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International Arbitrage Pricing Theory: Discussion

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the role played by the tax rate in Miller's work: people pay tax as a function of their *total* income from debt and equity securities and, irrespective of which firm pays the income, the same tax schedule is applicable. That is the reason why Miller reaches the conclusion that at equilibrium only the aggregate amount of debt matters but not the amount of debt of each firm.

I fail, however, to see why the external currency market equilibrium is analogous to Miller's equilibrium. The cost saving θ per dollar deposited for a particular bank can only depend on the amount of *its* offshore involvement. Similarly, the risk premium λ is meant to reflect, among other things, the bank's risk of default; evidently, it is specific to the bank and depends on the amount of deposit *it* has received. It is not clear, therefore, what economic reasoning allows Logue and Senbet to draw θ and λ , in Figure 1, as a function only of the *aggregate* amount of deposits received by the offshore system. I have of course no quibble with the fact that, at the margin, each bank is indifferent between increasing its domestic or its offshore activity. That is simply the optimality condition. The mystery is rather why this decision becomes indeterminate at the level of the individual bank as soon as some aggregate marginal condition is satisfied. There is no apparent reason why θ and λ may be treated as if they were market wide prices which depend on aggregate supply and demand, and on them only. Perhaps, the analysis could be clarified on this point.

This paper does represent a welcome attempt to model some of the economic motivations of offshore banking, which everyone feels are important. But some additional light thrown on microeconomic foundations would be even more welcome.

DISCUSSION

ALLAN W. KLEIDON AND PAUL PFLEIDERER*: This paper examines the consequences of putting the word "International" in front of Ross's Arbitrage Pricing Theory, or, in shortened form, of putting an "I" in front of "APT". Professor Solnik has played an important role in putting that same letter in front of another important acronym in asset pricing theory. The outcome of that effort was, of course, the IAPM, and it appears that it is largely dissatisfaction with the testability of that theory which motivates the work presented here. Professor Solnik discusses his work primarily in terms of international pricing and his results are, of course, interesting for that application. He also notes other potential applications, e.g. asset pricing with relative price uncertainty. First we want to discuss the theoretical considerations involved in extending the APT to an international setting and then consider what is gained on the empirical front by making this extension.

The major pitfalls one encounters in making a pricing theory international in its scope arise because of differences in investors' perspectives or their opportunities. Under differences in opportunities we recognize that foreigners may not be able to invest in the same assets as domestic investors due to outright restrictions on the flow of capital. Likewise, they may not have the opportunity to earn the

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same nominal returns as domestic investors due to differential taxes or transactions costs. The impact of these on the applicability of the APT is not considered here. Rather, the effect of differences in perspective is addressed. There are two concerns. Will a portfolio viewed as riskless by one class of investors be deemed riskless by all other classes? And, if one class of investors sees returns being generated by an m -factor process, will all other classes see an m -factor process at work when they measure returns in their own metric? Solnik argues that the answer to both of these questions is favorable to the extension of the APT to an international setting.

One must be a bit cautious in arriving at these conclusions. The exchange rate, which is assumed to be linearly related to the same m -factors as are asset returns, plays a crucial role in the analysis, and some extra assumptions beyond those typically required by the APT must be employed. The APT, it will be remembered, does not require that the idiosyncratic terms be mutually independent, only that their joint distribution satisfy conditions for the law of large numbers to hold. Solnik's proof requires a bit more than that. In the development of the IAPT we encounter [equation 6] the product of the idiosyncratic term of one asset, the foreign riskless bill (which is related in the obvious way to the exchange rate), and the idiosyncratic returns on the assets comprising the arbitrage portfolio. The weighted sum of these products may not have a vanishing variance even if the weighted sum of the idiosyncratic terms does. This is, we believe, more than an arcane technical point. In empirical work one usually extracts factors until most of the variation has been explained. One would like to stop at that point where the joint distribution of the residual terms is such that the risk associated with them nearly disappears in a well-diversified portfolio. This residual risk could be small when it is measured in terms of one currency, but much larger when measured in terms of another due to the characteristics of the idiosyncratic term associated with the exchange rate. The spirit of the APT, at least in its applications, is one of approximation. An arbitrage portfolio is *nearly* riskless and most assets are *close* to being linearly priced. The point here is a simple one: what may be a good approximation from one perspective, may be poor from another.

It is possible to check the adequacy of the underlying assumptions. Solnik notes "that the assumption of only m common factors and independent residuals imply severe constraints on the currency-asset covariance matrix". A direct test of whether these restrictions hold would be worthwhile as a diagnostic check on the model's assumptions.

Having brought up the subject of approximation, we note that the results rely on the so-called second order effects being small. In the continuous time framework these effects vanish, and as is argued in the paper, it is in the spirit of the Arbitrage Pricing Theory to use the smallest possible trading interval as the interval over which returns are defined. The APT does not, however, require this. The present theory does. This only argues that empirical testing of the model should be done in terms of the shortest available interval.

We believe that the results obtained in the paper are in another dimension more general than suggested. It is assumed that corresponding to each currency there exists a riskless bill (riskless in that currency) and that these are freely

traded. We are led to believe that in the absence of these bills, the results obtained in the paper would not be assured. In his appendix, Solnik derives what he claims are the pricing relations in the absence of these bills. These differ from those reported in the main part of the paper. This we find troubling. For the APT to have any import it must be possible to mimic riskless bills with portfolios of risky assets. The theory should be the same whether the bills exist or not.

We now consider briefly the gains realized in making this extension of the APT. The general advantages of the APT over the CAPM are well rehearsed by now. The fact that the latter requires an aggregation of individual portfolios makes its application to a partially segmented international market impossible. The APT does not suffer in this regard. We need only require that within the segmented market investors have access to enough assets to construct well-diversified portfolios. What is interesting in the IAPT is the emergence of testable restrictions not found in the APT, namely the fact that the lambdas associated with asset pricing in one country have a particular relation to those governing the asset pricing in another [equation 10]. This relation is determined by the factor loadings associated with movement in the exchange rates and it may be that this relation holds with more precision than the pricing relations associated with particular assets. Again, a direct test of the particular restrictions imposed on the covariance matrix is warranted, with the usual caveat concerning joint tests of the model and its assumptions.

DISCUSSION

JORGE BRAGA DE MACEDO*: What determines the net foreign asset position of the residents of a particular country is an important question in international economics. Rene Stulz's paper seeks to answer it in a framework where two countries differ in their technology and asset preferences, but where they produce the same good. In the Ricardo example, Portugal and England trade in wine (or cloth) alone. This eliminates the role of the relative price of national outputs, the exchange rate and thus allows for the existence of an international riskless asset. It also severely reduces the international flavor of the model and is not convincingly justified. Note that complexity is hardly a reason since the author and others have successfully constructed related models with more than one good.¹ Several other simplifications, convincingly justified in the paper, need no further elaboration.²

One interesting feature of the model is the now popular assumption—introduced some years ago by Ray Fair—that the two countries are initially identical, so that we have a closed economy model à la Cox-Ingersoll-Ross. It turns out, however, that the results generated by Stulz's elegant model hold even when this initial symmetry is not imposed, except for one which requires cross-country borrowing in the safe asset. Before proceeding to prove this claim, reference should be made to the special case of Bernoulli utility solved in Section 4. Since

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¹ See in particular Bortz (1982) and Stulz (1982).

² Some of the issues in strategic behavior are explored in Eaton and Gersovitz (1982).