

Response to “Damodaran’s Country Risk Premium: A Serious Critique”

I am flattered to see my name in the title of a paper and mentioned so often in the text (I counted 74 mentions), even if the paper is critical of my methods. That would suggest, and the authors seem to believe this, that I have developed a revolutionary way to measure and incorporate country risk in valuation. I wish it were true, but it is not. Much of what I do is evolutionary, and to understand why, all you have to do is to look at current practice in valuation. First, analysts at investment banks, who value emerging market companies, routinely add “country default spreads” to “mature market premiums” to get equity risk premiums. Second, they consider country risk, based on the country in which a company is incorporated, rather than where it operates. My approach basically applies two tweaks to these standard practices. First, I argue that if the default spread is the premium demanded for buying a government bond issued by the country, the additional equity risk premium for that country should be higher, because equities are riskier than bonds. I try to measure this relative risk by scaling the standard deviation of equities in that country to the standard deviation of bonds. Second, I posit that a company’s exposure to country risk comes from where it operates and not where it is incorporated. By my reasoning, a South African mining company that is incorporated in the United Kingdom and traded on the London Stock Exchange is still exposed to South African country risk, and Nestlé, notwithstanding its Swiss roots, is exposed to country risk because it gets a significant portion of its revenues from emerging markets. I have suggested lambda (measured in its most simplistic version, by dividing the revenues that the company generates in an emerging market by what the average company in that market derives from that market) as one way of incorporating this risk, but I am agnostic on how you bring in country risk exposures as long as you bring them in, as even a cursory analysis of my valuations (not my papers ... the actual valuations) would indicate.

There is also a subtext to this paper that accuses me of multiple sins. First, the authors contend that I am not being academic enough and too much of a practitioner, a novel critique for me, since I am usually accused of the opposite. Second, the authors argue that I am too much of

a pragmatist and I guess not enough of a purist (or at least not pure enough), though I am not quite sure whether to view this as an insult or a compliment. Third, I guess that the equations in the paper that are marshaled to make the case against me are a message from the authors that my approach is not rooted in theory. I proudly plead guilty to all three charges. Having seen academic theorists mangle (or worse, not even try to do) the valuation of real companies, I would rather be too pragmatic, too practitioner oriented and not enough of a theorist, when it comes to valuation.

On the issue at hand, whether country risk should be incorporated into valuation, let me save the authors the trouble and mount the case (and I think a much better one than they make) against my measures. Here are the possible scenarios where country risk can and should be ignored. The first is if all investors are globally diversified and a single factor model (like the capital asset pricing model [CAPM]) holds, there will be one global equity risk premium and beta will capture all country risk. That day may very well come, but the evidence on home bias in portfolio and barriers on trading across markets suggests that we are not there yet. The second is if country risk is diversifiable at either the company level or the investor level. For that to happen, country risk should be uncorrelated across countries, an assumption that would be at war with the substantial evidence that is accumulating that the correlations across markets have increased over the last two decades, not decreased. The third is that perhaps markets don’t care about country risk and that it is not priced in. That too is an empirical question, and the evidence (though still in its early phase) indicates that pricing multiples (PE, Price to Book, enterprise value [EV]/earnings before interest expense, income taxes, depreciation, and amortization expenses [EBITDA]) are lower in emerging markets, after controlling for growth, which is consistent with the argument that country risk does matter.

The authors do make legitimate points about measurement issues, and they may be surprised to hear that I agree with them more than I disagree. First, on my use of the relative standard deviation of equity and bond markets to scale up the country default spread: In using the

approach, I was drawing on the central tenet of the modern portfolio theory, where standard deviation is the measure of risk and expected returns are linear in standard deviation. Both assumptions are subject to debate, but if they are, it is not just my variation that will come under assault but most risk and return models. Second, on my use of revenues to estimate lambda, I do have a much longer discussion of lambdas in another paper, titled “Company Risk Exposure to Country Risk,” where I concede that using just revenues misses other aspects of country risk exposure. However, using the richer approaches to estimating a company’s risk exposure to country risk requires access to information on individual companies that is not widely available.

If the authors’ point is that country risk does matter and that they just do not like my way of dealing with it, fair enough, but the onus then is on them to offer an alternative, and I don’t see one. If we want to have an honest discussion of this topic, it is time to stop hiding behind portfolio theory and the CAPM. If the authors accuse me of violating the classic versions of these models, I again plead guilty. But recognize that if we lived in the Markowitz universe, there would be no privately owned businesses and illiquidity would never be an issue. The CAPM, in the forty years since its birth, has been twisted and modified beyond recognition; adding a small cap premium to your cost of equity or reducing your value by an illiquidity discount is clearly a violation of the model. The reason the CAPM persists is because practitioners have figured out ways to modify and adapt

the model to fit environments that it was never designed for. The arbitrage pricing model (APM), in spite of all of the ink that has been spilled, has never emerged as a serious contender to the CAPM in valuation, simply because it is great at explaining past returns but not particularly adept at forecasting expected returns (and costs of equity). In fact, much of the work in asset pricing in academic finance, at least since Fama/French a couple of decades ago, has become an exercise in pragmatism, where empirical data are used to adjust theoretical models to fit the real world. The multifactor models that I see in portfolio theory have multiple components that have no theory behind them and still are used in these models. (What is the theory that the authors see behind a small cap premium or price to book variables? What about momentum variables?)

There are those who enjoy playing theory games in ivory towers, but I would rather value real companies in real time. If that requires me to make assumptions and adjust models, I will. I am not wedded to my way of measuring country risk and will abandon it in a second, if I was offered a better alternative. I know that the authors do talk about building a unified theoretical model that will yield precise estimates of country risk. I eagerly look forward to that groundbreaking work, but I don’t see it in this paper.

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