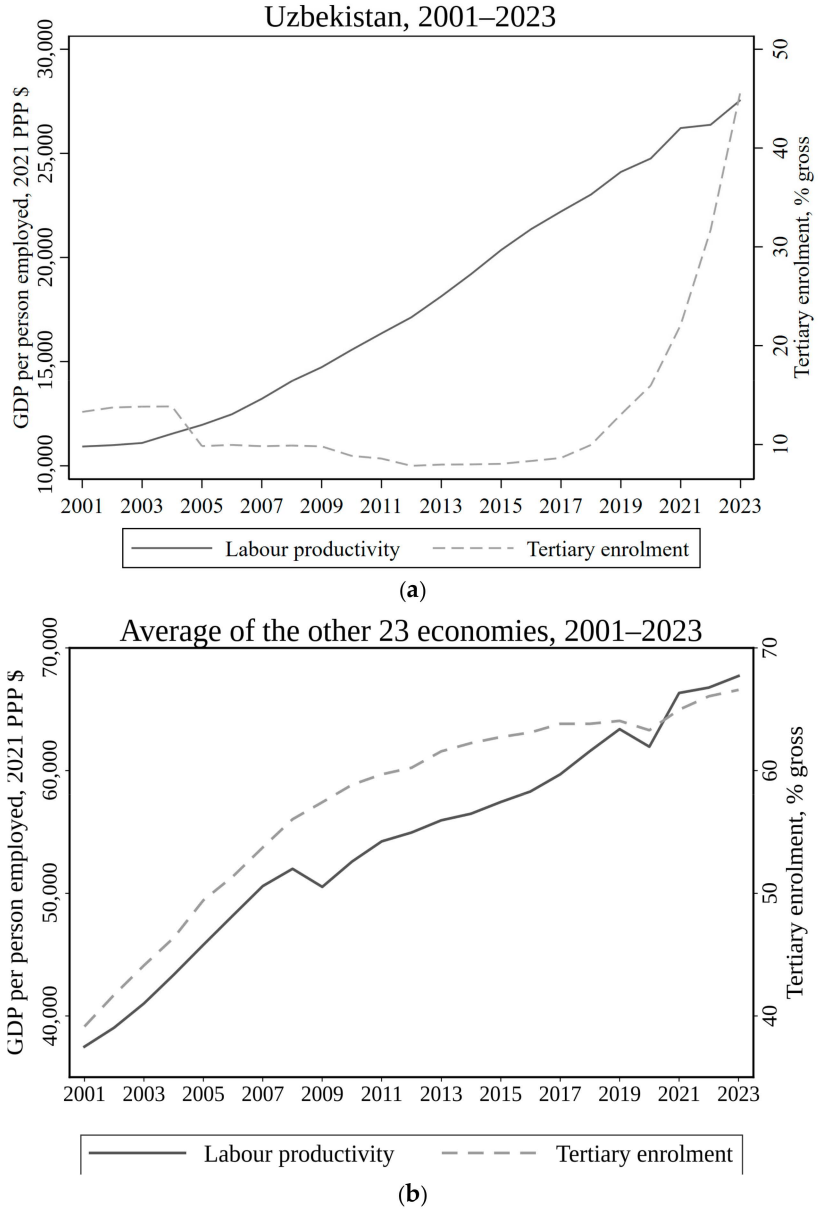


Figure 1. Labour productivity (GDP per person employed, constant 2021 PPP $; left axis) and gross tertiary enrolment ratio (%; right axis), 2001–2023: (**a**) Uzbekistan; (**b**) unweighted average of the other 23 economies in the sample.

This paper addresses three related questions. First, is there a stable long-run relationship between tertiary enrolment and labour productivity in transition economies once physical capital and population health are controlled for? Second, is this relationship homogeneous across countries, or does it differ systematically between economies with different institutional settings? Third, does Uzbekistan conform to the long-run relationship

observed in the wider panel, and how quickly does its productivity adjust to changes in educational capacity?

To answer these questions, we assemble a balanced panel of 24 transition economies in Central and Eastern Europe, the Western Balkans, the former Soviet Union and Mongolia over 2001–2023 from the World Development Indicators. We model labour productivity within an autoregressive distributed lag (ARDL) framework, which accommodates regressors that are integrated of order zero or one and separates short-run dynamics from the long-run relationship. Because pre-tests reveal strong cross-sectional dependence and slope heterogeneity, we estimate the model with the pooled mean group (PMG) estimator of Pesaran et al. (1999) on cross-sectionally demeaned data, compare it with mean group (MG) and dynamic fixed effects (DFE) estimators, and assess the results with cross-sectionally augmented ARDL (CS-ARDL), distributed lag (CS-DL) and common correlated effects mean group (CCE-MG) estimators (Chudik et al., 2016; Chudik & Pesaran, 2015; Pesaran, 2006).

The paper makes three contributions. First, it provides a systematic panel time-series assessment of the higher education–productivity nexus for transition economies, a group largely absent from the cross-country literature, using estimators that account for the common shocks that affected these economies simultaneously. Second, it documents that the pooled long-run association conceals marked heterogeneity: the relationship is positive among EU member states but negative among non-EU transition economies, a pattern that we interpret, as a hypothesis rather than a tested mechanism, as consistent with the view that the productivity returns to enrolment depend on quality, relevance and absorptive capacity. Third, it provides country-specific adjustment estimates showing that Uzbekistan does not exhibit statistically significant error correction towards the panel long-run relationship, which is consistent with the view that its recent enrolment boom has not yet been translated into productivity gains. Relative to earlier work, which mostly pools advanced and developing economies and reports either positive (Bassanini & Scarpetta, 2002) or insignificant (Pritchett, 2001; Delgado et al., 2014) macro-level effects of education, our results show that both findings can hold within one group of economies: the sign and size of the association differ across institutional settings, and the pooled positive estimate is sensitive to the estimator used.

2. Literature Review

2.1. Theoretical Background

Two broad channels link human capital to productivity. In augmented neoclassical and endogenous growth models, human capital is an accumulable input that raises output per worker directly (Lucas, 1988; Mankiw et al., 1992). In the tradition of Nelson and Phelps (1966), education instead raises the capacity of an economy to innovate and to absorb technologies developed elsewhere, so that the stock of human capital affects the growth rate of productivity rather than only its level (Romer, 1990; Benhabib & Spiegel, 1994). Vandenbussche et al. (2006) show that the composition of human capital matters as well: tertiary education is particularly productive in economies close to the technological frontier, where growth depends on innovation rather than imitation. This prediction is directly relevant for transition economies, which differ widely in their distance to the frontier. More recent evidence on absorptive capacity points in the same direction: for 14 emerging Asian economies, Lenkei et al. (2018) find a negative long-run effect of tertiary education on growth, which they attribute to the small share of tertiary-educated workers, limited opportunities to use their skills and emigration, whereas primary and secondary schooling have positive effects.

Both channels imply that the productivity effect of higher education materialises with a delay. Students must complete their programmes, enter the labour market and

accumulate experience before their skills are reflected in output, and firms must reorganise production to use them. A framework that distinguishes short-run adjustment from a long-run equilibrium relationship is therefore well suited to the question.

2.2. Empirical Evidence on Education and Productivity

Early cross-country growth regressions found a robust positive association between initial schooling and subsequent growth (Barro, 1991; Mankiw et al., 1992). Subsequent work questioned this result. Pritchett (2001) showed that increases in educational attainment were not associated with faster growth of output per worker, and Krueger and Lindahl (2001) attributed much of the fragility of macro-estimates to measurement error in schooling data. Once data quality is improved, the estimated returns to schooling at the macro level become larger and more stable (Cohen & Soto, 2007; de la Fuente & Doménech, 2006). Hanushek and Woessmann (2008) argue that cognitive skills, rather than years of schooling, explain cross-country growth differences, which implies that enrolment expansion without corresponding gains in quality may yield limited returns. Recent studies continue to document this tension. Angrist et al. (2021) show, using a new database of learning outcomes for 164 countries, that the global expansion of enrolment has not been matched by comparable gains in learning, and Égert et al. (2024) find that the effect of human capital on productivity operates mainly through quality and materialises only after long lags.

The cross-country evidence is therefore mixed, and the divergence has been traced to identifiable sources. Panel estimators that remove country fixed effects often yield insignificant or even negative coefficients on human capital (Islam, 1995), whereas cross-section estimates are typically positive. Delgado et al. (2014) find, using nonparametric methods, that average years of schooling has no significant effect on growth once other inputs are controlled for, while the meta-regression of Benos and Zotou (2014) shows that the estimated effect depends systematically on the education measure (enrolment, attainment or test scores), on whether the model is specified in levels or growth rates and on the estimator. A recurrent conclusion of this literature is that enrolment-based measures capture the flow into education rather than the stock of skills in the workforce, and that their association with productivity is weaker and less stable than that of attainment or cognitive-skill measures.

A smaller literature focuses on higher education and uses panel methods that allow for dynamics and heterogeneity. Bassanini and Scarpetta (2002) apply the PMG estimator to OECD countries and find that one additional year of average schooling raises long-run output per capita by about 6%. Gyimah-Brempong et al. (2006) report positive growth effects of higher education in Africa. Holmes (2013), by contrast, finds that neither the initial level nor the growth of higher education is significantly associated with growth in OECD or world samples, whereas technical skills and the employment of researchers are, and argues that the benefits of higher education depend on the skills it produces and on whether they are used in employment. At the regional level, Valero and Van Reenen (2019) show, for about 1500 regions in 78 countries, that a 10% increase in the number of universities per capita is associated with about 0.4% higher subsequent GDP per capita, partly through the supply of human capital and partly through innovation. Evidence that the payoff to higher education depends on whether graduates remain in and are used by the domestic economy also comes from the literature on high-skilled emigration (Docquier & Rapoport, 2012).

Methodologically, these studies differ in how they treat dynamics, slope heterogeneity and cross-sectional dependence, and their conclusions differ accordingly. System GMM estimators, widely used in growth panels (e.g., Nistor et al., 2018), are designed for many countries and few periods and impose common slopes; PMG and MG estimators exploit

longer time series and allow short-run heterogeneity (Bassanini & Scarpetta, 2002); and more recent studies use CS-ARDL estimators to account for unobserved common factors, often finding that human capital has a positive short-run but weaker or less direct long-run effect (Dahmani & Mabrouki, 2025). Because the choice of estimator matters, we report PMG, MG and DFE estimates together with CS-ARDL, CS-DL and CCE-MG estimates for the same data, so that the robustness of the conclusions to the estimator can be assessed directly.

2.3. Transition Economies and Uzbekistan

Transition economies inherited education systems designed for planned economies, with high formal attainment but skills that were often poorly matched to market demands. Micro-evidence documents that the returns to education rose substantially during the transition (Münich et al., 2005; Brunello et al., 2012). These findings suggest that the productivity content of a given level of enrolment varies across cohorts and institutional settings. Brunello et al. (2012) show that workers educated under communism earned returns to education in the 2000s similar to those of comparable Western European workers, but were more likely to be unemployed, retired or disabled, so that a large part of their education yielded no earnings at all. Education–job mismatch is also high and persistent in post-Soviet economies: Kupets (2016) documents an oversupply of tertiary graduates relative to skilled jobs in Ukraine, and the wider literature shows that such mismatch lowers the productivity returns to education (McGuinness et al., 2018). In Central Asia, higher education systems remained largely state-controlled after independence, and access was tightly rationed; in Uzbekistan, the number of places was kept low for two decades before the reforms that began in 2017 opened the sector to private and foreign providers (Ruziev & Burkhanov, 2018).

Despite the scale of these changes, macro-level evidence on the higher education–productivity relationship in transition economies is scarce, and existing panel studies seldom account for cross-sectional dependence, although common shocks—EU accession, commodity price cycles, the 2008–2009 crisis and the COVID-19 pandemic—affected these economies simultaneously. Ignoring such dependence can bias both inference and point estimates (Pesaran, 2006, 2015). Building on this literature, we test three hypotheses:

H1. *Tertiary enrolment and labour productivity are linked by a positive long-run relationship.*

H2. *The relationship differs between EU member states and non-EU transition economies.*

H3. *Because its expansion is recent, Uzbekistan deviates from the panel long-run relationship.*

3. Materials and Methods

3.1. Data and Sample

All data are taken from the World Development Indicators (World Bank, 2026) and were retrieved directly in StataNow 19 MP (StataCorp, College Station, TX, USA) with the *wbopendata* command. Table 1 defines the variables. Labour productivity (LP) is measured as GDP per person employed in constant 2021 purchasing power parity dollars. Human capital formation in higher education (TER) is measured by the gross tertiary enrolment ratio. We control for physical capital accumulation using gross fixed capital formation as a share of GDP (INV) and for population health using life expectancy at birth (LE). Trade openness is used in a robustness check. All variables, including the dependent variable, enter in natural logarithms, so that long-run coefficients are elasticities; cross-sectional demeaning (Section 3.3) is applied to the log-transformed series.

Table 1. Variables, definitions and sources.

Variable	Definition	WDI Code	Role
LP	GDP per person employed (constant 2021 PPP $)	SL.GDP.PCAP.EM.KD	Dependent
TER	School enrolment, tertiary (% gross)	SE.TER.ENRR	Main regressor
INV	Gross fixed capital formation (% of GDP)	NE.GDI.FTOT.ZS	Control
LE	Life expectancy at birth, total (years)	SP.DYN.LE00.IN	Control
TRADE	Trade (% of GDP)	NE.TRD.GNFS.ZS	Robustness

Source: World Bank (2026). All variables enter the models in natural logarithms.

The control variables follow from an augmented production function in which output per worker depends on physical capital, human capital and health (Mankiw et al., 1992; Bloom et al., 2004). The investment share proxies the rate of physical capital accumulation, which is the standard counterpart of the human capital term in the augmented Solow model; omitting it would attribute to enrolment the effect of capital deepening, with which enrolment is correlated over time. Life expectancy is included as a measure of health capital: healthier workers supply more effective labour and have longer working lives over which to use their education, and Bloom et al. (2004) show that life expectancy has a positive and significant effect on output in a production-function framework. Because life expectancy is strongly trending, we also report results without it. Trade openness captures the access to foreign technology and competition through which human capital may raise productivity (Frankel & Romer, 1999); because it is endogenous to productivity and strongly correlated with country size, it is used in a robustness check rather than in the baseline.

The initial sample comprised 28 transition economies. Four were excluded to obtain a balanced panel: Azerbaijan, Kazakhstan and Moldova, whose tertiary enrolment series begin only in 2006–2010 or contain long gaps before that, and Ukraine, whose productivity series ends in 2021. The baseline sample therefore contains 24 countries observed over 2001–2023 (552 observations). The sample ends in 2023 because this is the last year for which tertiary enrolment is available for all 24 economies: in the October 2026 update of the World Development Indicators, 2024 values are available for 16 of them and 2025 values for only three, and life expectancy is not yet available for 2025. Extending the baseline to 2024 would therefore require either dropping eight countries or extrapolating enrolment. Instead, we use the 2024 data in a robustness check for the 16 economies with complete data (Section 4.8). Isolated missing values of tertiary enrolment in North Macedonia (2011 and 2016), Russia (2010) and Tajikistan (2018–2019) were filled by linear interpolation; no values were extrapolated. Azerbaijan and Kazakhstan are added in a robustness check over 2006–2023, in which Kazakhstan's missing values for 2007–2009 are also interpolated. Apart from these five interpolated observations, the baseline panel contains no missing values, and no observations are dropped because of missing data; Table A1 in Appendix A reports the number and share of interpolated observations for each variable and sample. When lagged enrolment is used, the first observations of each country are lost and the panel becomes slightly unbalanced (Section 4.8).

The choice of the education measure deserves emphasis. The gross enrolment ratio measures how many people are currently studying, relative to the population of the corresponding age group; it does not measure how many workers have completed higher education, nor the skills they have acquired. Because graduates enter the labour market only several years after enrolment and then remain in it for decades, enrolment is at best a leading indicator of the future stock of educated workers, and it also includes part-time and distance students and students who do not graduate or who later emigrate. A long-run association between enrolment and productivity can therefore arise because enrolment anticipates changes in the educated stock, but also because both respond to common factors

such as income and institutional quality. We use enrolment because it is the only higher education indicator available annually for all 24 economies over 2001–2023: the World Bank attainment series (share of the population aged 25 and over that has completed at least short-cycle tertiary or at least bachelor's education) are available for only 15 and 10 of the 23 years on average, with between 1 and 23 observations per country, and the Barro and Lee (2013) attainment data are available only at five-year intervals and end in 2015. These gaps rule out the panel ARDL framework, but we use the attainment series in a fixed-effects check (Section 4.8). We address the timing problem directly by estimating models with enrolment lagged by one to four years (Section 4.8). The estimates should accordingly be read as associations between productivity and the scale of higher education participation, not as effects of the skills of the workforce.

3.2. Empirical Model

The long-run relationship of interest is

$$\ln LP_{it} = \theta_{0i} + \theta_1 \ln TER_{it} + \theta_2 \ln INV_{it} + \theta_3 \ln LE_{it} + u_{it} \quad (1)$$

where $i = 1, \ldots, N$ indexes countries and $t = 1, \ldots, T$ years. Embedding Equation (1) in an ARDL(p, q, q, q) model and reparameterising it yields the error-correction form:

$$\Delta \ln LP_{it} = \phi_i \left(\ln LP_{i,t-1} - \boldsymbol{\theta}_i' \mathbf{x}_{it} \right) + \sum_{j=1}^{p-1} \lambda_{ij} \Delta \ln LP_{i,t-j} + \sum_{j=0}^{q-1} \boldsymbol{\delta}_{ij}' \Delta \mathbf{x}_{i,t-j} + \mu_i + \varepsilon_{it} \quad (2)$$

where $\mathbf{x}_{it} = (\ln TER_{it}, \ln INV_{it}, \ln LE_{it})'$, $\boldsymbol{\theta}_i$ contains the long-run coefficients, $\boldsymbol{\delta}_{ij}$ the short-run coefficients and φ_i the speed of adjustment. A negative and significant φ_i implies that productivity returns to its long-run relationship after a shock, with a half-life of $\ln(0.5)/\ln(1 + \varphi_i)$ years. Given $T = 23$, we use an ARDL(1, 1, 1, 1) specification, which retains sufficient degrees of freedom for country-specific estimation. The lag order was chosen as follows. Country-by-country ARDL models with up to two lags, selected by the Schwarz Bayesian criterion (SBC), choose one lag of productivity in 16 of the 24 countries and at most one lag of enrolment in 21 of them; the PMG estimator requires a common lag order, and ARDL(1, 1, 1, 1) is the most parsimonious order that nests these choices for most countries while leaving 17 degrees of freedom per country. Because the Akaike criterion more often selects two lags, we also report ARDL(2, 1, 1, 1) and ARDL(1, 2, 2, 2) models (Section 4.8). Because the PMG likelihood is flat in some specifications, each model is estimated with both the Newton–Raphson and the modified (difficult) maximisation algorithm, and the solution with the higher log-likelihood is reported.

3.3. Estimation Strategy

The analysis proceeds in four steps. First, we test for cross-sectional dependence with the CD test of Pesaran (2015, 2020) and for slope homogeneity with the Δ test of Pesaran and Yamagata (2008). Second, we examine the order of integration with the first-generation panel unit root test of Im et al. (2003) and the second-generation CIPS test of Pesaran (2007), which is robust to cross-sectional dependence. ARDL models are valid for a mixture of I(0) and I(1) regressors but not for I(2) series. Third, we test for cointegration using the Kao (1999), Pedroni (2004) and Westerlund (2005) tests.

Fourth, we estimate Equation (2). The MG estimator (Pesaran & Smith, 1995) estimates separate regressions for each country and averages the coefficients; it is consistent under full heterogeneity but imprecise when T is small. The DFE estimator imposes homogeneity of all slope coefficients. The PMG estimator (Pesaran et al., 1999) constrains the long-run coefficients to be equal across countries while allowing intercepts, short-run coefficients, error variances and adjustment speeds to differ. The choice among the three

is guided by Hausman (1978) tests. Because the PMG, MG and DFE estimators assume cross-sectionally independent errors, the baseline estimates use cross-sectionally demeaned data, as suggested by Pesaran et al. (1999):

$$\tilde{z}_{it} = z_{it} - \frac{1}{N}\sum_{j=1}^{N} z_{jt} \tag{3}$$

for each variable z. Demeaning removes common shocks with homogeneous loadings, such as global business cycle fluctuations. For comparison, we also report conventional PMG estimates on the original data.

The PMG estimator was developed for panels in which both N and T are moderately large, and it has been applied to panels of similar dimensions, for example to 24 OECD countries over about 30 years in Pesaran et al. (1999) and to 21 OECD countries in Bassanini and Scarpetta (2002). With $N = 24$ and $T = 23$, the country-specific short-run coefficients are estimated from few observations, and the Hausman test of the long-run homogeneity restriction has limited power. The restriction is substantively strong: it assumes that all 24 economies share the same long-run elasticities, even though the Pesaran–Yamagata test rejects homogeneity of the slope coefficients as a whole. We therefore treat the pooled long-run coefficient as a cross-country average rather than as a structural parameter common to all countries, and we assess it against the MG estimator, which imposes no homogeneity, against the CCE-type estimators and against separate estimates for EU and non-EU economies.

Demeaning does not remove common factors with heterogeneous loadings. We therefore also estimate three estimators from the common correlated effects family, which augment each country regression with cross-sectional averages of the dependent and explanatory variables: the CS-ARDL estimator of Chudik and Pesaran (2015), the CS-DL estimator of Chudik et al. (2016) and the static CCE-MG estimator of Pesaran (2006). These estimators are robust to heterogeneous common factors, but, with $T = 23$, their country-level regressions have few degrees of freedom and their mean estimates are correspondingly imprecise.

Tertiary enrolment may be endogenous: richer and more productive economies can afford to expand higher education, and both variables may respond to omitted factors such as institutional quality. The PMG estimator does not solve this problem. The ARDL framework provides some protection, because with sufficiently augmented lags it yields consistent estimates of the long-run coefficients of a cointegrating relationship even when the regressors are endogenous (Pesaran & Shin, 1999; Pesaran et al., 1999), and the CS-ARDL estimator remains consistent when regressors are only weakly exogenous and there is feedback from the dependent variable (Chudik & Pesaran, 2015). We complement these properties with three checks: panel Granger non-causality tests in both directions (Dumitrescu & Hurlin, 2012), with bootstrap p-values that are robust to cross-sectional dependence (Lopez & Weber, 2017); a reverse PMG model in which enrolment is the dependent variable, which tests whether enrolment adjusts to deviations from a long-run relationship with productivity; and models in which enrolment enters with a lag. These checks can reveal reverse causality but cannot establish a causal effect of enrolment, and our estimates should be interpreted as long-run associations.

Finally, we estimate country-specific adjustment coefficients and short-run effects from the PMG model and conduct robustness checks that (i) replace current enrolment with enrolment four years earlier to reflect the time needed to complete a bachelor's degree, (ii) add trade openness, (iii) omit life expectancy, (iv) end the sample in 2019 to exclude the COVID-19 period, (v) estimate the model separately for non-EU transition economies and for EU member states, with demeaning within each group, and (vi) extend the sample to 26 countries over 2006–2023. In response to concerns about timing, endogeneity and stability, Section 4.8 adds (vii) alternative lag orders, (viii) enrolment lagged by one to three years, (ix) the endogeneity checks described above, (x) panel structural break tests

with unknown break dates (Ditzen et al., 2025; Karavias et al., 2023), which extend the approach of Bai and Perron (1998) to panels, together with estimates for sub-periods, and (xi) a fixed-effects check with educational attainment in place of enrolment. Country-level ARDL bounds tests for Uzbekistan use the ardl command (Kripfganz & Schneider, 2023; Pesaran et al., 2001). All estimations were carried out in StataNow 19 MP with the *xtpmg* (Blackburne & Frank, 2007), *xtdcce2* (Ditzen, 2018, 2021), *xtcd2*, *xthst* and *xtcips* commands. The complete do-file, which downloads the data and reproduces every table and figure, is available from the authors.

3.4. Software

All computations were carried out in StataNow 19 MP (StataCorp, College Station, TX, USA). The data were downloaded from the World Development Indicators with *wbopendata* (Azevedo, 2011). Built-in Stata commands were used for the Im–Pesaran–Shin unit root test (*xtunitroot ips*), the Kao, Pedroni and Westerlund cointegration tests (*xtcointtest*), the Hausman tests (*hausman*) and the interpolation of isolated missing values (*ipolate*). Community-contributed packages, installed from the Statistical Software Components (SSC) archive, were used as follows: *xtcd2* for the CD test of cross-sectional dependence; *xthst* (Bersvendsen & Ditzen, 2021) for the slope homogeneity test; *xtcips* (Burdisso & Sangiácomo, 2016) for the CIPS unit root test; *xtpmg* (Blackburne & Frank, 2007) for the PMG, MG and DFE estimators; *xtdcce2* (Ditzen, 2018, 2021) for the CS-ARDL, CS-DL and CCE-MG estimators and the residual CD tests; *ardl* (Kripfganz & Schneider, 2023) for the country-level ARDL models, lag selection and bounds tests; *xtgcause* (Lopez & Weber, 2017) for the Dumitrescu–Hurlin Granger causality tests; *xtbreak* (Ditzen et al., 2025) for the panel structural break tests; and *xtscc* (Hoechle, 2007) for the fixed-effects regressions with Driscoll–Kraay standard errors. The latest versions of all packages available in November 2026 were used.

4. Results

4.1. Descriptive Statistics

Table 2 reports descriptive statistics for the baseline sample. GDP per person employed ranges from about 4700 to 100,800 international dollars, and the tertiary enrolment ratio from 7.8% to 95.5%, reflecting the wide diversity of the sample. The pairwise correlation between ln *LP* and ln *TER* is 0.66, while ln *INV* is almost uncorrelated with the other variables. On average, enrolment rose from 46.1% to 70.9% in EU member states and from 31.2% to 61.3% in non-EU countries between 2001 and 2023.

Table 2. Descriptive statistics, 24 countries, 2001–2023 ($N = 552$).

Variable	Mean	SD	Min.	Max.
LP (PPP $)	52,642	23,872	4737	100,844
TER (%)	55.58	19.89	7.85	95.50
INV (% of GDP)	24.22	5.63	6.30	48.41
LE (years)	73.86	3.41	63.39	81.93
ln *LP*	10.719	0.625	8.463	11.521
ln *TER*	3.926	0.484	2.060	4.559
ln *INV*	3.161	0.232	1.840	3.880
ln *LE*	4.301	0.047	4.149	4.406

Pairwise correlations: ln *LP*-ln *TER* 0.658; ln *LP*-ln *INV* −0.039; ln *LP*-ln *LE* 0.665; ln *TER*-ln *INV* 0.011; ln *TER*-ln *LE* 0.360; ln *INV*-ln *LE* 0.010.

Figure 1a plots the two series for Uzbekistan, and Figure 1b plots the unweighted averages of the other 23 economies. Labour productivity rose continuously, from about

10,900 to 27,600 international dollars between 2001 and 2023, whereas tertiary enrolment declined from 13.9% in 2004 to 7.8% in 2012 and remained below 9% until 2017 before increasing sharply. The discontinuity in 2005 reflects a change in the enrolment series. The figure makes clear that Uzbekistan's productivity growth over most of the period occurred without any increase in tertiary participation. The contrast with the rest of the panel is marked. In the other 23 economies, the average enrolment ratio rose from 39.1% in 2001 to 60.2% in 2012 and 66.6% in 2023, and average productivity rose by 81% between 2001 and 2023, so that the two series moved together throughout the period (the correlation between their logarithms is 0.97). In Uzbekistan, productivity rose by 152% while enrolment first fell and then, after 2017, rose more than five-fold, and the correlation between the two log series is only 0.36. Uzbekistan thus combines the lowest initial enrolment ratio in the sample (13.3% in 2001) with the latest and steepest expansion, and over most of the period its productivity growth was unrelated to tertiary participation. This is why its country-specific estimates are examined in detail in Section 4.5, although it is estimated in exactly the same way as the other economies.

4.2. Cross-Sectional Dependence, Slope Heterogeneity and Unit Roots

Table 3 summarises the pre-tests. The CD statistics reject the null hypothesis of weak cross-sectional dependence for all four variables at the 1% level, and the Pesaran–Yamagata test rejects slope homogeneity ($\Delta = 3.792$; adjusted $\Delta = 4.447$; $p < 0.001$). Both findings support heterogeneous panel estimators and call for methods that account for cross-sectional dependence.

Table 3. Cross-sectional dependence and panel unit root tests.

Variable	CD Test	IPS Level	IPS First Diff.	CIPS Level	CIPS First Diff.
ln *LP*	74.70 ***	−1.152 (0.125)	−13.325 (0.000)	−2.681 **	−4.056 ***
ln *TER*	43.86 ***	−1.180 (0.119)	−6.396 (0.000)	−2.463	−2.873 ***
ln *INV*	13.08 ***	0.181 (0.572)	−12.582 (0.000)	−2.173	−4.069 ***
ln *LE*	70.01 ***	−5.032 (0.000)	−18.441 (0.000)	−2.632 *	−4.561 ***

CD: Pesaran (2015) test of weak cross-sectional dependence. IPS: W-t-bar statistic with *p*-values in parentheses; levels include a trend, lags selected by AIC (max. 2), cross-sectional means removed. CIPS: Pesaran (2007); levels with constant and trend (critical values −2.58, −2.66, and −2.81 at 10%, 5%, and 1%), first differences with constant (−2.07, −2.15, and −2.30). Slope homogeneity test (Pesaran & Yamagata, 2008): $\Delta = 3.792$, adjusted $\Delta = 4.447$, both $p < 0.001$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The IPS test does not reject a unit root in the levels of ln *LP*, ln *TER* and ln *INV* but rejects it for ln *LE*, whose trend-stationarity is consistent with the smooth evolution of life expectancy. The CIPS test, which allows for cross-sectional dependence, rejects the unit root for ln *LP* at the 5% level and for ln *LE* at the 10% level, but not for ln *TER* and ln *INV*. All first differences are stationary at the 1% level under both tests. The variables are therefore a mixture of I(0) and I(1) processes, with no evidence of I(2) behaviour, which is the setting in which ARDL-based estimators are appropriate.

4.3. Cointegration

Table 4 reports the cointegration tests. All five Kao statistics and all three Pedroni statistics reject the null hypothesis of no cointegration at the 5% level or better. The evidence from the Westerlund variance-ratio test, which allows for panel-specific cointegrating vectors, is weaker: it rejects the null only at the 10% level when a trend is included and does not reject it without a trend. On balance, the tests support a long-run relationship in the panel,

but the weaker Westerlund result counsels caution; the country-specific estimates below show that adjustment towards the long-run relationship is absent in several countries.

Table 4. Panel cointegration tests (ln *LP*, ln *TER*, ln *INV*, and ln *LE*).

Test and Statistic	Statistic	*p*-Value
Kao (1999)		
Modified Dickey–Fuller *t*	−2.042	0.021
Dickey–Fuller *t*	−3.154	0.001
Augmented Dickey–Fuller *t*	−4.154	0.000
Unadjusted modified Dickey–Fuller *t*	−2.099	0.018
Unadjusted Dickey–Fuller *t*	−3.182	0.001
Pedroni (2004), with trend		
Modified Phillips–Perron *t*	3.481	0.000
Phillips–Perron *t*	−2.224	0.013
Augmented Dickey–Fuller *t*	−2.188	0.014
Westerlund (2005)		
Variance ratio, no trend	−0.522	0.301
Variance ratio, with trend	1.399	0.081

H0: no cointegration; Ha: all panels are cointegrated (as implemented in the xtcointtest command of StataNow 19 MP). Kao: common cointegrating vector; Pedroni and Westerlund: panel-specific cointegrating vectors.

4.4. Panel ARDL Estimates

Table 5 presents the main results. Columns (1)–(3) use cross-sectionally demeaned data; column (4) reports the conventional PMG estimator on the original data. The Hausman test does not reject the long-run homogeneity restriction of the PMG estimator against the MG estimator ($\chi^2(3) = 3.57$, $p = 0.312$). The Hausman comparison of PMG and DFE does not reject the additional homogeneity restrictions of the DFE estimator either ($\chi^2(3) = 0.02$, $p = 0.999$), but these restrictions extend to the short-run coefficients and adjustment speeds, which the Pesaran–Yamagata test rejects, and the DFE residuals remain cross-sectionally correlated. The PMG estimator on demeaned data is therefore our preferred specification. The CD test on its residuals no longer rejects cross-sectional independence at the 5% level (CD = −1.73, $p = 0.084$), in contrast to the conventional PMG estimator (CD = 20.42, $p < 0.001$).

In the preferred specification, the long-run elasticity of labour productivity with respect to tertiary enrolment is 0.103 and significant at the 1% level: a 10% increase in the gross enrolment ratio (for example, from 40% to 44%) is associated with a long-run increase in labour productivity of about 1.0%. The long-run elasticity with respect to the investment share is 0.176 ($p < 0.01$), whereas life expectancy is not significant. The error-correction coefficient is −0.167 ($p < 0.01$), implying that about 17% of a deviation from the long-run relationship is corrected each year and that the half-life of a deviation is roughly 3.8 years. None of the short-run coefficients is significant: in this specification, changes in enrolment are associated with productivity through the long-run relationship rather than contemporaneously, which is consistent with the delay implied by theory. Sections 4.6–4.8 examine how the size and significance of this elasticity depend on the estimator, the sample period and the timing of the enrolment variable.

The comparison with column (4) illustrates the consequences of ignoring cross-sectional dependence. The conventional PMG estimator yields a larger enrolment elasticity (0.305), a negative investment coefficient and a large negative coefficient on life expectancy (−4.854), a combination that is difficult to interpret economically and that coincides with

strongly cross-correlated residuals. The MG estimates are imprecise, as expected with $T = 23$, but their enrolment coefficient (0.313) has the same sign as the PMG estimate.

Table 5. Panel ARDL(1, 1, 1, 1) estimates. Dependent variable: $\Delta \ln LP$.

	(1) PMG	(2) MG	(3) DFE	(4) PMG, Raw Data
		Long-run coefficients		
ln *TER*	0.103 ***	0.313	−0.031	0.305 ***
	(0.030)	(0.198)	(0.109)	(0.068)
ln *INV*	0.176 ***	−0.120	0.307 ***	−0.183 *
	(0.038)	(0.341)	(0.114)	(0.106)
ln *LE*	−0.598	8.324 *	0.243	−4.854 **
	(0.517)	(4.545)	(1.910)	(1.913)
		Short-run coefficients		
Error-correction term	−0.167 ***	−0.285 ***	−0.071 ***	−0.074 ***
	(0.037)	(0.044)	(0.013)	(0.014)
Δln *TER*	0.018	−0.049	0.019	0.022
	(0.038)	(0.042)	(0.020)	(0.035)
Δln *INV*	−0.005	−0.025	−0.010	0.090 ***
	(0.016)	(0.018)	(0.014)	(0.027)
Δln *LE*	−0.066	−0.300	−0.488 **	0.347
	(0.246)	(0.353)	(0.231)	(0.326)
Constant	−0.015	−0.015	0.000	2.321 ***
	(0.033)	(0.039)	(0.001)	(0.434)
Observations/countries	528/24	528/24	528/24	528/24
Residual CD (*p*-value)	−1.73 (0.084)	−1.53 (0.126)	−2.70 (0.007)	20.42 (0.000)
Hausman $\chi^2(3)$ (*p*-value)	-	MG vs. PMG: 3.57 (0.312)	PMG vs. DFE: 0.02 (0.999)	-

Columns (1)–(3): variables cross-sectionally demeaned. Column (4): conventional PMG on original data. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4.5. Country-Specific Adjustment and the Case of Uzbekistan

Figure 2 and Table 6 report the country-specific error-correction coefficients from the PMG model. The adjustment coefficient is negative and significant at the 5% level in 12 of the 24 countries, and at the 10% level in Mongolia. Adjustment is fastest in Albania, Bosnia and Herzegovina, and Kyrgyzstan, where the half-life of a deviation is less than one year, and slowest or absent in Russia, Serbia, North Macedonia, Estonia, Georgia, Montenegro and Tajikistan, where the coefficients are small and insignificant.

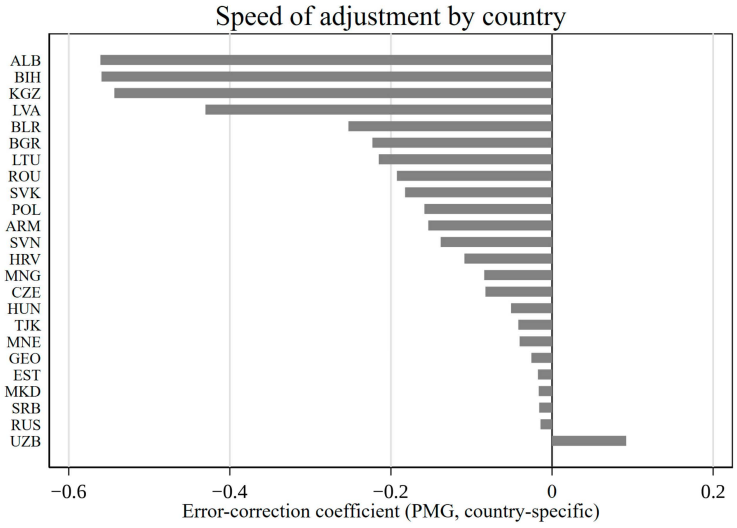


Figure 2. Country-specific error-correction coefficients from the PMG model on cross-sectionally demeaned data (Table 5, column 1). Negative values indicate adjustment towards the long-run relationship; standard errors are reported in Table 6.

Table 6. Country-specific error-correction and short-run coefficients (PMG, demeaned data).

Country	ECT	Δln *TER*	Δln *INV*	Half-Life (Years)
Albania	−0.561 *** (0.186)	−0.100 (0.108)	−0.083 (0.137)	0.8
Armenia	−0.154 *** (0.057)	−0.200 (0.219)	0.016 (0.092)	4.2
Belarus	−0.253 ** (0.124)	0.186 (0.131)	0.087 (0.058)	2.4
Bosnia and Herzegovina	−0.559 *** (0.159)	0.054 (0.075)	−0.162 *** (0.048)	0.8
Bulgaria	−0.223 ** (0.103)	−0.143 (0.133)	−0.085 (0.060)	2.7
Croatia	−0.109 ** (0.045)	0.195 (0.210)	0.090 (0.076)	6.0
Czechia	−0.083 (0.070)	0.150 (0.131)	−0.019 (0.105)	8.0
Estonia	−0.017 (0.134)	0.461 * (0.264)	0.098 (0.071)	39.5
Georgia	−0.026 (0.026)	−0.033 (0.061)	0.014 (0.058)	26.8
Hungary	−0.051 ** (0.025)	0.171 *** (0.052)	0.094 ** (0.041)	13.3
Kyrgyzstan	−0.543 *** (0.134)	−0.152 (0.140)	−0.146 ** (0.061)	0.9
Latvia	−0.430 *** (0.129)	0.073 (0.076)	−0.015 (0.042)	1.2
Lithuania	−0.215 ** (0.091)	−0.216 * (0.122)	0.066 (0.058)	2.9
Mongolia	−0.084 * (0.050)	−0.065 (0.210)	−0.046 (0.042)	7.9
Montenegro	−0.040 (0.033)	−0.042 (0.091)	−0.037 (0.051)	16.9
North Macedonia	−0.016 (0.040)	−0.066 (0.087)	−0.012 (0.072)	41.8
Poland	−0.158 ** (0.079)	0.320 (0.215)	0.001 (0.109)	4.0
Romania	−0.193 (0.138)	0.170 ** (0.086)	−0.136 (0.092)	3.2
Russia	−0.014 (0.086)	0.006 (0.026)	−0.014 (0.056)	48.2
Serbia	−0.016 (0.066)	−0.288 (0.195)	0.033 (0.089)	43.4
Slovakia	−0.182 (0.155)	0.175 (0.124)	0.060 (0.062)	3.4
Slovenia	−0.138 ** (0.064)	−0.233 (0.143)	−0.016 (0.089)	4.7
Tajikistan	−0.042 (0.029)	0.023 (0.104)	−0.013 (0.025)	16.2
Uzbekistan	0.092 (0.057)	−0.022 (0.037)	0.103 ** (0.045)	-

Standard errors in parentheses. Half-life = $\ln(0.5)/\ln(1 + \text{ECT})$; not defined for a positive ECT. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Uzbekistan is the only country with a positive adjustment coefficient (0.092, standard error 0.057, $p = 0.104$). Because the coefficient is not statistically different from zero at the 5% level, the appropriate conclusion is that there is no evidence of error correction for Uzbekistan, not that its productivity diverges from the long-run relationship. Country-level ARDL bounds tests point in the same direction: they do not reject the null hypothesis of no level relationship between productivity, enrolment and investment in Uzbekistan ($F = 1.36$, $t = 0.99$; with life expectancy added, $F = 3.50$, $t = -2.20$; all $p > 0.10$ for the I(1) bounds), although with 22 annual observations, these tests have low power. Several features of the Uzbek data account for this result. First, enrolment and productivity moved in opposite directions for most of the sample (Figure 1a): between 2001 and 2012 enrolment fell from 13.3% to 7.8% while productivity rose by 57%, so that, under a positive long-run enrolment coefficient, the gap between actual productivity and the level implied by the panel relationship widened year after year, and there was no deviation for adjustment to close. Second, productivity growth in this period was probably driven largely by factors outside the model, such as capital accumulation, structural change away from agriculture and favourable commodity prices, although we do not test these explanations. Third, the enrolment boom began only in 2018–2019, so that most of the additional students had not graduated by 2023. Fourth, the enrolment series contains a break in 2005. In the short run, the change in enrolment has no significant effect on productivity in Uzbekistan (−0.022, $p = 0.555$), whereas the change in the investment share has a positive and significant effect (0.103, $p = 0.020$). Over 2001–2023, productivity growth in Uzbekistan was thus associated with capital accumulation rather than with the expansion of higher education, which supports hypothesis H3.

4.6. Estimators Robust to Heterogeneous Common Factors

Table 7 reports the CS-ARDL, CS-DL and CCE-MG estimates, which allow for unobserved common factors with country-specific loadings. All four specifications pass the CD test on residuals. The mean long-run enrolment coefficient is positive in all specifications (0.068–0.086), close in magnitude to the PMG estimate, but it is not statistically significant; the CCE-MG coefficients come closest ($p \approx 0.15$). The CS-ARDL model implies fast adjustment (−0.724), and the investment coefficient is small and insignificant in all specifications. These results indicate that the positive pooled association is not an artefact of common shocks, but that the evidence for a positive mean effect is weak once full slope heterogeneity is allowed and each country's coefficients are estimated from 23 annual observations. The CS-ARDL estimates are also sensitive to specification: with two lags of the cross-sectional averages, the mean enrolment coefficient becomes negative (−0.234, standard error 0.258), and with enrolment lagged by one year, it is 0.297 (standard error 0.224); neither is significant. The significance of the baseline PMG elasticity therefore rests on the pooling restriction.

Table 7. Estimators robust to heterogeneous common factors. Dependent variable: ln *LP*.

	(1) CS-ARDL	(2) CS-DL	(3) CCE-MG	(4) CCE-MG + *LE*
ln *TER* (long run)	0.086 (0.180)	0.068 (0.083)	0.077 (0.053)	0.073 (0.051)
ln *INV* (long run)	−0.011 (0.079)	0.030 (0.042)	0.009 (0.031)	0.004 (0.033)
ln *LE*	-	-	-	0.003 (0.483)
Adjustment coefficient	−0.724 *** (0.073)	-	-	-
Observations	504	480	552	552
Cross-sectional averages (lags)	1	2	0	0
Residual CD (*p*-value)	−0.03 (0.979)	−1.40 (0.162)	−0.95 (0.342)	−1.08 (0.279)

Mean group estimates with cross-sectional averages of all model variables (Chudik et al., 2016; Chudik & Pesaran, 2015; Pesaran, 2006), estimated with *xtdcce2* (Ditzen, 2018, 2021). CS-ARDL is an ARDL(1, 1, 1) model; CS-DL includes current first differences of the regressors. Standard errors in parentheses. *** $p < 0.01$.

4.7. Robustness Checks

Table 8 summarises the robustness checks, all estimated with the PMG estimator on demeaned data. The long-run enrolment elasticity remains positive and significant when trade openness is added (0.050), when life expectancy is omitted (0.113), when the sample ends in 2019 (0.488) and when Azerbaijan and Kazakhstan are added over 2006–2023 (0.093). The investment elasticity is positive and significant in all these specifications. When current enrolment is replaced with enrolment four years earlier, the error-correction coefficient becomes indistinguishable from zero (−0.001), so the long-run coefficients are not identified; Section 4.8 shows that this is not specific to the four-year lag.

The size of the elasticity varies considerably across these specifications, from 0.050 to 0.488, and these differences should not be dismissed because all estimates are positive. The largest estimate, for 2001–2019, is accompanied by a much slower adjustment (−0.079) and a large coefficient on life expectancy (5.641), which indicates that in the shorter sample the long-run coefficients are less well determined and partly absorb the common trend in health. The difference also reflects the post-2019 period, in which the pandemic depressed

measured productivity while enrolment accelerated sharply in several non-EU countries, including Uzbekistan; adding these years flattens the estimated association. When the sample is restricted to 2010–2023 or the crisis years 2008–2010 are excluded, the elasticity is 0.113 and 0.126, respectively, close to the baseline (Table 9). We therefore interpret the robustness checks as supporting the sign of the pooled association in specifications with contemporaneous enrolment, but not a precise magnitude.

Table 8. Robustness checks: PMG estimates on cross-sectionally demeaned data.

Specification	ln *TER*	ln *INV*	ln *LE*	ECT	*N*	Residual CD
Baseline, 24 countries	0.103 *** (0.030)	0.176 *** (0.038)	−0.598 (0.517)	−0.167 *** (0.037)	528	−1.73 *
Enrolment at $t-4$ [1]	n.i.	n.i.	n.i.	−0.001 (0.001)	474	−0.77
Adding ln *TRADE* [2]	0.050 *** (0.011)	0.530 *** (0.025)	1.161 *** (0.386)	−0.105 *** (0.035)	528	−1.03
Without ln *LE*	0.113 *** (0.026)	0.169 *** (0.037)	-	−0.172 *** (0.039)	528	−2.06 **
Sample 2001–2019	0.488 *** (0.049)	0.362 *** (0.054)	5.641 *** (0.866)	−0.079 *** (0.028)	432	−2.03 **
Non-EU economies (13)	−0.279 *** (0.107)	0.587 *** (0.191)	10.039 *** (2.486)	−0.046 ** (0.020)	286	−2.42 **
EU member states (11)	0.215 *** (0.055)	0.244 *** (0.064)	−1.490 (0.917)	−0.188 *** (0.050)	242	−2.54 **
26 countries, 2006–2023	0.093 *** (0.036)	0.084 ** (0.041)	0.447 (0.605)	−0.246 *** (0.036)	442	−1.70 *

Long-run coefficients and error-correction term (ECT); standard errors in parentheses. In the group subsamples, variables are demeaned within each group. Non-EU: Albania, Armenia, Belarus, Bosnia and Herzegovina, Georgia, Kyrgyzstan, Mongolia, Montenegro, North Macedonia, Russia, Serbia, Tajikistan, and Uzbekistan. [1] The ECT is not significant, so the long-run coefficients are not identified (n.i.). [2] Long-run coefficient on ln *TRADE*: −0.263 *** (0.011). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9. Additional robustness checks: PMG estimates on cross-sectionally demeaned data.

Specification	ln *TER*	ln *INV*	ln *LE*	ECT	*N*
Baseline ARDL(1, 1, 1, 1) (Table 5)	0.103 *** (0.030)	0.176 *** (0.038)	−0.598 (0.517)	−0.167 *** (0.037)	528
ARDL(2, 1, 1, 1)	n.i.	n.i.	n.i.	≈0	504
ARDL(1,2,2,2)	0.110 *** (0.020)	0.306 *** (0.037)	−1.528 *** (0.327)	−0.195 *** (0.042)	504
Window 2010–2023	0.113 *** (0.020)	−0.052 (0.043)	−2.808 *** (0.365)	−0.319 *** (0.058)	312
Excluding 2008–2010	0.126 *** (0.025)	0.245 *** (0.039)	−0.993 * (0.542)	−0.164 *** (0.042)	456
Excluding Uzbekistan (23)	0.119 *** (0.034)	0.187 *** (0.042)	−0.818 (0.527)	−0.179 *** (0.036)	506
Excluding Tajikistan (23)	0.099 *** (0.030)	0.149 *** (0.038)	−0.920 * (0.513)	−0.176 *** (0.039)	506
16 countries, 2001–2023	n.i.	n.i.	n.i.	≈0	352
16 countries, 2001–2024	0.673 *** (0.087)	0.337 *** (0.044)	0.382 (0.822)	−0.100 *** (0.026)	368
Enrolment at $t-1$	−0.235 *** (0.056)	0.416 *** (0.067)	5.392 *** (1.281)	−0.079 *** (0.025)	516
Enrolment at $t-2$	−0.293 *** (0.046)	0.355 *** (0.048)	−1.953 * (1.031)	−0.104 *** (0.027)	502
Enrolment at $t-3$	n.i.	n.i.	n.i.	≈0	488
Enrolment at $t-4$ (Table 8)	n.i.	n.i.	n.i.	−0.001 (0.001)	474

Long-run coefficients and error-correction term (ECT); standard errors in parentheses. n.i.: not identified, because the pooled ECT converges to zero and the long-run coefficients are not determined. Windows and country exclusions are re-demeaned within the sub-sample; in the specification excluding 2008–2010, 2007 and 2011 are treated as adjacent periods. In the enrolment-lag rows, the enrolment variable is replaced by its lag. Each model was estimated with the Newton–Raphson and the modified maximisation algorithm; the solution with the higher log-likelihood is reported. *** $p < 0.01$, * $p < 0.10$.

The most informative check splits the sample. Among the 11 EU member states, the enrolment elasticity is 0.215 ($p < 0.01$), about twice the baseline estimate. Among

the 13 non-EU transition economies—the Western Balkans, the former Soviet Union and Mongolia—it is negative (-0.279, $p < 0.01$), and the adjustment coefficient is small (-0.046). Higher enrolment is thus associated with higher productivity among EU member states, but not in the non-EU group to which Uzbekistan belongs; the estimates do not show which features of the EU economies account for this difference. This supports hypothesis H2. The difference between the two group elasticities (0.494) is statistically significant ($z = 4.11$, $p < 0.001$). It does not arise because enrolment in the non-EU group grew more slowly: between 2001 and 2023, enrolment rose on average by 76 log points in the non-EU economies and by 46 log points in the EU members, while productivity rose by 83 and 55 log points, respectively. Within each group, the long differences (changes between 2001 and 2023) in enrolment and productivity are essentially uncorrelated across countries ($r = -0.14$ in the non-EU group and 0.11 in the EU group, both insignificant). The negative non-EU elasticity therefore reflects within-country co-movement over time: several economies with the largest increases in enrolment, such as Montenegro, Albania, and Bosnia and Herzegovina, recorded moderate productivity growth, whereas Georgia, Armenia and Tajikistan combined rapid productivity growth with moderate enrolment growth. The model does not identify why this pattern differs between the groups; possible explanations are discussed in Section 5.

4.8. Lag Structure, Timing of Enrolment, Endogeneity and Structural Stability

Table 9 reports additional PMG estimates on cross-sectionally demeaned data that address the lag structure, the stability of the long-run relationship, the influence of individual countries and the timing of the enrolment effect. The baseline elasticity is confirmed in the ARDL(1, 2, 2, 2) model (0.110, $p < 0.01$), in the 2010–2023 window (0.113, $p < 0.01$), when the crisis years 2008–2010 are excluded (0.126, $p < 0.01$), and when Uzbekistan (0.119, $p < 0.01$) or Tajikistan (0.099, $p < 0.01$) is excluded, in each case with significant error correction between -0.16 and -0.32. The result is therefore not driven by the global financial crisis or by the two economies with the weakest adjustment. Adding a second lag of productivity, by contrast, leaves the adjustment coefficient indistinguishable from zero, so that no long-run relationship is identified in the ARDL(2, 1, 1, 1) model. To use the most recent data, we also estimate the model for the 16 economies with complete data for 2024 (Albania, Armenia, Belarus, Bulgaria, Estonia, Hungary, Kyrgyzstan, Latvia, Lithuania, Mongolia, Poland, Russia, Serbia, Slovakia, Tajikistan and Uzbekistan). Over 2001–2024, the enrolment elasticity is 0.673 ($p < 0.01$), and deviations from equilibrium are corrected at 10% per year, whereas for the same 16 economies over 2001–2023, the adjustment coefficient converges to zero. Adding the latest year therefore strengthens rather than weakens the positive long-run association; the larger magnitude reflects the smaller sample and the sharp increases in enrolment recorded in 2024 in several economies, including Albania and Uzbekistan, and should be interpreted with caution.

The timing of the enrolment variable matters. When enrolment enters with a lag of one or two years, the long-run coefficient becomes negative (-0.235 and -0.293), and with a lag of three or four years, no long-run relationship is identified. The positive pooled elasticity is therefore a property of the contemporaneous co-movement of enrolment and productivity rather than of a mechanism in which cohorts of graduates raise productivity once they have entered the labour market. This pattern is consistent with the central argument of the paper: periods of rapid enrolment growth, which in many economies came from private, part-time and distance provision, were not followed by faster productivity growth, so the size of higher education participation is a weak predictor of future productivity. There is some evidence of a short-lived delayed effect in the short run, as the one-year lag of the change in enrolment is positive and significant in the ARDL(1, 2, 2, 2) model (0.085, $p = 0.006$).

Table 10 reports the endogeneity checks. The Dumitrescu–Hurlin tests do not reject the null hypothesis of Granger non-causality from productivity growth to enrolment growth, with one or two lags, and the reverse PMG model with enrolment as the dependent variable yields an insignificant long-run coefficient on productivity (7.49, standard error 4.97) and an adjustment coefficient close to zero (-0.009, $p = 0.055$). Enrolment therefore does not adjust to deviations from a long-run relationship with productivity, which is consistent with treating it as weakly exogenous in the PMG model and suggests that reverse causality is unlikely to be the main source of the pooled association. With T = 23, however, these tests have limited power, and they cannot exclude feedback from productivity to enrolment or a common response of both variables to omitted factors. The tests also do not detect Granger causality from enrolment growth to productivity growth at short lags, which is consistent with the absence of significant contemporaneous short-run effects in Table 5.

Table 10. Endogeneity checks and attainment-based estimates.

Test/Specification	Statistic	Std. Error/*p*-Value	*N*
A. Dumitrescu–Hurlin test			
H0: Δln *TER* does not cause Δln *LP*, 1 lag	Z-bar = 0.070	$p = 0.950$	528
H0: Δln *TER* does not cause Δln *LP*, 2 lags	Z-bar = 1.046	$p = 0.487$	528
H0: Δln *LP* does not cause Δln *TER*, 1 lag	Z-bar = 1.442	$p = 0.253$	528
H0: Δln *LP* does not cause Δln *TER*, 2 lags	Z-bar = 1.102	$p = 0.485$	528
B. Reverse PMG (dependent: ln _TER_)			
ln *LP* (long run)	7.492	(4.966)	528
Error-correction term	−0.009 *	(0.004)	528
C. Fixed effects, Driscoll–Kraay			
Attainment: at least short-cycle tertiary	0.021	(0.066)	362
Enrolment, same sample	0.113 ***	(0.031)	362
Attainment: at least bachelor's	−0.179 ***	(0.013)	245
Enrolment, same sample	0.119 **	(0.045)	245

Panel A: Dumitrescu and Hurlin (2012) test on first differences of the demeaned series, 2002–2023; *Z*-bar statistic with bootstrap *p*-values (499 replications) in the third column. Panel B: PMG ARDL(1, 1, 1, 1) with ln *TER* as the dependent variable. Panel C: Two-way fixed-effects regressions of ln *LP* on the education measure, ln *INV* and ln *LE* with Driscoll–Kraay standard errors; the enrolment rows use the same observations as the attainment rows above them. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The panel structural break test of Ditzen et al. (2025) on the demeaned data rejects the null hypothesis of no break ($UD\max = 41.07$; 1% critical value 5.87), and the sequential test selects two breaks, estimated in 2005 (95% confidence interval 2004–2006) and 2012 (2011–2013). The first coincides with the change in the Uzbek enrolment series and the second with the aftermath of the global financial crisis. The sub-period estimates in Table 9 show that the enrolment elasticity after the second break (0.113 for 2010–2023) and in the sample without the crisis years (0.126) is close to the baseline, so the long-run association is stable in sign and magnitude across these periods; the larger estimate for 2001–2019 in Table 8 reflects the slower adjustment in that window.

Finally, we replace enrolment with educational attainment. Because the World Bank attainment series are incomplete, we estimate static two-way fixed-effects models with Driscoll–Kraay standard errors, which are robust to cross-sectional dependence, on the

available observations. The share of the population aged 25 and over with at least short-cycle tertiary education is not significantly associated with productivity (0.021, standard error 0.066; 362 observations), and the share with at least a bachelor's degree has a negative coefficient (−0.179, standard error 0.013; 245 observations), whereas enrolment remains positive and significant in the same samples (0.113 and 0.119). Measured attainment of the adult population changes slowly and is recorded with gaps, so these estimates are imprecise guides to the long-run relationship; they nevertheless confirm that the positive association documented in this paper concerns participation in higher education rather than the stock of tertiary-educated adults.

5. Discussion

The results support a qualified version of the human capital view of productivity. In the pooled sample, tertiary enrolment and labour productivity are linked by a positive long-run relationship with an elasticity of about 0.10, which remains positive and significant, although its size varies between 0.05 and 0.49, when trade openness is added, life expectancy is omitted, the lag order of the regressors is increased, crisis years or individual countries are excluded, or the sample is extended. The qualification is that this association is contemporaneous: it does not carry over to lagged enrolment or to attainment-based measures, and it is imprecise under fully heterogeneous estimators. The evidence is therefore consistent with higher education participation moving together with productivity in the long run, but not with the view that expanding enrolment is by itself sufficient to raise future productivity. The absence of significant short-run effects and the moderate speed of adjustment are consistent with theories in which education affects productivity through the gradual accumulation of skills in the workforce and through the adoption of technology (Nelson & Phelps, 1966; Ruziyev et al., 2026; Yusupov et al., 2026a), rather than through an immediate change in output.

The pooled relationship, however, conceals systematic heterogeneity, which is the central finding of the paper. The positive association is driven by EU member states, while the association is negative among non-EU transition economies. This contrast echoes the macro-level evidence that the growth effects of education depend on its quality (Achilova et al., 2026; Hanushek & Woessmann, 2008, 2012) and the finding that tertiary education is most productive close to the technological frontier (Vandenbussche et al., 2006; Yusupov et al., 2026b). It is also consistent with micro-evidence from transition economies on the conditions under which education is rewarded (Brunello et al., 2012). In economies where rapid expansion is accompanied by limited quality assurance, weak links between universities and employers, and slow creation of skilled jobs, graduates may be employed in occupations that do not use their skills, or may emigrate, so that higher enrolment does not raise the productivity of the domestic workforce. This contrast reproduces, within one group of economies, the difference between studies of advanced and of emerging economies; it is compared with these studies, together with the other findings, in Table 11 below. The negative estimate for the non-EU group should not be read as evidence that higher education reduces productivity but as an indication that, over the sample period, enrolment in these economies expanded fastest where productivity gains were smallest. Several mechanisms could produce this pattern, and our data do not allow us to distinguish between them. EU accession tied the expansion of higher education to common quality-assurance frameworks and to integration into European value chains that employ graduates; non-EU economies expanded enrolment largely through private, part-time and distance provision with weaker quality control; graduates from non-EU economies are more likely to emigrate (Docquier & Rapoport, 2012) or to work in occupations that do not use their qualifications; and many of these economies are further from the technological frontier, where tertiary education

contributes less to growth (Vandenbussche et al., 2006). These are hypotheses consistent with the results, not findings of the analysis; testing them requires data on education quality, graduate employment and migration that are not available for the full panel.

Table 11. Comparison of the main findings with previous studies.

Study	Sample and Period	Education Measure; Method	Main Result	Comparison with This Study
Bassanini and Scarpetta (2002)	21 OECD countries, 1971–1998	Average years of schooling; PMG	One additional year of schooling raises long-run output per capita by about 6%	Same sign as the pooled (0.103) and EU (0.215) elasticities; magnitudes not directly comparable (stock vs. participation)
Holmes (2013)	OECD and world samples, since the 1960s	Level and increase in higher education; growth regressions	No significant relation with growth; employed researchers predict growth	Agrees with the insignificant CS-ARDL, CS-DL and CCE-MG estimates (Table 7)
Delgado et al. (2014)	Cross-country growth data, five schooling databases	Mean years of schooling; nonparametric regression	Schooling not a statistically relevant variable; test scores more informative	Agrees with the sensitivity of the enrolment effect to the estimator and with the attainment results (Table 10)
Lenkei et al. (2018)	14 Asian economies, 1960–2013	Education by level; heterogeneous panel ECM with cross-sectional dependence	Positive long-run effect of primary and secondary, negative effect of tertiary education	Same sign as the non-EU elasticity (−0.279); similar suggested explanations (absorption and brain drain)
Islam (1995)	Cross-country panel, 1960–1985	Average years of schooling; fixed-effects panel	Human capital insignificant or negative once country effects are included	Agrees with the fixed-effects attainment estimates (Table 10, Panel C)
Benos and Zotou (2014)	Meta-analysis of growth studies	Various measures and estimators	Estimated effects depend on the education measure, specification and estimator	Consistent with the differences between contemporaneous enrolment, lagged enrolment and attainment
Égert et al. (2024)	OECD countries	Quantity and quality (PISA and PIAAC) of human capital; productivity regressions	Quality three to four times as important as quantity; effects appear with long lags	Consistent with the absence of a positive effect of lagged enrolment and with the quality hypothesis for the EU/non-EU contrast
Dahmani and Mabrouki (2025)	MENA countries	Human capital; CS-ARDL	Positive short-run effect; long-run effect weaker and less direct	Agrees with the imprecise CS-ARDL long-run estimate and the positive lagged short-run effect (Table 9)
Valero and Van Reenen (2019)	About 1500 regions in 78 countries, 1950–2010	Universities per capita; regional fixed effects	10% more universities per capita: about 0.4% higher future regional GDP per capita	A small and delayed effect would not yet be visible in Uzbek aggregate data for 2018–2023

Studies are ordered by the finding of this paper to which they relate. PMG: pooled mean group; ECM: error-correction model; CS-ARDL: cross-sectionally augmented ARDL.

The weaker results from the CCE-type estimators point in the same direction. When each country is allowed its own slope and its own exposure to unobserved common factors, the mean enrolment effect remains positive but becomes imprecise, because the country-specific effects are dispersed around the mean. The PMG estimator obtains precision by pooling the long-run coefficient, and the Hausman test does not reject this restriction; the subsample results show, however, that the restriction is least tenable across groups of countries with different institutional settings. The pooled elasticity is therefore best interpreted as an average that conceals economically important differences.

Table 11 compares the main findings with those of earlier studies and indicates why they may differ. Three dimensions account for most of the differences: the education measure (enrolment ratios here; average years of schooling, attainment shares or test scores in most other studies), the sample (transition economies here; OECD, emerging or world samples elsewhere) and the estimator (pooled or fully heterogeneous, with or without allowance for cross-sectional dependence). First, the positive pooled elasticity agrees in sign with the PMG estimates of Bassanini and Scarpetta (2002) for 21 OECD countries over 1971–1998, in which one additional year of average schooling raises long-run output per capita by about 6%. Because they measure the stock of schooling and we measure participation, the two magnitudes cannot be compared directly, but in both studies the significance of the estimate rests on the same long-run pooling restriction. Second, when this restriction is relaxed, our results move towards the more sceptical part of the literature: the insignificant CCE-type estimates agree with Holmes (2013), who finds that neither the level nor the increase in higher education is significantly related to growth in OECD and world samples, and with Delgado et al. (2014), who find that mean years of schooling is not a statistically relevant variable in nonparametric growth regressions. Third, the EU/non-EU contrast mirrors the difference between studies of advanced and emerging economies. The EU elasticity (0.215) is in line with the positive OECD evidence, whereas the negative non-EU elasticity (−0.279) has the same sign as the negative long-run effect of tertiary education that Lenkei et al. (2018) estimate for 14 Asian economies over 1960–2013 with an error-correction model that also allows for cross-sectional dependence. They attribute this result to the small share of tertiary graduates, limited opportunities for highly educated workers and brain drain, which are also among the candidate explanations for our non-EU group. Fourth, the absence of a positive association for lagged enrolment and for attainment agrees with the meta-analysis of Benos and Zotou (2014), in which the estimated effects depend systematically on the education measure, with the insignificant or negative human capital coefficients obtained in fixed-effects panels by Islam (1995), and with evidence that the quality rather than the quantity of education drives productivity (Égert et al., 2024; Hanushek & Woessmann, 2008). It also matches the CS-ARDL results of Dahmani and Mabrouki (2025) for MENA countries, in which human capital has a positive short-run effect but a weaker and less direct long-run effect, as do our CS-ARDL estimates and the positive lagged short-run coefficient in Table 9. Our results therefore do not contradict earlier evidence; they show that the positive and the sceptical findings of the literature can both be reproduced within one group of economies, depending on the institutional setting, the education measure and the estimator.

For Uzbekistan, the findings carry a cautious message. The country's productivity growth over the past two decades does not appear to have been associated with the expansion of higher education; the significant short-run investment coefficient (Table 6) points to capital accumulation, and structural change is a further plausible but untested explanation; and there is no evidence that it adjusts towards the relationship observed elsewhere. The recent enrolment boom has not yet been reflected in productivity, which is not surprising given that most of the additional students had not graduated by 2023. The experience of the non-EU group suggests, however, that the eventual productivity returns may depend on whether the expansion is accompanied by improvements in quality, labour-market relevance and the capacity of firms to employ graduates productively. Holmes (2013) reaches a similar conclusion for OECD and world samples, finding that technical skills and the employment of researchers, rather than the number of graduates, are related to growth, and Valero and Van Reenen (2019) estimate that a 10% increase in universities per capita raises regional GDP per capita by only about 0.4%, with part of the effect operating through innovation, which requires research capacity and not only teaching capacity. The tripling of the annual

number of graduates in Uzbekistan between 2019 and 2024 (Kun.uz, 2025) means that the period in which the expanded cohorts enter the labour market is only beginning.

Several limitations should be acknowledged. First, the gross enrolment ratio measures current participation rather than the educational attainment or skills of the workforce, and it includes part-time and distance students; attainment-based measures are available only with gaps and show no positive association with productivity (Section 4.8), and skills-based measures are not available for the sample. Second, GDP per person employed relies in part on modelled employment estimates, and the Uzbek enrolment series contains a break in 2005. Third, with $T = 23$, the country-level regressions have few degrees of freedom, which limits the precision of the heterogeneous estimators. Fourth, the results describe long-run associations rather than causal effects. Panel Granger causality tests and a reverse PMG model find no evidence that productivity drives enrolment, but they cannot rule out that both respond to omitted factors such as income and institutional quality. Fifth, the PMG estimator relies on a long-run homogeneity restriction that is least tenable across groups of countries with different institutions, and the positive association depends on the contemporaneous timing of the enrolment variable. Future research could use attainment data by field of study, graduate labour-market outcomes and regional data within Uzbekistan to identify the channels through which higher education affects productivity.

6. Conclusions

This paper examined the short- and long-run relationship between tertiary enrolment and labour productivity in 24 transition economies over 2001–2023, with particular attention to Uzbekistan. Using panel ARDL models estimated on cross-sectionally demeaned data, the baseline PMG specification yields a positive long-run elasticity of labour productivity with respect to tertiary enrolment of about 0.10, together with a positive effect of fixed investment, and an adjustment speed of about 17% per year. This estimate holds for longer lags of the regressors, in sub-periods without the global financial crisis, when individual economies are excluded and when the sample is extended to 2024, and the tests we apply do not detect reverse causality from productivity to enrolment, although they cannot rule it out. The relationship is, however, heterogeneous: it is positive and larger among EU member states, negative among non-EU transition economies and imprecisely estimated when full slope heterogeneity and common factors are allowed for. It reflects the contemporaneous co-movement of enrolment and productivity and does not carry over to lagged enrolment or to attainment-based measures. Uzbekistan shows no statistically significant error correction towards the panel long-run relationship, and its productivity growth over the period was associated with investment rather than with higher education.

With these caveats, the results are consistent with three policy considerations, which follow from the pattern of the estimates rather than from variables included in the models. First, enrolment targets alone are an insufficient measure of the success of higher education reform; because the association between enrolment and productivity is context-dependent, the Uzbek target of more than 50% coverage by 2030 could usefully be complemented by targets for quality, graduate employment and research output. Second, the contrast between EU and non-EU economies suggests, although our models do not test this directly, that the productivity returns to expansion may depend on the capacity of the economy to absorb graduates, which would call for policies that link universities with employers, support private investment in skill-intensive sectors and reduce the mismatch between fields of study and labour demand. Third, because the effects of expansion appear with a delay, the productivity outcomes of the post-2017 reforms should be monitored over the coming decade, using data on graduate employment and earnings that would allow a more direct evaluation.

The study has limitations that qualify these conclusions: enrolment measures participation rather than the skills of the workforce, the time dimension is short for heterogeneous estimators, and the estimates are associations rather than causal effects. Future research could use attainment and skills data by field of study, information on graduate employment, earnings and emigration, and regional or firm-level data within Uzbekistan to identify the channels through which higher education affects productivity and to evaluate the post-2017 reforms once the expanded cohorts have entered the labour market. Four directions appear particularly promising. First, graduation and graduate-tracer data would allow the delay between enrolment, graduation and productivity to be modelled explicitly rather than through fixed lags. Second, combining enrolment with measures of quality, such as adult-skills assessments or accreditation outcomes, would allow a direct test of the quality hypothesis suggested by the EU/non-EU contrast. Third, sectoral and firm-level data could show whether graduates are absorbed by high-productivity sectors or by low-productivity services and the public sector. Fourth, as data for 2024 and later years become available for all economies, the analysis should be updated to cover the period in which the expanded Uzbek cohorts enter the labour market.

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Abbreviations

The following abbreviations are used in this manuscript:

ARDL	Autoregressive distributed lag
CCE-MG	Common correlated effects mean group
CD	Cross-sectional dependence
CIPS	Cross-sectionally augmented Im–Pesaran–Shin
CS-ARDL	Cross-sectionally augmented ARDL
CS-DL	Cross-sectionally augmented distributed lag
DFE	Dynamic fixed effects
ECT	Error-correction term
EU	European Union
GDP	Gross domestic product
IPS	Im–Pesaran–Shin
MG	Mean group
PMG	Pooled mean group
PPP	Purchasing power parity
WDIs	World Development Indicators

Appendix A

Table A1 reports the number of interpolated observations for each variable in the three samples used in the paper.

Table A1. Interpolated observations by variable and sample.

Variable	Baseline: 24 Countries, 2001–2023 ($N = 552$)	16 Countries, 2001–2024 ($N = 384$)	26 Countries, 2006–2023 ($N = 468$)
LP (GDP per person employed)	0 (0.00%)	0 (0.00%)	0 (0.00%)
TER (tertiary enrolment)	5 (0.91%)	3 (0.78%)	8 (1.71%)
INV (fixed capital formation)	0 (0.00%)	0 (0.00%)	0 (0.00%)
LE (life expectancy)	0 (0.00%)	0 (0.00%)	0 (0.00%)
TRADE (trade openness)	0 (0.00%)	0 (0.00%)	0 (0.00%)
All variables	5 (0.18%)	3 (0.16%)	8 (0.34%)

Number of interpolated observations, with the share of all observations of the variable (or, in the last row, of all variable-year observations) in parentheses. Interpolated values of tertiary enrolment: North Macedonia 2011 and 2016, Russia 2010 and Tajikistan 2018–2019 (baseline sample); Russia 2010 and Tajikistan 2018–2019 (16-country sample used in Table 9); the five baseline values plus Kazakhstan 2007–2009 (26-country sample used in Table 8). All values were filled by linear interpolation between adjacent observed values of the same country (StataNow 19 MP command *ipolate*); no values were extrapolated. TRADE is used only in the robustness check in Table 8.

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