World Bank Discussion Papers

Portfolio Investment in Developing Countries

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foreign investors might demand lower returns than in more correlated small OECD markets.

41. Since many of the openings occurred in 1991, it is too early to judge whether market capitalization will sustain a rise.

Comments

Campbell Harvey

I would like to apologize to Elaine Buckberg because I was nominated to discuss her paper only a couple of hours ago. With such short notice, I have not had time to prepare detailed remarks. However, I did see an early version of her paper and, as Elaine noted, there is a relation between what she is doing for emerging markets and what I did for developed markets in my 1991 *Journal of Finance* article.

Let me start my discussion with a few observations. First, I would like to draw together what Elaine does in her paper and what we heard Giorgio De Santis talk about earlier this afternoon. If you remember, what he was doing was basically what I call "unconditional" mean-variance analysis, and what Elaine is doing is "conditional" mean-variance analysis. I want to keep the distinction between unconditional and conditional in the background throughout my discussion.

Actually, I would like to revisit a comment that was made by Ross Levine in his discussion of Giorgio's paper. Ross was trying to judge the sort of things that would be useful for him in terms of learning something from the paper. One of the items that he mentioned was: "I'd like to see research that shows that more integration implies that the equity market does better." This is something tangible that you could actually take to people in a country and it has real policy implications. My response to his commentary hinges on two key words of his. The first word is "integration" and the second is "better." What I am going to suggest to you is that without the exercises that Giorgio and Elaine did, you really cannot say anything about "integration" and judge whether one market is doing better than another. This fundamental type of research establishes benchmarks so that we can evaluate whether one market is integrated or more integrated than it was, and that critical information can be used in policy decisions. So, I strongly disagree with Ross Levine's assessment of this research.

Let me also put on the table the point of view of practitioners. We saw a lot of economic equations and discussions of generalized methods of moments. These types of frameworks are critical in determining the meaning of "better." To me "better" means that an asset, or a whole market, performed better than expected. In other words, there is a level of risk that commands an expected reward, and this market happened to do better than you would expect based on its level of risk. This is identical to performance measurement in that we need to start with some measure of risk. To say that an asset manager exhibited superior performance solely on the basis of realized returns could be misleading. For example, the manager's portfolio could have an unacceptable level of risk—and she got lucky over the evaluation period. So we have to control for risk. To control for risk in a meaningful way, we need papers like Elaine's and Giorgio's. What these papers do is establish a framework to evaluate what risks are relevant and to measure those risks. So these papers have direct implications for judging what "better" means.

At the level of practical asset allocation, I said (during the presentation of my paper) that it is commonplace in allocation strategies to have an extra set of constraints that control the risk of the portfolio. It is not enough to maximize expected return for some level of portfolio volatility because the implied portfolio might have an oil beta of 10, and for most people that is an unacceptable level of risk exposure. If oil moves the wrong way, this strategy could have disastrous implications for your returns. So we need to establish what risk is and this is exactly the point of Elaine's paper.

I want to focus my discussion of Elaine's paper in terms of the model. As I said, I wanted to reconcile this "unconditional" and "conditional" business that we have heard a lot about today. What Giorgio was talking about is the following. If we measure risk *on average* so that each country has an average level of exposure to risk attributes, then can these average exposures explain the cross-sectional dispersion of *average* returns across different countries? That is, higher average risk in one country should be associated with higher average returns in that country. Giorgio found that if we used just industrial countries in the analysis, it seemed to be a reasonable description of the world. Higher beta risk is associated with higher expected returns. This is consistent with what I found in my 1991 *Journal of Finance* paper for OECD countries. One of the big exceptions was Japan. My data, which covered up to May 1989, suggested that Japan was overpriced at that time. Indeed, the model provided me with a framework to make statements like overpricing. So, according to the model, and the level of risk exposure that I estimated for Japan, the Japanese market price was too high. These types of statements are also implied in Giorgio's analysis.

Elaine is doing something different, and it is something that Stijn Claessens mentioned earlier this afternoon. Elaine

addressing the basic question of where the predictability comes from. So, let's examine one of her equations:

$$r_{it} = \beta_i r_{wt} + \varepsilon_{it}$$

Loosely speaking, a regression of the country excess returns on the world market excess returns should have no intercept, and the conditional expectation of the error term, ε_{it}, should be zero. What Giorgio was testing was whether ε_{it} is zero on average, that is, if we calculate all the ε_{it}'s, the mean should be zero. Elaine is testing whether the ε_{it}'s are predictable based on information available at time t-1. In other words, the ε_{it}'s are the model's errors. The model says they should be zero. Of course, the conditional mean of the ε_{it}'s, or the expected value of the model's errors, should be zero. If it is significantly different from zero, it means that you can predict the model's mistakes. In these cases, we reject the model.

Another way of looking at this exercise is in terms of explaining predictability.

$$E[r_{it}|Z_{t-1}] - \beta_i E[r_{wt}|Z_{t-1}] = E[\varepsilon_{it}|Z_{t-1}] = 0$$

"Statistical" predictability is the predictability implied by the expected error of the model asset pricing theory.

The first item I call "statistical" predictability. Here there is no formal model; it is just a regression of some variables that investors consider important—a forecasting model that predicts returns. How can we account for that predictability—how as Stijn said, can we explain that predictability? This is a very important question. The asset pricing model is critical to answering this question. This is really what Elaine does in her research. The model says that the "statistical" predictability should be captured by the beta times the expected return on the world benchmark portfolio. This is what I call the predictability implied by asset pricing theory. If this model is any good, then it provides forecasts that look like the statistical forecasts. So, this is a methodology whereby we can explain the predictability and the sources of the predictability. In this very simple model, predictability is being driven by changing risk premiums (the expected return on a world benchmark), or by potential changes in the beta (although Elaine assumes that the betas are constant over her sample).

Of course, this is only one model—and a very simple one. I have looked at some specific alternatives. Indeed, the main result in the paper is that for 14 markets she cannot reject integration. In my opinion, that number is overstated. I believe a lot more of these markets are segmented. If we do an alternative test—and you've probably seen this one before—if we include an intercept, the model says that intercepts should be zero.

$$r_{it} = \alpha_i + \beta_i r_{wt} + \eta_{it}$$

The intercept, α_i, is the so-called Jensen alpha. It is a performance measure. The model says that it should be zero. So, we could also estimate this model, and I actually have, in Harvey (1993). In many of the markets that are supposedly integrated, the alphas are significant. Remember, α_{it} is the difference between the model-predicted excess returns and the statistical expected excess returns. My evidence suggests less integration.

All of these statements assume that you have the right asset pricing framework. There are other models where a single source of risk is not a valid representation of the expected returns. There are models that explicitly allow for market segmentation. Certainly, some of my distinguished colleagues present here today have been pioneers in building the models of partial segmentation, mild segmentation, and complete segmentation.

Let me introduce a model that I am presently investigating, which might reconcile some of these issues of segmentation and integration.

$$\varepsilon[r_{it}|Z_{t-1}] = \emptyset^i_t (Z_{t-1})\, \lambda^w \text{ Cov } [r_{it}, r_{wt}|Z_{t-1}] + \{1-\emptyset^i_t (Z_{t-1})\}\, \lambda^i \text{ Var } [r_{it}|Z_{t-1}]$$

This model says that the expected excess returns on any asset i is determined by two risk measures. There is the covariance with the world portfolio, consistent with Elaine's model. Under the null hypothesis of complete integration, the $\emptyset^i_t$ parameter equals one. The λ^w is the coefficient of world relative risk aversion. If we are in a market that is completely segmented, then the covariance with the world does not matter. In that case, $\emptyset^i_t$ equals zero, then the second term kicks in. The second term is the variance of the individual market. λ^i represents the local risk aversion in that market. This is the model that allows for integration, segmentation, and for something in between. Importantly, it also allows the $\emptyset$ parameter to change over time, which implies changing degrees of integration. I expect that at the beginning of the sample the $\emptyset$ parameter is close to zero for most of the emerging markets' increases through time, as these markets become more integrated.

I tried to be constructive in my discussion. In my opinion, a model that allows for both segmentation and integration is more in tune with the sorts of markets that we are looking at. Indeed, this is also evident in the beta estimates in Elaine's paper. There is little relation between the betas and the expected returns. Turkey has a beta of almost negative 2 with an average excess return, in her sample, of 60 percent. Here is a country with negative beta and a huge positive average excess return. Admittedly, the model is rejected at the 10 percent level, but still the betas just do not match with what we see in the data. So, I think an important extension of this type of research is to use models that allow for full integration and segmentation and to trace how segmentation changes through time.