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Wainaina Martin Chege



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Wainaina, Martin Chege
Wake Tech Community College, Raleigh, North Carolina, United States

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Abstract

Purpose: This paper examined whether the rapid expansion of information and communication technology (ICT) generates Dutch disease effects in developing countries.

Methodology: Using an annual panel of 37 low- and middle-income economies covering 2005-2021, the study tested the two channels of the Corden-Neary framework. The spending effect was assessed by estimating the effect of ICT on the real effective exchange rate, and the resource movement effect by estimating the effect of ICT on agricultural, manufacturing and services value added. Estimation moved from fixed effects with Driscoll-Kraay standard errors to system GMM to address endogeneity and persistence and allowed the effects to change after the Covid-19 pandemic through an ICT-Covid interaction.

Findings: The results did not support a spending effect: ICT was not associated with real appreciation, and the negative coefficient implied a depreciation of about 2.0 per cent for a 10-percentage point rise in internet use, which was significant only with Driscoll-Kraay standard errors. Higher ICT use was associated with a lower manufacturing share of GDP, but this association was not robust in the dynamic specification and did not operate through the exchange rate. No clear effect was found for agriculture or services, and the effects did not strengthen after 2020.

Unique Contribution to Theory, Practice and Policy: The findings suggest that policymakers need not adjust exchange rate policy in response to digital expansion but should support manufacturing competitiveness directly and monitor sectoral structure as digital adoption deepens.

Keywords: *ICT, Dutch Disease, Real Exchange Rate, Sectoral Output, Panel Data, Developing Countries, Covid-19*

JEL classification: *O33, F31, C23, O14, L96*

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INTRODUCTION

Information and communication technology has spread quickly across developing countries over the past two decades. Mobile telephony, internet access and digital payments now shape how households save, firms trade and governments deliver services. The Covid-19 pandemic accelerated this process: restrictions on movement pushed firms, schools and public institutions towards online and mobile channels, and many economies recorded a step change in digital adoption. Policymakers generally welcome this trend because ICT can raise productivity and widen access to markets and finance.

A less examined question is whether a fast-growing digital economy can also change relative prices and the structure of production in ways that resemble Dutch disease. In the classic account of Corden and Neary (1982), a booming sector raises national income and draws resources towards itself. The resulting increase in demand for non-tradable goods pushes up their prices and appreciates the real exchange rate (the spending effect), while labour and capital move out of the lagging tradable sectors (the resource movement effect). The framework has been applied to natural resource discoveries, foreign aid and remittance inflows. Applying it to ICT is less common, although digital services exports, inflows of foreign investment into the sector, and remittances facilitated by mobile money could, in principle, work through analogous channels. The analogy is imperfect. Natural resource Dutch disease is driven by rent windfalls, whereas a Dutch disease mechanism in ICT would operate through different transmission vectors: productivity differentials between tradable and non-tradable sectors (the Balassa–Samuelson mechanism), service export revenues, and capital inflows, including digital foreign direct investment. Digital adoption is therefore not treated as equivalent to a resource rent. This paper instead tests whether these distinct channels generate spending and resource movement effects similar to those of a resource boom.

If ICT expansion appreciates the real exchange rate and shifts activity from agriculture and manufacturing towards services, countries may be weakening export competitiveness and industrial capacity even as they digitalize. If instead ICT raises productivity across all sectors, these effects may be weak or absent. Evidence on which of these outcomes prevails, particularly after the pandemic, is limited.

Problem Statement

Governments across developing countries are investing in digital infrastructure and promoting digital adoption as a route to productivity, inclusion and growth, yet the macroeconomic and structural consequences of rapid ICT expansion are poorly understood. If digital expansion appreciates the real exchange rate and draws labour and capital out of agriculture and manufacturing, it could erode export competitiveness and industrial capacity, which are the outcomes that Dutch disease studies document for resource booms, aid and remittances. If it does not, policymakers risk adopting defensive exchange rate or industrial measures that are not needed. The existing literature does not settle the question. Studies of ICT concentrate on aggregate growth and employment, while studies of Dutch disease concentrate on resource rents, aid and capital inflows. We are not aware of a study that tests whether ICT expansion operates through the real exchange rate and the composition of output, and evidence for the period after the Covid-19 pandemic, when digital adoption accelerated, is particularly limited. The problem addressed in this study is therefore the lack of evidence on whether ICT expansion in developing countries produces spending and resource movement effects of the Dutch disease type, whether the exchange rate is the channel through which sectoral output is affected, and whether these relationships changed after the pandemic.

Research Questions and Contributions

This paper addresses three questions. First, does ICT expansion affect the real exchange rate in developing countries, as a spending effect would imply? Second, does ICT expansion change the composition of output across agriculture, manufacturing and services, as a resource movement effect would imply? Third, did pandemic-induced forced digital adoption create a sudden, structural shift in service-sector agglomeration that accelerated real exchange rate appreciation relative to the pre-2020 trend? We also ask whether the effects differ across resource-rich and lower-income country groups. We answer them using an annual panel of low- and middle-income economies, a dynamic panel specification that deals with persistence and endogeneity, and an explicit test of whether the exchange rate is the channel through which ICT affects sectoral output.

The paper contributes in three ways. It extends Dutch disease analysis to the digital economy. It tests the spending and resource movement effects within one consistent framework rather than in isolation. It provides evidence for the post-Covid-19 period, and policy recommendations that follow from it. The remainder of the paper is organised as follows. Section 2 reviews the theory and develops hypotheses. Section 3 describes the data and the empirical strategy. Section 4 presents the results. Section 5 concludes with policy recommendations.

Theoretical Framework and Hypotheses

The Dutch Disease Framework

Corden and Neary (1982) and Corden (1984) analyse a small open economy with a booming tradable sector, a lagging tradable sector and a non-tradable sector. The boom has two effects. The resource movement effect pulls labour and capital into the booming sector and, through higher non-tradable wages, into non-tradables, so that the lagging tradable sector (usually manufacturing or agriculture) contracts. The spending effect arises because higher income raises demand for non-tradables, whose prices are set domestically, so the real exchange rate appreciates and the lagging tradable sector loses competitiveness. Empirical work has found evidence consistent with these effects for resource wealth (Sachs & Warner, 1995), foreign aid (Rajan & Subramanian, 2011) and remittances (Acosta et al., 2009).

ICT and the Real Exchange Rate

ICT can affect the real exchange rate through several channels, and their signs differ. Under the Balassa–Samuelson mechanism (Balassa, 1964; Samuelson, 1964), faster productivity growth in tradables raises wages economy-wide and therefore the relative price of non-tradables, appreciating the real exchange rate. If ICT raises productivity more in tradables, appreciation follows. If ICT mainly raises productivity in non-tradable services, the relative price of those services may fall instead. ICT can also raise national income and attract foreign investment and remittances, which raise domestic demand and appreciate the real exchange rate through the spending effect. Finally, better information and lower transaction costs can raise competition and reduce price differentials, which would depreciate it. The net effect is therefore an empirical question. We follow the standard literature on the fundamentals of the real exchange rate (e.g., Edwards, 1989) in controlling for productivity, the terms of trade, trade openness, government consumption and capital inflows.

ICT and Sectoral Output

ICT may shift the structure of production in two ways. First, as a booming or high-growth sector, it can attract labour and capital away from agriculture and manufacturing, which is the resource movement effect. Second, ICT can raise productivity within agriculture and manufacturing (for example through market information, digital payments and logistics), which would raise their share of output rather than reduce it. Services, which include ICT itself, are expected to gain share in either case. Because the output shares of the three sectors sum to approximately 100 per cent of GDP, the sectoral equations are not independent, and the services result partly mirrors the other two.

Empirical work on ICT and aggregate performance finds positive effects, but they are clearer in advanced economies than in developing ones. Röller and Waverman (2001) found a significant causal effect of telecommunications infrastructure on growth in 21 OECD countries, strongest once coverage approached universal service. Czernich et al. (2011) estimated that a 10-percentage-point rise in broadband penetration raised annual per capita growth by 0.9 to 1.5 percentage points in an OECD panel. For developing countries, the picture is less certain. Aker and Mbiti (2010) reviewed evidence of gains in agricultural and labour market efficiency from mobile phones in Africa but noted that evidence of macroeconomic effects was limited. A meta-analysis of 65 country-level studies (Wang et al., 2024) finds a positive average effect once publication bias is corrected, but a weaker and less robust one in developing countries, with mobile phones performing better than fixed lines. Niebel (2018) likewise finds that developing and emerging economies do not gain more from ICT than developed ones.

Evidence on productivity and structural change is thinner. Hjort and Poulsen (2019) use the staggered arrival of submarine internet cables in African countries and find higher employment, driven mainly by higher-skill occupations, together with more firm entry, higher firm productivity and more exporting. This is consistent with ICT raising productivity within firms rather than only drawing resources away from other activities, although it does not measure sectoral output shares. Saba and Ngepah (2022) study ICT diffusion alongside industrialisation and growth in a cross-country setting. Any link between ICT and the manufacturing share must also be separated from the wider trend that Rodrik (2016) documents: manufacturing shares now peak at lower income levels in recent industrialisers, a pattern he attributes to globalisation and labour-saving technological progress in manufacturing. Because this global trend affects manufacturing shares independently of ICT, its interaction with ICT adoption has to be taken into account so that the resource movement effect is not misattributed. In the empirical design, year effects absorb the average global deindustrialization trend, so the ICT coefficient on the manufacturing share is identified from differences across countries in the pace of ICT adoption. The interaction between the global manufacturing trend and ICT adoption is not estimated separately and is treated as a limitation (Section 5.3).

Studies of Dutch disease in developing countries have focused on resource booms, aid and capital or remittance inflows rather than on ICT. Lartey (2008) shows in a small open economy model that capital inflows can appreciate the real exchange rate and shift resources towards non-tradables, with larger effects when production uses more foreign capital. Ahmad et al. (2023) find for sub-Saharan Africa that aid inflows appreciate the real exchange rate, although about one-third of this effect is offset by external debt interest payments. Together with Acosta et al. (2009) on remittances and Rajan and Subramanian (2011) on aid, this literature shows that the exchange rate channel is central to Dutch disease. We did not find a study that tests whether ICT expansion works through this channel, and this paper aims to fill that gap.

Research gaps. Taken together, the literature leaves four gaps. First, Dutch disease studies in developing countries cover resource booms, aid, remittances and other capital inflows, but not ICT. Second, studies of ICT and growth estimate aggregate effects (Czernich et al., 2011; Niebel, 2018; Röller & Waverman, 2001; Wang et al., 2024) and do not examine relative prices or the real exchange rate. Third, the evidence on structural change is thin and, as in Hjort and Poulsen (2019), does not measure sectoral output shares, so the spending and resource movement effects of ICT have not been tested together with the exchange rate as the channel. Fourth, evidence on whether these relationships changed after the Covid-19 pandemic accelerated digital adoption is limited. The hypotheses below address these gaps: H1 tests the spending effect, H2 the resource movement effect and the exchange rate channel, and H3 the post-Covid and group differences.

Hypotheses

The framework yields the following testable hypotheses, summarised in Table 1.

- **H1 (spending effect):** ICT has a positive effect on the real exchange rate (appreciation).
- **H2 (resource movement effect):** ICT has a negative effect on the output share of manufacturing and agriculture and a positive effect on the output share of services. If the exchange rate is the transmission channel, the ICT coefficient falls in absolute size when the real exchange rate is added to the sectoral equation. In testing H2, the manufacturing result is read against the global deindustrialisation trend (Rodrik, 2016): year effects absorb its average component, so the ICT coefficient is interpreted net of that trend. Testing H2c formally requires a structural equation modelling (SEM) or system GMM mediation framework, such as the Sobel–Goodman test or modern causal mediation analysis, rather than observing coefficient attenuation in an OLS or dynamic panel regression, which is vulnerable to endogeneity. The attenuation comparison reported here is therefore indicative only.
- **H3a (post-Covid structural shift):** Pandemic-induced forced digital adoption created a sudden, structural shift that accelerated real exchange rate appreciation and raised the service-sector share relative to the pre-2020 trend, so the effects of ICT are stronger after 2020.
- **H3b1 (resource-rich economies):** ICT may exacerbate an existing Dutch disease, so the spending effect of ICT is larger in resource-rich economies.
- **H3b2 (low-income economies):** ICT infrastructure deficits may mute both the spending effect and the resource movement effect, so the effects of ICT are weaker in low-income economies.

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Table 1: Hypotheses and Expected Signs

Hypothesis	Parameter	Expected sign	Interpretation
H1	β1 (Eq. 1)	Positive	ICT appreciates the real exchange rate: spending effect
H2a	γ1 (Eq. 2), agriculture and manufacturing	Negative	ICT reduces the share of lagging tradable sectors: resource movement effect
H2b	γ1 (Eq. 2), services	Positive	ICT raises the share of services
H2c	κ (Eq. 3), agriculture and manufacturing; and γ1 in Eq. 3 compared with Eq. 2	Negative; |γ1| falls	Appreciation hurts tradables, and part of the ICT effect runs through the exchange rate
H3a	β2 (Eq. 1), γ2 (Eqs. 2 and 3)	Same sign as β1 and γ1	The effects strengthened after Covid-19
H3b1	β3 (Eq. 5), resource-rich	Positive	ICT exacerbates an existing Dutch disease: the spending effect is larger in resource-rich economies
H3b2	β3 (Eq. 5), low-income	Negative	ICT infrastructure deficits mute the effects in low-income economies

Data and Empirical Strategy

Sample and Data Sources

The sample comprised low- and middle-income economies as classified by the World Bank, excluding regional and income aggregates. The initial extract covered 131 countries annually over 2000-2025. Real effective exchange rate (REER) data were available for only 47 of them, so the remaining 84 were excluded. Observations with a missing value on any control variable were then dropped, which reduced the estimation sample to 37 countries (5 low-income, 13 lower-middle-income and 19 upper-middle-income) and between 549 and 574 country-year observations, depending on the specification. The usable observations fell between 2005 and 2021, with at most 17 years per country (15.4 on average). The post-Covid-19 period was therefore identified from 2020 and 2021 only. The panel was unbalanced, and the sample leaned towards economies with better-documented exchange rate data. The list of countries is given in Appendix B.

Most series were drawn from the World Bank World Development Indicators (WDI). The ICT series were compiled by the International Telecommunication Union (ITU) and the REER by the International Monetary Fund (IMF) International Financial Statistics, both distributed through WDI. Appendix A lists every series with its code and source.

Study Variables

Our key regressor is the share of the population using the internet. Mobile cellular subscriptions, fixed broadband subscriptions and a composite ICT index constructed by principal component analysis are used as alternatives in robustness checks. The dependent variable in the first model is the natural logarithm of the real effective exchange rate index, where an increase represents appreciation. The dependent variables in the second model are the value-added shares of agriculture, manufacturing and services in GDP. The control vector X includes the log of GDP per capita (to capture Balassa–Samuelson productivity effects), the

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terms of trade, trade openness, government consumption, foreign direct investment inflows, personal remittances and natural resource rents. Inflation is excluded from the baseline because a consumer-price-based real exchange rate is mechanically related to it; it is added in robustness checks. A Covid-19 indicator equals one from 2020 onward. Table 2 gives the definitions.

Table 2: Variable Definitions

Variable	Symbol	Definition and unit	Source
Real effective exchange rate	ln REER	Natural log of REER index, 2010 = 100; increase = appreciation	IMF IFS via WDI
ICT	ICT	Individuals using the internet, % of population	ITU via WDI
Covid-19 indicator	COVID	1 if year ≥ 2020, otherwise 0	Constructed
Agriculture	Y(agr)	Agriculture, forestry and fishing, value added, % of GDP	WDI
Manufacturing	Y(man)	Manufacturing, value added, % of GDP	WDI
Services	Y(ser)	Services, value added, % of GDP	WDI
Income per capita	ln GDPpc	Natural log of GDP per capita, constant US$	WDI
Terms of trade	TOT	Net barter terms of trade index	UNCTAD / World Bank via WDI
Trade openness	OPEN	Exports plus imports, % of GDP	WDI
Government consumption	GOV	General government final consumption expenditure, % of GDP	WDI
Foreign direct investment	FDI	Net inflows, % of GDP	IMF via WDI
Remittances	REM	Personal remittances received, % of GDP	IMF via WDI
Resource rents	RENT	Total natural resources rents, % of GDP	World Bank estimates via WDI

Model Specification

Let i index countries and t index years. Model 1 tests the spending effect:

$$ln\,REER_{it} = \alpha + \beta_1 ICT_{it} + \beta_2 (ICT_{it} \times COVID_t) + \delta' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where X is the vector of controls described above, μ is a country effect, λ is a year effect and ε is an error term. The year effects absorb global shocks, including the pandemic itself, so the Covid-19 indicator enters only through its interaction with ICT. H_1 predicts $\beta_1 > 0$, and β_2 tests whether the effect changed after 2020.

Model 2 tests the resource movement effect separately for each sector s (agriculture, manufacturing and services):

$$Y_{s,it} = \alpha + \gamma_1 ICT_{it} + \gamma_2 (ICT_{it} \times COVID_t) + \theta' X_{it} + \mu_i + \lambda_t + u_{it} \quad (2)$$

H_2 predicts $\gamma_1 < 0$ for agriculture and manufacturing and $\gamma_1 > 0$ for services. To test whether the exchange rate is the channel, we then add the log real exchange rate:

$$Y_{s,it} = \alpha + \gamma_1 ICT_{it} + \gamma_2 (ICT_{it} \times COVID_t) + \kappa\, ln\, REER_{it} + \theta' X_{it} + \mu_i + \lambda_t + u_{it} \quad (3)$$

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If κ is negative for the tradable sectors and the absolute value of γ1 is smaller in Equation (3) than in Equation (2), part of the effect of ICT on sectoral output operates through the real exchange rate, which is the spending effect. If γ1 is unchanged, the effect is a direct resource movement. Because the three sector shares sum to approximately 100 per cent of GDP, we interpret the services equation as the counterpart of the other two. The comparison of γ1 across Equations (2) and (3) is a coefficient-attenuation test. Because attenuation in a fixed-effects or dynamic panel regression is vulnerable to endogeneity, a formal test of H2c requires a structural equation modelling (SEM) or system GMM mediation framework, such as the Sobel–Goodman test or modern causal mediation analysis.

Real exchange rates and sector shares are persistent, so we also estimate a dynamic version of Model 1 that includes the lagged dependent variable (and similarly for Model 2):

$$ln\,REER_{it} = \alpha + \rho\, ln\,REER_{i,t-1} + \beta_1 ICT_{it} + \beta_2(ICT_{it} \times COVID_t) + \delta' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (4)$$

To examine heterogeneity (H3b1 and H3b2), we interact ICT with a country-group indicator GROUP, such as resource-rich versus other economies or low-income versus middle-income:

$$ln\,REER_{it} = \alpha + \beta_1 ICT_{it} + \beta_2(ICT_{it} \times COVID_t) + \beta_3(ICT_{it} \times GROUP_i) + \delta' X_{it} + \mu_i + \lambda_t + \varepsilon_{it} \quad (5)$$

Estimation Procedure

Estimation followed these steps. The study began with diagnostics and examined variance inflation factors, since ICT and income per capita were likely to be strongly correlated. Because the panel was unbalanced, the study tested for unit roots with Fisher-type tests based on augmented Dickey–Fuller regressions (one lag, cross-sectional means removed to absorb common shocks), which can be used with unbalanced panels. The CIPS test (Pesaran, 2007) was not used because it requires a balanced panel. The study attempted Pesaran's (2004) test for cross-sectional dependence and the Westerlund (2007) test for panel cointegration, but neither could be estimated: the panel was too unbalanced for the first, and the second accepts at most seven regressors. The Wooldridge test for serial correlation and a modified Wald test for groupwise heteroskedasticity were also applied to the fixed-effects residuals.

For the baseline estimates, the study estimated Equation (1) by pooled OLS, fixed effects and random effects, and chose between the latter two with the Hausman (1978) test. Equations (1) to (3) were then estimated by fixed effects with year effects, and the study reported these estimates with country-clustered standard errors and with Driscoll and Kraay (1998) standard errors (maximum lag of 3), which are robust to heteroskedasticity, autocorrelation and cross-sectional dependence in a panel with a long time dimension (Hoechle, 2007). Equations (2) and (3) were estimated on a common sample within each sector so that the ICT coefficients were comparable.

To address endogeneity and dynamics, the study recognised that ICT may respond to income and to the exchange rate, so the fixed-effects estimates may have been biased. It therefore estimated Equation (4) and the dynamic version of Equation (3) by two-step system GMM (Arellano & Bover, 1995; Blundell & Bond, 1998). ICT, the ICT–Covid interaction and the lagged dependent variable were treated as endogenous (and, in the sector equations, so was the log real exchange rate), while the controls and year dummies were treated as exogenous. The study used lags 2 to 4 as instruments and collapsed the instrument matrix to avoid instrument proliferation (Roodman, 2009), and it reported the Arellano–Bond AR(2), Hansen and difference-in-Hansen tests. Pooled mean group estimation (Pesaran et al., 1999) was also

attempted to separate short-run and long-run effects, but the estimator did not return estimates, so no long-run results are reported.

To test whether the effects differed across country groups, the study interacted ICT with an indicator for resource-rich economies (country mean natural resource rents above the sample median) and with an indicator for low-income economies (Equation 5), and it estimated Equation (1) separately for low-, lower-middle- and upper-middle-income economies. The resource-rich interaction was also applied to the sector equations.

Finally, the study checked robustness by re-estimating Equation (1) with alternative ICT measures (mobile cellular subscriptions, fixed broadband subscriptions, and a principal-component index of internet use and mobile subscriptions), with ICT lagged one year, with inflation added, with the real exchange rate winsorised at the 1st and 99th percentiles, and with resource-rich economies excluded.

RESULTS AND DISCUSSION

This section presents the descriptive statistics, the diagnostic tests and the estimates of Equations (1) to (5), following the order in Section 3.4. ICT is the percentage of the population using the internet, so ICT coefficients are per percentage point. Significance is reported at the

Descriptive Statistics

Table 3 summarises the variables for the 47 countries with real effective exchange rate (REER) data. Of the 131 countries in the initial World Bank extract, 84 had no REER observations and were excluded. REER is available for all 26 years (2000-2025) in 46 of the 47 remaining countries and for 17 years in the other. Mean internet use is 28.5 per cent of the population, but dispersion is wide (*SD* = 27.1), from almost zero to 98.0 per cent. On average, agriculture accounts for 13.8 per cent of GDP, manufacturing for 12.3 per cent and services for 51.4 per cent.

Table 3: Descriptive Statistics

Variable	N	M	SD	Min	Max
ln REER	1,213	4.60	0.22	3.98	6.76
ICT (% of population)	1,153	28.54	27.10	0.01	98.02
Agriculture (% of GDP)	1,170	13.78	9.63	0.00	55.01
Manufacturing (% of GDP)	1,129	12.31	6.08	0.00	44.98
Services (% of GDP)	1,162	51.39	10.37	0.00	77.21
ln GDP per capita	1,222	7.89	0.96	5.52	9.53
Terms of trade	934	103.97	18.50	19.90	195.80
Trade openness (% of GDP)	1,006	71.12	32.98	0.00	220.41
Government consumption (% of GDP)	982	14.67	6.12	0.00	43.48
FDI inflows (% of GDP)	1,199	3.84	4.24	−4.02	64.38
Remittances (% of GDP)	1,094	5.37	6.73	0.00	53.83
Natural resource rents (% of GDP)	1,023	8.08	9.58	0.00	88.60

Note. Sample of 47 countries, 2000–2025 (maximum 1,222 country-year observations per variable). *n* = number of non-missing observations. Source: authors' calculations from World Development Indicators and IMF data.

Table 4 compares means before and after 2020. Mean internet use rose from 21.1 to 59.2 per cent. Over the same comparison, mean log REER is higher after 2020 (4.643 against 4.591), the agriculture share is lower (13.0 against 14.0 per cent of GDP), the manufacturing share is

slightly lower (12.0 against 12.4) and the services share is higher (53.9 against 50.6). These are unconditional differences between unbalanced sets of observations, with only 225 post-2020 observations on ICT against 928 before, and they are not estimates of effects. The regressions below control for country and year effects.

Table 4: Means Before and After 2020

	2000–2019			2020–2025		
Variable	**M**	**SD**	**n**	**M**	**SD**	**n**
ln REER	4.591	0.186	937	4.643	0.294	276
ICT (% of population)	21.10	21.71	928	59.21	25.58	225
Agriculture (% of GDP)	14.02	9.84	898	13.00	8.89	272
Manufacturing (% of GDP)	12.40	6.17	867	12.04	5.76	262
Services (% of GDP)	50.63	10.87	890	53.87	8.08	272

Note. Periods are defined by the Covid-19 indicator. Sample of 47 countries.

ICT is strongly correlated with income per capita ($r = .56$, $p < .05$) and negatively with resource rents ($r = -.29$, $p < .05$), but its simple correlation with log REER is close to zero ($r = .03$). Variance inflation factors are low (mean 1.71, maximum 2.79 for ICT), so multicollinearity does not threaten the regressions. Income per capita is kept as a control because of the Balassa–Samuelson argument in Section 2.2.

The estimation sample is smaller than the descriptive sample. The regressions use 37 countries and between 549 and 574 country-year observations, because observations with a missing control are dropped; the terms of trade is the sparsest series (934 observations, against 1,213 for REER). The year dummies show that the usable observations fall in 2005–2021, at most 17 years per country (15.4 on average), so the Covid-19 indicator is identified from 2020 and 2021 only. Of the 37 countries, 5 are low-income, 13 lower-middle-income and 19 upper-middle-income.

Diagnostic Tests

Table 5 reports Fisher-type unit-root tests based on augmented Dickey–Fuller regressions, which can be used with an unbalanced panel. Cross-sectional means are removed to absorb common shocks. Log REER and the agriculture share reject the unit-root null in levels ($P = 191.26$ and 162.71, both $p < .001$). The manufacturing share rejects it on the P statistic ($P = 124.70$, $p = .019$) but only at the 10 per cent level on the inverse normal statistic ($Z = -1.50$, $p = .066$), so it is treated as borderline. ICT, the services share and log GDP per capita do not reject the null in levels ($p = .497$, .165 and .580), but all six series are stationary in first differences ($p < .001$). The variables are therefore of mixed order of integration, the case in which levels regressions can be misleading unless the variables are cointegrated.

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Table 5: Fisher-Type Panel Unit-Root Tests

	Levels		First differences		
Variable	**P (p)**	**Z (p)**	**P (p)**	**Z (p)**	**Order**
ln REER	191.26 (<.001)	−3.38 (<.001)	653.06 (<.001)	−19.48 (<.001)	I(0)
ICT	93.44 (.497)	1.03 (.849)	315.65 (<.001)	−10.50 (<.001)	I(1)
Agriculture	162.71 (<.001)	−3.06 (.001)	786.14 (<.001)	−22.29 (<.001)	I(0)
Manufacturing	124.70 (.019)	−1.50 (.066)	574.91 (<.001)	−17.87 (<.001)	I(0)
Services	107.25 (.165)	−0.33 (.371)	576.87 (<.001)	−18.29 (<.001)	I(1)
ln GDP per capita	90.61 (.580)	1.71 (.957)	416.97 (<.001)	−14.09 (<.001)	I(1)

Note. 47 panels. Augmented Dickey–Fuller regressions with one lag, panel-specific autoregressive parameters, panel means included, no time trend and cross-sectional means removed. H0: all panels contain unit roots. *P* = inverse chi-squared statistic (94 degrees of freedom); *Z* = inverse normal statistic. *p*-values in parentheses. I(0) = stationary in levels; I(1) = stationary after first differencing; I(0)? = borderline (rejected on *P* at 5 per cent, on *Z* only at 10 per cent).

Three planned diagnostics could not be completed. Pesaran's (2004) cross-sectional dependence test stopped because the panel is too unbalanced to give enough common observations. The CIPS test requires a balanced panel. The Westerlund (2007) cointegration test accepts at most seven regressors and Model 1 has nine. The pooled mean group estimator in Section 3.4 also failed to return estimates. We therefore cannot test formally for cointegration or cross-sectional dependence. Year effects are included in every regression and Driscoll and Kraay (1998) standard errors are robust to cross-sectional dependence, but the dynamic evidence rests on system GMM alone.

Table 6 reports the specification tests. The Hausman (1978) test favours fixed effects ($\chi^2(18) = 90.48$, $p < .001$), although the difference between the variance matrices was not positive definite and the test used 18 of the 25 coefficients, so it is only indicative. Fixed effects are in any case standard for macro panels and the country effects are strongly correlated with the regressors (the correlation is −.96 in Model 1). The Wooldridge test rejects the absence of first-order autocorrelation ($F(1, 36) = 385.57$, $p < .001$) and the modified Wald test rejects constant error variance across countries ($\chi^2(37) = 6{,}101.44$, $p < .001$). Errors are serially correlated and heteroskedastic, which supports the use of Driscoll–Kraay and country-clustered standard errors rather than conventional ones.

Table 6: Specification and Residual Diagnostics

Test	**Statistic**	***p***	**Conclusion**
Hausman test (fixed vs random effects)	$\chi^2(18) = 90.48$	<.001	Favours fixed effects (indicative)
Wooldridge serial correlation test	$F(1, 36) = 385.57$	<.001	First-order autocorrelation present
Modified Wald test (groupwise heteroskedasticity)	$\chi^2(37) = 6{,}101.44$	<.001	Heteroskedasticity present
Variance inflation factor (mean; maximum)	1.71; 2.79	—	No multicollinearity concern
Pesaran CD test	Not estimable	—	Panel too unbalanced
CIPS unit-root test	Not run	—	Requires a balanced panel
Westerlund cointegration test	Not estimable	—	Limit of seven regressors

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Note. The Hausman test compares fixed and random effects for Equation (1); its variance-matrix difference was not positive definite and its rank (18) was below the number of coefficients tested (25). The Wooldridge test uses ln REER, ICT and the controls. The modified Wald test uses the baseline fixed-effects model with year dummies. Variance inflation factors come from the pooled regression of Equation (1).

ICT and the Real Exchange Rate (Spending Effect)

Table 7 reports Model 1. The ICT coefficient is negative in every specification, the opposite of the sign predicted by H1. With country-clustered standard errors it is not significantly different from zero ($b = -0.0020$, $SE = 0.0014$, $p = .165$). With Driscoll–Kraay standard errors the same point estimate is significant at the 5 per cent level ($SE = 0.0008$, $p = .023$; 95% CI [−0.0037, −0.0003]). The effect is small: a 10-percentage point rise in internet use is associated with a 0.020 lower log REER, a real depreciation of about 2.0 per cent. The system GMM estimate has the same sign but is smaller and insignificant ($b = -0.0008$, $p = .256$). H1 is therefore not supported: there is no evidence of appreciation, and the evidence of a small depreciation depends on the choice of standard errors.

The ICT–Covid interaction is insignificant in all three specifications ($p = .371$, .104 and .526), so there is no evidence that the relationship changed in 2020–2021 and H3a is not supported for the exchange rate. Income per capita is the only control that is significant in both static models ($b = 0.431$, $p < .001$), consistent with the Balassa–Samuelson effect.

The system GMM column should not be relied on. The coefficient on lagged REER is 0.996 ($SE = 0.077$), implying near-unit-root persistence, and the specification tests fail: the Arellano–Bond AR(2) test rejects ($z = -3.64$, $p < .001$), the Hansen test rejects ($\chi^2(6) = 18.76$, $p = .005$) and the difference-in-Hansen test rejects the exogeneity of the level instruments ($p = .001$). We report it for completeness and base inference on columns (1) and (2). The PMG/ARDL estimates planned in Section 3.4 are not reported because the estimator failed.

Table 7: ICT and the Real Exchange Rate (Dependent Variable: ln REER)

Predictor	(1) FE, clustered SE	(2) FE, Driscoll–Kraay SE	(3) System GMM
ICT	−0.0020	−0.0020**	−0.0008
	(0.0014)	(0.0008)	(0.0007)
ICT × COVID	−0.0009	−0.0009	0.0005
	(0.0010)	(0.0005)	(0.0008)
Lagged ln REER	—	—	0.9957***
			(0.0774)
ln GDP per capita	0.4313***	0.4313***	0.0081
	(0.1109)	(0.0573)	(0.0095)
Other controls	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
Country effects	Yes	Yes	Differenced
R^2 (within)	.244	.244	—
N	569	569	569
Countries	37	37	37
Instruments	—	—	33
AR(2) *p*	—	—	<.001
Hansen *p*	—	—	.005

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Note. Standard errors in parentheses. * $p < .10$. ** $p < .05$. *** $p < .01$. Other controls are terms of trade, trade openness, government consumption, FDI inflows, remittances and natural resource rents. Driscoll-Kraay standard errors use a maximum lag of 3. System GMM is two-step with Windmeijer-corrected errors, ICT, ICT × COVID and lagged ln REER treated as endogenous (collapsed instruments, lags 2 to 4), controls and year dummies as exogenous. PMG/ARDL estimates are not reported because the estimator failed.

ICT and Sectoral Output (Resource Movement Effect)

Table 8 reports Equations (2) and (3) estimated on a common sample for each sector. Manufacturing is the only sector where the sign predicted by H2a appears. The ICT coefficient is negative and significant in Equation (2) ($b = -0.057$, $SE = 0.025$, $p = .031$) and Equation (3) ($b = -0.056$, $p = .046$), and the Driscoll–Kraay estimate is similar ($p = .027$). A 10 percentage point rise in internet use is associated with a 0.57 percentage point lower manufacturing share of GDP, about 4.6 per cent of the sample mean of 12.3. The ICT–Covid interaction is positive and significant ($b = 0.020$, $p = .026$), so the implied slope after 2020 is smaller in absolute size (about −0.037). The manufacturing association therefore weakened rather than strengthened after 2020, the opposite of H3a.

For agriculture the ICT coefficient is positive, contrary to H2a: 0.043 in Equation (2) ($p = .100$ with clustered errors, $p = .048$ with Driscoll–Kraay errors). For services it is small and insignificant ($b = 0.018$, $p = .485$), so H2b is not supported. Neither sector shows a clear change after 2020.

Table 8: ICT and Sectoral Output: Fixed Effects (Dependent Variable: Sector Value Added, % of GDP)

	Agriculture		Manufacturing		Services	
Predictor	**Eq. (2)**	**Eq. (3)**	**Eq. (2)**	**Eq. (3)**	**Eq. (2)**	**Eq. (3)**
ICT	0.043*	0.046*	−0.057**	−0.056**	0.018	0.020
	(0.026)	(0.025)	(0.025)	(0.027)	(0.025)	(0.025)
ICT × COVID	−0.002	−0.001	0.020**	0.020**	0.011	0.012
	(0.012)	(0.011)	(0.009)	(0.008)	(0.016)	(0.016)
ln REER	—	1.194	—	0.494	—	1.047
		(2.443)		(1.735)		(1.711)
ln GDP per capita	−7.348***	−7.853**	−1.365	−1.574	5.111**	4.667**
	(2.579)	(3.116)	(1.631)	(1.930)	(2.183)	(2.192)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Country and year effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (within)	.229	.232	.264	.265	.427	.429
N	562	562	560	560	562	562
Countries	37	37	37	37	37	37

Note. Fixed-effects estimates with country-clustered standard errors in parentheses. * $p < .10$. ** $p < .05$. *** $p < .01$. Equation (2) omits ln REER and Equation (3) adds it; both are estimated on the same sample within each sector. Other controls are terms of trade, trade openness, government consumption, FDI inflows, remittances and natural resource rents.

Table 9 reports Equation (3) with Driscoll–Kraay standard errors and with system GMM. The Driscoll–Kraay estimates reproduce the pattern in Table 8. The dynamic estimates pass the specification tests (AR(2) $p = .721$, .296 and .320; Hansen $p = .861$, .862 and .713), although

the number of instruments (37) equals the number of countries and the Hansen test may be weakened by instrument proliferation (Roodman, 2009). Sector shares are highly persistent (lagged coefficients 0.76 to 0.83). In these specifications the ICT coefficient is not significant at the 5 per cent level in any sector: the manufacturing coefficient keeps its sign but is smaller ($b = -0.022$, $p = .359$), the agriculture coefficient changes sign ($b = -0.006$, $p = .757$) and the services coefficient is negative ($b = -0.033$, $p = .055$). The manufacturing result is therefore not robust to the dynamic specification.

Table 9: ICT and Sectoral Output: Driscoll–Kraay and System GMM Estimates of Equation (3)

	Driscoll–Kraay FE			System GMM		
Predictor	**Agr.**	**Manuf.**	**Serv.**	**Agr.**	**Manuf.**	**Serv.**
ICT	0.046**	−0.056**	0.020	−0.006	−0.022	−0.033*
	(0.020)	(0.022)	(0.017)	(0.018)	(0.024)	(0.017)
ICT × COVID	−0.001	0.020***	0.012*	0.012	0.008	0.015
	(0.009)	(0.005)	(0.006)	(0.007)	(0.009)	(0.012)
ln REER	1.194	0.494	1.047	1.665	−0.905	0.942
	(0.767)	(0.879)	(0.909)	(2.150)	(1.063)	(1.562)
Lagged dependent variable	—	—	—	0.759***	0.780***	0.834***
				(0.073)	(0.167)	(0.092)
Controls and year effects	Yes	Yes	Yes	Yes	Yes	Yes
N	562	560	562	561	557	561
Countries	37	37	37	37	37	37
Instruments	—	—	—	37	37	37
AR(2) *p*	—	—	—	.721	.296	.320
Hansen *p*	—	—	—	.861	.862	.713

Note. Standard errors in parentheses. * $p < .10$. ** $p < .05$. *** $p < .01$. Agr. = agriculture; Manuf. = manufacturing; Serv. = services. Dependent variable is sector value added (% of GDP). Driscoll–Kraay standard errors use a maximum lag of 3. System GMM is two-step with Windmeijer-corrected errors; ICT, ICT × COVID, ln REER and the lagged dependent variable are treated as endogenous (collapsed instruments, lags 2 to 4), controls and year dummies as exogenous. Controls are those in Table 7.

Table 10 tests whether the exchange rate is the channel (H2c). Adding ln REER to the sector equations barely moves the ICT coefficient: for manufacturing it falls in absolute size by only 1.9 per cent (from −0.0568 to −0.0557), and for agriculture and services it rises. The coefficient on ln REER is positive and insignificant in all three sectors, whereas H2c requires a negative coefficient for agriculture and manufacturing. The indirect effects of ICT through the exchange rate (the Model 1 ICT coefficient multiplied by the ln REER coefficient) are close to zero and far smaller than the direct effects. The association between ICT and manufacturing, to the extent it exists, does not run through the real exchange rate, and H2c is not supported. We did not compute standard errors for the indirect effects and draw no inference beyond their size.

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Table 10: Channel Test: ICT Coefficient With and Without the Real Exchange Rate

Sector	γ_1 Eq. (2)	γ_1 Eq. (3)	κ (SE)	Change in $\lvert\gamma_1\rvert$ (%)	$\beta_1 \times \kappa$
Agriculture	0.0435	0.0461	1.194 (2.443)	+6.0	−0.0024
Manufacturing	−0.0568	−0.0557	0.494 (1.735)	−1.9	−0.0010
Services	0.0176	0.0199	1.047 (1.711)	+13.1	−0.0021

Note. γ_1 = ICT coefficient in the sector equation without (Eq. 2) and with (Eq. 3) ln REER; κ = coefficient on ln REER in Eq. (3) with country-clustered standard error in parentheses (none significant at 10 per cent); β_1 = ICT coefficient in Model 1 (−0.0020, fixed effects). $\beta_1 \times \kappa$ is the implied indirect effect of ICT through the exchange rate and has no standard error.

Robustness and Heterogeneity

Table 11 reports the heterogeneity tests of Equation (5). The interaction between ICT and the resource-rich indicator is negative and insignificant ($b = -0.0015$, $p = .387$), the wrong sign for H3b1. The interaction with the low-income indicator is positive and significant only at the 10 per cent level, the opposite of the sign predicted by H3b2 ($b = 0.0072$, $p = .086$); it implies an ICT slope of about 0.0057 for low-income economies, so there is no sign of depreciation there. The separate regressions by income group rest on 5, 13 and 19 countries, which makes clustered inference fragile. None shows a significant ICT effect; the only significant coefficient is a negative ICT–Covid interaction in upper-middle-income economies ($b = -0.0069$, $p = .011$). In the sector equations the interaction of ICT with the resource-rich indicator is also insignificant (agriculture −0.018, $p = .508$; manufacturing 0.009, $p = .682$; services 0.031, $p = .217$). H3b1 and H3b2 therefore receive no support.

Table 11: Heterogeneity in the Effect of ICT on the Real Exchange Rate (Dependent Variable: ln REER)

Predictor	Interaction models		By income group		
	Resource-rich	Low-income	Low	Lower-middle	Upper-middle
ICT	−0.0017	−0.0015	0.0063	−0.0032	0.0006
	(0.0016)	(0.0014)	(0.0107)	(0.0025)	(0.0021)
ICT × COVID	−0.0010	−0.0004	0.0039	0.0019	−0.0069**
	(0.0010)	(0.0009)	(0.0033)	(0.0016)	(0.0025)
ICT × Resource-rich	−0.0015	—	—	—	—
	(0.0017)				
ICT × Low-income	—	0.0072*	—	—	—
		(0.0041)			
Controls and year effects	Yes	Yes	Yes	Yes	Yes
N	569	569	72	192	305
Countries	37	37	5	13	19

Note. Fixed effects with country-clustered standard errors in parentheses. * $p < .10$. ** $p < .05$. *** $p < .01$. Resource-rich = country mean of natural resource rents above the sample median. Low-income follows the World Bank classification. Group main effects are absorbed by the country effects. Controls are those in Table 7. Columns (3) to (5) use 5, 13 and 19 clusters respectively.

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Table 12 reports the robustness checks. The baseline result, a small negative and imprecisely estimated ICT coefficient, is stable in sign and size when internet use is the ICT measure. In those variants the coefficient lies between −0.0014 and −0.0022 in every variant: lagging ICT one year (p = .167), adding inflation (p = .266), winsorising REER at the 1st and 99th percentiles (p = .151) and excluding resource-rich economies (p = .056). Inflation itself has a strongly negative coefficient (−0.0089, p < .001). The alternative ICT measures (mobile subscriptions, fixed broadband and a principal-component index of internet use and mobile subscriptions) are not significant on their own (p = .682, .869 and .154). Only the mobile subscriptions interaction with Covid-19 is significant (b = −0.0013, p = .025), which is again a negative association rather than an appreciation. We did not estimate the models with the Darvas (2012) real exchange rate or separately by region, as proposed in Section 3.4.

Table 12: Robustness Checks (Dependent Variable: ln REER)

Predictor	(1) Mobile subs.	(2) Fixed broad-band	(3) ICT index	(4) Lagged ICT	(5) Plus inflation	(6) Wins. REER	(7) Excl. resource-rich
ICT measure	−0.0004	0.0008	−0.0551	−0.0020	−0.0014	−0.0020	−0.0022*
	(0.0009)	(0.0048)	(0.0378)	(0.0014)	(0.0012)	(0.0014)	(0.0011)
ICT measure × COVID	−0.0013**	−0.0027	−0.0355	−0.0007	−0.0007	−0.0007	−0.0015
	(0.0006)	(0.0020)	(0.0217)	(0.0010)	(0.0009)	(0.0009)	(0.0018)
Inflation	—	—	—	—	−0.0089***	—	—
					(0.0022)		
Controls and year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	574	549	565	571	559	569	318
Countries	37	37	37	37	37	37	20

Note. Fixed effects with country-clustered standard errors in parentheses. * p < .10. ** p < .05. *** p < .01. ICT measure: mobile cellular subscriptions per 100 people (1), fixed broadband subscriptions per 100 people (2), first principal component of internet use and mobile subscriptions (3), internet use lagged one year (4), and internet use as % of population (5 to 7). Column (5) adds CPI inflation; column (6) winsorises ln REER at the 1st and 99th percentiles; column (7) excludes countries whose mean resource rents are above the sample median. Controls are those in Table 7.

Discussion

The analysis in the previous section indicates that the evidence does not point to a Dutch disease effect of ICT through the spending channel. ICT expansion is not associated with real appreciation. If anything, it is associated with a small depreciation, which keeps its sign across samples and specifications that use internet use but is statistically weak. This is consistent with the possibility, raised in Section 2.2, that lower transaction costs, greater competition or productivity gains in non-tradable services offset any demand-driven appreciation, although this study does not test those mechanisms. The control variables carry the same message. Remittances and resource rents, the inflows behind Dutch disease in the studies of aid and

remittances (Acosta et al., 2009; Rajan & Subramanian, 2011) and of resource wealth (Sachs & Warner, 1995), have negative and at most marginally significant coefficients in the Driscoll–Kraay model (p = .092 and .081) and are insignificant elsewhere. The only strong determinant of the real exchange rate is income per capita, in line with the Balassa–Samuelson mechanism.

The resource movement evidence is limited. Only manufacturing behaves as the framework predicts, with a lower share of GDP at higher ICT use, and that association disappears in the dynamic specification. Agriculture shows the opposite sign in the static models and services show no effect. Because the ICT coefficient is unchanged when the real exchange rate is added, any contraction of manufacturing is not a product of the spending effect. It may reflect a direct reallocation of labour and capital towards the digital sector, as the resource movement effect describes, or broader structural change that accompanies the spread of ICT. The data cannot separate these explanations. The contrast with resource booms, aid and remittances, where studies find appreciation together with weaker manufacturing, is that here the second part appears without the first.

Table 13 summarises the results against the hypotheses. These are associations from a sample of 37 countries and a post-Covid window of two years, and the dynamic estimates are less favourable to a Dutch disease reading than the static ones. The conclusions in Section 5 are drawn with that in mind.

Table 13: Summary of Hypothesis Tests

Hypothesis	Prediction	Evidence	Verdict
H1	ICT appreciates the REER ($\beta_1 > 0$)	$\beta_1 = -0.0020$; p = .023 (Driscoll–Kraay), .165 (clustered), .256 (GMM)	Not supported
H2a	ICT lowers agriculture and manufacturing shares ($\gamma_1 < 0$)	Manufacturing: $\gamma_1 = -0.057$, p = .031; not significant in GMM. Agriculture: γ_1 = 0.043, p = .100, wrong sign	Manufacturing partly supported; agriculture not supported
H2b	ICT raises the services share ($\gamma_1 > 0$)	γ_1 = 0.018, p = .485; GMM −0.033, p = .055	Not supported
H2c	$\kappa < 0$ for tradables and $\lvert\gamma_1\rvert$ falls when ln REER is added	κ = 1.194 (agriculture) and 0.494 (manufacturing), both insignificant; $\lvert\gamma_1\rvert$ falls 1.9% for manufacturing and rises for agriculture	Not supported
H3a	Effects stronger after 2020	REER: β_2 insignificant throughout. Manufacturing: γ_2 = 0.020, p = .026, opposite sign to γ_1	Not supported
H3b1	$\beta_3 > 0$ in resource-rich economies	Resource-rich: $\beta_3 = -0.0015$, p = .387	Not supported
H3b2	$\beta_3 < 0$ in low-income economies	Low-income: β_3 = 0.0072, p = .086 (opposite sign)	Not supported (opposite sign, 10% level)

Note. Coefficients are those in Tables 7 to 11. β_1 and β_2 are from Equation (1), γ_1 and γ_2 from Equations (2) and (3), κ from Equation (3), and β_3 from Equation (5).

Conclusion and Policy Recommendations

Conclusion

This paper sought to investigate whether ICT expansion generates Dutch disease effects in developing countries, using a panel of 37 low- and middle-income economies.

Absence of the spending effect: On the first question, ICT is not associated with real appreciation. The ICT coefficient on log REER is negative (−0.0020, p = .023 with Driscoll–Kraay standard errors, p = .165 with clustered errors), equivalent to a depreciation of about 2.0 per cent for a 10-percentage point rise in internet use, so there is no evidence of a spending effect. If anything, higher internet use is associated with a modest real depreciation, which suggests that digital expansion in developing economies does not trigger the standard spending effect that distorts external price competitiveness.

Fragile evidence for resource movement: On the second question, higher internet use is associated with a lower manufacturing share of GDP (−0.057 percentage points per percentage point, p = .031), a higher agricultural share that is only marginally significant (0.043, p = .100) and no change in the services share. The manufacturing association is not significant in the dynamic specification and does not run through the exchange rate, because adding log REER leaves the ICT coefficients almost unchanged. The associations of ICT with agriculture and services are positive or insignificant in the static estimates, which suggests that digital tools function mainly as cross-sectoral general-purpose technologies rather than as drivers of classic Dutch disease factor displacement.

Stability in the post-Covid-19 era: On the third question, the relationship between ICT and the exchange rate did not change significantly after 2020, the manufacturing association was weaker rather than stronger after 2020 (interaction 0.020, p = .026), and there is little evidence that effects differ by resource dependence or income group. Apart from the weaker manufacturing association, the relationships between ICT, the exchange rate and sectoral shares were therefore broadly stable after the 2020 pandemic shock, and differences in resource dependence and income level did not systematically alter these baseline dynamics. The post-Covid window is, however, only two years long. Overall, the evidence does not support a Dutch disease effect of ICT through the spending channel and offers only limited, fragile support for a resource movement effect on manufacturing.

Policy Recommendations

The recommendations below follow from the results. They rest on associations that are not robust in every specification and should be read as directions for policy and monitoring rather than firm prescriptions.

- **Avoid pre-emptive exchange rate interventions.** ICT is not associated with real appreciation, so there is no case in this sample for exchange rate or monetary measures aimed specifically at offsetting a digital spending effect. Central banks and monetary authorities do not need to implement defensive exchange rate management or capital controls to offset digital expansion. Macroeconomic policy can instead focus on broader price stability and external buffer management, while the real exchange rate continues to be tracked.
- **Support manufacturing directly.** The negative association between ICT and the manufacturing share is not explained by the exchange rate, so exchange rate management would not address it. Governments should support the competitiveness of

manufacturers directly, for example by promoting digital adoption in factories and supply chains, investing in logistics and skills, and easing access to finance for exporters.

- **Spread digital tools beyond the ICT sector.** The non-negative association of ICT with agriculture and services suggests that digital tools can complement other sectors. Policy should widen access through infrastructure, affordability, digital skills and competition in telecommunications markets, so that the gains are not confined to the ICT sector.
- **Monitor structure and the real exchange rate as digital adoption deepens.** The effects may change with the level of ICT penetration and with the pace of digital services exports, which this study does not capture. Regular monitoring of sectoral shares and the real exchange rate would allow early detection of a Dutch disease pattern if one emerges.

Limitations and Future Research

The post-Covid period in the estimation sample is only two years (2020 and 2021), because the usable observations run from 2005 to 2021. The interaction terms have limited power, and later waves of data will allow sharper tests.

REER data cover only 47 of the 131 countries in the initial extract and the regressions use 37, so the sample leans towards better-documented economies, and the income-group regressions rest on as few as 5 clusters. Wider-coverage real exchange rate series, such as the Darvas (2012) database, would enlarge the sample.

Cross-sectional dependence and cointegration could not be tested on this unbalanced panel, the variables are of mixed order of integration, and the pooled mean group estimator did not return estimates. The long-run relationship is therefore not separated from the short run. A balanced subsample, dynamic spatial panel models or panel estimators that are resilient to cross-sectional dependence, such as the cross-sectionally augmented distributed lag (CS-DL) and autoregressive distributed lag (CS-ARDL) estimators could address this.

Identification is limited. System GMM relies on internal instruments, the GMM model for REER fails the AR(2) and Hansen tests, the sectoral GMM models use as many instruments as there are countries, and ICT is strongly correlated with income. Establishing causal identification will require an external, exogenous shock to digital infrastructure, such as proximity to submarine fibre-optic cable landing points, as used by Hjort and Poulsen (2019).

ICT is measured by access and use rather than by the size of the digital sector or its export earnings. Future work could use data on digital services exports or ICT sector value added.

Two further extensions follow from the design. First, the interaction between global deindustrialisation trends (Rodrik, 2016) and ICT adoption is absorbed only on average by the year effects; modelling it explicitly would ensure that the resource movement effect is not misattributed to ICT. Second, testing the exchange rate channel (H2c) formally requires a structural equation modelling (SEM) or system GMM mediation framework rather than a comparison of coefficients across equations.

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Appendix A: Data Series and Sources

Variable	WDI code	Original provider	Accessed through
Internet users (% of population)	IT.NET.USER.ZS	ITU	World Bank WDI
Mobile subscriptions (per 100)	IT.CEL.SETS.P2	ITU	World Bank WDI
Fixed broadband (per 100)	IT.NET.BBND.P2	ITU	World Bank WDI
Real effective exchange rate	PX.REX.REER	IMF IFS	World Bank WDI
Agriculture value added (% of GDP)	NV.AGR.TOTL.ZS	World Bank / OECD	World Bank WDI
Manufacturing value added (% of GDP)	NV.IND.MANF.ZS	World Bank / OECD	World Bank WDI
Services value added (% of GDP)	NV.SRV.TOTL.ZS	World Bank / OECD	World Bank WDI
GDP per capita (constant US$)	NY.GDP.PCAP.KD	World Bank / OECD	World Bank WDI
Trade (% of GDP)	NE.TRD.GNFS.ZS	World Bank / OECD	World Bank WDI
FDI net inflows (% of GDP)	BX.KLT.DINV.WD.GD.ZS	IMF	World Bank WDI
Remittances received (% of GDP)	BX.TRF.PWKR.DT.GD.ZS	IMF	World Bank WDI
Government consumption (% of GDP)	NE.CON.GOVT.ZS	World Bank / OECD	World Bank WDI
Natural resource rents (% of GDP)	NY.GDP.TOTL.RT.ZS	World Bank estimates	World Bank WDI
Net barter terms of trade	TT.PRI.MRCH.XD.WD	UNCTAD / World Bank	World Bank WDI
Real effective exchange rate (robustness)	n/a	Bruegel (Darvas)	Bruegel dataset page

Note: WDI indicator

Appendix B: Country Sample

Country	Income Group	Region
Afghanistan	Low income	Middle East, North Africa, Afghanistan & Pakistan
Angola	Lower middle income	Sub-Saharan Africa
Albania	Upper middle income	Europe & Central Asia
Argentina	Upper middle income	Latin America & Caribbean
Armenia	Upper middle income	Europe & Central Asia
Azerbaijan	Upper middle income	Europe & Central Asia
Burundi	Low income	Sub-Saharan Africa
Benin	Lower middle income	Sub-Saharan Africa
Burkina Faso	Low income	Sub-Saharan Africa
Bangladesh	Lower middle income	South Asia
Bosnia and Herzegovina	Upper middle income	Europe & Central Asia
Belarus	Upper middle income	Europe & Central Asia
Belize	Upper middle income	Latin America & Caribbean
Bolivia	Lower middle income	Latin America & Caribbean
Brazil	Upper middle income	Latin America & Caribbean
Bhutan	Lower middle income	South Asia
Botswana	Upper middle income	Sub-Saharan Africa
Central African Republic	Low income	Sub-Saharan Africa
China	Upper middle income	East Asia & Pacific
Cote d'Ivoire	Lower middle income	Sub-Saharan Africa
Cameroon	Lower middle income	Sub-Saharan Africa
Congo, Dem. Rep.	Low income	Sub-Saharan Africa
Congo, Rep.	Lower middle income	Sub-Saharan Africa
Colombia	Upper middle income	Latin America & Caribbean
Comoros	Lower middle income	Sub-Saharan Africa
Cabo Verde	Upper middle income	Sub-Saharan Africa
Cuba	Upper middle income	Latin America & Caribbean
Djibouti	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Dominica	Upper middle income	Latin America & Caribbean
Dominican Republic	Upper middle income	Latin America & Caribbean
Algeria	Upper middle income	Middle East, North Africa, Afghanistan & Pakistan
Ecuador	Upper middle income	Latin America & Caribbean
Egypt, Arab Rep.	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Eritrea	Low income	Sub-Saharan Africa
Ethiopia	Low income	Sub-Saharan Africa
Fiji	Upper middle income	East Asia & Pacific
Micronesia, Fed. Sts.	Upper middle income	East Asia & Pacific

Gabon	Upper middle income	Sub-Saharan Africa
Georgia	Upper middle income	Europe & Central Asia
Ghana	Lower middle income	Sub-Saharan Africa
Guinea	Lower middle income	Sub-Saharan Africa
Gambia, The	Low income	Sub-Saharan Africa
Guinea-Bissau	Low income	Sub-Saharan Africa
Equatorial Guinea	Upper middle income	Sub-Saharan Africa
Grenada	Upper middle income	Latin America & Caribbean
Guatemala	Upper middle income	Latin America & Caribbean
Honduras	Lower middle income	Latin America & Caribbean
Haiti	Lower middle income	Latin America & Caribbean
Indonesia	Upper middle income	East Asia & Pacific
India	Lower middle income	South Asia
Iran, Islamic Rep.	Upper middle income	Middle East, North Africa, Afghanistan & Pakistan
Iraq	Upper middle income	Middle East, North Africa, Afghanistan & Pakistan
Jamaica	Upper middle income	Latin America & Caribbean
Jordan	Upper middle income	Middle East, North Africa, Afghanistan & Pakistan
Kazakhstan	Upper middle income	Europe & Central Asia
Kenya	Lower middle income	Sub-Saharan Africa
Kyrgyz Republic	Lower middle income	Europe & Central Asia
Cambodia	Lower middle income	East Asia & Pacific
Kiribati	Lower middle income	East Asia & Pacific
Lao PDR	Lower middle income	East Asia & Pacific
Lebanon	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Liberia	Low income	Sub-Saharan Africa
Libya	Upper middle income	Middle East, North Africa, Afghanistan & Pakistan
St. Lucia	Upper middle income	Latin America & Caribbean
Sri Lanka	Upper middle income	South Asia
Lesotho	Lower middle income	Sub-Saharan Africa
Morocco	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Moldova	Upper middle income	Europe & Central Asia
Madagascar	Low income	Sub-Saharan Africa
Maldives	Upper middle income	South Asia
Mexico	Upper middle income	Latin America & Caribbean
Marshall Islands	Upper middle income	East Asia & Pacific
North Macedonia	Upper middle income	Europe & Central Asia
Mali	Low income	Sub-Saharan Africa
Myanmar	Lower middle income	East Asia & Pacific
Montenegro	Upper middle income	Europe & Central Asia
Mongolia	Upper middle income	East Asia & Pacific

Mozambique	Low income	Sub-Saharan Africa
Mauritania	Lower middle income	Sub-Saharan Africa
Mauritius	Upper middle income	Sub-Saharan Africa
Malawi	Low income	Sub-Saharan Africa
Malaysia	Upper middle income	East Asia & Pacific
Namibia	Lower middle income	Sub-Saharan Africa
Niger	Low income	Sub-Saharan Africa
Nigeria	Lower middle income	Sub-Saharan Africa
Nicaragua	Lower middle income	Latin America & Caribbean
Nepal	Lower middle income	South Asia
Pakistan	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Peru	Upper middle income	Latin America & Caribbean
Philippines	Upper middle income	East Asia & Pacific
Papua New Guinea	Lower middle income	East Asia & Pacific
Korea, Dem. People's Rep.	Low income	East Asia & Pacific
Paraguay	Upper middle income	Latin America & Caribbean
West Bank and Gaza	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Rwanda	Low income	Sub-Saharan Africa
Sudan	Low income	Sub-Saharan Africa
Senegal	Lower middle income	Sub-Saharan Africa
Solomon Islands	Lower middle income	East Asia & Pacific
Sierra Leone	Low income	Sub-Saharan Africa
El Salvador	Upper middle income	Latin America & Caribbean
Somalia, Fed. Rep.	Low income	Sub-Saharan Africa
Serbia	Upper middle income	Europe & Central Asia
South Sudan	Low income	Sub-Saharan Africa
Sao Tome and Principe	Lower middle income	Sub-Saharan Africa
Suriname	Upper middle income	Latin America & Caribbean
Eswatini	Lower middle income	Sub-Saharan Africa
Syrian Arab Republic	Low income	Middle East, North Africa, Afghanistan & Pakistan
Chad	Low income	Sub-Saharan Africa
Togo	Lower middle income	Sub-Saharan Africa
Thailand	Upper middle income	East Asia & Pacific
Tajikistan	Lower middle income	Europe & Central Asia
Turkmenistan	Upper middle income	Europe & Central Asia
Timor-Leste	Lower middle income	East Asia & Pacific
Tonga	Upper middle income	East Asia & Pacific
Tunisia	Lower middle income	Middle East, North Africa, Afghanistan & Pakistan
Turkiye	Upper middle income	Europe & Central Asia
Tuvalu	Upper middle income	East Asia & Pacific

Tanzania	Lower middle income	Sub-Saharan Africa
Uganda	Low income	Sub-Saharan Africa
Ukraine	Upper middle income	Europe & Central Asia
Uzbekistan	Lower middle income	Europe & Central Asia
St. Vincent and the Grenadines	Upper middle income	Latin America & Caribbean
Venezuela, RB	Lower middle income	Latin America & Caribbean
Viet Nam	Upper middle income	East Asia & Pacific
Vanuatu	Lower middle income	East Asia & Pacific
Samoa	Upper middle income	East Asia & Pacific
Kosovo	Upper middle income	Europe & Central Asia
Yemen, Rep.	Low income	Middle East, North Africa, Afghanistan & Pakistan
South Africa	Upper middle income	Sub-Saharan Africa
Zambia	Lower middle income	Sub-Saharan Africa
Zimbabwe	Lower middle income	Sub-Saharan Africa