B.A.J. 5, I, 115-196 (1999)

THE METHODOLOGY OF ACTUARIAL SCIENCE

BY J. M. PEMBERTON

[Presented to the Institute of Actuaries, 26 October 1998]

ABSTRACT

This paper considers actuarial science within the context of the framework provided by the formal study of scientific method. A review of key points of recent developments within the methodology (study of method) of science and the methodology of economics is presented. A characterisation of actuarial science and its methods is then developed using as inputs the United Kingdom actuarial education syllabus and recent work of the profession, most notably Bell *et al.* (1998). The methods of actuarial science are then considered within the framework provided by formal methodology to propose an articulation of the methodology of actuarial science. This methodology is explored in relation to that of other sciences, and some of the implications and opportunities for actuarial science which arise from this investigation are identified. The paper concludes that actuarial science has a distinctive and potentially powerful empirical method of applied approximation. This methodological analysis is intended, in part, to add to the momentum of the programme concerned with furthering the use of actuarial methods within broader spheres (e.g. Nowell *et al.*, 1996).

KEYWORDS

Actuarial Science; Methodology; Philosophy; Method; Science; Economics; Actuarial; Causalism

CONTACT ADDRESS

J. M. Pemberton, B.Sc., F.I.A., Prudential Corporation plc, 142 Holborn Bars, London EC1N 2NH, U.K. Tel: +44(0)171 548 3530; Fax: +44(0)171 548 3555; E-mail: John.Pemberton@Prudential.co.uk

1. INTRODUCTION

1.1 *Overview*

1.1.1 The formal study of scientific method, the methodology or philosophy of science, has historically developed as a discrete subject. Whilst it has had close and active links with the natural sciences, it has historically been somewhat isolated from developments within the social sciences. Over the past twenty years this position has changed radically, especially within economics, with the emergence of a major sub-discipline focused on the study of economic methods. This methodological work is important for the development of effective scientific methods — it is now appropriate that actuarial science should develop similar analysis.

1.1.2 The actuarial profession has engaged in its own debates concerning methods during recent years. The ideas which have been developed within the methodology of science and economics have great relevance for these actuarial debates, providing a valuable framework within which the ideas can be addressed.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Some of these debates have been concerned directly with the relationship between the methods of actuarial science and mathematical economics.

1.1.3 This paper provides an overview of some of the key ideas from recent developments in the methodology of science and economics, and begins the task of setting the actuarial debates within this broader context. A consideration of the United Kingdom actuarial education syllabus is used as a starting point for characterising actuarial science. Over the past couple of years actuaries in North America have made advances in developing a systematic characterisation of the role of the actuary which provide a further helpful input (Bell *et al.*, 1998).

1.1.4 The aim of this paper is to articulate the distinctive characteristics of actuarial science, and to show how these can be used to further the programme within the profession which is aiming to clarify the nature of actuarial methods and to extend their use. The ambition is to initiate a debate on these matters. The paper is aimed principally at actuaries, but it is hoped that it will also be of interest to philosophers, especially those concerned with the social sciences.

1.1.5 To achieve its aim, this paper necessarily introduces much material from methodology that is unfamiliar to most actuaries. This material is principally contained in Sections 2 and 3. Actuaries who prefer to start with the meat of the paper might prefer to skip forward to Section 4, and refer back to these earlier sections as reference. This approach may, unfortunately, make some of the following terminology and concepts rather difficult.

1.2 *Outline of the Paper*

1.2.1 Section 2 summarises some recent developments in the methodology of science, highlighting the lack of agreement about the nature of science which has arisen since the collapse of positivism during the 1960s. Section 3 summarises some recent developments in the methodology of economics, highlighting how the collapse of positivism leaves the neo-classical tradition on uncertain ground. These summaries do no more than give an indication of some of the key issues within recent developments, and point to the depth of the investigations within these areas which are relevant to current debates within the actuarial profession.

1.2.2 Section 4 uses, *inter alia*, a consideration of the education material for U.K. actuaries and the recent work from North America (Bell *et al.*, 1998), to help develop a preliminary characterisation of the methods of actuarial science.

1.2.3 Section 5 explores the nature of actuarial methods against the backdrop of these broader methodological discussions, in order to propose an account of the methodology of actuarial science as an empirically-based science which uses low-level generalisations to build bottom-up approximate models.

1.2.4 Section 6 compares the methodology of actuarial science with that of economics and other sciences, showing the important distinctions between these methods and noting the need for caution in mixing them.

1.2.5 Section 7 discusses a specific area of application of economic and actuarial methods to provide a more concrete illustration of the contrasting approaches.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

1.2.6 Section 8 outlines some of the important implications and opportunities which arise from a consideration of the articulation of actuarial methods that has been developed.

1.2.7 Section 9 concludes that a methodological analysis of actuarial science is an important element in supporting the ambition to clarify the nature of actuarial methods and extend their usage.

2. Some Recent Developments in the Methodology of Science

2.1 *Introduction*

2.1.1 The philosophy of science, the formal study of the methods of science, is a vast subject with a history stretching back over two thousand years. Although it is intimately related to developments within science, it is largely a distinct area of study. The highly technical language and the self-referential nature of the subject, together with the intrinsically complex nature of many of the ideas, mean that this area is not easily accessible. This section attempts to make comprehensible the ideas from this area which have relevance to a consideration of actuarial methods, whilst avoiding excessive technical background. This implies steering a careful course between simplification and complexity. The focus is principally on the developments of the last 40 years, which may be presented as three main waves of thought: positivism and its demise in the 1960s; and two subsequent revolutions: firstly that of the sociology of science, which started in the late 1960s; and secondly that of causalism, which arose in the 1980s.

2.1.2 Although there is no space here to provide a full review of developments within the methodology of science, this section does aim to provide an indication of many of the key issues and sufficient references to the literature to allow those interested to explore further.

2.1.3 Introductory texts which are likely to prove helpful for actuaries include Blaug (1980), Caldwell (1994), Hausman (1992, Appendix) and Huber (1996, Chapter 5). Of other more general introductions to the philosophy of science, O'Hear (1989) is helpfully concise. The first half of Ian Hacking's book on 'representing' provides a critique of some recent developments from a specifically causalist perspective (Hacking, 1983).

2.2 *The Positivist Tradition and its Demise*

2.2.1 Influenced by great successes within the physical sciences, especially the development of relativity, positivists were great optimists about the potential power of science. Their major ambition was to identify a set of claims whose truth is certain, and to use these as the basis for science. It was proposed that certainty could be achieved in claims which relate to immediate sensory experience. Ernst Mach, one of the major influences on early positivists, dismissed the notion of intermediate objects (such as books, tables, etc.),

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

maintaining that all phenomena are reducible to statements concerning immediate sensations.

2.2.2 Positivist thinking was developed with the incorporation of formal logic as a device for clarifying the structure of claims. Logic became the second pillar, alongside sensation claims, in the programme to reduce claims to an objective basis. Much of this development rested on the great advances in formal logic produced by Bertrand Russell and Alfred North Whitehead (Russell & Whitehead, 1910). With the incorporation of logic in the first part of the twentieth century, the movement became known as 'logical positivism' and then 'logical empiricism'. It rose to become the orthodox school within the philosophy of science, dominating thinking in this area until the 1960s.

2.2.3 In its early and more radical form, positivism claimed that propositions which cannot be reduced to logically structured observation claims are not subject to the possibility of objective verification. The early positivists insisted that such claims failed the 'verifiability principle', and should thus be regarded as meaningless or metaphysical. Propositions banished to this class included claims concerning mental states such as expectations, beliefs or feelings. Unfortunately, such a verifiability principle also rejects the use of universal claims such as "all ravens are black", since no set of observations would verify such a claim. This was a serious problem — an attempt to undertake positivist science without such universal claims was a non-starter.

2.2.4 Many later developments are driven by a desire to address this problem. One way forward, adopted very widely by later positivists, was to regard propositions as scientific (as opposed to meaningless) if they were subject to empirical tests. Where the propositions were tested and succeeded in passing, the test gave rise to evidence that was taken by the positivists as encouraging a belief in the tested proposition. How a proposition became increasingly confirmed by progressive testing became the subject of 'inductive logic'.

2.2.5 The primacy given by positivism to claims concerning observables raised questions concerning the meaning of terms which referred to entities which are not observable, such as atoms, electrons, or magnetic fields. Are claims concerning such entities to be viewed as meaningless? Mach believed such terms imply an imprecision in the statement of scientific claims — they would eventually disappear as science progressed.

2.2.6 Later positivists recognised that the removal of such terms was problematic. The hypothetico-deductive (H-D) model of the structure of theory was adopted and developed by Carnap and Hempel (1965). The H-D method consists of four essential steps:

(1) make some assumptions/propose some laws;
(2) logically deduce some conclusions;
(3) empirically test the conclusions; and
(4) infer truth/falsity of assumptions/laws.

2.2.7 This account implies a distinction between the assumptions,

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

intermediate propositions and conclusions of a theory. Bridge laws link conclusions to observables in the real world, so that these propositions are testable, and thus satisfy the verifiability principle. Theoretic terms in the assumptions or intermediate propositions gain their validity from the role that they play in supporting testable conclusions. The verifiability principle was relaxed to allow that theoretic claims could receive confirmation from the testing of conclusions which they generated. What counted as confirmatory evidence was extensively discussed.

2.2.8 Early positivists sought to minimise the role of theory and theoretic terms by viewing them as merely tools for the efficient organisation of complexes of sense data, but later positivists, recognising that theory explains as well as merely organising, identified a more substantial role for both theories and theoretic terms. The most important development was that of the 'covering law' model or 'deductive-nomological' (D-N) model of explanation (Hempel, 1965). The covering law model suggests that a phenomenon is explained if a sentence describing the phenomenon can be deduced from a set of axioms which includes at least one law. The 'inductive-probabilistic' (I-P) model was later added to encompass effects that were given high probability of occurrence as a result of the operation of probabilistic laws.

2.2.9 *The collapse of positivism*

2.2.9.1 By the late 1950s, although the storm clouds were gathering heavily around positivism, with critics increasingly on the attack, it remained the orthodox school of thought within the philosophy of science. A decade later positivism had collapsed. A number of arguments were important in this rapid reversal.

2.2.9.2 Positivism rests on the existence of objective observation claims. Despite considerable work in this area, notably Wittgenstein's development of the notion of atomic sentences (Wittgenstein, 1921), no such objective observation claims have been identified. It would seem that the best candidates for such sentences are statements of immediate sensations such as "the facing plane is red". Unfortunately such statements concern personal experience — they are not directly in the public domain. The lack of a set of observation claims which can be verified within the public domain represents a serious problem for the positivists.

2.2.9.3 Other critics have noted that what we observe depends on our experience and perspective. We do not make statements indiscriminately about our observations, but pick out aspects of interest, and then interpret them in accordance with the theories to which we subscribe (e.g. O'Hear, 1989, Chapter 5). On this view, observation is inalienably theory-laden — the positivist assumption of objective observation is unattainable.

2.2.9.4 The precedence given to observable over non-observable has also been questioned. The border line between observable and non-observable entities rests on the particular ocular ability of the observer — should we accept entities as

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

existent which can be observed with the aid of a telescope or a microscope? Does the range of existent entities change as our technology improves? The border line is highly problematic. The underlying motivation for the positivists to prefer observation statements is that these relate to unintermediated experiences which we should be least able to doubt, but our experience of sight is intermediated through photons and the electro-chemistry of the human ocular system.

2.2.9.5 Developments of the H-D model in later versions of positivism helped to shift the focus from observable/non-observable to theoretic/non-theoretic distinctions, but attempts to draw this latter distinction in a compelling way remained unconvincing and controversial. A number of other problems with the H-D account have been identified. Although one of the aims of the H-D method is to avoid induction and remain on the more certain ground provided by deduction, in fact it requires an account of how the evidence provided by empirical tests leads to confirmation or corroboration of the assumptions/proposed laws. The inductive logic developed by later versions of positivism to achieve this has been criticised on a number of counts:

(1) Hume notes that the existence of an empirical regularity in the past does not guarantee that it will continue in the future — that all ravens so far inspected have been black does not prove that all ravens are black or that the next raven we inspect is black. This 'problem of induction' has remained unanswered (Hume, 1738).

(2) The ravens' paradox notes that the proposition that "all ravens are black" is logically equivalent to the proposition that "all non-black objects are non-ravens". We should wish to use a black raven as confirmatory evidence for the proposition that "all ravens are black", but it would seem that a non-black, non-raven — a red shoe or a yellow flower, for example — should count equally well. This is, to say the least, counter-intuitive.

(3) Any hypothesis, *H* say, can receive confirmation by appending it with a true theory, *T* say. {*H* and *T*} is now supported by evidence that supports *T*. In practice, as it is not possible to test assumptions/laws in isolation, this is problematic. We would seem to need some relevance criteria concerning the auxiliary theories *T* with which we append *H*, but, in practice, such criteria have proved difficult to formalise.

(4) On a separate tack, Nelson Goodman raised fresh objections to inductive logic with the definition of predicates such as 'grue' (Goodman, 1965). An object is grue if it is green at every point in time before the turn of the millennium, but blue at every point in time thereafter. As of today, any evidence that an object is green is also evidence that it is grue. This represents a rather embarrassing conundrum for inductive logic.

2.2.10 *Karl Popper*

2.2.10.1 Popper was not a positivist, and, indeed, spent much of his long academic career criticising positivism, but his rationalist philosophical stance is closely allied to that of the positivists.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

2.2.10.2 Popper, aware of the weakness of the positivist account in the area of inductive logic, noted the asymmetry which exists between confirming and disconfirming a universal claim. No set of observations is ever sufficient to establish the truth of a universal claim — that all ravens so far observed have been black does not prove that all ravens are black, the next one observed might be white. By contrast, a single counter-example is sufficient to establish that a universal claim is false. Popper proposed that the method of science should be concerned with falsifying propositions, and thus that the correct focus of science is with the set of claims which has not yet been falsified. He shared the positivists desire to distinguish between science and non-science, replacing the verifiability principle with the proposition that the claims of science need to be open to empirical testing which could show them to be false.

2.2.10.3 One of the problems with Popper's approach is that it is not clear how a proposition which has not yet been falsified is helpful. The set of such propositions is very large, and many of the propositions it contains are complex and unappealing (e.g. 'all grass is grue'). In practice, we need to be able to pick out the propositions from the unfalsified set which are plausible candidates for laws. Popper proposes that scientists should expose their propositions to the harshest possible testing. Propositions which have been subject to stringent testing and survived are regarded by Popper as 'corroborated', and are to be preferred as candidates for possible laws. If a proposition is falsified during this process, it might be amended to mean that it passes the test — this process might lead to progressive improvement of the propositions put forward.

2.2.10.4 Unfortunately, in attempting to find criteria for selecting between unfalsified propositions, Popper is addressing the same problem as that which inductive logic attempts to address. The solutions which Popper proposes would seem to suffer from many of the same flaws. The distinction between corroboration and confirmation is arcane. Moreover, modifying a proposition in response to failure does not necessarily imply that the proposition is improved. This approach might lead to the introduction of *ad hoc* changes which rescue the proposition from the falsifying test, but which do not produce an attractive or plausible theory.

2.2.10.5 A further problem for falsificationism is presented by Duhem's paradox. In order to test a proposition, it will normally be necessary to construct an experiment. The experimental situation will involve test equipment, and will be constructed having recourse to theories concerning the behaviour of such equipment. If an experiment designed to test a proposed law fails, it is not clear whether it is the proposed law or some of the other prior assumptions concerning the test situation which are at fault. It is not possible to test laws in isolation — in practice we test the law in conjunction with many auxiliary hypotheses. Pure falsification is not possible.

2.2.10.6 Although Popper spent many years debating these issues with his critics, he did not succeed in meeting their material objections. Popperianism is no longer a widely held view amongst philosophers.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

2.2.11 *More recent developments in empiricism*

Despite the collapse of positivism, the strict empiricist tradition continues to be a substantial strand of thought within contemporary philosophy of science (e.g. Van Fraassen, 1980; Sneed, 1979).

2.3 *Sociology of Science*

2.3.1 The 1960s saw the rapid rise of philosophical thinking that rejected the search for a rational basis of science, and focused, instead, on the existing practices of scientists and the historic development of scientific knowledge. Thomas Kuhn's *The Structure of Scientific Revolutions* (Kuhn, 1970), first published in 1962, is probably the most influential book within the philosophy of science published within the second half of the twentieth century.

2.3.2 *Thomas Kuhn*

2.3.2.1 Kuhn rejects the notion that the philosophy of science should be concerned with the search for unique methodological prescriptions for how science should be undertaken. He regards the empirical basis of science as necessary, but not sufficient, for determining scientific beliefs. Rather, theory choice is driven, in part, by the norms of the scientific community within which the scientist is working. These norms are, in part, a function of historic accidents, and change over time to provide a varying context for the activity of the scientific community. Moreover, exploration of the historic development of scientific knowledge suggested to Kuhn that these norms were, in the various areas of scientific study, subject to sudden periodic step changes.

2.3.2.2 From this account of the historic development of science Kuhn developed the concept of a 'paradigm', i.e. some agreement amongst a scientific community concerning theory and its application which provides a stable framework within which a coherent tradition of scientific research develops. Work in the paradigm is 'normal science' — it does not challenge the agreed framework which defines the paradigm. This implies that, in practice, scientists are tenacious in holding to the main theories which they support. Apparent disconfirmations of such theories are regarded, initially, as anomalies, and are not regarded as a reason for abandoning the paradigm. Eventually, however, a build-up of anomalies precipitates pressure for a paradigm shift, but the existing paradigm is not abandoned until an acceptable alternative has been developed. A revolutionary paradigm shift then occurs, with the abandonment of the old and the adoption of the new theories and paradigm.

2.3.2.3 Kuhn rejects the idea that a shift from one paradigm to another can be regarded, in a strictly rational sense, as scientific progress. The theories of one paradigm are different from those of another, and this implies that what the scientists choose to observe, how they interpret their observations, and even the terms which they use, will differ. This gives rise to a serious problem of incommensurability — as the perspective and language of scientists differs in different paradigms, their theories cannot be compared. Moreover, there is no

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

commonly accepted framework to which to appeal to decide between the merits of competing paradigms. Progress can occur within a paradigm as a result of the activity of normal science, but not between paradigms.

2.3.2.4 Perhaps not surprisingly, Kuhns' views have been vigorously attacked by those from other methodological camps. Popper, for instance, criticises the anti-falsificationism of normal science as unscientific. Kuhn's rejection of the objective and rational basis of science has found favour with few traditional methodologists.

2.3.2.5 The descriptive accuracy of Kuhn's account of science has been questioned. The suddenness and radicalness of theory change implied by Kuhn's notion of paradigm-shifts seems historically inaccurate. The distinction between periods of normal science and paradigm-shifts does not seem clear cut in practice. It would not seem that there is always just one paradigm in existence, nor that paradigms give rise to long periods of normal science. There has been an extensive debate concerning Kuhn's use of the term paradigm, from which the consensus view which emerges is that the term is vague and is used in a number of different ways, but the paradigmatic basis of science provides the substance of Kuhn's methodological analysis. Without this, not much is left of his methodological framework.

2.3.2.6 Perhaps most damaging, though, has been the attack on Kuhn's radical version of the incommensurability thesis. Evidence from the practice of science does not support the view that different paradigms are like untranslatable languages — practising scientists do generally communicate effectively concerning the nature of the world and understand, with little problem, to what other scientists' terms refer.

2.3.3 *Imre Lakatos*

2.3.3.1 In contrast to Kuhn, Lakatos stresses the existence of competition between rival theories and the gradualness with which theories gain or lose their dominance. He allows commensurability between competing theories, and disputes Kuhn's suggestion that progress can only be made within normal science — for Lakatos the growth of knowledge within science is clear.

2.3.3.2 Lakatos develops the insight of Kuhn that theories do not exist in isolation, but that they are often intimately linked within an area of exploration, within his account of the methodology of scientific research programmes (SRPs) (Lakatos, 1970). He argues that, as theory is developed over time within a research programme, the correct unit of assessment is the SRP rather than the theory itself. An SRP is defined by a 'hard core' of theory which is unquestioned, a 'protective belt' of additional hypotheses or theories which the SRP investigates, a positive heuristic (which guides what sort of questions should be asked and how), and a negative heuristic (which guides what sort of activity is prohibited). An SRP is classified as either progressive of degenerative. In a progressive SRP, practitioners are able to develop theories which have additional empirical content, and thus develop science. Degenerative SRPs, on the other

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

hand, develop increasingly *ad hoc* theory, and may, in the limit, become psuedo-scientific.

2.3.3.3 In a sense Lakatos also develops the later work of Popper. Although the notion of an SRP is at odds with 'naïve' falsificationism, later versions become increasingly 'sophisticated', and effectively permit the development of theory through progressive amendment, provided this avoids too much *ad hoc*ery. Lakatos's rules for guiding the testing and development of theory adopts many of Popper's proposals.

2.3.3.4 As an historic account of the progress of science, Lakatos's account has been found by many to be convincing. Lakatos has proposed that his account of SRPs may be used to reconstruct the history of science in order to reveal its internal logic. It is far less clear what effective guidance is provided by Lakatos for how science should proceed. He provides no direct basis for assessing theory. Attempts to interpret his account of progressive SRPs as providing a prescription for scientific method have foundered on the ambiguity of the rules.

2.3.4 *Paul Feyerabend*

2.3.4.1 Feyerabend develops the rejection of rationalism proposed by Kuhn by actively embracing relativism. Relativism rejects the notion of mind-independent objective truth in favour of the notion that truth depends on our perspective of the world and the theories we choose to believe. Feyerabend proposes an extreme version of incommensurability, in which no two distinct theories are comparable, because even small modifications of theory imply that they use terms in different ways. This account of science supposes that the proponent of a theory *p*, say, could not converse effectively with a scientist who rejected *p*. This latter scientist operates in a world defined, in part, by the theory not-*p*, which is incommensurable with the world which incorporates *p*.

2.3.4.2 This radical incommensurability thesis leads Feyerabend to reject the positivist notions of meaning invariance and their parsimonious attitude to theory (the drive to find a small number of theories with wide application), which they achieve, principally, by means of the consistency requirement that theories should agree with previous theories, but explain more. Rather, Feyerabend supports the proliferation of theories, even where they overlap or repeat the claims of prior theory.

2.3.4.3 Feyerabend rejects methodological prescriptions for science, and argues that a wide variety of approaches can produce worthwhile science. The best approach is to permit everything within science — this leads to what he describes as his anarchic, or Dadaist, theory of knowledge (Feyerabend, 1975).

2.3.4.4 Unfortunately, whilst Feyerabend's work provides a stimulating challenge to more conventional methodologies, it provides no adequate account of the compelling success of science, and no practical programme for developing this success. His extreme incommensurability argument raises doubts concerning the plausibility of communication which seem unwarranted, and does not seem to accord with the historic development of scientific theory. To encourage unlimited

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

theory proliferation, and even permit the holding of mutually inconsistent theories, is highly problematic for the practising scientist.

2.3.5 *Post-modernism*

Although post-modernism is not a cohesive movement with a common agenda, it does form an important element within contemporary methodological thinking (Best & Kellner, 1991). Post-modernists share their rejection of the 'modernist' notion that the world can be adequately captured with simple laws, and have mostly embraced relativism.

2.4 *Causalism*

2.4.1 The nature of causation has long puzzled philosophers, and has been the subject of lively debate since the work of the ancient Greeks. Aristotle, for example, took the exploration of causes and the laws which describe their operation as the very substance of science. Causation was troubling to positivists, because it threatens to introduce concepts which were not directly observable, and which were sometimes viewed by the positivists as being akin to occult powers. The positivists thus adopted Hume's view of causation, that causes are reducible to regularities amongst observable phenomena (Hume, 1738). The causalist movement, which has developed over the past twenty years, is perhaps best understood as a reaction against Hume's view of causation.

2.4.2 We have seen that, under the covering law model of scientific explanation, a phenomenon is explained just when it is deduced from some empirical law. The law required is a universal regularity at the observable level, the sort of law of which Hume would approve. Given a raven x, and the law that "all ravens are black", we may explain the blackness of x, but establishing such regularities is problematic in a number of ways, some of which, as we have seen, contributed to the collapse of positivism.

2.4.3 The 'reduction to regularity' view of causes is inadequate on several fronts. It does not provide an account which embraces the mechanism of the cause; if we open a clock we may reveal the mechanism, which allows us to understand how the winding of the clock causes the movement of the hands. Moreover, the regularity view implies a symmetry between cause and effect which is untenable: by manipulating a cause we may bring about the effect; but by manipulating the effect we do not bring about the cause. The classic example is that of the shadow of a flag pole (e.g. Salmon, 1984, p95) — we may explain the length of the shadow by reference to the height of the pole, but we cannot explain the height of the pole by reference to the length of the shadow.

2.4.4 Causalism has a different account of explanation, an account which rests on taking causes seriously, as more than mere regularities. A phenomenon is explained by citing a relevant cause. The causalist account provides a solution to one of the troubling problems of the positivist account which has remained unresolved; that some universal claims are acceptable as laws whilst others are not. Claims such as: "all the coins in my pocket are copper" may be true, but

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

they do not seem to be suitable as laws — they lack some quality of necessity which would seem to be required for a universal claim to be law-like. Attempts to provide a precise account of this quality of law-likeness have foundered. By citing a cause which produces the effect, the effect is explained by means of the operation of the cause; causal necessity does the job that is required.

2.4.5 The causalist movement argues that causes need to be taken seriously in order to understand the practice of contemporary science. In practice, scientists do not spend their time repeating old experiments in order to become convinced of their laws by the weight of regularity — indeed, a new experiment performed just once, if performed in just the right way to reveal compelling results, is often sufficient to convince (Cartwright, 1989).

2.4.6 Perhaps the best starting point for understanding the roots of contemporary causalism is the extensive writing of John Stuart Mill. Mill proposes that our scientific models of the world should be based on the major causes which are relevant. "When an effect depends on a concurrence of causes, these causes must be studied one at a time, and their laws separately investigated, if we wish, through the causes, to obtain the power of either predicting or controlling the effect; since the law of the effect is compounded of the laws of all the causes which determine it" (Mill, 1843, 6.9.3). The model will not, generally, be exact, because it necessarily simplifies by omitting many minor disturbing causes. It is interesting that, while Mill has confidence in such modelling, he also acknowledges the non-additivity of causes — causes which are generally minor may, under some circumstances, have major impact. One of Mill's major contributions is his explication of the 'method a priori' (or 'deductive method'). This method is concerned to establish the validity of some propositions which may then be used to derive, via deduction, some valid claims about the world (see e.g. Hausman, 1992, p143).

2.4.7 Amongst more recent philosophers, John Mackie has played a particularly valuable role in updating our account of causation after the extended period during which the subject was much neglected (Mackie, 1974). Mackie provides a careful critique of the Humean account of causation, and develops a richer account from an essentially empiricist basis. He distinguishes between causation 'in-the-objects', our concept of causation, and our knowledge of causation. By considering practical examples of our common use of causes, including consideration of the concept used by the legal profession, Mackie clarifies the notion of explanatory cause. We might, for example, cite a gas leak as the cause of an explosion — even though the gas leak, by itself, would be insufficient to cause the explosion without the presence of some agent of ignition, sufficiently constrained ventilation, etc. Of course, there are other possible causes of an explosion other than a gas leak, so that this is not a necessary condition. Mackie's analysis suggests that an explanatory causal circumstance is an INUS condition — an insufficient, but non-redundant, part of an unnecessary, but sufficient, condition.

2.4.8 Wesley Salmon, who has provided one of the most careful analyses of

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

what it is to explain, has also come to the view that a causal basis is required to provide an adequate account of scientific explanation (Salmon, 1985). For Salmon, explanation involves placing the phenomenon to be explained into the context of the causal pattern of reality. In contrast to Mackie, he discusses causal processes rather than events, and explores the notions of causal production and causal propagation. Salmon has been one of the most compelling proponents of the causalist view that our explanations of the world, and thus our scientific models, need to be based on a causal understanding of the structure of the world.

2.4.9 By reference to examples within physics, Nancy Cartwright has explored further the ways in which the notion of laws as precise regularities at the observable level are problematic (Cartwright, 1983). Planetary motion, for example, does not correspond precisely to a model based on the inverse square law, because planets are not point masses, frictional forces are non-zero and minute electrostatic forces are present. The model turns out to be merely an approximation to reality, albeit a very powerful one. The covering law account of explanation is wrong. The best laws that we have are phenomenological laws — the laws which state low level regularities which may be observed within limited localities. For Cartwright, it is causes which give rise to such regularities, the deeper structure of reality is constituted of capacities, what Mill would call tendencies (Cartwright, 1989). Cartwright follows Mill in acknowledging the causal complexity of reality — the causes which are important and the way they interact vary from situation to situation. Our models of reality are approximations, because they invariably omit at least minor causes. Tony Lawson, following Bhaskar, adopts a different ontology from that of Cartwright, but their views on causation are strikingly similar in many respects (Lawson, 1997).

2.4.10 Ian Hacking has proposed that we should "count as real what we can use to intervene in the world to affect something else, or what the world can use to affect us" (Hacking, 1983). This develops the causalist position, by proposing that our most certain knowledge of the world stems, not from passive observation, but from interaction. Hacking is, for example, persuaded that electrons exist because they can be used as a spray to produce required effects. From this entity realist standpoint, Hacking regards the representation of reality as central to science, but rejects the notion that there is a single correct representation of reality. In Hacking's view, it is the belief in this single correct representation of the world which has driven the search for incorrigible truth, but, in rejecting the search for such truth as misguided, Hacking is not lapsing into relativism. Rather, he takes the knowledge we derive from experimentation as providing science with a firm handle on reality. The view that such knowledge is only loosely linked with theory closely reflects the insight of Cartwright that low-level phenomenological laws are not 'covered' by high-level laws in the way in which the covering law model would imply. Our models of reality should respect local knowledge in order to provide adequate approximations. This view that the world is best described by a patchwork of models is developed further in Cartwright (1998).

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

2.4.11 Ronald Giere is not a causalist, but his account of the way in which models represent and approximate reality develops many of the ideas of causalists along cognitive lines (Giere, 1988).

2.4.12 An important strand of the recent methodological consideration of the relationship between models and reality has been a renewed focus on the complex issues involved in idealisation (Cartwright & Jones, 1998). Idealisations, such as the frictionless plane or the point mass, play an important role in physics. In economics, idealisations, such as the model of perfect competition, often involve the use of contrary-to-fact assumptions, which serve to remove part of the complexity, e.g. we assume perfect knowledge and homogeneous goods. Idealisations are simplifications of reality; their purpose is often to define an environment which is tractable to analysis. Much recent consideration has focused on the question of how it is that idealisations are helpful in our understanding of reality, and how behaviour in a idealised model relates to the behaviour of reality.

2.4.13 In physics, the existence of universal constants and powerful regularities, at least in the laboratory or astronomy, provide the basis for compelling laws and for models which have great predictive capability. Throughout the twentieth century the use of models and idealisations within physics has been widely adopted as the preferred role-model for their use by other sciences, and has driven the search for laws, but this supposition of 'modernism', that reality can be adequately described by simple laws, has recently been challenged, especially by the causalists and the post-modernists. Outside physics, especially within the social sciences, the problem of model/reality fit tends to be more difficult. In many ways reality is less uniform, and there are generally more causal influences. Observable regularities are not as powerful, and we do not have universal constants. Such realities thus raise particular problems for modelling and idealisation. Simple models contrast with the complex nature of realities — the relationship requires careful consideration. In opening consideration to the variability of reality, we highlight the context of model application — how is the model used in practice, and how does it deal with the specific variations found in the locality in which it is to be applied? These are topics which philosophy is now starting to address, and one important starting point is the consideration of practice within science (e.g. Collins, 1985).

2.5 *Conclusion*

Positivism, or strict empiricism in its various guises, dominated twentieth century thinking within the philosophy of science in the Anglo-Saxon world, until its dramatic collapse in the 1960s. The critical questions concerning the nature of science, and how it should be conducted, have since then been the subject of a vigorous debate in which the viewpoints of the various participants have differed to an extreme extent. This debate continues, making the methodology of science an exciting and dynamic area of study. There is currently no single orthodoxy in this area.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

3. Some Recent Developments in the Methodology of Economics

3.1 *Introduction*

3.1.1 This section reviews some recent developments in the methodology of economics, in order to extend the methodological discussion of the last section, and provide further context for consideration of actuarial methods.

3.1.2 Until some 20 years ago, comment on economic methods tended to be the preserve of economists venturing part-time into philosophy — limited material was available. Since then the methodology of economics has become established as a well-defined area of study in its own right, with many full-time professional philosophers and economists, an explosion in amount written, and the start of the *Economics and Philosophy Journal* in 1985 (Backhouse, 1994, pp1-15; Caldwell, 1994, Preface). This has led to the discussions becoming far more rigorous, as their foundations within the framework provided by the methodology of science have become firmer and more explicit.

3.1.3 Economics shares with actuarial science an interest in modelling financial realities — the methodological debate within economics provides a rich context for consideration of actuarial methods. It also provides an important input to the framework necessary to compare economic and actuarial methods, especially in areas where these are alternatives.

3.1.4 The ambition of this section is limited to outlining some of the key issues which have emerged from this work. In order to prepare the ground for the methodological discussion, the following sub-section provides a characterisation of current economics.

3.2 *A Characterisation of Current Economics*

3.2.1 *Introduction*

The range of endeavour within economics continues to become ever broader and more complex. A full review of the subject would not be possible here, nor, indeed, very helpful. This section provides a brief characterisation of some of the key areas in economics, which is helpful from a methodological point of view. The three areas covered are the neo-classical tradition, econometrics and institutional economics. Although this covers a considerable amount of the work which is currently undertaken in this field, it certainly omits large areas too, not least the Austrian and Marxist schools and such recent developments as behavioural finance (see e.g. Olsen, 1998).

3.2.2 *The neo-classical tradition*

3.2.2.1 Neo-classical economics comprises much of the material with which actuaries will be familiar through the actuarial syllabus. The enduring characteristics of this tradition are a focus on equilibria, the use of mathematical models, and the assumption of rational agents with perfect knowledge of markets, who engage in maximising behaviour.

3.2.2.2 The tradition finds its roots in the marginalist revolution of William

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Stanley Jevons and Leon Walras, which began around 1870 (Staley, 1989). Jevons promoted the notion of utility as a measurable quantity which could be used in economics. He distinguishes between total consumption utility and marginal utility, proposing what is now called the Law of Decreasing Marginal Utility. Jevons established the familiar results linking marginal utility and price.

3.2.2.3 Walras is now recognised for his pioneering contribution to general equilibrium theory, in which the economy is modelled as a series of interconnected markets. The price of a product is taken to depend on the prices of substitutes and complements, incomes, tastes, technology and available resources. By ascribing utility curves to consumers, and assuming maximising behaviour, Walras developed a set of equations for the product markets. By assuming full employment, Walras established equations to describe the factor markets. Bringing these interdependent sets of equations together, Walras observed that he had the same number of equations as unknowns, and deduced (not quite correctly) that he had solved the problem of how to formulate a general equilibrium theory of the economy.

3.2.2.4 The neo-classical tradition was consolidated by Alfred Marshall, who dominated economics from around 1890 to his death in 1924. Marshall's contribution is substantial and broad. He continued the investigation of marginal utility theories, considering the role of money, and investigating price and income elasticities of demand, well illustrated by his discussion of 'giffen goods'; he developed further the theory of the firm, looking at marginal substitution of factors of production, especially the marginal product of labour; he popularised the notion of consumer surplus; he established the distinction between stable and unstable equilibria; he established a distinction between short-run and long-run effects; and he developed a theory of money.

3.2.2.5 In the 1930s this tradition was developed into the area of monopolistic competition, with the writing of E. H. Chamberlin and Joan Robinson. This work will be familiar to actuaries through the diagrams of the monopoly firm, in microeconomics, in which marginal revenue is equated with marginal cost. The important tool here is marginal revenue, the increment to the income of the firm through an increase in price, which allows for the anticipated fall in sales volume.

3.2.2.6 Of course, the most influential work of economics during this period was John Maynard Keynes' book *The General Theory of Employment, Interest and Money* (Keynes, 1936). Keynes' classic work includes theories of the financial system, foreign trade, effective demand, liquidity preferences, marginal propensity to consume, marginal efficiency of capital, and income multiplier effects. Keynes did not present the theories as general equilibrium models, but as lines of one-way causation. It was only later that Keynes was brought partially into the neo-classical consensus, by such work as that of J. R. Hicks, who reformulated Keynes' theories as the well-known IS-LM general equilibrium model. As Axel Liejonhufvud points out, Keynes was concerned with exploring

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

disequilibria — his exploration is that of the causal structures of the economy which guide its dynamics (Leijonhufvud, 1968).

3.2.2.7 This summary of some of the key work within neo-classical economics allows us to see the methodological consistency of this tradition, even though the subject matter has changed. As Daniel Hausman would have it: "Neo-classical economics is the articulation, elaboration, and the application of equilibrium theory" (Hausman, 1992, p272). The central methodological feature is the postulation of timeless, mathematical models, often represented on two-dimensional graphs, which rely upon simplifying assumptions about the world.

3.2.2.8 A more recent central development in neo-classicism has been the Arrow-Debreu axiomatisation of market equilibrium, which develops the work originally pioneered by Walras to formalise the conditions for the existence of a market equilibrium (Debreu, 1959; Arrow, 1963). This work established idealisation in economics on a new level of mathematical sophistication, and established the basis for the development of modern mathematical finance theory. The theory employs explicit formalisation devices, such as the efficient market hypothesis (see e.g. Fama, 1970; Samuelson, 1965), the assumptions of the perfect knowledge and rationality of agents in a situation of equilibrium, and the qualities of no arbitrage and optimality. This material, which is characterised by abstract mathematical proofs, now forms the standard basis of finance theory taught to investment analysts and MBA students through such text books as Brearley & Myers (1996), Elton & Gruber (1981) and Ingersoll (1987). The following features are characteristic of such mathematical economics:

— Set-theoretic assumptions are made, in which some of the mathematical entities are labelled with the names of phenomena within the real world; e.g. "the possible consumption universe is a subset of n-space which is convex and closed".
— Idealising assumptions are made which simplify the real world; e.g. "All investors have a common time horizon and homogeneous beliefs about the mean and variance of the price distributions of all investment assets" (Ingersoll, 1987, p92).
— The idealising assumptions are interpreted set-theoretically (and, in practice, ensure the problem is mathematically tractable).
— Mathematical statements/theorems are derived by logico-mathematical deduction from the assumptions, i.e. proofs are presented.
— The derived mathematical statements (i.e. theorems) are reinterpreted as claims about the real world using the labels applied to the selected mathematical entities; e.g. under the given assumptions, there is a unique vector in n-space which solves, slack-free, the consumer allocation problem for any given price vector in n-space and positive wealth.

3.2.2.9 Economic work in relation to such ideals comprises three areas: (1) theoretical; (2) empirical; and (3) applied modelling. One of the main strands of

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

theoretical work is concerned with the 'relaxation' of the assumptions of these ideals. Given an assumption which is regarded as stringent, perhaps from an empirical consideration of the locality in question, an alternative assumption set is sought which omits this assumption, but which is sufficiently powerful to sustain some relevant proofs or models. Empirical work in this area is concerned with testing the accuracy of the deductions from mathematical economics (see e.g. Elton & Gruber, 1981, Chapter 13; Roll, 1977). Examples of the application of models based on such ideals to practical situations include the work of practitioners in pricing options, and work calculating economic values of businesses.

3.2.3 *Econometrics*

3.2.3.1 The standard dictionary definition of econometrics is: "the statistical analysis of economic data and their interrelations." The diverse and changing nature of the activity of econometricians means that a more precise definition of econometrics becomes controversial.

3.2.3.2 Amongst the mainstream activity of econometricians is the building of models of all, or part, of specific real economies, in which the variables represent quantities in the real economy, and mathematical relationships between the variables are used to capture the relationships (e.g. causal, definitional or technical) between these variables. The early classic models, such as the macrodynamic models of Tinbergen, were dynamic, generally specified to capture the existing state of the economy, and run to show the likely path of the economy over future time periods.

3.2.3.4 Statistical economics has attempted to establish regularities directly from inspection of the data without the use of prior theories. Mary Morgan, in her excellent history of econometrics (Morgan, 1990), cites Mitchell and Persons' attempts to avoid an a priori theory. Persons, for example, working on business cycle theory, developed the techniques of seasonal adjustment, de-trending and averaging, in order to make regularities more visible. These techniques form part of the standard econometric approach to data preparation today. However, as Schumpeter observes, even this approach implicitly amounts to a theory that there are four groups of causal influences: cyclical; seasonal; long-term; and accidental.

3.2.3.5 In practice, it is difficult to persuade the economic data to reveal compelling regularities without some guidance. Econometrics is characterised by taking some theory (in a suitable mathematical form) as a starting point for model building. Although, these days, economic measures are invariably chosen as the model parameters, early econometric models include that of Jevons, which was based on sunspots, and that of Moore, which was based on the cycles of Venus. In practice, there is considerable difficulty in selecting the list of economic variables to include in the model, and in defining the relationships which should exist between them. A lack of consensus amongst economists ensures there are always many possible theories which could be used to help determine the model.

3.2.3.6 An important development in econometrics was Haavelmo's work to establish probability theory as the basis for the statistical work of econometricians

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

(Haavelmo, 1944). The restatement of economic claims in probabilistic terms established the basis for the testing of theories, rather than merely the measurement of their effects. Adopting a probabilistic basis for econometric statistics highlighted the problem that economic data are not generally drawn from independent distributions in the way that would be desirable. Much of econometrics has become concerned with meeting this problem — Peter Kennedy comments: "What distinguishes an econometrician from a statistician is the former's preoccupation with problems caused by violations of statisticians' standard assumptions; owing to the nature of economic relationships and the lack of controlled experimentation, these assumptions are seldom met" (Kennedy, 1992).

3.2.3.7 The focus of econometrics has tended to narrow over the years. Around the 1940-50 period, econometrics involved the active manipulation of both theory and data in order to derive a good fit between the model and reality. Increasingly the focus has fallen on the statistical manipulation of the data, rather than on the theory. Modern courses in econometrics almost invariably lead heavily with the tuition of the theory of statistics (Kennedy, 1992, p1).

3.2.3.8 The classic econometric approach is thus to employ a theory which is expressed in mathematical form, and to use a statistical analysis of the data to quantify the strength of the causal influences, or other relations, posited by the theory. A focus on measurement is a central feature of econometrics.

3.2.4 *Institutional economics*

3.2.4.1 Institutional economics became an established body of thought after the First World War, championed by, amongst others, Thorstein Veblen, and "rose to a crescendo some time in the 1920s threatening at one moment to become the dominant stream in American economic thought" (Blaug, 1980, pp 86-87). Although the influence of the school has not sustained this peak, it has continued as a major strand of thinking through the work of such economists as Gunnar Myrdal, J. K. Galbraith, Warren Samuels and Willard Mueller, and the publication of its major journal, *The Journal of Economic Issues*.

3.2.4.2 Institutional economics rejects the notion advanced by the neo-classical school, that the economy can be adequately captured by universal, timeless, mathematical models based on rational agents. Instead, they view the economy as an unfolding history which takes place in the context of a broader social historic process. Emphasis is placed on the role of non-economic factors, such as the legal and cultural framework, and the characteristics of the institutions which play key roles within the economy. The institution is chosen as the basic unit of analysis in preference to the individual. The term 'institution' is taken to refer to far more than just corporate structures. According to Veblen's widely accepted definition, it refers to "the settled habits of thought common to the generality of men" (Veblen, 1961, p239) — it embraces the fixed habits of society which provide a resistance to change.

3.2.4.3 As the concept that the economy changes over time is central to institutional economics, there is a focus on the underlying reasons for such

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

change. Technology, in its broadest sense, is generally taken as the main driver of change, although other drivers, such as cultural shifts, are also embraced. Institutionalists take the view that the historic context provides an important part of the explanation which allows us to understand how the economy works. This history comprises, in large part, an account of the tension between the forces for change, such as technology, and the institutional resistance to change.

3.2.4.4 Institutional economic analysis is characterised by a consideration of conflict. The behaviour of agents are taken to be driven by diverse motivations, which might include a thirst for power, a sense of adventure, or custom and habit —not merely the 'rational' actions of economic man. An important strand within this work is an analysis of the role of power, especially institutional power, within the economy.

3.2.4.5 Considering economic phenomena within the context of an unfolding history, places an emphasis on an understanding of the overall system whose history is being explored. Components of the system change in the context of the development of the overall system — there is a focus on causal mechanisms which operate at the macro level within the economy, as well as an acceptance of changes which result through the aggregation of the behaviour of individual agents. The economy, or subject matter under investigation, is viewed as having emergent qualities.

3.2.4.6 This view of the economy as historically developing implies that the claims of institutional economists are contextualised, they relate to specific economies or systems at specific times. This contrasts with the universal and timeless claims of the neo-classical tradition. The nature and validity of such claims requires monitoring through time, as experience unfolds. This contextualisation of claims places a limitation on the body of 'laws' to which institutional economics can lay claim — this is seen as a major weakness of the school by its opponents.

3.2.4.7 The subject matter of institutional economics is wide-ranging, encompassing "technology, power, institutions, the state of the working class, instrumentalism, pragmatism, conflict and its resolution, social forces, distribution, evolution, philosophical-ethical relativism or absolutism, progress, contextualism, economic organisation and control . . . deliberative social control and/or social change, institutional design and performance, socialization of the corporate system, economic planning, the economic role of government, the logic of reform and/or industrialization, humanism, and, inter alia, economic development and growth" (Samuels, 1969). Indeed, the breadth of the subject-matter, which appears somewhat disparate, together with the absence of a central body of theory, has led some to question the cohesion of institutional economics. This is a question which methodologians have now started to address.

3.3 *Methodology of Economics*

3.3.1 *Introduction*

Recent work within the methodology of economics has helped to characterise

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

and clarify the nature of the methods used in economics. The previous sub-section indicates the diversity of the methods used. The philosophy of science has also had a powerful direct influence on how economics is done. This section sets out some of the key points.

3.3.2 *Methodology pre-1980*

Probably the most influential pre-1980 works on the contemporary methodology of economics are those of John Stuart Mill (Mill, 1843) and Milton Friedman (Friedman, 1953). These are outlined below. For further background on earlier work, Blaug provides a concise discussion of the early development of ideas in the methodology of economics, tracing the path from Adam Smith through John Stuart Mill and John Neville Keynes, to Lionel Robbins (Blaug, 1980). Backhouse provides a helpful list of some of the key methodological texts produced before 1970 (Backhouse, 1994, p1).

3.3.3 *John Stuart Mill's methodology*

According to Hausman, Mill's method, a priori, is the method to which neo-classical economists "(regardless of what they say in methodological discussions) still apparently subscribe." (Hausman, 1992, p124). Consistent with his broader philosophy of science, Mill regards the laws of economics as based on an understanding of the major causes which operate within the economy. Some such laws may be known by introspection, for example that agents prefer more wealth to less. Others may be derived by consideration of empirical information, for example the laws of diminishing marginal returns. The method, a priori, then allows the behaviour of an economy to be deduced from the knowledge of the laws which has been established.

3.3.4 *Friedman's methodology*

3.3.4.1 Friedman's 1953 essay is almost certainly the best known methodological work amongst economists, and the most commonly cited justification for the methods of the neo-classical school. "Friedman's argument has become so pervasive (despite disagreement over details) that the positivist method is found in one form or another in most of the introductory economic texts" (Wilber & Harrison, 1978, pp65/6). Lipsey's well-known text book provides, perhaps, the best example (Lipsey, 1963).

3.3.4.2 Friedman adopts many of the important strands of ideas from the positivist orthodoxy of the day, including strong elements of Popper. Perhaps the dominant theme of his paper is the adoption of the hypothetico-deductive (H-D) method within economics, which allows Friedman to argue for the irrelevance of the realism of assumptions. Under the H-D method, the assumptions of a theory are not tested directly, but are validated indirectly by empirical testing of the conclusions of the theory. This allows Friedman to reject many of the critics of neo-classical economics who had highlighted the inaccuracy of the assumptions

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

often used. Friedman's essay places the empirical testing of theorems at the heart of economic science — in itself a controversial proposal.

3.3.4.3 Friedman's paper certainly has major short-comings as a work of philosophy, even accepting a positivist backdrop (see e.g. Nagel, 1963). Whilst his observation that a model is a simplification of reality would be widely accepted, his argument for the complete irrelevance of the realism of assumptions is not plausible. In economics, it is at least as difficult as in other sciences (arguably more so) to demarcate assumptions which relate to unobservables or theoretic terms. The assumptions of economics are generally stated in terms which are naturally interpretable as observable claims. Since the assumptions, themselves, can be deduced trivially from the assumption set, a sharp divide between the treatment of the assumptions and the conclusions is problematic.

3.3.5 *Methodology post-1980*

3.3.5.1 The content of methodological debate within economics before 1980 reflected the dominant themes within the philosophy of science prior to the collapse of positivism in the 1960s: the logical structure, testing and status of theories. As the debate became increasingly dominated by full-time professional methodologians, its content came to reflect more closely current developments within the mainstream philosophy of science.

3.3.5.2 Influential works from Blaug (1980) and Caldwell (1982 — revised 1994), considered the implications of Popperian philosophy. As Backhouse observes, this tended to set Popperian philosophy as a central theme in the 1980s, either from those in favour or those reacting against. De Marchi provides a helpful collation of the important strands in this debate (de Marchi, 1988). The philosophical problems of Popper's ideas, especially falsificationism, is exacerbated, in the context of economic realities, by the problem of identifying the sort of powerful regularities at the observable level which would underpin laws (by extensive compelling corroborations). As Popper's ideas became less dominant, other economic methodologians explored the ideas of philosophers such as Kuhn, Lakatos (e.g. Latsis, 1976) and Feyerabend (e.g. McCloskey, 1983).

3.3.5.3 The debate has tended to focus on mainstream neo-classical economics, comprising a dialogue between the defenders and critics. Since Friedman, the defenders have tended to be in the minority, an exception being Hausman (Hausman, 1992).

3.3.6 *The methodology of the neo-classical tradition*

3.3.6.1 Both Mill's method, a priori, and Friedman's methodology can be taken as underpinning the neo-classical use of assumption-driven models, although in distinct ways. Mill's method, a priori, is concerned to establish the validity of some propositions which may then be used to derive, via deduction, some valid claims about the world. This approach looks consistent with that adopted by earlier neo-classical economists, such as Marshall, who believed in

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

the validity of the fundamental assumptions of economics as having the status of laws. More recently Hausman has, perhaps, been the leading proponent of such methods, offering an account of economics as an inexact science (Hausman, 1992).

3.3.6.2 Friedman's shift to the H-D method allows the realism of assumptions to be abandoned, and this has important implications in supporting the shift towards more sophisticated mathematical versions of neo-classicism. The current methodological issues are best debated in the context of the contemporary mathematical approach to neo-classical economics.

3.3.7 *The methodology of mathematical economics*

3.3.7.1 Table 1 summarises the use of the H-D method by mathematical economics.

Table 1. The H-D method in mathematical economics

Step	Action	Comment
1	Make assumptions	Develops a number of alternative sets of plausible assumptions
2	Logically deduce some conclusions	Determines what follows trivially from the assumptions (in a mathematical sense) Very heavy focus on this step, especially in elementary education material Mathematical virtuosity is integral to such economics
3	Empirically check conclusion	Some focus in academic papers on this step Always fierce debate about whether conclusions accord with empirical evidence
4	Draw inferences re. assumptions	Almost never explicit Mathematical economics strongly committed to standard assumptions — even where direct tests of these are unconvincing

3.3.7.2 Of the three areas of activity identified previously as comprising mathematical economics: (1) theory may be identified with the first and second steps of the H-D method; and (2) empirical testing with the third. (3) practical modelling does not fall within the H-D schema.

3.3.8 *Theoretic work*

We have noted that much theoretic work is concerned with 'relaxing' assumptions. Theorists are aware that precise assumptions do not hold exactly in reality, and therefore seek to explore what happens if an assumption is removed. In practice, as most popular economic models do not incorporate unnecessarily strong assumptions, it is rarely possible to find an alternative set of assumptions which supports equally powerful results, but is, in aggregate, less stringent. To suggest an architectural analogy, it may be possible to replace an arch in a building by a pair of columns and a rolled steel joist (RSJ), or, alternatively,

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

remove it altogether, provided that the roof is also removed. This is generally the sort of 'relaxation' available in mathematical economics — assumptions may be removed at the expense of replacing them with other equally strong assumptions, or at the expense of reducing the power of the model. In practice, this theoretic process results in the proliferation of sets of adequate assumptions, rather than in the reduction of the assumption set or its strength.

3.3.9 *Empirical testing*

3.3.9.1 In practice, the process of testing some model empirically involves, not just choosing the parameters which best fit the available data, but also of choosing the most suitable form of the model, i.e. the assumption set which works best for the purpose. This would seem to add to the difficulties, discussed above in connection with the H-D process, of drawing inference to the validity of the assumptions.

3.3.9.2 The conclusions of such empirical investigations of the validity of the claims derived by deduction, are invariably controversial. This area is characterised by conflicting opinions — disputes rage over such questions as whether the market is efficient, or whether stock prices mean revert. In the lively debate over the empirical validity of the capital asset pricing model, Roll has suggested that the theory may, even in principle, be untestable (Roll, 1977).

3.3.9.3 One reason for dispute concerning the validity of such claims is a lack of clarity concerning their statement, especially in regard to their quantifiers. Hausman suggests that the claims of neo-classical economics are to be interpreted as being qualified by some vague *ceteris paribus* clauses which are not explicitly stated (Hausman, 1992). These clauses act to limit the domain of application of the claims and to guide the degree of approximation which is to be expected. If this is correct, then it is perhaps not surprising that the failure to state these *ceteris paribus* clauses leads to disagreements. There are certainly instances where the deduced claims are regarded as truths, that is to say as precise and universal descriptions of reality. The proposition that the value of an option does not depend on the expected growth of the stock price (Black & Scholes, 1973) is one example (e.g. Cox & Rubinstein, 1985). The lack of powerful regularities available in financial realities means that counter examples are always readily available to prevent the credible claim of such propositions as truths (e.g. Black, 1989). Claims are, in practice, at best 'often approximately right' and this is fully consistent with these claims sometimes being very wrong (Pemberton, 1998a). In a similar way, the claims that "the earth is spherical" is often useful in astronomy, but is hopeless for those concerned with the geography of mountains.

3.3.10 *Applied modelling*

Applied modellers build pragmatic models which are useful for some purpose, such as the pricing of options or the valuation of businesses. Such models may have similar mathematical forms to those derived from related theory, but the focus is on the selection of a form and parameters which allow the model to

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

provide a robust description or prediction in the localities with which they are concerned. Applied modellers use pragmatic methods for deriving the parameters which they feed into their models, and the skill of the modeller is almost invariably integral to this process — such skill has no place within the theory. In practical applications, it is necessary to develop parameter descriptions which achieve sufficient empirical adequacy, and these may be at odds with the assumptions of the theory. The relationship between the theory and such applied models is thus complex. The recent shifts in methodology, noted in Section 2, point to the usefulness of further exploration of this relationship through practical case studies of the sort developed by Harry Collins (Collins, 1985).

3.3.11 *Traditional economists' criticisms of mathematisation in economics*

3.3.11.1 The debate concerning the correct role of mathematics within economic method goes back to beyond the 1920s. Alfred Marshall, for example, was amongst those to express concern about the use of mathematics: "a good mathematical theorem dealing with economics hypotheses was very unlikely to be good economics; and I went more on the rules — (1) Use mathematics as a shorthand language, rather than as an engine of enquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the mathematics. (6) If you can't succeed in (4) burn (3). This last I did often." (Pigou, 1925, p427).

3.3.11.2 Marshall's preference for laying an emphasis on the case study, and limiting the focus on the mathematics, was widely shared by two generations of economists. John Maynard Keynes, supporting this view, expressed the concern that, if rigorous mathematical methods were used, the author could not keep in mind all the reservations and qualifications and allowances needed in economic thinking (Keynes, 1936).

3.3.11.3 Although these objections to mathematisation pre-date the shift of economics in this direction, the concerns which are expressed by these traditional economists continue to strike a sympathetic chord amongst many practising economists today (e.g. Gray, 1997; Olsen, 1998). The concerns have been side-stepped, rather than addressed.

3.3.12 *Philosophers' criticisms of mathematisation in economics*

3.3.12.1 Section 2 discussed how a number of serious criticisms of the H-D approach helped to lead to the collapse of positivism. It was the authority of the positivist orthodoxy within the philosophy of science, as interpreted by, most importantly, Friedman, that provided the basis for the mathematical wing of the neo-classical school to overcome the objections of more traditional economists and establish itself as the dominant neo-classical grouping. Mathematisation in economics has historically been predicated on the authority of the H-D method — the collapse of positivism leaves it in need of fresh support.

3.3.12.2 The discussion, above, of the practice of mathematical economics

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

notes the proliferation of assumptions, and the common practice of choosing assumptions to tailor the model to best fit the data. These features raise additional problems for the application of the H-D method.

3.3.12.3 Consideration of the way in which the H-D method is practised has led methodologists to suggest that mathematical economics is not an empirical science. As we have noted, Friedman's methodology, parts of which bear the distinct stamp of Popper, propounds the empirical testing of theories. The significant school of methodologians who have shared a Popperian viewpoint has agreed on the need for serious testing of economic propositions, which should provide for the rejection of those that fail. However, Hausman expresses the strong consensus view in stating "Friedman's advice has rarely been followed, and to implement it would require that microeconomics be radically transformed" (Hausman, 1992, p275).

3.3.12.4 Alexander Rosenburg goes further, suggesting that neo-classical economics has now become a branch of applied mathematics: "Much of the mystery surrounding the actual development of economic theory — its shifts in formalism, its insulation from empirical assessment, its interest in proving purely formal, abstract possibilities, its unchanged character over a period of centuries, the controversies about its cognitive status — can be comprehended and properly appreciated if we give up on the notion that economics any longer has the aims or makes the claims of an empirical science of human behaviour. Rather we should view it as a branch of mathematics, one devoted to examining the formal properties of a set of assumptions about the transitivity of abstract relations" (Rosenburg, 1994, p230).

3.3.12.5 On this view, as Rosenburg notes, the claim to validity of economic policy advice is largely surrendered. This is consistent with Terence Hutchinson's view that: "interest in policy decisions has declined significantly . . . while the aim of prediction has been explicitly rejected, or inexplicably demoted, by a growing number of academics" (Hutchinson, 1994, p27). Mirowski has been amongst those to suggest a sociological explanation of the mathematisation of economics, which has come to ensure that "the pedagogy is arranged so that the student can come to believe the mathematics on its own can dictate the right or wrong answers" (Mirowski, 1994, p64). On a similar tack, Leontief observes that: "Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions" (Leontief, 1982).

3.3.12.6 Within the actuarial literature, Philip Booth, in his helpful explication of the Austrian economic perspective, has been amongst those to urge caution in the use of mathematical models (Booth, 1997).

3.3.12.7 Hausman suggests that the empirical problems of economics are insurmountable, and reverts to "the old fashioned view that . . . the method of economics is deductive, and confidence in the implications of economics derives from confidence in its axioms rather than from testing their implications"

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

(Hausman, 1992, p1). This goes full-circle in contradiction of Friedman. Such a view would seem incompatible with mathematical economics, where the direct justification of assumptions along the lines proposed by Mill is explicitly rejected.

3.3.13 *Economic practice*

Despite this barrage of criticisms, the mathematical wing of the neo-classical tradition has remained the dominant school within economics. The main reason for this would seem to be the lack of a popular alternative.

3.3.14 *Methodology of econometrics*

3.3.14.1 One of the major aims of econometrics is to make theory operationally effective through the measurement of key parameters of the economy, and the use of such measurements to assist in the development of predictive models. More controversial are the claims of some econometricians, such as Tinbergen, to be able to use such measurements both to test the validity of existing theories and to develop new theory, e.g. by identifying new variables which are statistically relevant as candidates for parameters. J. M. Keynes, who has been amongst the most aggressive critics of econometrics, was, perhaps, the leading proponent of the view that econometrics could play no role in either determining or testing theory, but merely "of giving quantitative precision to what, in qualitative terms we know already as the result of a complete theoretical analysis" (Keynes, 1939, p560).

3.3.14.2 One of the most substantive points of disagreement which underlies this debate concerns the validity of the statistical techniques on which econometrics is based. Keynes argues that these statistical techniques rely upon such assumptions as the completeness of the chosen factor list; the independence, measurability and linearity of these factors, and the correct specification of the relevant time-lags — conditions which are not found in practice. The defenders of econometrics argued that, whilst it is true that such ideal conditions are not met in practice, the conditions of the economy often approximate sufficiently closely to these conditions to make the method effective.

3.3.14.3 Another recurrent theme within the critique of econometric methods has been concern about the strength of the implied assumptions concerning the stability of the economy. Lionel Robins, for example, argues that: "there is no reason to suppose that uniformities are to be discovered. The 'causes' which bring it about that the ultimate valuations prevail at any moment in time are heterogeneous in nature: there is no grounds for supposing that the resultant effects should exhibit uniformity over time and space" (Robbins, 1932, p99). The failure of econometrics to achieve any significant degree of predictive success, despite the increasing size of econometric models and the computer power which they use, has led many to conclude that the instability of the economy places severe limitations on the power of econometrics. The rational expectations school claims to have identified one reason why such stability should not be expected — the parameters of the economy are themselves a function of the expectations

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

of agents, these are dictated by the state of the economy, and will change over time.

3.3.14.4 In practice, econometrics has not achieved the successful predictive record, over the past several years, for which its proponents would have hoped. It has become apparent that stability is an elusive quality within real economies, creating the need to continually re-specify econometric models as circumstances unfold.

3.3.15 *Methodology of institutionalism*

3.3.15.1 The criticism of neo-classicism from the competing institutional school has been vigorous. Wilber & Harrison reject what they call the 'formal method', the use of prior assumptions and deductive logic, within economics, suggesting "the instability of economic data makes a historic generalisation exceedingly problematic" (Wilber & Harrison, 1978, p67), and noting the failure of such methods to achieve their aim of predictive success. "Formal models simply cannot handle the range of variables, the specificity of institutions and the non-generality of behaviour" (Wilber & Harrison, 1978, p72).

3.3.15.2 The institutional approach defines a number of methodological ground rules, for example: "inquiry must be empirically based, examine process, see social change as evolutionary, focus on institutions, and recognize the important role of values" (Wisman & Rozansky, 1991).

3.3.15.3 Wilber & Harrison, in making the authoritative case for institutional economic methods, assert that the 'holistic, systemic and evolutionary' nature of economic subject-matter implies the need for a different form of explanation. Their analysis of institutional economics suggest that the school does, indeed, engage in a distinctive mode of investigation, that this is common to the diverse activities across the school, and it is this which lends the school its cohesion. They suggest, moreover, that institutional economists have a common mode of explanation — a form of systematic story-telling that they term 'pattern modelling'.

3.3.15.4 According to this account, a central element in this investigative mode of institutional economics, in which the subject-matter selected is viewed as a unified whole, is the 'participant-observer method', the first step of which is the 'socialisation' of the theorist. During this process, the participants, starting as far as possible without preconceived ideas or models, allow the subject-matter to impress upon them its essential structures. This requires getting close to the concrete reality of the unified system to be studied. Recurrent themes which emerge during this process are identified, and these are used to develop hypotheses about the subject-matter, which are then checked against a wide variety of the available data. Attempts to build up a many-sided picture of the subject-matter are encouraged. Once this process is well-advanced, a model may be proposed which links together some of the hypotheses in order to describe the subject-matter. The description of the unified whole which emerges, which is characterised by its focus on recurrent themes, is termed a 'pattern model'. This

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

process allows for further refinement of the model over future periods, as data continue to emerge.

3.3.15.5 Wilber & Harrison note that: "In general, the emphasis in formal models is on laws, while in a pattern model it is on facts or on low level empirical generalisations", and conclude that the "use of a pattern model appears appropriate when an explanation involves many diverse factors, each of which is important; when the patterns or connections amongst these factors are important; and when these patterns can be observed in the particular case under study. Use of the covering law model appears more appropriate when one or two factors or laws determine what is to be explained and when these factors or laws are better known and understood than the specific instance." Unsurprisingly, this leads then to the conclusion that the pattern modelling approach of the institutional school is that appropriate to most economic realities.

3.3.15.6 However, the institutional approach is controversial, and it has remained a minority view amongst economists. Wisman & Rozansky suggest two reasons. Firstly, that neo-classical economics provides a primary support for the market economy, whereas the connection of institutionalism to such ideologically preferred structures is less clear, and secondly, a methodological concern that, in failing to identify a body of laws, institutional economics appears less scientific. They trace this conservatism with respect to laws to a concern about the stability of economic reality, commenting that this "is only a mistake if important trans-historical and trans-cultural regularities exist and they are ignored due to a presumption to the contrary". They conclude that, by emphasising this latter as an empirical question, institutionalists might develop a more constructive dialogue with neo-classical economists.

3.4 *Conclusion*

There is a considerable variety of methods used within economics. The recent debate about these methods is broad and substantial, and worth careful consideration.

4. A Preliminary Characterisation of Actuarial Science

4.1 *Introduction*

4.1.1 This section uses the following two inputs as a basis for characterising actuarial science:

(1) A review of the methods taught within the core educational texts of actuarial science in the U.K. The methods which are taught to new entrants, and the related models, provide the core knowledge and competencies of the profession, on which more advanced developments are based. (The idea of characterising a science by reference to its educational texts is due to Nancy Cartwright (see Cartwright, 1983; or Giere, 1988).)

(2) The paper on 'General Principles of Actuarial Science', presented to the 26th International Congress of Actuaries (Bell *et al.*, 1998), which summarises

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

extensive recent work undertaken by the actuarial profession in the United States of America and Canada looking at the nature of actuarial science.

4.1.2 The consistent view of actuarial science which emerges from a consideration of these inputs is summarised in a brief characterisation which is designed to be helpful in setting actuarial science in the context of the wider methodological debate.

4.1.3 A consideration of the wider actuarial literature and practice within the U.K. confirms that this characterisation captures the methods employed within the mainstream of our discipline. In practice, actuaries do, at times, draw on a wide range of techniques from other disciplines. Mathematisation methods, chaos theory and neural networks have, for example, been amongst the methods recently employed by actuaries. It is not helpful automatically to treat such approaches as forming a core part of the method of actuarial science. Where such techniques are helpful in meeting the aims of the actuary, they are to be welcomed. An articulation of actuarial methodology provides a starting point for understanding the relationship between such methods and those traditionally used by actuaries, and of ensuring methodological consistency where such methods are used in combination. Any account of the methodology of actuarial science necessarily entails a view on which methods it comprises — a debate on such views is to be encouraged.

4.1.4 The characterisation of actuarial science presented in this paper is based on Anglo-Saxon experience. Insofar as actuarial traditions in other territories embody fundamentally different methods, they are not covered within this paper, but would certainly form an interesting topic for further research.

4.2 *Review of the Educational Syllabus used by U.K. Actuarial Students*

4.2.1 *Actuarial education texts*

A synopsis of the core educational material used for actuaries within the U.K. is set out in the *Student Handbook* (SH), published by the Institute and the Faculty of Actuaries. This synopsis contains a list of the objectives of each course. It is the core reading material specified for each of the courses which is taken as the base material for this review.

4.2.2 *Observations on education texts*

4.2.2.1 The following are observations on this material:

(1) Courses A (Fundamentals of Actuarial Mathematics) and D (Actuarial Mathematics) form the core teaching on actuarial methods — their declared aim is "to provide a sound understanding of the actuarial philosophy and the actuarial scientific method." (SH, p9). Key elements of this teaching are:
- — valuation models based on the discounting of future probability-weighted cash flows;
- — simple stochastic interest rate models;
- — methods for collecting data to examine past experience;

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

— methods for monitoring actual against expected experience;
— methods for graduating empirical mortality data to produce smoothed mortality tables;
— an account of recent mortality experience and the key causal influences upon these; and
— the use of different factors to classify risk.

(2) Courses B (Economics and Finance) and C (Statistics) aim to provide an elementary grounding in these related subjects.
(3) Each of the more advanced courses (E - H) is split into two components:
— developing knowledge of the underlying framework (e.g. types of contracts, institutions, tax, accounting, legislation, practice, professional guidance); and
— applying the actuarial philosophy and the actuarial scientific method using an appropriate analysis of the problem/situation.

(4) Each of the courses F, G and H contains sections on:
— the selection of assets in the context of the liabilities;
— bases for valuing assets and liabilities; and
— asset/liability matching requirements.

4.2.3 *Comments*

4.2.3.1 These observations show that a key feature of actuarial methods is the use of empirical observational data of either the situation to be modelled or of similar situations. Often this is used to estimate cash flows, commonly with the use of probability weightings to a range of outcomes. Discounting is used to produce an approximate measure of value. The basic model-type used is of deterministic form — the stochastic model approach is mentioned.

4.2.3.2 Actuarial training includes guidance on how to collect empirical data and use these as a basis for actuarial models — notable examples being the use of mortality, lapse and other decrement data. This use of observational data often results in the derivation of simple step functions, such as the age-step mortality table, to approximate reality. Such step functions can be used to provide approximate integrations, either using standard tabulations or through the power of modern technology.

4.2.3.3 The stability of the empirical observations over time is examined. If the data exhibit a stable trend over time, this may be built into the model — e.g. the secular improvement in annuitant mortality. Where the historic observations fluctuate significantly from one period to the next, the model will generally be recognised as being less reliable than would be the case where the data exhibit a stable pattern over time.

4.2.3.4 The structure of the course emphasises the focus of the educational syllabus on application of the theory (especially in investment, life, general insurance, and pensions). The student is trained to develop practical solutions to

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

specific real situations (e.g. the funding rate for a given pension scheme, or the premium on a given life assurance contract). Account is taken of the features of the specific situation which are thought material, e.g. rules for the amount of benefit to be paid on death, withdrawal, survival, etc., and these are used to develop appropriate assumptions for the model. Many of the claims that actuaries make about reality are thus situation-specific (as opposed to global truths).

4.2.3.5 Central to the training of actuaries is the recognition of the approximate nature of the models which are developed. The practical tuition ensures that members of the profession monitor the performance of their models against emerging experience, and modify their models as appropriate. Recognition is given to the approximate nature of the model results by:

(1) The use of much celebrated actuarial caveats and disclaimers — this feature of actuarial advice is so ingrained, in practice, that it is often caricatured humorously. It does, however, play a valuable role in drawing attention to the level of uncertainty that is inherent in the given situation, and the extent to which the actual outcome might vary from that which is expected. Often, attention is drawn to key exogenous causal features which could have material influence.
(2) Margins are often taken so that, for example, additional amounts may be held as reserves to guard against adverse experience. The margins may be implicit, through the use of conservative assumptions, or explicit, as in the case of statutory solvency margins on life assurance business. The size of the margin will often reflect the degree of uncertainty — extra margining may be established in situations where experience (relevant empirical data) is limited.
(3) Showing the sensitivity of the result to changes in the key assumptions made in developing the model.

4.2.3.6 The role of the judgement of the actuary is recognised as essential to this modelling process. Particular value is ascribed to previous experience of using similar models in similar situations.

4.3 *Review of the North American Work to Develop General Principles of Actuarial Science*

4.3.1 The Society of Actuaries and the Casualty Actuarial Society have joined forces to develop a set of general principles of actuarial science. The work to date was presented to the 26th International Congress of Actuaries (Bell *et al.*, 1998), and some further development is anticipated over the coming year.

4.3.2 A key figure behind this work is Sam Gutterman, the previous President of the Society of Actuaries. He set out in his last presidential address his views on the necessity for the redesign of the actuary — crystallising the principles of actuarial science is a major component of this project (Gutterman, 1996). Gutterman's preferred definition of an actuary forms the underpin for the later jointly written paper:

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

"Actuaries are professionals who identify and analyse the implications of future possibilities, especially with respect to risk. In conducting their analyses, actuaries develop one or more models to estimate the financial impact of future uncertain events. The models reflect the decision maker's objectives and risk tolerances, and incorporate explicit assumptions based on:

— historical experience from similar types of exposures or related phenomena; and
— in-depth knowledge of the environment in which the future experience will occur, which can differ from the environment from which data were obtained.

This knowledge enables actuaries to assess:

— the relevance and reliability of available historical or related data; and
— the validity of the models and their sensitivity to changes in assumptions and model specifications;

resulting in an assessment of a range of model results over potential scenarios.

To enable the decision-maker to effectively operate in an environment of risk and uncertainty, actuaries interpret the results of these models so that practical alternative approaches can be developed to manage future outcomes. Because of the difficulty of predicting future contingent events and their consequences, these approaches are often dynamic; that is, periodic evaluations of emerging experience and prospects for future change are utilised to enable actuaries to appropriately modify their estimates or to more appropriately manage the risks undertaken."

4.3.3 Bell *et al.* characterises actuarial science as an 'applied' science which is "based on generally accepted concepts and observations distilled from the experience of practitioners. The principles of actuarial science are extracted from this experience." (Bell *et al.*, 1998, p147). The paper goes on to outline these principles. The characterisation of actuarial science as 'applied', accords with the observation of the central importance of tackling problems in specific real situations within the U.K. educational material. We may characterise the main objective of actuarial science as being to provide advice on institutional (in its widest sense) policy decision-making in specific real situations.

4.3.4 Bell *et al.* goes on to outline the nature of stochastic models as "mathematical models in which the representation is expressed in terms of probabilities". They comment that: "A stochastic model does not predict the outcome of a single experiment prior to its being carried out. However, it can be used to derive an estimate of the expected value (and other characteristics) of a random variable ... Stochastic models can be based on previous experiments or can utilize prior assumptions about the probabilities of various events". A scenario is "a set of assumptions about future conditions". In the scenario model approach, "alternative scenarios could be the source of the possible present values, and the respective likelihoods of the scenarios could provide the probabilities".

4.4 *U.K. Statement on the Role of the Actuary*

In the U.K., the Presidents of the Institute and the Faculty have sponsored work to develop a statement which encapsulates the role of the actuary. The

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

proposition that 'Actuaries Make Financial Sense of the Future' and the supporting statement (Ferguson & Grace, 1997) is consistent with the characterisation set out above. This early work by the U.K. profession provides an excellent start, but is less advanced than that of the profession in North America. It is to be hoped that further work will be sponsored in this area, perhaps in conjunction with our North American colleagues. A helpful starting point might be a codification of U.K. practitioner principles to consider alongside those outlined in the recent U.S. work.

4.5 *Scenario and Stochastic Modelling*

4.5.1 The characterisation of Bell *et al.* emphasises strongly the importance of scenario/stochastic models. Both scenario and stochastic models are also an integral part of the current practice of the U.K. actuarial profession — this is an area where practice is ahead of our educational syllabus. A useful reference work on stochastic models is Daykin *et al.* (1994). Feldblum sets out an interesting discussion of the relationship between scenario models and stochastic models (Feldblum, 1995). Redington was critical of stochastic modelling, on the grounds of lack of statistical regularities which could sustain such mathematical models of the future (see e.g. 'Prescience and Nescience', Redington, 1983). Huber also addresses the controversy (Huber, 1996, p142).

4.5.2 The following summary is intended to grasp the essential elements of these modelling methods.

4.5.3 *Scenario modelling*

4.5.3.1 The usual starting point for such a model is the development of a small number of pictures of possible futures which are defined by a qualitative account of how the world would be. The description extends only to the areas of interest to the modeller, but the set of scenarios is intended to span a wide range of possible futures. The definition of the scenario generally aims to limit some dimensions of uncertainty in order to underpin future projections — this works best if the picture painted within the scenario facilitates an intuitive sense of whether it approximates to other possible outcomes. An example of a possible set of such pictures is:

(1) EMU survives in the founder countries for the next 15 years, but the U.K. does not join;
(2) EMU survives in the founder countries for the next 15 years, and the U.K. joins;
(3) one or more founder members leave EMU during the next 15 years; and
(4) EMU collapses during the next 15 years.

4.5.3.2 A quantitative dimension is generally added to this qualitative picture by making a set of assumptions concerning parameters of interest for each of the scenarios. Parameters of interest might be U.K. GDP growth, Sterling exchange rates, or foreign exchange business volume for some specified period. The

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

quantified assumptions are estimates of the parameters which seem appropriate within the context of the possible future which has been painted.

4.5.3.3 It is usual to consider a model for some quantity of interest, which is a dependent variable of the parameter set quantified in the scenario. Quantities of interest might be the profit or solvency capital of some company for some specified period. By considering the outcome for this quantity of interest in the various scenarios, insight is sought into the range of possible outcomes. Additional insight may be achieved by sensitivity-checking the impact on the dependent variable to changes in the key quantitative assumptions in any of the scenarios. Sometimes probabilities are associated with each of the scenarios — this allows a probability weighted expected value of the dependent variable to be calculated.

4.5.3.4 It is clear that this process of scenario modelling entails considerable skill. The scenarios identified certainly do not cover all possible futures — in practice it is necessary to keep the number small in order to ensure that the model is manageable. Careful choice of suitable scenarios is therefore essential. Usually it is appropriate to choose a set of distinct scenarios which are sufficiently extreme to bound the range of likely outcomes.

4.5.3.5 A scenario model might be used to assess the broad quantum of risk arising from uncertainty concerning the future environment. It might also be used to identify the types of environment which are likely to be most problematic. Such insights may allow management actions to be taken to control the degree of risk.

4.5.3.6 Scenario modelling is by no means a specifically actuarial activity, it is also commonly used in applied economics. Scenario modelling is used as a method of dealing with the high degree of uncertainty concerning the future which exists in financial realities. Experience has shown that single assumption sets are generally insufficient to capture the range of possible futures.

4.5.4 *Stochastic modelling*

4.5.4.1 Stochastic modelling is closely related to scenario modelling. In its common actuarial use, a number of specified quantitative parameters (e.g. an interest rate, an inflation index) are generated by a Monte Carlo simulation that steps forward through time, generating the parameters at each time step as a function of the parameters at the previous time step together with some random variation. Generally the state of the parameters at the time horizon of interest (i.e. some specified number of time steps) is recorded as output from the run. By using a sufficiently large number of runs, insights are gained into the degree of variation in possible futures. Often a probability of $1/n$, where n is the number of runs used, is implicitly or explicitly attached to the possibility that the parameters are as recorded in the run output. This allows a probability density function to be estimated for quantities of interest. Again, it is common to employ a model of some quantity of interest expressed as a function of the parameters generated by the stochastic model.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Table 2. Characterisation of actuarial science

	Feature	Characterisation
1	Objective	Institutional policy advice in specific situations
2	Method	Build model appropriate to specific situation to capture its future behaviour
3	Reality modelled	Financial
4	Models	Discounted cash flow (DCF) models Deterministic, scenario and stochastic models
5	Empirical	Models are based on: (1) understanding of reality to be modelled, including its causal structure (2) historic facts of situation to be modelled, or similar situations (3) knowledge of changes in the environment which will modify future experience Model is monitored as experience unfolds, and modified as appropriate
6	Approximation	Range of possible futures acknowledged Sensitivity-checking of results to assumptions and model specification Use of scenario and stochastic models
7	Modelling skill	Modelling skill is required to build appropriate model—especially: (1) determine structure of model appropriate to situation (2) identify historic data relevant to situation to be modelled (3) adjust model to reflect likely difference of future from past experience Experience of use of models in similar situations recognised as valuable Principles of modelling are extracted from experience

4.5.4.2 Although the use of stochastic models is now widespread, and many of the outputs are used to support decision-making, little investigation has been attempted into the reliability of the insights which are offered by stochastic models. A common intuition is that the results are less reliable at short durations (i.e. very few time steps) or long durations, where the model tends to converge back towards a deterministic model determined by the parameters chosen. One of the key assumptions is the stability over time of the process which generates the parameters. Although new techniques are being developed which build in greater degrees of variability, and arguably more realistic output distributions, the problem of validating such models remains.

4.6 *Conclusion*

The North American view of actuarial science ties up closely with that derived

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

from a review of the U.K. educational material. There appears to be a high degree of consensus concerning the nature of actuarial science, and this can be captured within a simple characterisation, as is given in Table 2.

5. THE METHODOLOGY OF ACTUARIAL SCIENCE

5.1 *Introduction*

5.1.1 The methodology of actuarial science today is somewhat less advanced than the methodology of economics in the period around 1980. Since a debate on the nature of actuarial science towards the end of the last century (e.g. Sprague, 1875; Young, 1880, 1897), little formal analysis of actuarial method appears to have been attempted. More recent contributions include those of Frank Redington (Redington, 1986) and work on the nature of actuarial models (e.g. Jewell, 1980; Hickman, 1997). The first work within the profession which employs an extensive understanding from formal methodology is some recent excellent analysis by Paul Huber, considering actuarial economic models (Huber, 1996; Huber & Verrall, 1999).

5.1.2 The maelstrom of change within philosophy has had an ubiquitous and important impact upon the operation of science. Interestingly, mainstream actuarial practice, with its looser links to academia and its strong practical orientation, has remained relatively immune to such change when compared with other disciplines, and this has tended to insulate the distinctive characteristics of actuarial science.

5.1.3 By exploring the characterisation of actuarial science derived in the previous section, in the light of the methodological overviews presented earlier, this section crystallises some observations concerning the method used by actuaries.

5.2 *Methodological Assessment of Actuarial Science*

5.2.1 Points 1 and 2 of Table 2 imply a concern with the modelling of specific realities. Importantly, we observe that actuarial science is not concerned with the development of laws.

5.2.2 The applied and situation specific nature of actuarial science has been a crucial influence on the methodological characteristics which it has developed. A key characteristic is a respect for the context within which the model is to be applied. This respect is demonstrated in the model-building stage as well as in the model-application stage. The actuarial approach uses local knowledge of the situation to be modelled.

5.2.3 Points 3, 4 and 5 of Table 2 indicate that actuarial science starts with an investigation of the data relevant to the specific situation to be modelled. The actuary's training and experience will guide an investigation of the data relevant to the causal drivers of the situation — for example, the key influences on the cash flow pattern, such as mortality and lapse rates. The existence of stable

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

regularities is regarded as an empirical matter — where a decrement rate has been stable for some historic period, this will lend confidence to the predictive power of a model which uses this rate as a parameter.

5.2.4 *How actuarial science operates with limited regularities*

5.2.4.1 This account of the actuarial approach shows how it manages to operate effectively within realities which are changing, uncertain and irregular, but which exhibit some limited regularities, by searching for and using these limited regularities to derive low-level generalisations. Actuarial science builds models with some degree of predictive power, based on these low-level generalisations. By regarding stability as an on-going empirical matter, actuarial science recognises the limits of the regularities available, both at each point in time and over time.

5.2.4.2 In addition to this treatment of deterministic models as a method for dealing pragmatically with the limited regularities of the financial realities, actuarial science also makes use of scenario and stochastic models. Scenario models allow the range of outcomes of a model to be explored, by using the available partial knowledge about the possible range of futures which is envisaged—this allows approximate bounds to be placed on the uncertainty. Stochastic models make assumptions concerning certain probabilities, either directly or via algorithms, in order to define, within the model, the degree of uncertainty about the future. This is likely to be an effective method where the assumptions of the model capture features of reality which are sufficiently stable, but leaves open the problem of model risk — the possibility that the structure and parameters of the model are not a good approximation to the relevant reality for the period of interest.

5.2.5 *Allowing for the changing pattern of reality*

5.2.5.1 In addition to the projection forward of the limited regularities available within the historic data, actuarial science uses an empirical investigation of changes in the key drivers of the situation in order to anticipate changes between the nature of past and future regularities. Where new causal influences give rise to a lack of stability (e.g. the rapid growth of HIV), further investigation into the changing causes and their implications is appropriate. This investigation of the underlying drivers is consistent with a causalist perspective on science.

5.2.5.2 Allowing for the changing nature of reality is a highly challenging task for the model builder, especially when reality is changing rapidly or in ways that are new. It is clear that phenomena do sometimes occur that have not previously occurred. For models to capture reality in such situations, they need to capture the essential causal structure of the reality, and respect the nature of these causes.

5.2.5.3 For example, during the early days of HIV there was considerable uncertainty about the future pattern of mortality. Actuaries noted that medical

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

experts identified the underlying cause as blood-based viral transmission. The principal mechanisms for such transmission were identified as anal intercourse, the use of unsterilised syringes, blood transfusion, and (more controversially) vaginal intercourse. Using this causal account of the situation allowed a scenario model of possible future mortality rates to be developed. As experience was monitored over subsequent years, the degree of uncertainty recognised by actuaries has been considerably reduced.

5.2.6 *Approximation*

5.2.6.1 Point 6 in Table 2 refers to approximation in actuarial science. A recognition of the limited nature of the regularities available at each point in time, the limited nature of the available data in the light of a complex reality, and the recognition of the change over time of the reality being modelled, all contribute to the actuary's cautious attitude to the accuracy of the model results. It is in the cautious form of the statement of conclusions, in the recognition of ranges of possible outcomes, and in the recognition of the need for on-going empirical checking of the model, that we witness the actuary's strong embrace of approximation.

5.2.6.2 The actuary uses a knowledge of the key causal drivers relevant to the situation in order to consider the implications of possible shifts in the key causal parameters (e.g. economic variables, lapse experience, mortality, etc.) upon the outcome of the model. Rapid shifts in causal parameters lead the actuary to recognise an increased level of uncertainty in the forecasts of the model — the degree of approximation is broader.

5.2.7 *Modelling skill*

Point 7 in Table 2 refers to modelling skill. Actuarial science has long recognised the need for skill within the process of developing and using models in an applied context. This derives, in large part, from the recognition of the need for respect for the specific features of the situation being modelled. As Bell *et al.* observe, actuarial principles derive from the bedrock of actuarial experience — this experience is predominantly that of applied modelling. Much of the professional literature is concerned to share experience of applied modelling.

5.3 *Summary of Key Methodological Characteristics of Actuarial Science*

Viewed from the broader methodological perspective outlined in earlier sections, the key characteristics of actuarial science which emerge from these observations are that it:

- — is not concerned with a search for laws;
- — operates consciously within realities which exhibit limited regularities, and treats the existence of regularities as an empirical matter;
- — is concerned with developing contextual models to develop approximate predictions of the future, not with truths;
- — uses the available local knowledge;

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

— uses knowledge of the causal structure of reality to help develop predictive models; and
— acknowledges a valuable role for modelling skill.

5.4 *Methodological Comments on Actuarial Method*

5.4.1 This summary indicates that actuarial science has a number of clearly identifiable characteristics which give it a distinctive character. The cautious empirical approach to the use of regularities would seem to be an effective method of operating, which fully embraces Hume's concern that we cannot have certain knowledge of the future.

5.4.2 *A possible model for science in realities which lack powerful regularities*

5.4.2.1 The actuarial method would also seem to be an effective way forward in the light of concerns about modernism, that simple laws do not adequately describe complex realities. Rather than appeal to putative laws, actuarial science deals directly with low-level generalisations, recognising the limited nature of available regularities.

5.4.2.2 Actuarial science is concerned with the development of models which approximate the behaviour of reality and have a degree of predictive power, not with truth. The approach to models is consistent with the representational view of models which has recently emerged, as discussed in Section 2 (e.g. in the work of Giere, Hacking, Cartwright).

5.4.2.3 Acceptance of the non-uniformity of reality implies the need for knowledge of the variations in the specific locality of interest. This recognition of the variability of reality, and thus the need for the use of local knowledge, places new emphasis on the context of model application. Again, this is consistent with recent moves in philosophy to explore the *de facto* 'rules' for the use of models which are adopted by practitioners — the practitioner lore (see e.g. Collins, 1985).

5.5 *A Classification of Model Types*

5.5.1 The above analysis indicates that models are central to the actuarial method. A number of differing types of model are used, some simple and some more complex. Table 3 sets out a classification of model types relevant to applied modelling as a basis for further characterisation of actuarial methods.

5.5.2 Actuaries use models of all three types. A good example of an assumption-based model is a rule of thumb, e.g. "a good annual pre-tax profit from a life business is 1% of funds under management". A main assumption here is that there is a sufficiently stable relationship between profit and funds under management — to challenge this is to suggest that this is not a very helpful rule of thumb. The 1% figure would not be challenged by an individual datum that differed empirically, but would be by a mass of data. A rule of thumb model has the characteristic of being simple, easy to apply, and usually has some intuitive rationale. Most importantly, the result is acknowledged as crude and

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Table 3. Classification of model types

Model type	Characteristics of model type
Assumption-based models	Defined by universal, often powerful, assumptions Facts may be used to establish parameter values Facts not used to challenge the assumptions which define the model
Fact-based models	Assumptions driven predominantly by local facts Specific to situation or type of situation Form of model implies some unchallenged assumptions (e.g. value is a function of future cash flows) Sensitivity checking of other assumptions Conclusions/forecasts are approximate and tentative
Extended fact-based models	Fact-based model adjusted to reflect knowledge of changing causal influences Where empirical base of facts is limited, assumptions may be used based on experience to fill in the gaps

approximate. If a detailed report produces a value of 1.94 and my rule of thumb says 1.8, then this is some reassurance which provides an initial check on the detailed report. If the report were to suggest 8.72 for the 1.8 rule of thumb number, then this would point to the need for further investigation.

5.5.3 The characteristic of a rule of thumb is that it employs a powerful simplifying assumption, and that the transparently simple nature of the model makes apparent the broadness of its approximation. This may be contrasted with mathematical models in which set theoretic assumptions are used as a basis for the deduction of models which are complex – such models are not the traditional home of actuaries.

5.5.4 Common actuarial models are fact-based, e.g. actuarial DCF models of a life office or of a pension fund using lapse ratios, mortality and real expense growth assumptions derived from historic experience. Although DCF models do rest on the assumption that value is an additive function of weighted cash flows, the pattern and weighting of the cash flows is left open to the modeller. Other assumptions, such as the discount rate, will generally be subjected to sensitivity-checking. As noted in the analysis above, actuaries typically extend their fact-based models by considering the changing nature of the causal influence of the situation, and modifying their models accordingly.

5.5.5 Scenario and stochastic models are generally more difficult to classify. Although very simple stochastic models may be fact-based, Paul Huber's discussion of stochastic models used by actuaries to capture economic risks shows that these models are by no means methodologically simple (Huber, 1996; Huber & Verrall, 1999).

5.6 *Conclusion*

5.6.1 Actuarial science does have a powerful and distinctive method. By developing predictive models of specific situations based on low-level

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

generalisations, derived from a causal understanding of such realities, it establishes models which are robustly tied to reality. Actuarial science does not hypothesise models which are precisely correct universally, but rather it proposes models which, it is tentatively confident, are approximately correct in a specific situation under consideration, and methods for monitoring experience so that confidence in the models will generally rise over time.

5.6.2 The distinctive characteristics of the methodological model provided by actuarial science and its potential to operate with limited regularities suggest that it is worthy of careful consideration by methodologists.

6. A Methodological Comparison of Actuarial Science with Economic and Other Sciences

6.1 *Introduction*

6.1.1 Having reviewed the methodology of economics, and set out an account of the methodology of actuarial science, this section compares the two. The overlap of economics and actuarial science, in addressing financial realities, renders this comparison particularly fruitful. This comparison helps to develop the account of the methodology of actuarial science set out in the previous section, and to deepen the understanding of a number of points. It also provides an important part of the methodological framework for considering recent debates within the actuarial profession. Casting these debates in this new light should help to move them forward.

6.1.2 The main comparison is between actuarial science and the neo-classical tradition within economics, the school which currently dominates thinking within economic work in finance. However, the methodological comparison with econometrics and institutional economics also provides some important insights.

6.2 *A Comparison of Actuarial Science and Neo-Classical Economics*

6.2.1 *Comparison of objectives*

6.2.1.1 The energetic debates in the methodology of economics have involved some lively disagreements about its objectives. Is economics about prediction, explanation, mathematical virtuosity, law-discovery, persuasion, or some other ends? Hutchinson suggests "the shift in economists' views as to the aims or ends of their subject is mainly responsible for, and has complicated, the explosion of the methodological controversy in the last decade or so" (Hutchinson, 1994, p 28).

6.2.1.2 This paper has observed that much less confusion or ambiguity exists within the actuarial domain — the focus is very clearly upon developing models with some degree of predictive power to assist in institutional policy decisions. This clarity of objective of actuarial science side-steps much of the abstract intellectual debate of the methodology of economics, and provides an important characteristic of actuarial methodology.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

6.2.2 *Assumption-based vs fact-based models*

6.2.2.1 Section 5 notes that, whilst actuarial science uses a range of model-types, its dominant methodological style is the use of empirically derived low-level generalisations within extended fact-based models. Neo-classical economics, especially in its recent mathematical guise, employs the H-D method to derive claims about the world from assumptions — its models are essentially assumption-based. The bottom-up approach of actuarial science thus contrasts with the top-down approach of neo-classical economics.

6.2.2.2 In physics, the presence of powerful regularities and universal constants means that some simple assumptions capture the relevant facts very accurately. In astronomy, the assumption of point masses and frictionless movement are very good approximations to the facts throughout a wide area of application. Under these circumstances, there is not a clear distinction between assumption-based and fact-based models — the two coincide. In financial realities there are no universal constants and regularities are less powerful. Simple universal assumptions do not, thus, fit the facts in the same compelling way as they may sometimes in physics. There is, thus, a much clearer distinction between models based on simple universal assumptions and models based on the facts.

6.2.2.3 The doubts which currently surround the H-D method render the status of claims deduced from assumptions as uncertain. For the applied scientist, this uncertainty may be side-stepped by interpreting such models as single-scenario models, in which the assumptions are assumed to hold over the future period of interest. The question of interest in this applied modelling context is whether the model defines a scenario which is approximately correct in the given situation to which it is applied. This perspective on assumption-based models helps to move away from a discussion of whether such models are correct. For many purposes with which the actuary is concerned, it will be appropriate to define a model which incorporates more than one scenario. Although a single-scenario model may often be approximately correct, the same model may also sometimes be very wrong.

6.2.3 *Dealing with limited regularities and approximation*

Section 5 discussed how actuarial science deals with limited regularities and approximation. The assumption-driven approach of neo-classical economics, by contrast, is not stylistically well-suited to limited and changing regularities. It is not generally easy to make adjustments to the powerful assumptions used so as to reflect the flexing of causal circumstances. Deductive logic does not admit the flexibility to admit uncertainty. One way forward might be to develop an account of how the vague *ceteris paribus* clauses modify the deduced claims, perhaps extending the proposals of Hausman (Hausman, 1992).

6.2.4 *The use of judgement within applied modelling*

6.2.4.1 The applied modelling activity of neo-classical economics can be directly compared with the applied modelling work within actuarial science. By

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

contrast, the theoretic and empirical work in neo-classical economics, which we have seen are closely associated with the use of the H-D method, have no immediate counterparts within actuarial science, which is not concerned with a search for laws.

6.2.4.2 Prime examples of the application of mathematical economic models are to be found in the use by investment market practitioners of mathematical finance models, such as the capital asset pricing model (CAPM), and the Black-Scholes option-pricing equation. Observation of such practitioners indicates that they manage to fit the models to the real situation by establishing a method for choosing parameters which achieves the right result. These methods fall outside of the theory of the model. A good example is the development by practitioners of implied volatility surfaces, parameterised for term and moneyness of the option (Pemberton *et al.*, 1998; Pemberton, 1997a). The context of application generates its own rules, accepted wisdoms, rules of thumb, and practices for choosing models and model inputs which form a practitioner lore, generally unwritten, which is taught, accepted and used on an everyday basis by practitioners. This practitioner lore involves explicit recognition of the role of skill as an ineliminable part of the modelling process.

6.2.4.3 Applied modelling has two components — model building and model application. There would not appear to be any examples of effective modelling of specific financial situations which do not make recourse to judgement. The actuarial approach brings judgement into the model-building process, as well as using it in model application. In mathematical economics, the use of powerful universal assumptions and deductive logic largely removes judgement from the context of model building — applied modelling, using the models of mathematical economics, tends to lay the emphasis on the use of judgement within the context of model application.

6.2.4.4 There are advantages to the actuarial approach of using judgement in both areas. This allows all the available knowledge to be brought into the model. Both local empirical knowledge of the regularities relevant to the specific circumstances, and knowledge of the causal structure of the reality to be modelled, can be used. By maximising the use of empirical knowledge, the role of judgement can be limited to either filling in for missing facts or interpreting the implications of the changing causal influences upon the future. Explicitly recognising the role of judgement allows the areas and manner of its use to be documented and discussed within open professional debate. Where judgement is restricted to the context of application of the model, as is generally the case within the application of mathematical economic models, a divide is created between theory and practice. Often the role of judgement is not explicitly admitted, but remains closet, and thus undiscussed. Moreover, it may not be easy to exercise judgement in an effective manner where it is squeezed into the process of model application.

6.2.5 *Summary/conclusion*

6.2.5.1 Actuarial science and neo-classical economics are methodologically

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

very different. The former is largely concerned with building models bottom-up from low-level generalisations, whilst the latter operates principally top-down. Actuarial science has no counterpart of the search for general laws evident within neo-classical economics. There is a much more direct comparison between actuarial science and the body of activity involved in applying neo-classical models to specific realities these have strong similarities in their empirical approach.

6.2.5.2 These methodological differences imply that naïve intermingling of actuarial and economic methods is unsound. Integrating the methods of actuarial science and neo-classical economics requires considerable care — there is much to be learnt from an investigation of the areas in which differences between the two approaches arise.

6.2.5.3 The skill of the modeller forms an integral part of the actuarial method. As noted above, the context of model application is an area with which methodologists are only now beginning to grapple, but it is by no means clear that any form of applied modelling could avoid a similar acceptance of judgement. Further philosophical consideration of this issue is required.

6.2.5.4 A next step is to provoke a consideration of these methods within the broader philosophical community. Developing a careful articulation of the methodological model provided by actuarial science is a pre-requisite for this. The latest shifts in thinking of some of the most influential strands of philosophical opinion hold out the hope that the methods of actuarial science may provide a valuable role model.

6.3 *A Comparison of the Methodology of Actuarial Science and other Economic Methods*

6.3.1 Econometrics is concerned with using data to establish models of reality, although, as discussed above, this does not imply a rejection of theory. Whilst this approach shares with actuarial science a concern with the relevant empirical data, the approach to approximation and adjustments for the changing pattern of influences would appear to be quite different.

6.3.2 There is a strong resonance between the emphasis of institutional economics on low-level generalisations, in the light of a careful investigation of the situation of interest, perhaps through socialisation, and the method of actuarial science. Huber correctly notes important parallels between the pattern modelling approach favoured by the institutionalists and the scenario approach of actuarial science (Huber, 1996, p178).

6.3.3 Further investigation of the relationship between actuarial science and the range of economic methods which lie outside of the neo-classical school would be fruitful.

6.4 *A Comparison of the Methodology of Actuarial Science and other Social and Natural Sciences*

6.4.1 Although the dominant model for the methodology of science is that of

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

physics, and this has had great influence on the methodologies of social science, perhaps most notably in neo-classical economics, there are other important methodological models. We have noted above the methodological models of institutional economics and econometrics. Other sciences, such as biology, geology, psychology, sociology, political science and statistics, each have a distinctive methodology. Actuarial science is by no means alone in the challenges which it faces in establishing models of complex realities.

6.4.2 In the field of biology, Stephen Jay Gould has discussed the danger of making simplifying assumptions concerning the intellectual worth of humans, such as that adopted by the craniologists or some psychologists (Gould, 1981). In sociology, concerns about the modernist programme's search for laws has given rise to a powerful post-modernist movement (Best & Kellner, 1991). In politics, axiomatic systems, such as that of Marx, have suffered widespread reversals. In psychology, axiomatic systems, such as that of Freud, are increasingly challenged by more eclectic and systemic approaches, which lay a greater emphasis on the knowledge of the practitioner (Kvale, 1992; McNamee & Gergen, 1992). Growing divisions between theory and practice in this area have led to a split of the U.S. professional body. In statistics, the validity of standard statistical tests has been called into question, on the grounds that the underlying assumptions, such as independence and normality, are invalid (Draper, 1995). In management science, the methods used increasingly recognise the need to make extensive use of the deep knowledge of managers within a business in order to contextualise the business model.

6.4.3 Many of these methodological developments, which reflect a growing concern with the use of axiomatic models, have relevance for actuarial science. There is much to be learnt by the actuarial profession from a consideration of these broader areas.

7. Case Study: A Methodological Review of Option Valuation

7.1 *Introduction*

7.1.1 So far this paper has addressed methodological issues from a theoretic perspective. This section looks at an example of the application of this methodological theory within a specific practice area — that of option valuation. This provides an excellent example for a number of reasons:

(1) it is generally recognised as the pinnacle of success of mathematical neo-classical economics;

(2) it provides an excellent and clear example of a proof within mathematical economics (probably the one most familiar to actuaries);

(3) there is an enormous amount of activity in applied modelling using mathematical economic models – and this has a distinct and characterisable practitioner lore; and

(4) it provides a concrete application in which actuarial and mathematical economic methods may be compared.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

7.1.2 This section outlines how option valuation exhibits the characteristics of neo-classical economics, which have been described in the previous sections. Option valuation exhibits clearly the distinction between the three areas of focus of mathematical economics: (1) theory; (2) empirical testing; and (3) applied modelling. The educational material focuses heavily on theory. The treatment of the mathematical conclusions suggests that the proofs have sometimes fostered unwarranted beliefs.

7.1.3 The section also compares the approach of actuarial science.

7.2 *Option Valuation Theory and Empirical Tests*

7.2.1 The centrepiece of the work in this area is the classic proof of Black & Scholes (Black & Scholes, 1973). The paper lays out clearly the assumptions which are used, e.g.:

"(a) the short-term interest rate is known and is constant through time.

(b) the stock price follows a random walk in continuous time with a variance rate proportional to the square of the stock price...The variance rate of the return on the stock is constant" (Black & Scholes, 1973, p640).

7.2.2 These assumptions are used in conjunction with the principle of no arbitrage, in order to derive some partial differential equations which must be satisfied. To solve the equations, appeal is made to mathematical solutions developed within thermodynamics. The solution is then stated, and some statements in English are then made, which reinterpret the formula as claims about real options, e.g.: "From equations (13) and (14), it is clear that xw_1/w is always greater than one. This shows that the option is always more volatile than the stock" (Black & Scholes, 1973, p645).

7.2.3 Much has been written on the relaxation of the assumption set of Black & Scholes (e.g. see review in C. W. Smith, 1976). Smith concludes that "No single assumption seems crucial to the analysis" (C. W. Smith, 1976, p4) — any of the assumptions can be removed if we replace them with equally strong alternatives or accept weaker conclusions. "The solution to the option pricing problem when the stock price movement is discontinuous involves the unobservable expected return on the stock" (C. W. Smith, 1976, p31). If the diffusion assumption is dropped, then the powerful conclusions of Black-Scholes are lost.

7.2.4 Much has also been written on tests of the empirical adequacy of the Black & Scholes solution (e.g. Black & Scholes, 1972; Beenstock, 1982). One point is certainly clear, that there is no agreement on whether the tests confirm or disconfirm the theory. Beenstock, for example, in contrast to the optimism of Black and Scholes, concludes that "Option prices are sensitive to the stochastic processes that determine underlying stock prices" (Beenstock, 1982, p40).

7.3 *Option Valuation Applied Modelling*

Given an option to value, a practitioner considers the characteristics of the

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

option, selects an appropriate model to use, and then chooses numerical values for the required inputs to the model. This process of model selection and use is guided by practitioner lore, rather than by the theory of neo-classical economics associated with the proof of the model. Although the practitioner lore concerned with the valuation of options is well-known amongst a large international group of option specialists, it still remains largely unwritten. An outline of some of the key points of this lore is set out in Pemberton *et al.* (1998).

7.4 *Practitioner Lore*

7.4.1 The models most commonly used by practitioners to value options, in practice, are variants of Black-Scholes (e.g. binomial models). Parameters required as inputs to the valuation process are the key contract terms (e.g. term to expiry, strike price, etc.) and an assumption concerning the volatility of the underlying instrument. The precise form of practitioner lore is often dictated by the proprietorial guidelines associated with the models of the individual investment banks and institutions.

7.4.2 The critical free parameter is the volatility, and generally this is chosen either by reference to the historic volatility of the underlying or by reference to an implied volatility surface. Typically, the implied volatility surface is derived by fitting a smooth surface to a set of implied volatilities of options for which prices are available. In the case of each option, the implied volatility is the value of the volatility parameter which equates the option valuation formula assumed with the observed option price. The surface chosen usually expresses the implied volatility as a function of the term and the moneyness of the option.

7.4.3 Whether historic or implied volatilities are used, the actual parameter value chosen to value a given option involves the skill of the practitioner. The differing views of appropriate model and parameter values will give rise to a range of quoted prices within the market. In practice, implied volatilities are predominantly used as a basis for quoting prices, so that, for example, this morning's prices are effectively estimated as a smoothed version of yesterday's price with some adjustment for the overnight time value. This ensures that the range of price estimates across the market is generally quite narrow.

7.4.4 There are many widely accepted guidelines on how the models should be used. For example, if there is a large readily identifiable downside risk present, such as a general election, for example, then the practitioner might consider using a skew parameter within the model. The existing pattern of prices is used to guide this choice.

7.4.5 The assumption of practitioner lore in this area, that volatility is a function of term and moneyness, is in tension with the constant volatility assumption underlying the theory (see e.g. Simons, 1998, p25). 'Relaxation' of the constant volatility assumption allows it to follow some simple patterns that are known in advance, but not for material levels of variation and uncertainty. This tension implies a complex relationship between the models used by practitioners and the theory.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

7.4.6 Andrew Smith provides an excellent insight into practitioner lore in a recent discussion of model risk (Pemberton *et al.*, 1998, Section 2.4). He notes that a single over-arching model covering all the options in an investment bank's portfolio does not prove practical — rather it is necessary to use a series of overlapping models tailored for specific areas (e.g. equity options, fixed-interest options, etc.). Dealing with the relationship and choice between this patchwork of models is a major challenge for the skill of modellers.

7.5 *Option Valuation Educational Texts generally focus on Theory*

Option valuation is a standard component of introductory texts on mathematical economics within the area of finance (see e.g. Brearley & Myers, 1996; Elton & Gruber, 1981; Ingersoll, 1987; Hull, 1997). Such texts generally focus on the mathematical derivation of the Black-Scholes pricing formula from various sets of assumptions. Some of the texts, especially those that are more specialist, provide some limited comments on the use of the formula, but practitioner lore is not generally taught, or even referred to, so that the practice of option valuation is not presented as a process of judgementally-based approximation.

7.6 *The Implications of Option Valuation Proofs for Beliefs*

7.6.1 Black & Scholes' 1973 paper tends to present their formula as the solution to the value of an option — it contains no mention of approximation or the skill required to choose input parameters. The paper appears to have led some to believe that the Black-Scholes formula provides an exact solution to the value of an option. Cox & Rubinstein, for example, write a chapter under the heading 'An Exact Option Pricing Formula' (Cox & Rubinstein, 1985, Section 5), whilst others make such comments as: "Black and Scholes succeeded in solving their differential equations to obtain exact formulas for the prices of European put and call options" (Hull, 1997, p240) or "We would now like . . . an exact option-valuation model — a formula we can plug numbers into and get a definite answer. The search for that formula went on for years before Fischer Black and Myron Scholes finally found it." (Brearley & Myers, 1996, pp572/3). Although other texts present a more realistic view (see e.g. Elton & Gruber, 1987, Chapter 20), the confusion has led some students to believe that Black-Scholes provide the 'correct' theoretic solution. The perpetuation of such misleading theoretic tuition material would seem to be an unnecessary encouragement of unwarranted beliefs.

7.6.2 Black-Scholes state that "the option value as a function of the stock price is independent of the expected return on the stock" (Black & Scholes, 1973, p644). There is widespread evidence within the literature that the Black-Scholes proof has given rise to a belief in this precise universal claim.

7.6.3 More recently, it has been generally recognised that the Black-Scholes formula is sometimes limited as a tool for producing approximate option values. "It is always a matter of judgement which simplifying assumptions can be made and whether the resulting model is sufficiently accurate for the purpose for which

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

it is being used" (Simons, 1997). Fischer Black's 1989 paper (Black, 1989) outlines a number of circumstances when the Black-Scholes formula is likely to be very wrong. Perhaps the best example is in bid situations, where the shape of the price distribution for the underlying is likely to be double-humped.

7.7 *A Comparison with the Actuarial Approach to Option Valuation*

7.7.1 Due to the limitations of space within this paper, I have addressed the important topic of the actuarial method within option valuation, and its comparison with the approach of mathematical economics, in separate papers (Pemberton, 1997; Pemberton, 1997a). These papers show how the actuarial method can be applied in this area by treating an option quite naturally as an uncertain future cash flow. The actuarial approach facilitates approximation, and allows consistency with the valuation of other asset and liability cash flows.

7.7.2 From the perspective of actuarial science, each of the option valuation models of mathematical economics represent a scenario. For example, the basic Black-Scholes model represents a scenario in which the stock price follows a diffusion process with constant variance, and in which tax and risk interactions are assumed away. It would seem that, for many short-dated equity options, this scenario is often a good approximation to the real world circumstances, but this differs very considerably from the bid situation scenario where the distribution becomes markedly double-humped. The mathematical economics approach does not naturally provide a good basis for understanding the degree of approximation of the models under a suitable range of future real world scenarios. Using the shape of the distribution of the underlying at the expiry date, as in the actuarial method, may provide a more helpful basis for addressing this question of approximation. The Black-Scholes solution is achieved precisely if a lognormal shape assumption is used in conjunction with a risk-free discount rate.

7.7.3 Interestingly, some of the major investment banks have now moved away from teaching the classic mathematical economic justification for their models, and moved towards teaching a discounting of uncertain cash flows approach, which is more in accord with actuarial methods.

7.7.4 Although much of the work of actuaries in such areas as liability guarantees is concerned with the valuation of quite complex options, the profession has been rather slow to develop and articulate a distinctive approach to option valuation. It has often preferred to intermingle the use of the mathematical economic model of option values with actuarial models. Such intermingling may result in conflicts arising in the assumptions which are being used. For example, actuarial stochastic investment models generally imply price movement distributions for equities which have fat tails. This is certainly a welcome feature of David Wilkie's stochastic investment model (Wilkie, 1986). Wilkie has proposed using this model in conjunction with Black-Scholes option prices (Wilkie, 1987). The latter prices can be derived to form a lognormal assumption for the equity price movement distribution. Compatibility of these values with the Wilkie model has not been demonstrated.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

7.7.5 Robert Clarkson provides helpful contributions in this area (Clarkson, 1997).

8. Implications and Opportunities for Actuarial Science

8.1 *Introduction*

This paper has provided an articulation of the methods of actuarial science which reflects recent important developments within both the methodology and the practice of related sciences. This articulation provides a basis for strengthening and furthering the use of these actuarial methods. It provides a sound basis for determining the relationship between actuarial science and mathematical economics, and thus for developing the use of mathematical economic methods and models within the context of actuarial science. It also provides a basis for a fresh articulation of the methods of actuarial science within various areas of application, and the refinement or upgrading of the account of these methods to make use of methodological insights.

8.2 *The Opportunity for Actuarial Science to Develop alongside Mathematical Economics*

8.2.1 The opportunity for actuaries which arises from mathematical economics is the integration of such methods with those of actuarial science, rather than the abandonment of actuarial science in favour of mathematical economics.

8.2.2 *The need for exploration of the relationship between actuarial science and mathematical economics*

8.2.2.1 An important and valuable contribution to the U.K. actuarial profession, in recent years, has been a number of papers introducing the ideas of mathematical economics (e.g. Exley *et al.*, 1997; Kemp, 1996; A. D. Smith, 1996). There is widespread agreement that these methods are different from those of actuarial science, and this has given rise to an extensive debate within the profession (e.g. Institute of Actuaries, 1993).

8.2.2.2 Section 6 identifies the major methodological differences between actuarial science and mathematical economics, and notes the need for care in attempting to integrate these methods. A careful case-by-case consideration of the methodological implications of juxtaposing actuarial and neo-classical economic methods is required. It would be unwise to assume the soundness of an 'intermingled' method in the absence of such an investigation. An example of possible model incompatibilities was noted in Section 7.

8.2.2.3 In order to progress further the use of mathematical economic models within the profession, the next step is to explore their relationship with those of actuarial science. Exploring this relationship is likely to strengthen our understanding of both sets of methods. The methodological discussion within this paper provides a framework for this further investigation.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

8.2.3 *The opportunity for a synthesis within applied financial modelling*

8.2.3.1 The interests of actuarial science in using applied models of financial realities are shared by those practitioners who apply the models of neo-classical economics within this area. Section 6 notes that considerable common ground exists between these groups, and this includes the use of experience to assess likely ranges of parameter values and suitable adjustments to the model using the experience and skill of the modeller. This common ground raises the possibility of forming a synthesis between these groups, from which the actuarial profession could learn much.

8.2.3.2 Many practitioners have extensive experience of applying mathematical economic models in real situations. Further empirical investigation into the nature of the practitioner lore in this area, and the extent to which this embodies systematic wisdom, would be helpful.

8.2.3.3 On the other hand, actuarial science contains a mature framework of empirical investigation. Many practical issues concerning the empirical application of models have been debated over the years within the academic, yet pragmatic, environment which exists within the profession. This experience could complement the capability of existing practitioners, who use neo-classical economic models, to produce jointly an enhanced applied modelling capability.

8.2.4 *The need for care in using mathematical economic models within applied science*

Mathematical economic models are derived from universal assumptions. Although there may be some scope to choose a set of assumptions which is relevant to the reality being modelled, the extent to which the model is contextualised is limited. The use of judgement within the applied modelling process is thus largely restricted to the context of model application. The use of such models calls for particular care.

8.2.5 *Ensuring a suitable range of scenarios*

8.2.5.1 The powerful assumptions used in mathematical economics often define quite a narrow range of scenarios. Where there is the ability to alter numerically some parameters within the mathematical model which is derived, this allows for consideration of a family of scenarios. However, this family will not generally span the space of all possible scenarios.

8.2.5.2 For scenario modelling to be effective, it is important that the range of scenarios should be chosen having regard to the nature of the real situation being investigated, and should be sufficiently broad. The choice of scenarios should not be guided merely by mathematical convenience. It may be that limiting consideration to the scenarios implied by the powerful assumptions of neo-classical economics does not provide a set of scenarios which covers, in a helpful way, the set of possible futures. It is quite possible that there are scenarios which dictate very different outcomes from those implied by the assumptions used.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

8.2.5.3 Increasingly, the power of modern technology allows the treatment of a broader range of models through the use of approximation techniques—this offers increased scope to choose a more complete set of tractable scenarios (see e.g. Focardi, 1996).

8.2.6 *Clarifying the role of proofs within applied modelling*

8.2.6.1 Section 3 discusses the role of proof within the H-D process, and how this forms part of the theory component of neo-classical economics. It was noted that the relationship between this theory and applied modelling activity, which makes use of neo-classical economic models, is complex. From a practical point of view, there is a need for the actuarial profession to determine what use it should make of the proofs of neo-classical economics for the purpose of practical applied modelling using neo-classical economic models.

8.2.6.2 Proofs may often provide insights into the behaviour of the model in the scenario implied by the assumptions. From an applied modelling standpoint, however, it is crucial to ensure a balanced view across a range of potential scenarios which are relevant. It is important that focusing, in detail, on one scenario, or a small range of scenarios, does not upset this balance. It is more important to understand how the form of behaviour within the model will play out, approximately, in a range of possible futures, rather than its detailed implications within a single specific scenario.

8.2.6.3 Another possibility is that the proof elucidates certain principles (e.g. the 'no arbitrage' principle). If these principles are relevant only to the single scenario, it is right to question whether they need to be taught. If the principles have application across a wider range of scenarios, then it may be better to isolate the relevant principles from the specific context of the proof, and to teach the general principle in this less specific context.

8.2.6.4 Another possible argument is that the model could not be easily defined or developed without the assumption-theoretic framework of the proof. It is difficult to identify examples where this is the case.

8.2.6.5 There is certainly scope for the actuarial profession to apply mathematical economic models in an empirical manner. The case for teaching the mathematical derivations of these models remains to be made. Whilst understanding these proofs may, in some circumstances, be helpful, it is important to check that teaching the proofs meets the applied modellers' needs. There may be, for example, a risk that teaching such proofs distracts attention from the approximate nature of the model — this may be confusing to the student of actuarial science, especially at the stage when it is important to encourage the student to develop experience and practical judgement of approximation through repeated use of models. It would be helpful for the profession to establish, on a case-by-case basis, some compelling examples of direct benefit to the applied modeller of learning the proof in question. Careful case-by-case consideration of the value of such proofs is required.

8.2.6.6 Where such proofs are taught, it important that they form part of a

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

broader account of how to develop models which provide adequate approximations to the reality in question. It is essential that they are not interpreted as implying the truth of the conclusions deduced. As noted in Section 7, there is strong evidence that some practitioners in the option valuation field have fallen into this 'proof belief danger', and thus effectively ended up believing their own simplifying assumptions.

8.2.6.7 The teaching of a proof may be thought of as adding a tool to the actuarial tool kit. There is a need to be discriminating and effective in selecting the tools which meet the requirements of our applied science. Theoretic tools have a cost to acquire, may be expensive to maintain, inappropriate for the applied environment, or simply irrelevant to the task of the practitioner. A culinary skills course that focuses too heavily on chemistry or biology theory is unlikely to be successful in producing top class chefs (although, clearly, a summary of some of the key relevant points of chemical or biological theory might be helpful).

8.3 *Developing and Extending the use of Actuarial Science*

8.3.1 The following are examples of areas in which the articulation of the actuarial methods developed above might be used as a basis for extending the use of actuarial methods. This is not, by any means, intended to be an exhaustive list, but rather to provide some illustrations.

8.3.2 *Valuation models*

8.3.2.1 The valuation of patterns of future uncertain cash flows is the traditional home of actuarial practice. More recently, it has been proposed that a pricing approach, consistent with the methods of mathematical economics, should be preferred to traditional actuarial approaches (e.g. Mehta, 1992; Exley *et al.*, 1997). These issues are worthy of careful and extensive consideration. Due to the limited space available within this paper, I have explored this subject in a separate paper, 'The Value of Actuarial Values' (Pemberton, 1998). Although observable prices contain much information about values, they do not make necessary allowance for the investor-specific tax and risk position — this local contextual information is assumed away if we adopt prices directly as values. The paper argues for the development and articulation of actuarial values in a way which explicitly respects the information contained within prices.

8.3.2.2 This area is a nice example of the use by actuaries of contextualised models. These models have been developed over many years in connection with a number of specific liability types; most notably life assurance, general insurance and pensions; and capture the asset/liability risks specific to the situation. Perhaps the most celebrated example of such a valuation model is the statutory net premium valuation (NPV) method for with-profits life funds. The use of a discount rate based on the yield on the fund, together with resilience tests based on the actual asset mix, aims to capture the asset aspects of the risk. The model's ability to capture reality derives, in part, from a theoretic understanding of how

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

the model works (e.g. an 'artificially' low valuation rate of interest rate provides implicit margins to cover, *inter alia*, future bonus declarations), but also from many years of practical experience of using the model in real situations. This is a good example of a model that aims to capture the main features of reality in an approximate fashion, while brushing rather broadly over many of the details — it currently provides the basis for the statutory monitoring of the solvency of the U.K. life assurance industry. In recent years changes to the nature of the products written, principally the introduction of unitised with-profits business, have led to a debate about the extent to which the NPV basis remains an adequate approximation (Scott *et al.*, 1996; Wright *et al.*, 1998). It is interesting to note that the solutions proposed by these working parties are pragmatic developments of the existing contextual models, rather than a radical shift to the use of models based on capital market theories.

8.3.2.3 This active focus on the development of valuation models, and the tension which exists between the actuarial and mathematical economic approaches within this area, makes this an excellent area in which to explore how contextualised models might be developed using the fresh insights provided by mathematical economics.

8.3.3 *Option valuation*

8.3.3.1 We noted, in Section 7, that the application of actuarial methods to option valuation offers the opportunity to develop more robust value approximations which are consistent with other assets and liabilities within the portfolio. The preferred method of option valuation is likely to depend on its purpose. If we wish to update option price quotations from last night to this morning, it is likely to be best to use an implied volatility surface, perhaps in conjunction with some simple version of the Black-Scholes formula. Actuaries are often concerned to deal with the assets in the context of the liabilities, and, for this purpose, an actuarial valuation model is likely to be preferable. Valuation requires, *inter alia*, respect for the investor-specific tax and risk position (Pemberton, 1998).

8.3.3.2 There is a strong argument that regulators should be concerned with values as well as with interpolated prices. Many of the options of interest to the actuary are embedded options for which no closely-related transaction prices are available. Even where closely-related option prices exist, there is a tendency for these markets to become illiquid when circumstances are volatile. Wilkie has suggested an approach along actuarial lines, looking at historic experience to assess reasonable worst case outcomes (Wilkie, 1992). As Malcolm Kemp points out, this is likely to lead to reserves of an order of magnitude or more greater than under conventional methods, based on mathematical economic models (Kemp, 1994, Section 4.6). Wilkie's approach is in tune with prudential guidance issued by the Department of Trade and Industry, which calls for provisions for adverse changes in respect of derivatives (DTI, 1995). The point is also reflected in the new actuarial guidance on the use of derivatives (Guidance Note 25),

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

which requires consideration of both historic data (Section 4.1.6) and appropriate resilience tests (Sections 4.1.10 and 4.1.11).

8.3.3.3 Recent developments within mathematical economic approaches to option valuation are increasingly embracing the need for more contextualisation of the standard models in order to capture reality adequately. Examples include adopting a jump diffusion process (Kemp, 1996, Appendix B), modelling the changing volatility of the underlying (Ritchken, 1997), and adopting more generalised diffusion processes (e.g. Hudson, 1998). All such developments require the specification of further parameter values, implying the need for further application of the skill of the modeller in fitting the model to the relevant reality.

8.3.3.4 The relationship between actuarial and mathematical economic models in this area is thus increasingly complex. There are potentially significant implications of using actuarial rather than mathematical economic models. Further careful investigation of the relationship between these models is required. There are potentially major advantages, both in respect of simplicity and reliability, to the adoption of actuarial methods for option valuation. There is considerable scope for the profession to make a major contribution through a constructive programme of development within this area (Pemberton, 1997, pp17-19).

8.3.4 *General insurance*

General insurance is an area in which actuarial science is already well entrenched. Coutts & Thomas (1997), for example, provide a fine recent account of the use of an actuarial stochastic modelling approach to general insurance. Interesting boundary issues arise with mathematical models of risk, which it would be helpful to address from a formal methodological perspective.

8.3.5 *Banking*

8.3.5.1 The banking industry has generally adopted the capital markets models of risk, but actuarial cash flow models have considerable relevance to this business, and there is a major opportunity to develop the use of actuarial methods within this area (Allan *et al.*, 1998). The increasing overlap between the life and banking industries creates an interesting new opportunity to explore the relationship between the actuarial and capital market paradigms.

8.3.5.2 Within the banking area, tensions exist between the building block method of determining capital requirements and the value at risk (VAR) approach. The building block approach implies the use of global simplifying assumptions concerning the nature of risk. Under the VAR method, local facts are used to develop a model for the specific situation — this is closer to an actuarial approach, especially where a simulation approach is adopted. From the regulatory point of view, the VAR approach is more problematic, because it is less uniform, and thus more difficult to control, but the greater degree of realism within this approach has advantages (Allan *et al.*, 1998, ¶2.6.9).

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

8.3.6 *Corporate finance*

In the corporate finance area, simple actuarial techniques are increasingly being adopted by investment bankers as pressure grows to develop increasingly accurate and sophisticated valuation models. The actuarial profession has a major opportunity to help those starting to use such techniques to develop a competitive advantage.

8.3.7 *Risk management for projects*

One area in which actuarial methods have wide application is in the analysis and management of project risk. A recent paper, prepared jointly by the U.K. actuarial profession and the Institution of Civil Engineers, provides an excellent account of how actuarial methods may be applied in this area (Institution of Civil Engineers *et al.*, 1998).

8.3.8 *Other areas*

Section 6 noted methodological developments in other social and natural sciences which would seem relevant to actuarial science. The articulation of actuarial methods developed in this paper shows them to be distinctive and powerful within realities in which there are limited regularities. It would be interesting to explore the potential for actuarial methods within these other areas. In development economics, for example, there has been widespread and heated criticism of the failure of the models of Western neo-classical economics to respect the cultural characteristics local to many of the contexts in which they are applied (e.g. George, 1994). The actuarial model of science would seem to have considerable potential application in this area. Further investigations in such areas are beyond the scope of this paper.

9. Conclusion

9.1 Actuarial science has a distinctive method which is well-suited to operating within realities in which there are limited regularities. It uses local empirical knowledge to grasp low-level generalisations, has respect for the pattern of local causal influences, and builds bottom-up models for the purpose of establishing approximations. It recognises the role of skill in applied modelling, and treats it as central to the method. It is currently focused on financial realities.

9.2 The methodological insight that more mathematics is often less science is consistent with the intuition of the actuarial profession. Following the spirit of Redington's celebrated comment, it might be suggested that an applied scientist is a mathematician, but an applied scientist who is just a mathematician would be a very poor applied scientist.

9.3 An articulation of the methods of actuarial science, such as that developed here, provides a basis for strengthening and furthering their use. Exploring the relationship between actuarial science and mathematical economics

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

provides a basis for developing a more holistic understanding and approach to financial modelling in a number of areas, perhaps, most notably, those in which capital market models are currently applied. There is scope to develop a synthesis amongst the work of applied modellers in the financial sphere, which should include a clarification of the role of proof for this activity. Areas in which there is major scope to develop the use of actuarial methods include valuation modelling, option valuation, banking, corporate finance, project risk management and development economics. The next steps to realise the potential of actuarial science involve developing further such applications, and, in parallel with this, extending the debate and understanding of actuarial methods to which this paper has sought to contribute.

9.4 As well as having powerful methods, the actuarial profession also has strong skills and capability, a close-knit professional framework well-developed in the exchange of ideas, and extensive practical experience of modelling financial realities. This provides a basis for the profession to make a valuable contribution within a broader sphere of application — it is important that the profession accepts a leadership role in order to fulfil this potential.

9.5 There is already a strong programme within the profession concerned with developing an account of actuarial methods and furthering their use. The work sponsored by Gutterman in the U.S.A. (Bell *et al.*, 1998), and Ferguson & Grace in the U.K. (Ferguson & Grace, 1997), together with a new focus on the future role of the profession (Nowell *et al.*, 1996), give this programme considerable substance. The methodological account of actuarial science, using the formal study of scientific methods, introduced within this paper, aims to add further momentum to this programme, with the aim of helping to carry the application of our methods firmly into broader spheres.

REFERENCES

ALLAN, J.N., BOOTH, P.M., VERRALL, R.J. & WALSH, D.E.P. (1998). The management of risks in banking. *B.A.J.* **4**, 707-802.

ARROW, K. (1963). The role of securities in the optimal allocation of risk bearing. *Review of Economic Studies*, **31**, 91-96.

BACKHOUSE, R.E. (1994). *New directions in economic methodology*. Routledge.

BEENSTOCK, M. (1982). The robustness of the Black-Scholes option pricing model. *Investment Analyst*, October 1982, 30-40.

BELL, L.L., BRENDER, A., BROWN, R.L., DICKE, A.A., GRAMER, C.R., GUTTERMAN, S., HUGHES, M.A., KLUGMAN, S., LUCKNER, W., MCMURRAY, M.A., PHILBRICK, S.W., ROBERTS J.N., TERRY, K.F., TAN, J.H., WALTERS, M.A., WOLL, R.G. & WOODS, P.B. (1998). General principles of actuarial science. *Transactions of the 26th International Congress of Actuaries*, **1**, 145-170.

BEST, S. & KELLNER, D. (1991). *Postmodern theory: critical interrogations*. Macmillan Press.

BLACK, F. (1989). How to use the holes in Black-Scholes. *Journal of Applied Corporate Finance*, **1**.

BLACK, F. & SCHOLES, M. (1972). The valuation of option contracts and a test of market efficiency. *Journal of Finance*, **27**, 399-417.

BLACK, F. & SCHOLES, M. (1973). The pricing of options and corporate liabilities. *Journal of Political Economy*, **82**, 637-654.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

BLAUG, M. (1980). *The methodology of economics or how economists explain.* Cambridge University Press, New York.

BOOTH, P.M. (1997). Knowledge, wisdom, or understanding? *The Actuary*, May 1997.

BREARLEY, R.A. & MYERS, S.C. (1996). *Principles of corporate finance (fifth edition).* McGraw-Hill Series in Finance.

CALDWELL, B. (1994). *Beyond positivism. Revised addition.* Allen and Unwin, London.

CARTWRIGHT, N. (1983). *How the laws of physics lie.* Clarendon Press.

CARTWRIGHT, N. (1989). *Nature's capacities and their measurement.* Clarendon Press.

CARTWRIGHT, N. (1998). *The dappled world.* Cambridge University Press (forthcoming).

CARTWRIGHT, N. & JONES, M. (1998). *Correcting the model: idealization and abstraction in science.* Poznan (forthcoming).

CLARKSON, R.S. (1997). An actuarial theory of option pricing. *B.A.J.* **3**, 321-410.

COLLINS, H. M. (1985). *Changing order.* University of Chicago Press.

COX, J.C. & RUBINSTEIN, M. (1985). *Options markets.* Prentice-Hall.

COUTTS, S.M. & THOMAS, T.R.H. (1997). Modelling the impact of reinsurance on financial strength. *B.A.J.* **3**, 583-654.

DAYKIN, C.D., PENTIKÄINEN, T. & PESONEN, M. (1994). *Practical risk theory for actuaries.* Chapman and Hall.

DEBREU, G. (1959). *Theory of value.* Yale University Press.

DE MARCHI, N. (Editor) (1988). *The Popperian legacy in economics.* Cambridge University Press, Cambridge.

DEPARTMENT OF TRADE AND INDUSTRY (1995). *Use of derivative contracts in insurance funds; DTI Insurance Division.* Prudential Note 1995/3.

DRAPER, D. (1995). Assessment and propagation of model uncertainty. *Journal of the Royal Statistical Society, Series B,* **57**, 45-97.

ELTON, E. J. & GRUBER, M. J. (1981). *Modern portfolio theory and investment analysis.* John Wiley & Sons.

EXLEY, C.J., MEHTA, S.J.B. & SMITH, A.D. (1997). The financial theory of defined benefit pension schemes. *B.A.J.* **3**, 835-966.

FAMA, E.F. (1970). Efficient capital markets: a review of theory and empirical work. *Journal of Finance,* May 1970.

FELDBLUM, S. (1995). Forecasting the future: stochastic simulation and scenario testing. In *Incorporating risk factors in dynamic financial analysis.* Casualty Actuarial Society, 152-177.

FERGUSON, D.G.R. & GRACE, P.H. (1997). Actuaries make financial sense of the future. *Letter to members of the Institute and the Faculty of Actuaries,* 10 April 1997.

FEYERABEND, P.K. (1975). *Against method: outline of an anarchist theory of knowledge.* NLB, London.

FOCARDI, S. (1996). From equilibrium to non-linear dynamics in investment management. *Journal of Portfolio Management,* Summer 1996.

FRIEDMAN, M. (1953). *Essays in positive economics.* University of Chicago Press, Chicago.

GEORGE, S. (1994). *A fate worse than debt.* Penguin.

GIERE, R.N. (1988). *Explaining science: a cognitive approach.* University of Chicago Press.

GOODMAN, N. (1965). *Fact, fiction, forecast (second edition).* Bobbs-Merrill, New York.

GOULD, S.J. (1981). *The mismeasure of man.* Penguin.

GRAY, J. (1997). Overquantification. *Financial Analyst Journal,* November/December 1997.

GUTTERMAN, S. (1996). Presidential address: the redesign of the actuary. *The Actuary* (note: U.S. not U.K. version), December 1996.

HAAVELMO, T. (1944). The probability approach in econometrics. *Supplement* to *Econometrica,* **12**.

HACKING, I. (1983). *Representing and intervening.* Cambridge University Press, Cambridge.

HAUSMAN, D. M. (1992). *The inexact and separate science of economics.* Cambridge University Press.

HEMPEL, C.G. (1965). *Aspects of scientific explanation.* Free Press.

HICKMAN, J.C. (1997). Introduction to actuarial modeling. *North American Actuarial Journal,* **1**, 3, 1-5.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

HUBER, P.P. (1996). *A study of the fundamentals of actuarial economic models.* Doctoral thesis for City University (available from the Institute of Actuaries Library).

HUBER, P.P. & VERRALL, R.J. (1999). The need for theory in actuarial economic models. *B.A.J.* **5**, 377-395.

HUDSON, R.L. (1998). Does quantum stochastic calculus have a role to play in mathematical finance? Nottingham University, unpublished manuscript.

HULL, J.C. (1997). *Options, futures and other derivatives (third edition).* Prentice-Hall International.

HUME, D. (1738). *A treatise of human nature.* Reprinted: Clarendon Press, 1966.

HUTCHINSON, T.W. (1994). Ends and means in the methodology of economics. In *'New directions in economic methodology'.* ed. R. Backhouse, Routledge.

INGERSOLL, J.E. (1987). *Theory of financial decision making.* Rowman & Littlefield.

INSTITUTE OF ACTUARIES (1993). This house believes that the contribution of actuaries to investment could be enhanced by the work of financial economists. *J.I.A.* **120**, 393-414.

INSTITUTE AND FACULTY OF ACTUARIES (1997/8). *Student handbook.*

INSTITUTION OF CIVIL ENGINEERS, FACULTY of ACTUARIES, INSTITUTE of ACTUARIES (1998). *Risk analysis and management for projects.* Thomas Telford.

JEWELL, W.S. (1980). Models in insurance: paradigms, puzzles, communications and revolutions. *Transactions of the International Congress of Actuaries,* **S**, 87-141.

KEMP, M.H.D. (1994). Provisions for 'adverse deviations' for U.K. insurers. Unpublished manuscript.

KEMP, M.H.D. (1997). Actuaries and derivatives. *B.A.J.* **3**, 51-180.

KENNEDY, P. (1992). *A guide to econometrics. (third edition).* Blackwell.

KEYNES, J.M. (1936). The general theory of employment, interest and money. *The collected writings of John Maynard Keynes,* **VII**, 1973. Macmillan Cambridge University Press.

KEYNES, J.M. (1939). Professor Tinbergen's method. *Economic Journal,* **49**, 558-68.

KUHN, T. (1970). *The structure of scientific revolutions. (second edition).* University of Chicago Press, Chicago.

KVALE, S. (1992). *Psychology and postmodernism.* Sage Publications.

LAKATOS, I. (1970). Falsification and the methodology of scientific research programmes. In *Criticism and the growth of knowledge,* ed. Lakatos & Musgrave. Cambridge University Press, Cambridge.

LATSIS, S.J. (editor) (1976). *Method and appraisal in economics.* Cambridge University Press, Cambridge.

LAWSON, T. (1997). *Economics and reality.* Routledge.

LEIJONHUFVUD, A. (1968). *On Keynesian economics and the economics of Keynes.* Oxford.

LEONTIEF, W. (1982). Academic economics. *Science,* **217**, 104-107.

LIPSEY, R.G. (1963). *An introduction to positive economics.* Weidenfeld and Nicolson.

MACKIE, J.L. (1974). *The cement of the universe.* Oxford University Press, Oxford.

MCCLOSKEY, D. (1983). The rhetoric of economics. *Journal of Economic Literature,* **21**, 135-58.

MCNAMEE, S., Gergen, K.J. (1992). *Therapy as social construction.* Sage Publications.

MEHTA, S.J.B. (1992). Allowing for asset, liability and business risk in the valuation of a life office. *J.I.A.* **119**, 385-455.

MILL, J.S. (1843). *A system of logic.* Reprinted 1949 by Longman, Green & Co, London.

MIROWSKI, P. (1994). What are the questions? In *New directions in economic methodology,* ed. R. Backhouse, Routledge.

MORGAN, M.S. (1990). *The history of econometric ideas.* Cambridge University Press, Cambridge.

NAGEL, E. (1963). Assumptions in economic theory. *American Economic Review: Papers and Proceedings,* **53**, 211-219.

NOWELL, P.J., MCCUTCHEON, J.J., TOMPKINS, P.D.G., MIDDLEMISS, K.A., SCOTT, P.G., HITCHCOX, A.N., GOFORD, J., DASH A.C., BATTAMS, E.A., MILNE, R.J.H., GOODWIN, E.M., CAMPBELL, G.B.L. (1996). *The future of the profession. B.A.J.* **2**, 325-427.

O'HEAR, A. (1989). *An introduction to the philosophy of science.* Clarendon Press, Oxford

OLSEN, R.A. (1998). Behavioral finance and its implications for stock-price volatility. *Financial Analyst Journal,* March/April 1998.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

PEMBERTON, J.M. (1997). *Equity option valuation made simple – and more reliable.* Paper presented to the Staple Inn Actuarial Society.

PEMBERTON, J.M. (1997a). *Actuarial option valuation – value consistency considerations.* Paper for the 1997 Actuarial Investment Conference.

PEMBERTON, J.M. (1998). *The value of actuarial values.* Paper presented to the Staple Inn Actuarial Society.

PEMBERTON, J.M. (1998a). *Why idealized models in economics have limited use. Correcting the model: idealization and abstraction in science,* Ed. Nancy Cartwright and Martin Jones, Poznan, (forthcoming).

PEMBERTON, J.M., MEHTA, S.J.B., PAXTON, T.A., SMITH, A.D., VAN BEZOOYEN, J. (1998). An update on option pricing techniques. *AFIR/Investment Conference Proceedings, 1998.*

PIGOU, A.C. (ed.) (1925). *Memorials of Alfred Marshall.* London.

REDINGTON, F.M. (1986). *A ramble through the actuarial countryside. Collected papers and speeches.* Ed G. Chamberlain. Institute of Actuaries Student Society.

RITCHKEN, P. (1997). Valuation of models for risk management and overcoming model risk. Case Western Reserve University, Unpublished manuscript.

ROBBINS, L. (1932). *An essay on the nature and significance of economic science.* Macmillan, London.

ROLL, R. (1977). A critique of the asset pricing theory tests; part I: on past and potential testability of the theory. *Journal of Financial Economics,* **4,** 2, 129-176.

ROSENBURG, A. (1994). What is the cognitive status of economic theory? in *New directions in economic methodology.* ed. R. Backhouse, Routledge.

RUSSELL, B. & WHITEHEAD, A.N. (1910). *Principia mathematica.* Cambridge University Press, Cambridge.

SALMON, W. (1985). *Scientific explanation and the causal structure of the world.* Princeton University Press, Princeton.

SAMUELS, W. (1969). On the future of institutional economics. *Journal of Economic Issues,* **3** (September), 67-72.

SAMUELSON, P.A. (1965). Proof that properly anticipated prices fluctuate randomly. *Industrial Management Review,* **6,** 41-49.

SCOTT, P.G., ELLIOTT, S.F., GRAY, L.J., HEWITSON, T.W., LECHMERE, D.J., LEWIS, D. NEEDLEMAN, P.D. (1996). An alternative to the net premium valuation method for statutory reporting. *B.A.J.* **2,** 527-585.

SIMONS, K. (1997). Model error. *New England Economic Review.*

SMITH, A.D. (1996). How actuaries can use financial economics. *B.A.J.* **2,** 1057-1193.

SMITH, C.W. (1976). Option pricing: a review. *Journal of Financial Economics,* **3,** 3-51.

SNEED, J.D. (1979). *The logical structure of mathematical physics.* D. Reidel Publishing Company.

SPRAGUE, T.B. (1875). On the usefulness of mathematical studies to the actuary. *J.I.A.* **18,** 403-416.

STALEY, C.E. (1989). *A history of economic thought from Aristotle to Arrow.* Blackwell.

VAN FRAASSEN, B.C. (1980). *The scientific image.* Clarendon.

VEBLEN, T. (1961). *The place of science in modern civilisation.* Russell and Russell, New York.

WILBER, C.K. & HARRISON, R.S. (1978). The methodological basis of institutional economics: pattern modelling, storytelling, and holism. *Journal of Economic Issues,* **XII,** 1.

WILKIE, A.D. (1986). A stochastic investment model for actuarial use. *T.F.A.* **39,** 341-403.

WILKIE, A.D. (1987). An option pricing approach to bonus policy. *J.I.A.* **114,** 21-76.

WILKIE, A.D. (1992). On the prudential valuation of written options. *Transactions of the 24th International Congress of Actuaries.*

WISMAN, J.D. & ROZANSKY, J. (1991). The methodology of institutionalism revisited. *Journal of Economic Issues,* **XXV,** 3.

WITTGENSTEIN, L. (1921). *Tractatus logico-philosophicus.* Routledge and Kegan Paul.

WRIGHT, P.W., BURGESS, S.J., CHADBURN, R.G., CHAMBERLAIN, A.J.M., FRANKLAND, R., GILL, J.E., LECHMERE, D.J. & MARGUTTI, S.F. (1998). A review of the statutory valuation of long-term insurance business in the United Kingdom. *B.A.J.* **4,** 803-864 and 1029-1058.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

YOUNG, T.E. (1880). Can a law of mortality be represented in a mathematical form? *J.I.A.* **22**, 139-140.

YOUNG, T.E. (1897). The nature and history of actuarial work as exemplifying the mode of development and the methods of science. *J.I.A.* **33**, 97-131.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

ABSTRACT OF THE DISCUSSION

Mr J. M. Pemberton, F.I.A. (introducing the paper): Why do we need to look at actuarial methods? Any actuarial problem that we face requires us to choose a method, and the choice available within the financial area is ever increasing. Therefore, the choice becomes more difficult. We can choose in an *ad hoc* or in an intuitive fashion, or, preferably, we can set our choice within a more formal framework. The methodology of science provides such a framework. It is difficult and complex; but it is subject to a history of over 2,000 years. It provides, probably, the only framework in which we can embed the consideration of our methods.

The paper summarises recent developments in the methodology of science and in the methodology of economics. It also places actuarial science in the context of the formal study of method.

In order to develop a broader perspective, the profession needs to clarify the methods of our science. We need to identify its key distinctive characteristics. These are that it is empirical, applied and approximate. We need to consider actuarial methods alongside those of other disciplines: economics, with which we share the financial field; and other sciences, such as statistics and psychology. Then we need to develop our own methods by understanding the relationship between our methods and those of other sciences, and exploring how actuaries might use these other methods within the context of actuarial science.

The methods of economics offer particular opportunities. Good work has been done within this profession in recent years, introducing ideas from mathematical economics, in particular, into this forum. To take that forward, we need to explore further the relationship with actuarial science, and to develop a more holistic understanding of, and approach to, modelling.

Modelling is central to actuarial science, and the relationship between our models and reality is crucial to the validity of actuarial science. Our models are not always exactly correct. Indeed, they are not even always approximately correct. Even the good models that we have in finance are, at best, sometimes approximately right, and occasionally very wrong. The demise of the Long Term Capital Management hedge fund illustrates dramatically how models can sometimes be very wrong, and creates a defining moment in the history of financial modelling. The retrospective recognition of error in such models is not a sound management approach. We need to recognise, in advance, that a model can be very wrong. Mathematised models need to be regrounded in empirical facts. Actuaries are very well placed to take a leadership role in such developments.

The aim of the paper is to start a process of extending our understanding of actuarial methods in order to encourage the development of those methods, to extend their effective use into new fields and to extend, indeed, the use of new methods within the context of actuarial science.

The differing disciplines of the philosophy of science, economics and actuarial science are all large areas in their own right. To cover the cross connections requires a broad range of concepts, and the depth of analysis in the paper is limited. The essential aim is to start the debate.

Professor N. Cartwright (a visitor, opening the discussion): The author offers us a fine critical comparison of the techniques for modelling and predicting in actuarial science versus those commonly employed in more 'basic', or theoretical, sciences, especially in theoretical economics. The interest in this project will be two-fold.

The first is efficiency, as mentioned by the author. Actuaries, like practitioners of other professional disciplines, have to be good at what they do, and continually improve how they do what they do. This may leave little time for reflection on the principles underlying their methods or on general strategies for improvement. Often the skills and ways of thinking that are required to practise a discipline are disjoint from, and sometimes even interfere with, those that make one good at understanding the roots of the methodology practised. This leads to inefficiency. Problems are addressed in a piecemeal way, and often from scratch, lessons are not accumulated nor shared, and the solutions arrived at can suffer. Not only may they be based on ill-thought-out assumptions, but they may not make the best use of the information that is available, given the assumptions made.

The author aims to reduce this kind of inefficiency by articulating, as explicitly as possible, the

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

methodological principles that we have available, by evaluating them and by improving on them where possible. This study is a start in that direction.

The second point of interest for actuaries is a warning that we find in the work; actuarial science is a practical science with its own evolved methods. These methods are local, bottom-up and skill-based; and they are, on the whole, well suited to the kinds of highly specific, highly variable problems arising in the highly uncontrolled environments that actuarial science confronts. We should be careful, then, about taking over methods or principles from sciences that have different aims, solve different kinds of problems, and work in different environments.

In particular, we should be wary of principles and methods imported from theoretical economics, which are, themselves, often borrowed from modelling strategies used in physics, where phenomena tend to be regular and repeatable, and predictions are, for the most part, confined to the well-controlled circumstances of a laboratory or a precisely engineered and well-shielded device, a laser for example. This is not to say that actuaries have nothing to learn from economics. In each case we need to think about exactly what results in economics mean, and how they can be combined with modelling strategies that are more closely attentive to the content and details of the problem under consideration.

From my point of view, as a philosopher of science, the author's work has additional interest. Until recently both the history and the philosophy of science have been *theory dominated*. Even when we study how real concrete systems are modelled and how real predictions are made, we cast the problems as ones of *how theory is applied*. Think, for instance, of the usual term, in my field, for sciences that deal with real things. They are not called 'practical sciences', but rather 'applied sciences'; that is sciences that *apply theory*. The author's study is part of a growing literature that looks at how science treats real concrete systems, remaining neutral at the start about whether the best or the usual methods are top-down, that is from abstract theory to concrete model, or not. I think that the author's study of actuarial science can make a significant contribution in this area.

The author stresses three features of actuarial modelling:

(1) *Skill-based*. This counters one standard interpretation of the hypothetico-deductive method, an interpretation which I call the 'vending machine' view. The theory is about a vending machine. You feed it input in certain prescribed forms for the desired output; it gurgitates a while; then it drops out the sought-for prediction. This is a characterisation of the positivist understanding of the hypothetico-deductive method. The central point of the method for positivists was the elimination of creativity in the use of theory. That was essential to the concept of *scientific objectivity* that positivism embraced. Scientific objectivity excluded expertise and judgement.

(2) *Causalism*. Actuarial models are not deduced from a small collection of abstract principles assumed to be universal, such as the principles of general equilibrium theory or the axioms of rational decision or utility theory. Instead, they assemble the causes thought likely to be operating in a given case, and try to calculate what the effects of their combined action will be in the given circumstances. The choice of which are relevant is based on judgements about the case in hand; so, too, is the prediction of how they will act all together. For actuarial science, there are no universal principles for telling you how different kinds of causes will act in a particular combination in which they occur. What you might have hoped for were principles like the rule of vector addition in classical mechanics, for which John Stuart Mill hoped to find analogues in economics, but nothing so helpful has been found.

(3) *Localism*. Much of the reasoning in actuarial modelling is context — and content — dependent. Whether a factor that is present will be causally relevant or not, and, if relevant, how it will act, can depend very much on the circumstances in which it is embedded. This is one of the chief reasons why expertise matters so much, as opposed to putting in the general principles and turning the crank to get what drops out.

Recent work in the philosophy of science, even within the theory-dominated school, acknowledges the author's first characteristic as a general feature of scientific modelling. The author talks much about skill-based modelling. In fact, we have come round to agreeing with him in the philosophy of science. Consider, for example — to take something very far away from what you do — the development of the standard BCS model for superconductivity. Before BCS, it was widely believed that superconductivity was a quantum phenomenon — the trick was to find the right characterisation

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

of superconducting materials to bring the superconducting phenomena under the quantum theory. You could not look at a superconducting material and figure out how to describe it in quantum mechanical terms. We had to figure out what the right model was, and to construct it. In the end, BCS modelled superconductors as materials in which the attractive forces between electrons (which were mediated by phonons) were stronger than the repulsive Coulomb forces between the electrons. This concept of what a superconducting material is works very well to bring superconducting materials under quantum theory, but before the BCS concept, there were a great number of other quantum descriptions proposed. The history of how BCS came up with their theory is that they gradually accumulated enough detailed local knowledge to do it better than the physicists before them. It is examples like this that has led philosophers of science to acknowledge that skill-based modelling is typical throughout the sciences.

Philosophers are more reluctant to cede the author's other two features as characteristic of scientific modelling in general. We can see the reason for this reluctance by considering the old distinction between the *context of discovery* and the *context of justification*.

It does not matter, philosophers have argued, where we get our models — Kekole, for instance, is supposed to have arrived at the correct structure of benzene molecules by falling asleep and dreaming, in front of the fire, of snakes with their tails in their mouths. What matters, we have been taught in philosophy, is our justification for accepting the models once they are dreamed up. Here is where universal principles come into their own. BCS proposed that getting the phonon-mediated forces between electrons to outweigh the Coulomb forces will cause resistanceless flow of electricity, that is, it will cause superconductivity. There is a causal claim. Should you believe that? Or, in economics, someone proposes that significant loss of skill during periods of unemployment will cause persistence of unemployment shocks, since entrepreneurs will not believe workers to be productive enough to make it profitable to open new jobs. Is that a plausible causal connection?

How do we answer questions like these? We all know that is what theory does for us when we have it; it shows us whether or not the putative cause really can have the effects that are claimed for it. Quantum theory allows us to deduce that resistanceless current will flow if the one kind of force outweighs the other; for the skill-loss and persistence of unemployment shocks, rational decisions theory does the job, but what works where we have no general principles?

Of course, ultimately it does not help to answer: "the local knowledge and experience of the practitioner". You are the practitioners. How do you decide which hypotheses, even of your own, to trust? In this kind of case we will probably need a very detailed methodology to help — so we are back where the author began his remarks — and one that differs radically from one practical science to another. Nevertheless, a methodology is what we need and, to a great extent, what we actually have. As the author points out, you teach your students how to price options. The methodologies have to become far more explicit, otherwise we are not in a good position to criticise and improve upon them. We need an account of our methods that is explicit enough to allow us to see where they will be reliable and where not, and to what extent, and how exactly we must hedge our bets when the degree of reliability is uncertain.

This is the really exciting part of the author's efforts to open up the study of the methodology of actuarial science. Actuarial science needs it in order to improve its own models; and, in particular, to avoid the mistake of simply passing over the context of justification to abstract economics, which cannot do the job. The philosophy of science, in general, needs this kind of work, because we have virtually no accounts available of how to justify concrete causal claims where they do not follow from abstract theory. So I applaud and encourage the author in his work, since it will be of great service to both of our professions.

Dr A. S. Macdonald, F.F.A.: I agree with much of what the author has to say: about the difference between the 'hard' mathematical models of physics and the 'soft' mathematical models of other applications; about the need for judgement within a context; and about the need to avoid confusing models and reality. There are major examples of just such errors.

However, I disagree with the author's conclusion, in ¶9.1, that "actuarial science has a distinctive method". Also, I do not think that it is free of the vices that the author sees in financial economics.

The basis of actuarial work is every bit as axiomatic as the basis of financial economics. The

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

example of the net premium valuation, in ¶8.3.2, is a good one. It rests on what I would just call assumptions, but the author calls axioms, such as: constant, deterministic interest; a mathematical model for mortality, and methods for estimation within that model; an artificial premium; no expenses; no future bonus; and so on. It leads to conclusions obtained by mathematical calculation (in other words, proofs) which have to be interpreted in the real world. Only long familiarity with the net premium valuation can possibly lead to the belief that it is closer to reality than, say, the Black-Scholes model. To adapt the author's terminology, we might call this the 'net premium belief danger'. This shows up particularly clearly in ¶8.3.2.2, where the author holds up the mathematical dodge of valuing at an artificially low interest rate as a positive feature, while in ¶7.6.2 he worries about the appearance of an artificial growth rate in the Black-Scholes valuation formula. Both are mathematical artefacts, and both derive from the formulation of the problem, using very strong mathematical assumptions. It is, as the author says, a problem if some users do not recognise this as a mathematical artefact, but that is not in any way a critique of the use of mathematics.

It is nice to think that actuaries have an innate caution in applying mathematical models that could be exported usefully. It might even have some truth, but an outsider might take some convincing. In comparing actuarial science and financial economics, the author is not comparing like with like. Applications of financial economics, in the real market, have typically been to short-term, individual contracts. In essence, even in the last 25 years, the experiments have been repeated many times over, and the limitations of too strong a belief in the models have been exposed. This is not the case with actuarial science, because it is dealing, typically, with long-term and collective problems. Here we have a sample of one. There is no empirical evidence, whatsoever, that actuarial science, in the United Kingdom, is capable of negotiating a low inflation environment with greater success. The great advantage of actuarial science, for actuaries, is its long time scale, and its ability to control inter-generational transfers buries past mistakes. High inflation is also very helpful in this respect. I am not saying that actuarial science will fall short in future, I am just saying that empirical evidence of its adequacy, on its own timescale, and freed of its own context, is lacking. We should bear that in mind when we are looking at the experience elsewhere.

The author overdoes the suggestion that economists are looking, in all cases, for laws of nature. That might have been true some time ago (for example, in studies of business cycles at the end of the last century, or in parts of equilibrium theory), but it is not universally true today. The fact that such a multitude of models can be put forward and explored, for example, for asset prices, argues rather for a good deal of empiricism and pragmatism that the author would reserve for things actuarial. Do not forget that actuaries also once sought laws of nature, of mortality, for example. The practitioners of financial economics are much less attached to analogies with physical laws than the author suggests.

Actuaries make their own systemic errors. If the author wishes to make a case for things distinctively actuarial, then he might, at least, include these in the list. They would include: pensions mis-selling; failure to control lapse rates; and reserving for guaranteed annuities. Since these all involve significant commercial, rather than mathematical, error, I find the author's faith in actuarial 'practitioner lore' unconvincing. It is arguable that the resilience of institutions advised by actuaries owes at least as much to context (especially their ability to make transfers between different generations and between different groups of stakeholders) as to any innate actuarial wisdom.

In trying to compare actuarial science with financial economics, the author has devoted great effort to abstracting financial economics from its context, but rather little effort to doing the same for actuarial science. If actuarial science, and its mathematical roots, were to be exposed to the same critique as financial economics, there would be no difference at all, and ¶6.3.2 is only the result of the author's ingrained familiarity with the context in which the former is applied, studied and written about.

In several places the author touches on the theoretical tone of much of the financial economics literature, for example in ¶7.5 and Section 7.6. This leads to his 'proof belief danger', namely that any exploration of the consequences of a mathematical model will mislead at least some readers into believing the truth of the consequences in the real world. Maybe this is a real risk, but if so, it arises from the fact that many of those exposed to financial mathematics lack the sophistication to switch

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

between the model world and the real world with sufficient clarity. That is, the danger, if any, lies, not in the message transmitted, but in the message received. I do not think that this is a major problem for our profession; anyone who can recognise for themselves the 'proof belief danger' in the net premium valuation method, for example, should be able to do so in respect of any of the models of financial economics.

So, how is the 'proof belief danger' to be avoided? Do you have to avoid the mathematics, or should you obtain a better understanding of the entire model? The former might support the Anglo-Saxon preference for generalists over specialists, seeming to stem from a belief that the more of mathematics you know, the more you will be seduced by it. It is as if the mind can hold only so much, and a bit more mathematics must push out something else, like caution or common sense. I do not think that a serious case can be made along these lines.

Having a better understanding of mathematics means learning to speak the language, and there is really no other way. In a large London bookshop I counted about 300 books on finance and financial economics. They define the language of this particular discourse. Financial mathematics cannot be uninvented, and it will not go away. The one actuarial book on the same shelves was a history of the last 150 years of the actuarial profession.

Is it reasonable to suppose that all future useful work, of the sort that the author encourages, will eschew all mathematics except that which actuaries currently command, or is it more likely that financial economists and others will continue to add hard-won experience to their own toolkits? I would emphasise that, right from the start, a major theme of financial economics has been the robustification of the models. That is not something that only actuaries see the need for. There can be little doubt about the source and direction of future progress and influence.

The author has made a useful and valiant effort to deconstruct financial economics and actuarial science. Perhaps, because he has an outsider's view, he has pursued the former more vigorously, although none of his criticisms would be unfamiliar to workers in that field. Perhaps, because he is an insider, he has not put actuarial science under the same microscope, and he sees differences that are more to do with context than with content.

Mr C. D. Daykin, C.B., F.I.A., Hon.F.F.A.: This paper forces us to examine the principles of our practice as actuaries and the principles of what we call actuarial science.

It was almost 10 years ago that I was made the Chairman of the Education Strategy Working Party, which started the task of turning our education syllabus into a form based on explicit learning objectives. The then recently appointed Chief Education Executive, Ken Gardner, forced us to go back to basics and to try to enunciate what we thought was the underlying philosophy of actuaries in order to build the objectives of the syllabus on that. With assistance from the other members of the working party, I drafted a statement of the actuarial philosophy and actuarial scientific method which, subsequently, was incorporated in the syllabus, so that we now have, for example, in Subject A, the specific objective that, on completion of the course, the trainee actuary will be able to demonstrate a knowledge of the actuarial philosophy and the actuarial scientific method. I am not sure that many trainee actuaries will have a deep knowledge of the actuarial philosophy and actuarial scientific method, since most actuaries, probably, do not have a very deep understanding of those things either.

We also have related aims for the later subjects, such as the aim of the life insurance course, which is to ensure that all newly-qualified actuaries have the ability to apply the actuarial scientific method and actuarial techniques necessary to enable long-term business to be operated on sound financial lines.

It is clear that, at the time, we were only making a first entrance into the area of actuarial philosophy. We had certainly not thought the issue through to the extent that the author obviously has, in trying to position actuarial philosophy and actuarial methodology in the spectrum of approaches in the philosophy of science. However, in April 1991, we published a report entitled 'The Initial Training of Actuaries', in which we identified seven fundamental axioms of the actuarial thought process, described there as a statement of 'actuarial philosophy'. These are:

(1) a study of past experience facilitates the making of reasonable future projections;
(2) elements of risk and uncertainty can, in most situations, be understood and managed;
(3) regard should always be had to the long-term consequences;

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

(4) the analysis should, as far as possible, explore the fundamental mechanisms and relationships at work, and should not be based on superficial appearances;
(5) mathematical modelling can be an appropriate strategy for handling the interactions of probability and investment return;
(6) a good actuarial model should not be unnecessarily complicated, bearing in mind that the objective is to provide practical results; and
(7) further experience should be fed back to aid the subsequent development of the model and the assumptions made.

We also formulated what we described as the 'actuarial scientific method', which others have described more recently as the actuarial control cycle. This is the process by which practical conclusions and advice are formulated through:
(1) identifying information requirements, and collecting what is needed;
(2) structuring, presenting and analysing information;
(3) formulating assumptions and constructing models to process information for various purposes; and
(4) monitoring and appraising emerging experience against the model and the assumptions, with a view to possible improvements in the model and identification of any additional need for information.

It is, perhaps, not surprising, since one of the author's sources for his study was the syllabus of the Faculty and the Institute of Actuaries' examinations, that there are some resonances of these points in this paper. In particular:
(1) the emphasis on practical problem solving rather than on the creation of a theoretical framework;
(2) the importance of data analysis and judgement, combined with the feedback loop;
(3) the causalist philosophy of trying, where possible, to understand the underlying relationships that we describe rather than just modelling the outcomes; and
(4) an emphasis on the understanding and management of risk.

The paper is helpful in elucidating differences between the actuarial approach and the philosophical paradigms of econometrics, on the one hand, and financial economics, on the other. From the mid-1980s onwards I participated in several international conferences on the solvency of insurance enterprises, which were designed to bring together the financial economists and actuaries who were doing research in this field. In the early meetings it was fascinating to find how little a meeting of minds there was between the two disciplines, between people who were trying to solve essentially similar problems. Much of the characterisation of the differences between actuaries and financial economists which is given in the paper rings true in this respect, since the actuaries were clearly approaching the modelling problem from an essentially pragmatic experiential basis, whereas the financial economists were deriving theoretical models based on a series of axioms or assumptions, and then trying to see how the models could be fitted to reality. It was interesting that, as time went on, with more contact between the two groups, there was some rubbing off of the experience in each case. The most productive point of contact seemed to be in the area of realistic market-value-based cash-flow modelling, which was the product of the stochastic simulation approaches of the actuaries, and also gradually seen to be necessary by the financial economists if they were to have any hope of making their models reflect the more complicated realities of the real world.

However, a basic difference which we identified between traditional actuarial thinking and the philosophical framework of financial economics is encapsulated in the difference between value and price. Actuaries largely seek to place values on cash flow streams, whereas financial economists believe that the market should do that for them. In their view, value is a subjective concept, whereas price is objective. However, a problem with this is that price, if it exists in a market where there is buying and selling, is, in fact, determined by the players on the margin who are willing to buy and sell at the marginal price. There remains the fundamental problem that different players hold different ideas of the intrinsic value of an investment, because they are holding the asset for different reasons, for different periods and in different fiscal and other contexts.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

This difference came to the fore when we were developing the actuarial syllabus for Subject E (investments), in applying the idea that actuaries should follow the actuarial scientific method, because the investment world does not fit the actuarial scientific method in the same way that the other subjects of life insurance, general insurance and pensions do, since market and price considerations predominate. However, we may have missed a trick in not explicitly applying the actuarial scientific method to the control and management of risk in investment portfolios. There have been some recent well-publicised examples of problems where risk management has not been carried out, and where the following through of the mathematics of a model has not led to a sensible outcome. I think that the actuarial approach, were it to be properly followed, would not place so much faith in the mathematics of the models themselves, albeit using those models as a guide, but would test the resilience of the models to different possible adverse outcomes, with due regard to the financial consequences of the downside risk.

The disturbances that have occurred in these financial markets seem to me to give actuaries an opportunity to capitalise on a rather more holistic approach to modelling and risk management, and to advance our cause to be seen as people with something to add in these areas. One of the consequences of the difference in philosophy between value and price is the fact that actuaries are often concerned with control systems and with managing risk in the long term. The examples which have been given about net premium valuations really point to that issue, since that is not a way of determining value, as such, but is a way of controlling a system. Actuaries have often been concerned with those types of control issues. Pension funding presents a similar type of problem. However, the market approach of the financial economists crystallises a view of the future into a snapshot view, through the use of market or fair value of assets and liabilities. This is one of the reasons why accountants find themselves in a very uncomfortable position, trying to rationalise how to square up the fair value of assets and fair value of liabilities approach to the balance sheet with concern about the consequences of financial volatility on the earnings stream of companies.

Recent developments in financial economics have included the development of so-called 'value at risk' models. This is another subject which has been brought up as something that we, in the actuarial profession, ought to study. These models emerged primarily in the banking sector, but, as a well-known professor of financial economics remarked at a conference recently, 'value at risk' is something which actuaries have been evaluating for years in the practical application of risk theory concepts and the use of what actuaries used to describe as the probability of ruin.

Actuaries and financial economists are moving on to a derivative of these earlier ideas, something which is called the 'expected policyholder deficit', or the 'expected stakeholder deficit', in a more generalised concept. This incorporates both the probability of having financial loss and also the quantum of that loss.

One of the problems that we have in talking to financial economists is our understanding of what is meant by risk, since much of early financial economics worked from a definition of risk which was based on volatility of market values. Many economists still do not appear to recognise that there is such a thing as matching of assets and liabilities.

As actuaries, we have an important contribution to make to thinking in this area. Differences emerged at a political level between the International Accounting Standards Committee and the International Forum of Actuarial Associations in the discussion of the proposed new accounting standard for pension liabilities in company accounts — now E19(revised) — and the same differences of philosophy will be at the fore in the discussions on the insurance accounting standard which is currently under consideration. Accountants start from the presumption that fair value must be used both for assets and for liabilities. Whilst that is not too difficult a concept to work with for the assets, it does present a challenge on the liabilities side. The accountants, driven by financial economics, would like there to be a market assessable value of the liabilities which could be used in this context. In their view, this should be independent of the outcome, and should be essentially independent of the assets which lie behind the liabilities, so that the liabilities would be seen as having a market assessable value in their own right. Actuaries, on the other hand, are inclined to argue that the value of the assets and the value of the liabilities must be inextricably linked, and that you can only determine the liability value by looking at what the assets are behind those liabilities. The value of the liabilities which are backed by matching assets is a very different thing from the value of the same liabilities when there are no such matching assets.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Thus, the paper raises a very wide spectrum of issues which we need to grapple with, as a profession, if we are to maintain the distinctiveness of our contribution to society. Actuaries are not accustomed to thinking at the philosophical level, perhaps because of our essentially practical orientation, and perhaps because we do not have as deep a literature as some other disciplines, as has already been mentioned. We also do not have as many people working in actuarial science research in the universities as would be the case in a discipline like financial economics. However, we need to think more deeply about the framework in which we operate.

We are about to introduce a new examination on actuarial modelling into the syllabus. A similar topic is being introduced into the Society of Actuaries syllabus, where it is going to be handled through a practical intensive seminar rather than as an examination. As all the major actuarial associations are moving in the direction of seeing the need for modelling to be considered as a discipline in its own right, which underpins much of the work of actuaries, we need to make sure that our actuaries of the future not only understand the mechanics and the techniques of such models, but also have an appreciation of the philosophical paradigm in which actuaries are operating, and the way in which that relates to the way in which other people build and use models.

There is going to be a working party looking at the parallel exercise of the Society of Actuaries' discussion draft on General Principles of Actuarial Science, which has some inter-relationships with this paper. I hope that we can make a useful contribution on that.

Mr I. J. Kenna, A.I.A.: In ¶3.2.1 the author admits that he has omitted the Marxist schools. In ¶6.4.2 he states that "In politics, axiomatic systems, such as that of Marx, have suffered widespread reversals". In fairness to Marx, he claimed only to make a concrete analysis of a concrete situation. That is why he said that he was not a Marxist. Following the death of Mao Zedong in 1976, Marxists in China split into two schools of thought: one, led by Mao's widow, Chiang Ching, and three associates, advocated acting according to the principles laid down by Mao; and the other, led by Deng Xiaoping, advocated seeking truth from facts. Acting according to the principles laid down over nearly 50 years to deal with entirely different situations was a non-starter. There may be eternal truths, but not all truths are eternal. When the Chinese Marxists started seeking truth from facts, opinions differed as to the theoretical consequences of the facts. Eventually, the socialist market economy evolved in China. The market is allowed to operate freely within a framework of law. A place has emerged for sciences, such as actuarial science and sociology.

In Britain, for many years, we acted according to the principles laid down by John Maynard Keynes. These principles were devised to prevent a repetition of a 1930s-type situation. By 1980, Keynesianism looked like a good recipe for uncontrolled inflation. There are now a number of other axiomatic systems, including neo-classical economics.

Actuarial science is a bottom-up science. As is stated in ¶6.2.5.1, "Actuarial science has no counterpart of the search for general laws evident within neo-classical economics". It is a pity that the author goes on to imply that actuaries should be mere technicians in the service of the axiomatic system of neo-classical economics. Most actuaries would vigorously deny being Marxists. However, actuaries do seek truth from facts and not facts from received 'truth'. Economists have more to learn from actuaries than actuaries have to learn from economists.

Dr P. P. Huber, F.I.A.: I agree with the author that the philosophy of science provides valuable insights for actuaries. In particular it illustrates the fallibility of our knowledge, and hence justifies the need for pragmatism and judgement when applying this knowledge.

The philosophy of science is also useful, because it studies the development of knowledge and the criteria for theory selection. How should actuaries choose between alternative assumptions or models? This question is important, because we need to be able to defend actuarial judgement using universal standards. Actuaries cannot afford to divorce themselves from the rest of the scientific community and invent their own standards for knowledge.

This is an area where the paper does not go into much detail. Model selection is assumed to be largely judgemental and based on professional experience. However, professional judgement is a weak criterion for model selection unless it is accompanied by a structured theoretical approach. Models that are assumed to apply only locally are rarely incorrect, by definition, and, as a result, they are

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

generally not particularly useful because of their limited domain of application. Moreover, judgements based purely on past empirical evidence, or 'fact-based' models, are especially problematic because of Hume's problem of induction and the Duhem-Quine thesis.

Hence, having a theoretical framework is essential, even though it might be based on 'unrealistic' simplifying assumptions. Theoretical frameworks define concepts that are necessary for communication, they enable knowledge to develop over time, and they are essential for testing beliefs.

For these reasons actuaries should keep abreast of theoretical developments, including developments in financial economics, in an active way. We should be critical of these theories, but we should not dismiss them lightly in favour of 'local' beliefs that are not subject to the same rigorous testing procedures. Hence, further consideration should be given to the subject of actuarial judgement and model selection.

Mr D. R. Linnell, F.I.A.: I was, until recently, an unashamed professional-judgement man when it came to the funding of pensions schemes. It was a control process which fed back, and one hoped that one never got too much surplus or too much deficit. The challenge that the accountants have now given us, via the financial economists, is to tie our methods in to a process which starts from the axiom that there is a price there set by the market, which determines the value of the assets and, by extension, of the liabilities. So far, what I have seen is an oversimplification, and I am unhappy with the way in which the models are currently appearing and developing. The challenge that we, as a profession, have is to take those models and make them work in the real world. The paper struck a chord, and shows me some of the reasons for my concerns. It sets out some of the theoretical background which we, as actuaries, are going to have to pick up in order to keep up with the other professions.

Professor P. P. Boyle, F.I.A. (in a written contribution that was read to the meeting): Although the author concedes the usefulness of the recent advances in financial economics, he is somewhat critical of their adoption as part of the actuarial tool kit. Most observers agree that the significant advances in our understanding of investment models have taken place in the last 30 years. It seems necessary for the profession to become aware of these developments and to use them where appropriate. Fortunately, there is strong evidence that this is happening in many jurisdictions.

The author contrasts the actuarial approach to applied modelling with the methodology of what he terms mathematical economics. Here I must take strong issue with his terminology. He confuses mathematical economics, which, by its nature, involves the use of mathematics to solve problems in economics, with financial economics, which studies problems related to finance from an economic perspective. This is a particularly unfortunate misuse of terminology in the present paper, since the author is often critical of what he terms the mathematisation of economics. It is the case that the models of financial economics use mathematics, but that is not their main focus.

The author contrasts the applied models used by actuaries with the economic models which he suggests are involved in general theories and the search for universal laws. The investment models used in financial economics are often derived from assumptions, but they are tested and retested against the data in a scientific manner, and modified where appropriate. It seems more helpful to regard this as a useful body of knowledge that actuaries can draw on when making investment projections. It is necessary to understand these models in order to make a critical evaluation of whether or not they are appropriate. The author emphasises the critical evaluation aspect, but surely one must understand the models before one can evaluate them critically. This is why it is essential to include modern financial economics in the basic education of actuaries.

The author warns us against the dangers of using mathematics, and stresses the importance of intuition. In modelling uncertainty in a scientific manner we have no option but to use probability and statistics. It is true that some of the advanced models in finance use technical mathematics. This does not mean that we abandon intuition; quite the reverse. Intuition is now even more important than ever before. The author is very sceptical of proofs. Indeed, he seems to be suggesting that we attach warnings for actuaries stating: "This proof could be dangerous to your health". In the case of important models, it is only through a careful analysis of the proof that we can see which assumptions are critical, and which ones are less critical, for a particular model. If we want actuaries to be critical users of investment models, then they must understand them at a deep level, and such understanding

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

is best obtained by a detailed, as well as an intuitive, appreciation of the proof. The use of mathematics does not mean that we abandon intuition; indeed, the underlying mathematics serves to hone our intuition.

Nowhere is this point better illustrated than in the case of the Black-Scholes model. The author's description of this model, in Section 7, draws heavily from the original Black & Scholes (1973) paper. Our understanding of this model and the key intuitions behind the approach have been enriched by subsequent research since the publication of the Black & Scholes paper. The Black-Scholes methodology is not restricted to the simple case discussed by Black & Scholes. For example, the two assumptions mentioned by the author in ¶7.2 are not essential in any way.

The Black-Scholes methodology is based on two key insights. The first key principle used is the no arbitrage principle. This means that, in well functioning markets, two portfolios with the same payouts must trade at the same market price. The second insight is that, if you can replicate a contingent claim or a derivative using existing traded securities, then you can value this derivative. From these two ideas one can derive the option pricing formula. The current option price is expressed in terms of a portfolio of the traded securities.

The Black-Scholes model has been modified and extended in a variety of ways, since its introduction in 1973, to accommodate a variety of input assumptions. The survey paper of Smith (1976) is 22 years old, and is now quite out of date. For example, the option model has now been extended to handle stochastic volatility, stochastic interest rates, non-lognormal diffusions, the inclusion of transaction costs and the inclusion of default risk. The author notes that the assumption of constant volatility is not valid, and that the model is often modified, in practice, to handle this. This is part of the normal scientific evolution to modify the model in the light of practical experience, and I agree with the author that this calls for skill and judgement. However, I would argue that, in order to see how best to modify the model, it is necessary to understand it at a deeper level. One needs to know which assumptions are essential and which can be modified. Without understanding the deeper workings of the model, it is not clear how one can do this.

The recent financial difficulties of the large United States hedge fund, Long Term Capital Management (LTCM), nicely illustrate the importance of judgement in the use of models. This hedge fund, advised by two Nobel laureates in finance, lost several billions of dollars by taking highly leveraged positions. The most basic lesson in finance is that there is a relationship between risk and return, and that the large returns earned by the fund before it ran into troubles were not risk free. The name 'hedge fund' is a misnomer. This fund was taking large risky positions and was notoriously secretive about releasing information on its investment strategies. The ultimate risk takers were not properly informed about the risk that they were taking (as happened in the case of Lloyd's some years ago), but, in this case, the risk takers were very sophisticated. The real problem was not that sophisticated investors lost money, rather that the complex structure of the firm's investments triggered systemic risk in the financial system.

However, the demise of this hedge fund does not invalidate the models of financial economics any more than an aeroplane crash invalidates the principles of aerodynamics. It reminds us of the need to use good business sense and judgement in investment decisions, and on this I am sure that the author concurs.

The author refers to Thomas Kuhn's influential book, *The Structure of Scientific Revolutions* (Kuhn, 1970). Kuhn argues that, during periods of transition, an established scientific paradigm becomes challenged by a new paradigm that does a better job of explaining the world. As the author notes, the believers in the old paradigm hold on tenaciously to the theories which they support. Eventually the new paradigm becomes accepted by the scientific community. Could it be that we are witnessing this transition taking place in the actuarial profession with regard to the adoption of financial economics?

Mr J. Goford, F.I.A. (closing the discussion): On the philosophical arguments, Mr Kenna said that economists do have a lot to learn, and especially from actuaries. We are proud of that. Dr Huber said that model selection is more than judgemental, and that actuaries must keep abreast of developments in financial economics. Professor Cartwright advised us to be wary of principles derived from

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

mathematical economics, and, in a sterling defence, Dr Macdonald acknowledged that there were some weaknesses in financial economics — indeed, it is easier to see the weaknesses because of the more short-term nature of their problems. The long-term nature of problems in actuarial science makes the weaknesses more difficult to show. He maintained that financial economics is not all laws, that there is a good deal of pragmatism, and that the 'proof belief danger', while it may be valid, is not necessarily solved just by avoiding the mathematics.

Mr Daykin gave us an illustration of the interaction of actuaries and economists over his meetings on the solvency of life offices, and advised us that the crunch was in the distinction between value and price. I believe that when actuaries derive their values they do pay as much attention as they can to transaction prices, where they are available.

On models and their characteristics, it was averred that they can go wrong, and LTCM's failure has been given as an example. We were advised not to confuse the output of models and reality. Dr Macdonald said that he does not agree that actuarial science has a distinctive method. He gave the example of the use of the net premium valuation, where his arguments were that it is not dissimilar to the way in which Black-Scholes worked — with some untenable assumptions. This argument does not support his view that actuarial science does not have a distinctive method.

Mr Sharp [an amplified version of what he said is printed as a written contribution] encouraged us to incorporate some actuarial methodology which could cope better with inflation and help with the distribution of equity between stakeholders more fairly. As to the future, Mr Daykin mentioned value at risk models, which need some further refinement. There were also suggestions that we take time to think at a more philosophical level. I was surprised that that was equated with getting more input from the universities. Whilst that is all to the good, I also think that there must be time taken by practitioners to do some additional thinking. The author is an example of the sort of output that can happen when a serious practitioner starts to think.

This paper helps us better to know ourselves and how we think. It allows us to put our way of thinking into a global context. It enables us to understand how our way of thinking fits with others, and to be more accommodating to a broad church of methodologies. It is outward looking and accepting of other methodologies. I would contrast that with the paper by Bell *et al.* (1998), which is in danger of attaching an inward-looking, fixed and over-defined set of rules to the profession. It is because of this danger that I ask that, whenever Bell *et al.* is aired, this paper is put alongside it to keep the outward-looking characteristics of the profession alive. Indeed, I would characterise the paper, itself, as demonstrating the actuarial methodology. It does not purport to disclose immutable truths about the ways that actuaries work, but casts light, after careful observation and analysis, and so builds confidence in the characterisation of the actuarial methodology.

Above all, a greater understanding of how we do what we do helps our communication, which, for many actuaries, could do with some improvement. It helps us to understand, for example, that actuarial methodology is specific to the situation and to the problem. This helps us to explain how we need to understand the situation before we know what data to seek and before we can decide on the methodology that we might use to tackle the problem. We come neither with pre-packaged solutions nor with immutable truths. Our need to understand the situation first does, therefore, make it more difficult for us to sell our services than for those with pre-packaged solutions that may give accurate answers to questions, but to questions which may not be relevant to the problem.

An open issue is that of 'actuarial judgement'. It is either arrogant nonsense, and we should exorcise it, or it is a real process of logical development and intuitive thinking from experience. I would like to see a further exposé of this phenomenon that we hold so dear, to give it more credibility. The author mentions that we may take in techniques from other disciplines. He mentions mathematisation methods, chaos theory and neural networks. I would like to see some overviews of these techniques, and how they fit and contrast with actuarial methodology, with their likely applications and limitations — in rather the same way as we are regarding financial economics in the new syllabus.

By way of example, a small spin-off from this paper is that it gives actuaries a greater understanding of the level at which to use the models at our disposal to provide answers to situation-specific questions. Examples of those levels are:

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

— Do we use an existing model in a situation which is a repeat of one where it has been well tried and tested?
— Can we apply an existing model with some of the parameters varied?
— Can we apply an existing model with some of the methodology varied?
— Can we use the structure of an existing model with some new modules?
— Do we need to construct a new model, cannibalised from other models?
— Do we need to start again?

This paper helps us to view the world as a 'both/and' world. It is both a world in which the actuarial methodology is valuable and powerful enough to deserve a profession to itself, and it is a world in which the profession can work with other disciplines in a complementary and constructive manner. I am heartened to see that the elements of the actuarial control cycle are firmly within the actuarial methodology. They are already included in the Australian syllabus, and will soon be included in ours.

The Senior Vice-President (Mr M. Arnold, F.I.A.): I would like to relay the comments that I have received from the President. He believes that this is a valuable paper that the author has presented. He believes that, in the past, we have tended to live in a small actuarial world of our own, and have not been as aware as we might have been of the wider world around us. The paper helps us to place our actuarial approach to life in a wider context.

The President continues that there is, currently, a strong need to describe what actuarial work is, and what we mean by actuarial principles. As the author mentions in the paper, work is currently ongoing in this area, both here and in North America, and the President agrees with the author that it is important that we continue this work in order to clarify how we can broaden the scope of the actuarial profession and work with those in other disciplines. He concludes with a very strong word of welcome for the paper, and hopes that the discussion it generates will help us to see more clearly exactly how actuaries can 'make financial sense of the future'.

It remains for me to express my own thanks, and I am sure the thanks of all of us, to the author for his paper.

Mr J. M. Pemberton, F.I.A. (replying): I am delighted by Professor Cartwright's encouragement in the direction of the work proposed by the paper and by the suggestion that actuarial science has, potentially, a valuable role to play in supporting current philosophical work in exploring the context of model application.

I am also pleased with the agreement of Dr Macdonald about the concern of unthinking mathematics and his positive comments generally. I agree that actuarial science is not free of a number of vices. I do not agree that actuarial science is as axiomatic as mathematical economics. Dr Macdonald cites the net premium valuation (NPV) as a case. There are important differences between the NPV approach of actuaries and the axiomatic approaches of the mathematised schools. Particularly, the NPV model is highly contextualised. We do need to understand the use of the model in its contextual role in order to understand the model. It is essentially a simple model, but the simplifications undergo a sensitivity check in the process. What is difficult is to undertake a sensitivity check of assumptions such as whether stock prices follow a diffusion process, and that is one of my concerns with some mathematical economic models. There are some important principles which are different between the bottom up and the top down approaches. Simple, approximate models can be scientific, if they are correctly used.

One of my concerns is the role of proof within mathematical economics. This is a separate issue from the use of the formulae which derive from these proofs. I am happy, provided it is correctly used, for the Black-Scholes formula to be used as a rule-of-thumb approximation. What I think is as yet unshown is the value of the proof.

We could not abstract actuarial science from its context in the same way as we could abstract the principles of financial economics, because actuarial science seems to be fundamentally a context-dependent subject. The problems of actuarial science are real, but they are different from those of neo-classical economics.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

I am glad that Dr Macdonald accepted the proof belief danger. I do not believe that it lies just in the eyes of students; I believe that it is inherent in a number of the educational texts — and I gave some examples of this within Section 7.6. Mathematics is not the only way forward, an alternative is to use a more empirically-based approach. I suggest a combination of the two is what we should explore now.

Mr Sharp expressed concern [see written contribution] about inflation, and I agree that any financial method must make appropriate treatment of inflation.

I agree with Mr Daykin that VAR is essentially an old actuarial approach. This is an important area in which actuarial science and financial economics could work together to improve our joint understanding better. Matching of assets and liabilities is an area in which actuaries could make a major contribution. We can tie the use of market values together with actuarial values, provided that we make appropriate adjustments to our market values for elements like taxation and risk. We should provide an explicit consistency between values and prices.

I agree with Dr Huber for the need for the actuarial profession to keep abreast of the available frameworks in any of the other financial areas, particularly financial and mathematical economics, and also that there is a need to further consider the role of actuarial judgement and model selection. Considering those topics is precisely the aim of the kinds of case studies which I want to do, looking at what Professor Cartwright called the context of model application, which is now such an important area within the wider methodology of science.

Professor Boyle wrote about the difference between mathematical economics and financial economics. I have researched, carefully, the use of these terms; there are actually very few dictionary definitions or even common usage definitions, and that is why I have been very careful to define my terms. My concern is principally with axiomitisation, and therefore I define and use the term mathematical economics carefully and in a way which I believe is valid. Models are modified heavily with use, to fit in with the available empirical data. We can learn much in this area from practitioners.

In the case of Black-Scholes, Professor Boyle notes that we do not need any of the assumptions, provided that we replace them with other credible assumptions. The no arbitrage principle is certainly a valuable thing to learn about, but there is no need to learn about it explicitly within the context of a proof of Black-Scholes. He also suggested that actuarial science might be giving way to a new paradigm, but, unfortunately, this 'new' paradigm, mathematical or financial economics, is principally based on positivism, and it was positivism which collapsed in the 1960s. In Kuhnian terms, mathematical economics is now the old paradigm. Recent developments point to its ending and its replacement with a new, more empirically-based paradigm within financial modelling, which is more consistent with actuarial approaches.

WRITTEN CONTRIBUTIONS

Mr C. G. Lewin, F.I.A.: I believe that the paper helps establish the place of actuarial science within the whole range of possible approaches to questions involving risk and finance. It indicates the kinds of situation where the actuarial approach is likely to be most useful, that is where there is a degree of stability which can be modelled from past experience with a reasonably high degree of confidence, allowing for trends as necessary. The traditional actuarial approach is likely to be less successful in situations where the degree of volatility is so great that long-term relationships are of little value as a guide. Actuaries may, however, be able to make a useful contribution in such situations, even though different techniques may be required.

I was pleased to see the complimentary comment, in ¶8.3.7, about RAMP (risk analysis and management for projects), which was developed by a joint committee (of which I was chairman) between the actuarial and civil engineering professions. RAMP is firmly rooted in our traditional actuarial approach, in that it seeks to establish, where practicable, a quantitative measure of the risks involved in a project, based on past experience in similar projects. However, there are many risks, in practice, where such past experience is not available, but this does not mean that the RAMP process then breaks down. On the contrary, a great deal can be learned by assigning hypothetical values to the risk and carrying out sensitivity testing. For example, for the purpose of risk mitigation, it may not be necessary to know the precise value of the probability of a risk occurring.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Thus, we should not attempt to place boundaries round our core actuarial methodology, but should rather use it as a base from which to explore other relevant issues using different techniques.

Mr C. D. Sharp, F.I.A. (who spoke at the meeting, and who subsequently submitted this amplified contribution as a replacement for what he said): To my mind, the author has been bemused by mathematics into ignoring the obvious. As I read the paper, I was forcibly reminded of the story of 'The Emperor's New Clothes', in which the small boy blurted out the awkward fact that the Emperor was naked, something that his courtiers had found it politic to ignore. I found it quite unreal for a paper such as this to contain only one brief reference to inflation, the factor which, in the longest term, makes a nonsense of all our nice forecasts, and which has, in my opinion, caused us to neglect that responsibility to the various stakeholders who so many of our Presidents have said it was our duty to respect.

Surely, any unbiased observer would consider it to be unreal for us to call our methodology scientific when we persistently ignore the obvious effects of inflation and treat our unit of measurement, the pound, as a fixed and stable quantity. Unless, and until, we modify our methodology to take account of this vital factor, I hold that we cannot justifiably claim our methodology is sufficiently soundly based for it to be regarded as scientific. In this paper's 62 pages of complex argument and discussion there seems to be, in ¶4.5.4.1, only one slight, and then indirect, reference to the effect of inflation on the pound, our basic unit of measurement, while, according to my reading of the figures supplied by the Bank of England, during my professional lifetime this has diminished by a factor of the order of 27.

Two of the main functions of any currency are: to facilitate the exchange of goods and services at a point of time; and to provide a means of storing purchasing power over periods of time. It is the latter aspect which is mostly relevant for the long-term contracts involved in our life assurance and pension work, so that my second critical question is whether, by ignoring inflation, our methodology has not resulted in unfair decisions in respect of those relatively powerless stakeholders for whom, as a profession, we purport to take responsibility?

This particular weakness in our technique has concerned me for a long time, and, as far back as about 1952, I suggested, in this Hall, a technique by which life policies could be linked with equities to provide some degree of protection. At the time the suggestion was regarded as too heretical by the actuary of the company that I was then with, and it was not pursued, leaving it to others to do so, with the results that we now know. The recent break in the stock market highlights the fact that this technique has its weaknesses, and I suggest that the time has come for us to take off the blinkers and to examine our methodology to see whether, and to what extent, it is practicable to make direct allowance for the inflation that so deeply affects most of the longer-term contracts for which we regard ourselves as responsible. I suggest that a judicious blend of investment in Government index-linked bonds, with a suitable proportion in selected equities and/or property, could well establish adequate backing for contracts, providing, directly, some protection against a specified limited degree of inflation. If this were carried through to all our financial contracts, it would, I suggest, go far to justify our claim that our methodology has a scientific basis.

It may be argued that current indications are that inflation may be held down to something under 3% p.a., but there is no guarantee that this will be so, and, even at that level, the face level of typical contacts is likely to be diminished by something like 75% over, say, 50 years. In my opinion, this results in a degree of unfairness to some of the stakeholders affected by the assumptions in our present methodology, which I do not believe that the profession ought to tolerate any longer.

From my own experience, I can point to two areas where our present methodology seems to have resulted in unfair treatment. When, at the beginning of 1970, I retired, it seemed sensible to make paid up a whole-life non-profit policy that I then held. Today the purchasing power of that reduced sum assured, again according to those Bank of England statistics, has dropped to a little over 10% of the original face value. Is it not true that, by ignoring the obvious effect of inflation, our methodology resulted in the transfer of at least part of that loss to the with-profits policyholders and/or the shareholders? If this is true, can it really be regarded as fair? To make use of what I understand today is regarded as a somewhat archaic phrase, can we justifiably claim that such results of our actions are 'in the public interest'?

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

There is another example of a similar kind — the pensions that I was entitled to in 1970 suffered heavily during the inflationary years of the 1970s and the 1980s, and although, subsequently, they have partially been made up, is it not true that the effective reduction in the purchasing power of those pensions in the early years contributed, because of our faulty methodology, towards the handsome surpluses which the scheme actuaries used to justify recommending contribution holidays for the employers. If, instead of being mesmerised by the undoubted sophistication of our mathematical techniques, we stand back and look at the reality highlighted by the real facts, can we justly claim that our work is scientific, so that we can justifiably claim to have been fair to those for whom succeeding Presidents have acknowledged that we have a special responsibility?

I now direct attention to another aspect of our methodology, which, I suggest, needs to be reconsidered. This is the most appropriate form of pension fund investments. By now, I believe that it is widely recognised, even in our own profession, that pensions are a socio-economic problem rather than the purely monetary one that we have found it expedient to assume. In my opinion, this is becoming dangerous, because the weight of those pension entitlements, expressed as demands on our future national economy, is almost certainly going to be inflationary in its effect. As recent debates on pensions and the ageing population have indicated, it is becoming more and more obvious that, unless our monetary funding results in real capital investment, it is highly questionable whether these will be sufficiently effective to enable the diminishing band of future workers to produce the goods and services needed to satisfy what they regard then as their legitimate claims, and, in addition, the corresponding needs for food, shelter, and, above all, care that our ageing population will claim via their pensions and their savings.

It must be remembered that, if those pensions and savings are still legally expressed in notional non-indexed pounds (and, apart from the civil and local public services, I believe this to be generally the case), then, by political pressure, they, the workers, are likely to push for a sufficient degree of inflation to balance the relative demands.

I therefore suggest that it is time for the profession, as a whole, to take a pragmatic approach to pension fund investment, and that, with economists, we should jointly explore how, and to what extent, it is practicable for at least some part of the massive funds already available to be re-directed into real investments, which should help to spread the burden. To me, one obvious example is that of safe and well-run sheltered housing, such I have seen in the U.S.A., and to some extent, in Australia. If even 1% of the larger funds were devoted to this, it should mean that many of the elderly who, at present, are reluctant to sell their existing all too large houses, could be encouraged to do so, which would immediately solve, at least in part, the financial problem of their ever increasing need for medical and other care, and would also make those houses available for younger, larger families.

I believe that, properly handled, this paper and this discussion could have started a debate which is important for the future of the whole profession, but for this to be effective, it needs active encouragement by the Councils who, in an area such as this, need to be proactive.

Professor A. D. Wilkie, C.B.E., F.F.A., F.I.A.: I am sufficiently old to be an unreconstructed positivist, and I take the view that, unless there are reasonably objective tests for determining the truth or falsehood of a scientific hypothesis, then there is not much point in arguing about whether it is true or false. Science is about truth, and its hypotheses can be tested. Philosophy is not science. Its hypotheses cannot be tested.

I do not, however, reject all philosophy as worthless. Like literary criticism (or art or music criticism), philosophy may provide an interesting viewpoint on the world (or the book, piece of music or painting) that helps one to see things in a new light. There is no true or false literary criticism, although certain ideas may be fashionable or unfashionable among critics, and these may be helpful, or unhelpful, to readers of books.

However, I do not find the philosophy of science, as described by the author, at all helpful. Nor did I find the books he refers to as helpful, although I have read some of them. Hausman (1992), for example, contains the sentence (p290): "The intuition that explanations reduce contingency is explicated as the claim that explanations show that descriptions of the phenomenon to be explained are *entailed* by statements of the relevant laws and initial conditions". Few of the terms introduced have been defined previously. I find this incomprehensible, rather than enlightening.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

Cartwright's book, *How the Laws of Physics Lie*, seems to me to set up straw men, in the form of physicists who believe Newton's laws of motion, for example, to be absolute truths, and then she suggests that such laws are better interpreted as one of several possible models of the real world, albeit a very useful one. Most actuaries, and I suspect most physicists, would have agreed with her without the apparent attack on physics.

Actuaries occasionally refer to 'laws' such as Gompertz' or Makeham's law, but we are well aware that these are not absolute laws, only formulae that are useful for graduating mortality data, and, in recent years, we have referred to them as Gompertz' formula and Makeham's formula, and, indeed, we have gone on to generalise them in the GM(r,s) series of formulae.

The author seems to suggest, in ¶2.2.3, that the statement that all ravens are black cannot be shown to be true. This seems to me to be an ornithological question, not a philosophical one, and it is one that is patently verifiable according to ordinary standards of scientific proof. Ravens (*Corvus corax*) have been known for centuries (to Noah according to *Genesis*, 8,7 and to Elijah according to *I Kings*, 17, 4-6; in both cases the Greek Septuagint uses the word κοραξ, so there should be no doubt about the identity of the bird). They are widespread, although not frequent, throughout Europe, northern Asia and northern America. Interesting questions that might be asked about their colour are: "are there albino or piebald ravens, as there are white blackbirds?" or "are there colour varieties of ravens, resembling the carrion crow (*Corvus corone corone*) and the hooded crow (*Corvus corone cornix*), which are now thought to be two varieties of the same species, which are found in different areas, and which can interbreed, but seldom do?" The answer to both these questions is no, ordinary ravens are everywhere only black, so far as has been observed for the last three thousand years. However, there are closely related species to which the name raven has also been given. In north Africa the brown-necked raven (*Corvus umbrinus* or *corvus ruficollis*) is very like an orindary raven, but it has a brown neck, and further south in Africa the white-necked raven (*corvus albicollis*) has a white nape. In the southwest U.S.A. and Mexico the white-necked raven (*corvus cryptoleucus*) is found which is externally black, but has the bases of the feathers white, especially on the neck. And there are other 'ravens' elsewhere.

To reverse the colour we can ask: "are there non-white swans?" The answer to this question used to be 'no', since all five species of northern swan are white, but the discovery of Australian black swans (*Chenopis atrata*) and the South American black-necked swan (*Cygnus melanchoriphus*) showed that this question now requires an affirmative answer. In fact, Australian black swans have white feathers at the trailing edges of their wings, so they are not all black.

It can be seen that the colour of any non-bird is irrelevant to all these questions, and it is only the colouring of related species, not even variegated species like peacocks or pigeons, that helps the ornithologist.

[I am indebted to Andrew Wilson for assistance on these ornithological matters.]

If the philosopher really means us to consider the general statement, 'all X are Y', then the existence or non-existence of any non-Y objects may be helpful. If nothing is non-Y then clearly no X are non-Y. Thus, one can confidently say, to change species, that no parrots are 'grue' (green up to 31 December 1999, blue thereafter), because no natural species or object has any awareness of dates such as 1 January 2000, which is a man-made concept, peculiar to the Christian world (although now widely used). It is possible that some computer programs (which do know the date) may display elements on their screens that are 'grue', but neither ravens, parrots nor grass are.

The point of this lengthy ornithological diversion is that it exemplifies what, to my mind, is actual scientific method, which is based, in the first place, on observation. We do not need to enquire closely (as some philosophers seem to) as to what we mean by an observation. The suggestion that we do not really *see* a raven, we only experience black patches of particular shapes on our retinas, seems to me a trivial quibble. The author seems to accept this quibble in ¶2.2.9.3, but it seems to me that anyone who does not accept ordinary observation is so unscientific that his views on science are of no value whatever.

Observation of birds and the rest of the natural world, over long periods and in many regions, allows us to make reasonable generalisations about the characteristics of (*inter alia*) birds, their colours (uniform or variegated), how these are inherited, and how they vary. Sometimes additional

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

observations are made (e.g. black swans), sometimes additional information is available, such as the DNA analysis that has allowed the species of the plant kingdom to be reclassified on a new basis. I cannot see that philosophy is of any assistance to science of this kind.

Observation is hardly mentioned in this paper, and is briefly dismissed. Experiment does not seem to feature at all. Many philosophers seem to me to adopt an armchair, or introspective, mode of thinking, and not to base their ideas on experience. Mathematics and logic (which can be claimed as a branch of mathematics) also fall into this introspective category, and are therefore not science, though enormously useful to scientific method. '2+2=4', regardless of what is being counted, or of whether anything exists to count. Likewise, the Black-Scholes formula inevitably follows mathematically from the assumptions made, whether or not there are any options traded in any market, or even any shares on which to base options. However, if observed option prices do not conform with the formula with the assumption of constant volatility, then we must question the assumptions, not the mathematics.

The author has a good point in that much (perhaps all) actuarial work is based on models. Like all mathematical models, these models are neither true nor false in themselves, but they may represent reality well or badly (and whether any one model represents particular features of reality well or badly is a testable scientific statement), and they may be more or less useful than other models for the purpose in hand. These are not necessarily connected characteristics; a model may be a poor representation of reality, but, nevertheless, may be quite useful for the right purposes. Also, alternative models of the same features may help understanding. At one time there were alternative views about the nature of light: corpuscles or waves; in due course, a more complicated model explained both aspects better.

Models may need to be complicated to be useful. Weather forecasting requires extremely heavy computing, with very large and complex mathematical models. Too simple models do not do. The difficulty of weather forecasting has also demonstrated the difficulty of using particular types of non-linear dynamic models, popularly, but misleadingly, called 'chaotic'; but to understand chaotic models requires sufficient mathematical understanding too.

The author treads on dangerous ground when he gets onto statistics, and even more so onto mathematics. In ¶3.2.3.6 he quotes Haavelmo (1944), and in ¶3.3.14.2 Keynes (1939). Neither of these, to judge by what is quoted, seems to have been aware of statistical methodology, especially modern time series methods, where one is seeking a deterministic 'skeleton' which takes account of interrelationships or correlations of the factors, so that the 'residuals', the unexplained parts, are plausibly independent, and possibly also identically distributed. This is quite different from only being able to deal with independent and identically distributed variables.

Chemists break down molecules into their constitutent atoms. This does not mean that they can only deal with atoms.

In ¶4.2.3.2 the author shows his unfamiliarity with one aspect of actuarial work. For the last thirty years mortality tables have been constructed as continuous functions, by graduating μ_x, not the one-year probabilities q_x. The approximate integration methods in normal use assume that functions such as l_x, μ_x, etc. are continuous (or at least have only a few discrete jumps). Furthermore, the modern multiple state models used for PHI or AIDS modelling rely on continuous models, not discrete ones (see 'The Analysis of Permanent Health Insurance Data', in *CMI Reports*, **12**, 1991, or Wilkie, 1988, 'An Actuarial Model for AIDS', in *J.I.A.*, **115**, 839-853). The continuous model is preferable to a discrete one, because one can state the assumptions more clearly, derive the relevant formulae more easily, and then use discrete methods to approximate the numerical answers more acurately.

The author, in Section 8.2.6, suggests that it is undesirable for actuaries to learn 'proofs'. In my view this would be a dissaster for the profession, if those who are responsible for our education system were to agree.

First, by studying the methodology of a proof, one learns how to manipulate mathematics to prove other results. An example is the old actuarial formula $A_x=1-d.\ddot{a}_x$. If one just learns this result without knowing the proof, it does not tell one whether the similar statements $A_{x:\overline{n}|}=1-d.\ddot{a}_{x:\overline{n}|}$, and $a_{xy}=1-d.\ddot{a}_{xy}$ are true or not; they both are; but ${}_nA_x$ (a temporary assurance) is not equal to $1-d.{}_n\ddot{a}_x$.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

The general proposition is true if the event in question is certain, not if it might not happen. I do not see how one learns these other results without understanding the proof of the first result.

Secondly, understanding the way in which a result has been derived helps one to remember the result, or to be able to reproduce it if one has forgotten it. The formula for the sum of a geometric series (a, ar, ar^2, ..., for n terms) is not too difficult to remember; but if I have to calculate the sums of series like: a, $2ar$, $3ar^2$, ...; or a, $4ar$, $9ar^2$, ... (which are needed to calculate the duration and convexity of bonds); it is easier (for me at least) to remember the method of proof than to remember the rather complicated formulae that result.

Thirdly, understanding the 'proof' of the Capital Asset Pricing Model or the Black-Scholes formula requires one to understand the assumptions on which these models are based, and when they might apply or not apply. If one does not understand how the result is derived, then one cannot form any independent judgement about when or whether the model is applicable. The CAPM fails as an overall equilibrium model if (not necessarily only if) different investors work in different curencies. The Black-Scholes model fails as a universal option pricing formula if (and again not necessarily only if) the price of the underlying is influenced by trading in options, or if the process for the underlying is a jump process rather than a diffusion.

Actuaries who do not wish to understand the methodology of financial economics are putting themselves in the weak position of either rejecting it, because they do not understand it (apparently the author's position), or accepting it unthinkingly and uncritically (the position perhaps of too many in the City). I would like to see a thinking actuarial profession that understood the methodology and could add to the sum of knowledge on the subject, as Kemp (1997) and Smith (1996) have done.

Examples of the misuse or misunderstanding of financial economics are the unthinking quotation of 'betas' on shares, and the false statement that the CAPM implies that all combinations of risk-free assets and risky assets along the efficient frontier are equally desirable (see Exley, Mehta & Smith, 1997).

The author, like the authors of the paper by Bell *et al.* (1998) seems to assume that there is something distinctive about the methods of actuarial science. I see no justification whatever for this view. The statistical methods used by actuaries are the same as (or should be the same as) the best methods used by statisticians; the demographic methods used by actuaries should be the same as the best used by demographers; the economics and financial economics should be the same as the best used by economists and financial economists. To argue that actuarial science, actuarial principles or actuarial methods are different in nature from any other science is nonsense, and makes us look foolish to other scientists.

Actuaries are distinctive because of the fields to which they apply their skills (insurance, pensions and other financial institutions), and, perhaps, in the combination of skills they use. In particular, we deal with "The consideration of all monetary questions involving separately or in combination the Mathematical doctrine of probabilities and the priniples of interest", to quote from the preamble to the Institute Charter; but we are no longer unique in this. Those who work in operational research, business studies, financial economics, even accountancy, are all now aware of discounted present values and probability methods and how to combine them.

To pretend that our methodology is distinctive will run us into the great danger of ignoring developments in neighbouring fields, which, instead, we should be absorbing into our actuarial toolboxes. One can suggest that most actuaries are similar to engineers, who may not be deep theoreticians, but who use the tools provided by physicists, chemists, metallurgists, geologists and others to provide the best advice they can to their clients. We should try to do the same for our clients; but that means using the best tools available, even if they have been invented by others.

The author subsequently wrote: I have now had a chance to read more carefully the comments made at the meeting and to read the written contributions, and would make a few additional points.

Dr Macdonald draws a distinction between financial economics and mathematical economics. The former term is subject to varying interpretations, particularly within the actuarial profession, and its use invariably raises difficult ambiguities. I have, therefore, avoided its use throughout the paper. In my closing remarks I disagreed with Dr. Macdonald's suggestion that actuarial methods are as axiomatic as those of mathematical economics. This is an important point on which I think it helpful to expand a little. Taking the case of the net premium valuation (NPV) model, which Dr Macdonald

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press

cites, there is no possibility of the proof belief danger arising, for the simple reason that there is no proof offered! Actuaries entertain the NPV model as an approximate crude model, which repeated use within the given context of life fund valuation has shown to be reasonably reliable to date within that context. It is used, *pro tem*, as providing an approximate valuation. The important point is that it is not proved from a set of prior assumptions. The use of the Black-Scholes formula in a fashion similar to that in which actuaries use the NPV model would be unobjectionable. What the paper questions is the role of the proof of this formula — this requires further justification.

Professor Boyle addresses the same issue, supporting the teaching of the proof of Black-Scholes on the basis that this provides some deeper insight into the Black-Scholes model. Unfortunately, he is not explicit about the sort of deeper insights that are to be obtained from studying the proof, and how these might be helpful in the practical task of valuing options. Having given up the universal applicability of the original Black-Scholes solution, as Professor Boyle admits is necessary, an account of how a valuation model is to be chosen in the face of a given valuation situation is required. This is the substance of practitioner lore in this area, as referred to in Section 7.4. We need to show some practical examples of how the teaching of the proof informs practitioner lore in a helpful way.

The sort of point that might support the case for teaching the proof, and is perhaps implicit in Professor Boyle's remarks, is that it helps to make apparent the relationship between the valuation formula and the no arbitrage principle in a way which allows us to choose a better valuation model or better valuation parameters. This may be so, but the literature, as yet, appears not to provide any examples of how this actually works. The term 'arbitrage', itself, requires careful treatment; we should distinguish between cases in which there is an opportunity to lock in a guaranteed profit through a set of simultaneous market trades and those where an expected profit is generated over the long term under some long-term assumption set. The former is a powerful driver of price consistency, but the latter has a far less clear influence over the structure of market prices (see e.g. Pemberton, 1998, p18). These important practical distinctions are disguised within the idealised world defined by the Black-Scholes assumptions, which suggests that this is an unhelpful way in which to introduce these important concepts.

It is interesting to note how Professor Boyle refers to the testing of assumptions — this would appear to be an almost explicit admission of reliance on the hypothetico-deductive (H-D) approach, illustrating nicely one of the main points of the paper. He has not addressed the methodological concerns about the H-D method which are identified. He also misunderstands the paper in his suggestion that it "stresses the importance of intuition". On the contrary, the paper is concerned with exploring how to minimise the use of intuition by maximising the appeal to empirical evidence. It is the massive leaps of intuition used within mathematical economics in choosing its assumptions, and in applying its models to specific realities, that is a central concern.

I would reassure Mr Kenna that I certainly do not wish that actuaries should be mere technicians in the service of neo-classical economics — I agree very strongly that our science has much to offer in a broader frame of reference.

Dr Huber raises Hume's problem of induction and the thesis of Duhem-Quine, which, as discussed in the paper, are concerned with the issue of truth inference. These have little relevance to the approximate relationship between models and reality with which actuaries are concerned, and are certainly no hurdle to the use of fact-based models in this way.

I agree with the point made by both Mr Daykin and Mr Goford that the actuarial control cycle embodies well the empirical method of actuaries (see e.g. Goford, J. (1985). *The Control Cycle*. Paper to the Institute of Actuaries Student Society, February 1985).

Professor Wilkie, in his written contribution, helpfully confirms that his methods derive from a logical positivist standpoint. As the paper notes, logical positivism collapsed in the 1960s, and is no longer regarded as a tenable position by any contemporary school within methodology. The actuarial profession needs to use more recent thinking as its basis for development. Professor Wilkie has misread Section 8.2.6, which argues that we should be selective in the proofs which we teach and be clear about the purpose — I agree that the proof he cites concerning actuarial formulae is one which is helpful.

https://doi.org/10.1017/S1357321700000416 Published online by Cambridge University Press