

The Cost of Capital in Developing Countries

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The Cost of Capital in Developing Countries

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Abstract

Lowering the cost of capital is essential for the energy transition, energy security, and international development.^{1,2} Yet transaction-level evidence on financing costs in developing countries remains scarce, limiting the effective design and evaluation of policy.^{3,4} Here, we assemble and analyze the first large-scale dataset of cost of capital in emerging markets based on 845 infrastructure transactions obtained directly from 19 investors, spanning 66 developing countries and 15 sectors from 2009–2026. We use this dataset to make preliminary observations on variations in developing countries’ cost of capital, finding that borrowing costs differ substantially from prevailing estimates and that commonly used sovereign-risk measures provide relatively little explanatory power beyond currency, year and tenor controls. Private projects often borrow below their sovereigns, and development finance institutions appear to lend at higher rates than the private sector. We also find that as investors execute more transactions in a country they invest at lower costs, a pattern we term the “investor confidence effect.” These results imply that the high cost of capital faced by developing countries could in part be reduced by policies catalyzing investors’ initial projects in new markets.

Main

The cost of capital, i.e., the rate a firm or project must pay to borrow money, is among the most important determinants of global investment^{1,2}. In developing countries, where borrowing costs are high, the cost of capital often dictates whether and to where private capital flows^{3,4}. Many interventions designed to increase investment such as guarantees, subsidies, feed-in tariffs or blended finance operate by seeking to lower the cost of capital⁵. This is particularly important for the global energy transition, as clean energy projects have high up-front capital expenditures relative to fossil projects, increasing the influence of cost of capital on overall project viability^{6,7}.

Despite its importance, there is almost no observed transaction-level cost of capital data in developing countries. Instead, analyses rely on indirect determinations such as surveys, proxies or models, which offer important insights but pose significant limitations^{8–11}. Expert surveys, where market participants estimate the financing cost of a hypothetical project, are useful for policy discussions¹², but do not offer empirical or granular data on how borrowing costs vary. Proxies such as sovereign bond yields, credit default swaps (CDS) or credit ratings also offer useful insights^{7,13,14}, but many developing countries lack the necessary market liquidity for such measurements, which is why the most widely used proxy, Damodaran's country default spread, estimates many countries' CDS based on comparably rated governments^{15–17}. Sovereign proxies also cannot distinguish between public and private borrowers, even though their borrowing rates may differ significantly, especially for infrastructure projects^{1,18}. Models generally back out an estimate for firms' cost of capital from bids in electricity auctions¹⁹, but auctions are largely confined to solar and wind projects, and data remains rare outside high-income markets²⁰.

Several commercial datasets record transaction data on private investment (e.g., Bloomberg, InfraClear, IJGlobal/GreenStreet). However, while we identified 308 deals with some cost of capital data across the tens of thousands of transactions on these platforms, they lacked key necessary information about elements such as the presence of guarantees or other concessional measures (which would distort pricing), the identity of the lender and the project leverage. In addition, the vast majority predated 2019, limiting their usefulness.

In the most comprehensive literature review to date, from 2025, Steffen et al. identify 1,429 cost of capital datapoints in academic literature across 68 countries from 32 primary studies⁸. As they make clear, however, only eighteen of these were based on data elicited from transactions in developing countries, covering five projects drawn from two studies, all in India, with the remainder from surveys, listed-equity analysis and reverse-engineered auction results. Such meta-analyses are important to establish the existing baseline of cost of capital knowledge, and the authors note the importance of obtaining transaction data in the future. More widely, the need for more cost of capital data is widely acknowledged, and both the OECD and G20 have called for more transparency around developing countries' borrowing costs^{21,22}. Without empirical cost of capital data, policies intended to lower borrowing costs cannot be effectively evaluated, and

development finance institutions (DFIs) cannot verify that they are following their mandates to lend at the market rate rather than undercutting private investors.

The cost of capital in developing countries

This paper leverages a new dataset of 845 infrastructure transactions obtained directly from 19 investors, spanning 66 developing countries and 15 sectors over 2009–2026. Our primary outcome variable is the cost of debt, expressed as a USD-equivalent spread over the Secured Overnight Financing Rate (SOFR). We focus on infrastructure transactions because they are comparatively homogeneous and therefore comparable across geographies, allowing country and sector premia to be isolated more clearly than in general corporate borrowing^{24,25}.

The base model uses country, sector, currency and year fixed effects together with a control for debt tenor. To assess how this debt measure relates to overall cost of capital, we also examine WACC and cost of equity where available and re-estimate our models with leverage controls on the subset reporting capital structure. The base model explains 55% of the variation in observed spreads. On the subsample with capital structure data, adding leverage and investor-experience controls raises R^2 to 0.66. Country is the most important factor, followed by year, sector, currency and tenor. These descriptive decompositions, along with full coefficients and standard errors are reported in Supplementary Information.

Country premia

We estimate country-specific average spreads, which we call country premia, controlling for sector, currency, year and tenor. Among countries with more than one transaction, estimated country premia range from 215 bps (China) to 805 bps (Zimbabwe) (SI Table S3.2). The most expensive countries are overwhelmingly in Sub-Saharan Africa (525 bps against 390 to 518 bps elsewhere; SI Table S3.2), which is also the region with widest variation in spreads (Figure 1).

Figure 1A – Country premia (bps, 95% confidence intervals)

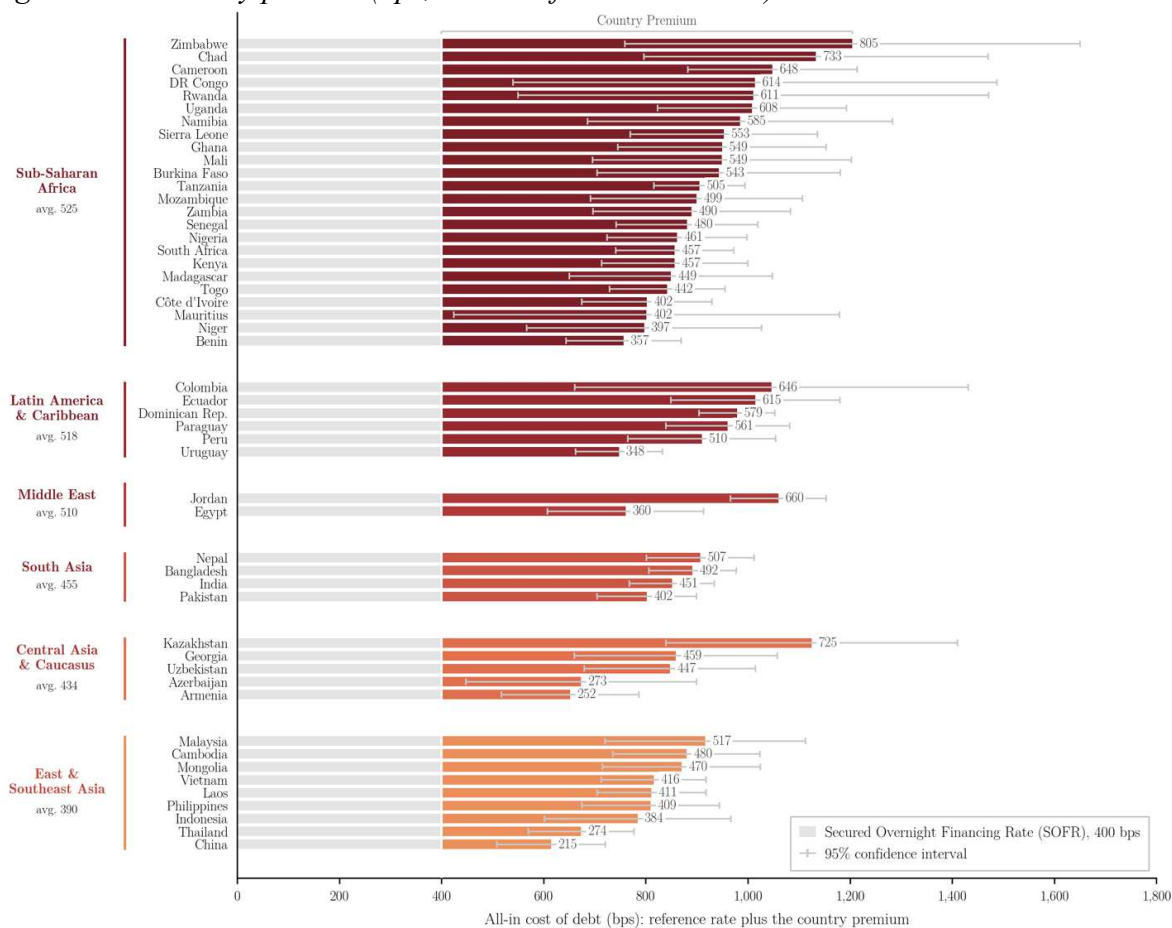


Figure 1B – Country premia (Map)

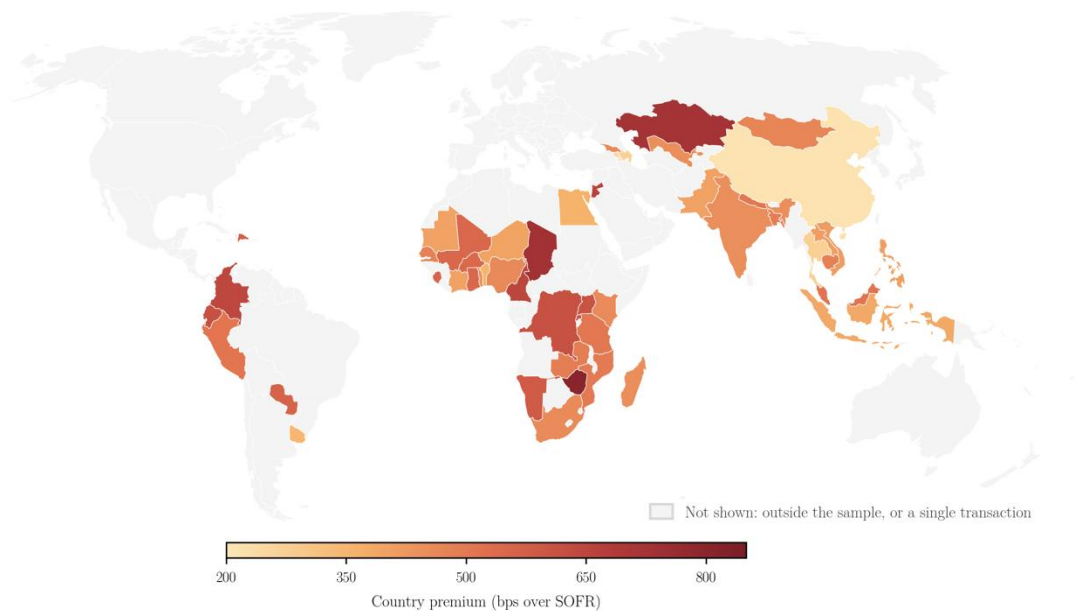


Fig. 1 - Country premia for the countries with more than one transaction (bps over SOFR), controlling for sector, currency, year and tenor, with 95% confidence intervals. Regional averages are unweighted means, including single-transaction countries.

Sector premia

Borrowing costs vary substantially across sectors. Relative to utility-scale solar, the reference category, off-grid solar is 367 bps higher ($p < 0.001$), agriculture is 123 bps higher ($p = 0.02$), and mining is 220 bps lower ($p = 0.04$). The remaining sectors lie within ~60 bps of utility-scale solar. Results are shown in *Figure 2* and should be interpreted as adjustments to the country premia, which use solar as a reference.

Figure 2 – Sector premia (bps, 95% confidence intervals)

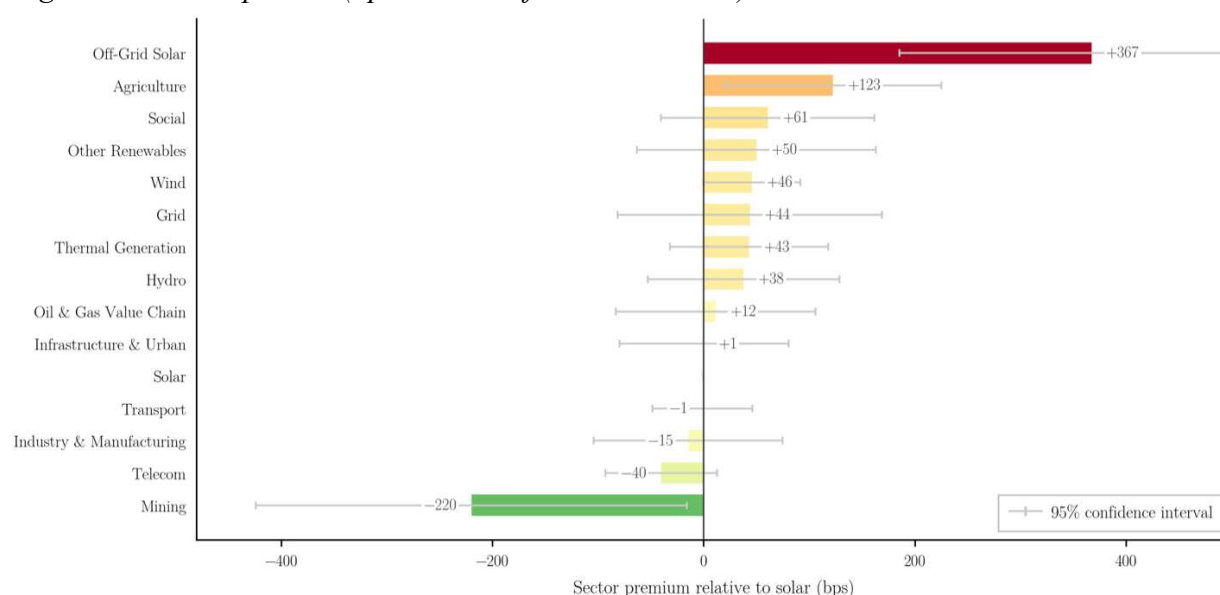


Fig. 2 - Estimated sector effects (bps) relative to utility-scale solar, the reference category, controlling for country, currency, year and tenor, with 95% confidence intervals. Estimates are adjustments to the country premia reported in Fig. 1.

Currency effect

Local-currency borrowing is associated with a USD-equivalent spread 105 bps lower than hard currency borrowing (USD or EUR), after conversion using cross-currency swap rates ($p < 0.001$; Extended Data Fig. 1). One possible interpretation is that investors add a premium for currency risk when revenues are predominantly in local currency but debt service is in a hard currency²⁶. However, differences in hedging availability, borrower selection and loan structure may also contribute.

Development finance institutions versus private lending

Accounting for country, sector, currency, year and tenor, we find that DFIs lend at 34 bps higher than the private sector, though this is not statistically significant ($p = 0.41$). The result is unchanged under clustering by country ($p = 0.66$), investor ($p = 0.67$), sector ($p = 0.31$) or two-way ($p = 0.71$; SI Table S5.2)²⁷. Restricted to hard-currency transactions the differential rises to 197 bps ($p = 0.05$), and on transactions reporting capital structure to 199 bps ($p < 0.01$) (SI Tables S5.5 and S10.3).

DFI lending also differs in structure from private investors: DFI loans have a median project size of USD 60 million against USD 4.6 million for private investors, roughly thirteen times larger, and are 2.5 years longer in tenor ($p < 0.001$; Extended Data Fig. 2).

Year effects

Observed spreads fell overall by roughly 200 bps during the last decade, but the decline is not uniform: spreads were broadly flat from 2015 to 2019, averaging 486 bps, and rose during the COVID period to an average of 542 bps in 2020 and 2021 (Figure 3). Since COVID, observed spreads have fallen, averaging 359 bps from 2022 to 2026 and reaching 293 bps in 2026.

Figure 3 – Year effects (bps, 95% confidence interval)

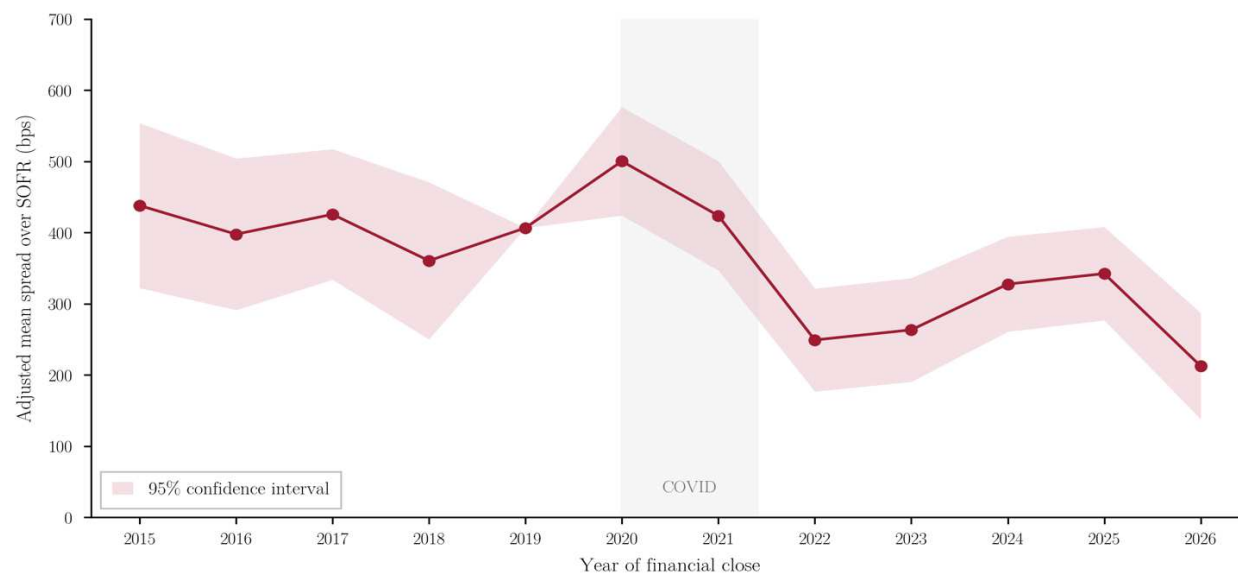


Fig. 3 - Year effects show adjusted mean spread over SOFR (bps) by year of financial close. Each point is the intercept plus that year's fixed effect.

Comparison to existing estimates of cost of capital and other macro indicators

We regress observed spreads against a wide range of potential country-level proxies to understand correlation. Established measures of country risk such as sovereign CDS/default spreads and credit ratings add relatively little explanatory power beyond currency, year and tenor controls (Table S6.1): Damodaran's default spread and Moody's sovereign rating add incremental R^2 of 0.004 and 0.012, respectively. Among the 29 indicators tested individually, broad money as a share of GDP

(incremental $R^2 = 0.029$), energy imports (0.022) and domestic credit to the private sector (0.019) have the largest, though still modest, incremental R^2 values, followed by electrification (0.013), inflation (0.011), reserves in months of imports (0.011), government effectiveness (0.011) and rural electrification (0.010)^{28–32}. A five-fold cross-validation exercise gives broadly similar predictive performance (SI Table S6.2 and Extended Data Fig. 3). With none of the 29 indicators examined having material explanatory power, we interpret the ordering as evidence of a generally low correlation between macroeconomic indicators and observed borrowing costs, rather than that any indicator is a causal determinant of financing costs.

We find that infrastructure projects frequently have observed spreads below the corresponding Damodaran country default spread.¹⁸ Across the sample, 46% of transactions price below that benchmark, and in 51 of the 61 countries covered by both datasets at least one transaction does so. Among hard-currency deals, 54% price below the Damodaran default spread, and 39 of 56 countries have at least one such deal (SI Table S6.3).

Investor effects

We find a significant association between the number of deals an investor has executed in a market and lower spreads on their loans. Compared to the average first transaction in the market, each additional investment is associated with an approximately 50 bps reduction in spreads, plateauing at 218 bps cheaper from the fifth deal onwards (Figure 4). Pooling investors' fourth transactions onward and comparing them to earlier deals using a binary threshold gives a 157 bps reduction for later deals ($p < 0.001$). With investor fixed effects, the threshold estimate is 103 bps ($p < 0.001$) and the fifth-deal estimate 118 bps ($p < 0.05$). With investor-country pair fixed effects, the threshold effect is 64 bps ($p = 0.12$). The effect strengthens when leverage is added to the fixed-effect specifications (SI Table S5.4b).

We term the association between the number of deals an investor has executed in a market and reduced spreads the “investor confidence effect,” and our results are consistent with, but do not yet establish, a mechanism whereby investors reduce spreads as they gain confidence in a market. For context, the 218 bps reduction in spreads between first and fifth deals is substantially greater than the gap between the most expensive region (Sub-Saharan Africa, 525 bps) and the cheapest (Southeast & East Asia, 390 bps).

We conduct a series of sensitivity checks around the investor confidence effect, using the average spread specification as a base. First, we find the effect to be independent of cumulative investment in a market: the World Bank PPI database count is not significantly related to spreads ($p = 0.32$), and the investor-experience estimate is similar when the PPI count is included (–160 bps, $p < 0.001$)³². The effect is also robust to clustering by country, investor, sector, or both (SI Table S5.2). Dropping any single investor or any country leaves the effect coefficient between –105 and –261 bps (significant at $p < 0.01$ in all 82 re-estimations; SI Table S5.9). Estimated separately on types

of investor, spreads from the fourth deal onwards are 242 bps lower for private investors ($p < 0.001$) against 108 bps for DFIs ($p = 0.053$), which we may expect given many public lenders have specific mandates to lend in developing countries.

Figure 4 – The investor confidence effect (bps, 95% confidence intervals)

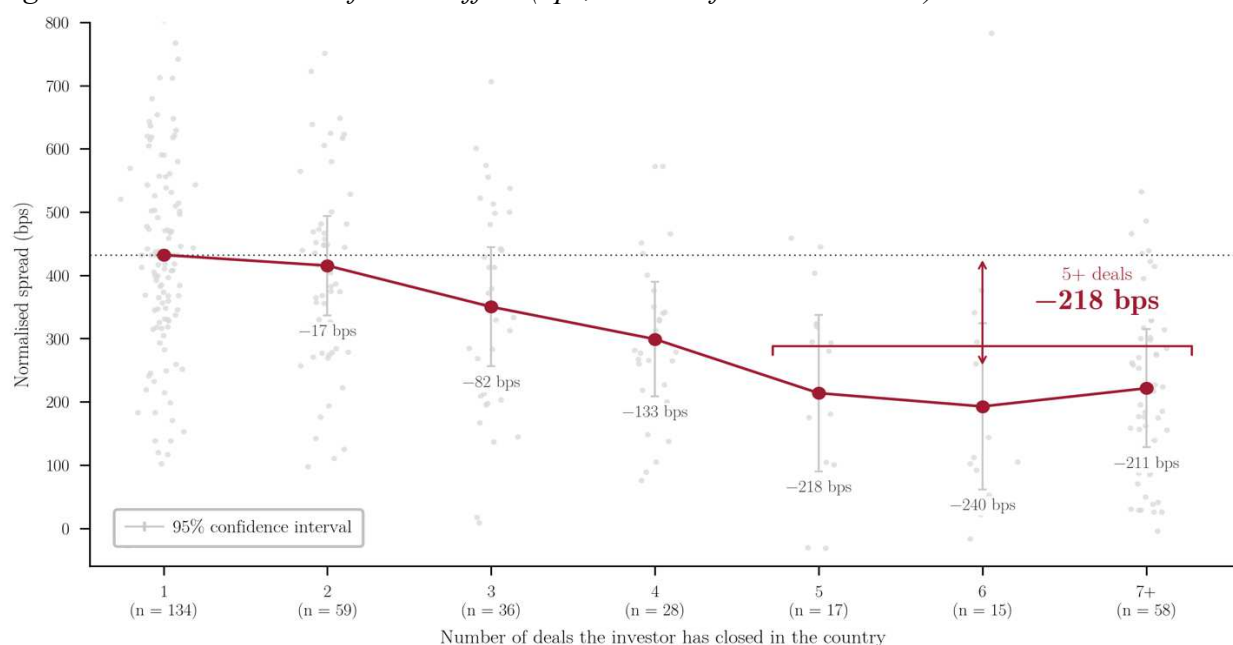


Fig. 4 - The investor confidence effect. Adjusted mean spread over SOFR (bps) by the number of transactions the investor had previously closed in that country, controlling for country, sector, currency, year and tenor, with 95% confidence intervals. Estimated on the subsample that excludes national development banks lending only in their home market.

Cost of equity and leverage

Cost of equity in our sample is a median multiple of 1.98x cost of debt, estimated on the 91 transactions where both are reported. The middle half of transactions fall between 1.5x and 2.6x, and the tenth and ninetieth percentiles are 1.3x and 3.1x. We find no systematic relationship between the multiple and country risk (Extended Data Fig. 5). WACC falls with leverage (0.41pp per 10pp, $p < 0.001$, tested on the 226 transactions reporting both) and leverage is higher in countries with higher country premia (on the 207 transactions that report both capital structure and cost of debt, leverage rises by 3.0pp per 100 bps of country premium, $p = 0.10$, and by 4.3pp when each investor is compared only with itself, $p = 0.02$), contrary to the traditional trade-off prediction that risk reduces optimal leverage, though consistent with findings that firms in weaker-institution countries use more debt and less equity^{33,34,35}.

To examine whether debt-side results are sensitive to capital structure, we add leverage as a control to the base model on the 317 transactions with available data. On this subsample, the declining time trend holds, the DFI differential rises and the base investor confidence effect is essentially

unchanged by the leverage control (from -159 to -160 bps, $p < 0.001$). The additional investor confidence effect specifications increase modestly when leverage is included in the model with investor controls (-132 bps, $p < 0.001$) and with investor-country controls (-76 bps, $p = 0.10$). Overall, the model's R^2 rises to 0.59 with a leverage control and to 0.66 after adding investor-transaction controls on the 279 transactions in the subsample.

The investor confidence effect reproduces when the reported WACC is used as the outcome in place of the cost of debt (-160 bps against -157 bps). Currency and year effects are not comparable across these outcomes, because the reported WACC is the currency of the transaction and is not converted (SI Table S10.4).

The investor confidence effect also appears when using cost of equity instead of cost of debt. Within the 130 transactions reporting cost of equity, of which 42 are investors' fourth or later transactions in a country, the threshold specification gives a 222 bps reduction after a fourth deal ($p = 0.15$) versus 157 bps on cost of debt, consistent with a constant cost-of-equity-to-debt multiple.

Limitations

Our analysis has several limitations. The first is possible selection bias: our sample is drawn from investors willing and able to share data. Participating institutions are therefore not a random draw of investors, and organizations willing to share data may differ from those that are not. The contributors span multilateral and bilateral development banks, national development banks, local and international commercial banks, and private funds, but this breadth does not completely remove the possibility of systematic selection.

Second, our cost of debt measure captures a loan's interest rate but not fees, grace periods, or differences in repayment structure. This means that two loans could theoretically have the same reported cost of debt but different effective borrowing rates. In discussion, our data providers indicated that these extra costs are unlikely to correlate systematically by country or sector, so in our view these factors may add noise to our premia but are unlikely to introduce bias to our conclusions. Nonetheless, we cannot establish from the available data that these omitted terms are unrelated to country, investor type or experience.

Third, the country premia are averages over the sample period, controlling for year, and so do not capture within-country change over time. Our specification also treats each country's premium as constant across sectors, and each sector's premium as constant across countries, whereas in practice the two are likely to interact (e.g., the premium for solar versus mining in Kenya may not equal that in India). This limitation could be dealt with in future by determining region or country-specific premia via two-way fixed effects once enough data has been collected.

Finally, several causal pathways could generate the same investor confidence effect that we observe, not only investors gaining confidence from repeated exposure or successful deals. It may be, for instance, that certain investors both systematically price lower and are more likely to execute repeat deals (e.g., because they have a mandate to do so), or that certain investors have markets where they both lend repeatedly and lend cheaply (e.g., because of market-specific advantages such as local funding or a local presence). In addition, later projects may differ systematically from earlier ones, for instance as investors gain ability to distinguish between projects in markets where they are more familiar.

We address some of these alternatives by adding investor fixed effects and investor-country pair fixed effects, under which the association attenuates from -157 bps in the baseline threshold specification to -103 bps with investor fixed effects and -64 bps with investor-country pair fixed effects. Nonetheless, using the current data we cannot establish a causal relation between investor in-market experience and lower spreads.

We note that any incompleteness in a lender's deal history would work against the investor confidence effect; if a lender has unreported deals in a country, we will incorrectly classify repeat deals as first deals, diminishing the differential. This could also in part explain the attenuation under the investor-control specifications: a relationship whose early deals predate our records would incorrectly be classified as a flat price path, and with only 25 of the 125 investor-country pairs containing a fourth deal or later (SI Table S5.4), each pairing carries substantial weight.

What determines cost of capital?

Our results provide several insights into the cost of capital in developing countries. First, it is notable how large a proportion of spread variation is accounted for by a small number of high-level variables. Our base model has an R^2 of 0.55, meaning that country, sector, currency, year and debt tenor account for over half of spread variation. Adding controls for leverage and for the number of deals an investor has executed in a market increases R^2 to 0.66 on the 279 transactions that report a capital structure. While this should not be interpreted as a clean partition between “macro” and “project-specific” as some omitted project characteristics may be correlated with included variables, the results nevertheless show that a relatively compact set of high-level characteristics captures a substantial share of the dispersion in debt spreads despite not addressing measurement error or factors such as creditworthiness of the borrower, the experience of the team, the strength of contract and off-taker, etc. One potential explanation is investors showing relatively little differentiation between projects in emerging markets, relying instead on more general factors such as country. Our data does not elucidate the causal reasons for this pattern, for instance whether projects are seen as equally risky due to geography or because only the uniformly best projects can access capital.

Second, we note the importance of country premia in private sector borrowing costs. The country-only model explains 31% of variation - more than half the full base model R^2 of 55% (SI Table S4.1) - and the remaining fixed effects generally relate to transaction structure rather than credit risk. Year effects capture macroeconomic conditions common to all deals. Currency effects reflect transaction denomination rather than creditworthiness. Debt tenor likewise reflects transaction structure, not credit risk. Sector effects, while indicative of project risk, may also reflect other structural factors of the transaction: mining projects likely have strong off-takes, possibly in USD, or benefit from implicit government support, while agriculture and off-grid solar projects aggregate small borrowers into a single transaction, likely paying in local currency. Once these transaction-structure factors are set aside, variation in spreads can largely be explained by country, which may suggest, again, the predominance of geography in investor pricing.

Third, despite the importance of country premia, standard measures of country risk perform poorly as predictors of spreads. Sovereign ratings are often cited as a starting point for how investors price country risk, yet while regressions of both Moody's sovereign rating and Damodaran's default spread against observed borrowing costs show minor correlation, each adds almost no additional explanatory power beyond currency, year and tenor (and less than macro variables such as broad money, energy imports and domestic credit to the private sector). Likewise, 46% of transactions have spreads below their Damodaran sovereign-risk benchmark, rising to 54% among hard-currency deals, implying that a substantial share borrow below their sovereign^{18,36,37}. While notable, this comparison should not be interpreted strictly as a “sovereign ceiling” being violated: Damodaran’s spread is a proxy for sovereign credit risk, whereas project loans may differ from sovereign obligations in collateral, seniority, guarantees, contractual protections, revenue structure, currency and recovery prospects. The benchmark comparison is therefore further evidence that project-finance pricing is not mechanically bounded by sovereign-risk measures.

Conversations with investors who contributed data to this paper suggest one potential unifying explanation for these patterns. They noted that infrastructure projects provide essential services that citizens depend on, meaning the political economy of such loans differs from sovereign bonds. Similarly, when discussing the low correlation between cost of capital and sovereign risk measures, investors told us that their assessment of country risk is heavily informed by direct on-the-ground experience, including project due diligence, time spent in the market, and the success of prior transactions. If pricing rests on experiential knowledge rather than on ratings, indicators such as the depth of the banking system and reliance on imported energy need not be the direct causes of borrowing costs to predict them more accurately than standard credit metrics. Development indicators may instead proxy broader conditions that investors observe directly such as the capacity of citizens to pay their bills or the physical reliability of operations in a country.

The investor confidence effect

Many of the investors who contributed data to this paper described that their investment committees become more confident in markets where they have already executed transactions, resulting in lower pricing. They suggested that this familiarity can arise both from accumulated knowledge of regulatory processes and local counterparties, as well as from information about whether prior projects performed as expected. We interpret this as a potential pathway for the “investor confidence effect” we observe in our data, and we describe any causal process where increased familiarity with a market lowers spreads as “breaking the ice”.

The investor confidence effect has some basis in the literature on relationship banking, which shows that repeated interaction between investors and specific borrowers lowers borrowing costs^{38–40}. Bharath et al., for instance, find that repeated lending reduces spreads by 10 to 17 bps. However, while the corporate lending literature studies firm-specific relationships, in our data, the reduction in spreads operates at the country level: an investor with more deals in a country has, on average, significantly lower spreads on subsequent deals, regardless of borrower (or sector).

The investor confidence effect may help understand one of the most striking patterns in our data: the high cost of capital in Sub-Saharan Africa. In the sample used for the investor confidence analysis, the average Sub-Saharan African country premium is 520 bps versus 445 bps elsewhere, a 75 bps difference. At the same time, African lending relationships are also thinner: 44% of Sub-Saharan African deals are an investor's first transaction in that country versus 33% elsewhere (Extended Data Fig. 4). Controlling for the number of deals an investor has completed in each country, the average Sub-Saharan African premium falls to 450 bps and the average elsewhere to 429 bps, narrowing the gap from 75 to 21 bps (SI Table S5.6).

Policy implications

Our results identify a descriptive pattern that could suggest a novel pathway to achieve lower borrowing costs in developing countries: catalyzing specific investors’ first transactions in new markets in order to “break the ice.” While the current analysis does not establish that catalyzing an investor’s first transactions would cause later rates to fall, if the investor confidence effect operates causally, then supporting an investor through a limited number of transactions in a new market could result in a substantial permanent lowering of the rates that that investor offers. Efforts directed at ensuring early investments could therefore have a multiplier effect that would outlast the original policy without further public cost.

If future causal evidence supports an investor-learning mechanism, we see several policy instruments that could be evaluated. First, providing an investor with full or partial guarantees for a limited number of initial projects in new markets could provide country-exposure while not requiring them to assume excessive local risk. Second, blended finance instruments – i.e., concessional loans or grants that allow projects to pay market rates to commercial investors while achieving an overall lower cost of capital – could be targeted at investors’ first transactions in new

markets. Third, governments or DFIs could take junior tranches or anchor stakes in investment funds on the condition that the fund deploy into markets in which it has not previously invested.

The subsidies and de-risking mechanisms above need more study and careful design, both for effectiveness and to avoid wasting public money. The current data cannot establish whether lower spreads on later transactions arise from successful prior projects, accumulated information, changes in market conditions, or selection into repeat transactions. A useful next step would be to test whether investors who eventually make repeated investments already price their first observed transaction differently from investors who never return. Any facilities designed to break the ice would need to guard against gaming, whereby investors take initial credit support with no intention of returning to the market⁴¹. More generally, since the investor confidence effect may derive at least in part from deals succeeding rather than from the act of investing alone, catalyzing initial investment is not a substitute for the fundamental work of reducing risk in these markets but a complement to it, directed at the portion of the cost of capital that reflects unfamiliarity.

Non-financial mechanisms could also be studied to leverage the investor confidence effect without public subsidy. Support for early-stage project development could increase the pipeline of investable transactions, without which investors cannot accumulate experience. Efforts to encourage data-sharing such as standardized reporting of project costs of capital and realized returns could make one institution's experience available to others, as presently we find that one investor's record does not impact the pricing of another. Neither the number of transactions our other contributors have already closed in a country nor the number of infrastructure projects in the World Bank PPI database is significantly related to spreads (+1.2 bps per deal, $p = 0.20$, and +1.2 bps per project, $p = 0.32$, respectively, SI Table S5.8).

The cost of capital will shape developing countries' energy systems for decades to come. This makes the clean energy transition, as well as ensuring stable global growth and addressing international development, dependent on lowering these economies' borrowing costs. Our results, based on the first transaction-level observations of cost of capital around the world, suggest new possible avenues for how this might be done. Future work, with more access to data from a wider set of investors, will be essential to test and validate our conclusions.

Methods

Data compilation

Our dataset leverages private project finance data provided by nineteen investors, including multilateral development banks, development finance institutions, national development banks, local and international commercial banks, and private asset managers. All transactions are market-rate, meaning we exclude any concessionally financed investments, including those that contain grants, guarantees or blended finance.

The full dataset comprises 845 transactions, with 81% of observations between 2020 and 2026. Each institution provided anonymized transaction data by populating a standardized template covering geography, sector, financial-close date, project size, cost of debt (measured as either a spread over a floating-rate benchmark or a fixed rate), cost of equity (measured as the forecast equity IRR at financial close), weighted average cost of capital, debt tenor, currency and leverage.

Out of the total 845 transactions, 704 are included in our regressions. We exclude: 40 loans quoted over a domestic reference rate that is not yet confirmed by the contributor; 31 loans carrying export credit agency cover; 29 loans to financial intermediaries where the end-sector cannot be determined; 12 projects spanning multiple jurisdictions where a country premium cannot be isolated; 9 datapoints representing multiple tranches of a single facility; 9 inflation-indexed deals whose spreads cannot be converted to USD over SOFR; 7 concessional or grant-funded transactions; 2 guarantees, whose price is a guarantee fee rather than a project cost of debt; 1 loan awaiting the contributor's split of its fixed and floating tranches; and 1 loan missing key information.

We focus primarily on the cost of debt, the component of cost of capital that we observe directly, expressed as a USD-equivalent spread over the secured overnight financing rate (SOFR). Debt contributes the majority of project finance funding, and is therefore the key element in the cost of capital²³. Data availability is also a factor, as many of our contributing partners invest only via debt and did not provide project costs of equity, meaning that the weighted average cost of capital (WACC) cannot be constructed across the full sample. To determine any differences between an analysis of the cost of debt and the overall cost of capital, including equity, we rerun all models described in this section with an added leverage control for the 317 transactions where leverage is available. We also rerun our tests using both cost of equity and WACC where available, instead of cost of debt, and present several specific findings around the cost of equity.

Debt spread homogenization & data normalization

Our dataset contains loans issued at fixed and floating rates, denominated in USD, EUR and 22 local currencies. The full methodology for constructing a single comparable debt measure is set out in the Supplementary Information (SI). In summary, each loan is converted to a USD floating-

rate equivalent spread over SOFR: USD floating-rate spreads are taken as quoted over SOFR or, for loans closing before the market transition, over LIBOR (SI Section S1); USD fixed-rate loans are converted to floating via the corresponding-tenor interest-rate swap on Bloomberg at financial close; non-USD loans are taken as fixed, or converted to a fixed rate where needed via interest rate swaps from Bloomberg, then swapped into a USD spread using historical fixed-to-floating cross-currency swap rates for each currency and tenor. For the euro specifically we use floating-to-floating cross-currency swaps, as that is the quotation available on Bloomberg. Cross-currency swap rates were provided by TCX, a specialist emerging-market hedging facility for the period covered by the database. Bid-offer spreads were supplied by TCX alongside the swap data and deducted from the cross-currency swaps

As a check, we compare the TCX swap curves to those on Bloomberg for the Indian rupee, one of the few currencies with consistent Bloomberg data across time and tenors. Bloomberg rates differ from TCX by an average of 0.1pp or less, without systematic bias in either direction. Re-estimating the main results on the 248 hard-currency transactions alone, which require no currency conversion, the country premia correlate at 0.76 with the full-sample estimates across the 41 countries with more than one hard-currency transaction, and the investor confidence effect and the declining time trend persist and the DFI differential rises (SI Table S5.5).

Several project datapoints were provided in formats that did not directly supply a market cost of debt at financial close. First, 110 transactions were reported as a WACC rather than a loan price. For these the cost of debt is back-solved from the WACC using the project's capital structure, with the cost of equity assumed to be 2.0 times the cost of debt (the median across our sample). Second, 34 projects report an unlevered project return at financial close rather than a loan price. For these we divide the unlevered return by the same 2.0 multiple to give the project's cost of debt. Because the 2.0 multiple is an assumption applied to 144 of the 704 transactions, we re-estimate every result on the 560 transactions whose cost of debt was reported directly. The country premia correlate at 0.87 with the full-sample estimates, and the investor confidence effect, the DFI result and the local-currency discount all hold; the tenor gradient does not (SI Table S5.7).

Base model specification

Our base model decomposes the spread on each transaction into a country premium, a sector premium, a currency premium, a year premium, a debt tenor premium, and a deal-level residual. We estimate these components jointly by ordinary least squares with heteroskedasticity-robust (HC1) standard errors:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{t(i)} + \lambda \cdot Tenor_i + \varepsilon_i$$

Where:

- $Spread_i$ is the USD-converted spread over SOFR for transaction i on the day the loan was made;
- $\gamma_{c(i)}$ is a set of country deviations from the intercept α , which estimates the premium of the average country across each of the 66 countries in our sample;
- $\delta_{s(i)}$ is a set of sector fixed effects for 15 sector groupings with solar as the reference category;
- $\phi_{Currency(i)}$ is a currency fixed effect distinguishing hard-currency (USD and EUR, the reference);
- $\tau_{t(i)}$ is a set of year fixed effects for the 17 years in which our sample contains transactions, with 2019 as the reference year;
- λ is the coefficient on loan tenor, in bps per year, centered at the sample mean of 12.6 years, so that α and the country premia are evaluated at an average-tenor loan;
- ε_i is the residual, i.e., deal-level variation outside country, tenor, sector, currency and year.

We include an intercept α , which is the average cross-country spread over SOFR, and express the country dummies γ_c as deviations from this average. The sum of $\alpha + \gamma_c$ gives the average spread of country c over SOFR, which we call the country premium. A country premium of 605 bps, for example, indicates that the average infrastructure loan in that country prices at $SOFR + 6.05\%$, accounting for sector, tenor, currency, and year. In estimation each country coefficient is the premium, and the intercept and the country deviations are recovered by taking the mean of the estimated premia as the intercept and expressing each country's premium as its difference from that mean. Local-currency transactions are pooled into a single category rather than each carrying its own fixed effect, as they are collinear with the country fixed effects, and euro and dollar transactions are likewise pooled into a single hard-currency category. Solar is the reference sector because it is the largest in the sample, giving the most precisely estimated baseline, and 2019 is the reference year as a midyear in our dataset.

Project size has no significant association with spreads and is therefore not included in the base model. Debt tenor is included in the model because it is related to spreads, though in the opposite direction to the usual term-premium intuition and not at a statistically significant level (-3 bps per year, $p = 0.15$).

Our baseline standard errors are heteroskedasticity-robust (HC1), which treats deals as statistically independent. However, while each country receives a premium and each year an effect, a time-limited national shock such as a currency crisis or an election would shift every deal in a country in the same direction but would not be captured by the global year effects for the period (or the aggregate country premium). Likewise, deals from the same investor share that institution's cost of funds, risk appetite and pricing policy, so an individual investor's deals are also unlikely to be independent. Residuals may therefore be correlated within countries and within investors. To

account for these possible interdependences, in the SI we report our key specifications clustering by country, by investor, and, because a pair of deals may share both, using two-way clustered standard errors that combine the two and net out the double-counted overlap, following Cameron, Gelbach and Miller.

Investor effects

We test whether an investor's experience in a market is associated with pricing. For each transaction i , n_i is the number of transactions that investor $I_{(i)}$ has closed in country $c_{(i)}$, so $n_i = 1$ for an investor's first deal in a country, $n_i = 2$ for its second, and so on. We estimate n_i linearly, as indicator categories (one, two, three, four, and five or more deals), and as a single binary indicator for $n_i \geq 4$. All investor-effect specifications are estimated on a subsample that excludes national development banks investing only in their home market, as these investors are unlikely to change pricing from experience given their mandate:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{t(i)} + \lambda \cdot Tenor_i + \beta^n \cdot n_i + \varepsilon_i,$$

Where:

- γ, δ, φ and τ are the fixed effects of the base model, and λ the tenor coefficient;
- β^n is the marginal effect of one additional deal on the spread;
- ε_i is the deal-level residual.

To distinguish investor-specific experience from a general improvement in a country's project environment as more infrastructure is financed, we estimate a second specification that replaces the investor's own deal count with a market-wide measure of experience. We use the cumulative number of infrastructure projects recorded in the World Bank's Private Participation in Infrastructure (PPI) database for country c up to year t , denoted $M_{c(i),t(i)}$. The specification is otherwise identical³²:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{t(i)} + \lambda \cdot Tenor_i + \beta^{PPI} \cdot M_{c(i),t(i)} + \varepsilon_i.$$

As a further check, we estimate a third specification that includes both the investor's own deal count and the market-wide measure together, so that any change in spread from multiple investments is identified holding the country's accumulated project experience fixed:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{t(i)} + \lambda \cdot Tenor_i + \beta^n \cdot n_i + \mu^{PPI} \cdot M_{c(i),t(i)} + \varepsilon_i.$$

The above specifications test whether, accounting for country, sector, currency, year and tenor, investors' later deals are associated with lower or higher spreads than the average deal in the market. We also test whether an individual investor's own deals price lower as its experience accumulates, rather than relative to a generic investor's first deal. We therefore estimate two additional specifications. First, we add investor fixed effects, so that each investor's average pricing level is absorbed. $\psi_{I(i)}$ is a set of fixed effects for the investors in the subsample:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{l(i)} + \lambda \cdot Tenor_i + \psi_{l(i)} + \beta^n \cdot n_i + \varepsilon_i.$$

Second, we test how an investor's subsequent deals price relative to their own investments in that country. We replace the country and investor effects with a fixed effect for every investor-country pair, so that the only remaining comparison is the same investing relationship observed at different levels of experience. $\mu_{l(i),c(i)}$ denotes investor-country pair fixed effects, which subsume both γ and ψ . In each case n_i enters in the same forms as above (deal categories and the four-deal threshold).

$$Spread_i = \alpha + \mu_{l(i),c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{l(i)} + \lambda \cdot Tenor_i + \beta^n \cdot n_i + \varepsilon_i.$$

Development finance institutions versus private lending

To test whether DFIs lend at comparable rates to the private sector we add an investor type indicator to the base model. DFI_i is a binary variable with one for DFIs and zero for private investors:

$$Spread_i = \alpha + \gamma_{c(i)} + \delta_{s(i)} + \varphi_{Currency(i)} + \tau_{l(i)} + \lambda \cdot Tenor_i + \zeta \cdot DFI_i + \varepsilon_i$$

DFI investors are ADB, Africa50, BII, BOAD, CAF, FONPLATA, and JICA. Non-DFI investors are A.P. Moller Capital, Ashmore, Blue Dot Network Projects, CrossBoundary Energy, Cygnum, Ecobank, HSBC, Old Mutual Alternative Investments and Inspired Evolution. FDN, IDCOL and NIIFIL are excluded from the DFI test, and from the investor-experience specifications, as national development banks lending only in their home markets.

Cost of equity and leverage

Our base model uses cost of debt as its outcome variable, in large part because it is available for our full sample, while cost of equity was reported for only 130 of the usable 704 transactions. We use this subsample to examine how equity is priced versus debt, both as a robustness check when making inference about cost of capital from cost of debt and also as a standalone investigation into cost of equity. In addition, 317 transactions reported capital structure, i.e., equity-to-debt ratio, allowing us to test whether our results are robust while controlling for leverage on this subsample.

Using cost of equity and leverage data from the available subsample, we construct an equity premium (a project's cost of equity minus its cost of debt), an equity multiple (the ratio of cost of equity to cost of debt), and a post-tax WACC, using corporate tax rates from KPMG. We estimate regressions of these measures with currency and year controls against country premia, leverage and investor confidence measures. Because cost of equity is available for only 130 transactions and is defined as forecast equity IRR at financial close, these analyses are substantially less powered and should be interpreted as descriptive robustness checks. We also test leverage as an additional control for the debt-side results, while recognizing that leverage may be endogenous to financing conditions and project risk.

Separately, we use the reported leverage ratios to test the robustness of our debt-side results, re-estimating the base model and the investor effects, DFI test and time-trend specifications with leverage as an additional control on the subsample with reported leverage.

Additional models

To test how well a given indicator predicts observed spreads, we replace the country and sector fixed effects from our base model with a single continuous country-level measure, such as the share of the rural population with electricity access, and measure how much of the cross-country variation each can account for. For each deal i in country c at closing year t :

$$Spread_i = \alpha + \beta X_{c(i),t(i)} + \varphi_{Currency(i)} + \tau_{t(i)} + \lambda \cdot Tenor_i + \varepsilon_i$$

Where:

- Fixed effects φ , τ and ε are those of the base model, and λ the tenor coefficient;
- $X_{c(i),t(i)}$ is a country-level variable for country c at the time nearest to closing year t .
- ε_i is the deal-level residual.

We test each indicator individually rather than in a single multivariate model, since these country-level indicators are strongly correlated with one another and a combined regression could not separate their contributions. We retain currency and year fixed effects and the tenor control since these are deal-level characteristics rather than features of the country risk we are trying to account for with the indicator. We drop sector fixed effects because sectors cluster geographically in our sample and so would absorb a substantial share of the cross-country variation we are testing.

In total, we test 29 variables. They comprise two practitioner-oriented country-risk measures (Damodaran’s country default spread and Moody’s foreign-currency sovereign rating), six governance indicators from the World Bank Worldwide Governance Indicators, and 21 development, macroeconomic and energy indicators from the World Development Indicators. Variables are matched to the country and closing year as closely as available. Each indicator is tested separately because the indicators are strongly correlated. For each variable we report the incremental R^2 , the gain in explained variation over a baseline containing only currency, year, and tenor, estimated on the deals for which the variable is observed. Table S6.1 in Supplementary Information reports all 29, and Table S6.2 repeats the comparison using five-fold cross-validation.

Data availability

The transaction-level datapoints supporting this work were provided under confidentiality agreements that permit access only to named researchers at the participating universities. Individual transaction records, and any data from which individual transactions could be identified, therefore cannot be made publicly available as a condition of obtaining the data. The aggregated outputs underlying all findings are available: full regression results, including all country and

sector premia with standard errors and sample sizes, are reported in the Supplementary Information. Enquiries about data access should be directed to the corresponding author, who will consult the contributing institutions as access beyond the aggregated outputs would require the agreement of each institution whose data is involved.

Code availability

All analysis code is written in R and is available from the corresponding author on request. The code comprises an annotated script that reproduces the regressions reported in the paper and the Supplementary Information. Analyses were run under R version 4.1.2 using the `readxl`, `sandwich` and `lmtest` packages. Because the analysis dataset contains transaction-level records that cannot be released (see Data availability), the code cannot be executed end to end by readers without access to those data; it is provided in full on request so that every sample restriction, transformation and estimation reported in the paper can be inspected and independently verified.

Supplementary Information

Supplementary Information is available for this paper.

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Author contributions

E.S. conceived the study, assembled the dataset and drafted the manuscript. D.P.S. and C.H. supervised the research, advised on the study design and the interpretation of the results, and revised the manuscript. All authors read and approved the final version.

Competing interests

E.S. is a partner at Article Six Group, an infrastructure finance advisory firm. C.H. is a shareholder and non-executive director of Vallorix, an infrastructure technology firm.

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