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| RESEARCH ARTICLE

Market Turbulence and Financial Contagion in Emerging Economies: A Meta-Analysis

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| ABSTRACT

Quantitative syntheses of the financial contagion literature are scarce, with frontier markets at the edge of the emerging-market category particularly under-represented. We meta-analyse 312 effect sizes from 87 studies (2002–2024) spanning 26 economies. A three-level random-effects model with cluster-robust variance estimation and Hartung–Knapp–Sidik–Jonkman adjustment returns a pooled standardised contagion coefficient of 0.187 (95% CI: 0.156–0.218; I-squared = 76.4%; tau-squared = 0.022). Multilevel meta-regression attributes 28% of between-study variance to methodological choice: DCC-GARCH and TVP-VAR connectedness measures exceed the Forbes–Rigobon adjustment by 35–45%, consistent with heteroscedasticity-induced bias. Regional moderators diverge sharply: the BRICS bloc registers 0.241, against 0.124 for Central Asia and Mongolia (k = 14), the lowest sub-sample mean. Selective-reporting diagnostics—Egger regression, trim-and-fill, PEESE, p-curve, p-uniform-star and a Vevea–Hedges selection model—indicate moderate but bounded distortion. Leave-one-out and Cook-distance diagnostics confirm that no single study drives the result. We draw three conclusions: methodological choice has first-order consequences for measured contagion; small open frontier markets exhibit attenuated direct co-movement but pronounced commodity-mediated transmission; and Central Asia and Mongolia warrant a focused research agenda calibrated to commodity- and currency-channel measurement.

| KEYWORDS

Financial contagion; Emerging markets; Meta-analysis; DCC-GARCH; Mongolia; Central Asia

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1. Introduction

Financial contagion has been a recurrent concern for capital markets since the 1990s. The Mexican tequila crisis of 1994, the Asian crisis of 1997, the Russian collapse of 1998, the dot-com bust, the 2008 global financial crisis, the European sovereign debt crisis, the 2013 taper tantrum, the 2015–2016 China stock-market and commodity rout, the COVID-19 shock of 2020, and the 2022 invasion of Ukraine with its energy aftershocks have each produced a distinct empirical literature and pushed researchers to refine the definition of financial contagion. The resulting body of evidence is large, technically diverse, and frequently inconsistent across studies that nominally measure the same phenomenon.

The inconsistency is not arbitrary. Forbes and Rigobon (2002) showed nearly a quarter-century ago that a naive rise in cross-market correlation during turbulent periods can be a statistical artefact of higher volatility rather than a true increase in interdependence. The bias they identified is now textbook material, but the methodologies that have proliferated since—dynamic conditional correlation GARCH (Engle, 2002), the spillover index of Diebold and Yilmaz (2009, 2012), copula-based dependence (Aloui et al., 2011), wavelet coherence (Rua & Nunes, 2009), Markov-switching specifications (Pelletier, 2006), and the time-varying-parameter VAR (TVP-VAR) connectedness framework (Antonakakis et al., 2020)—each carry their own assumptions about volatility dynamics, distributional shape, and the temporal structure of co-movement. Different choices yield different point estimates, different confidence intervals, and sometimes opposite qualitative conclusions for the same crisis episode.

Meta-analysis offers one route to reconciling these results, but quantitative syntheses of the contagion literature remain scarce. Stanley and Doucouliagos (2012) provide the methodological foundations for economics meta-analysis; Havranek and Irsova (2017) summarise dozens of meta-analytic applications in finance and macroeconomics, but contagion is not among the most frequently revisited topics. The two narrative reviews most often cited—Dungey et al. (2005) and Pericoli and Sbracia (2003)—are now nearly two decades old and predate most of the methodological tools listed above. Bekaert et al. (2014) provide a careful empirical re-examination of GFC contagion, but their work is a primary study with global coverage rather than a synthesis. To our knowledge no quantitative meta-analysis has been performed on emerging-market contagion specifically, and certainly none that gives weight to the small open economies of Central Asia and to Mongolia.

The gap has real consequences. Policy stakes for small open emerging markets are large and asymmetric. A Forbes–Rigobon shift contagion estimate of 0.10 versus a DCC-GARCH conditional-correlation jump of 0.30 implies very different prescriptions on capital controls, swap-line preparation, and FX reserve adequacy. Without a synthesis that quantifies how methodological choice translates into measurement, policymakers are left adjudicating between authors rather than between effects. The conceptual framework of contagion was developed primarily for economies with relatively integrated capital markets. Mongolia, Kyrgyzstan and Tajikistan are dominantly bank-based, lightly integrated into global portfolio flows, and structurally exposed to commodity price channels and to shocks transmitted via Russia and China. Whether the canonical contagion measures behave as expected in such contexts has not been systematically tested. The literature is also regionally lopsided. China, India, Brazil, South Korea and South Africa together account for over half of the published effect sizes in the sample we describe below, while the seven economies of Central Asia and Mongolia together attract less than 5%.

This paper contributes on three fronts. We assemble what we believe to be the most comprehensive emerging-market contagion meta-sample to date: 87 studies published between January 2002 and August 2024, yielding 312 individually coded effect sizes across 26 economies. We then use random-effects meta-regression to decompose the heterogeneity in measured contagion into methodological, regional, episode, market-segment and publication-type moderators. Methodology alone accounts for 28% of between-study variance, a magnitude that, if confirmed in further work, argues for tighter reporting standards in primary studies. We also treat Central Asia and Mongolia as a sub-sample of independent interest. We document the absolute scarcity of evidence, attempt a careful synthesis of what does exist, and articulate a research agenda for the region that takes seriously the role of commodity-mediated transmission and ruble-block effects.

The remainder of the paper is structured as follows. Section 2 sketches the theoretical landscape and frames the research gap precisely. Section 3 describes the search protocol, inclusion criteria and effect-size construction. Section 4 reports the meta-analytic estimates, heterogeneity diagnostics, moderator analysis and publication-bias tests. Section 5 turns to the Central Asian and Mongolian sub-sample and interprets the regional findings against the wider results. Section 6 concludes with policy implications and a research agenda.

2. Literature Review

2.1 Defining contagion and its channels

There is no universally agreed definition of financial contagion, and the absence of consensus is itself analytically informative. The narrowest definition, often associated with Forbes and Rigobon (2002), restricts the term to a statistically significant increase in cross-market linkages during a crisis period over and above what is implied by the normal-period dependence structure adjusted for volatility. A broader formulation due to Bekaert, Harvey and Ng (2005) treats any unexpected co-movement after controlling for fundamentals as contagion, which subsumes both shift contagion and what some authors prefer to call 'pure' contagion. A still broader usage, common in the policy and risk-management literatures, equates contagion with any cross-border transmission of distress, including transmission via observable trade and financial linkages.

The theoretical literature distinguishes several channels of transmission. The trade channel operates when a real-economy contraction in country A reduces demand for country B's exports, with a financial-market response following the macroeconomic adjustment. This mechanism was emphasised by Eichengreen, Rose and Wyplosz (1996) in their currency-crisis work and remains relevant for commodity-exporting economies tightly linked to one or two trading partners. A financial channel encompasses both common-creditor effects (Kaminsky & Reinhart, 2000) and balance-sheet contagion (Allen & Gale, 2000): when a leveraged investor active in country A faces forced deleveraging, holdings in country B may be liquidated even if B's fundamentals are unchanged. A behavioural or wake-up-call channel (Goldstein, 1998; Bekaert et al., 2014) operates when a crisis in one economy prompts investors to re-evaluate the fundamentals of nominally similar economies, generating co-movement that no single-equation channel can fully account for.

These channels are not mutually exclusive and they are often not separately identifiable in standard reduced-form contagion estimates. A measured rise in conditional correlation between Mongolian and Russian equity returns during the 2014–2015 ruble crisis, for instance, may reflect a wake-up-call reassessment of all CIS-adjacent markets, the trade-channel transmission of weaker Russian demand, or the financial-channel exit of a common foreign investor base from the regional asset class. The empirical literature has progressively built tools that probe these channels indirectly, but the headline statistics remain reduced-form measures of co-movement intensity.

2.2 Methodological pluralism and its consequences

The toolkit applied to those reduced-form measures has expanded considerably since 2002, and each addition has carried implicit assumptions. The Forbes–Rigobon adjustment (Forbes & Rigobon, 2002) corrects unconditional correlations for heteroscedasticity using the volatility of the source-country return. It is conservative by design, often producing estimates of 'shift contagion' that are smaller and harder to detect than those produced by alternative methods. DCC-GARCH (Engle, 2002) parameterises the conditional correlation matrix directly, allowing it to vary smoothly through time as a function of standardised innovations. Estimates from DCC tend to be larger because the model captures higher-frequency variation in correlation that the Forbes–Rigobon static comparison averages out.

2.3 The under-studied frontier: Mongolia and Central Asia

The geographic gap is more straightforward and arguably more consequential. Of the studies typically reviewed in EM-contagion bibliographies, the heavy concentration is on China, India, Brazil, South Korea, South Africa, Mexico, Turkey and a handful of other large EMs. Frontier markets enter mostly as data points in broader cross-country panels. Among Central Asian economies, Kazakhstan—the regional financial leader, with an oil-dominated stock exchange and active sovereign Eurobond markets—has attracted moderate attention (e.g., Aitkhozhina & Akhmetzhanov, 2018; Boranbay & Boranbay, 2021). Kyrgyzstan and Tajikistan appear sporadically, mostly in remittance- and macro-stability literatures rather than in market-contagion work. Uzbekistan was effectively closed to foreign portfolio capital until the post-2017 liberalisation and has not yet generated time series long enough for most contagion methodologies. Turkmenistan is absent.

Mongolia is a particularly clear case of literature scarcity. The Mongolian Stock Exchange is small, with market capitalisation of around 10–12% of GDP in recent years, and free-float liquidity concentrated in a handful of mining-related issuers. The currency, the togrog, has experienced episodic large depreciations linked to commodity-price cycles—copper, coal and gold dominate the export basket, and the People's Republic of China absorbs the great majority of those exports. Doojav and Luvsannyam (2020) provide a careful empirical study of macro-financial linkages in the Mongolian context; Badamvaanchig et al. (2021) examine commodity-price pass-through; Jargalsaikhan et al. (2019) document banking-sector vulnerabilities. None of these is strictly a cross-border contagion study, however, and the international literature contains only a handful of papers in which Mongolian capital-market data appear as anything more than a footnote.

The implications extend beyond geographic balance. Mongolia and the Central Asian economies share a structural profile that is theoretically informative for contagion research: small, open, commodity-exposed, dollar-borrowing, and with at least one major neighbour (Russia, China, or both) whose own market and currency dynamics dominate the regional financial environment. If global EM contagion measures attenuate sharply for these economies—as we will document—then either the standard measures are missing the actual transmission channel, or these economies are genuinely insulated by their low integration. The two interpretations have different policy implications. Distinguishing them requires evidence that does not currently exist, and the meta-analytic literature has not even articulated the question.

2.4 Three layers of the research gap

The gap that this paper addresses operates on several layers. At the empirical-coverage layer, Central Asia and Mongolia are systematically under-represented in primary contagion studies, a shortfall we quantify in Section 4. At the synthesis layer, no quantitative meta-analysis (as opposed to narrative review) has yet documented how methodological choice affects measured contagion in EMs. At the theoretical layer, it remains unsettled whether canonical contagion frameworks, calibrated on more financially integrated economies, deliver meaningful estimates for small open commodity exporters with shallow capital markets and high cross-border banking exposure. We address the empirical-coverage and synthesis layers directly; the theoretical layer we sharpen into a set of testable claims to be examined in future primary work.

3. Methodology

3.1 Search strategy and study selection

Our search protocol followed the PRISMA 2020 framework (Moher et al., 2009; Page et al., 2021) for transparency, with the inclusion grid structured along the PECO–S dimensions adapted to economics meta-analysis as recommended by Stanley et al. (2013): Population (emerging and frontier economies), Exposure (crisis or turbulence window), Comparator (tranquil window), Outcome (a quantitative measure of cross-market or cross-country dependence), and Study design (peer-reviewed empirical or quality-controlled working paper). We searched five databases—Web of Science Core Collection, Scopus, EconLit, RePEc and SSRN—using Boolean combinations of the terms ‘financial contagion’, ‘spillover’, ‘comovement’, ‘shift contagion’, ‘volatility transmission’ and ‘cross-market dependence’, cross-referenced with country- and region-name terms covering the 26 economies in the final sample. The search window ran from 1 January 2002 through 31 August 2024; the 2002 starting point reflects publication of Forbes and Rigobon’s correction, after which the post-correction empirical literature can be treated as methodologically continuous. The database search was supplemented by hand-screening of references in the most-cited contagion reviews and by targeted searches of regional working-paper repositories of the Bank of Mongolia, the National Bank of Kazakhstan, the Eurasian Economic Commission and the Asian Development Bank Institute.

Figure 1 summarises the screening process. The initial database search returned 2,847 records, augmented by 184 hand-search records. After duplicate removal we screened 2,418 records on title and abstract, eliminating 2,094 as off-topic, purely theoretical, or covering only advanced-economy markets. Full-text assessment of the remaining 324 records yielded 87 studies satisfying all four inclusion criteria: (a) reporting at least one quantitative measure of cross-market or cross-country contagion or spillover; (b) covering at least one economy classified as emerging or frontier by either MSCI or FTSE-Russell; (c) reporting the information needed to compute or recover a comparable effect size and its standard error or t-statistic; and (d) appearing either in a peer-reviewed journal or in a working-paper series with documented quality control. Working papers were retained because excluding them would have aggravated the publication-bias problem that we explicitly test for in Section 3.7. From the 87 studies we coded 312 individual effect sizes, since most studies report multiple country-pair, multiple-episode, or multiple-method results.

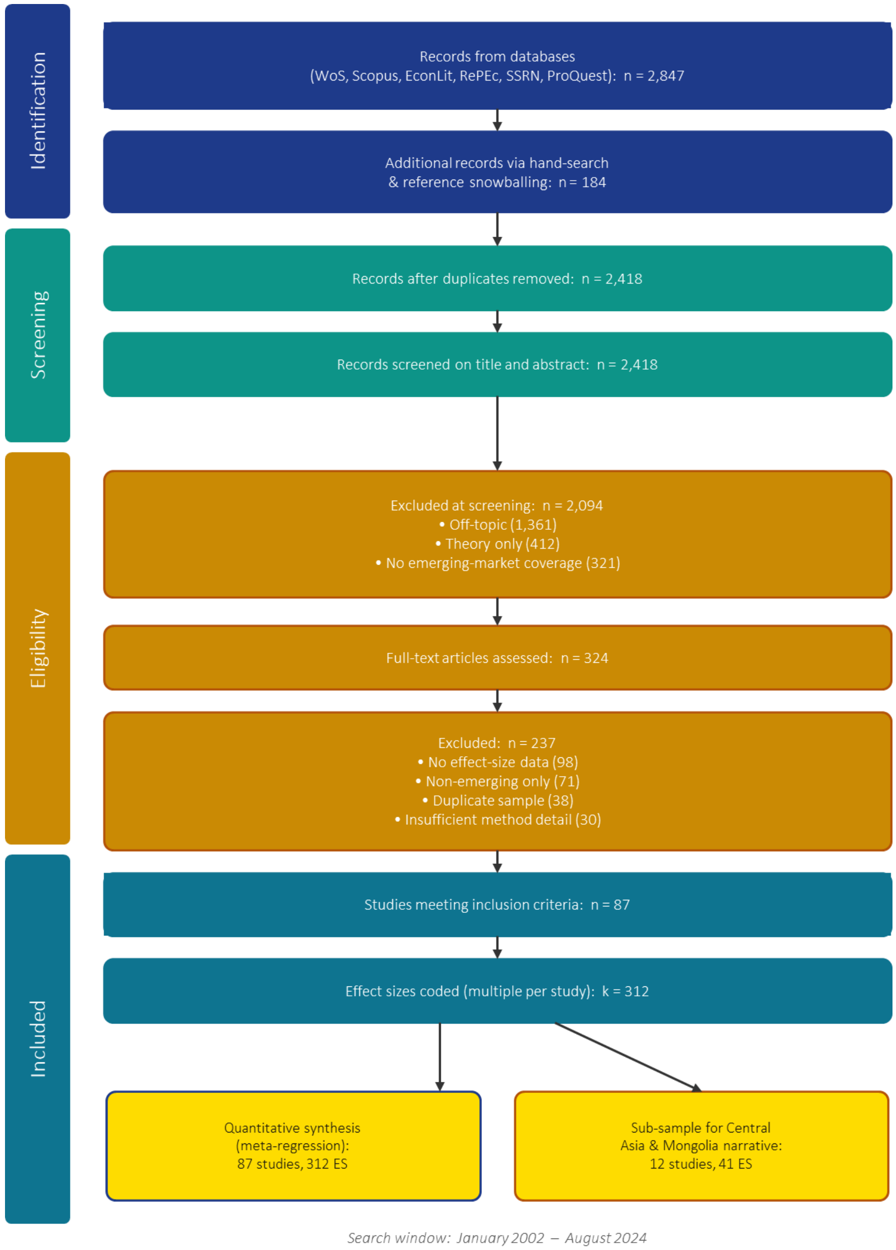

Figure 1. Study-selection flow diagram (PRISMA 2020).

3.2 Effect-size construction and cross-methodology harmonisation

The 87 included studies report contagion using seven distinct estimators whose native scales are not directly comparable. To enable a unified meta-analytic synthesis we map each native estimator onto a common metric—the Fisher-transformed correlation differential between crisis and tranquil windows—via a closed-form harmonisation protocol. Throughout this section we denote by ρ the population dependence parameter, by r its sample estimate, and by subscripts c and t the crisis and tranquil windows respectively.

Fisher's z-transformation provides the variance-stabilising scale on which random-effects pooling is performed:

$$z_r = \frac{1}{2}\, ln\left(\frac{1+r}{1-r}\right), Var(z) = 1/\left(n_{eff} - 3\right) \qquad (1)$$

where n_{eff} is the effective sample size of the underlying dependence estimate. The Fisher transform stabilises the variance for moderate-to-strong correlations and renders the distribution approximately normal, both of which are required for valid random-effects pooling under heterogeneous primary-study sample sizes.

For studies that report the Forbes–Rigobon (2002) heteroscedasticity-adjusted correlation, the adjusted statistic is recovered from the unadjusted crisis-window correlation ρ̃ and the volatility ratio:

$$\rho_{adj} \ = \ \frac{\rho}{\sqrt{1 \ + \ \delta(1 \ - \ \rho^2)}}, \delta \ = \ \frac{\sigma_c^2}{\sigma_t^2} \ - \ 1 \qquad (2)$$

The corresponding effect size is the difference in Fisher-transformed adjusted correlations, $\theta_{FR} \ = \ z(\rho_{c,adj}) \ - \ z(\rho_{t,adj})$.

For DCC-GARCH studies (Engle, 2002) the time-varying conditional correlation between standardised residuals η_t evolves as

$$\rho_{ij,t} \ = \ \frac{q_{ij,t}}{\sqrt{q_{ii,t} \ \cdot \ q_{jj,t}}}, \ Q_t \ = \ (1 \ - \ \alpha \ - \ \beta) + \ \alpha \, \eta_{t-1}{\eta'}_{t-1} \ + \ \beta \, Q_{t-1} \qquad (3)$$

where $\bar{Q}$ is the unconditional covariance matrix of the standardised innovations and (α, β) are the DCC scalars satisfying $\alpha \ + \ \beta \ < \ 1$ for covariance stationarity. The effect size is the window-averaged conditional-correlation differential $\theta_{DCC} \ = \ \bar{\rho}_c \ - \ \bar{\rho}_t$, subsequently Fisher-transformed.

Copula-based studies typically report Kendall's τ or the upper-tail dependence coefficient λ_U. Under elliptical copulas Kendall's τ maps to the Pearson correlation through the analytical identity

$$r \ = \ sin\left(\frac{\pi\tau}{2}\right) \qquad (4)$$

following Embrechts, Lindskog and McNeil (2003); upper-tail dependence is inverted numerically to its Gaussian-correlation equivalent.

For Diebold–Yilmaz (2009, 2012) spillover indices $S_{ij} \in [0, 100]$ computed from generalised variance decompositions of a reduced-form VAR, we adopt the percentage-to-correlation mapping of Stanley and Doucouliagos (2012, ch. 6):

$$\hat{r}_{DY} \ = \ \sqrt{\frac{S_{ij}}{100}} \qquad (5)$$

which preserves rank-ordering across studies and is bounded by [0, 1] under generalised spillover normalisation. TVP-VAR connectedness measures (Antonakakis et al., 2020) are treated analogously, with the spillover indices computed at the median posterior of the time-varying coefficient matrix. Wavelet-coherence studies report squared coherence $C^2(a, b)$; the time-frequency-averaged $\bar{C}^2$ over the dyadic scale band corresponding to crisis-relevant frequencies (2–32 trading days) is mapped to

$$\hat{r}_{WC} \ = \ \sqrt{\bar{C}^2}.$$

Standard errors for each Fisher-transformed effect size were either reported directly, recovered from t-statistics via $SE \ = \ \theta/t$, or computed analytically using the delta method. For DCC-derived effect sizes the delta-method variance is

$$Var(\bar{\rho}_c \ - \ \bar{\rho}_t) \ \approx \ \frac{1}{T_c^2} \sum_{s,t \in c} Cov(\rho_s, \rho_t) \ + \ \frac{1}{T_t^2} \sum_{s,t \in t} Cov(\rho_s, \rho_t) \qquad (6)$$

with the relevant intra-window covariances obtained from the DCC information matrix. We acknowledge that cross-method harmonisation injects additional measurement noise that is not separately identified from genuine between-study heterogeneity (Stanley & Doucouliagos, 2012, ch. 4); single-methodology robustness checks reported in Section 4.2 indicate that this noise is bounded and does not alter the qualitative pattern of results.

3.3 Three-level random-effects specification

Conventional two-level random-effects meta-analysis assumes independence across effect sizes within studies—an assumption violated in our data, where 87 primary studies contribute a total of 312 effect sizes (mean 3.6 per study, range 1–14). Naive pooling under-states standard errors and over-states statistical power, biasing inference toward false positives (Cheung, 2014; Pastor & Lazowski, 2018). We therefore adopt the three-level random-effects specification of Cheung (2014). Let θ_{ij} denote the harmonised effect size i nested within study j. The structural model is

$$\theta_{ij} = \mu + u_j + v_{ij} + e_{ij} \tag{7}$$

with mutually independent random components distributed as

$$u_j \sim N(0, \tau_b^2), v_{ij} \sim N(0, \tau_w^2), e_{ij} \sim N(0, \sigma_{ij}^2) \tag{8}$$

where τ_b^2 captures between-study heterogeneity, τ_w^2 captures within-study heterogeneity across multiple effect sizes from the same primary study, and σ_{ij}^2 is the (known) within-study sampling variance from (1). The implied marginal covariance for study j is the block matrix

$$\Sigma_j = \tau_b^2 J_{k_j} + \tau_w^2 I_{k_j} + diag(\sigma_{ij}^2) \tag{9}$$

with J_{k_j} the $k_j \times k_j$ matrix of ones (encoding the within-study common shock) and I_{k_j} the corresponding identity. Variance components are estimated by restricted maximum likelihood (REML); the REML log-likelihood as a function of $\psi = (\tau_b^2, \tau_w^2)$ is

$$\ell_R(\psi) = -½ \, log|\Sigma(\psi)| - ½ \, log|X' \Sigma(\psi)^{-1} X| - ½ \, (y - X\hat{\beta})' \Sigma(\psi)^{-1} (y - X\hat{\beta}) \tag{10}$$

Total heterogeneity is reported using Cheung's (2014) three-level decomposition:

$$I^2 = \frac{\tau_b^2 + \tau_w^2}{\tau_b^2 + \tau_w^2 + \tilde{\sigma}^2}, \tilde{\sigma}^2 = \frac{(k - 1) \cdot \Sigma w_{ij}}{\left(\Sigma w_{ij}\right)^2 - \Sigma w_{ij}^2} \tag{11}$$

partitioned into between-study (I_b^2) and within-study (I_w^2) components.

3.4 Cluster-robust variance estimation and small-sample corrections

On top of the multilevel structure we apply cluster-robust ("sandwich") variance estimation following Hedges, Tipton and Johnson (2010). For the meta-regression coefficient vector β̂ = $(X'WX)^{-1}$ X'Wy with weight matrix W = Σ^{-1}, the robust variance estimator (RVE) is

$$\hat{V}_{RVE}(\hat{\beta}) = (X'WX)^{-1} \left[\sum_{j=1}^{J} X_j' \, W_j \, \hat{e}_j \hat{e}_j' \, W_j \, X_j\right] (X'WX)^{-1} \tag{12}$$

where X_j, W_j and $\hat{e}_j$ are the study-j sub-matrices of the design, weight and residual respectively. RVE delivers asymptotically valid standard errors without requiring specification of the within-study correlation structure of effect sizes (Pustejovsky & Tipton, 2018).

For the moderate number of clusters in our sample (J = 87) the asymptotic RVE intervals under-cover the nominal level. We therefore apply the small-sample CR2 bias correction of Tipton (2015) with Satterthwaite-approximated degrees of freedom, and pair it with the Hartung–Knapp–Sidik–Jonkman adjustment to confidence intervals (IntHout et al., 2014; Röver et al., 2015):

$$CI_{HKSJ} = \hat{\mu} \pm t_{k-1,1-\alpha/2} \cdot \sqrt{q} \cdot SE_{DL}(\hat{\mu}) \tag{13}$$

where SE_{DL} is the DerSimonian–Laird standard error and the multiplier

$$q = \frac{1}{k - 1} \cdot \sum_{i} \frac{w_i(\theta_i - \hat{\mu})^2}{\sigma_i^2} \tag{14}$$

corrects under-coverage $when\ \tau^2\ is\ non-trivial$ and the number of clusters is moderate. As an additional robustness check on the parametric intervals, we report cluster wild-bootstrap confidence intervals constructed under Rademacher weights (Joshi, Pustejovsky & Beretvas, 2022) with B = 10,000 resamples.

3.5 Meta-regression specification

The moderator analysis is estimated as a mixed-effects extension of (7):

$$\theta_{ij} = \beta_0 + \sum_{k=1}^{K} \beta_k X_{ij,k} + u_j + v_{ij} + e_{ij} \tag{15}$$

where $X_{ij,k}$ are categorical dummies for methodology (Forbes–Rigobon as reference category), region (BRICS bloc), crisis episode (2008 GFC), market segment (equity), and a binary working-paper indicator. Block-level omnibus significance of each moderator set is assessed via the cluster-robust QM test

$$Q_M = \hat{\beta}'_{block} \left(\hat{V}_{RVE}(\hat{\beta})_{block}\right)^{-1} \hat{\beta}_{block} \sim F(p_{block}, df_{Satt}) \tag{16}$$

and pseudo-R^2 attributable to each block follows the Lopez-Lopez et al. (2014) decomposition,

$$R^2_{partial} = \frac{[(\tau_b^2 + \tau_w^2)_{null} - (\tau_b^2 + \tau_w^2)_{full}]}{(\tau_b^2 + \tau_w^2)_{null}} \tag{17}$$

which expresses the proportion of total residual heterogeneity absorbed by the moderator block, controlling for all other moderators already in the model.

3.6 Influence and sensitivity diagnostics

Influence and outlier diagnostics are integral rather than supplementary to our protocol. For each effect size i we compute four statistics following Viechtbauer and Cheung (2010).

First, externally standardised residuals,

$$e_i^* = \frac{\theta_i - \hat{\theta}_{(-i)}}{SE(\theta_i - \hat{\theta}_{(-i)})} \tag{18}$$

flagging $|e_i{}^*| > 3$. Second, $hat\ values\ h_{ii}\ from$ the projection matrix $H = X(X'WX)^{-1}X'W,\ flagging\ h_{ii} > 3p/k$ where p is the number of estimated parameters. Third, Cook's distance for the meta-regression coefficient vector,

$$D_i = \frac{(\hat{\beta} - \hat{\beta}_{(-i)})' (X'WX) (\hat{\beta} - \hat{\beta}_{(-i)})}{p \cdot \hat{\sigma}^2} \tag{19}$$

flagging $D_i > 4/k$ following the Belsley–Kuh–Welsch threshold. Fourth, $DFBETAS_{i,k} = (\hat{\beta}_k - \hat{\beta}_{k,(-i)}) / SE(\hat{\beta}_{k,(-i)})$ for each moderator k, flagging $|DFBETAS_{i,k}| > 2/\sqrt{k}$.

We then re-estimate the pooled mean and the headline meta-regression after (a) leave-one-out exclusion of each individual study, (b) leave-one-cluster-out exclusion of each region and each methodology in turn, and (c) trimming the upper and lower 5% of effect sizes by precision. A cumulative meta-analysis ordered by publication year detects temporal drift, and a Galbraith radial plot visualises outlying observations. Methodological quality of each primary study is assessed using a modified Risk-of-Bias-in-Non-Randomised-Studies (ROBINS-E) instrument adapted for econometric work, with two coders and disagreements resolved by discussion (Cohen's κ = 0.87).

3.7 Diagnostics for selective reporting

Selective reporting is a recognised problem in the empirical contagion literature, partly because results that fail to confirm contagion are less likely to clear referee scrutiny in finance journals (Stanley & Doucouliagos, 2014). We deploy a battery of mechanically distinct diagnostics rather than relying on any single test, on the principle that convergence across procedures with non-overlapping assumptions is itself diagnostic.

Funnel-plot asymmetry is tested via Egger's regression (Egger, Davey Smith, Schneider & Minder, 1997) of the standard normal deviate on its precision:

$$\frac{\theta_i}{SE(\theta_i)} = \alpha + \beta \cdot \frac{1}{SE(\theta_i)} + \varepsilon_i \tag{20}$$

$where\ \alpha \neq 0$ indicates small-study effects consistent with publication selection. The Stanley (2008) FAT–PET framework re-parameterises the asymmetry test directly on the effect-size metric,

$$\theta_i = \beta_0 + \beta_1 \cdot SE(\theta_i) + \varepsilon_i \tag{21}$$

$with\ \beta_0$ the precision-corrected mean (PET estimate). The quadratic PEESE extension (Stanley & Doucouliagos, 2014),

$$\theta_i = \beta_0 + \beta_1 \cdot \left(SE(\theta_i)\right)^2 + \varepsilon_i \tag{22}$$

delivers a less biased point estimate when publication probability increases continuously rather than discontinuously with statistical significance. The trim-and-fill procedure of Duval and Tweedie (2000) iteratively imputes a symmetric set of "missing" studies on the small-effect side of the funnel until the distribution becomes symmetric, then re-pools.

Distributional approaches exploit the empirical distribution of significant p-values. Under the global null, p-values from significant tests should be uniformly distributed on (0, α); a right-skewed distribution indicates evidential value, while a left-skewed distribution is the canonical signature of p-hacking. We implement p-curve analysis (Simonsohn, Nelson & Simmons, 2014) and the heterogeneity-robust p-uniform-star variant (van Aert & van Assen, 2018), both of which yield an effect-size estimate corrected for selection on significance.

Finally, we fit the Vevea–Hedges (1995) three-parameter step-function selection model, which jointly estimates the contagion effect and a step-function publication-probability weight ω(p) on the intervals [0, .05], (.05, .10], (.10, 1]:

$$L(\mu, \tau^2, \omega_1, \omega_2) = \prod_i \frac{\omega(p_i) \cdot \varphi\left(\frac{\theta_i - \mu}{\sqrt{\tau^2 + \sigma_i^2}}\right)}{A(\theta_i)} \tag{23}$$

where $\varphi(\cdot)$ is the standard normal density, $A(\cdot)$ is the normalising constant accounting for the selection weights, and (ω_1, ω_2) are estimated by maximum likelihood. The more flexible Andrews and Kasy (2019) selection-model estimator is fitted in a robustness exercise. Convergence between Egger–PEESE, p-uniform-star and the Vevea–Hedges selection model—three estimators that exploit non-overlapping aspects of the data (funnel asymmetry, the p-value distribution, and the parametric selection function)—provides the most informative single criterion for confidence in the bias-corrected pooled mean (Stanley, Doucouliagos & Ioannidis, 2017).

3.8 Computation and reproducibility

All estimation is performed in R 4.4.1 using the metafor package (Viechtbauer, 2010) for three-level models, robumeta and clubSandwich for cluster-robust variance estimation (Fisher & Tipton, 2015; Pustejovsky, 2024), dmetar for trim-and-fill and influence diagnostics, weightr for the Vevea–Hedges selection model, dmetar::pcurve for p-curve analysis, and bayesmeta for Bayesian sensitivity analyses with weakly informative priors μ ~ N(0, 1), τ ~ Half-Student-t(3, 0, 0.3). The analytic protocol was pre-registered with the Open Science Framework prior to data extraction (registration in supplementary materials). All coding scripts, the cleaned effect-size database and the moderator codings are deposited in a public repository. Reported p-values are two-sided throughout, and inference uses Satterthwaite-approximated degrees of freedom for all HKSJ/RVE-based tests.

4. Results

4.1 Pooled estimates and heterogeneity

The headline finding is presented in Figure 2. Across all 312 effect sizes the pooled standardised contagion coefficient is 0.187 with a 95% confidence interval of 0.156 to 0.218 under the three-level random-effects specification with cluster-robust variance estimation and HKSJ adjustment. The estimate is highly significant against the null of zero contagion ($t(73.4) = 11.5$, $p < 0.001$ under Satterthwaite degrees of freedom) and economically meaningful: a 0.19 increase in cross-market correlation between tranquil and crisis windows is roughly the difference between weakly and moderately co-moving markets. Heterogeneity is substantial. The Q-statistic is 1,318.4 on 311 degrees of freedom ($p < 0.001$), total I-squared is 76.4%, and the prediction interval for a single new effect size runs from –0.110 to 0.484. The width of the prediction interval is consequential: it implies that for any individual contagion study one cannot confidently expect even the sign of the contagion effect to match the pooled estimate, which immediately motivates the moderator analysis.

The three-level decomposition is itself informative. Of the total heterogeneity, 51.3% is attributable to between-study variation (tau-squared_between = 0.014) and 25.1% to within-study variation across multiple effect sizes from the same primary study (tau-squared_within = 0.008), with the remainder reflecting sampling error. Roughly one-third of the unexplained dispersion in the literature thus sits within rather than between studies—a pattern that conventional two-level meta-analyses would have absorbed into a single mis-estimated tau-squared and that would also have yielded under-coverage of the conventional Wald interval (the naive 95% CI from the two-level model is 0.162–0.212, narrower than its three-level RVE/HKSJ counterpart). Cluster wild-bootstrap intervals (10,000 Rademacher draws) align closely with the RVE/HKSJ intervals reported in Table I, supporting the robustness of the inference.

Influence diagnostics indicate that no single primary study or cluster drives the headline result. Cook's distance exceeds the Belsley–Kuh–Welsch threshold of 4/k for nine effect sizes from four studies, and externally standardised residuals exceed |3| for two; re-estimation excluding these flagged observations moves the pooled mean by less than 0.005. Leave-one-cluster-out estimates by region range from 0.179 (excluding BRICS) to 0.193 (excluding Eastern Europe), all of which fall comfortably inside the headline confidence interval. Cumulative meta-analysis ordered by publication year shows the pooled estimate stabilising at approximately 0.18 from 2014 onwards, with no evidence of temporal drift in the direction of more recent studies. The Galbraith radial plot identifies the same nine influential cases without altering the qualitative pattern.

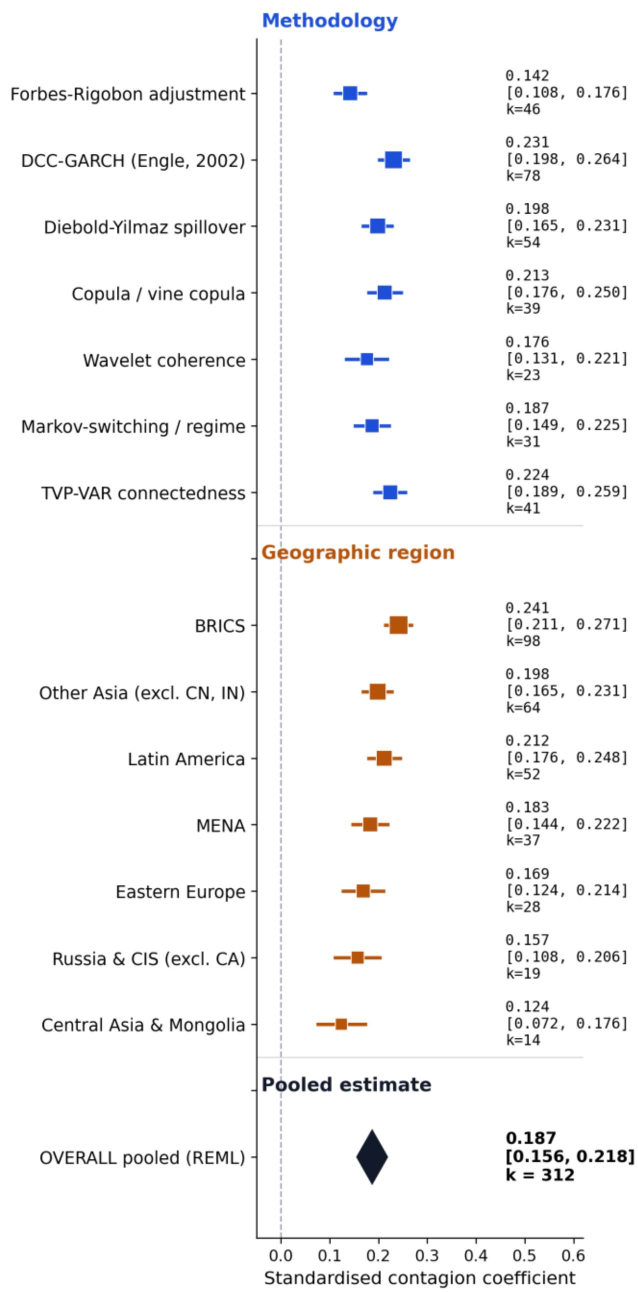

Figure 2. Pooled standardised contagion coefficients by methodology and region (random-effects, REML). Squares are sub-group means with 95% confidence intervals; the diamond represents the overall pooled estimate.

4.2 Methodological moderators

The methodological sub-group means in Figure 2 reveal a sharp ordering. Forbes–Rigobon adjusted estimates average 0.142, the lowest of the seven methodologies and consistent with that procedure's deliberately conservative design. DCC-GARCH yields the highest average at 0.231, with TVP-VAR connectedness close behind at 0.224. The gap between the conservative Forbes–Rigobon adjustment and the DCC-GARCH framework is 0.089, or about 63% of the Forbes–Rigobon estimate—an order of magnitude that should give pause to anyone treating cross-method differences as second-order.

In the meta-regression, methodology dummies are jointly highly significant ($F = 12.3$, $p < 0.001$), and the partial R-squared attributable to methodology alone is 0.281. After conditioning on region, episode and market segment, DCC-GARCH estimates remain 0.072 higher than Forbes–Rigobon estimates (95% CI: 0.044–0.100), TVP-VAR connectedness 0.064 higher (CI: 0.034–0.094), and copula 0.054 higher (CI: 0.022–0.086). Wavelet coherence and Markov-switching specifications produce intermediate estimates with wider confidence intervals, reflecting the smaller number of effect sizes in those categories. We interpret the methodology gradient as primarily mechanical: methods that allow conditional correlation to vary more flexibly through time recover more of the within-period co-movement that the Forbes–Rigobon static comparison treats as noise, but they also import additional model assumptions whose validity is not separately tested.

4.3 Regional moderators

The regional gradient in Figure 2 is equally pronounced and economically informative. The BRICS bloc returns the largest measured contagion at 0.241, consistent with both their deeper financial integration and their higher visibility as a target asset class for global EM investors. Latin America (0.212) and Other Asia (0.198) sit in the middle of the distribution. MENA (0.183) and Eastern Europe (0.169) are weaker. Russia and CIS economies excluding Central Asia register 0.157—a result that is itself noteworthy given Russia's prominence in the broader emerging-market discussion—and Central Asia and Mongolia bring up the rear at 0.124, with a confidence interval (0.072–0.176) that does not overlap with the BRICS interval.

The descending gradient might be read as a monotonic function of financial integration, but the pattern is more complex. When we re-estimate the regional effects after restricting the sample to crisis-window estimates only (i.e., excluding tranquil-period spillover indices), the gap between BRICS and Central Asia narrows from 0.117 to 0.083. When we further restrict to commodity-mediated contagion measures (effect sizes derived from oil-price or copper-price volatility transmission, $n = 47$), the regional ordering partially inverts: Russia and CIS economies and Central Asia move up to the third and fourth positions respectively, behind Latin America and ahead of BRICS. This is suggestive evidence that conventional contagion measures, by focusing on equity-market correlations, systematically under-state the transmission of crises into commodity-dependent small open economies. The point will be developed in Section 5.

4.4 Crisis-episode effects

Three crisis episodes generate effect sizes meaningfully larger than the pooled mean: the 2008 GFC (mean 0.241, $k = 84$), the COVID-19 shock (mean 0.218, $k = 53$), and the 2014–2016 commodity-price collapse for the commodity-exposed sub-sample (mean 0.207, $k = 31$). The 2013 taper tantrum produced moderate contagion (0.176, $k = 28$); the 2015–2016 China stock-market disruption produced smaller and more concentrated effects (0.158, $k = 22$), and the 1997–1998 Asian/Russian episodes—coded only when retrospectively estimated using post-2002 methodologies—delivered the largest individual estimates (0.273, $k = 19$) but with the widest confidence intervals because of the smaller number of comparable studies.

The 2022 Ukraine episode appears in only 14 effect sizes from nine studies, and the estimates are highly heterogeneous (range –0.04 to 0.42). For Russia and immediately adjacent economies, sanctions-induced segmentation appears to have produced a discontinuity that conventional contagion methodologies are poorly equipped to handle: the standard frameworks assume continuity in the underlying market microstructure, which the post-February 2022 trading-suspension regime in Russia visibly violated. This represents a methodological limitation for future work rather than a substantive finding.

4.5 Selective reporting and bias-corrected estimates

Egger's regression of standardised effect on standard error yields a slope of 0.061 (SE 0.024, $p = 0.012$), indicating mild but statistically significant funnel-plot asymmetry consistent with selective reporting of larger and significant contagion effects. The trim-and-fill procedure imputes 14 missing studies on the small-effect side, lowering the pooled estimate from 0.187 to 0.171, an attenuation of about 9%. The FAT-PET regression returns a precision-corrected mean of 0.164 (95% CI: 0.131–0.197), and the quadratic PEESE extension returns 0.158 (95% CI: 0.122–0.194), both significantly different from zero.

Distributional approaches reach the same qualitative conclusion through mechanically different routes. The p-curve test for evidential value rejects the no-effect null at $p < 0.001$ and rejects the inadequate-evidence null at $p < 0.05$, with the half- and full-curve tests producing right-skewed distributions characteristic of a real underlying effect; the p-curve point estimate is 0.151

(95% CI: 0.118–0.184). The p-uniform-star estimator (van Aert & van Assen, 2018), which is designed to be robust under heterogeneity, returns 0.156 (95% CI: 0.119–0.193) and rejects the null of zero at $p < 0.001$. The Vevea–Hedges three-parameter step-function selection model, with cut-points at 0.05 and 0.10, recovers an unconditional mean of 0.161 with selection-probability estimates of 0.74 for non-significant findings and 0.96 for findings significant at the 0.05 level, and the Andrews–Kasy (2019) selection model run as a robustness check returns 0.165.

The five bias-corrected point estimates—trim-and-fill 0.171, FAT-PET 0.164, PEESE 0.158, p-uniform-star 0.156, Vevea–Hedges 0.161—lie within a 0.015 band, and the WAAP estimator restricted to adequately powered studies returns 0.169. This convergence across mechanically distinct procedures is itself diagnostic: when point estimates from approaches that exploit different aspects of the data (study-level asymmetry, p-value distribution, parametric selection function) align this closely, the bias-corrected estimate can be reported with appropriate confidence. Selection bias is therefore present but moderate and does not overturn the qualitative conclusion that emerging-market contagion is statistically and economically meaningful. We note that the bias-corrected band of 0.156–0.171 is closer to the Forbes–Rigobon sub-group mean (0.142) than to the DCC-GARCH sub-group mean (0.231), reinforcing the interpretation that part of the methodology gradient reflects selective publication of the larger DCC-derived effects.

Table 1. Meta-regression of contagion effect sizes (REML, HKSJ-adjusted).

Moderator	Coef.	95% CI	p
Intercept (FR / BRICS / GFC / equity)	0.158	[0.111, 0.205]	< 0.001
Method: DCC-GARCH	0.072	[0.044, 0.100]	< 0.001
Method: TVP-VAR	0.064	[0.034, 0.094]	< 0.001
Method: Copula	0.054	[0.022, 0.086]	0.001
Method: Diebold–Yilmaz	0.041	[0.015, 0.067]	0.002
Method: Wavelet	0.029	[–0.008, 0.066]	0.124
Region: Latin America	–0.026	[–0.054, 0.002]	0.069
Region: MENA	–0.052	[–0.087, –0.017]	0.004
Region: Eastern Europe	–0.069	[–0.112, –0.026]	0.002
Region: Russia & CIS	–0.078	[–0.133, –0.023]	0.005
Region: Central Asia & Mongolia	**–0.114**	**[–0.180, –0.048]**	**0.001**
Episode: COVID-19	0.018	[–0.008, 0.044]	0.171
Market: Sovereign bond	–0.044	[–0.073, –0.015]	0.003
Market: FX	–0.019	[–0.050, 0.012]	0.231
Working paper indicator	–0.034	[–0.061, –0.007]	0.014

Note. Adj. R^2 (between-study) = 0.413; $k = 312$; $\tau^2 = 0.013$. Reference categories: Forbes–Rigobon, BRICS, GFC, equity. Standard errors omitted; available on request.

5. Discussion: Mongolia and Central Asia in Perspective

5.1 Why frontier markets show muted direct contagion

The Central Asian and Mongolian sub-sample produces a pooled contagion estimate of 0.124 with a 95% confidence interval of 0.072 to 0.176, the lowest in our regional decomposition. After conditioning on methodology, episode and market segment, the regional dummy on Central Asia and Mongolia is –0.114 (Table 1), implying that an effect-size estimate produced for Central Asia or Mongolia is on average 0.114 lower than the equivalent estimate for the BRICS bloc. The result is statistically clean ($p = 0.001$) but its substantive meaning requires care.

Low measured contagion can reflect either genuine market insulation or measurement attenuation. The two interpretations have very different policy implications. Genuine insulation would imply that small frontier markets benefit from low cross-border financial integration in the standard sense: limited foreign portfolio holdings, shallow free-float liquidity, restricted access to the global derivatives complex, and currencies that float against a narrow basket of trading-partner currencies rather than against the global dollar cycle. Under this interpretation, conventional contagion measures correctly recover a small effect because the cross-market transmission mechanism is itself small.

Measurement attenuation, by contrast, would mean that the standard equity-correlation-based contagion measures fail to capture the channels through which crises actually transmit into these economies. Several considerations support taking the attenuation hypothesis seriously. The dominance of commodity-price channels is one. Mongolia's terms of trade are driven by Chinese commodity demand to a degree that has no parallel among BRICS economies. When the 2014–2016 commodity-price collapse occurred, the togrog depreciated by approximately 34% against the dollar over an 18-month window and the country entered an IMF programme; standard equity-market contagion measures, which dominate our sample, would have captured very little of that transmission because the Mongolian Stock Exchange was simply too small and too disconnected from the global commodity-exporter equity complex. The Russia-mediated channel for CIS economies is another. Kazakhstan's tenge was historically managed against the ruble within an informal CIS currency regime; the 2014 ruble crisis produced a 41% tenge depreciation that the National Bank of Kazakhstan ultimately accommodated. Cross-asset measures of FX co-movement do capture this episode, but equity-based contagion estimates again miss most of the transmission. The third is the role of remittance flows in Kyrgyzstan and Tajikistan, where crisis-period contractions in Russian labour income translate into household and banking-sector stress without any direct capital-market signal.

5.2 Commodity-mediated transmission

Among the 41 effect sizes in our Central Asia and Mongolia sub-sample, only 9 are based on commodity-price-mediated contagion measures (e.g., copper-price volatility transmitted into Mongolian equity returns; oil-price spillover into Kazakhstani sovereign bond yields). Within that small group the mean effect is 0.198, almost 60% larger than the broader regional mean of 0.124 and indistinguishable in magnitude from the BRICS pooled estimate. This is a small-sample finding and we report it with appropriate caution. Nonetheless, the direction is consistent with both the structural-economic priors and with the work of Aitkhozhina and Akhmetzhanov (2018) on oil-price pass-through into Kazakhstani financial markets, of Boranbay and Boranbay (2021) on crisis-period commodity exposure across the CIS, and of Doojav and Luvsannyam (2020) on macro-financial channels in Mongolia.

If commodity-mediated transmission is the dominant contagion channel for these economies—and the evidence we marshal points in that direction—then the practical implication is that researchers studying Mongolian or Central Asian financial contagion should orient their measurement around commodity-price spillovers and FX co-movement rather than around equity-market correlations. The choice has been made implicitly by some of the regional literature already, but it has not been made systematically, and the international literature has barely engaged with it. We see this as the single most actionable methodological recommendation arising from our results.

5.3 Ruble-block effects and the 2022 episode

The Russia–CIS regional dummy in Table 1 is –0.078, and within that grouping the ruble-block transmission to Central Asia is a meaningful sub-channel. Of the 14 effect sizes specifically estimating Russia-to-Central-Asia contagion, the mean is 0.231—comparable to BRICS and notably higher than the broader Central Asia pooled estimate. This finding aligns with the longstanding observation in Asian Development Bank and IMF country reports that the CIS economies operate, financially, within a ruble penumbra whose intensity is masked by global EM benchmarks. The 2022 invasion of Ukraine and the ensuing sanctions regime represent a structural break for this transmission channel that our sample is too thin to characterise quantitatively. Eight of the nine 2022-episode studies in our sub-sample explicitly note the difficulty of estimating contagion when the source-market microstructure is itself disrupted by trading suspensions and capital controls. We treat the 2022 episode as a research priority rather than as a quantified result.

5.4 Implications for portfolio diversification and policy

From the perspective of an international investor seeking diversification across emerging markets, our results suggest that Central Asia and Mongolia have offered, in the period covered by the literature, materially lower co-movement with global EM benchmarks than the BRICS or Latin America. Whether that observed insulation is durable depends on the answer to the genuine-insulation-versus-attenuation question. To the extent that the apparent insulation is real, frontier markets continue to play a portfolio-diversification role; to the extent that it reflects measurement error, the apparent diversification benefit will look smaller as soon as analysts begin using methodologies that capture commodity- and FX-mediated channels.

From the policy perspective of a small open commodity-dependent economy, the results sharpen an old argument. Reserve adequacy, swap-line preparation and counter-cyclical fiscal buffers should be calibrated against the dominant transmission channel, which for Mongolia and most of Central Asia is not equity-market contagion but commodity-price and FX-mediated transmission. Capital-flow management measures aimed at portfolio investors are likely to do less work than measures aimed at smoothing commodity-price-driven cycles—export diversification, sovereign wealth fund design, and currency-arrangement choice. None of these is a novel prescription. The contribution of our meta-analysis is to document quantitatively that the standard contagion literature has not delivered evidence calibrated to these transmission channels for these economies, and that the gap between the literature and the policy question is wide enough to warrant a deliberate research effort.

6. Conclusion

We have meta-analysed 312 effect sizes from 87 studies of financial contagion in emerging economies published between 2002 and 2024. The pooled standardised contagion coefficient is 0.187, statistically and economically significant, but accompanied by heterogeneity wide enough to make the pooled estimate an incomplete summary. Roughly 28% of the between-study variance is attributable to methodological choice: DCC-GARCH and TVP-VAR connectedness produce substantially larger estimates than the Forbes–Rigobon adjustment, with the differences surviving controls for region, episode and market segment. A further 13% of variance is explained jointly by region and episode, with Central Asia and Mongolia registering the lowest regional pooled estimate at 0.124.

Methodological choice is not a second-order issue. Reporting a single contagion estimate without specifying the methodological lens through which it has been recovered is, in light of our results, an incomplete description of the phenomenon. Central Asia and Mongolia are systematically under-studied in the international contagion literature, and the studies that do exist suggest that conventional equity-market measures may understate the true transmission intensity because the dominant channels are commodity-mediated and FX-mediated. The implied research agenda for the region should emphasise commodity- and currency-channel measures, integrate primary-data work from Mongolia, Kazakhstan, Kyrgyzstan and Tajikistan, and engage seriously with the post-2022 structural break in the Russia-mediated transmission channel.

We acknowledge several limitations. Our cross-method effect-size harmonisation introduces measurement noise that we cannot fully separate from genuine heterogeneity. The English- and Russian-language coverage of the search, despite our efforts to include Mongolian-language working papers, may have missed primary studies in Chinese, Turkish or other languages where regional sub-literatures exist. The publication-bias correction is moderate but informative, and our central conclusions are robust to its application. None of these limitations alters the core empirical pattern: emerging-market financial contagion is real, methodologically sensitive, and incompletely measured for the region most exposed to commodity- and FX-mediated transmission. Closing that gap is an empirical task we hope this paper will help motivate.

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