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Foreign Direct Investment and Host-Country Capital Formation: Measurement–Estimand Alignment, Entry Modes, and the Robustness of Inference in a Global Panel

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Research Article

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Abstract

Foreign direct investment (FDI) is often treated as a homogeneous source of external finance, although announced greenfield projects and cross-border mergers and acquisitions (M&A) represent economically distinct transactions, and prior evidence on their relationship with host-country investment remains contradictory. This study examines whether these entry modes have statistically discernible conditional associations with host-country gross fixed capital formation and whether these inferences remain stable across empirical designs that preserve or alter the underlying estimand. The empirical warehouse combines UN Trade and Development World Investment Report 2026 statistical annexes with World Development Indicators for 2003–2025. The preferred dynamic specification uses 3,111 country-year observations from 162 economies over usable estimation years 2004–2024, with country fixed effects, region-by-year shocks, macroeconomic controls, and lagged capital formation. A same-estimand audit varies common-shock treatment, estimator, weighting, and internal-instrument design, while broader checks alter timing, functional form, common-factor treatment, and outcome definition. Capital formation is strongly persistent ($\rho = 0.700$), but raw GDP-scaled greenfield and M&A coefficients are statistically imprecise in the preferred model and remain so across diagnostically retained same-estimand GMM designs. A positive greenfield coefficient appears under an inverse-hyperbolic-sine transformation and in one alternative System-GMM specification, but the former changes the functional-form estimand and the latter rejects a relevant Difference-in-Hansen subset test. The findings therefore support measurement–estimand discipline and harmonized robustness inference rather than a universal entry-mode ranking, implying that policy evaluation should distinguish investment form, measurement, and realized productive-capacity outcomes rather than infer development effects from an aggregate FDI label alone.

1. Introduction

Foreign direct investment occupies an unusual position in development and macroeconomic policy. Governments compete to attract foreign capital because external investment can finance productive capacity, connect firms to multinational production networks, and transmit technology and organizational know-how. Yet, the category labelled FDI combines transactions with sharply different implications for domestic capital accumulation. A greenfield project is associated with the creation or expansion of productive facilities, whereas a cross-border acquisition primarily transfers ownership of an existing asset and may or may not be followed by additional investment. The distinction has become more consequential as global investment has become increasingly concentrated across economies and strategic sectors, while development gains depend on whether foreign investment translates into productive capacity, employment, skills, and technology diffusion (UN Trade and Development [UNCTAD], 2026). Treating all FDI as economically equivalent can therefore obscure the mechanism that matters for host-country capital formation.

The central theoretical and empirical puzzle is not whether FDI is beneficial. The unresolved issue is why studies that examine FDI and domestic investment reach opposing conclusions, even when they address closely related questions. Early panel evidence reported weak or crowding-out relationships in developing economies (Agosin & Machado, 2005), while later work associated FDI with the displacement of private investment under some governance settings (Morrissey & Udomkerdmongkol, 2012). Farla et al. (2016), however, showed that alternative proxies and methodological refinements can reverse the crowding-out

conclusion, directly demonstrating that inference is sensitive to how the theoretical constructs are operationalized. More recent dynamic-panel evidence reports a positive association between aggregate FDI and physical capital accumulation in a smaller developing-country sample (Emako et al., 2023), whereas recent System-GMM evidence again reports crowding out in a larger developing-country panel (Magbondé et al., 2025). The empirical disagreement therefore reflects more than geographic heterogeneity; it raises a deeper measurement-and-identification problem concerning what FDI variable is being measured, what investment outcome is being used, and which dynamic assumptions support the estimate.

Entry mode heterogeneity sharpens this puzzle. Harms and Méon (2018) argue that Greenfield investment should be more closely tied to additions to the host-country capital stock because M&A payments partly compensate previous owners rather than automatically financing new capacity. Evidence from developing Asia is consistent with a stronger Greenfield relationship with domestic capital formation than with M&A flows (Gopalan et al., 2018). The other results are less supportive of a simple hierarchy. Ashraf and Herzer (2014) report substantial Greenfield crowding out and no significant M&A relationship in developing countries, whereas Nguyen et al. (2021) find Greenfield complementarity and M&A crowding out in Vietnam. Recent studies on entrepreneurship, innovation, and firm-level productivity have similarly shown that entry mode consequences depend on host conditions and outcomes rather than following a universal order (Ahn et al., 2025; Damioli & Marin, 2024; Nguyen, 2023). Therefore, the entry-mode label is theoretically informative, but the expected sign remains conditional on displacement, follow-on investment, local linkages, and measurement.

Against that background, the study evaluates two related empirical issues. First, it tests whether announced Greenfield investments and cross-border M&A sales are conditionally associated with host-country gross fixed capital formation after accounting for investment persistence, country heterogeneity, macroeconomic controls, and region-specific annual shocks. Second, it examines how robust the resulting entry-mode inference is when the empirical architecture changes, distinguishing comparisons that preserve the raw estimand and estimation equation as closely as possible from exercises that alter timing, transformation, common-factor structure, or the outcome itself. The empirical warehouse covers 2003–2025, while the primary inferential window ends in 2024 because the 2025 macroeconomic controls are materially incomplete. The preferred specification uses 3,111 country-year observations from 162 economies and combines a lagged dependent variable, country fixed effects, and region-by-year shocks. The design does not claim a quasi-experimental source of exogenous variation; the fixed-effects estimates are interpreted as conditional within-country associations, while GMM specifications are used as endogeneity-sensitive robustness checks.

The main evidence does not support statistically precise average associations between either raw GDP-scaled entry-mode measure and host-country fixed capital formation. Capital formation is strongly persistent, with a preferred-model persistence coefficient close to 0.70, but the Greenfield and M&A coefficients are statistically imprecise in the preferred specification. A targeted same-estimand audit strengthens rather than weakens that focal conclusion. Replacing region-by-year shocks with global year effects on the identical 3,111-observation sample leaves both entry-mode coefficients near zero and statistically imprecise. On the harmonized SG1 equation and sample, the matched fixed-effects model is likewise imprecise, and the retained parsimonious, deeper-lag, ultra-parsimonious, one-step, and Difference-GMM variants do not produce a statistically precise Greenfield or M&A coefficient. A positive Greenfield coefficient appears under the inverse-hyperbolic-sine transformation and in one alternative System-GMM specification, but these results have different evidential

status: the transformation changes the functional form, whereas the significant GMM specification rejects the Difference-in-Hansen test for the lagged-dependent-variable instrument subset. The central result is therefore not a pervasive sign reversal. It is a more specific pattern in which coefficient magnitudes vary across dynamic estimators, while the absence of statistically precise raw-scale entry-mode associations is comparatively robust across the diagnostically retained same-estimand specifications.

The study contributes in three connected ways, with the primary contribution centered on measurement–estimand alignment and disciplined robustness inference rather than on the entry-mode distinction itself. First, it separates two sources of apparent empirical fragility that are often conflated. A targeted same-estimand audit shows what changes when the raw outcome, raw entry-mode measures, timing of the focal regressors, and observed equation are held as constant as the estimators permit, while the broader sensitivity architecture shows what changes when functional form, timing, common-factor treatment, or outcome definition also change. This distinction sharpens the interpretation of estimator sensitivity because it avoids treating coefficients from different estimands as if they estimated the same parameter. Second, the study clarifies the estimand boundary between announced Greenfield values, cross-border M&A sales, and total host-country fixed capital formation. That boundary limits automatic crowding-in or crowding-out interpretations that would require information on domestic ownership not contained in the outcome measure. Third, the analysis provides a disciplined robustness benchmark: isolated statistically significant estimates are reported but are not promoted when matched-specification comparisons or instrument diagnostics do not support the same substantive conclusion. The contribution is therefore one of measurement discipline, inferential decomposition, and estimator triangulation; it does not constitute a new estimator or a quasi-experimental identification design. The remainder of the paper is organized as follows. Section 2 synthesizes the theoretical and empirical literature and develops the hypotheses and research question. Section 3 describes the data, measurement, empirical specification, identification boundary, and robustness plan. Section 4 presents the empirical evidence. Section 5 discusses theoretical, methodological, and policy implications together with limitations. Section 6 concludes.

2. Literature Review and Theoretical Development

2.1 FDI and host-country capital formation: complementarity versus displacement

The relationship between foreign investment and host-country capital formation is theoretically ambiguous because foreign entry can either add to the demand for productive assets or displace investment that would otherwise have been undertaken by incumbent firms. A complementarity mechanism arises when foreign projects create new capacity, raise the demand for domestic intermediate inputs, relax financing constraints for linked firms, or generate productivity and market-expansion opportunities that induce additional investment (Ahn et al., 2025; Harms & Méon, 2018). A displacement mechanism arises when foreign firms compete for credit, skilled labor, land, market share, or other scarce inputs, reducing expected returns to domestic investment (Ashraf & Herzer, 2014; Nguyen et al., 2021). Therefore, the net macroeconomic relationship depends on whether the direct capacity addition and indirect linkage effects dominate competitive displacement.

The empirical literature reflects this ambiguity. Agosin and Machado (2005) find that FDI often leaves domestic investment unchanged and sometimes displaces it across developing regions. Morrissey and Udomkerdmongkol (2012) report crowding out of private investment in a developing country panel and connect the magnitude to governance conditions. Farla et al. (2016) challenge both the measurement and estimation choices underlying this conclusion and show that refinements can reverse the crowding-out finding. Emako et al. (2023) report a positive physical-capital accumulation relationship for aggregate FDI in a smaller developing country panel, while Magbondé et al. (2025), using a larger developing country panel and System GMM, again report crowding out. The sequence of results is important because the disagreement cannot be summarized as a simple evolution toward consensus. Instead, the literature itself provides evidence that operationalization and estimator design can alter substantive conclusions.

The present study differs from that literature in one critical respect: the dependent variable is total host-country gross fixed capital formation rather than a constructed measure of domestic-owned private investment. This distinction narrows the permissible interpretation. Total fixed capital formation captures macroeconomic additions to fixed assets in the host economy regardless of ultimate ownership; therefore, a positive coefficient cannot be interpreted automatically as domestic-investor crowding in, and a negative coefficient cannot be interpreted automatically as displacement of domestic-owned capital. The outcome is appropriate for asking whether entry-mode measures co-move with host-country capital accumulation; however, it does not identify who owns the incremental capital.

2.2 Entry modes as distinct economic transactions

Disaggregating FDI by establishment mode provides a mechanism-based alternative to treating aggregate FDI as homogeneous. Greenfield investments normally entail the establishment or expansion of productive facilities and therefore have a direct conceptual link to new physical capital. Cross-border M&A, by contrast, transfers control over an existing entity and may initially represent a payment to previous owners rather than a contemporaneous addition to the host capital stock. Harms and Méon (2018) formalize this distinction and find stronger growth effects for greenfield investments. Gopalan et al. (2018) similarly report a stronger greenfield relationship with domestic capital formation in developing Asia.

The capital-creation logic does not imply that Greenfield investment must generate a positive net association. New foreign facilities may displace domestic producers, intensify input-market competition, or induce domestic firms to postpone investment when expected profits fall. Ashraf and Herzer (2014) report Greenfield crowding out in a broad developing country panel. M&A also admits competing mechanisms. Acquisition payments do not necessarily create new capacity on impact, but post-acquisition restructuring, technology transfer, managerial reorganization, or follow-on investment may subsequently raise capital formation. Conversely, acquisition-driven consolidation can reduce investment if ownership transfer substitutes for capacity expansion. Nguyen et al. (2021) illustrate the possibility of sharply different dynamic patterns, finding Greenfield complementarity and M&A crowding out in Vietnam.

Recent evidence outside the narrow capital formation outcome reinforces the principle that entry mode is a boundary condition rather than a universal ranking. Nguyen (2023) finds that both modes can support domestic entrepreneurship, with Greenfield spillovers depending more strongly on host capacity and institutional support. Damioli and Marin (2024) show that foreign entry can affect local innovation differently

across Greenfield and acquisition modes, with Greenfield entry reducing patenting among less-experienced inventors in their European regional sample, while acquisitions leave aggregate patenting broadly unchanged. Such findings make a deterministic Greenfield-good/M&A-bad hierarchy difficult to defend. The more defensible theoretical position is that entry mode changes the transmission mechanism and therefore changes which empirical outcome, time horizon, and measurement strategy are informative.

2.3 Measurement, dynamics, and inference sensitivity

Measurement is central because the focal entry-mode variables are not accounting equivalents. Announced greenfield project values and cross-border M&A sales measure different events, while balance-of-payments FDI includes additional financial components. Recent measurement research likewise emphasizes that conventional FDI statistics need not map closely onto real multinational production because financial motives and organizational structures can affect recorded flows (Wacker et al., 2024). A model that treats these concepts as interchangeable therefore assumes a correspondence that the underlying transactions do not guarantee. The argument advanced by Farla et al. (2016) is consequently broader than a replication dispute: proxy construction can determine whether an estimated relationship appears as crowding in, crowding out, or no relationship. The present study extends this concern to entry-mode inference by examining transaction-specific measures and asking whether their substantive interpretation remains stable across alternative empirical architectures.

Dynamic persistence creates a second layer of sensitivity. Investment is inherently state-dependent because adjustment costs, project gestation, financing commitments, and irreversibility make current capital formation dependent on prior investment. Therefore, a lagged dependent variable is substantively justified, but it also complicates the estimation of panels with country effects. Dynamic-panel GMM provides one response by exploiting internal moment conditions (Arellano & Bond, 1991; Blundell & Bond, 1998), although instrument validity and proliferation require explicit scrutiny. Roodman (2009) shows that large or weakly informative instrument sets can overfit endogenous variables and weaken overidentification tests, making instrument reduction and sensitivity analysis essential rather than optional.

Cross-sectional dependence provides a third layer. Global financial conditions, commodity cycles, crises, trade shocks, and geopolitical disruptions can simultaneously affect many economies, potentially leaving common components in both the regressors and the error process. Common-correlated-effects methods build on the framework introduced by Pesaran (2006), using cross-sectional averages to proxy unobserved common factors. In dynamic settings, performance depends on the time dimension and factor representation (Everaert & De Groote, 2016; Juodis & Reese, 2026). Ditzen (2018) provides an operational framework for dynamic CCE estimation in unbalanced panels. The implication for the present study is methodological rather than mechanical: no single estimator is presumed to dominate. Credible interpretation requires examining whether substantive conclusions survive economically plausible alternatives.

Taken together, the closest prior literature establishes two relevant but only partially integrated lines of evidence: entry-mode studies show that Greenfield investment and cross-border M&A can have different relationships with capital formation, growth, entrepreneurship, and innovation (Ashraf & Herzer, 2014; Gopalan et al., 2018; Harms & Méon, 2018; Nguyen et al., 2021; Nguyen, 2023; Damioli & Marin, 2024). Separately, the FDI–domestic-investment debate shows that proxy construction and estimation choices can materially alter

substantive inferences (Farla et al., 2016; Magbondé et al., 2025). The incremental question addressed here is not whether Greenfield investment and M&A are different economic transactions. It is whether mode-specific conclusions about host country capital formation remain robust when transaction measurement, timing, common factor treatment, dynamic persistence, functional form, and internal instrument design are assessed within one common global empirical framework. Consequently, this study positions its novelty as an inference and measurement–estimand contribution rather than as a claim to have introduced the Greenfield–M&A distinction.

2.4 Hypothesis development and research question

The literature yields competing mechanisms rather than an unambiguous directional prediction. Greenfield investment has a direct capacity-creation channel because new facilities can add to the host capital stock, but it may also displace incumbent investment through product- and factor-market competition. Cross-border M&A has a weaker immediate accounting link to new capital because it transfers ownership of existing assets, although post-acquisition restructuring and follow-on investment may subsequently raise capital formation. Because these competing mechanisms do not imply a unique ex ante sign, the hypotheses are deliberately non-directional tests of whether each entry-mode measure has a statistically discernible conditional association in the preferred model. They are not intended as claims that one mode should dominate the other. The stability of any detected association is addressed separately by Research Question 2.

H1a. Announced greenfield investments are associated with host-country gross fixed capital formation.

H1b. Cross-border M&A sales are associated with the host country's gross fixed capital formation.

Beyond these average associations, this study examines whether substantive inference is robust to alternative empirical architectures.

Research Question 2. How robust are the estimated Greenfield and M&A associations across same-estimand changes in common-shock, estimator, weighting, and instrument treatment, and across broader timing, functional form, common factor, and restricted outcome sensitivity analyses?

The hypotheses are evaluated using the preferred dynamic fixed-effects specification, while Research Question 2 is evaluated using the robustness architecture described in Section 3.

3. Data and Methodology

3.1 Data architecture, unit of analysis, and study period

The unit of analysis is the economy-year. The empirical warehouse combines the UNCTAD World Investment Report 2026 statistical annexes with World Development Indicators (UNCTAD, 2026; World Bank, 2026). The warehouse covers 2003–2025 because the Greenfield series begins in 2003. The primary inferential window ends in 2024 because the 2025 macroeconomic controls remain materially incomplete for a substantial set of economies. The warehouse itself is retained through 2025 for descriptive and future-update purposes.

The preferred complete-case dynamic specification contains 3,111 country-year observations from 162 countries. Because the model includes one lag of fixed capital formation, the usable estimation years are

2004–2024. Complete case restrictions produce an unbalanced estimation panel, even though the underlying country-year warehouse is organized in a common annual index. No missing values were imputed in the analytical data warehouse.

3.2 Variable measurement and construct interpretation

The dependent variable is host-country gross fixed capital formation (GFCF), measured as a percentage of GDP. GFCF measures host-country fixed-capital accumulation and is deliberately not labelled domestic-owned investment. The interpretive boundary is important because the World Development Indicators aggregate does not identify the nationality of asset owners. The mean GFCF in the preferred-model sample is 23.316 percent of GDP, with a median of 22.434 percent.

The first focal regressor is announced Greenfield investment value (GF in the model), scaled by GDP. The second is cross-border M&A sales value (MA in the model), also scaled by GDP. This distinction follows the entry-mode literature, which treats new-establishment investments and ownership-transfer transactions as different economic objects (Gopalan et al., 2018; Harms & Méon, 2018). In the preferred-model sample, Greenfield investment averages 3.860 percent of GDP and is strongly right-skewed, whereas M&A sales average 0.562 percent of GDP, have a median close to zero, and include negative observations. The distributional contrast is one reason the robustness architecture includes an inverse-hyperbolic-sine transformation rather than relying on a conventional logarithm that would discard zero and negative observations (Aihounton & Henningsen, 2021).

The baseline macroeconomic control vector (X) comprises real GDP growth (%), trade openness (% of GDP), and consumer price inflation (%). The controls are included to condition the entry mode associations on contemporaneous macroeconomic performance, external integration, and price instability. Country identifiers were harmonized to ISO3-year keys before estimation.

3.3 Preferred dynamic specification

The preferred model is a dynamic country-fixed-effects specification with region-by-year shocks:

$$GFCFit = \alpha i + \lambda r(i), t + \rho\ GFCFi, t-1 + \beta G\, GFit + \beta M\, MAit + \gamma\prime\ Xit + \epsilon it.$$

In the expression, GFCF, GF, MA, and X are defined in Section 3.2; i and t index economies and years, and r(i) denotes the region containing economy i. The parameter ρ is the GFCF persistence coefficient; β and β correspond to GF and MA; γ is the coefficient vector on X; α_i is the country effect; $\lambda_{r(i),t}$ is the region-by-year effect; and $\varepsilon_{i,t}$ is the idiosyncratic error. GFCF at t − 1 is the one-year lag. Region-by-year interactions allow annual shocks to vary across broad geographic regions. Standard errors are clustered by country.

3.4 Identification boundary and econometric threats

The preferred specification does not establish causal identification. Country fixed effects remove time-invariant heterogeneity, region-by-year shocks absorb a broad class of geographically heterogeneous common shocks, and the lagged dependent variable captures persistence, but reverse causality and time-varying omitted variables can remain. Capital formation can itself attract foreign investors, and unobserved policy reforms, financial conditions, or sectoral shocks may affect both entry-mode investment and capital accumulation. The preferred coefficients are therefore interpreted as conditional within-country dynamic associations.

Dynamic-panel GMM is used to assess sensitivity to endogenous-regressor treatment rather than convert the observational design into a definitive causal experiment. The audited GMM specifications use collapsed, lag-restricted instrument sets. In the primary System-GMM robustness specification (SG1), the lagged dependent variable is instrumented with lags 1–2 and the Greenfield and M&A variables with lags 2–3; lagged macroeconomic controls and year indicators enter as standard instruments. SG2 retains those lag windows but also treats the lagged macroeconomic controls as GMM-style variables with lags 1–2. SG3 keeps the lagged-dependent-variable window at 1–2 but moves the Greenfield and M&A instrument window to lags 3–4. The ultra-parsimonious SG4 specification restricts the lagged-dependent-variable block to lag 1 and the Greenfield/M&A block to lag 2. The Difference-GMM comparator (DG1) uses the SG1 lag windows without the level equation. All retained GMM instrument blocks are collapsed. The diagnostic sequence includes Arellano-Bond serial-correlation tests, Hansen tests of overidentifying restrictions, Difference-in-Hansen subset tests, instrument counts relative to country groups, and coefficient stability across instrument designs. Non-rejection of these tests is treated as a necessary diagnostic screen rather than proof of instrument validity, consistent with the concerns emphasized by Roodman (2009).

Cross-sectional dependence is evaluated because global macroeconomic shocks can induce residual co-movement in economies. Dynamic common-correlated-effects (DCCE) specifications are estimated as common factor robustness checks following the logic of CCE methods (Ditzen, 2018; Everaert & De Groote, 2016; Juodis & Reese, 2026; Pesaran, 2006). The DCCE specifications are not presumed to dominate the preferred model; residual cross-sectional dependence is inspected after estimation and enters the model selection interpretation.

3.5 Missing values, distributional features, and sensitivity plan

The main analysis uses complete observations for the preferred dynamic specification with no imputation. The Greenfield and M&A distributions motivate functional-form sensitivity because a small number of observations can be large relative to GDP, and the M&A series can be negative. The inverse-hyperbolic-sine (IHS) specification preserves zero and negative values while reducing the influence of extreme magnitudes.

The robustness strategy is organized by threat rather than by statistical outcome. The first layer is a same-estimand audit. It holds the raw total-GFCF outcome and raw GDP-scaled Greenfield and M&A measures fixed and, where possible, harmonizes the observed equation and estimation sample before changing the common-shock structure, dynamic-panel estimator, GMM lag depth, instrument count, or one-step versus two-step weighting. In particular, the preferred region-by-year fixed-effects model is compared with a matched-sample year-effects model; SG1 is compared with a fixed-effects model estimated on the SG1 sample and observed equation; SG3 and SG4 vary only the depth or parsimony of the GMM instrument windows relative to SG1; and a one-step SG1 checks sensitivity to the two-step weighting step. The difference GMM is accompanied by a fixed-effects comparator on its effective sample. SG2 is retained as a diagnostic contrast rather than a pure estimator-only comparison because it additionally changes the instrument classification of the lagged macroeconomic controls. The second robustness layer changes broader features of the empirical object. Lagged macroeconomic controls assess conditioning-variable timing; distributed lags evaluate whether entry-mode relationships unfold over multiple years; lead-placebo terms examine anticipatory predictive structure; inverse-hyperbolic-sine transformations change functional form while preserving zero and negative observations; DCCE specifications address unobserved common factors; and the private fixed-capital-

formation outcome provides a restricted-sample alternative outcome. Accordingly, the analysis distinguishes estimator/instrument sensitivity under a closely harmonized estimand from sensitivity that also reflects changes in functional form, timing, effective sample, or outcome definition. No coefficient dispersion is interpreted as variation in a single structural parameter when those empirical objects differ.

4. Results

4.1 Descriptive evidence and sample characteristics

Table 1 reports the descriptive statistics for the exact complete-case sample used by the preferred model. Host-country fixed capital formation averages 23.316 percent of GDP and exhibits substantial persistence in the raw data. The two FDI-mode measures have very different empirical distributions. Announced greenfield investment is highly right-skewed, with a mean of 3.860 percent of GDP and a maximum exceeding 370 percent of GDP. Cross-border M&A sales have a median of only 0.022 percent of GDP, include negative values, and have a much smaller mean. The distributional contrast reinforces the conceptual argument that the two series should not be treated as interchangeable measures of a common flow. The annual medians are shown in Figs. 1 and 2.

Table 1
Descriptive statistics for the preferred-model estimation sample

Variable	N	Mean	SD	Median	Min	Max
Host-country gross fixed capital formation (% GDP)	3111	23.316	7.364	22.434	0.000	78.001
Lagged host-country gross fixed capital formation (% GDP)	3111	23.300	7.425	22.393	0.000	78.001
Announced Greenfield investment (% GDP)	3111	3.860	11.412	1.506	0.000	370.114
Cross-border M&A sales (% GDP)	3111	0.562	2.591	0.022	-31.980	80.104
GDP growth (%)	3111	3.444	5.490	3.663	-54.402	86.827
Trade openness (% GDP)	3111	89.214	56.426	76.271	0.000	442.620
Inflation (%)	3111	6.305	17.380	3.523	-16.860	557.202

Notes: Sample = 3,111 observations from 162 economies. Descriptive statistics use the exact complete-case sample of the preferred dynamic specifications. Percentages are expressed as percentage points, unless stated otherwise.

Notes: Annual medians are computed on the preferred-model complete-case sample; the number of economies can vary by year because the estimation panel is unbalanced after complete-case restrictions.

Notes: Annual medians are computed on the preferred-model complete-case sample; the number of economies can vary by year because the estimation panel is unbalanced after complete-case restrictions.

4.2 Hypotheses H1a and H1b: dynamic entry-mode associations

The preferred specification confirms substantial persistence in host country fixed capital formation. The coefficient of lagged fixed capital formation is 0.700 ($p < 0.001$). The corresponding coefficient is 0.707 in the dynamic two-way fixed-effects comparator and 0.701 when the macroeconomic controls are lagged. The stability of the persistence parameter across the main dynamic specifications indicates that capital formation adjusts gradually rather than behaving as a sequence of independent, annual realizations.

The raw GDP-scaled entry-mode coefficients provide no statistically precise directional relationship in the preferred model. The Greenfield coefficient is -0.001 ($p = 0.891$), and the M&A coefficient is 0.031 ($p = 0.423$). The dynamic two-way fixed-effects comparator yields the same qualitative conclusion, with a Greenfield $p = 0.947$ and M&A $p = 0.309$. Relative to the sample mean of fixed capital formation, the preferred Greenfield point estimate is economically negligible, while the M&A point estimate remains too imprecise to support a stable substantive interpretation. Accordingly, neither H1a nor H1b is supported in the preferred specification. Table 2 presents the main dynamic estimates.

Among the controls, trade openness is positively associated with fixed capital formation in the preferred and dynamic two-way fixed-effects specifications. GDP growth is not statistically precise in the contemporaneous preferred specification but becomes positive when lagged controls are used. Inflation is weakly negative in the preferred and two-way fixed-effects specifications. These control coefficients are conditioning relationships rather than separate causal estimates and are not elevated to independent findings.

Table 2
Main dynamic fixed-effects results

Variable	Dynamic TWFE	Region×Year FE (Preferred)	Lagged controls
Lagged GFCF (% GDP)	0.7067***	0.6997***	0.7009***
	(0.0256)	(0.0274)	(0.0249)
Greenfield investment (% GDP)	0.0005	-0.0010	0.0016
	(0.0076)	(0.0074)	(0.0078)
M&A sales (% GDP)	0.0404	0.0309	0.0384
	(0.0396)	(0.0385)	(0.0409)
Real GDP growth	0.0336	0.0142	0.0738***
	(0.0340)	(0.0316)	(0.0279)
Trade openness	0.0291***	0.0315***	0.0050
	(0.0080)	(0.0086)	(0.0056)
Consumer-price inflation	-0.0112*	-0.0107*	-0.0066
	(0.0062)	(0.0064)	(0.0069)
Country FE	Yes	Yes	Yes
Year FE	Yes	No	Yes
Region×Year shocks	No	Yes	No
N	3,111	3,111	3,109
Economies	162	162	162
Within R^2	0.555	0.584	0.552

*Notes: Country-clustered standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. GFCF = gross fixed capital formation; TWFE = two-way fixed effects; FE = fixed effects. The lagged-controls specification uses lagged real GDP growth, trade openness, and consumer-price inflation. The preferred specification is associational rather than causal.*

4.3 Research Question 2: broader timing and functional-form sensitivity

The timing results do not reveal a stable delayed Greenfield or M&A relationship. In the distributed-lag specification, the contemporaneous and first two lags of Greenfield are jointly insignificant (p = 0.332), and the cumulative coefficient is also imprecise (p = 0.747). The corresponding M&A joint test has a p-value of 0.399, with a cumulative p-value of 0.154. The lead-placebo terms are statistically insignificant for both Greenfield (p = 0.839) and M&A (p = 0.220). The placebo evidence is consistent with the absence of an obvious anticipatory predictive structure in that specification, but the test does not establish exogeneity. Supplementary Table S2 presents the results of the distributed-lag tests.

The strongest functional form departure arises under the inverse-hyperbolic-sine transformation. IHS Greenfield is positive and statistically significant (0.260, p = 0.041), whereas IHS M&A remains statistically insignificant (p = 0.141). The transformed Greenfield estimate is not interpreted as a robust positive relationship because the significance disappears under the raw-scale preferred specification, the distributed-lag model, and most of the dynamic panel robustness tests. This contrast directly supports the measurement-sensitivity interpretation. The functional form results are reported in Supplementary Table S1.

4.4 Common-factor robustness

Two pooled dynamic common-correlated-effects specifications provide a direct check on unobserved common-factor concerns. The first uses 2,841 observations from 146 economies and estimates persistence at 0.666, while the second uses 2,748 observations from 139 economies and estimates persistence at 0.712. Greenfield and M&A remain statistically imprecise in both specifications. Residual cross-sectional dependence nevertheless remains strongly detectable, with CD statistics of 9.76 and 10.29, respectively, and p-values below 0.001. The DCCE evidence therefore reinforces the lack of a stable entry-mode coefficient but does not demonstrate that the common-factor problem has been fully removed. The models are retained for robustness evidence rather than promoted to the preferred specification.

4.5 Dynamic-panel GMM robustness and instrument sensitivity

The parsimonious System-GMM specification (SG1) uses 33 instruments for 162 country groups. The AR(2) test yields p = 0.160, the overall Hansen test yields p = 0.418, and the Difference-in-Hansen test for the lagged-dependent-variable instrument subset yields p = 0.519. Greenfield and M&A remain statistically imprecise (p = 0.206 and p = 0.819, respectively). An ultra-parsimonious specification with 30 instruments again leaves both entry mode coefficients imprecise, with AR(2) p = 0.732 and Hansen p = 0.404. The deeper-lag System-GMM specification and Difference-GMM comparator lead to the same substantive conclusion. These models differ materially in coefficient magnitude, but none of the diagnostically retained designs yield a statistically precise entry mode coefficient.

One GMM specification, SG2, produces a positive Greenfield coefficient with p = 0.019. The model simultaneously rejects the Difference-in-Hansen test for the lagged-dependent-variable instrument subset at the 5 percent level (p = 0.047). The significant coefficient is therefore not treated as preferred evidence. A one-step implementation of the SG1 instrument design also leaves Greenfield and M&A statistically imprecise. The cross-design pattern therefore does not support the view that Greenfield significance is robust to reasonable instrument and weighting choices.

All audited robust GMM implementations issue a singular weighting matrix warning and rely on a generalized inverse. The warning does not mechanically invalidate the estimates but lowers confidence in using GMM as the headline estimator. Combined with the moderately long usable time dimension and the remaining common-factor concerns, the evidence supports the use of GMM as an endogeneity-sensitivity family rather than as a device that establishes causal identification. Table 3 summarizes the DCCE and principal GMM robustness estimates.

Table 3
Dynamic common-factor and GMM robustness

Model	N	Groups	Instr.	ρ	Greenfield	M&A	AR(2) p	Hansen p	Residual CD
DCCE_P1	2841	146		0.666	0.000	0.039			9.76
					(0.116)	(0.187)			
DCCE_P2	2748	139		0.712	-0.010	0.075			10.29
					(0.062)	(0.546)			
SG1	3109	162	33	0.854	0.128	-0.069	0.160	0.418	
					(0.100)	(0.299)			
SG4 ultra-parsimonious	3109	162	30	0.829	-0.007	0.132	0.732	0.404	
					(0.130)	(0.452)			
SG3 deeper FDI lags	3109	162	33	0.825	0.201	0.334	0.412	0.864	
					(0.200)	(0.763)			
DG1	2947	160	29	0.800	0.071	0.144	0.495	0.747	
					(0.110)	(0.560)			

Notes: DCCE = pooled dynamic common-correlated-effects estimator; SG = System GMM; DG = Difference GMM; Instr. = number of instruments; ρ = coefficient on lagged GFCF; CD = residual cross-sectional-dependence statistic. All GMM instrument sets are collapsed. SG1 uses lag 1–2 for the lagged dependent variable and lag 2–3 for Greenfield/M&A, with lagged macroeconomic controls and year indicators as standard instruments. SG3 uses lag 1–2 for the lagged dependent variable and lag 3–4 for Greenfield/M&A. SG4 uses lag 1 for the lagged dependent variable and lag 2 for Greenfield/M&A. DG1 uses the SG1 lag windows without the level equation. SG1 is the primary GMM robustness specification. SG2 is reported separately because it retains the SG1 lag windows but additionally instruments lagged macroeconomic controls with lags 1–2; its significant Greenfield coefficient coincides with rejection of the lagged-dependent-variable Difference-in-Hansen subset test (p = 0.047).

4.6 Same-estimand and harmonized inference audit

The targeted audit separates the estimator and instrument sensitivity from changes in the underlying empirical object. On the exact 3,111-observation preferred sample, replacing region-by-year shocks with global year effects changes the Greenfield coefficient from − 0.0010 to 0.0005 and the M&A coefficient from 0.0309 to 0.0404; both remain statistically imprecise. The common-shock comparison therefore changes neither the scale nor the substantive interpretation of the focal coefficients in an economically meaningful way.

A stricter harmonization uses the SG1 observed equation, lagged controls, year effects, and effective sample. The matched fixed-effects comparator yields Greenfield = 0.0016 (p = 0.835) and M&A = 0.0384 (p = 0.349). SG1 changes those point estimates to 0.1276 (p = 0.206) and − 0.0686 (p = 0.819), but deeper FDI-mode lags (SG3), an ultra-parsimonious instrument set (SG4), one-step SG1, and Difference GMM also leave both focal coefficients statistically imprecise. The magnitude range is nontrivial; therefore, the audit does not imply estimator irrelevance; rather, it shows that the preferred lack of raw-scale statistical precision persists across the retained same-estimand designs. SG2 is shown as a diagnostic contrast because it additionally changes

the instrument classification of the lagged macroeconomic controls. Its positive Greenfield coefficient is statistically significant, but the lagged-dependent-variable Difference-in-Hansen subset test rejects at p = 0.047; therefore, SG2 is not promoted as preferred evidence.

Table 4
Same-estimand and harmonized estimator/instrument audit

Model	N	Instr.	ρ	Greenfield	M&A	AR(2) p	Hansen p
FE matched to SG1	3109	—	0.701	0.0016 (0.0078), p=.835	0.0384 (0.0409), p=.349	—	—
SG1 two-step	3109	33	0.854	0.1276 (0.1004), p=.206	-0.0686 (0.2988), p=.819	0.160	0.418
SG3 deeper FDI lags	3109	33	0.825	0.2010 (0.1999), p=.316	0.3337 (0.7633), p=.663	0.412	0.864
SG4 ultra-parsimonious	3109	30	0.829	-0.0066 (0.1298), p=.960	0.1316 (0.4516), p=.771	0.732	0.404
SG1 one-step	3109	33	0.836	0.1816 (0.1737), p=.297	0.3527 (0.4485), p=.433	0.349	0.418
SG2 controls GMM†	3109	39	0.849	0.1662 (0.0704), p=.019	-0.1144 (0.2979), p=.701	0.103	0.298
DG1	2947	29	0.800	0.0713 (0.1095), p=.516	0.1439 (0.5599), p=.798	0.495	0.747
FE matched to DG1	2946	—	0.687	0.0006 (0.0077), p=.937	0.0403 (0.0419), p=.338	—	—

Notes: Coefficients are reported with standard errors in parentheses and exact Stata p-values. The fixed-effects comparators use the same observed equation and, as closely as the estimator permits, the same effective sample as the corresponding GMM model does. All GMM instrument sets were collapsed. SG1 uses lag 1–2 for the lagged dependent variable and lag 2–3 for Greenfield/M&A; SG3 moves the Greenfield/M&A window to lags 3–4; SG4 uses lag 1 for the lagged dependent variable and lag 2 for Greenfield/M&A. DG1 removes the level equation while retaining the SG1 lag windows. † SG2 additionally instruments lagged macroeconomic controls with lags 1–2 and is not preferred because the Difference-in-Hansen test for the lagged-dependent-variable subset rejects at p = 0.047. The audit was a robustness comparison, not a causal model-selection ranking.

4.7 Restricted-outcome and additional sensitivity evidence

The restricted private fixed capital formation outcome uses only 1,243 observations from 83 countries. Greenfield is borderline at p = 0.055, whereas M&A is statistically imprecise at p = 0.384. The substantially smaller and geographically uneven sample prevents the results from being interpreted as representative of the global panel. Therefore, the broader robustness architecture must be interpreted using two layers. When the raw outcome and raw entry-mode measures are held fixed and the observed equation is closely harmonized, the focal conclusion of statistically imprecise Greenfield and M&A associations is robust across the retained common shock and dynamic panel designs. In contrast, selected changes in functional forms or instrument classifications could alter statistical significance. This partitioning makes the source of sensitivity more precise and avoids attributing every difference to the estimator alone.

The specification-stability audit makes this distinction explicit. Across the retained raw-scale total-GFCF specifications, coefficient magnitudes vary, and some signs differ, but the diagnostically preferred evidence does not show pervasive sign switching or a stable statistically significant entry-mode effect. Greenfield point estimates in the broader raw-scale audit are positive in most specifications but can be negative in selected designs, while M&A is positive in most specifications. These sign counts are descriptive rather than a specification-curve test because the models differ in common-shock treatment, dynamic assumptions, instrument design, and in some cases effective sample. The targeted same-estimand audit reported in Table 4 shows more directly that the preferred absence of statistical precision is not an artifact of choosing the region-by-year fixed-effects specification: matched fixed-effects, SG1, deeper-lag SG3, ultra-parsimonious SG4, one-step SG1, and Difference GMM all leave the two entry-mode coefficients statistically imprecise. The isolated raw-scale Greenfield significance in SG2 is reported as a diagnostic contrast and not promoted because its lagged-dependent-variable Difference-in-Hansen subset test rejects at the 5 percent level.

A finite-temperature sensitivity check reached the same conclusion. A calendar split-panel jackknife (SPJ) raises the lagged-capital-formation coefficient from 0.700 in the preferred dynamic fixed-effects model to 0.794 (country-cluster bootstrap SE = 0.038; 95% percentile interval [0.714, 0.871]). In contrast, the SPJ Greenfield estimate is 0.005 (SE = 0.016, p = 0.744; 95% percentile interval [-0.018, 0.046]) and the M&A estimate is 0.044 (SE = 0.064, p = 0.490; 95% percentile interval [-0.030, 0.217]). All 499 country-cluster bootstrap replications were usable. Therefore, the correction materially raises the estimated persistence but does not generate a precise directional association for either entry mode. Because the two half-panels contain 157 and 156 economies, respectively, after complete-case restrictions, the SPJ is retained as a finite-T diagnostic rather than being promoted to the preferred estimator. Supplementary Table S3 reports the full-, half-panel, and SPJ estimates.

5. Discussion

The empirical evidence addresses the hypotheses and research question more clearly than a conventional crowding-in/crowding-out classification. After conditioning on investment persistence, country effects, macroeconomic controls, and region-specific annual shocks, neither raw Greenfield investment nor M&A sales displays a statistically precise average association with host-country fixed capital formation; accordingly, H1a and H1b are not supported in the preferred specification. The same-estimand audit materially sharpens this conclusion. Replacing region-by-year shocks with global year effects on the identical sample does not change the focal inference, and the matched fixed-effects/GMM comparisons show that the lack of statistical precision persists across the diagnostically retained SG1, SG3, SG4, one-step SG1, and Difference-GMM

designs. The coefficient magnitudes are nevertheless sensitive to the dynamic estimator and instrument design. The strongest changes in statistical significance arise outside the harmonized set—under the IHS transformation and under SG2, which changes the instrument classification of lagged controls and rejects a relevant Difference-in-Hansen subset test. The main finding is therefore better described as robust imprecision of the raw-scale average association under same-estimand comparisons, combined with sensitivity of magnitude and selected significance results when the empirical object or instrument classification changes.

The result provides evidence consistent with one explanation for the long-standing disagreement in the FDI–investment literature. Competing studies have reported crowding out, crowding in, and no relationship between FDI and domestic investment (Agosin & Machado, 2005; Emako et al., 2023; Farla et al., 2016; Magbondé et al., 2025; Morrissey & Udomkerdmongkol, 2012). The present evidence indicates that not all forms of specification variation should be treated alike. Within a closely harmonized raw-scale estimand, changing the common-shock structure or the retained dynamic-panel estimator changes the coefficient magnitude more than it changes the focal conclusion about statistical precision. Larger inferential changes emerge when the functional form, outcome definition, timing, or instrument classification is altered. This pattern is consistent with the view that measurement and empirical architecture can contribute to divergent findings, but it does not by itself replicate or reconcile every published estimate. The present outcome is total host-country fixed capital formation rather than domestic-owned private investment, while the focal FDI variables distinguish announced Greenfield projects from cross-border acquisition sales. The empirical objects therefore do not map one-to-one onto the crowding concept used in earlier studies. A statistically imprecise Greenfield coefficient in the present design should not be interpreted as evidence that Greenfield projects never create productive capacity; rather, the aggregate within-country relationship is not estimated precisely enough to separate direct capacity addition from displacement, realization failure, and other offsetting channels.

The entry-mode results also qualify a simple presumption that Greenfield investment should uniformly dominate M&A because it is closer to new capital creation. Harms and Méon (2018) and Gopalan et al. (2018) provide evidence consistent with stronger Greenfield macroeconomic benefits, while Ashraf and Herzer (2014) find Greenfield crowding out and Nguyen et al. (2021) document a different dynamic pattern in Vietnam. The present global results do not reproduce a statistically precise positive Greenfield coefficient on the raw scale. The contrast may reflect differences in outcome definitions, geographic composition, time periods, transformations, and dynamic structure. Recent entry-mode studies in entrepreneurship, innovation, and firm-level productivity likewise show that host conditions and outcome choice can alter the apparent ranking of Greenfield and acquisition modes (Ahn et al., 2025; Damioli & Marin, 2024; Nguyen, 2023). Theoretical treatment of entry mode should therefore emphasize mechanisms and boundary conditions rather than a fixed welfare ranking; the present aggregate analysis supports this interpretation but does not directly validate any single transmission mechanism.

The inferential evidence is equally consequential. Capital formation exhibits strong persistence; thus, static specifications risk attributing serial adjustment to contemporaneous FDI variables. The calendar split-panel jackknife reinforces this point: correcting finite-T dynamic fixed-effects bias raises the persistence estimate from approximately 0.700 to 0.794, while the Greenfield and M&A coefficients remain statistically imprecise ($p = 0.744$ and $p = 0.490$, respectively; both percentile intervals include zero). The same-estimand audit adds a complementary result: on the SG1 observed equation and sample, the matched fixed-effects model yields

Greenfield and M&A coefficients of 0.0016 and 0.0384, respectively, whereas SG1 yields 0.1276 and − 0.0686; deeper-lag, ultra-parsimonious, one-step, and Difference-GMM variants generate different magnitudes, yet none of the diagnostically retained designs produces a statistically precise focal coefficient. Thus, the dynamic estimator and instrument treatment matter for magnitude without overturning the main raw-scale inference. Cross-sectional dependence remains material in global macro panels. The DCCE results leave both entry-mode coefficients imprecise but retain substantial residual dependence, illustrating that adding common-factor proxies does not automatically solve every dependence problem, particularly when the effective time dimension is modest (Everaert & De Groote, 2016; Juodis & Reese, 2026). An auxiliary Pesaran CD test on the preferred and matched-TWFE residuals could not be computed with the available implementation because the estimation sample is highly unbalanced with insufficient common observations; no FE/TWFE residual-CD statistic is therefore claimed.

The policy implications are correspondingly narrower but more defensible. Investment-promotion agencies and development authorities should not infer productive capacity gains merely from an increase in an aggregate FDI measure, nor should they assume that Greenfield announcements necessarily translate one-for-one into realized fixed-capital formation. Evaluation frameworks should distinguish new-establishment projects from ownership-transfer transactions and, where data permit, track project realization, follow-on investment, local sourcing, and capacity expansion. The evidence does not identify a causal policy effect and, therefore, does not justify a blanket preference for one entry mode. The relevant policy lesson is measurement discipline: incentives should be tied to observable development outcomes rather than the FDI label alone.

Several limitations define the boundary of the conclusions. Announced Greenfield project values do not equal realized balance-of-payments investments. Cross-border M&A sales can be zero or negative and represent ownership transactions rather than new productive assets. Total gross fixed capital formation cannot distinguish domestic from foreign ownership. The preferred model remains observational, leaving reverse causality and time-varying omitted variables as unresolved threats. The calendar split-panel jackknife indicates that finite-T correction raises the estimated persistence of capital formation but leaves both focal entry-mode coefficients imprecise; however, this diagnostic is not a definitive bias correction because complete-case restrictions yield different country coverage across the two half-panels. Residual cross-sectional dependence remains in the DCCE specifications. A direct residual-CD test for the preferred and matched-TWFE models could not be computed in the auxiliary audit because the relevant panel was too unbalanced for the available implementation; therefore, the absence of such a statistic should not be interpreted as evidence of cross-sectional independence. GMM inference depends on internal moment assumptions and is accompanied by singular weighting-matrix warnings in the audited runs. The same-estimand audit shows that focal statistical imprecision is robust across the retained GMM designs, but this does not validate the internal instruments or create causal identification. The private-investment robustness sample is substantially smaller and less globally representative. In addition, the primary inferential window ends in 2024 because 2025 macroeconomic controls remain incomplete. These limitations prevent universal claims about the welfare ranking of FDI entry modes.

Future research can address these limitations by sharpening the mechanism rather than merely adding estimators. Project-level or firm-level data could distinguish announced from realized Greenfield capital expenditures and identify whether acquisitions are followed by expansion or restructuring. Bilateral data could

separate origin-country shocks from host-country responses. Natural experiments or policy reforms that change the relative cost of Greenfield entry and acquisition could strengthen causal identification. Longer panels with richer common-factor structures could improve the separation of global shocks from domestic adjustments. Finally, explicit measurement-error models could treat Greenfield announcements, M&A transactions, and balance-of-payments FDI as imperfect indicators of distinct latent investment processes rather than interchangeable measures.

6. Conclusion

This study examines how announced greenfield investment and cross-border M&A sales are related to host-country fixed capital formation in a global dynamic panel and how robust the resulting inference is across empirical architectures. The preferred specification uses country-fixed effects, region-by-year shocks, macroeconomic controls, and lagged capital formation. The robustness design explicitly separates a same-estimand layer—matched common-shock and dynamic-panel comparisons using the raw total-GFCF outcome and raw GDP-scaled entry-mode measures—from broader exercises that alter timing, functional form, common-factor treatment, or outcome definition.

The central empirical result is deliberately measured. Host-country fixed capital formation is strongly persistent, and a calendar split-panel jackknife raises the persistence estimate from about 0.700 to 0.794, but neither raw GDP-scaled Greenfield investment nor M&A sales exhibits a statistically precise average association in the preferred model. The same-estimand audit shows that this focal lack of precision survives a matched year-effects model and the diagnostically retained parsimonious, deeper-lag, ultra-parsimonious, one-step, and Difference-GMM specifications. Coefficient magnitudes vary materially across these dynamic designs, but no retained same-estimand model yields a statistically precise Greenfield or M&A coefficient. Isolated significant Greenfield estimates arise under the inverse-hyperbolic-sine transformation and SG2; the former changes functional form, while the latter changes the instrument classification of lagged controls and rejects the relevant lagged-dependent-variable Difference-in-Hansen subset test. The evidence therefore supports neither a universal null mechanism nor a stable Greenfield-versus-M&A ranking.

The principal contribution is a measurement–estimand and robustness-of-inference contribution rather than a new universal sign for the FDI–investment relationship. The study first shows why announced greenfield projects, cross-border M&A sales, and total host-country fixed capital formation should not be interpreted as interchangeable accounting objects. It then decomposes robustness into comparisons that preserve the raw estimand and comparisons that change the empirical object. This decomposition reveals a more precise result than a generic claim of estimator sensitivity: the preferred conclusion of statistically imprecise raw-scale average entry-mode associations is comparatively robust to retained estimator, weighting, instrument-depth, and common-shock changes, even though coefficient magnitudes vary; larger inferential departures emerge when functional form or instrument classification changes. This does not constitute a new estimator or causal identification strategy. Instead, it provides a disciplined empirical benchmark for distinguishing specification-sensitive magnitudes from conclusions that persist across harmonized designs.

Empirical evidence does not support a causal policy ranking of Greenfield investment and M&A. Policy evaluation should instead distinguish investment modes, verify realized capacity creation, and avoid using headline FDI values as indicators of development impact. The measurement-estimand and harmonized-

comparison principles used here may also be informative for other macro-panel settings in which closely related measures represent different economic transactions. However, the empirical findings themselves should not be generalized beyond the observed sample and measurement system without additional validation.

Stronger future identification requires project-level realization data, ownership information, bilateral shocks, or quasi-experimental policy variation capable of separating foreign-investment selection from host-country investment demand. Future robustness work should likewise distinguish changes in the estimator from changes in the estimand rather than combining them into a single sensitivity label. Until stronger identification and measurement are available, harmonized sensitivity analysis and explicit measurement boundaries provide a more credible basis for inference than choosing the specification that yields the strongest directional result.

Declarations

Competing interests

The authors declare that there are no known competing financial interests or personal relationships that could have influenced the work reported in this study.

Declarations of AI Use

The authors used artificial intelligence (AI) tools only for language editing, academic style refinement, formatting assistance, and consistency checking. The AI tool was not used to generate data, perform statistical analyses, fabricate results, create references, or make substantive theoretical or empirical decisions. The authors reviewed and verified all scientific claims, model specifications, numerical results, citations, interpretations, and final manuscript content, and took full responsibility for the manuscript.

Ethical Approval

Ethical approval was not required for this study because it did not involve human or animal subjects. This study relied exclusively on country-level macroeconomic and institutional panel data sourced from publicly accessible databases.

Informed Consent

Informed consent was not required because the study did not involve any human participants. All analyses were conducted using aggregated secondary data at the national level.

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Author Contribution

N.S. conducted all aspects of the research and manuscript preparation, including conceptualization, methodology, data collection, formal analysis, writing of the original draft, reviewing, and approving the final version for submission.

Data Availability

The data used in this study are publicly available and can be accessed via the Zenodo repository at https://doi.org/10.5281/zenodo.22831766

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Figures

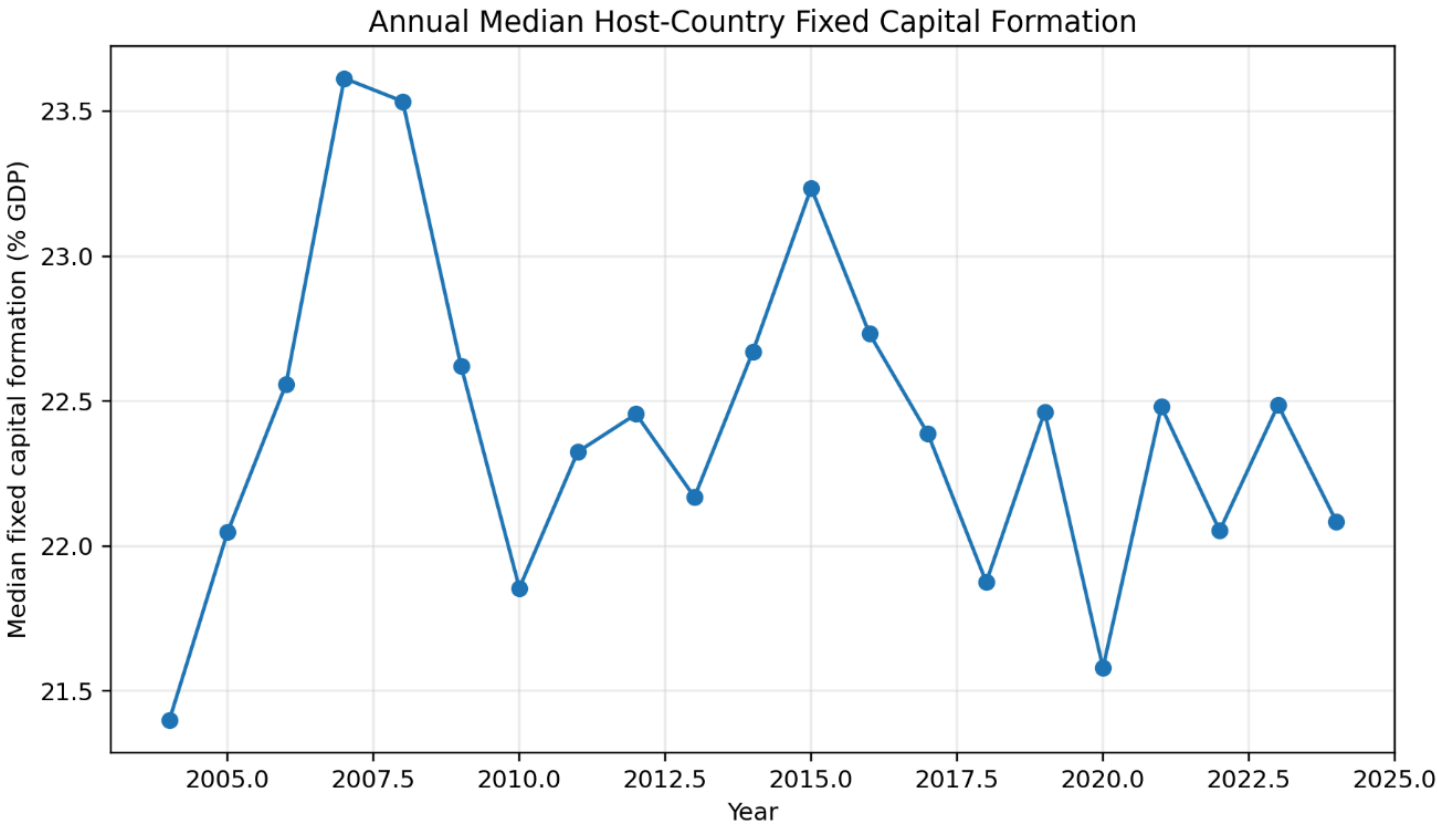


Figure 1

Annual median host-country fixed capital formation

Notes: Annual medians are computed on the preferred-model complete-case sample; the number of economies can vary by year because the estimation panel is unbalanced after complete-case restrictions.

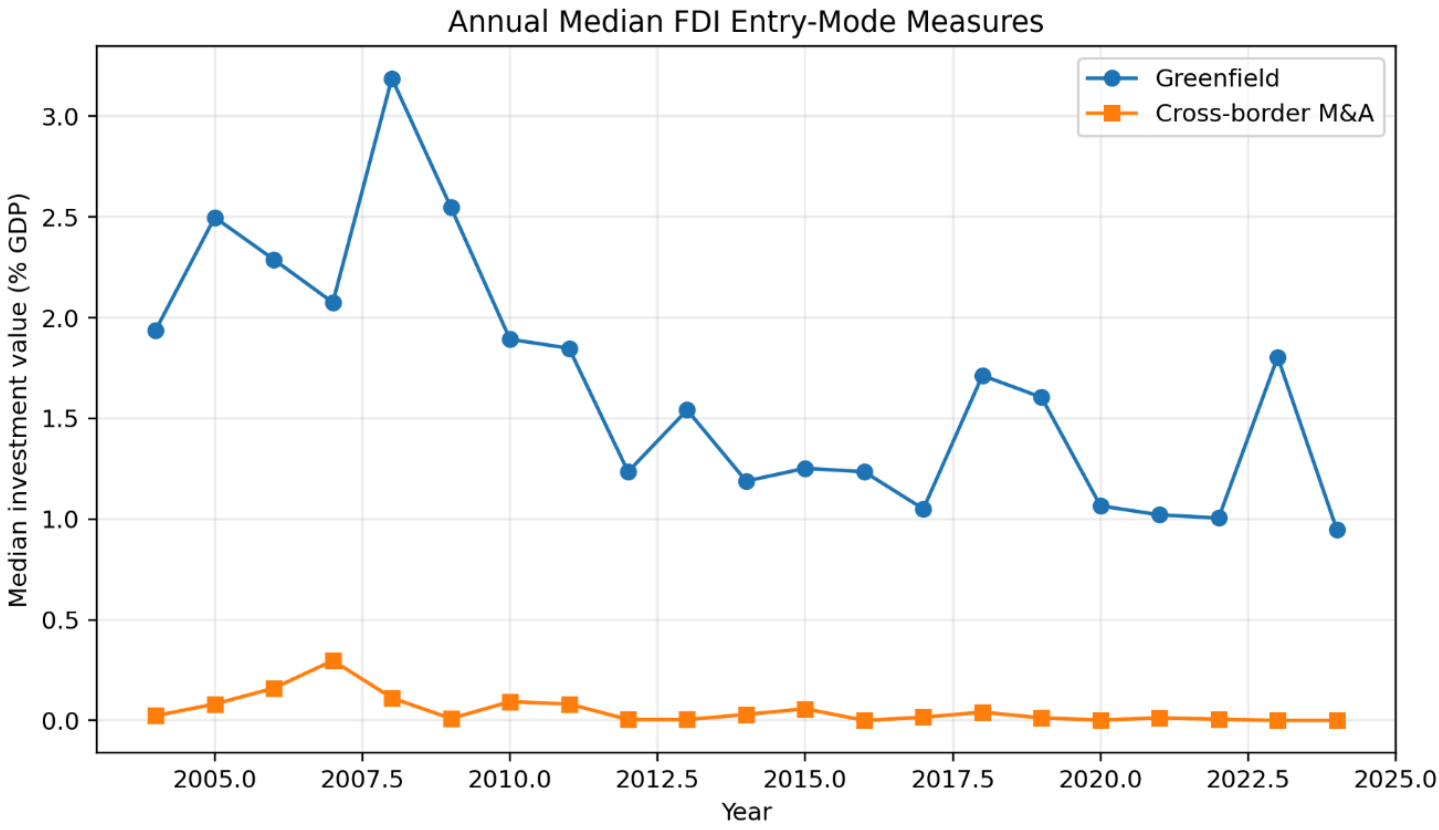


Figure 2

Annual median FDI entry-mode measures

Notes: Annual medians are computed on the preferred-model complete-case sample; the number of economies can vary by year because the estimation panel is unbalanced after complete-case restrictions.

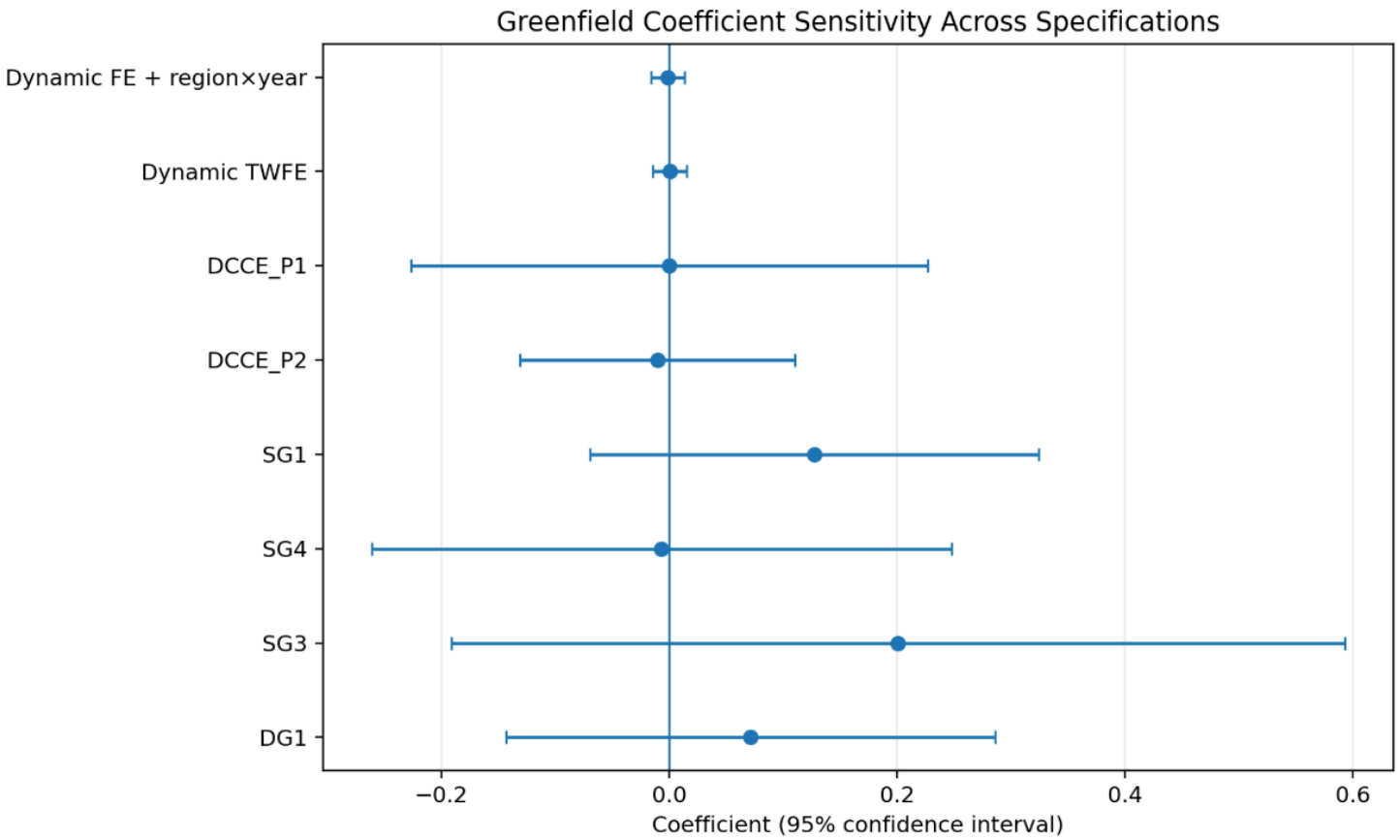


Figure 3

Greenfield coefficient sensitivity across raw-scale specifications

Notes: Points are coefficient estimates; horizontal bars are 95% confidence intervals based on reported standard errors. The figure is a descriptive sensitivity summary and does not rank estimators.

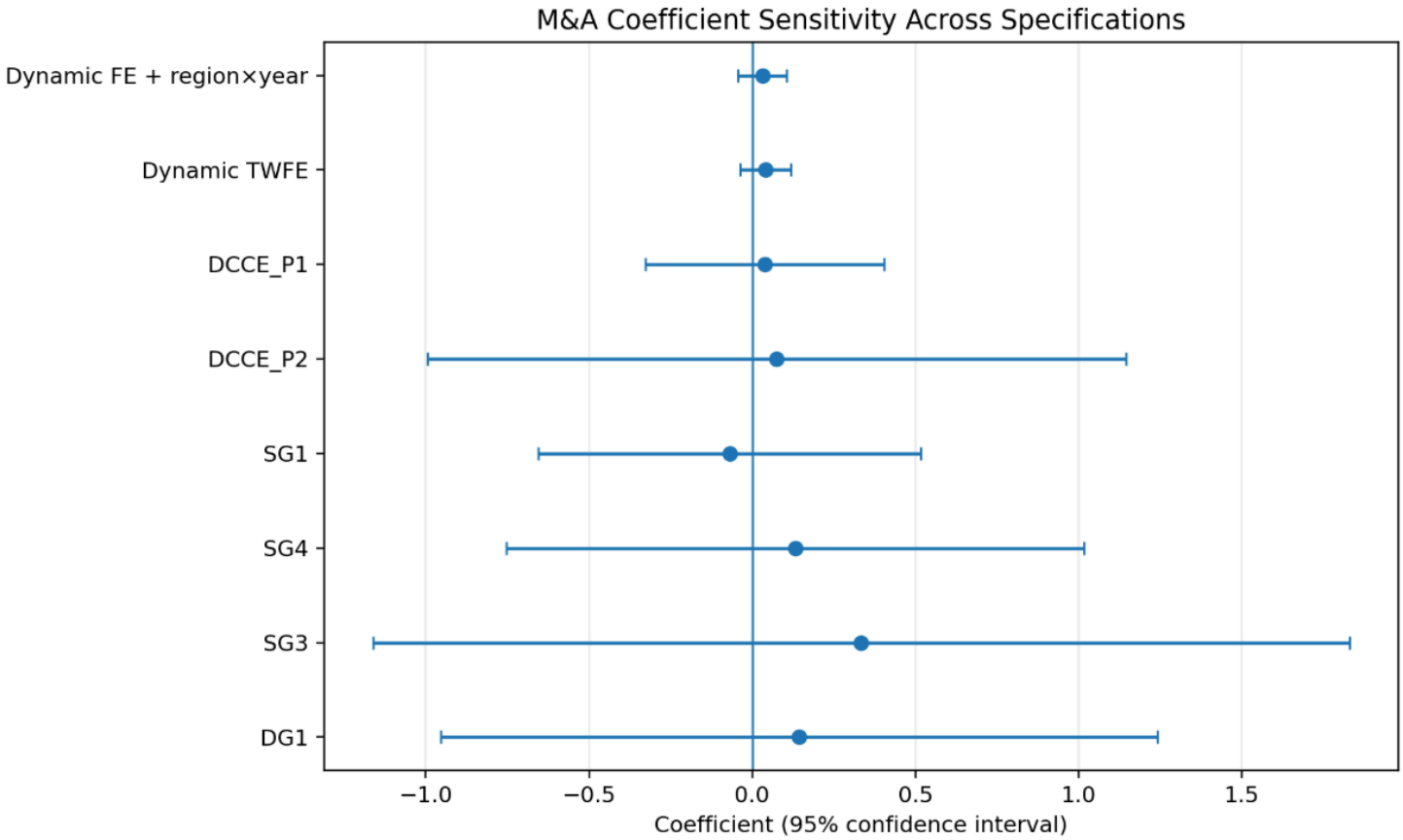


Figure 4

Cross-border M&A coefficient sensitivity across raw-scale specifications

Notes: Points are coefficient estimates; horizontal bars are 95% confidence intervals based on reported standard errors. The figure is a descriptive sensitivity summary and does not rank estimators.

Supplementary Files

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- SupplementaryTableS123.docx