EDITORIAL AND ANNOUNCEMENTS

EDITORIAL

ACTUARIES AND FINANCIAL ECONOMISTS

At the recent AFIR Colloquium in Orlando there were two meetings which gave me cause to think. I disagreed with the ideas being put forward, and I realised why I disagreed and why I prefer my own views to those being put forward. Let me explain.

The introductory lecture was given by Professor Stephen Ross. His theme was the effect of 'survivorship' on studies of manager performance. If you consider only fund managers that have been in business for the whole of a given period, you miss out those that have ceased business during the period. Since they may have ceased business because their performance was poor, the performance of the survivors is biased upwards. The same would be true of companies, though I do not recollect Ross saying this.

Ross then drew an analogy with rivers. Records of the level of the water in the Nile have been kept for millenia. The level appears to be statistically stationary. But, Ross argued, this was because the Nile was a survivor: river levels were more like random walks, but if the water level got too low the river dried up and if it got too high it became a lake (*laughter*). The Nile had been a survivor.

This analogy, though entertaining, does not support Ross's case, but it does support a different case. Over the few millenia we are taking about no major rivers have either dried up or become lakes. It is indeed in the nature of the water level in rivers to rise and fall, sometimes seasonally, and with more or less randomness, but still around some sort of central position. (Over geological time things may have been different.)

But so it is also with many investment time series. The dividend yield on ordinary shares seems to rise and fall about a central position, which varies from country to country but is typically in the 3% to 5% range. Interest rates too are stationary in the long run. The evidence for this also stretches back for millenia, as recorded by Sidney Homer (*A history of interest rates: 2000 B.C. to the present,* 1963).

The stationarity of dividend yields on shares is an example of the statistical time series concept known as cointegration, about which an increasing number of papers and books are appearing. The logarithms of share prices and of share dividends are cointegrated, so their difference, the logarithm of the yield, is stationary. The stationarity of dividend yields seems to have been first modelled by the Maturity Guarantees Working Party of the Institute of Actuaries and the Faculty of Actuaries in Britain in the late 1970s (see *Journal of the Institute of Actuaries,* **107**, 101-212, 1980), but it is now becoming recognised by other authors.

A technical difficulty is that a random walk, whether pure or modified (i.e. an I(1) series in the terminology of cointegration) has very similar short-run behaviour

to a stationary autoregressive model (an I(0) series) with a rather slow mean reversion. Similarly, it is not possible to distinguish, in the short run, between a continuous Brownian motion and an Ornstein-Uhlenbeck process.

It is interesting that empirical time series investigation of stock market data on the eastern side of the Atlantic seems to have been more prolific than on the western side. This is not just because more actuaries have been involved in it, but it is at least partially so. Actuaries have longer memories and longer time horizons than some others in the investment market, and they should be able to make a helpful contribution to the world-wide discussion about investment modelling.

I am grateful to Stephen Ross for his analogy: but I submit that it supports my case better than it does his.

The second discussion in Orlando that I wish to refer to was one in which a number of papers on option pricing were discussed. One, by J. Ph. Jousseaume, made the useful distinction between what I shall call the 'actuarial value' of an option, and the 'arbitrage value'. For this purpose assume that the price of the underlying security performs a logarithmic Brownian motion, and consider the value of a European option. The distribution of the price of the security at the exercise data is lognormal, and it is easy to calculate the expected value of the option at this date. The formula involves the mean drift of the security price as well as the standard deviation. This expected value at maturity can then be discounted at some chosen interest rate to give a present value. This is the way actuaries traditionally calculate present values of contingent payments.

If we then follow the no-arbitrage line of argument, we substitute risk-neutral or martingale probabilities for the true probabilities. We replace the mean drift of the share price by the risk-free interest rate (with possibly also a term $\sigma^2/2$, depending on our definitions). We also use the risk-free interest rate for discounting, and we then obtain the Black-Scholes option pricing formula.

The no-arbitrage argument has gained great weight in recent years, and in the discussion Elias Shiu described it as 'the fundamental theorem of asset pricing'.

Why, then, do I think that the no-arbitrage argument is based on a fundamental misconception? First, the assets that most source investors, i.e. individuals, hold are not readily tradeable: these include private houses, pension rights and insurance policies. The financial intermediaries, those institutions whose liabilities are insurance policies and pension rights, equally cannot trade their liabilities. If these liabilities include options (which may be implicit or explicit) the intermediaries cannot necessarily set up a hedge portfolio with which to match these liabilities. They therefore have to consider the matching of assets and liabilities in a more traditional actuarial way, allowing for the possibility of mismatching, and allowing also for additional reserves to cover the risks of mismatching. Thus they need to value the options included in the liabilities using the actuarial value rather than the arbitrage value.

But even when trading is possible, as in many investment markets, perfect arbitrage is not possible. The problem is volume. In the derivation of the Black-Scholes diffential equation, it is asumed that the writer of a call option can set up a hedge portfolio by buying a suitable fraction of a share, *and that this does not affect the price of the security.*

It is plausible that the writing and hedging of a single option on an IBM share does not affect the price of IBM shares. But if the purchaser of the option asks for sufficiently many billion options, then the writer's hedging could only be done by putting forward a take-over bid for the company.

A fuller description of the process would include an additional term in the Black-Scholes differential equation, reflecting the change in the price of the security as a result of the hedging operation. This would make the equation more realistic, but a great deal harder to solve, because information about the sensitivity of share prices to volume demanded does not seem to have been readily modelled, although it has no doubt been investigated empirically.

The Maturity Guarantees Working Party that I have already referred to made essentially the same point when it was proposed to it that what is now called 'portfolio insurance' would allow life offices to offer policyholders an implicit put option on unit-linked contracts. The idea that life offices, in large volumes, could adopt a policy of selling shares when share prices were falling, without making them fall further, was considered by the Working Party to be unrealistic. Whether or not the stock market crash of October 1987 was caused by computer trading of portfolio insurance seems uncertain, but there is no doubt about the direction in which such trading would operate.

The question of volume is of relevance for the supervision of options markets. While many intermediaries may be able to balance their books appropriately, there must be source purchasers of options, and end writers who have not hedged; these may have cover in the form of matching securities or matching cash, but their ability to write options is limited by the cover they have available. The writer of uncovered options, like any insurance company writing risks that are not reinsured, requires additional solvency reserves.

This is not just an academic point. The solvency of the whole market in options trading would be threatened by the insolvency of any major payer. It is important that supervision is considered in an actuarial way, taking account of liquidity, mismatching and the solvency reserves that should be required.

This brings me to my third subject, one which was not inspired by any particular discussion at Orlando. The Capital Asset Pricing Model is an equilibrium model of the market, based on many assumptions, some of which can readily be relaxed without destroying the model, but others which seem to be both fundamental to it and mistaken. The three obstructions seem to me to be tradeability, *numeraires* and time horizons.

I have already noted that the personal assets of most individuals are not readily tradeable, nor are they readily sub-divisible; individuals therefore have very restricted investment opportunities. Life offices and pension funds consequently have substantial non-tradeable liabilities, which are not the same for all institutions. If they include their particular liabilities in a portfolio selection model, even if all the other assumptions of the CAPM are maintained, different institutions may well end up with different efficient frontiers (and not just different optimum portfolios along the same frontier).

Different investors and different institutions may also work using different numeraires. This may be because they work in different currencies, or because some

work in 'real' and others in 'nominal' terms. The numeraire may be a matter of choice, or it may be directed by the liabilities. The life office with fixed money liabilities may well find a different efficient frontier from the pension fund whose liabilities are index-linked annuities, even if all other things are equal.

Time horizons may also be influenced by the liabilities. A mature pension fund may have a shorter horizon than a growing one, just as an older investor may have a shorter time horizon than a younger one. Fund managers who are judged on their quarterly performance may have even shorter time horizons. It may be possible to reconcile these different approaches by appealing to the uniformity of successive periods, but if my first proposition holds, that many investment series are stationary and autoregressive and thus are not independent from period to period, then a longer time period is not just a succession of identical shorter ones. The variance does not increase proportionately with time and the efficient frontier varies with the time horizon.

These three features lead me to conclude that any satisfactory equilibrium model needs to take account of the volume of investment in each sector, the volume committed to each type of non-traded liability, the volume using different numeraires, and the volume using different time horizons. The concepts are the same as those of 'market segmentation' in the bond market, different sectors of which may be dominated by investors with different tax positions or different 'preferred habitats' of duration or maturity date.

The equilibrium positions in such a model, taking account of volume, is much harder to find, and the elegant results derived from the CAPM may not be so readily forthcoming.

But actuaries should be among those who can recognise the individuality of each particular investment institution, and can adjust concepts of efficiency to match the requirements of each institution. We should not throw out the baby with the bathwater. Although I am criticising the naive CAPM applied on a global scale, the portfolio selection paradigm is undiminished, and may even be strengthened when it is realised that each institution has its own efficient frontier, and cannot necessarily rely on 'the market' finding the frontier for it.

In a debate at the Institute of Actuaries in London in March 1993 (see *Journal of the Institute of Actuaries,* **120**, 393-414, 1993), I proposed the motion that "*this house believes that the contribution of actuaries to investment could be enhanced by the work of financial economists*". I was ably seconded by Jim Tilley, and the opposing point of view was put forward by Terry Arthur and Robert Clarkson. I should like to propose to readers of *ASTIN Bulletin* and to members of AFIR that the reverse proposition is also true, that "*the work of financial economists in investment could be enhanced by the contribution of actuaries*". Let us see more papers to support this proposition in the pages of *ASTIN Bulletin.*

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