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THIS HOUSE BELIEVES THAT THE CONTRIBUTION OF ACTUARIES TO INVESTMENT COULD BE ENHANCED BY THE WORK OF FINANCIAL ECONOMISTS

[A Debate on the above Motion held by the Institute of Actuaries, 22 March 1993]

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Seconded by J. A. TILLEY, Ph.D., F.S.A.
Opposed by T. G. ARTHUR, B.Sc., F.I.A., F.I.S., F.P.M.I.
Seconded by R. S. CLARKSON, B.Sc., F.F.A., F.I.M.A.

Background Reading

The contribution of financial economists to investment is contained in many books and papers.

Relevant journals include:

Econometrica
Harvard Business Review
Journal of Banking and Finance
Journal of Business
Journal of Economic Theory
Journal of Finance
Journal of Financial Economics
etc.

Comprehensive mathematical approaches are contained in:

Duffie, D. (1988). *Security markets: stochastic models*. Academic Press.
Ingersoll, J. E. (1987). *Theory of financial decision making*. Rowman & Littlefield.
Merton, R. C. (1990). *Continuous-time finance*. Basil Blackwell.

Special mathematical aspects are discussed in:

Hull, J. (1989). *Options, futures and other derivative securities*. Prentice-Hall.
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General introductions include:

Brealey, R. A. & Myers, S. C. (1991). *Principles of corporate finance.* 4th edition. McGraw-Hill.
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There are also many books of readings or collected papers, including the *Transactions of the 1st and 2nd AFIR International Colloquia.*

Some Terminology

Terminology used by financial economists includes the following:

Utility Theory: a logical way for an individual to make consistent choices among probability distributions of different, usually financial, outcomes by calculating the expected value of a *utility function*. This can be elaborated into *state preference utility theory*, in which the utility function differs according to the state of the world in which the individual finds himself, e.g. alive or dead.

Portfolio Selection using *Mean-Variance Optimisation*: a method, originally proposed by Markowitz, using *quadratic programming*, i.e. maximising the value of a quadratic function of many variables, subject to constraints, usually linear. Portfolios consist of different fractions invested in different securities. The *feasible region* consists of those portfolios which are possible, given the constraints. The *efficient frontier* is the boundary of the feasible region which includes *efficient portfolios*, i.e. portfolios which cannot be bettered either in terms of expected higher return or lower variance by another portfolio. Risk is defined in terms of variance.

Capital Asset Pricing Model: an *equilibrium model*, that uses the mean-variance portfolio selection model to demonstrate certain results, subject to many rather restrictive conditions, including the idea that investors agree in their knowledge and views, except that they have different utility functions. A further assumption is that there is a *risk-free asset*, which provides a specific return with certainty. One result is that the expected return on any risky security is linearly dependent on the regression coefficient (*beta*) of its return on the return on the total market of risky securities. This allows the two-dimensional mean-variance diagram to be collapsed into a single ranking in a risk/reward table.

Arbitrage Pricing Theory: an elaboration of the capital asset pricing model, in which stock returns are assumed to depend linearly on a number of different factors, not just on one market return.

Time-Series Model: any model that relates the movements of share prices or any other economic variable in time in a specified stochastic way. Continuous time-series models are also called *stochastic processes*. A *diffusion process* is a continuous process where the probability of discrete jumps is negligible. Increments in a diffusion process are usually normally distributed. In a pure *Wiener process*, the increments over any time period are independent of those over any other. Many diffusion processes can be constructed where the residual elements are a Wiener process. A *jump process* is a continuous process where the variable under consideration can change its value instantaneously by more than an infinitesimal amount. The typical jump process is a *Poisson process*, in which jumps occur at independent intervals.

Stable Paretian Distribution or *Lévy-Stable Distribution*: a series of distributions, of which the *normal distribution* is one and the *Cauchy distribution* is another, which have the property that the sum of stably distributed variables, with the same characteristic parameter, is also distributed stably with the same characteristic parameter. Apart from the normal distribution, all these distributions have infinite higher moments, and are mathematically not easy to deal with.

Binomial Model: a model for share price movements (also for yield curve movements) in which time is divided into discrete intervals; during each interval the share price may jump up or down by specified amounts with specified probabilities. Binomial models can be simplified if the result of one jump up and one jump down is the same as the result of one jump down and one jump up, so that the otherwise explosive number of future possibilities is diminished. As the time intervals are reduced, the limit of the binomial model may be an appropriate diffusion process.

Arbitrage: the ability, by buying and selling different securities, to construct a portfolio with net zero cost and a guaranteed positive profit. Normally it is assumed that markets are arbitrage-free, i.e. it is not possible to set up such a portfolio.

Immunisation or *Hedging*: any way of setting up an arbitrage-free portfolio.

Black–Scholes Option Pricing Formula: a formula for the value of particular types of option, derived from assumptions that the share price follows a particular diffusion process, and that an *arbitrage-free portfolio* can be set up, which provides zero return with zero risk. A similar formula can be derived as the expected value of the outcome of an option at expiry, subject to suitable assumptions.

Asset-Liability Modelling: any method of modelling in which the liabilities of a particular investor, usually an institution, are taken explicitly into account.

Mean Variance Optimisation and the *Capital Asset Pricing Model* are both single-period models. Both take account of only two moments (mean and variance) of the return; higher order models have been discussed theoretically, but are hardly used in practice. The estimates of means, variances and covariances are often derived from observed returns over short intervals and assume that these returns are independent, not taking account of longer-term autocorrelations or cross-correlations.

A number of papers have compressed multi-period models into a single period by making assumptions about reinvestment. A very few have used *stochastic programming* as a methodology for multi-period investment, but this idea is not yet well developed.

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ABSTRACT OF THE DEBATE

Professor A. D. Wilkie (proposing the motion): It is my duty and my pleasure to propose the motion that "This house believes that the contribution of actuaries to investment could be enhanced by the work of financial economists".

Our opponents will have you believe that actuaries know everything. While I think we know a great deal about our own business, I am not so conceited as to think that we know a great deal about everyone else's business too. The field of the financial economist is large; the books and papers published on the subject are more than any of us can keep up with, and the influence of financial economists on investment, particularly outside the United Kingdom, is far greater than that of actuaries. Our proposition is that it is worth listening to what financial economists have to say, not just because financial economics is widespread, but because financial economists approach investment with what, we believe, are sound actuarial principles. They are on the same side as we are; and I deeply regret that we have, for too long, treated them as opposition.

I begin with a little history. Once upon a time, a very long time ago, a group of people got together to set up a life insurance company. They knew that they needed to assess the possible future lifetimes of prospective policyholders, and so they decided to get medical advice. They brought a doctor onto the board, and they employed doctors in the company. These doctors assessed each application carefully, made their own judgement about the expectation of life of the individual proposer and, with the aid of an accountant who was good at compound interest, they calculated the appropriate premium to provide, with certainty, a sum at the end of the allotted number of years. Business increased, and, in due course, the directors decided that it was necessary to value the outstanding liability of the company in respect of the policies then in force. So they got the team of doctors to examine all the policyholders again, and make their new assessments of the expected future lifetime of each policyholder. The accountant then drew up a valuation.

One of the directors had a friend who was a mathematician. This friend pointed out that, since people seemed more inclined to die as they got older, an economical way of assessing premiums and carrying out the valuation might be to classify policyholders according to their current age. Sex was not a relevant factor, because the policyholders were all men. There was a great deal of opposition to the mathematician's ideas, particularly from the doctors. How could naive statistics replace their years of experience in assessing individual cases? How could somebody's future lifetime possibly be treated as a random variable? What would become of all the doctors employed by the company if these new-fangled statistical ideas were used? Life was really much more complicated than the mathematician thought it was. The mathematician had no influence on the board of this company, but he and his mathematical colleagues had influence over other insurance companies that were set up, which did so much better than the one dominated by doctors that that one soon went out of business. Thus the actuarial profession was born.

This is just a story, but those who work in non-life insurance will recognise the conflict between individual case estimates of outstanding claims and the actuarial or statistical approach. I hope that those who work in investment can also see the parallel. The actuarial way of assessing anything, investments included, is to use proper statistical methods combining sensible probabilistic models—the life table, the distribution of size of loss, the run-off triangle—with good professional judgement and sound business sense.

Why have actuaries been so reluctant to apply good statistical methods in investment? Why have we allowed others to steal our clothes? In 1952, Frank Redington, a distinguished British actuary, presented a paper entitled 'Review of the Principles of Life Office Valuations'[1], which introduced his concepts of matching and immunisation. His paper was not a statistical one, and was rather like the *Cutty Sark*, one of the last great sailing ships in an age that was already being taken over by steamships.

In the same year, 1952, a young student, Harry Markowitz, published his paper on portfolio selection[2]. It is surprising that an actuary had not written that paper sooner. It looks at the reduction in risk, as measured by variance, in a portfolio that contains a number of investments whose returns are correlated. Actuaries, for years, had been looking at the reduction in risk of a portfolio of insurance liabilities, and how that liability could be reduced by reinsurance, but they had never explicitly spelt out what happened when the results of the liabilities or the assets were correlated. Markowitz did. From his work has grown a flood of books and papers using the methodology of his portfolio selection model. Sharpe's diagonal model simplified the computations, the capital asset pricing model (CAPM) was derived as an economic equilibrium model, and from this the whole concept of stock betas, risk-adjusted interest rates, etc., has been derived. The arbitrage pricing model is a further elaboration.

All these are built on the same foundations as insurance, and hence actuarial science. Indeed, utility theory comes into the first chapter of the new American textbook by Bowers, Gerber *et al.* on *Actuarial Mathematics*[3]. Utility theory has even squeezed its way into the British actuarial syllabus. Yet, I expect that Mr Clarkson will tell you that von Neumann and Morgenstern, the inventors of modern utility theory, got it all wrong. He is a brave man to take on one of the greatest mathematicians of the century. Einstein, when he proposed his theory of relativity, did not say that Newton was wrong, only that he was insufficiently accurate over sufficiently long distances.

I am critical of the CAPM. It assumes that all investors have the same beliefs, and that all have the same type of liabilities. It also assumes single-period investment. However, rather than say that the CAPM is all wrong, I would rather treat it as being right as a first approximation, and then go on to see how it should be adjusted. Actuaries are well aware that we need multi-period models, and Andrew Wise and I are among those who have done work on asset-liability modelling, proposing a way of compressing the experience of several years into a single time horizon, within which the portfolio selection model can then be used. You do not need the CAPM to use a portfolio selection model for your own investments. The CAPM could be improved; but I think that the proper actuarial approach is to try to improve it, rather than to ignore it.

Economists' models of the whole market include equilibrium models, that is where the market gets to if everyone has the same knowledge and everyone agrees with one another. Equilibrium models are enormously useful in economics, but they do not tell the whole story. Dynamic models, which show what happens when people have only partial information, or disagree with one another, are also valuable, but very much harder to analyse. Equilibrium models are like stationary populations in actuarial work, showing what would happen in the long run if circumstances did not change, and hence in which direction things may tend to move at any instant.

The next important date is 1973, when Fisher Black & Myron Scholes produced their seminal article on option pricing[4]. The Black–Scholes option pricing model has been enormously influential, and I have shown how it can be applied to the assessment of options inherent in actuarial liabilities. Although Black and Scholes developed their formula using mathematics that is unfamiliar to most actuaries—stochastic calculus—their result can equally well be obtained by a simple actuarial method, in which the expected value of the option at expiry is discounted to the present. The mathematics required is less than a page of school calculus. It is precisely the way that an actuary would value, for example, a stop loss or excess of loss reinsurance contract. Many of the assumptions required for the Black–Scholes formula can also be criticised, and there are many books and papers discussing how to take more features into account. However, we should not dismiss the simple first approximation. After all, the traditional actuarial approach to compound interest is to assume a single uniform rate of interest for all years into the future. How unrealistic! Of course interest rates are likely to change in the future; of course there is a yield curve at any point of time; of course the stochastic variation in interest rates or investment returns should be taken into account, but for a great many purposes we get by without doing so.

Any financial economist or well-informed accountant, who came across our methods for the first time, would recognise them as ludicrously unrealistic. Yet, once we had explained our methodology, they would surely accept that, for some purposes, our method of assuming a single uniform rate of interest has much to be said for it, and that we know quite well what we are doing in comparison with more elaborate models, which we are also interested in investigating. So it is, in reverse, with the work of financial economists. Our opponents may tell you that the work of financial economists is fundamentally flawed and that some of their models are naive and unrealistic. Yet, before we look at the mote in our neighbour's eye, let us consider the beam in our own. We should consider, not just the elementary models presented in first-year investment textbooks—and in this field there are a great many more such textbooks than there are actuarial ones—but also the 1,001 articles in the many journals in this field that analyse, discuss, criticise and improve the original models.

I am quite willing to criticise some of the assumptions of financial economists and some of the ways in which their work has been misinterpreted. A naive application of stock betas in the stock market, without understanding the complications of the estimation of betas, and the naive application of the random walk model of stock prices—valid enough in the short term—into the farther distant future, are among them.

My own stochastic investment model attempts to remedy some of the deficiencies of the random walk hypothesis, as does Mr Tilley's work on yield curves and interest rates. Yet there is no point in rushing into yield curve models without understanding what has been proposed by Cox, Ingersoll & Ross(5), or by Heath, Jarrow & Morton(6), or by Ho & Lee(7), amongst others.

This brings me back to Redington. The amount of work that has been done, particularly in the United States of America and in other countries outside the U.K., to develop Redington's basic model, is enormous. Concepts of duration (the first derivative) and convexity (the second derivative) are commonplace in bond markets nowadays, and the statistical concepts of hedging or immunisation are widely understood. Are we such jealous gardeners that we honour the seed and ignore the tree that has grown from it?

I appreciate that the statistical methods and some of the mathematical methods used by financial economists are more advanced than we in Britain *require* in our actuarial training, but they are not more advanced than many actuaries actually know. Our education system makes a great mistake in taking in good mathematicians and spending 4 or 5 years knocking the mathematics out of them. It is all too easy in this hall to get a sympathetic laugh by ridiculing those who introduce an integral sign or a correlation coefficient into the discussion. Those people will be pleased that I have used no formulae here, but, if we do not preserve our mathematical skills, then we shall end up being taken over by MBAs, management consultants or accountants. We are already in danger of losing the influence we once had in the investment field. Nowadays investment managers and market makers employ real rocket scientists, those with PhDs or good degrees in mathematics, rather than qualified actuaries. Unless we retain our mathematical heritage, we shall lose out; and unless we learn, master and improve the work of financial economists, we shall lose out again.

I urge you to put aside your conceit, to resist the blandishments of the opposition, to remember your scientific background, to lay aside your prejudices, and to vote in favour of this motion.

Mr T. G. Arthur (opposing the motion): "Yes, it can be done." So trumpeted *The Economist*, if not the financial economist, on 5 December 1992, in an article on beating the market. The article's sub-title read as follows: "For nearly 40 years economists have preached that investors can earn above-average returns only by taking extra risks or striking lucky. Recent evidence suggests that clever investors can outwit the market after all." An article in a later copy of the same magazine informed us that it has been discovered that, by buying cheap and selling dear, the market can be beaten handsomely. Well, well, well, full marks for originality!

We should not condemn economists outright. After all, there have always been a handful of economists who bely the idea that economic forecasting exists to make astrology seem respectable. My seconder will refer to John Maynard Keynes. I prefer the Austrians, Friedrick von Hayek, and his mentor, Ludwig von Mises, whom I could never condemn. We should not even condemn financial economists outright, but the articles from which I have quoted illustrate their constant twisting and turning in their efforts to find theories which fit the facts. Evolving actuarial theory is far more robust, without taking such gyrations on board.

Financial economists have given us modern portfolio theory (MPT), then the CAPM, then arbitrage pricing theory (APT). Elton & Gruber's work(8) tells us that the concept of using a multi-index return generating process to immunise a set of liabilities is new. Is it really new?

My seconder is an acknowledged expert in the mathematics of investment risk, and so is the proposer. I would merely like to thank my learned colleague at this juncture for proposing the motion, and to suggest that he has set up something of a straw man (or men). He gave an historical example in which a reactionary medical profession suggested that mathematics could not replace it or even aid it. He then said that the CAPM should not be rejected, even though it is wrong. It should be accepted as a first approximation for us to build on, but he then went on to say that using a single interest rate, which actuaries have done for simplicity, is *not* a first approximation, it is simply unrealistic.

If you drop a coin in the dark in the street, there is not much point in going to the nearest lamp post, which might be 100 yards away, to look for it, because that is the only place that is light. You will not find the coin there. This is half the problem with financial economics as currently preached. The proposer spoke about the Black–Scholes model; a model that is, I suggest, essentially an actuarial technique. We should develop and build upon sound actuarial techniques, not fundamentally flawed financial ones.

I agree with the proposer that it is arrogant to think that your field of expertise is different from anybody else's, but the case for efficient markets rests precisely on such arrogance. In what other field would anyone argue that the market is 100% efficient? Do we use a pin when we go shopping for cars, carpets, conferences or consulting actuaries? Of course not, so why do we use a pin for portfolios? The engine of all improvement, and indeed, at a more philosophical level the essence of life itself, is, I suggest, discrimination between bad, good and better still.

It is demonstrably true that portfolio investment is no different from other activities. Buying a village corner shop is an investment. Building a house or a factory is an investment. Any project is an investment. Several projects—in other words, several investments—may be strung together and carried out as a company or a group of companies.

The whole of MPT and the CAPM theory depend on the idea that risk is measured by short-term variance of return. It is assumed, and I quote from Diana Harrington's classic and up-to-date *User's Guide*[9], "that all investors have identical time horizons". What sort of an assumption is that? For those of you who think that the long term is merely a succession of short terms, may I remind you that an Olympic sprinter is not in the record books as a marathon runner, even though he has accumulated much more than 26 miles in his sprinting career. However, "identical time horizon is a valid assumption", say two eminent financial economists, Brealey & Myers, in their classic textbook[10]. They go even further; "*risk* is the same for all investors". So, we can rank assets and asset classes uniquely according to their risk, and so according to their rewards. Hence, they give us 63 years of capital market history in one easy lesson, showing that equities have naturally out-performed other classes, because they are riskier. Did it not occur to Brealey and Myers that they may have been using a faulty measure? I think they might laugh at me if I said that government spending, as a proportion of GNP, doubles every 60 years. It has in the last 60, but cannot do so again in the next 60, because it would then be almost 100% of GNP! That is why equity returns cannot be repeated, because, if they were, price/earnings ratios would have to climb to 30 or 40, and payout ratios would have to climb to over 100%. If they had looked at the more reliable earnings growth over those 63 years, they would have found an underlying rate of return several percentage points lower, which would have blown their risk theory to pieces.

Risk is in the eye of the beholder. Consider countries and currencies, because here the proponents of the one-eyed view of risk must answer a difficult question. Unless we want to play silly games with the English language, U.S. dollars are more risky than U.K. pounds to someone living in the U.K. Anyone retiring from a pension scheme with a lump sum to invest to supplement a meagre income will give you the same answer, unless he or she intends to live in the U.S.A. So U.S. dollars, being riskier, will yield more on average. Reversing the situation, U.K. pounds will yield more to a U.S. pensioner, because they are more risky to someone living in the U.S.A. They cannot both be right! Another way of illustrating the same fallacy is to look at the two varieties of gilt-edged securities, fixed-interest and index-linked gilts. Financial economics would have us believe that one type is more risky than the other, but which one? Ask an insurer issuing fixed annuities and you will get one answer. Ask another insurer issuing price-linked annuities and you will get a different answer. So which type has the higher expected return? I will leave that question to the financial economists.

Once it is appreciated that risk is in the eye of the beholder, and that asset classes cannot be ranked uniquely, we can begin to investigate the many consequences. I shall mention only two. The first is that while most, but not all, investors need a greater expected return before they take extra risks, they may not get it even on an expected basis. You cannot get something for nothing. It is a non-sequitur to produce universal risk/reward trade-offs from a belief that investors need to be rewarded for taking risks. The second consequence is rather more far reaching. No investment, unless it is kept under the bed in cash, can be risk free. Those who want a return must hand over their assets to somebody else. If it is not a direct loan, then it is in some sort of project with risks attached to it. If companies scrap their equity and use loans instead, the new loans are just as risky as the old equities were.

The cosy world of MPT, mean variances, the CAPM and efficient markets is not the real world we know. The better financial economists—and there are some who are outstanding—have known this for some time. That is why they have moved on to more elaborate models. APT, for example, is an acknowledgment that the CAPM is wrong, because it uses only one element, the market. So APT suggests a multi-factor model, but, and I quote Diana Harrington again, "the new theory did not say anything about how the identity and magnitude of these multiple factors should be determined". Let me guess that they will use that wonderful tool called hindsight! So, the better financial economists have not only thrown out the CAPM, but they have also thrown out APT.

What next, I wonder? How long will it be before we get to the inefficient market theory? Indeed, when one of the major participants in the short-term bond market is government, in effect fixing short-term interest rates by decree, it is certain that the market is inefficient, simply because it is partly rigged. I rest my case.

Dr J. A. Tilley, F.S.A. (seconding the motion): There is ample proof that many actuaries, including British actuaries, believe the motion before us. The creation of AFIR as a section of the International Actuarial Association is a solid piece of evidence. At first blush, it would seem that the acronym AFIR, which stands for 'Actuarial Approach for Financial Risks', supports the converse proposition—namely, that financial economists' contribution to investment could be enhanced by the work of actuaries. While this proposition is also undoubtedly true, let us remember that the primary intention of the founders of AFIR was to bring together actuaries from around the globe who are interested in the analysis of financial risks.

I quote from the proposer's introduction to the four volumes of scientific papers from the 2nd AFIR Colloquium: "An AFIR Colloquium has several purposes: to introduce actuaries to the concepts of financial economics; to allow financial economists to present their ideas to actuaries; and for both to apply these ideas to the financial institutions with which actuaries are most concerned—insurance companies everywhere, pension funds in many countries and credit institutions in a few." I ask you to observe that the 2nd AFIR Colloquium was hosted by the U.K., and was chaired by none other than our distinguished opponent in this discussion, Mr Arthur. Moreover, his seconder, Mr Clarkson, played a significant role at both the 1st and 2nd AFIR Colloquia, not only by his own claim, but also in the opinion of others. Quite obviously, therefore, our opponents have a formidable task in this debate.

In support of the proposition, I would like to draw attention to two papers from the 2nd AFIR Colloquium. The paper entitled 'The Financial Actuary and the European Consumer', by Edward Levay[11] refers to actuaries of the third kind, the now familiar term that was coined by Professor Hans Bühlmann to describe the new group of mathematical experts who unfold their skills on the investment side of insurance or banking. Such actuaries have already embraced the truth of the proposition that we are debating, and the proposer and I hope that there are many of you here! The other paper, entitled 'The use of Martingales in Actuarial Work', by Andrew Smith[12], describes the generous rewards that befall those who learn the mathematics of stochastic processes and stochastic calculus. Actuaries who find such theories intimidating, and who prefer to put off or even avoid studying his paper, can begin a successful journey into the field of financial economics by way of any of the good graduate textbooks that are sparing in their use of heavy mathematics.

On 23 March 1993 I shall present a lecture entitled: 'Stochastic Investment Models in Actuarial Work: Applications in Insurance and Pensions'. In it I will highlight the uses of stochastic investment models in the areas of valuation, pricing and portfolio selection, and I will discuss pension finance and strategic asset allocation as an example of a portfolio decision problem involving the evaluation of risk/reward trade-offs.

The natural sphere of the traditional actuary is the liability side of the balance sheet, whereas the natural sphere of the investment professional is the asset side of the balance sheet. For the modern pensions actuary these two spheres have coalesced. Thus, in a modern pension finance model the evolution of both the pension cash flows and the capital markets must be simulated, so that the financial impact of pursuing a particular investment strategy can be analysed. There are two areas in which actuaries who build such models can benefit from the work of financial economists: first, in choosing realistic stochastic processes for the relevant financial and economic variables such as share prices, interest rates, and inflation; and second, in constructing appropriate efficient frontiers to evaluate the risk/reward trade-offs among various asset allocation strategies. Is it not arrogant to presume that an actuary's understanding of a pension scheme cannot be enhanced by the work of financial economists?

The evaluation of risk/reward trade-offs and the concept of an efficient frontier are topics in the subject of modern portfolio theory. In his paper, 'The Measurement of Investment Risk'[13], Mr Clarkson bludgeons MPT on many fronts, most notably on the measurement of risk. He is not alone in suggesting that variance is not synonymous with risk. Financial economists themselves have challenged that assumption, and have noted that risk is more properly associated with adverse outcomes than with beneficial ones, that is that an asymmetric measure of risk is preferable to a symmetric measure such as variance.

This is not a debate about whether everything in the field of financial economics is correct, applicable, or even practical, from the viewpoint of the actuary. It is not a debate about whether the theories, models, and tools of financial economists are incomplete or imperfect. It is not a debate about the incredible claim that all the work of financial economists must be swept away and be replaced by a completely new approach. It is most certainly not a debate about the narrow-minded view that financial economists have no role to play in improving their own theories, models and tools. Instead, it is a debate about whether there is some body of work in the field of financial economics that can enhance the contribution of actuaries in the field of investments.

The proposer has told you a story about actuarial history. His account emphasised the topic of mortality, which is fundamental to actuarial science. I want to discuss the theory of interest, another pillar of actuarial science, and, without question, the primary contribution of actuaries in the area of investments. How better to convince you to vote in support of the motion before this house, than to present an apparently straightforward actuarial example, involving the theory of interest, that requires insights from the field of financial economics before it can be solved.

We know how to compute the present value of an immediate annuity certain of £100 p.a. for a term of 5 years. Suppose that the current prices of default-free zero-coupon bonds of maturities 1, 2, 3, 4 and 5 years are £95, £90, £85, £80 and £75, respectively, per £100 paid at maturity. The present value of the 5-year immediate annuity certain of £100 p.a. is the sum of these bond prices—namely, £425. Notice that I have conveniently chosen the zero-coupon bond prices to be equivalent to a flat yield curve of 5% p.a. when yields are expressed as simple annual rates of discount. When expressed in the standard form as compound annual rates of interest, it can be seen that the yield curve actually rises from 5·26% for the 1-year zero-coupon bond to 5·92% for the 5-year zero-coupon bond.

The problem is an example in which the coin can be found under the opposer's shining lamp post, and this particular coin is well worth finding. Suppose that the annuity certain is split into two parts, which I shall call contingent annuities A and B. Under contingent annuity A, any £100 annual payment is made if, and only if, the 1-year discount rate at the time of payment is 5% or higher. Under contingent annuity B, any £100 annual payment is made if, and only if, the 1-year discount rate at the time of payment is lower than 5%. In other words, component A pays only when the 1-year yield is at least as high as it was at the outset, and component B pays only when the 1-year yield is lower than it was at the outset. The question is: what are the fair prices of the contingent annuities A and B?

This problem might seem fairly easy to solve, but too few actuaries know how to solve it. Traditional actuarial tools are not sufficient to yield the correct answer—they may yield an answer, but it will undoubtedly be wrong—even though the problem clearly belongs to the theory of interest. Indeed, for many actuaries it is a challenge to determine what information is needed before the problem can be solved. Some knowledge about the stochastic process by which interest rates evolve is needed. Do the fair prices of the annuity components depend on the views about the interest rate held by prospective buyers? Do they depend on the utility functions of investors or their wealth? Is there some notion of market equilibrium that bears on the solution of the problem? Without any further information, is it possible to determine whether component A is worth more or less than component B? The methodology required for solving the apparently simple annuity problem that I have posed is option pricing theory. Option pricing theory is an important part of the work of financial economists, and the science of computing present values is an important part of the contribution of actuaries to investment. Thus, the truth of the proposition before this house is strongly supported, if not actually proved!

Mr R. S. Clarkson, F.F.A. (seconding the opposition to the motion): "The characteristics . . . assumed by the classical theory happen not to be those of the economic society in which we actually live, with the result that its teaching is misleading and disastrous if we attempt to apply it . . . ." These words summarise very eloquently my views on financial economics, but they were written more than 50 years ago—by John Maynard Keynes[14].

Capital market theory is an immature and misguided science, with no relevance to the financial society in which we actually live, but how can the methods of financial economists be misguided when they have the support of the proposer, one of the most eminent actuaries of our time? Most of the methods which he applies to investment problems, from his essentially academic viewpoint, assume that the financial world is linear and in equilibrium. From my quite different practical and mathematical viewpoint, it is obvious that the financial world is far from linear and rarely in equilibrium.

I shall concentrate on the opposite of the motion, which I would express very starkly as follows:

"Financial economists are guilty of promoting a narrow-minded Stone Age methodology, which has no relevance to the financial world in which we actually live", and I shall produce evidence to support three specific and very serious charges against financial economists.

My first charge is that the promotion of theories involving linearity, normal distributions and equilibrium by financial economists is little short of fraudulent. Linear models are, in general, not only inefficient in terms of parameters, but also unstable. After I described in 1972 how to build a non-linear gilts model[15], linear models soon became as extinct at the dodo. The Bank of England modified their gilts model to accommodate non-linearity[16], while Professor Gordon Pepper, who had been experimenting with net redemption yield models, but found them too unstable[17], exploited versions of my non-linear model[18]. Some years later I built a non-linear equity model, which can be regarded as a more robust version of the Weaver & Hall linear regression model, discussed here in 1967[19]. Both Professor Pepper and I had experimented extensively with linear time series models of the Box-Jenkins type, but we both rejected them many years ago. When the proposer's stochastic investment model, which is built around Box-Jenkins time series, was discussed at the Faculty in 1986[20], I explained why a more robust non-linear approach was needed, particularly for the crucial inflation series, where—as the Government Actuary and numerous others had pointed out—the frequency of negative values was far too high.

My non-linear stochastic model for inflation[21] was described as follows in the 'Report on the Wilkie Stochastic Investment Model', discussed in this hall in 1992[22]: "[Clarkson's] model, which relies very much on actuarial judgement rather than being formulated within a conventional statistical framework, provides an improved fit for post-war U.K. inflation data and results in significantly fewer negative values . . . than is the case with the Wilkie . . . model." Game, set and match to the non-linear actuarial approach.

Financial economists use utility theory in much the same way as an inebriated man uses a lamp post; for support, not illumination. In his 1970 book, *Portfolio Theory and Capital Markets*[23], Sharpe admits that the use of mean and variance alone 'may suppress too much reality', and concludes that a different utility curve may be needed to compare portfolios of different riskiness. This is precisely the conclusion I drew in my paper to the 1st AFIR Colloquium[24]. The entire Markowitz methodology of mean/variance analysis then collapses.

Another glaring weakness is the use of statistical tests which assume that distributions are normal. The mean absolute deviation techniques introduced to actuaries by Jack Plymen[25] are far more appropriate for constructing confidence limits. When Mandelbrot showed, in 1963, that many financial series were nowhere near normal[26], Cootner said "If he is right, . . . almost without exception past econometric work is meaningless"[27]. In his book on fractal geometry[28], Mandelbrot exposes the statistical trickery employed by financial economists and describes some of their methods as 'suicidal'.

My second specific charge is that the general methodology of financial economists is unscientific in the extreme. Before formulating a numerical theory in any branch of science, it is essential to find out what key components are involved. The theory of heat made little progress until it was recognised that two distinct components were involved: the quantity of heat and temperature. I have shown how a measure of financial risk can be constructed very easily, once we recognise the two distinct components of impact and probability. Financial economists, on the other hand, use historic variability of return simply because it is easy to measure.

Financial economists have also been unscientific in their reliance on primitive measurement tools. A good analogy here is with the Hubble Telescope, which orbits the earth and can send back images with far higher resolution than can be obtained from any earth-based telescope. My non-linear models for gilts, equities and inflation[13] greatly improve the detail in which we can study financial series, since—for the same goodness of fit—a much higher degree of statistical stability is achieved.

Unlike theoretical physicists, who have used thought experiments to great effect, financial economists have been very unimaginative. Consider a simple thought experiment relating to market efficiency. Utility companies and high technology companies are at the opposite ends of the predictability spectrum. With utilities there is a very high degree of predictability of future profits, but with high technology stocks, not only will the long-term profitability vary significantly from company to company, but the short-term price variability will also be materially higher, since changes in perceptions of future profits can be quite extreme.

Now, consider a large sample of unit trusts, where four are high technology funds and one is a utilities fund. The proposer and financial economists say that there is a unique linear relationship, known as the capital market line, between long-term return and risk—as measured by short-term variability—and that a particular unit trust will be above or below this capital market line, depending on how well its manager has performed on a risk-adjusted basis. Common sense and actuarial training tell us that this financial fairy tale is utter nonsense. The high technology funds will have such high short-term variability that they are likely to appear in the bottom half of the ranking, regardless of their returns, while the utilities fund will have such a low short-term variability that it is likely to come out near the top of the list, regardless of its long-term return.

My example mirrors one of the so-called triumphs of finance theory, Jensen's strong level efficiency tests on U.S. mutual funds[29]. Out of 115 funds studied, the rankings of the four science or high technology funds were 76, 89, 110 and 115; all in the bottom half, and including the very bottom fund. The one utilities fund was top of the list by a ridiculous margin; its risk-adjusted return was more than $2\frac{1}{2}$ times that of the fund in second place. This lack of homogeneity completely invalidates the use of short-term variability as a proxy for risk. The concepts of the capital market line and the risk-adjusted return, alpha, have also to be thrown out. However, Jensen plodded on regardless. His so-called evidence of strong level efficiency, which was a cornerstone of the efficient market hypothesis, is, in my opinion, as extreme an example as you will ever see of 'rubbish in, rubbish out'.

My third charge is that financial economists have caused far higher levels of financial suffering than need be the case in our modern society. There are three particular dangers:

(1) The narrow-minded focus on risk crowds out the much more important activity of assessing realistic long-term values, and thereby stabilising market levels and reducing the scope for financial carnage on the scale that occurred in December 1974 and October 1987. We need to pay more, not less, attention to classic investment papers such as 'Cyclical Changes in the Level of the Equity and Gilt-Edged Markets'[30].

(2) Recent variability seriously understates the true level of risk, because it does not incorporate a downside element for disasters. It is like an architect in an earthquake zone designing an office block on the basis that a major earthquake cannot happen, since there has not been one recently.

(3) Even when a financial earthquake has occurred, financial economists often pretend that it has not. The proposer, for instance, largely suppressed the collapse in U.K. markets at the end of 1974 when fitting his stochastic model, but this quite amazing 'rejection of outliers' is an accepted part of the narrow-minded Box-Jenkins methodology.

I have shown beyond any reasonable doubt that financial economists are guilty on each of these three specific charges. Being guilty on any one would be enough to justify my opposing the motion, but being guilty on all three should not leave any doubt in your mind.

I now consider the items in the introductory notes:

(1) Utility theory, as presented by financial economists, must be thrown out. It is essentially linear, and even von Neumann and Morgenstern[31], who started it all, admit that their crucial first axiom may be 'neither appropriate nor even convenient'.
(2) Portfolio selection models are also out. They involve, not only utility theory, but also the financial economists' idiotic definition of risk.
(3) The CAPM and beta must go as well. They involve the fairy tale called the capital market line.
(4) APT, as an elaboration of the CAPM, is just as conceptually flawed.
(5) Most time series models are linear in nature, and hence highly suspect.
(6) Stable Paretian and similar distributions are closer to reality than normal distributions, but, unlike actuaries, financial economists have not yet found the tools to handle them.
(7) The binomial model is a very crude approximation to reality.
(8) Most asset/liability models equate risk to variability of return, and so must go.
(9) We are left with nothing more than the Black–Scholes model. It can stay, but only because we can derive it more simply using my measure of risk, as I showed in my paper to the 1st AFIR Colloquium[24].

The opposer has used his wide experience as a consulting actuary to ridicule the financial economists' methodology. I have used my experience as a professional investor and my training as a mathematician to warn you about this Trojan Horse of statistical trickery. Why have these methods not been discredited long ago? Keynes might have summed it up in the same way as he described Marxism[14]: "It is amazing that a doctrine so illogical and so dull can have exercised so powerful and so enduring an influence", but we all know how rapidly the Communist regimes in Eastern Europe disintegrated, once the people realised that there was a better way.

There is a better way than that of the financial economists. Their definition of risk leads to absurdity, whereas my measure of risk leads to semi-variance as a very special case and to the Black-Scholes model. Actuaries already know that mean absolute deviation techniques give a very powerful tool to handle distributions that are not in equilibrium and are nowhere near normal. Actuaries can handle non-linearity with ease, but this is an area in which financial economists are still in the Stone Age.

To help educate them, we have here, with acknowledgment to Professor Pepper, who first suggested the idea, a physical model of our non-linear financial world: a twisted rope ladder (with wooden rungs). Any small area is approximately 2-dimensional, but over its whole length it is clearly 3-dimensional, and in statistical terms any attempt to describe it as linear is—to quote Mandelbrot—'suicidal'.

I enjoyed the proposer's history lesson about Markowitz, but in the investment world you have to keep abreast of the times. He made no reference to the recently published second edition of Markowitz's 1959 book, *Portfolio Selection*[32], in which the author states that, when asked by one advocate of semi-variance (the downside measure of risk), why his 1987 book made no mention of semi-variance, he replied that, while semi-variance was the more plausible measure, the strengths of variance were the reduced computer resource requirements and the success of mean-variance utility approximations. He then pointed out that mean-semi-variance approximations could be perceptibly better, and that computer costs had fallen markedly since 1959. Financial economists, having sold investment practitioners 'down the river' with variance, are 'up the creek without a paddle', now that Markowitz, the grandfather of it all, has reverted to favouring the downside measure of semi-variance.

Nothing that the proposer said can counter my demolition of the basic theories of financial economists. All they have done is to promise that their new, improved, 'ingredient Z' time series methods, about to be elaborated, will do better than the basic methods.

If the foundations of financial economics are rotten to the core, anything built on them can only be just as unsound in theory and just as dangerous in practice. I am reminded of an elaborate, but desperate, mathematical argument put forward in October 1957 by one of the last surviving members of the Flat Earth Society, who was trying to show that the newly-launched Russian Sputnik proved once and for all that the earth was flat. The proposers of the motion are inviting you to join the financial equivalent of the Flat Earth Society. A new approach is needed, which is both non-linear and dynamic, and the U.K. actuarial profession can provide it. The necessary building blocks are already in place, and are well documented in our actuarial literature. However, you must first throw out the preposterous motion before you, or we will all be dragged back into the financial Stone Age. In doing so, you will send out as unmistakeable a message to the financial community as the tearing down of the Berlin Wall sent out to the world in 1989, when Marxism, having failed to live up to its empty promises, was swept aside to make way for a better approach.

Mr J. Plymen: I quote from the Introduction to Chapter 1, of the 1991 edition of Markowitz's book[32]. "This monograph is concerned with the analysis of portfolios containing large numbers of securities. Throughout we speak of portfolio selection rather than securities selection. A good portfolio is more than a long list of good stocks and bonds." The objective of Markowitz was to determine policy, that is the proportion of bonds and common stocks to be held in the portfolio, not to select individual shares. Most people who read Markowitz think that he was talking about an all-equity portfolio, but this is not the case. American portfolios, at that time, contained a high proportion of bonds.

Between 1960 and 1965 I was actively engaged with the Society of Investment Analysts, in charge of the Computer Commission, which produced a report every year on the progress of computerised investment models. My first report described Markowitz's book[33] as an interesting academic exercise, but with no practical value. The next year, in accordance with my normal practice, I wrote to all the participants in the earlier report, asking them to what extent their models had been developed, and asking about further progress. Markowitz wrote back some time in 1962 to say that there had been no developments regarding his model; that he had given up any interest in finance, and that he was employed by a computer company. This is in agreement with the last page of his 1991 book, where he said: "Having written out in 'Markowitz 1959' what I had to say about portfolio theory, my own interest had wandered elsewhere; namely, at that time the design and development of a theme script programming language to facilitate the building of discrete event simulators." I do not want to go into detail about what that means; but the fact is that Markowitz has never been in sympathy with the CAPM. There is no mention of the CAPM in his book.

There is no doubt that, on theoretical grounds, risk would be better represented by the semi-variance, but, of course, variance is much more convenient to use when mathematical analysis is needed. In his 1991 book, Markowitz devotes a whole chapter to the dilemma between the use of variance or semi-variance. In Chapter 10 he suggests that the use of utility functions may, to some extent, correct the error arising from the use of variance alone. I have little doubt that the qualms which Markowitz had, regarding the use of variance, precluded him from participating in the next phase of the development on his work, the CAPM.

I maintain that Markowitz committed a major howler in his original work in his method of measurement of risk. Risk has several components. There is the disaster risk, namely that of a company disappearing completely, and there is the remaining risk. Markowitz assumed that the returns given by past statistics were gross of risk, and he estimated returns gross of risk. When dealing with disaster risk, this assumption is incorrect, as the returns are calculated from the experience of surviving stocks, the disaster having disappeared from the statistics. In the majority of healthy companies the disaster risk is small. Ignoring for present purposes the disaster risk, the normal cost of under-performance is included in the lower price of the shares in the last 5 years. There is no way in which the statistic can be used to produce the performance gross of risk. In the same way, if you are assessing the performance by investment analysis, you cannot get the performance gross of risk. If you accept the fact that performance has to be measured net of risk, then the risk is implicit in the performance. There is no measure of risk, and there is no way in which the risk can be analysed. The whole CAPM analysis of risk falls down, because you do not have any risk to measure.

I now consider that financial economists' sole interest is in their own finances, not in those of their clients. Sharp and his followers saw the equity market going up like a rocket, and became tremendously interested in the selection of equities, because they wanted to make money out of it. They developed the CAPM, throwing aside all Markowitz's qualms about the risks. They perpetrated a tremendous confidence trick, persuading the investment community that it was a waste of money to spend it on research, and that it was much better to spend it on fees to experts of MPT. Justification of an MPT service has been made by pointing out that in the U.S.A. \$200m p.a. is expended on fees for MPT services, showing how MPT has been successful in making money for its sponsors, but not for its clients.

Mr J. M. Pemberton: There is an extensive literature of the highest quality on the methodology of economics, which impinges directly upon this motion. Financial economics rests crucially upon the use of idealised models—models defined by reference to simplifying assumptions. These assumptions are recognised as false. Milton Friedman justified the use of such false assumptions in his famous 1953 paper, 'The Methodology of Positive Economics'[34], but that paper has been criticised in its own terms by Ernst Nagel[35] and others. More recently, the positivist philosophical base upon which the Friedman paper rests has collapsed.

A new orthodoxy within scientific methodology is causalism, which rejects Humean attempts to reduce causes to regularities, and insist that causes be taken seriously. We need causes and not just correlations. Nancy Cartwright, a leader of the causalists, asks not "Are false assumptions acceptable?", but rather "How do models relate to reality?"[36] This debate is not well known amongst actuaries—many of the ideas may seem arcane to members of our profession, but these are precisely the ideas we require to address the tension between the methods of financial economics and those of actuarial science.

Since Black & Scholes published their celebrated paper[4], it has been widely accepted that one of the finest examples of financial economics is its solution of the option pricing problem. However, it is now generally realised by practitioners that things are not that simple. When they value options, they now revert widely to the use of discounting techniques. Such techniques are, of course, actuarial.

The concern that the idealised models of financial economics lose touch with reality is practical as well as methodological. It is a concern well articulated by the opposer. The debate concerning the use of idealist techniques is fundamental to our science—we are increasingly adopting such methods, not just within the investment arena.

The ability of economics to predict effectively has long been in doubt, but that ability is essential to actuarial science. The debate concerning the use of new techniques is, therefore, crucial. The methods of actuarial science and financial economics are in conflict, and the case for discarding our traditional methods is not proven. In recognising the tension between financial economics and actuarial methods, the members of this profession are uniquely well placed to play a constructive role in the wider methodological debate. My consideration of the methodological issues leads me to believe that actuarial techniques are precisely those which are required to solve many problems traditionally dealt with by economics. The potential for application of our science is far wider than has traditionally been supposed. Far from importing the methods of financial economics, we should be exporting the methods of actuarial science.

Mr N. F. C. De Rivaz: About 7 years ago I did a considerable amount of work on investment theory with a colleague who adopted a more empirical approach to developing realistic stochastic models. I was searching for a sound theoretical basis for the work as a foil to the interesting, but unpredictable, results emanating from the stochastic model; and in doing so I stumbled across a fact that gave me great insight into investment risk. A chapter in Markowitz's original work on portfolio theory[33], entitled 'Return in the Long Run', shows, as I did, that investment returns in the long run will be distributed about the geometric mean. Every mathematician knows that the geometric mean of a set of numbers is always less than or equal to the arithmetic mean. In fact, there is a direct relationship between the riskiness of an investment and the amount by which the arithmetic mean exceeds the geometric mean.

I now give a simple example which is easy to prove, but which I have not seen referred to in the literature. If we have an investment whose log return is distributed normally with mean g and variance s^2, then $\log(1+a) = g + \frac{1}{2}s^2$, where a is the arithmetic return. I found this relationship to hold to within 1 part per 1,000 for the 66-year history of the BZW equity index, normalised for inflation. The geometric return is, of course, the result of raising the log return to the power of e, that is $\exp(g) = 1 + \text{geometric return}$. By taking logs, it is obvious that the long-term return is equal to the log return, provided that the time periods are independent and identically distributed. The law of large numbers takes care of the rest, as the mean of the sum of independent random variables will tend to the normal distribution. The interesting feature of this result is that any investment policy which chooses to do anything other than maximise the log return will almost certainly be defeated by one that does. It, therefore, seems an eminently suitable strategy for actuaries interested in long-term investment.

In 1991 Markowitz reprinted his original book with notes[32], and in these notes he refers to two papers by Samuelson, published in the early 1970s[37,38], entitled 'The Fallacy of Maximizing the Geometric Mean in Long Sequences of Investing or Gambling'. However, I am not convinced by Samuelson's arguments, and nor, it seems, is Markowitz. He feels that the policy of maximising the log return should be known as the long-run growth maximising policy, and any departure from this should be justified.

As I have shown, there is a direct link between the geometric return and the arithmetic return that relates to the variance for a normal distribution. The calculation of the log return gives a more natural understanding of the riskiness of more complicated distributions with, say, small, but significant likelihoods of making very heavy losses. It seems that Markowitz only moved into using the variance because it was easier to handle mathematically, and he found an approximate formula linking the mean and variance to the log return: $g \doteqdot \log(1+a) - \frac{1}{2}\text{variance}/(1+a)^2$, with g and a defined as before. A return to fundamentals could do much to avoid the increasingly confusing discussion of variances, semi-variances and complex utility functions.

Mr S. J. Green: Dr Chaim Weizmann, the first President of Israel, tells the following story in his autobiography. Before the First World War, when he was Professor of Chemistry at Manchester University, he used to have tea parties every Sunday for his junior colleagues. One of these was Dr Marie Stopes, who was the pioneer of birth control. She would arrive and say, "Dr Weizmann, I have just made a startling discovery", and she would tell him all about it. A few weeks later, she would arrive and say, "Dr Weizmann, I have just made another startling discovery." Dr Weizmann asked, "What was it?" The reply was, "The discovery I made a few weeks ago does not work." That sums up for me the history of financial economists.

The efficient market hypothesis is generally credited to Fama in his classic paper of 1965[39]. In 1988, in a paper written jointly with French[40], he wrote, "there is much evidence that stock returns are predictable"; in other words: "the efficient market theory is a lot of tosh even in its weakest form".

Another person closely associated with efficient market theory was Professor Barr Rosenburg. He created a commercial company, Barra, which produced programs to replicate indices, and which laid the foundations for index-tracking portfolios. Barra has been very successful, but over the years the company discovered that markets were inefficient, and so it began to offer programs based on these inefficiencies. There were alpha funds and tilted alpha funds which outperformed the indices. Barr Rosenburg has now founded a successful quantitative investment management company which, as far as I can tell, invests mainly on the basis of market inefficiencies. So, two main proponents of the efficient market hypothesis now say that it is untrue.

Just listen to what the proposer said in the discussion of the paper, 'Allowing for Asset Liability and Business Risk in the Valuation of a Life Office'[41], "the assumptions on which the basic CAPM are based are many, and are wholly unrealistic", and, "papers have been written discussing how the strict conditions of the basic CAPM can be relaxed".

In a paper presented to the 1st AFIR Colloquium[42], Mary Nisbet wrote, "both types of [option pricing] model were derived on the assumption that the markets for shares and options are frictionless", meaning that there are no expenses. Try telling that to your stockbrokers or your market makers! The quotation continues: "and that the underlying share pays no dividend during the life of the option. Much subsequent theoretical ingenuity has been employed in attempts to reconcile the real world of market frictions and dividend paying shares with the ideal world of option pricing theory".

Mr A. J. Wise: Mr Tilley's definition of the argument in favour of the motion, that financial theory should aid the actuary in his work, is valid. As an illustration, Mr De Rivaz said that it is better to look at the logarithm of investment returns over a period, because compounding produces log-normal distributions. However, if the individual assets are log-normally distributed over a period of time, then the distribution of the composite portfolio is not. You can add together normal distributions to get a normal distribution of a portfolio outcome. You cannot add together log-normal distributions in a similar way.

If you cannot add together log-normal distributions, is there some way of combining assets in such a way that a log-normal distribution applies to the portfolio? The answer is yes, if a portfolio is invested, say, 70% in equities and 30% in bonds, and rebalanced constantly throughout time, so the proportions by market value do not vary. The distribution of the portfolio outcome is log-normally distributed, given that the elements are so too. That is interesting, but then what is the overall return on the portfolio distribution? I believe that few actuaries know the answer to this question, but actuaries who are advising on investment policy ought to know what the formula is for calculating the rate of return on a portfolio using constant rebalancing of proportions throughout, given the individual sector returns. So far as I can see, stochastic calculus and the geometric Gauss-Weiner process are needed in order to arrive at the answer. That supports the motion, because actuaries need financial theory just as mathematical physicists need pure mathematics in order to get to models of the real world.

Professor G. T. Pepper: I am a Professor at City University Business School, where I am Director of the Centre for Financial Markets, and define myself as a financial economist. I consider that the proposers have been incompetent, in that they have not defined what they mean by 'financial economist'. Therefore, I am able to choose my own definition, which is that a financial economist is a believer in the capital markets theory of the efficient market hypothesis. The efficient market hypothesis is correct. The deduction from it, that all price movements are the result of unexpected news announcements, is incorrect.

I now refer to Pegler's paper, 'The Actuarial Principles of Investment'[(43)], where he states that investments should be such as to maximise the expected yield for the minimum risk. I wish that he had reformulated that to maximise the expected yield for the minimum risk of loss and the minimum volatility. If you have two investments, with identical expected returns and identical risk of loss, most investors would prefer the investment with the more stable return to the one with the more volatile return. The majority of academic financial economists fail to distinguish between volatility (or stability of return) and risk of loss. It is far worse than that: I made a fool of myself at the University because I failed to understand the conventional academic definition of risk. Academics can use words to mean something different to everyday usage, and the result can be most misleading. In academic terminology, the return on an investment is said to be *uncertain* when probabilities cannot be attached to the various possible outcomes. *Uncertainty* includes above-average as well as below-average return. The word *risk* is used when probabilities can be attached. In academic terminology, there can be a *risk* of above-average return. Also in academic terminology, risk of loss in normal usage is *downside uncertainty*.

The proposers ought to have defined risk. The misuse of English words is responsible for the failure of communication.

Mr A. D. Smith: The opposer has provided an example which well illustrates the motion. He considers a U.K. investor who is considering buying dollars, and thinks that, because of the higher risk, he should get a higher expected return. Then he considers a U.S. investor who thinks the same thing about sterling, and concludes that the theory is preposterous, because they cannot both get a positive expected return. Let us suppose that both investors agreed that, with a probability of half, sterling was going to depreciate by a factor of two relative to the dollar, and that, also with a probability of half, sterling was going to appreciate by a factor of two relative to the dollar. The person who has invested in dollars then looks at his portfolio. With a probability of half, he has half what he had before; and with a probability of half he has double what he had before. The expected value is $1\frac{1}{4}$. The U.S. resident does the same calculation with sterling, and calculates the same answer, $1\frac{1}{4}$! Both are bigger than one.

This example shows how actuarial intuition can be applied beyond its bounds of validity to produce false conclusions. It strongly supports the case that we need to go back and do some rather more detailed mathematics—and that includes financial economics.

Mr P. J. Lee: My comment is rather similar to that of Mr Smith. I should like the opposers of the motion to tell us how they would use traditional actuarial mathematics to calculate the value that actuaries making pension fund valuations should place on limited price indexation (LPI). If they are not to use a stochastic investment model, which I take as coming from the realm of financial economics, how are they to do it using traditional actuarial mathematics?

Mr P. A. Randall: If we take too literal an interpretation of the motion, as Mr Tilley did, we are bound to vote in favour of it. Financial economists could surely enhance the work of actuaries in some fashion or other.

The contribution of actuaries to investment has been essentially practical, for the sound and effective management of financial institutions, underpinned by a number of theoretical developments. It has been based on keeping our feet placed pretty firmly on the ground. We move away from that at our peril. We should interpret the motion as a choice between taking the corpus of actuarial work to date as the starting point for our work for the future, or switching horses and attempting to adapt financial theory to actuarial applications. I do not believe it would be in our interests to take that latter course.

Mr N. F. C. De Rivaz: When I referred to the log-normal distribution earlier in the discussion, I oversimplified things, but I think that sometimes this helps people to see the wood despite the trees. I was satisfied, as an actuary, that I was taking a margin in my favour, because I felt that somehow the market bounced back a little, but over an annual interval I could not detect it. The assumption of independence would, therefore, overstate the riskiness. It is quite clear that, over shorter intervals, particularly over weeks, days or months, and maybe years, there is obviously more structure to the market that should be taken into account. I now realise that there are much better modelling tools available to work over shorter time periods of up to a year or so, that begin to capture the market's non-linear dynamics.

I have made my own specialisation in fractal analysis, chaos theory and neuro-computing. I let the computer do the work. It is now possible to analyse economic time series in far greater depth than before. Actuaries need to understand some of these new tools. Maybe we do not need to understand how they work, but we need to understand the concepts behind them, and develop practical approaches to using them. It is far too easy for someone who is not a statistician to over-analyse a series, leaving very little freedom in the model, which was my concern over the original stochastic models I was building. I made this mistake again recently when I fitted some data to a new neural network that I am working with. It was beginning to learn quite well, and then it learned a little bit too well—in fact it was obvious that it was using considerable hindsight!

One of the strengths of the actuaries' approach has been to build bases which are capable of explanation and replication. Would it be too much to look for a stochastic model that was built up from believable hypotheses that both fitted the market data and also gave us real insight into market behaviour?

One of the techniques used in the area of strategy is systems dynamics, where a group of managers get together and explain how their business works. A sort of flight simulator can be created for them, using a computer, which gives a feel for the market. I have developed a very simple model of investment markets to show how complex behaviour can result. It has two types of investors: long-term investors—I suppose that includes actuaries—the good people who invest when things are cheap and sell when they are dear; and short-term traders, working on the market's momentum, who sell if prices are going down and buy if they are going up. Even a simple model like this can create some very interesting behaviour.

We need to do two things. First, we need to adopt a much simpler concept of risk. I have made a suggestion about that. Second, we need to start to use some of the new tools to analyse historical data in much more realistic ways. We should take the market's non-linearity into account explicitly, and then build conceptual and understandable models of behaviour which help us to analyse short-term market movements. By taking out the short-term market movements, which are chaotic rather than random in nature, we will be able to uncover the true underlying randomness.

Mr S. J. Green: Mr Tilley has used the expression 'default-free'. Part of MPT specifies that there should be risk-free investments. I have not yet come across an investment which is default-free or risk-free. Government stocks have, in the past, failed to be repaid on the due date. If they have been repaid by the Government, the bank may have lost the payment. That is not default-free. It is late, and once it is late, the calculations are wrong. For actuaries there is no such thing as a risk-free investment.

When Peter Moore produced his paper some 21 years ago on MPT[44], I criticised it because it provided only a snapshot. What we needed is the equivalent of a video camera. I am prepared to accept any mathematics which gives me that extra dynamic effect. At the moment, everything that I have seen that has come from financial economists is too flat, too two-dimensional.

Mr T. G. Arthur (the opposer, summing up): Mr Plymen supported us, and pointed out that Markowitz was specifically concerned with another age. In particular, he was looking at a bond/equity ratio, and he may well have done the equivalent of turning in his grave if he could see what use was going to be made of his theories later on. Mr Plymen also reminded us of a very practical consideration, that personal career advancement is never far away, but I would not like to attribute a monopoly of that to financial economists.

We also had support from Mr Pemberton, who was arguing for a sound philosophical and methodological basis before looking at correlation. I fully support that. My idol, von Mises, has pointed out that quantitative approaches to economics are limited. Every change shuffles the whole price array. The most powerful tool is true understanding. Any event can have an infinite number of mathematical explanations. The same applies to interpretations of history. You should be a good historian before you make interpretations of historical events. I thank Mr Pemberton for that.

Mr De Rivaz made some interesting points about the difference between geometric and arithmetic means. He also referred to a new science called fractal analysis, which Mr Clarkson knows much more about than I do, and he might like to suggest a study of his non-linear inflation model. We also received support from Mr Green, who tore away some of the Emperor's clothes in suggesting that we get one discovery after another. He also told us that Barr Rosenburg has indeed repented, as I hoped.

Mr Wise, probably feeling sorry for the proposers, pointed out that financial theory is needed. The question is not whether or not financial theory is needed; we know that, but what we are criticising is a body of theory which has been identified broadly with the subject of financial economics to-date. Constant rebalancing is rather impractical. Henry Ford once said that if you have a complicated answer to a problem you still have a problem.

Professor Pepper defined financial economists as people who believe in capital market theory. He pointed out that risk is primarily because of downside potential, and sometimes downside catastrophe potential. I am sure that Mr Smith will correct me if I am wrong, but I believe he ascribed the same probabilities to something doubling and something halving. I support Mr Randall's point that we are not saying that financial economics has not produced anything of use to us at all. What we have been offered so far does not help us very much. That is the position we are taking.

Mr Lee asked us how we would deal with LPI without using stochastic models. The difficulty we have is that we want to use stochastic models. It is simply a question of which stochastic models we use, and the foundations upon which they are built. Mr Tilley gave us a clever example about interest rates and discounted cash flow to show that actuaries need some knowledge of stochastic processes. Of course we do, but this is not the question; the relevant question is whether the theory of financial economics is realistic. Risk is volatility, say the financial economists. Cash is risk-free. Dividend policy has no effect on share prices, and pigs can fly!

The proposer suggested that we look at the work of von Neumann and Morgenstern[31], the theory of games and economic behaviour. Mr Clarkson has looked at that, and has written a paper to be presented to the next AFIR Colloquium, entitled, 'Some Observations on the Theory of Games', which suggests further that utility theory remains fatally flawed. The time series model put forward by the proposer—and no doubt he will try to improve it—has itself received some criticism. Professor

Harvey of the London School of Economics has reviewed the model, and has said that the methodology is essentially as advocated by Box & Jenkins[45]. This methodology is less popular than it was, and it can be particularly difficult to apply in a multi-variable context. It makes the whole modelling procedure more complex than it needs to be, and it can lead to formulations of lag structures which are not easy to handle. Of course, better models will become available, but they will not all come from financial economists, and I think that Mr Clarkson's non-linear inflation model is probably the best we have at the moment.

If the proposers want to win this debate by defining actuarial science as a sub-set of financial economics, then so be it. However, so far as valid financial economic theory is concerned, the reverse is probably true. If financial economists want to be useful, I invite them to join us in developing genuine non-linear risk theory and asset/liability coordination, based on the premises and models we have already formulated and in going forward from there.

I am confident that you will support us by rejecting the motion.

Professor A. D. Wilkie (the proposer, summing up): You have heard the opposers quote from various financial economists who got things wrong; but I do not think that they have referred to anything written more recently than 1963. Mr Plymen quoted Markowitz. His books are very good mathematical explanations of portfolio selection, but they are nothing more than that.

The opposer quoted from *The Economist*. I do not think any of you believe all that you read in newspapers, never mind in *The Economist*. However, we have not been talking about economists in general, but about financial economists. Professor Pepper knows perfectly well what we mean by the terms 'financial economist' and 'financial economics', although he pretended that he did not. I refer you to the journals and the textbooks quoted in the background reading.

Mr Green would like actuaries to look at three-dimensional models, but traditional actuarial work has not progressed beyond one. It is hard enough getting most actuaries to take the step from one to two dimensions, never mind three. By moving from one to two, I mean looking at the second moment of a distribution rather than just the mean. Here I am thinking of conventional actuarial work, and not the more advanced actuarial work that is going on at present. Traditional actuarial work uses just one expected value for the interest rate, one expected mortality table, and so on. Not all traditional actuarial work is wrong, nor is all financial economics right, but there is a great deal in the world of financial economics that actuaries would do well to look at, that is distinct from the naive financial economics that I criticised earlier on.

There is much that we can learn from chaos theory and from the use of neural networks, and quite a lot of people know the difference between log-normal and normal distributions, not only financial economists, but actuaries too. Mr Wise and Mr De Rivaz pointed out that, if you assume log-normal distributions for the return on the elements within a portfolio, and add them together, then you get problems, because the distribution of the total return is neither normal nor log-normal. That point has already been taken up by financial economists, who are well aware of the elementary definition of risk as variance. If the distribution is symmetrical or has only two parameters, then the mean and variance are the only two parameters available! There are many articles about this in the financial economics literature, and before we start throwing stones so vigorously in that direction, let us find out what has been said, and learn from what has been done.

Non-linear is an adjective I understand, but I do not understand it in combination with the nouns to which it has been applied. What is non-linear risk theory? If you have a portfolio of two investments and you know the return on each, what do you do except to add them together? That is a linear combination of results. Financial economists are not saying that everything else is linear, that utility curves are straight lines or that distributions are straight lines. They are well aware of the complexity of the economic world.

They are also aware of the complexities introduced by transaction costs. It is normal practice to begin with nice assumptions. First, let us assume there are no transaction costs, and then let us assume that there are transaction costs and find out about their effects. Some of you may have learned something about mechanics at school. Let us assume there is no friction, or let us assume that there is no wind resistance. Now, separately, let us assume we are firing a bullet or throwing a particle into the air, and there is real air and real wind resistance. People making aeroplanes do not assume that the naive models, where there is no wind resistance, apply, because that is not helpful. Aeroplanes do not fly if there is no wind resistance. So just as physicists or engineers have less elaborate models and more elaborate models, simple ones that you are taught at school and more elaborate ones that you use in practice; so financial economists have simple models and elaborate models. Unless actuaries understand what both the simple and the elaborate models are about, we will find that—like the doctors in my story—we will be left behind by those who actually know a great deal more about it.

I hope that many of you will realise that there is something that we ought to be learning from financial economists, and that the contribution of actuaries to investment could be enhanced, and should be enhanced, by the work of financial economists. I hope, therefore, that you will vote in favour of the motion.

The President (Mr L. J. Martin): All those in favour of the motion, "This house believes that the contribution of actuaries to investment could be enhanced by the work of financial economists", please indicate your support. Thank you. All those against the motion, please indicate your opposition. Thank you.

The motion is clearly carried. Thank you all very much.

I should like to leave a thought with you. Professor Wilkie said that we should preserve our mathematical skills and retain our mathematical heritage. I hope that that is one thing that we all agree upon.