

Does Financial Development or Natural Resource Wealth Protect Foreign Direct Investment in Sub-Saharan Africa from Global Economic Policy Uncertainty?

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Abstract: Sub-Saharan African governments are advised that deeper financial systems and larger resource endowments insulate foreign direct investment (FDI) from global shocks. We test both on 48 economies, 2000 to 2024. The Global Economic Policy Uncertainty (GEPU) index takes one value per year for every country, so with country but no year effects its coefficient cannot be separated from other global developments. Adding year effects strengthens the resource result by 29 per cent, so the standard design attenuates the effect it seeks. Resource dependence amplifies rather than dampens transmission: a one standard deviation higher oil rent share deepens the FDI response to a one-point rise in GEPU by 0.0087 percentage points of GDP, a contraction 2.1 points deeper over the 2020 spike for an economy at 40 per cent oil rents. Financial development does neither, and the null excludes protection above a quarter of a percentage point. The amplification is graded by sunk cost: largest for oil, 40 per cent as large for minerals, absent for the diffuse rents forming two thirds of the standard aggregate. Because a country characteristic multiplies a common shock, classical standard errors overstate precision by 40 per cent; randomisation inference returns $p = 0.017$.

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I. INTRODUCTION

Foreign direct investment is the external financing source that Sub-Saharan African governments court most actively and control least. Inflows are small relative to world totals, concentrated in a handful of destinations, and unstable from year to year in ways that bear little relation to domestic policy effort. A government can liberalise trade, establish a promotion agency and offer fiscal incentives, and still find that receipts move with conditions set elsewhere. The most prominent of those conditions has been policy uncertainty in the major economies, which in the index used here rises from an annual average of 66 in 2006 to 315 in 2020.

Two structural remedies dominate the policy response. The first is financial deepening: a domestic system that intermediates well should let a host absorb external shocks and substitute local for foreign finance when global capital retreats. The second is resource endowment: oil and mineral

wealth cannot be relocated, so investment seeking it should be less footloose than investment seeking markets, and resource-rich hosts should retain inflows when uncertainty rises. Both claims are widely repeated. Neither has been tested against a design capable of supporting it.

The obstacle is arithmetic rather than data. A global index takes one value per year. In a panel with country effects and no year effects, its coefficient is identified from variation it shares with every other global development of the period: the commodity cycle, the global liquidity cycle, United States monetary policy, the diffusion of value chains and the pandemic. Any of these can generate the estimated association and none can be excluded. Adding year effects solves the problem but removes the coefficient, which is why it is rarely done, and the conditional effects on which policy advice actually rests are then read from a specification that cannot deliver them.

The average effect of uncertainty on cross-border investment is not in dispute. Firm-level acquisition data, bilateral flows and multi-country panels point the same way, and the effect is larger where investment is more irreversible (Julio & Yook, 2016; Choi et al., 2021; Jardet et al., 2023). The theory is equally settled: when investment is sunk and information arrives over time, waiting has option value, so greater dispersion of expected returns raises the threshold return required to commit and postpones the decision (Bernanke, 1983; Dixit & Pindyck, 1994; Bloom, 2009). Financial frictions reinforce this, since uncertainty widens spreads and raises the external finance premium precisely when firms would draw on it (Gilchrist et al., 2014; Gulen & Ion, 2016). Work on African hosts reaches the same conclusion about the average and begins to ask about the conditional, reporting that institutional quality shapes the association (Avom et al., 2020; Ogbonna et al., 2022) and that host characteristics interact with uncertainty in explaining FDI determinants (Canh et al., 2020), alongside a broader literature on the region's investment drivers (Asiedu, 2002; Mijiyawa, 2015; Ezeoha & Cattaneo, 2012). At the macroeconomic level, financial development reduces the output cost of uncertainty shocks (Carrière-Swallow & Céspedes, 2013; Karaman & Yildirim-Karaman, 2019), and one strand reports the same for inflows (Nguyen & Lee, 2021).

The resource case is weaker than its popularity suggests, because the same theory supports the opposite prediction. Extractive projects are the most irreversible investments in the region and have the longest payback horizons, so by the option-value logic they should be the most sensitive to uncertainty. Resource-dependent economies also transmit global shocks into larger domestic fiscal and exchange rate swings (van der Ploeg & Poelhekke, 2009; Cavalcanti et al., 2015), and a booming extractive sector contracts the tradable activity that might otherwise diversify exposure (Corden & Neary, 1982). Evidence that resource endowment fails to attract non-extractive investment is consistent with resource FDI being a distinct and more fragile category rather than an anchor (Poelhekke & van der Ploeg, 2013).

We proceed in three steps: replicate the conventional specification exactly, add year effects so the interaction becomes the identified quantity, and treat inference as a first-order question rather than a formality. Four contributions follow. First, on identification, adding year effects raises the estimated resource interaction by 29 per cent and improves its precision under every variance estimator, so the standard omission has been biasing the literature toward understatement. Second, on inference, the same coefficient carries p-values from 0.003 to 0.075 across four estimators, because a country characteristic multiplied by a common shock has effective degrees of freedom governed by the number of years; randomisation inference returns $p = 0.017$. Third, on measurement, disaggregating total rents recovers an ordering the aggregate conceals, running from oil through minerals to nothing, which tracks sunk cost and payback horizon. Fourth, on transparency, the specified models retain 42 of 48 countries and 784 of 1,200 country-years, and

relaxing one control that is missing for Nigeria throughout restores four countries and nearly doubles the estimate.

Section 2 sets out data and method, Section 3 the results, Section 4 the discussion and policy implications, and Section 5 concludes.

II. METHODOLOGY

➤ Framework and Hypotheses

The option-value account is the organising frame. An investment that is partly irreversible and can be deferred carries an option value of waiting, so an increase in the dispersion of expected returns raises the threshold return at which a firm commits and postpones investment even when expected returns are unchanged (Bernanke, 1983; Dixit & Pindyck, 1994). The magnitude scales with irreversibility, so the account predicts heterogeneity across hosts according to the composition of the investment they attract rather than a uniform response. The question is therefore conditional by construction.

Financial frictions supply a second channel. Uncertainty raises the external finance premium and the value of internal funds, so constrained firms cut investment more than unconstrained ones (Gilchrist et al., 2014; Gulen & Ion, 2016). At the level of a host economy, deeper intermediation should substitute for the retrenchment of parent-firm balance sheets and allow local partners to co-finance when external funding tightens.

- Hypothesis 1. Financial development attenuates the negative response of FDI to global economic policy uncertainty.

For resources the same theory yields opposing predictions. The protective reading holds that resource-seeking investment is locationally bound, so the marginal value of waiting is lower than for investment choosing among production platforms. The amplifying reading holds that extractive projects are the most irreversible and longest-horizon investments available, that they carry commodity price risk which itself rises with global uncertainty, and that resource-dependent economies transmit global shocks into domestic instability more strongly. We state the hypothesis as policy advice implies it and test it against its opposite.

- Hypothesis 2. Natural resource dependence attenuates the negative response of FDI to global economic policy uncertainty.

If the amplifying mechanism runs through irreversibility and payback horizon, any resource effect should be graded by the character of the extractive activity rather than uniform across whatever national accounts classify as a rent.

- Hypothesis 3. Any resource effect is concentrated in capital-intensive, long-horizon extractive activity and is absent for diffuse rents.

➤ *Data*

The panel is annual and covers all 48 Sub-Saharan African economies from 2000 to 2024, giving 1,200 potential country-years. The dependent variable is FDI net inflows as a share of GDP. Financial development is domestic credit to the private sector as a share of GDP. Resource dependence is measured by oil, mineral and coal rents and by total natural resource rents, each as a share of GDP; the difference between total rents and the sum of the three named components is natural gas and forest rents, which we treat as

a separate residual. Controls are trade openness, consumer price inflation, GDP per capita in current dollars and population. These come from the World Development Indicators (World Bank, 2025). Global uncertainty is the annual Global Economic Policy Uncertainty index, a weighted average of national indices built from newspaper coverage (Baker et al., 2016; Davis, 2016). Table 1 defines every variable and gives its source, and Table 2 reports descriptive statistics.

Table 1 Variables, Definitions and Sources

Variable	Definition	Source
FDI	Foreign direct investment, net inflows (% of GDP)	World Development Indicators
GEPU	Global Economic Policy Uncertainty index, annual	policyuncertainty.com; Baker et al. (2016); Davis (2016)
FD	Domestic credit to the private sector (% of GDP)	World Development Indicators
OIL	Oil rents (% of GDP)	World Development Indicators
MR	Mineral rents (% of GDP)	World Development Indicators
CR	Coal rents (% of GDP)	World Development Indicators
NR	Total natural resource rents (% of GDP)	World Development Indicators
OTHER	Natural gas and forest rents, computed as NR minus OIL, MR and CR	Derived
TRADE	Trade (% of GDP)	World Development Indicators
INFL	Inflation, consumer prices (annual %)	World Development Indicators
GDPCAPITACUR	GDP per capita (current US\$)	World Development Indicators
POP	Population, total	World Development Indicators
GDPGROWTH	GDP growth (annual %)	World Development Indicators
EMP	Employment	World Development Indicators

- *Notes.* All series are taken from the workbook underlying the study. The natural gas and forest residual is computed

as total natural resource rents less oil, mineral and coal rents. Rent series are available through 2021.

Table 2 Descriptive Statistics, 48 Countries, 2000-2024

Variable	N	Mean	Median	SD	Min	Max
FDI inflows (% of GDP)	1170	4.30	2.52	7.66	-17.29	103.34
GEPU index	1200	149.33	128.69	68.08	66.32	315.37
Domestic credit to the private sector (% of GDP)	1028	19.09	13.29	21.53	0.00	142.42
Trade openness (% of GDP)	999	68.08	58.40	34.07	2.00	222.18
Inflation (%)	1090	10.91	5.36	34.73	-16.86	557.20
GDP per capita (current US\$)	1170	2097.44	956.47	2947.75	109.59	19,142
Population	1200	20,002,257	11,064,620	31,072,577	81,131	232,679,478
Oil rents (% of GDP)	1010	3.86	0.00	10.30	0.00	82.78
Mineral rents (% of GDP)	1018	1.21	0.02	3.06	0.00	28.81
Coal rents (% of GDP)	1010	0.64	0.00	3.46	0.00	48.72
Natural gas and forest rents (% of GDP)	1014	5.73	3.78	5.70	-0.00	40.41
Total natural resource rents (% of GDP)	1014	11.42	7.86	11.02	0.00	88.59

- *Notes.* Counts are non-missing observations out of 1,200 potential country-years. FDI is reported unwinsorised here; the estimations winsorise it at the 1st and 99th percentiles, which are -3.82 and 33.97 per cent of GDP.

All moderating variables are standardised, so an interaction coefficient reads as the change in the FDI response to a one-point movement in GEPU associated with a one standard deviation difference in that characteristic. One standard deviation is 10.3 percentage points of GDP for oil

rents, 3.1 for mineral rents and 21.5 for private credit, which fixes the scale for every magnitude reported below. GEPU is centred at its sample mean, so the direct effect of a moderating variable is evaluated at average global uncertainty rather than at zero, a value the index never approaches.

Three features of the data govern what can be estimated. GEPU is identical across all 48 countries within each year, so it carries no cross-sectional variation and the panel contains

25 distinct values of the treatment rather than 1,200. The resource rent series end in 2021, so any resource specification is estimated over 2000 to 2021 whatever the nominal window; specifications using only financial development extend to 2024. And coverage of the controls is incomplete in a way correlated with the variables of interest, which Section 2.6 addresses directly.

Two measurement choices follow from the data rather than from convention. Because FDI as a share of GDP reaches 103 per cent in one country-year and falls to minus 17 per cent in another, the dependent variable is winsorised at the first and ninety-ninth percentiles for the main results

and reported unwinsorised in the robustness table. And because total natural resource rents is a sum whose components behave very differently, we decompose it rather than treating it as a single construct. The residual category, natural gas and forest rents, is dominated in this region by fuelwood harvest measured in the national accounts, an activity that is diffuse, labour-intensive and unconnected to foreign investment, whereas oil, gas and hard-rock mining are point-source and capital-intensive. Treating these as one variable imposes a homogeneity restriction that Section 3.5 tests and rejects. Figure 1 plots the uncertainty index against mean inflows for oil-dependent and other economies and previews the pattern the estimates formalise.

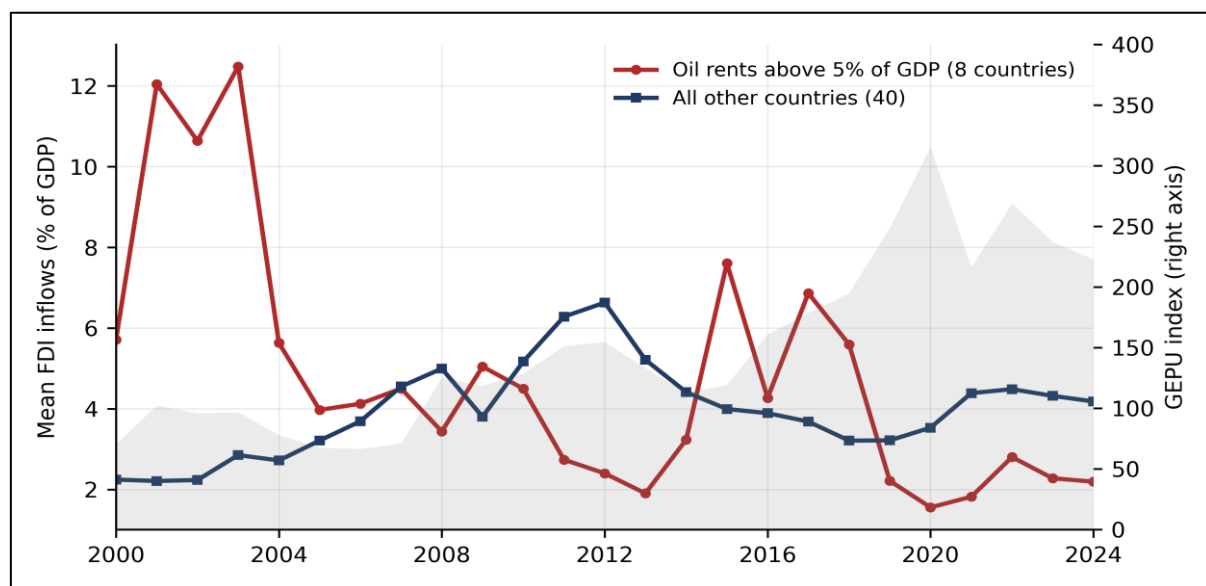


Fig 1 Global Policy Uncertainty and FDI Inflows, 2000-2024

- Notes. Shaded area: the annual GPEU index, right axis. Lines: unweighted mean FDI inflows as a share of GDP within each group. Oil-dependent economies are those whose mean oil rents over the sample exceed 5 per cent of GDP: Angola, Chad, the Republic of Congo, Equatorial Guinea, Gabon, Nigeria, South Sudan and Sudan.

➤ Specification

Two specifications are estimated. The first is the conventional one, reported so that later departures are traceable to a specific change:

$$FDI(i,t) = \alpha(i) + \rho \cdot FDI(i,t-1) + \beta_1 \cdot GPEU(t) + \beta_2 \cdot M(i,t) + \beta_3 \cdot [M(i,t) \times GPEU(t)] + \gamma'X(i,t) + \varepsilon(i,t) \quad (1)$$

Where $M(i,t)$ is the standardised moderating variable, X the control vector and $\alpha(i)$ country effects. There are no year effects, which is what permits β_1 to be estimated and what makes it uninterpretable.

The second adds year effects $\lambda(t)$:

$$FDI(i,t) = \alpha(i) + \lambda(t) + \rho \cdot FDI(i,t-1) + \beta_2 \cdot M(i,t) + \beta_3 \cdot [M(i,t) \times GPEU(t)] + \gamma'X(i,t) + \varepsilon(i,t) \quad (2)$$

Here $\lambda(t)$ absorbs the global index and every other global development of that year, observed or not. The main effect of uncertainty is no longer estimated, which is appropriate because it was never identified. What remains, β_3 , is the quantity the research question concerns: whether FDI in economies with more resource dependence, or deeper financial systems, responds differently to global uncertainty than FDI elsewhere. The lagged dependent variable is retained because FDI is persistent and because the panel unit

root evidence places the dependent variable near the stationarity boundary in levels. Its coefficient of roughly 0.37 implies a long-run multiplier of 1.59.

➤ What the Interaction Identifies

The move from equation (1) to equation (2) changes the estimand, and it is worth stating what survives. In equation (2), β_3 is the derivative of the FDI response to global uncertainty with respect to the moderating characteristic. It

answers the question the title poses, which is comparative: does investment in a more resource-dependent or more financially developed economy react differently from investment elsewhere? It does not answer, and cannot answer, what global uncertainty does to the region as a whole, because that quantity has no within-sample counterfactual. Every country experiences the same index in the same year, so the average effect is absorbed into the year effects along with everything else common to that year.

This is a limitation of the data rather than of the method, and it applies with equal force to studies that appear to estimate the average effect by omitting year effects. Those studies do not recover the average effect of uncertainty; they recover the combined effect of uncertainty and every contemporaneous global development, with weights determined by the covariance between them. The comparative quantity is the one the data can support, and it is also the quantity that policy requires, since a government cannot change the level of global uncertainty but can change the structural features that govern its own exposure to it.

The design has a further implication for interpretation. Because exposure is measured contemporaneously in the main specification, a country that becomes more resource-dependent during the sample contributes to identification through that change as well as through its level. We report a variant in which exposure is fixed at its 2000 to 2004 average, which removes any possibility that rents respond to the same shocks as FDI, at the cost of misclassifying economies whose extractive sectors came on stream later.

➤ *Estimation and Inference*

Estimation is by least squares with country and year dummies. Four questions about the error structure are settled empirically rather than asserted. Whether fixed effects are preferred to random effects is tested. Whether residuals retain serial correlation after the lagged dependent variable is included is tested by regressing the first-differenced residual on its own lag, under which the null of no first-order serial correlation implies a coefficient of minus one half. Cross-sectional dependence is tested with the Pesaran (2015) CD statistic, and stationarity with the Pesaran (2007) cross-sectionally augmented panel unit root test, which is appropriate precisely because dependence is present.

Inference requires more care than is customary here. The regressor of interest is the product of a country characteristic and a variable common to all countries in a year, so within a country it varies only through the global series. Errors correlated within countries therefore inflate its sampling variance well beyond what classical panel standard errors imply, and the effective number of independent shocks is the number of years, not country-years. We report classical, country-clustered, two-way clustered and Driscoll and Kraay (1998) standard errors side by side, with two-way clustering as the primary choice because it allows arbitrary correlation in both dimensions.

Because none of these is exact with 42 cross-sections and roughly twenty effective time-series observations, we

also report randomisation inference. Country exposure profiles are reassigned at random across countries 2,000 times, the model re-estimated on each draw, and the observed coefficient compared with the resulting placebo distribution. The procedure holds three things fixed that asymptotic inference must approximate: the marginal distribution of exposure, which is highly skewed here; the time-series structure of the shock, including its trend and its 2020 spike; and the panel dimensions, including the unbalanced contribution of individual countries. What varies across draws is only which country receives which exposure profile, which is precisely the assignment the null hypothesis concerns. A p-value from this procedure therefore requires no assumption about the number of independent clusters in either dimension. As a check on the lagged dependent variable, which introduces Nickell (1981) bias of order one over T , we report a static specification and two-step system and difference GMM estimators (Blundell & Bond, 1998) with collapsed instruments, the Windmeijer (2005) correction and the instrument count held below the number of cross-sections (Roodman, 2009).

➤ *The Estimation Sample*

The nominal panel is 1,200 country-years. Specifications with the full control set are estimated on 784 observations from 42 countries, an average of 18.7 years per country rather than 25, and the distribution is uneven: Malawi contributes five, Eswatini and South Sudan seven, Ethiopia eleven. We report this because it is material and because any reader can recover it from published diagnostics.

The attrition is not random. Six countries leave the resource specifications entirely. Eritrea and Somalia lack the dependent variable or the resource series for most of the period, which is missingness in the ordinary sense. Burundi, Liberia, Nigeria and São Tomé and Príncipe leave for a different reason: trade openness is missing for them throughout, so one control removes them although the dependent variable, the resource series and the other controls are complete. Nigeria is the region's largest economy and largest oil producer, and a study of oil dependence and foreign investment that excludes it is measuring something narrower than it appears to. We therefore treat the control set as an object of analysis and report in Section 3.7 what happens when trade openness is relaxed. Table A2 gives coverage for every country.

III. RESULTS

➤ *Replication and the Estimation Sample*

Before any change of specification, the eight regressions of the source study were re-estimated on the same workbook. Table A1 compares them. Every coefficient reproduces to seven decimal places, and the observation counts, parameter counts, R-squared values, residual sums of squares, standard errors of regression and F statistics all match exactly. What follows are therefore differences of method, not of data.

The replication also fixes the true dimensions of the estimation samples, which the source study does not state and

which can be recovered exactly from the printed diagnostics. The sum of squared residuals divided by the squared standard error of the regression gives the residual degrees of freedom, and the Akaike criterion together with the log likelihood then pins down the observation and parameter counts; the Schwarz and Hannan-Quinn criteria, which are not used in the solution, reproduce to six decimal places, confirming the recovery.

The two bivariate specifications retain all 48 countries. Every specification with the full control set retains 42, and the largest such sample is 787 of 1,200 potential country-years. Coverage is also uneven within the retained group, averaging 18.7 annual observations per country: Malawi contributes five, Eswatini and South Sudan seven, and Ethiopia eleven, while twenty-nine countries contribute twenty or more. An unbalanced panel is not itself a problem, but it means that the effective weight of individual economies in the estimate departs substantially from equal weighting, which Section 3.7 examines directly.

➤ Diagnostics

Table 3 reports the specification tests. The Hausman statistic is 179.36 on six degrees of freedom with a p-value below 0.001, so fixed effects are strongly preferred to random effects. The differenced-residual test returns a coefficient of minus 0.489 against a null value of minus one half, with $t = 0.35$ and $p = 0.726$, so the dynamic specification leaves no detectable first-order serial correlation.

Table 4 reports cross-sectional dependence and panel unit root tests. Dependence is pervasive: the CD statistic

rejects the null of weak dependence at any conventional level for every series, from 7.33 for FDI to 118.03 for income, with private credit at 59.76 and total rents at 28.75. This has two consequences. It makes year effects desirable on statistical as well as identification grounds, since a common factor structure is what year effects absorb. And it makes inference that assumes independence across countries unsafe. The unit root test does not reject at five per cent for FDI, private credit, trade openness, mineral rents, total rents or income in levels, and rejects decisively for all series in first differences, which supports retaining the lagged dependent variable.

Taken together the two tables settle three modelling choices that are usually asserted. Fixed effects are appropriate because the Hausman test says so, not because panel studies conventionally use them. The lagged dependent variable is justified because the residuals are serially uncorrelated once it is included and because the series are close to non-stationary in levels without it. And year effects are justified twice over, by the identification argument of Section 3.3 and by cross-sectional dependence strong enough that a common factor structure is the natural description of the data.

Variance inflation factors for the regressor set range from 1.03 to 3.08, so multicollinearity among the variables actually used is not a concern. This is worth stating precisely, because the published correlation matrix contains four pairs above 0.60 and one at 0.967, between constant-price and current-price GDP per capita. Those two are never entered together, so the conclusion holds, but it does not follow from the claim that all correlations are low.

Table 3 Specification Tests

Test	Statistic	p	N	Conclusion
Hausman test, fixed versus random effects	chi-square(6) = 179.36	<0.001	895	Fixed effects preferred
First-order serial correlation in residuals	rho = -0.489, t = 0.35	0.726	811	Null of no serial correlation, rho = -0.5, not rejected

- Notes. Both tests use the baseline dynamic specification with country effects. The serial correlation test regresses

the first-differenced residual on its own lag; under the null of no first-order serial correlation the coefficient equals -0.5.

Table 4 Cross-Sectional Dependence and Panel Unit Roots

Series	Pesaran CD	p	CIPS level	CIPS first difference	Countries
FDI inflows	7.33	<0.001	-2.252	-3.956	47
Domestic credit	59.76	<0.001	-1.816	-3.253	44
Trade openness	8.55	<0.001	-1.486	-3.446	42
Inflation	28.15	<0.001	-2.948	-4.986	46
Oil rents	7.89	<0.001	-2.275	-2.993	25
Mineral rents	25.42	<0.001	-1.441	-3.116	37
Total resource rents	28.75	<0.001	-2.078	-3.097	46
GDP per capita	118.03	<0.001	-1.267	-3.309	47

- Notes. The Pesaran (2015) CD statistic tests the null of weak cross-sectional dependence. The Pesaran (2007) cross-sectionally augmented panel unit root statistic is reported for levels and first differences; the 5 per cent critical value is -2.34, so values above it fail to reject a unit root.

➤ Identification: what Year Effects do

Table 5 makes the identification problem and its resolution visible, and Figure 4 displays the same estimates graphically in panel (a). The upper panel is the conventional specification with country effects only; the lower panel adds

year effects. The reported coefficient is the interaction between standardised oil rents and GEPU.

With country effects alone the interaction is 0.0068 in absolute value. With year effects it is 0.0087, an increase of 29 per cent, and precision improves under all four variance estimators. This reverses the usual presumption. Controlling for the global environment more thoroughly does not absorb the effect; it removes an offsetting confound and reveals a larger one. Global uncertainty is positively correlated with other global developments that raised investment in resource-dependent economies over this period, most obviously the commodity cycle, and a specification that lets one coefficient carry all of them nets the two influences against each other.

The direct GEPU coefficient in the conventional specification is 0.0011 in absolute value with a standard error of 0.0022. This is not evidence that global uncertainty leaves FDI unaffected. It demonstrates that the coefficient is not identified, since it competes with every other global development of the period and those do not all push the same way. The source study reports a coefficient of the same order

and reads its insignificance as a substantive finding about the time-invariance of GEPU within a fixed-effects framework. The reading is understandable but the quantity does not support it: an unidentified coefficient close to zero is uninformative about the underlying effect, whatever its standard error.

The direction of the change when year effects are added is itself informative about the confound. Had global uncertainty simply been standing in for a general downturn affecting all countries, absorbing the global year would have left the interaction unchanged, since a common shock cannot generate differential responses. Had it been standing in for something that hurt resource-dependent economies specifically, the interaction would have fallen. It rises, which means the omitted global variation was working in the opposite direction to the uncertainty channel for exactly the economies whose exposure identifies the interaction. The commodity supercycle is the obvious candidate, and Section 3.7 shows that the sub-period in which it dominates is precisely the sub-period in which the interaction cannot be detected.

Table 5 What Year Effects do to the Resource Interaction

Specification	Variance estimator	Coefficient	Std. error	p	N	Countries
Country effects only, as published	Classical	-0.006796	(0.002848)	0.0173	784	42
Country effects only, as published	Clustered on countries	-0.006796	(0.004546)	0.1353	784	42
Country effects only, as published	Clustered on countries and years	-0.006796	(0.003198)	0.0339	784	42
Country effects only, as published	Driscoll-Kraay	-0.006796	(0.003660)	0.0638	784	42
Country and year effects	Classical	-0.008749	(0.002942)	0.0030	784	42
Country and year effects	Clustered on countries	-0.008749	(0.004900)	0.0746	784	42
Country and year effects	Clustered on countries and years	-0.008749	(0.003419)	0.0107	784	42
Country and year effects	Driscoll-Kraay	-0.008749	(0.003500)	0.0126	784	42

➤ *Notes.* The reported coefficient is on the interaction between standardised oil rents and the GEPU index, from the otherwise identical specification. The upper panel is the published design. The lower panel adds year fixed effects, which absorb the GEPU main effect.

➤ *Main Results*

Table 6 reports the main specifications, all with country and year effects and two-way clustered standard errors.

The oil interaction is 0.0087 in absolute value with a standard error of 0.0034 in column 2, and 0.0098 with a standard error of 0.0042 when financial development enters alongside in column 4. The sign contradicts Hypothesis 2. Oil dependence does not shield foreign investment from global uncertainty; it makes that investment more sensitive to it. The direct effect of oil rents is also strongly negative, at 3.23 percentage points of GDP per standard deviation, so oil-dependent economies receive less foreign investment on average and lose more of it when uncertainty rises.

The magnitude is material. GEPU rose by 66.8 points between 2019 and 2020. Applying the point estimate, an economy with oil rents at 40 per cent of GDP experienced a

contraction 2.05 percentage points of GDP deeper than one at the sample mean of 3.9 per cent, rising to 3.25 points in the long run given persistence of 0.370. At 20 per cent oil rents the figures are 0.92 and 1.45 points. Against mean FDI of 4.3 per cent of GDP, these are large differences. Figure 2 traces the differential effect across the observed range of oil dependence with its confidence band.

Financial development shows nothing. The interaction is 0.0013 with a standard error of 0.0013 in column 3 and 0.0004 with a standard error of 0.0011 in column 4, and the direct effect is also insignificant. The point estimate is positive in both, which is the direction Hypothesis 1 predicts, but it is an order of magnitude smaller than the resource interaction and nowhere near conventional significance. It is not being suppressed by a correlated regressor. Private credit and oil rents correlate at minus 0.175 across the panel and the two interactions at minus 0.164, so the terms are close to orthogonal, and removing the resource terms moves the financial development interaction only from 0.0004 to 0.0013 while leaving its p-value at 0.303. Hypothesis 1 is rejected, and rejected informatively rather than for want of power. The upper bound of the ninety-five per cent interval implies that a one standard deviation increase in private credit, 21.5

percentage points of GDP, changed the FDI response to the 2020 shock by at most 0.26 percentage points of GDP, about six per cent of mean FDI. Protection of the magnitude policy discussion assumes can be ruled out.

The controls behave as expected and are stable across columns. Trade openness carries a positive coefficient between 0.065 and 0.086 and is significant at one per cent throughout, which is the single most reliable determinant of inflows in these data. Inflation carries a small positive coefficient that is significant in three of six columns, and income per capita and population are individually insignificant once country and year effects are present, as they largely should be since both are close to deterministic

within a country over this horizon. Persistence is high and precisely estimated, between 0.370 and 0.400 across specifications.

Columns 5 and 6 substitute total resource rents and mineral rents. Both interactions are negative and significant, at 0.0055 and 0.0036 in absolute value. The mineral result departs from the conventional specification, under which that interaction is insignificant, and it matters for interpretation: the amplification is not confined to hydrocarbons, as the source study concluded, but extends to hard-rock extraction at roughly 40 per cent of the oil magnitude. What separates the categories that respond from those that do not is examined next.

Table 6 Main Results

Variable	(1)	(2)	(3)	(4)	(5)	(6)
FDI (t-1)	0.399750*** (0.080296)	0.369763*** (0.089226)	0.399466*** (0.094794)	0.379541*** (0.100486)	0.384079*** (0.093277)	0.393962*** (0.095535)
GEPU	-0.001084 (0.002197)					
Oil rents		- 3.227651*** (0.722481)		- 3.204334*** (0.748119)		
Oil rents x GEPU		-0.008749** (0.003419)		-0.009830** (0.004202)		
Financial development			0.917818 (0.765389)	0.318988 (0.961574)		
Financial development x GEPU			0.001326 (0.001287)	4.18e-4 (0.001124)		
Total resource rents					- 1.967316*** (0.610360)	
Total resource rents x GEPU					-0.005534** (0.002334)	
Mineral rents						-0.005728 (0.079880)
Mineral rents x GEPU						-0.003629* (0.001960)
Population	1.74e-8 (3.18e-8)	4.97e-8 (5.54e-8)	5.02e-9 (4.09e-8)	4.70e-8 (7.04e-8)	4.23e-8 (5.07e-8)	2.21e-8 (4.77e-8)
Trade openness	0.068034*** (0.017765)	0.085663*** (0.027868)	0.065478*** (0.020161)	0.084306*** (0.030665)	0.085324*** (0.024807)	0.070694*** (0.020669)
Inflation	0.003630** (0.001767)	0.003420 (0.002872)	0.004175 (0.002776)	0.003069 (0.003708)	0.005303*** (0.002001)	0.005063** (0.002047)
GDP per capita	-1.52e-4 (1.60e-4)	1.95e-5 (1.83e-4)	-3.16e-4* (1.91e-4)	-6.73e-5 (2.47e-4)	-1.83e-4 (1.67e-4)	-2.44e-4 (1.64e-4)
Observations	895	784	785	709	787	787
Countries	42	42	41	41	42	42
Within R-squared	0.263	0.295	0.254	0.290	0.267	0.255

- Notes. Standard errors clustered on countries and years in parentheses. All specifications include country and year fixed effects. Moderating variables are standardised. ***

$p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Column 1 retains country effects only so that the GEPU main effect can be shown; it is not separately identified and is reported for completeness.

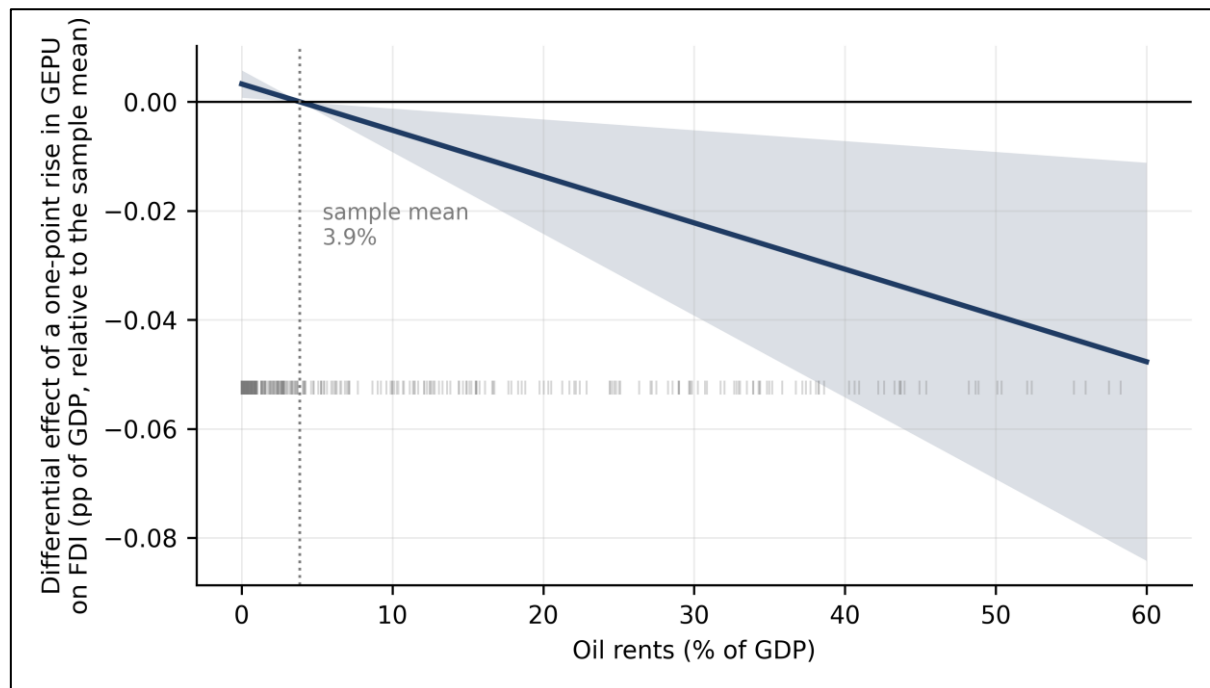


Fig 2 Differential Effect of Global Uncertainty on FDI Across Levels of Oil Dependence

- *Notes.* Point estimates and 95 per cent confidence intervals of the effect of a one-point rise in GEPU on FDI at each level of oil rents, expressed relative to an economy at the sample mean of 3.9 per cent, computed from Table 6, column 2. Tick marks show the observed distribution of oil rents.

➤ *Which Resources, and Why the Aggregate Underperforms*

Table 7 and Figure 3 disaggregate total rents into components, each entered with its own interaction. The ordering is clear and not arbitrary: 0.0087 for oil ($p = 0.011$), 0.0036 for minerals ($p = 0.064$), 0.0010 for coal ($p = 0.450$) and 0.0009 for the natural gas and forest residual ($p = 0.848$). Total rents, the standard aggregate, gives 0.0055 ($p = 0.018$), between the components it combines.

The direct effects reinforce the same reading. Oil rents carry a direct coefficient of 3.23 percentage points of GDP per standard deviation with a p -value below 0.001 and total rents 1.97 with a p -value of 0.001, while mineral, coal and residual rents are individually indistinguishable from zero in levels. Resource dependence therefore operates on the level

of inflows through hydrocarbons specifically, and on their sensitivity to global conditions through extractive intensity more broadly.

Two implications follow. The amplification is graded by the character of the extractive activity rather than by resource abundance as such. Oil and gas projects involve the largest sunk commitments and longest payback horizons, hard-rock mining smaller ones, and the residual is dominated by forest rents reflecting subsistence harvest that attracts no foreign investment. That ordering is what the option-value account predicts, and it supports Hypothesis 3.

The second implication concerns measurement. The natural gas and forest residual averages 66.6 per cent of total measured rents in this sample. An aggregate in which two thirds of the measured quantity is inert will attenuate whatever signal the remainder carries, and its performance will vary across samples with the composition of rents rather than with the economics. That is a concrete explanation for the instability of findings in a literature relying almost exclusively on the aggregate.

Table 7 Which Component of Natural Resource Rents?

Rent component	Interaction with GEPU	Std. error	p-value	Direct effect	N
Oil rents	-0.008749**	(0.003419)	0.011	-3.2277***	784
Mineral rents	-0.003629*	(0.001960)	0.064	-0.0057	787
Coal rents	-0.001031	(0.001363)	0.450	0.1705	784
Natural gas and forest rents	-0.000882	(0.004589)	0.848	0.6528	787
Total natural resource rents	-0.005534**	(0.002334)	0.018	-1.9673***	787

- *Notes.* Standard errors clustered on countries and years in parentheses. All specifications include country and year fixed effects. Moderating variables are standardised. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Each row is a separate

regression in which the stated rent component replaces oil rents, entered both directly and interacted with GEPU. The natural gas and forest residual averages 66.6 per cent of total measured rents.

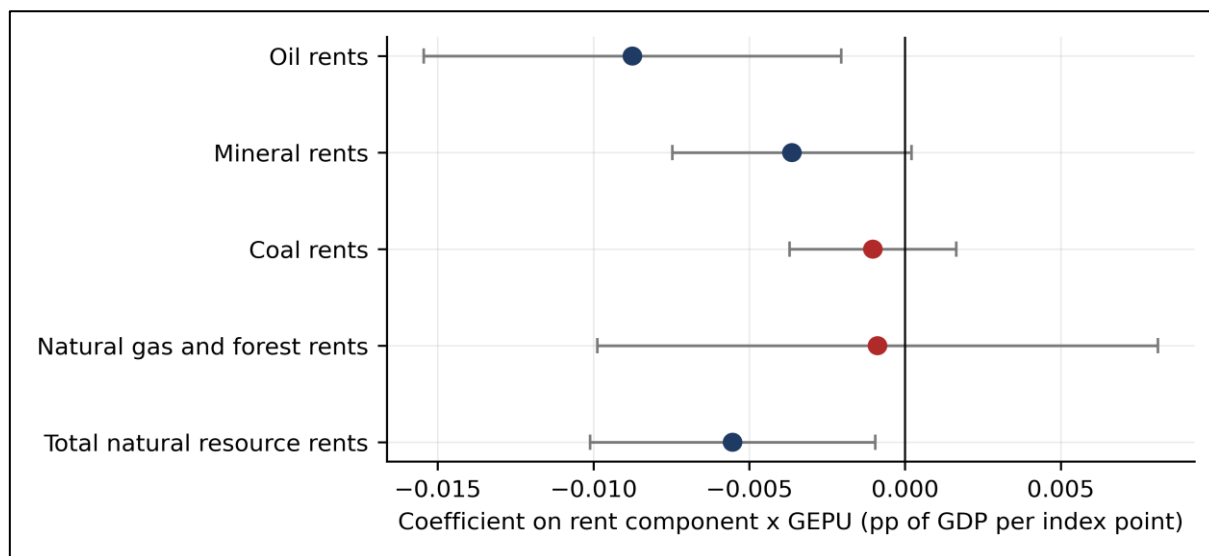


Fig 3 The Interaction by Rent Component

- *Notes.* Coefficients on the interaction between each rent component and GEPU, with 95 per cent confidence intervals, from the separate regressions of Table 7. Navy denotes significance at the 10 per cent level, red otherwise.

➤ Inference

The four estimators in Table 5 agree on the point estimate and disagree on precision. For the preferred specification the interaction of 0.0087 carries a p-value of 0.003 under classical standard errors, 0.075 under country clustering, 0.011 under two-way clustering and 0.013 under Driscoll-Kraay. Figure 4a shows the corresponding intervals.

To see how much is at stake, note that the four estimators differ by a factor of two in the standard error while agreeing on the coefficient to six decimal places. A reader shown only the classical result would conclude that the interaction is significant at one per cent; a reader shown only the country-clustered result would conclude that it falls short of five per cent. Both would be reading the same estimate.

The spread is not a nuisance to be resolved by choosing the smallest number. It follows directly from the structure of the regressor. Because the interaction varies within a country only through a series common to all countries, errors correlated within countries inflate its sampling variance far beyond what independence implies. Country clustering is the most conservative of the four; two-way clustering additionally allows correlation across countries within a year. The classical estimator, which is the default output of standard panel software and the usual basis for reporting results of this kind, understates the standard error by about 40 per cent relative to two-way clustering and about 70 per cent relative to country clustering.

Randomisation inference settles the question without relying on these approximations. Reassigning country oil exposure at random 2,000 times places the observed coefficient in the 1.7th percentile of the placebo distribution, $p = 0.017$. Figure 4b shows that distribution. Associations this strong arise about once in sixty draws under random assignment.

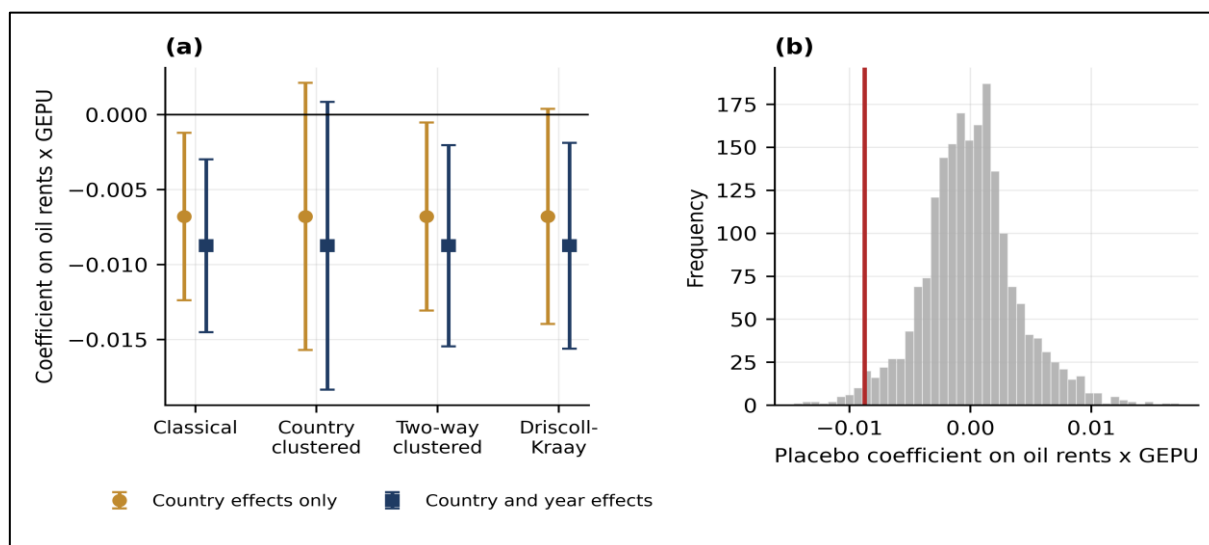


Fig 4 Inference on the Resource Interaction

- *Notes.* Panel (a): the estimate and its 95 per cent interval under four variance estimators, for the published design and with year effects added. Panel (b): the distribution of placebo coefficients from 2,000 random reassignments of country oil exposure, with the observed estimate marked.

➤ *Robustness*

Table 8 reports the robustness suite. The point estimate is stable across every variation and in several cases larger than the baseline.

The most consequential check concerns the control set. Dropping trade openness restores Burundi, Liberia, Nigeria and São Tomé and Príncipe, raising the sample from 784 observations across 42 countries to 912 across 46, and the interaction rises in absolute value from 0.0087 to 0.0150 with a standard error of 0.0028. Removing all controls retains 48 countries and 965 observations and gives 0.0153 with a standard error of 0.0026. The baseline is therefore conservative, and the attenuation is attributable to the loss of the region's largest oil producer rather than to conditioning as such.

Two results correct the interpretation of the original robustness exercise. Excluding 2020 leaves the interaction at 0.0075 with a standard error of 0.0028, and excluding 2020 and 2021 leaves it at 0.0062 with a standard error of 0.0016. Precision improves rather than deteriorates in both cases, so the finding is not an artefact of the pandemic year, which is the natural concern given that 2020 is the largest uncertainty shock in the sample.

One further variation bears on how the sample is weighted. Restricting attention to the twenty-nine countries contributing twenty or more annual observations, which removes the least balanced cases, gives 0.0098 with a standard error of 0.0051 on 608 observations, marginally larger than the baseline and significant at ten per cent. The unevenness documented in Section 3.1 is therefore not generating the result, though the loss of thirteen countries costs precision.

Sub-period estimates place the relationship in the later part of the sample. Over 2000 to 2011 the interaction is 0.0072 with a standard error of 0.0072 and is indistinguishable from zero; over 2012 to 2021 it is 0.0192 with a standard error of 0.0089. The earlier period coincides with the commodity supercycle, when resource-rich hosts attracted record inflows and the option value of waiting was low. The later period combines higher and more volatile uncertainty with weaker commodity prices. Resource dependence has become a source of vulnerability rather than always having been one, which helps reconcile these results with an earlier literature, based largely on pre-2010 data, that treated endowment as an unambiguous attractor.

The estimate survives a series of specification changes with little movement. Dropping the lagged dependent variable gives 0.0070 with a standard error of 0.0032, so the result is not a product of the dynamic form. Adding own GDP growth gives 0.0085 and adding employment 0.0088, so it is not proxying for the domestic cycle or for labour market conditions. Replacing population and income in levels with their logarithms, which is the more conventional scaling and avoids coefficients reported in units of $1e-8$, gives 0.0089. Leaving the dependent variable unwinsorised raises it to 0.0113, so the winsorisation is conservative rather than result-generating. Leave-one-country-out estimation returns coefficients between 0.0040 and 0.0112 in absolute value, significant at ten per cent in 46 of 48 cases. The exception is the Republic of Congo, the most oil-dependent economy in the sample, whose exclusion moves the p-value to 0.427. That is the arithmetically expected consequence of removing the top of a skewed exposure distribution, and it means the estimate characterises the most oil-dependent economies rather than a smooth regional gradient.

Two results run against the pattern and are reported for that reason. Fixing exposure at its 2000 to 2004 average, which removes any concern that contemporaneous rents respond to the same shocks as FDI, gives 0.0055 with a standard error of 0.0037, not significant at conventional levels. And the dynamic panel estimators in Table 9 do not identify the interaction separately: two-step system GMM returns 0.0027 with a standard error of 0.0023, and the Hansen p-value of 0.957 for the shorter lag set signals weak rather than valid instruments in the sense Roodman (2009) describes. Internal instruments are poorly suited to a regressor formed as the product of a slow-moving country characteristic and a common series, so we read this as evidence that the tool has little to say here rather than as evidence against the least-squares results. The lagged dependent coefficient is nonetheless recovered at 0.44 in system GMM against 0.37 under least squares, and the Arellano-Bond test rejects first-order but not second-order serial correlation in the differenced residuals, so the dynamic structure itself is well specified. Nickell bias of order one over T is in any case small at twenty-plus periods and falls on the persistence parameter rather than on an interaction between a predetermined characteristic and an external series.

Taken together the robustness evidence supports a reading in three parts. The sign and approximate magnitude of the resource interaction are not in doubt: they survive every change of control set, sample window, functional form and outlier treatment we have applied, and the randomisation test rules out the possibility that the pattern arises from a chance alignment of exposure with the shock. Its precision is genuinely uncertain and depends on assumptions about the error structure that the data cannot adjudicate, which is why we report the full ladder rather than a single p-value. And its interpretation is confined to the most oil-dependent economies and to the period since 2012, rather than describing a stable regional gradient.

Table 8 Robustness

Specification	Oil x GEPU	SE	N	Countries
Baseline, country and year effects	-0.008749**	(0.003419)	784	42
Omitting trade openness	-0.015009***	(0.002753)	912	46
Omitting all controls	-0.015325***	(0.002573)	965	48
Unwinsorised dependent variable	-0.011343**	(0.004437)	784	42
Excluding 2020	-0.007489***	(0.002760)	745	42
Excluding 2020 and 2021	-0.006150***	(0.001589)	706	42
2000 to 2011	-0.007160	(0.007240)	383	40
2012 to 2021	-0.019162**	(0.008947)	401	42
Static, no lagged dependent variable	-0.007005**	(0.003169)	812	42
Adding own GDP growth	-0.008534**	(0.003507)	784	42
Adding employment	-0.008840***	(0.003331)	784	42
Logs of population and income	-0.008855**	(0.003519)	784	42
Predetermined exposure, 2000-2004 average	-0.005459	(0.003711)	763	41

- Notes. Standard errors clustered on countries and years in parentheses. All specifications include country and year fixed effects. Moderating variables are standardised. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Each row is a separate

regression. The two rows that omit controls are reported because the trade openness series is missing throughout for Burundi, Liberia, Nigeria and Sao Tome and Principe, so that control alone removes them from the sample.

Table 9 Dynamic Panel Estimates

Estimator	FDI (t-1)	Oil rents	Oil x GEPU	SE	Instruments	Countries	Hansen p	AR(1) p	AR(2) p
Two-step system GMM, lags 2-3	0.4431***	-0.2645	- 0.002685	(0.002319)	15	42	0.957	0.000	0.871
Two-step system GMM, lags 2-4	0.4568***	-0.3168	- 0.002614	(0.004248)	16	42	0.625	0.000	0.875
Two-step difference GMM, lags 2-3	0.3585***	- 1.6403**	- 0.005386	(0.018486)	8	42	0.565	0.001	0.776

- Notes. Two-step estimates with collapsed instruments and the Windmeijer (2005) correction; standard errors in parentheses. All variables are demeaned by year so that time effects are removed before estimation and the instrument count stays below the number of cross-sections. A Hansen p-value close to one indicates weak rather than valid instruments.

IV. DISCUSSIONS AND POLICY IMPLICATIONS

Neither structural remedy performs as advertised. Resource endowment does not anchor foreign investment against global uncertainty; it amplifies the transmission, and the amplification is graded by sunk cost and payback horizon, being largest for oil, roughly 40 per cent as large for minerals, and absent for coal and for the diffuse rents that dominate the standard aggregate. Locational specificity, which the protective argument invokes, fixes where an investment must occur if it occurs at all, but does nothing to reduce the value of waiting. An investor weighing a thirty-year hydrocarbon commitment while the direction of global trade and monetary policy is unpredictable faces exactly the configuration in which deferral is most attractive, and the fact that the oil lies in one particular country does not change that calculation.

Financial development neither protects nor amplifies, and the interval excludes protection of an economically

meaningful size. The explanation is not that finance is irrelevant to investment but that the constraint uncertainty imposes is not the constraint domestic credit depth relieves. Uncertainty raises the option value of waiting on an irreversible commitment, which is a property of the project and the information environment. Financial development relieves the external finance premium and the funding constraint, the margins on which uncertainty operates where large firms borrow domestically and local finance is deep enough to substitute for retrenching foreign funding. With median private credit at 13.3 per cent of GDP in this sample, and the marginal extractive project financed on a parent balance sheet, that substitution margin is small. The result is a statement about the level of financial development reached in the region, not a universal claim about finance.

The measurement result deserves emphasis because it is actionable beyond this paper. Two thirds of what the standard indicator counts as natural resource rents in this region is natural gas and forest rents, and the forest component reflects subsistence activity with no connection to foreign investment. Any study using total rents is averaging a strong signal with a large inert component, and the resulting attenuation will vary across samples with the composition of rents rather than with anything economically meaningful. That is a plausible reason why the resource and FDI literature has struggled to replicate itself, and the remedy is available in the same data source that supplies the aggregate.

Four implications follow for policy. Resource endowment should be treated as a source of external vulnerability rather than a substitute for resilience. The corollary is not that resource-rich economies should discourage extractive investment, which is neither feasible nor desirable, but that they should not count endowment as protection; diversification of the investment base, usually justified by the level of growth, has a second justification in its variance.

Resilience will not arrive as a by-product of financial deepening at current levels. Financial sector reform is justified on many grounds, including domestic investment, firm entry and household risk management, but insulating FDI from global uncertainty is not among them on this evidence. The more relevant question for a finance ministry is whether the system can absorb a commodity-driven revenue shock without transmitting it onward, which requires different instruments from those that raise the credit-to-GDP ratio.

Instruments should target the dispersion of expected returns rather than their level. Because the mechanism runs through the option value of waiting, measures that raise the expected return without reducing its variance will not restore investment during uncertain periods, and tax holidays and one-off incentives fall into that category. Stabilisation funds with rule-based triggers, fiscal frameworks anchored on a conservative reference price, adequate reserve buffers and extractive contract terms that are predictable and not subject to renegotiation all act on the variance instead.

The sub-period result carries its own implication. The amplification is absent before 2012 and strong afterwards, which corresponds to the shift from the commodity supercycle to a period of higher and more volatile global uncertainty with weaker commodity prices. Policy designed on the experience of the 2000s will therefore understate the exposure resource-dependent economies now face, and the case for building buffers is stronger than the average relationship over the full sample suggests. It also means that studies drawing on pre-crisis data are not wrong so much as describing a regime that has since changed, which is a more useful way to read the divergence between this result and the earlier literature than treating either as a failure of replication.

Finally, statistical capacity binds. Nigeria is absent from the specified models here, and from any comparable study using the same standard control set, because one series is missing for it throughout; four countries are lost for that reason alone. The resource rent accounts end in 2021, so the period since, including the commodity disruption after the invasion of Ukraine and a global tightening cycle, cannot yet be examined. The marginal return to completing a handful of series in the region's largest economies is high.

Four limitations bound the interpretation. The identifying variation is a single global time series, so with roughly twenty effective time-series observations no amount of cross-sectional data substitutes for the fact that the shock is common; we report four variance estimators and

randomisation inference rather than selecting one, but the constraint cannot be estimated away. The estimate is an intensity effect and extreme oil dependence is concentrated in few economies, so excluding the Republic of Congo leaves the same sign without statistical significance. The predetermined-exposure specification, the cleanest defence against reverse causality, has the same sign and about 60 per cent of the magnitude but is not significant, so that concern is mitigated rather than eliminated. And aggregate FDI conceals composition; a direct test of the mechanism would separate greenfield extractive investment from other categories, which announcement-level data would permit.

V. CONCLUSION

This paper asked whether financial development or natural resource wealth protects foreign direct investment in Sub-Saharan Africa from global economic policy uncertainty. Working from an annual panel of 48 economies over 2000 to 2024, and from a design in which the global index is absorbed by year effects so that the conditional effect is what is estimated, the answer to both parts is negative, and the second part is negative in a specific and graded way.

Financial development does not insulate FDI. The moderating effect is close to zero and the confidence interval excludes protection larger than a quarter of a percentage point of GDP for a one standard deviation increase in private credit. Resource dependence does not insulate FDI either; it amplifies the contraction. A one standard deviation higher oil rent share deepens the FDI response to a one-point rise in GEPU by 0.0087 percentage points of GDP, which over the 2019 to 2020 spike implies a contraction 2.1 points of GDP deeper for an economy at 40 per cent oil rents than for one at the sample mean, and 3.3 points deeper in the long run. Randomisation inference over 2,000 reassignments of exposure returns $p = 0.017$.

Three further results shape the interpretation. The amplification is graded by sunk cost and payback horizon, largest for oil, roughly 40 per cent as large for minerals, and absent for coal and for the natural gas and forest rents that make up two thirds of the standard aggregate, which explains why that aggregate performs poorly. It is concentrated in the period since 2012 rather than being a stable feature of two decades. And it is not a pandemic artefact: removing 2020 and 2021 leaves the estimate intact and more precisely determined.

Two methodological points generalise. Adding year effects to a specification containing a global index does not absorb the conditional effect of interest; here it raises the estimated interaction by 29 per cent, because the global index had been netting the uncertainty channel against the commodity cycle. And when a country characteristic is interacted with a shock common to all countries, effective degrees of freedom follow the number of periods rather than the number of country-years, so default panel standard errors substantially overstate precision. Both points apply to any study that interacts a national characteristic with a global

index, which describes a large and growing share of the applied macroeconomics of small open economies.

The paper also carries a message about how this literature reports its own evidence. The source specification, the sample it actually estimates on, and the standard errors it presents are each defensible in isolation and each conventional in the field. Taken together they produce a coefficient that is attenuated by an omitted set of fixed effects, estimated on two thirds of the available panel with the region's largest oil producer excluded by a single missing control, and reported with a standard error roughly 40 per cent too small. None of this is unusual and none of it is deliberate. It is what happens when design choices are made by convention rather than by argument, and the corrective is to state the estimand, report the sample that supports it, and show the inference under more than one assumption.

For policy the implication is that resilience to global uncertainty must be built deliberately. It is not delivered by financial deepening at the levels currently observed in the region, and it is not conferred by resource endowment, which does the opposite. It requires fiscal and monetary frameworks that decouple the domestic environment from the global commodity and policy cycle, and an investment base broad enough that the most irreversible category of project is not the one on which the external accounts depend.

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APPENDIX

Table A1 Replication of the Published Regressions

Published table	N replicated	N published	Countries	R-squared replicated	R-squared published	Max absolute coefficient difference
Table 4 baseline FE	895	895	42	0.503583	0.503583	4.30e-07
Table 5 oil only	965	965	48	0.498673	0.498673	2.44e-07
Table 6 financial development only	976	976	48	0.486165	0.486165	3.14e-07
Table 7 oil with controls	784	784	42	0.521269	0.521269	4.89e-07
Table 8 oil interaction	784	784	42	0.528964	0.528964	3.82e-07
Table 9 FD interaction	785	785	41	0.500766	0.500766	4.82e-07
Table 11 minerals interaction	787	787	42	0.495364	0.495364	4.17e-07

- Notes. Each published regression was re-estimated on the same workbook. The final column reports the largest absolute difference between the replicated and published coefficients across all parameters in that equation.

Table A2 Data Coverage by Country

Country	Years in the main sample	FDI	Credit	Trade	Inflation	Oil rents	GDP per capita
Angola	20	25	10	23	25	22	25
Benin	18	25	25	25	25	18	25
Botswana	21	25	25	25	25	22	25
Burkina Faso	21	25	25	25	25	22	25
Burundi	0	25	24	0	25	22	25
Cabo Verde	21	25	24	25	25	22	25
Cameroon	21	25	20	25	25	22	25
Central African Republic	21	25	23	25	25	22	25
Chad	21	25	22	25	25	22	25
Comoros	21	25	24	25	24	22	25
Congo, Dem. Rep.	16	25	24	25	17	22	25
Congo, Rep.	21	25	24	25	25	22	25
Côte d'Ivoire	21	25	25	25	25	22	25
Equatorial Guinea	17	25	24	20	25	18	25
Eritrea	0	12	15	12	0	12	12
Eswatini	7	25	25	12	20	22	25
Ethiopia	11	25	9	14	25	22	25
Gabon	21	25	20	25	25	22	25
Gambia	21	25	22	25	25	22	25
Ghana	21	25	24	25	25	22	25
Guinea	17	25	22	25	20	22	25
Guinea-Bissau	21	25	25	25	25	22	25
Kenya	21	25	24	25	25	22	25
Lesotho	15	25	25	17	25	22	25
Liberia	0	25	23	0	23	22	25
Madagascar	21	25	25	25	24	22	25
Malawi	5	25	24	7	25	22	25
Mali	21	25	25	25	25	22	25
Mauritania	21	25	15	25	25	22	25
Mauritius	21	25	25	25	25	22	25
Mozambique	17	25	25	25	20	22	25
Namibia	19	25	8	25	22	22	25
Niger	21	25	25	25	25	22	25
Nigeria	0	25	23	0	25	22	25
Rwanda	21	25	25	25	25	22	25
Senegal	21	25	25	25	25	22	25
Seychelles	21	25	17	25	25	22	25

Sierra Leone	15	25	25	25	18	22	25
Somalia	0	25	7	13	0	9	25
South Africa	21	25	24	25	25	22	25
South Sudan	7	8	13	8	16	8	8
Sudan	21	25	23	25	23	22	25
São Tomé and Príncipe	0	25	24	0	25	21	25
Tanzania	21	25	25	25	25	22	25
Togo	21	25	25	23	25	22	25
Uganda	21	25	14	25	25	22	25
Zambia	21	25	12	25	25	22	25
Zimbabwe	12	25	21	25	13	22	25

- *Notes.* Entries are non-missing annual observations out of 25. ‘Years in the main sample’ is the number of country-years entering the specification of Table 6, column 2. Six countries contribute none: Eritrea and Somalia for want of the dependent variable or the resource series, and Burundi, Liberia, Nigeria and Sao Tome and Principe solely because trade openness is missing for them throughout.