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The impact of South-South and North-South FDI on energy intensity in developing countries

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Abstract

This study is the first to examine the impact of both North-South FDI and South-South FDI on energy intensity in developing countries. It is also the first in the FDI-energy intensity literature to carefully control for the endogeneity of FDI using several IV techniques, as well as the first in this literature to use a panel Granger causality approach. We find that while South-South FDI contributes to reductions in energy intensity in developing countries, there is no evidence that North-South FDI reduces energy intensity in developing countries. This finding holds even when we use panel cointegration methods.

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1. Introduction

There is a small but growing number of studies on the impact of foreign direct investment (FDI) on economy-wide energy intensity — the ratio of energy use to GDP — in developing countries. The idea behind these studies is that (if the shares of agriculture, manufacturing, and services in GDP are held constant) an estimated negative effect of FDI on economy-wide energy intensity can be interpreted as evidence of a transfer of energy-saving technology from multinational enterprises (MNEs) to their affiliates and local firms in the host country.[1] If such a transfer exists, then developing countries can achieve energy savings per unit of output and thus economic growth without a proportional growth in energy use and the associated environmental problems — such as air and water pollution, noise due to wind turbines and other energy projects, radioactive waste from nuclear energy production, and greenhouse gas emissions from fossil-fuel fired power plants (see, e.g., Herzer and Schmellmer, 2022). Therefore, the effect of FDI on energy intensity in developing countries is not only of academic interest but also of relevance to policy makers concerned with both economic development and sustainable development.

The available evidence, however, is inconclusive. While the results of some studies suggest that FDI reduces energy intensity in developing countries (see, e.g., Mielnik and Goldemberg, 2002; Herzer and Schmelmer, 2022),[2] others find insignificant effects (see, e.g., Hübler and Keller, 2010; Kretschmer et al., 2013).

However, one potential problem with estimating the effect of FDI on energy intensity is the likely endogeneity of FDI. For example, countries with low energy intensity are typically characterized by lower greenhouse gas emissions and less environmental degradation. If foreign investors prioritize sustainable and environmentally friendly practices, they may avoid regions with poor environmental records. In this case, the estimated coefficients overstate the negative causal effect of FDI on energy intensity in developing countries. Alternatively, if countries with high energy intensity have less stringent energy policies and lower environmental standards, leading to reduced production costs, they may attract more FDI than low energy intensity countries for cost reasons. In this case, the estimated coefficients understate the negative effect of FDI on energy intensity in developing countries.

Herzer and Schmelmer (2022) account for these biases using system generalized methods of moments (GMM), but most studies ignore the likely endogeneity of FDI (see, e.g., Mielnik and Goldemberg, 2002; Kretschmer et al., 2013), and some studies use a one-year lag of their FDI variable to account for endogeneity problems (see, e.g., Hübler and Keller, 2010; Mimouni and Temimi, 2018; Herzer and Schmelmer, 2022). However, as is well known, and as theoretically shown by Bellemare et al. (2017), the results in regressions with lagged independent variables may still be biased by reverse causality. Moreover, it is well known that the absence of a correlation between the current value of a variable Y_{it} and the past value of a variable X_{it} does not necessarily imply the absence of a contemporaneous effect of X_{it} on Y_{it}.

Another potential problem with estimating the effect of FDI on energy intensity in developing countries is that the energy-saving effect of FDI from developed to developing countries (North-South FDI) may differ from the energy-saving effect of FDI from one developing country to another (South-South FDI). If this is the case, then using total FDI may conflate these effects and lead to misleading conclusions.

[1] Such a transfer is typically referred to as a "technique effect". The technique effect is closely related to the so-called "pollution halo effect", which occurs if multinational firms transfer environmentally friendly technology to developing countries and this technology diffuses to local firms.

[2] Herzer and Schmelmer (2022) distinguish between greenfield FDI and cross-border M&As. They find that while the effect of cross-border M&As on energy intensity is insignificant in upper-middle-income countries, greenfield FDI exerts a negative and significant impact on energy intensity in these countries. They also find that both greenfield FDI and cross-border M&As have an insignificant impact on energy intensity in low- and lower-middle-income countries.

To understand why North-South FDI and South-South FDI may have different effects on energy intensity, consider the reasonable assumption that the technological gap between multinational enterprises (MNEs) from developed countries and local firms in developing host countries is larger than the technological gap between MNEs from developing countries and their domestic counterparts. On the one hand, a larger technology gap implies a greater potential for the transfer of energy-saving technology. Therefore, North-South FDI may contribute more to reductions in energy intensity compared to South-South FDI. On the other hand, a larger technology gap also suggests lower absorptive capacity, meaning domestic firms in many developing countries may struggle to adopt energy-saving technology from North-South FDI. Conversely, if there is a smaller technology gap between foreign and local firms, facilitating easier technology absorption, energy-saving technologies are predominantly absorbed through South-South FDI. Additionally, multinational enterprises (MNEs) from developing countries tend to establish stronger linkages with local firms compared to those from developed countries (see, e.g., UNCTAD, 2006). This enables them to integrate more deeply into host economies, potentially leading to significant technology spillovers to local firms. Consequently, South-South FDI may have a greater negative effect on energy intensity than North-South FDI.

Another relevant point is that there is evidence that foreign affiliates of MNEs from developing countries tend to be less energy intensive than North affiliates of North firms (see, e.g., Lipsey and Sjöholm, 2011). This could be interpreted as support for the pollution haven hypothesis, which predicts that MNEs from developed countries will relocate their energy-intensive operations to developing countries where environmental policy is relatively weak. Under this hypothesis, it is possible that North-South FDI, via an increase in the relative size of energy-intensive industrial sectors, even leads to an increase in energy intensity.

Overall, it is thus likely that the impact on energy intensity differs between North-South and South-South FDI. However, this issue has not been investigated to date, despite its clear policy relevance: If there are differences in the effects of FDI on energy intensity between North-South and South-South FDI, then knowledge of these differences could be of value to policy makers in developing countries who face the practical problem of identifying those potential foreign investors that are more likely to transfer energy-saving technology.

Given these considerations, the objective of this study is to examine the impact of South-South and North-South FDI on energy intensity in developing countries using econometric methods that account for the likely endogeneity of FDI. More specifically, we make two main contributions to the literature. First, we do not focus on total FDI, but consider South-South and North-South FDI, using an unbalanced panel of 57 economies over the period 2009 to 2019. Second, we employ three estimation methods that allow estimation of causal effects in the presence of endogeneity: the system GMM estimator of Blundell and Bond (1998), the difference GMM estimator of Arellano and Bond (1991), and Lewbel's (2012) instrumental variable method. In addition, as a robustness check we examine the Granger causal relationship between North-South FDI and energy intensity, and between South-South FDI and energy intensity using the panel Granger causality approach recently developed by Juodis et al. (2021).

Since these methods do not account for non-stationary data, there is a potential risk of spurious regressions if the data are non-stationary. However, this risk is expected to be small when there are few observations per country and a large number of countries, as in the present study. Nevertheless, we address this potential risk by applying panel unit root and cointegration techniques to a subsample of (40) countries with sufficiently long time series ($T_i \geq 10$).

Cointegration implies the existence of a (non-spurious) long-run relationship between two or more non-stationary variables. The advantage of cointegration estimators is that they are consistent under cointegration even if the regressors are endogenous. However, a problem with panel unit root and cointegration methods in the present case is that these methods may produce biased results when the number of time-series observations is small relative to the number of cross-sectional units. Therefore, panel unit root and cointegration methods are used here as a robustness check rather than as the main analytic tool.

To preview our main results, we find across all our estimation methods that South-South FDI contributes to reductions in energy intensity. The estimated effect of North-South FDI on energy intensity, in contrast, is statistically insignificant in all specifications but one (where it is weakly significantly positive).

The remainder of this study is organized as follows. In Section 2 we present our basic empirical model and outline our data sources and definitions. Our results are discussed in Section 3. Section 4 concludes.

2. Model and data

Our basic empirical model is

$$\log ENERGY_{it} = \alpha \log ENERGY_{it\text{-}1} + \beta \log FDI_{it} + \gamma \log X_{it} + \mu_i + f_t + \varepsilon_{it} \tag{1}$$

where i and t are country and time indices; log denotes the natural logarithm; $ENERGY_{it}$ represents economy-wide energy intensity, measured as the ratio of primary energy use (in megajoules) to real GDP (in PPP terms); and FDI_{it} denotes two FDI variables. The first is the ratio of the stock of FDI from developed countries to GDP of developing country i, $NorthSouthFDI_{it}$; the second is the stock of FDI from developing countries relative to GDP, $SouthSouthFDI_{it}$. To avoid collinearity between $NorthSouthFDI_{it}$ and $SouthSouthFDI_{it}$, we include these variables separately.[3]

X_{it} is a vector of control variables including real GDP per capita, $GDPPC_{it}$, imports as a percentage of GDP, IMP_{it}, the consumer price index (used as a proxy for the energy price), CPI_{it}, gross fixed capital formation as a percentage of GDP, $GFCF_{it}$, and industrial value added as a percentage of GDP, IND_{it}. We also control for country fixed effects, μ_i, and common time effects, f_t.

Data on the control variables are taken from the World Development Indicators (WDI) (available at https://databank.worldbank.org/source/world-development-indicators). The (nominal) data used to construct our FDI variables are from the Coordinated Direct Investment Survey (CDIS) database of the IMF (available at https://data.imf.org/?sk=40313609-F037-48C1-84B1-E1F1CE54D6D5), which reports data on bilateral FDI stocks.[4] Both FDI variables are ratios to (nominal) GDP. The (nominal) GDP data to construct these ratios are also from the WDI, like our data on energy intensity.

It should perhaps be explicitly noted that the CDIS database does not report bilateral FDI flows. Thus, we are unable to check the robustness of our results using FDI flows (relative to GDP), which are also often employed in empirical FDI studies. However, this is not a serious problem, because FDI stocks are generally considered a better measure of the presence of foreign firms than FDI flows, given that FDI stocks more effectively capture long-run effects due to the accumulation of flows (see, e.g., Chintrakarn et al., 2012). More specifically, the use of FDI stocks ensures that the effects of FDI are not limited to the period in which the investment is made, thereby fully accounting for the effects of both new and established foreign firms.

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[3] It would be interesting to extend our analysis by using bilateral FDI data at the sector or industry level to gain insight into whether sectoral differences exist in the effects of North-South and South-South FDI on energy intensity in developing countries. Given the lack of bilateral FDI data at the sector or industry level, such an extension is unfortunately not possible, however.

[4] We aggregate the bilateral FDI data to South-South FDI and North-South FDI. To construct our measure of South-South FDI [North-South FDI], we classify a country as developing [developed] country if it is officially listed as a low- or middle-income [high-income] country by the World Bank in its World Development Reports (available at https://www.worldbank.org/en/publication/wdr/wdr-archive) in more than half of the years between 2009 and 2019.

Combining the data from both sources, and excluding tax havens and countries with less than one million people,[5] yields an unbalanced panel dataset of 57 developing countries with data between 2009 and 2019.[6] The minimum number of observations per country is 2, while the maximum is 11; the average number of observations per country is 9.5.

3. Results

We estimate equation (1) using system and difference GMM. Both techniques (which are designed for small-*T* large-*N* panels such as the one used here) are dynamic panel methods that account for endogeneity while avoiding the well-known "Nickell bias" (that arises from applying a fixed effects estimator to a lagged dependent variable model in a panel with small *T*). In addition, we use the Lewbel (2012) instrumental variable estimator, which, however, is not designed for dynamic panels. Therefore, we do not include $\log ENERGY_{it-1}$ in the Lewbel regressions.

All three estimators use internal instruments. While the system and difference GMM estimators construct instruments using lagged observations, the Lewbel estimator exploits heteroskedasticity to construct instrumental variables.

The estimation results along with diagnostic tests are presented in Table 1. The Arellano and Bond (1991) tests for second-order serial correlation (AR2) indicate that the GMM residuals exhibit no second-order serial correlation, and the Hansen tests of overidentifying restrictions (HANSEN) fail to reject the validity of the instruments in the GMM models. Moreover, the number of instruments is always smaller than the number of countries. We therefore conclude that the GMM models presented in columns (1) – (4) are correctly specified, like the Lewbel models presented in columns (5) and (6), which pass the Hansen test of overidentifying restrictions, the Kleibergen-Paap rk *LM* test of underidentification, and the Kleibergen-Paap rk Wald *F* test of weak identification.

Turning to the estimated coefficients on the FDI variables, we see that coefficient on $\log NorthSouthFDI_{it}$ is insignificant in all three specifications, whereas $\log SouthSouthFDI_{it}$ has a negative and significant coefficient in all three specifications.

For brevity, we do not discuss the results for the control variables in detail here, but note that the coefficients on the control variables are largely consistent with the literature on the determinants of energy intensity in developing countries. The only exception is the coefficient on $\log IMP_{it}$, which, in contrast to previous estimates in the literature, is positive and significant in most specifications. The most likely explanation for the positive coefficient is

[5] We exclude tax havens because most FDI into tax havens does not generate value adding activity. There is therefore no reason to assume that FDI into tax havens generates significant effects on energy intensity. The reason for excluding countries with less than one million people is that their FDI to GDP ratio is highly volatile due to single large transactions, including large profit repatriations. Their FDI to GDP ratio is therefore not a meaningful measure of the foreign value-adding activities of MNEs.

[6] The countries in our sample are Albania, Algeria, Armenia, Azerbaijan, Bangladesh, Belarus, Benin, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Bulgaria, Burkina Faso, Cambodia, China, Cote d'Ivoire, El Salvador, Georgia, Ghana, Guatemala, Honduras, India, Indonesia, Kazakhstan, Kyrgyz Republic, Lebanon, North Macedonia, Malaysia, Mali, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Niger, Nigeria, Pakistan, Paraguay, Peru, Philippines, Romania, Russia, Rwanda, Senegal, Serbia, South Africa, Sri Lanka, Tanzania, Thailand, Togo, Turkey, Uganda, Ukraine, and Zambia.

that while many imported goods and services are not a channel for technology spillovers, increased imports imply that transport-related energy use increases.[7]

Table 1. System GMM, difference GMM, and Lewbel IV results

	System GMM		Difference GMM		Lewbel IV	
	(1)	(2)	(3)	(4)	(5)	(6)
log$ENERGY_{it-1}$	0.965***	0.982***	0.878***	0.682***		
	(0.050)	(0.048)	(0.136)	(0.172)		
log$NorthSouthFDI_{it}$	-0.020		-0.024		0.094	
	(0.016)		(0.026)		(0.095)	
log$SouthSouthFDI_{it}$		-0.024**		-0.068**		-0.034***
		(0.010)		(0.030)		(0.010)
log$GDPPC_{it}$	0.003	-0.047***	-0.214	-0.052	-0.168***	-0.183***
	(0.013)	(0.017)	(0.228)	(0.236)	(0.022)	(0.031)
logIMP_{it}	0.093**	0.070**	0.173***	0.196*	0.020	0.141**
	(0.042)	(0.030)	(0.062)	(0.105)	(0.038)	(0.050)
logCPI_{it}	0.061	0.068*	0.036	0.026	0.481***	0.474***
	(0.054)	(0.037)	(0.075)	(0.111)	(0.067)	(0.070)
log$GFCF_{it}$	-0.038	-0.030	-0.036	-0.024	0.043**	0.038**
	(0.032)	(0.021)	(0.036)	(0.049)	(0.021)	(0.018)
logIND_{it}	0.027	0.047*	0.096*	0.123	-0.102	0.037
	(0.023)	(0.027)	(0.049)	(0.079)	(0.073)	(0.053)
AR2 (p-value)	0.440	0.431	0.459	0.327		
No. of instruments	42	42	37	37		
HANSEN (p-value)	0.248	0.519	0.411	0.321	0.111	0.554
Kleibergen-Paap rk *LM* statistic (p-value)					0.000	0.005
Kleibergen-Paap rkWald *F* statistic					15.312	22.238
No. of countries	57	57	57	57	57	57
No. of observations	511	511	454	454	550	550

Notes: The dependent variable is log$ENERGY_{it}$. The lagged dependent variable was treated as predetermined; the time dummies, logIND_{it}, and log$GFCF_{it}$ were treated as exogenous; and log$NorthSouthFDI_{it}$, log$SouthSouthFDI_{it}$, log$GDPPC_{it}$, logIMP_{it}, and logCPI_{it} were treated as endogenous in the GMM procedures. We used the orthogonal deviations transformation of Arellano and Bover (1995) rather than the first-difference transformation because the former has the advantage of preserving sample size in panels with gaps (as in our panel). As a rule of thumb, GMM can exhibit the problem of too many instruments when the number of instruments is greater than the number of cross-sectional units. To reduce the risk of instrument proliferation (which can overfit endogenous variables), the number of lags was restricted to up to five lags. In addition, to ensure that the number of instruments does not exceed the number of countries, the instrument matrix was collapsed. We used the two-step estimator with Windmeijer's (2005) standard errors for the GMM procedures. Only the FDI variables were instrumented in the Lewbel regressions. When country dummies are included in the Lewbel regressions, a warning message is displayed that the estimated covariance matrix of moment conditions is not of full rank and standard errors and model tests should be interpreted with caution. We therefore approximated the fixed effects using the country means of the variables. AR2 is the Arellano-Bond test for second-order autocorrelation. HANSEN is the Hansen test of overidentifying restrictions. The Kleibergen-Paap rk *LM* and Wald *F* statistics correspond to tests of underidentification and weak identification. The critical values of the Kleibergen-Paap rk Wald *F* statistic for a maximal IV relative bias of 5, 10, 20, and 30 percent are 21.23, 11.51, 6.42, and 4.63, respectively. Numbers in parentheses are heteroskedasticity-consistent standard errors. *** (**) [*] indicates significance at the 1% (5%) [10%] level.

[7] In Table A1 in the appendix, we check the robustness of our results to the inclusion of total factor productivity (TFP_{it}) as a measure of the level of technology, logged (logTFP_{it}), and a rule of law index ($RULEofLAW_{it}$) as a measure of institutional quality. The TFP index is from the Penn World Table (PWT) 10.01 (https://www.rug.nl/ggdc/productivity/pwt/?lang=en) and is based on the residuals of a production function with physical capital, human capital, and raw labor as inputs. Since our panel dataset is cross-sectionally dominated, we use the CTFP index from the PWT, which is normalized to one for the USA and is thus suitable for TFP comparisons across countries, rather than the RTFPNA index, which is normalized to one for the year 2017 for all countries and is therefore suitable for comparisons of TFP over time. The rule of law index is from the Worldwide Governance Indicators (available at https://info.worldbank.org/governance/wgi/) and is measured on a −2.5 to +2.5 scale. Given the limited availability of data on both TFP_{it} and $RULEofLAW_{it}$, we are forced to use a much smaller sample of countries (39) in this robustness check than in our estimates in Table 1. The results of this robustness check show that our main findings are robust to the inclusion of TFP and rule of law (and the use of a smaller sample of countries).

As is well known, the presence or absence of Granger causal or lagged effects says nothing about the presence or absence of contemporaneous effects. Since, however, the diffusion of spillovers of energy-saving technology may take some time, we also examine the Granger causal effects of North-South and South-South FDI on energy intensity (while controlling for the lagged dependent variable). To this end, we use the method recently developed by Juodis et al. (2021), which has superior size and power compared to traditional tests. It should perhaps be noted that this method corrects for Nickell bias using the split-panel jackknife method. Unfortunately, however, the Juodis et al. (2021) test requires a balanced panel. Therefore, we apply it to a subsample that only includes countries with complete data between 2010 and 2019 (38 countries).[8] Using the same control variables as above and a lag length of one year (as suggested by the BIC) yields the results presented in Table 2.

Table 2. Panel causality tests

Null Hypothesis	Wald	Coefficient on the lagged explanatory variable
log$NorthSouthFDI_{it}$ does not cause log$ENERGY_{it}$	0.110	-0.028
log$SouthSouthFDI_{it}$ does not cause log$ENERGY_{it}$	0.023	-0.014**
log$ENERGY_{it}$ does not cause log$NorthSouthFDI_{it}$	0.069	0.390*
log$ENERGY_{it}$ does not cause log$SouthSouthFDI_{it}$	0.228	-0.410

Notes: Since the test requires a balanced panel, we constructed a subsample that only includes countries with complete data between 2010 and 2019 (38 countries). All tests include country fixed effects, and we used demeaned data to account for common time effects. All tests are based on one lag, as suggested by the BIC, and include control variables (lagged one period). The column headed Wald reports the p-value of the Wald test of the null hypothesis that the lagged explanatory variable is significantly different from zero. This p-value is equal to the p-value of the z-statistic of the coefficient on the lagged explanatory variable. For all tests, we used heteroskedasticity-consistent standard errors. ** (*) indicates significance at the 5% (10%) level.

The results suggest that while there is no Granger causality from North-South FDI to energy intensity, South-South FDI Granger-causes energy intensity with a negative sign. For completeness, we also report the results of the "reverse" Granger causality tests, with log$NorthSouthFDI_{it}$ and log$SouthSouthFDI_{it}$ as the endogenous variables, and log$ENERGY_{it}$ as the exogenous variable. From these results, there is evidence (at the 10% level) of a positive Granger causal relationship from energy intensity to North-South FDI, whereas energy intensity has no Granger-causal effect on South-South FDI. It is needless to say that the latter does not imply absence of contemporaneous effects of energy intensity on South-South FDI.

Since, however, the above methods do not account for potential non-stationary data, the question arises whether our results change if we use non-stationary panel techniques. To assess whether this is the case, we restrict our sample to countries with at least 10 time series observations, yielding a subsample of 40 countries.[9] The reason for using this subsample is that panel unit root and cointegration methods are not feasible in our full sample given the relatively small number of observations for some countries. Moreover, because the existence of cointegration between two (or more) non-stationary variables is known to be robust to the addition of further variables, we focus on the main variables of interest: log$NorthSouthFDI_{it}$ and log$SouthSouthFDI_{it}$. Thus, we examine two bivariate relationships: (1) the relationship

[8] The countries in this sample are Armenia, Azerbaijan, Bangladesh, Belarus, Bolivia, Bosnia and Herzegovina, Brazil, Bulgaria, Cambodia, China, El Salvador, Georgia, Guatemala, Honduras, India, Indonesia, Kazakhstan, Kyrgyz Republic, North Macedonia, Malaysia, Mexico, Moldova, Mongolia, Morocco, Mozambique, Nepal, Nigeria, Pakistan, Paraguay, Philippines, Romania, Russia, Serbia, South Africa, Thailand, Turkey, Ukraine, and Zambia.

[9] The countries in this sample are Armenia, Azerbaijan, Bangladesh, Belarus, Bolivia, Bosnia and Herzegovina, Brazil, Bulgaria, Cambodia, China, El Salvador, Georgia, Ghana, Guatemala, Honduras, India, Indonesia, Kazakhstan, Kyrgyz Republic, North Macedonia, Malaysia, Mexico, Moldova, Mongolia, Morocco, Mozambique, Nepal, Nigeria, Pakistan, Paraguay, Philippines, Romania, Russia, Serbia, South Africa, Thailand, Turkey, Uganda, Ukraine, and Zambia.

between log$NorthSouthFDI_{it}$ and log$ENERGY_{it}$, and (2) the relationship between log$SouthSouthFDI_{it}$ and log$ENERGY_{it}$.

The first step in this examination is to pre-test the variables for unit roots. We use the panel unit root tests of Levin et al. (2002) and Pesaran (2007) for this purpose. As is well known, the Levin et al. (2003) test assumes cross-sectionally independent residuals and may suffer from size distortions in the presence of error cross-sectional dependence. To account for cross-sectional dependence due to common time effects, we demean the data by subtracting the cross-sectional means from the data and use the demeaned data in place of the original data to perform the Levin et al. (2002) test.[10] Since the Pesaran (2007) test accounts for error cross-sectional dependence via the use of weighted cross-sectional averages, we apply this test to the raw data. Both tests are performed both with country-specific intercepts (*c*) and country-specific intercepts and time trends (*c, t*). The results are presented in Table 3.

The Levin et al. (2003) tests reject the unit-root null for all three variables, regardless of whether country-specific intercepts or country-specific intercepts and country-specific time trends are included. The Pesaran (2007) tests do not reject the null hypothesis of unit root for all three variables only when country-specific intercepts and country-specific time trends are included. Thus, the results of these tests are ambiguous regarding whether log$ENERGY_{it}$, log$NorthSouthFDI_{it}$, and log$SouthSouthFDI_{it}$ are stationary or non-stationary (in the sense that they have a unit root). If the variables are stationary, then there is no reason to be concerned that the results in Table 1 and 2 are spurious. If the variables are non-stationary, there is a risk of spurious regressions. Although it is reasonable to assume that this risk is small in short panels such as the one used here, it is not zero. Given the results in Column (4), we therefore assume that log$ENERGY_{it}$, log$NorthSouthFDI_{it}$, and log$SouthSouthFDI_{it}$ have unit roots.

Table 3. Panel unit root tests

	Levin et al. (2002)		Pesaran (2007)	
	(1)	(2)	(3)	(4)
	c	*c, t*	*c*	*c, t*
log$ENERGY_{it}$	0.004	0.000	0.010	0.191
log$NorthSouthFDI_{it}$	0.000	0.000	0.009	0.638
log$SouthSouthFDI_{it}$	0.000	0.000	0.462	0.997

Notes: c (*t*) indicates that the tests include country-specific intercepts (and time trends). Given the small number of time-series observations, only one lag was used in the tests. The Levin et al. (2002) tests are based on demeaned data to account for error cross-sectional dependence due to unobserved common factors; the Pesaran (2007) tests account for error cross-sectional dependence due to unobserved common factors via the use of (weighted) cross-sectional averages (and are therefore based on the original data). Reported values are *p*-values.

Under this assumption, the next step is to test for cointegration between log$NorthSouthFDI_{it}$ and log$ENERGY_{it}$ and between log$SouthSouthFDI_{it}$ and log$ENERGY_{it}$. Table 4 reports results of cointegration tests based on models with country-specific trends (and fixed effects).[11] Since the Pedroni (1999) tests assume error cross-sectional independence, we use the demeaned data for these tests. For the Gengenbach et al. (2016) and Banerjee and Carrion-i-Silvestre (2017) tests, which account for error cross-sectional dependence (via the use of weighted cross-sectional averages), we use the raw data. Since all these tests indicate that cointegration exists between log$NorthSouthFDI_{it}$ and log$ENERGY_{it}$ and between log$SouthSouthFDI_{it}$ and log$ENERGY_{it}$, as shown in Table 4, we proceed to estimate these relationships using two panel cointegration estimators: the panel FMOLS (PFMOLS) and panel DOLS (PDOLS) estimators of Kao and Chiang (2001).

To control for cross-sectional dependence due to omitted common factors, we again use the demeaned data. Moreover, we include country-specific trends to control explicitly for the

[10] Using demeaned data is equivalent to using the residuals from regressions of each variable on time dummies.
[11] The trends are statistically significant in the majority of countries, and the evidence in favor of cointegration is weaker when using models without time trends. Thus, it is important to include country-specific time trends.

country-specific effects of any omitted factors that evolve relatively smoothly over time. In addition, to ensure that our results do not suffer from error cross-sectional dependence due to common factors, we test for cross-sectional dependence in the residuals from our regressions using the cross-sectional dependence test of Juodis and Reese (JR) (2022).[12]

Table 4. Panel cointegration tests

Panel A: Tests for cointegration between log$NorthSouthFDI_{it}$ and log$ENERGY_{it}$				
	Pedroni (1999)		Gengenbach et al. (2016)	Banerjee and Carrion-i-Silvestre (2017)
	Panel statistics	Group mean statistics		
PP t-statistics	-4.770***	-7.660***		
ADF t-statistics	-4.079***	-7.687***		
ECM t-statistic			-12.308***	
CIPS statistic				3.382*
Panel B: Tests for cointegration between log$SouthSouthFDI_{it}$ and log$ENERGY_{it}$				
	Pedroni (1999)		Gengenbach et al. (2016)	Banerjee and Carrion-i-Silvestre (2017)
	Panel statistics	Group mean statistics		
PP t-statistics	-5.480***	-5.747***		
ADF t-statistics	-6.032***	-7.099***		
ECM t-statistic			-6.590***	
CIPS statistic				3.491*

Notes: The dependent variable in the Pedroni (1999) and Banerjee and Carrion-i-Silvestre (2015) tests is log$ENERGY_{it}$; the dependent variable in the test of Gengenbach et al. (2016) is Δlog$ENERGY_{it}$. All tests include trends and intercepts. The Pedroni (1999) tests are based on one lag, and we employed the Newey-West bandwidth selection using the Bartlett kernel. Given the limited number of time-series observations available here, no lags of the first differences of the variables (and lo lags of the first differences of the cross-sectional averages) were included in the Gengenbach et al. (2016) tests. The results from the Banerjee and Carrion-i-Silvestre (2017) tests are based on unit root test specifications that include no lags of the first differences. Since the Banerjee and Carrion-i-Silvestre (2017) test requires a balanced panel, we used a subsample of 38 countries with complete data between 2010 and 2019 for this test. All tests reject for large negative values. The Pedroni (1999) statistics are distributed as standard normal. The critical value for the Gengenbach et al. (2016) t-test (for $N = 50$) at the 1% significance level is -3.067. The 5% [10%] critical value for the Banerjee and Carrion-i-Silvestre (2017) statistic is -3.52 [-3.37] (for $T = 10$ and $N = 50$). Since Banerjee and Carrion-i-Silvestre (2017) do not report critical values for $T < 30$, we use the critical values from the working paper version of their article (Banerjee and Carrion-i-Silvestre, 2011). *** [*] indicates rejection of the null hypothesis of no cointegration at the the 1% [10%] level.

The PFMOLS and PDOLS estimates of the relationships between log$NorthSouthFDI_{it}$ and log$ENERGY_{it}$ and between log$SouthSouthFDI_{it}$ and log$ENERGY_{it}$ are presented in Table 5. As can be seen from the table, the Juodis and Reese (2022) test indicates that the there is no common factor-induced cross-sectional dependence in the residuals, and the estimated coefficient on log$SouthSouthFDI_{it}$ is negative and statistically significant in both regressions, whereas the coefficient on log$NorthSouthFDI_{it}$ is positive and weakly significant in the PFMOLS regression and positive but insignificant in the PDOLS regression.

Finally, we evaluate the magnitude of the estimated effects of South-South FDI on energy intensity. The estimated elasticities of energy intensity with respect to South-South FDI in Table 1 and 5 range between -0.068 and -0.024. Multiplying these values by the ratio of the average growth rate of $SouthSouthFDI_{it}$ (5.875%) to the average growth rate of $ENERGY_{it}$ (-1.357%) in our 57-country sample yields 0.104 and 0.294, respectively. These values imply a predicted average reduction in energy intensity due to South-South FDI that accounts for between about 10% and 30% of the actual average reduction in energy intensity in our sample

[12] We use the Juodis and Reese (2022) test rather than the standard Pesaran (2004) test because the latter has no power to detect error cross-sectional dependence when the estimated models include time dummies (or cross-sectional averages) or are based on demeaned data. The Juodis and Reese (2022) test is a modified version of the Pesaran (2004) test that does not suffer from this problem.

during the period 2009 to 2019. Thus, our estimates imply a substantial (but not implausibly large) effect of South-South FDI on energy intensity.

Table 5. Estimates of the long-run relationship between log*NorthSouthFDI*$_{it}$ and log*ENERGY*$_{it}$ and the long-run relationship between log*SouthSouthFDI*$_{it}$ and log*ENERGY*$_{it}$

	PFMOLS		PDOLS	
	(1)	(2)	(3)	(4)
Long-run coefficient on log*NorthSouthFDI*$_{it}$	0.029*		0.053	
	(0.015)		(0.038)	
Long-run coefficient on log*SouthSouthFDI*$_{it}$		-0.024**		-0.062***
		(0.010)		(0.021)
JR (*p*-value)	0.149	0.156	0.176	0.221
No. of countries	40	40	40	40
No. of obs.	395	395	315	315

Notes: PFMOLS = panel FMOLS estimator of Kao and Chiang (2001); PDOLS = panel DOLS of estimator of Kao and Chiang (2001). The dependent variable in the PFMOLS and PDOLS regressions is log*ENERGY*$_{it}$; the dependent variable in the PMG regression is Δlog*ENERGY*$_{it}$. All regressions include country fixed effects and individual time trends. The PDOLS regressions were estimated with one lead and one lag of the first-differenced regressor. All regressions were performed using demeaned data to account for error cross-sectional dependence due to unobserved common factors. JR is the cross-sectional dependence test of Juodis and Reese (2022) applied to the residuals from the regressions. Numbers in parentheses are heteroscedasticity- and autocorrelation-consistent standard errors. *** (**) [*] indicates significance at the 1% (5%) [10%] level

4. Conclusion

Using an unbalanced panel of 57 economies over the period 2009 to 2019, we found, based on stationary panel methods, that South-South FDI has a negative effect on energy intensity in developing countries, a finding that is robust to the use of non-stationary panel methods. The estimated effect of North-South FDI on energy intensity, in contrast, is statistically insignificant in all but one regression, where it is marginally significant and positive. Thus, our overall conclusion is that, while North-South FDI generally does not contribute to reductions in energy intensity in developing countries, South-South FDI tends to reduce energy intensity in these countries.

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Appendix

Table A1. System GMM, difference GMM, and Lewbel IV results using additional control variables

	System GMM		Difference GMM		Lewbel IV	
	(1)	(2)	(3)	(4)	(5)	(6)
log*ENERGY*$_{it-1}$	0.898***	0.904***	0.714***	0.559**		
	(0.099)	(0.072)	(0.260)	(0.208)		
log*NorthSouthFDI*$_{it}$	-0.004		-0.037		0.184	
	(0.024)		(0.025)		(0.193)	
log*SouthSouthFDI*$_{it}$		-0.025**		-0.065**		-0.024***
		(0.010)		(0.030)		(0.070)
log*GDPPC*$_{it}$	-0.038	-0.004	-0.273	0.135	-0.032	0.128***
	(0.049)	(0.044)	(0.202)	(0.410)	(0.046)	(0.046)
log*IMP*$_{it}$	-0.005	-0.012	0.180*	0.197*	-0.108**	0.071
	(0.042)	(0.058)	(0.103)	(0.104)	(0.052)	0(.051)
log*CPI*$_{it}$	0.127*	0.075	-0.006	-0.010	0.172*	0.340***
	(0.073)	(0.066)	(0.111)	(0.153)	(0.091)	(0.116)
log*GFCF*$_{it}$	-0.034	-0.015	-0.045	-0.048	-0.077	-0.044
	(0.034)	(0.028)	(0.055)	(0.071)	(0.079)	(0.090)
log*IND*$_{it}$	0.043	-0.033	-0.001	0.064	-0.410***	-0.369***
	(0.093)	(0.081)	(0.084)	(0.127)	(0.091)	(0.092)
log*TFP*$_{it}$	0.006	-0.083	0.083	-0.110	-0.542***	-0.794***
	(0.135)	(0.130)	(0.129)	(0.213)	(0.083)	(0.076)
RULEofLAW$_{it}$	-0.011	-0.008	-0.024	-0.062	-0.039	-0.117**
	(0.036)	(0.027)	(0.071)	(0.053)	(0.047)	(0.049)
AR2 (*p*-value)	0.496	0.647	0.641	0.599		
No. of instruments	39	39	38	38		
HANSEN (*p*-value)	0.404	0.906	0.404	0.833	0.132	0.690
Kleibergen-Paap rk *LM* statistic (*p*-value)					0.003	0.049
Kleibergen-Paap rkWald *F* statistic					16.011	24.034
No. of countries	39	39	39	39	39	39
No. of observations	350	350	311	311	374	374

Notes: The countries in this sample are Armenia, Benin, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, China, Cote d'Ivoire, Guatemala, Honduras, India, Indonesia, Kazakhstan, Kyrgyz Republic, Malaysia, Mexico, Moldova, Mongolia, Morocco, Mozambique, Namibia, Niger, Nigeria, Paraguay, Peru, Philippines, Russia, Rwanda, Senegal, Serbia, South Africa, Sri Lanka, Tanzania, Thailand, Togo, Turkey, Ukraine, and Zambia. The dependent variable is log*ENERGY*$_{it}$. The lagged dependent variable was treated as predetermined; the time dummies, log*IND*$_{it}$, log*GFCF*$_{it}$, log*TFP*$_{it}$, and *RULEofLAW*$_{it}$ were treated as exogenous; and log*NorthSouthFDI*$_{it}$, log*SouthSouthFDI*$_{it}$, log*GDPPC*$_{it}$, log*IMP*$_{it}$, and log*CPI*$_{it}$ were treated as endogenous in the GMM procedures. We used the orthogonal deviations transformation of Arellano and Bover (1995) rather than the first-difference transformation because the former has the advantage of preserving sample size in panels with gaps (as in our panel). As a rule of thumb, GMM can exhibit the problem of too many instruments when the number of instruments is greater than the number of cross-sectional units. To reduce the risk of instrument proliferation (which can overfit endogenous variables), the number of lags was restricted to up to four lags in the system GMM estimations and to two to six lags in the difference GMM estimations. In addition, to ensure that the number of instruments does not exceed the number of countries, the instrument matrix was collapsed in both the system GMM estimations and the difference GMM estimations. We used the two-step estimator with Windmeijer's (2005) standard errors for the GMM procedures. Only the FDI variables were instrumented in the Lewbel regressions. When country dummies are included in the Lewbel regressions, a warning message is displayed that the estimated covariance matrix of moment conditions is not of full rank and standard errors and model tests should be interpreted with caution. We therefore approximated the fixed effects using the country means of the variables. AR2 is the Arellano-Bond test for second-order autocorrelation. HANSEN is the Hansen test of overidentifying restrictions. The Kleibergen-Paap rk *LM* and Wald *F* statistics correspond to tests of underidentification and weak identification. The critical values of the Kleibergen-Paap rk Wald *F* statistic for a maximal IV relative bias of 5, 10, 20, and 30 percent are 21.31, 11.49, 6.36, and 4.56, respectively. Numbers in parentheses are heteroskedasticity-consistent standard errors. *** (**) [*] indicates significance at the 1% (5%) [10%] level.