LEVEL II

(12)

MODELS IN INSURANCE: PARADIGMS, PUZZLES,
COMMUNICATIONS AND REVOLUTIONS

by

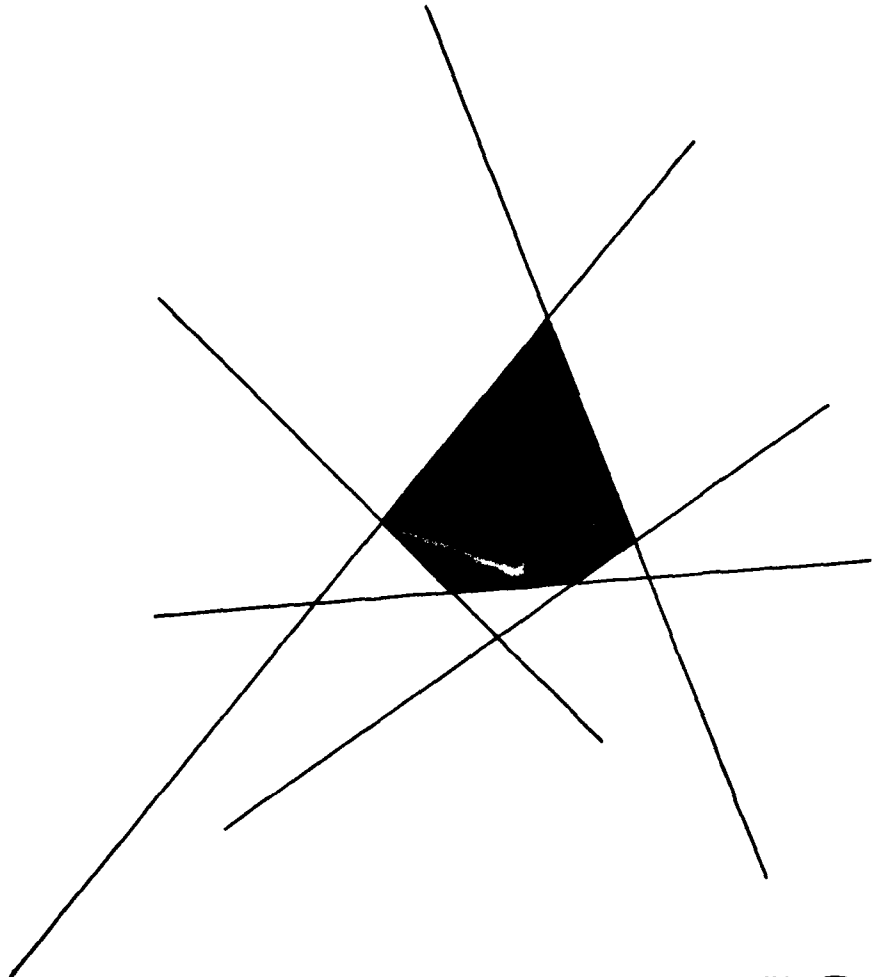WILLIAM S. JEWELL

AD A088596

OPERATIONS
RESEARCH
CENTER

DTIC
SELECTED
SEP 3 1980
D

UNIVERSITY OF CALIFORNIA · BERKELEY

80 8 29 006

LEVEL Ⅱ

MODELS IN INSURANCE: PARADIGMS, PUZZLES,

COMMUNICATIONS AND REVOLUTIONS[†]

by

William S. Jewell
Department of Industrial Engineering
and Operations Research
University of California, Berkeley

DTIC
S ELECTE D
SEP 3 1980
D

JUNE 1980   ORC 80-10

REPORT DOCUMENTATION PAGE		READ INSTRUCTIONS BEFORE COMPLETING FORM
1. REPORT NUMBER ORC-80-10	2. GOVT ACCESSION NO. AD-A088 596	3. RECIPIENT'S CATALOG NUMBER
4. TITLE (and Subtitle) MODELS IN INSURANCE: PARADIGMS, PUZZLES, COMMUNICATIONS AND REVOLUTIONS.		5. TYPE OF REPORT & PERIOD COVERED Research Report,
		6. PERFORMING ORG. REPORT NUMBER
7. AUTHOR(s) William S. Jewell		8. CONTRACT OR GRANT NUMBER(s) AFOSR-77-3179
9. PERFORMING ORGANIZATION NAME AND ADDRESS Operations Research Center University of California Berkeley, California 94720		10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS 2304/A5
11. CONTROLLING OFFICE NAME AND ADDRESS United States Air Force Air Force Office of Scientific Research Bolling AFB, D.C. 20332		12. REPORT DATE June 1980
		13. NUMBER OF PAGES 125
14. MONITORING AGENCY NAME & ADDRESS(If different from Controlling Office)		15. SECURITY CLASS. (of this report) Unclassified
		15a. DECLASSIFICATION/DOWNGRADING SCHEDULE

16. DISTRIBUTION STATEMENT (of this Report)

Approved for public release; distribution unlimited.

17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

18. SUPPLEMENTARY NOTES

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)

Insurance Models	Insurance Business Planning
Life Contingencies	Utility Theory
Risk Theory	Risk Classification
Bayesian Statistics	
Credibility Theorem	

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

(SEE ABSTRACT) —> next pg.

ABSTRACT

Every scientific community reveals its shared beliefs and values, its great achievements and persistent problems, and its current state-of-the-art and evolutionary future through its model-building activity and its scientific communications. To survey and comprehend the field of actuarial science, then, one must examine, classify, and comment upon the basic paradigms -- the accepted concepts-models-puzzles-solutions -- that are revealed in the literature of risk and insurance theory.

This paper attempts such a survey, based upon the communications submitted to Topic 1 of the 21st ICA, 'Generalized Models of the Insurance Business,' and upon a selected portion of the explosive numbers of papers and books which have appeared in the open literature in the last decade. Clearly, many of the basic paradigms are still in a state of flux, as new ideas and methods, in many cases imported from other disciplines, have begun to compete with previously accepted paradigms, causing, in some cases, displacements of methodology and minor revolutions in conceptual approach.

In addition to a general model survey, the paper considers the influence of ideas from other scientific disciplines, and then examines in detail two specific areas where traditional modelling has been called into question -- the classical approach to statistical estimation and prediction, and the fair-premium approach to risk classification. In conclusion, some inferences about the near-term future of insurance modelling are made.

1473

INTRODUCTION

It is a distinct privilege and a personal pleasure to address the 21st ICA on the topic of "Generalized Models of the Insurance Business". In addition to thanking the Organizing Committee for their invitation, I would also like to acknowledge the role of the U.S. Section of the International Actuarial Association in graciously proposing my name for membership in this international scientific community; however, I must say that it was not at all clear to me that the by-laws require that every new member must present a communication at his first International Congress!

As I read through the 42 papers offered on this topic, I became somewhat uneasy at the variety and divergence of the concepts and models presented, and this feeling was only heightened when I began to reflect upon the many and varied papers and textbooks in insurance and risk theory which have appeared in the past decade. To comprehend all of this is, as we say in English, "like trying to take a drink of water out of a firehose." What could a physicist-engineer-operations researcher who has not had extensive actuarial practice hope to add to this topic, which is central to the profession?

Nevertheless, model-building is an activity which is common to all scientific disciplines, and upon which scientific historians and philosophers have had much to say. I resolved, therefore, to first of all set out a philosophical framework -- a model of model-building -- in which we could begin to understand this recent explosion of activity in insurance model-building.

SOME PHILOSOPHY ON MODELS AND MODEL-BUILDING

Basic Characteristics

All of you are, I am sure, familiar with the means and ends peculiar to what we call the scientific approach [B47, W4]. This complex of shared values and attitudes towards problem-posing and problem-solving is so ingrained in our everyday work and communication with our colleagues that it is difficult to explain to the layman exactly what we mean by hypothesizing, experimentation, measurement, model construction, calibration and validation, and implementation.

The communication problem becomes even more difficult when we attempt to define what we mean by a model, for the term is used in bewildering variety of ways by the scientific community, ranging from material representations of phenomena through factual interpretations of real-world behavior to purely symbolic idealizations of abstract theories [B47]. For our purposes, we can, however be more pragmatic, and attempt a definition as follows:

> A model is a set of verifiable mathematical relationships or logical procedures which is used to represent observed, measurable real-world phenomena, to communicate alternative hypotheses about the causes of the phenomena, and to predict future behavior of the phenomena for the purposes of decision-making.

We might call this an operational definition, for I have purposely excluded certain gedanken exercises by focusing on observable, measurable phenomena, and insisting that the ultimate goal of model-building is either as a tool for communicating with other scientists and society at large about the nature of the

phenomena, or for predicting and making decisions about the future behavior of the phenomena -- which in our case are all the risky contingencies associated with life, death, and the loss of economic property on this Earth. I hope you agree with this starting point.

Paradigms, Puzzles, Communications, and Revolutions

But to gain a better perspective upon current activity in models and model-building in actuarial research and insurance practice, we shall need a broader Weltanschauung than a mere description of model characteristics and model-building activities; I have adopted the title and organization of this paper from the seminal ideas put forward by Thomas S. Kuhn in The Structure of Scientific Revolutions [K7].

To paraphrase broadly, Kuhn defines a paradigm as those universally accepted scientific achievements, concrete concepts-models-puzzles-solutions-examples, that for a time provide model solutions for a community of scientific professionals; more generally, it can stand as a banner for the entire constellation of symbolic generalizations, shared beliefs, judgement values, techniques, and so on shared by the community -- for example, those ideas, concepts, models, and solutions shared by the actuaries of the world. When such a paradigm is universally accepted, then the researchers in this community are free to engage in normal science, that is, the highly-directed, paradigm-based theoretical, experimental, and empirical investigations which provide the elaboration and mop-up work needed to apply the paradigm in the businesses and general society which support the

4

community.

But, in addition to the "bread-and-butter" work permitted
by normal science, it must be recognized that these accepted
scientific laws, models, and concepts are inherently self-limiting
in guiding the theoretical activity of the profession, since the
paradigm itself provides the criteria for choosing future research
areas, which can only be assumed to possess solutions of an
accepted and understood nature. In other words, normal science

> "...seems an attempt to force nature into the preformed
> and relatively inflexible box that the paradigm supplies.
> No part of the aim of normal science is to call forth new
> sorts of phenomena; indeed those that will not fit the
> box are often not seen at all. Nor do scientists normally
> aim to invent new theories, and they are often intolerant
> of those invented by others. Instead, normal scientific
> research is directed to the articulation of those phenomena
> and theories that the paradigm already supplies." [K7, p.24].

But how, then, is science to make progress and overcome this
inherent limitation? Kuhn believes that, at first, progress
occurs precisely because of this restriction of focus and attention
to detail, which means that, as scientists increasingly satisfy
the needs of the society which pays the bill, they will tend to
turn their research attention to puzzles, "that special category
of problems that can serve to test ingenuity or skill in solution."
Increasingly, communications with other research workers become
more esoteric and inaccesible to the general public, research
monographs and conferences are devoted to single mathematical
puzzles, and there is increasing tension between the theoreticians
and the practitioners, who feel that there are important real
problems still unsolved, but find it more and more difficult to
communicate with the specialists. Nevertheless, the normal science

seems to be progressing more and more rapidly and predictably, as judged by the shared paradigm.

Then, something begins to happen which provokes a <u>crisis</u> within the profession. In the physical sciences, this might be anomalies in experimental data which cannot be explained away, or the empirical discovery of a completely new phenomenon not covered by the old paradigm. I believe the correct analogy in actuarial science, which is governed by the laws of economics and the marketplace, is that some new phenomenon, such as hyper-inflation, changing living habits, or the application of novel technology begins to affect our collective social and economic behavior, which in turn contradicts the assumptions behind traditional insurance products. Or, the occurrence of certain natural phenomena, such as earthquakes, transportation disasters, sickness and disease, affects insurance statistics through the ways in which man and nation attempt to adapt and organize themselves to combat these disasters.

At first, the reaction to these crises is simply increased activity within the old paradigm, as attempts are made to study the anomaly and to patch up those methods and models which worked so well in the past. But at some point, the difficulty in the paradigm-nature fit will not be able to be set right by the traditional processes, and, precisely because of the excellent, specialized communication network between specialists:

> "...the anomaly itself now comes to be generally recognized as such by the profession. More and more attention is devoted to it by more and more of the field's most eminent men. If it still continues to resist...many of them may come to view its resolution as <u>the</u> important subject matter of the field" [K7, p.82].

Many divergent partial solutions will be attempted, and specialists
from neighboring disciplines will try their hand at resolving the
anomaly through the introduction of other points of view and
methodologies. Corporate management, regulators, and legislators
will also try to resolve matters directly through their powers,
rather than waiting for the community to resolve the anomaly.
Through this proliferation of ad hoc adjustments, the rules
governing the paradigm will become increasingly blurred, practi-
tioners may begin to disagree on the nature and basic hypotheses
of the field, and shared standards of value and judgement may be
called into question.

Then finally occurs what Kuhn calls a scientific revolution --
the appearance of a competing paradigm which begins to accumulate
a weight of evidence and coherence and to attract an increasing
number of disciples and camp-followers -- especially if it satis-
factorily resolves the pressing anomaly and provides useful guides
to action by the practitioners who pay the bills and the regulators
and legislators who answer to the general public. But this
revolutionary process may proceed slowly, for it often requires
an important, discontinuous shift in world-view in the scientific
community. Some practitioners are forever resistant, because
lifelong, productive careers and reputations commit them to an
older tradition of normal science. And often, the arguments which
are most convincing in favor of the new paradigm are not easily
explained in the old terminology. "In a sense that I am unable
to explicate further, the proponents of competing paradigms
practice their trades in different worlds" [K7, p.150].

Evolutionary progress according to Kuhn, then, must occur

through a series of discontinuous steps: the formulation of a
successful paradigm and the development of the professional
community which shares that paradigm; the solution of a large
variety of practical problems which establishes the discipline
and leads to an active phase of normal science; then, a move-
ment towards more and more "purposeless" puzzle-solving and
increasingly specialized and esoteric communication; followed,
sooner or later, by an anomaly in theory or an application disaster
which forces a crisis upon the profession. The resolution of
this crisis requires the appearance, testing, and acceptance of
a competing paradigm which more successfully solves the problem
at hand. But because this scientific revolution causes a dramatic
shift in values and concepts, its further growth and influence
cannot be predicted, but only be discussed, tested, and applied
by the reformed community, which must adapt to survive.

A Personal View

My personal view, if you will permit, is that something like
this progression described by Kuhn is, in fact, now occurring in
insurance modelling. Perhaps revolution is too strong a term.
Nevertheless, I hope to convince you that, as revealed by your
own communications, we are at a very interesting epoch in the
history of actuarial science -- on the one hand, there is a
fruitful and prosperous synthesis between and multiplication of
basic paradigms which are shared by all, and yet, at the same
time, there is a progression towards more and more academic
puzzle-solving, together with disquieting news from the real-world-
application front line.

Let us begin by considering those risk and insurance business paradigms upon which we all agree and surveying their current development, with a few remarks on their strengths and weaknesses. I will next comment upon the introduction of new points of view from other scientific disciplines, and then consider in detail two specific areas where a crisis is in progress -- statistical estimation and prediction, and risk classification.

In selecting additional references to demonstrate the variety and growth of the field, I have rather arbitrarily limited myself to articles which have appeared within the last five years, or which seemed to be particularly useful. And because many national society journals were not available to me, these contributions are also underrepresented. My apologies to colleagues who find their favorite references missing.

BASIC RISK PARADIGMS

General Characteristics

Traditionally, the basic models of risk are divided into two distinct classes: those used in life insurance companies, and those which arise in non-life applications. This class distinction has been slowly vanishing, as new risk coverages have demanded a combination of the two approaches, and as work on more easily shared insurance business models has progressed.

As stated by Professors Amsler [80.1], Bühlmann [76.20], and Franckx [76.21], and many others, there seems now to be general agreement that the basic mathematical models common to all branches of insurance have three key elements:

(1) One or more random variables which characterize the major dimensions of the risk, such as duration, size, and number;

(2) A set of well-defined states of nature, separated by observable transition events or epochs, together with a deterministic or stochastic law of motion between the states;

(3) An economic function, associated with the underlying random variables and/or the states and transition events, which may also be deterministic or random, but is most often linked to uncontrollable economic externalities, such as market growth, inflation, currency risk, etc., but also to economic performance under the control of the company, such as profit margin, portfolio performance, etc.

Even though one or more of these elements may appear to be missing in the simplest models, it is usually merely suppressed by long-standing convention, hypothesis, or for reasons of simplicity. As one begins to construct more and more complex models of actual insurance operations, or to build large-scale

simulations, then all of these factors begin to come into play --
indeed, one is often forced to synthesize and orchestrate a
number of simpler, specialized risk models.

To illustrate this rather philosophical point, let us consider
some of the basic risk paradigms currently in use. Some of the
results expressed in this Section may seem like "old wine in new
bottles," but I hope that the new bottles and labels will help
you in reorganizing your wine cellar, and eliminating vintages
which are past their prime years.

We shall use tildes to denote random variables, and E and
V to denote the expectation and variance operators, respectively.
In other words, $\tilde{x}$ is a random variable with observed value x ,
and $E\tilde{x}$ and $V\tilde{x}$ are its mean and variance.

Life Contingencies

In life assurances, the state space is extremely simple --
a person is living, and then after the unique event, death, makes
a transition to deceased status; the basic random variable of
interest is $\tilde{T}_x$, the <u>remaining lifetime</u> from moment of under-
writing until death, given the information (x) , usually the
individual's age, sex, health, etc. The probability distribution
of $\tilde{T}_x$ is usually given by an empirically observed <u>mortality
table</u>, although certain analytic laws are sometimes explicated,
calibrated, and used. Often, the remaining life of an individual
insured at age x , but now aged x+t , given that he is still
alive, is assumed to be given by the same ("non-select") table
or law. Life companies worry a lot about receiving business from
individuals who may know more about their own personal mortality

than that reflected in this general law.

There are two primary cash flows of interest: $1 lump sum payable at time t , and a hypothetical stream of $1 per year, payable continuously from time zero to time t . The economic functions of interest are the associated present values at the origin. If the force of interest is δ per year, then we find easily the present value of the lump sum as $A(t) = \exp(-\delta t)$, and the present value of the continuous annuity as $a(t) = \delta^{-1}[1 - \exp(-\delta t)]$. These functions and their present values can be moved forward h time units by multiplying by the "shift operator", $\exp(\delta h)$; different fixed "face amounts" of F$, or f$/year just multiply A and a , respectively; more complex combinations of cash flows follow by superposition, or super-position and shifting, or convolution. For instance, the present value of an annuity of $1/year from epoch t_1 to t_2 is simply $a(t_1,t_2) = a(t_2) - a(t_1)$; a perpetuity beginning at t_1 has value $a(t_1,\infty) = \delta^{-1} - a(t_1) = \delta^{-1}A(t_1)$; etc. Even a variable-face continuous annuity of amount $f(t)$ $/year at time $t(0 \le t \le \infty)$ has present value $\int e^{-\delta t} f(t)\, dt = \int A(t)f(t)\, dt$, from which integration by parts gives $F(0+) + \delta\int A(t)F(t)\, dt$, with $F(t)$ the integral of $f(t)$; from this "transform calculus" a whole new variety of forms and interpretations can be gotten. But that's essentially all there is to economic forms in life assurance.

The link to the underlying risk process is through the durations or epochs of these forms which are now random variables like $\tilde{T}_x$. For instance, the present value of $1 paid at the death of (x) is simply the transformed random variable $A(\tilde{T}_x)$, and

the present value of $1/year payable until the death of (x) is the transformed random variable $a(\tilde{T}_x) = \delta^{-1}[1 - A(\tilde{T}_x)]$. Making the necessary transformation between a given analytic form of the density for $\tilde{T}_x$ to the densities of these new values, and calculating the moments, etc., is a standard exercise in a first-year course in probability.

In fact, as has been forcibly stated many times [G8, G11, H17, T13], the fair premiums which are of interest to the actuary are nothing more than the mean values of these random variables; in classical notation:

$$\bar{A}_x = EA(\tilde{T}_x) \quad ; \quad \bar{a}_x = Ea(\tilde{T}_x) \quad .$$

Note that this approach to fair premiums is independent of whether $\tilde{T}_x$ is continuous or quantized to end-of-year deaths, and that periodic rather than continuous payments will be reflected in the choice of economic function, not necessarily in the range of the random variable. Further, many of the so-called "theorems" relating different fair premiums are trivial in this context, and (see above) may often hold for the random variable as well as for its mean value.

Multilife contingencies are also simpler from the point of view of the random variable. Consider, for example, the reversionary continuous annuity to (x) after (y) , and suppose we know the joint density of the two random remaining lifetimes, call them $\tilde{T}_x$ and $\tilde{T}_y$. If we define a new random variable, $\tilde{T}_{xy} = \min(\tilde{T}_x, \tilde{T}_y)$, then by direct arguments, this new economic function is nothing more than the present value of a continuous annuity paid from

$\tilde{T}_{xy}$ to $\tilde{T}_x$, i.e., $a(\tilde{T}_{xy}, \tilde{T}_x) = a(\tilde{T}_x) - a(\tilde{T}_{xy})$. Not only is the usual magical identity true for the random discounted value as well as the mean, it can be seen immediately that the assumption of independence of the two lives is not needed, provided simply that the marginal density of $\tilde{T}_{xy}$ is correctly calculated. In an obvious analogy to reliability engineering, more complicated multi-life contingencies are easily handled through Boolean logic, and associated min and max operators.

Another advantage of modelling directly with the random variables is that variances and other moments can easily be obtained, Hattendorf's theorem proven, prospective reserves calculated, and even full distributions of outcomes obtained (see [G8, H11, T13] and references therein).

The reason that I have spent so long on this elementary modelling is, first of all, to convince you that a major educational overhaul is overdue in this area. I regularly teach the material covered in life contingency textbooks to (post)graduate engineering and statistics students in about six hours of lecture; we are then free to analyze variances and reserves (such as the adaptive reserve model described in the Section on Bayesian Statistics), do mortality estimation, or to proceed to casualty risk models. I admit that, at the undergraduate level, where a student may have had only one course in probability theory with little application, it might take longer, say, thirty hours, to introduce the basic principles of life contingencies; but by using random variable concepts and methods with which the student is already familiar, it is possible to move quickly into more "advanced" topics. In my opinion, there is a mismatch

between the formation and the capabilities of today's students, and the demands which are placed upon them using the traditional expected-value models. Furthermore, by not properly laying down the fundamental concepts of random variables and the basic model assumptions, a potential for future change in actuarial modelling is being wasted -- for example, in developing newer models for equity-linked assurances, pension and health-care applications, or in strengthening corporate modelling and simulation.

Secondly, I suppose I must say something about the archaic notation with which the life actuary is burdened, and which is the subject of continued, in my opinion rather pointless, discussion [76.22, B22]. In comparison with other scientific communities, it seems as if actuaries lead the way in insisting that all of the hypotheses about the model, not just the features of interest, be hung as "bells and whistles" on the underlying variable, as in

$$\substack{T,i \\ n|m}a^k_x \quad \text{or} \quad (k)(n{:}m)a(x)(i;T) \quad .$$

Although this approach may guarantee full-employment for typesetters, it does not help in actual computations (where the parameters would be passed to the computation subroutine by global variables, a standardized calling sequence, a data block, etc.), and it definitely impedes scientific communication outside of actuarial circles. Being rigid about notation for mean values, and requiring that the density for $\tilde{T}_x$ always be written $_Tp_x\mu_{x+T}$, etc. makes the problem of defining efficient, easily recognized random variable notation more difficult, as examination of some of the earlier references will reveal.

Finally, from a modelling point of view, focusing on the mean values of discounted random cash flows can obscure the fundamental hypotheses being made, lead to erroneous interpretations and conclusions, may give incorrect or misleading financial projections, and will impede the construction of more general, meaningful models -- in short, "normal science" at its worst. Because this point is so important, I would like to give a simplified example using random variables.

Suppose we are trying to find the fair level continuous premium, Π_x \$/year, to charge (x) for a life assurance of \$1, payable at death. There are two random cash flows involved -- the receipt of premium income until death, and the payment for benefits provided at death. Their difference -- the underwriting gain to the company -- is also a time-dependent random cash flow. Suppose we let $\tilde{G}(0)$ be the present value, at the moment of underwriting, of this random future gain. Using random variables introduced previously, we obtain:

$$\tilde{G}(0) = \Pi_x a(\tilde{T}_x) - 1 \cdot A(\tilde{T}_x) \quad .$$

Now, to find the premium rate, we must invoke a new modelling assumption -- the equivalence principle, which states that we define a random cash flow to be "fair" if its mean value is zero (and note that even this assumption may be modified if we adopt the viewpoint of utility theory, see later). This new hypothesis that $E\tilde{G}(0) = 0$ is what furnishes the classical result $\Pi_x = \bar{A}_x/\bar{a}_x$. But now we find directly other results, such as the variance of this random gain:

$$V\{\tilde{G}(0)\} = \left[1 + \frac{\Pi_x}{\delta}\right]^2 V\{e^{-\delta \tilde{T}x}\} \quad ,$$

which is not always significant for human mortality, but adds a considerable risk in engineering-economic equipment replacement studies.

As time t passes, and the individual has not yet expired, the random current value grows to $\tilde{G}(t) = e^{\delta t}\tilde{G}(0)$. A priori, this new random variable has zero mean for a fair premium rate, but, at time t , we will know that death has not yet occurred, and the distribution of the random remaining lifetime, $\tilde{T}_{x+t}$, will have to be recalculated, according to $\tilde{T}_{x+t} = \left[\tilde{T}_x - t | \tilde{T}_x > t\right]$. Using basic definitions and rearranging terms, we find the current value of random underwriting gain at time t , given that (x) is still alive, to be:

$$\tilde{G}(t) = \Pi_x \delta^{-1}\left[\exp(\delta t) - 1\right] - \left[1 \cdot A(\tilde{T}_{x+t}) - \Pi_x a(\tilde{T}_{x+t})\right]$$

We recognize the first term as the accumulated "sure-thing" premium income; the second term is the random future net liability to the company, discounted back to epoch t -- indeed, it is immediate that the mean value of this discounted liability is just the legal level-premium reserve, $_tV_x$. So far so good.

But now, having "led you down the garden path" with familiar results, slightly generalized, let us calculate an unfamiliar variable -- the random amount of gain to the company at the moment of expiration, which must be

$$\tilde{G}(\tilde{T}_x) = \Pi_x \delta^{-1}\left[\exp(\delta \tilde{T}_x) - 1\right] - 1 \quad .$$

A priori, this amount is not zero in expectation -- indeed, a little

calculation will show that for small forces of interest:

$$E\{\tilde{G}(\tilde{T}_x)\} = \delta V\{\tilde{T}_x\}/E\{\tilde{T}_x\} > 0 \qquad (\delta \to 0) \quad,$$

or for usual interest rates and standard mortality tables, about 5-10% of the face value, on the average, is "left on the table" for the company at expiration! This unexpected result cannot even be developed through the use of classical expected-value notation. And, I submit, explaining why this result does not affect the profitability of the company is a valuable exercise for the student in understanding the basic models of life contingencies.

Health and Pension State Models

The element of a state variable is especially important when we progress to the more complex models of health and pension insurances. As shown in Figure 1, we imagine that the covered individual moves from one state to another at certain random transition epochs, and that these states define different coverages or benefits and different mortality laws. In health insurance, these states are the observable differences in health, conditions of heart, limbs, or teeth, progress of a disease, marital and family status, parturition, etc.; in pensions, the states can refer to active or disability status, eligibility and participation status, wage category, employment and participation history, vesting, retirement status, and so on. In most cases, death is the terminal state, although it is often important to condition on the previous status or history.

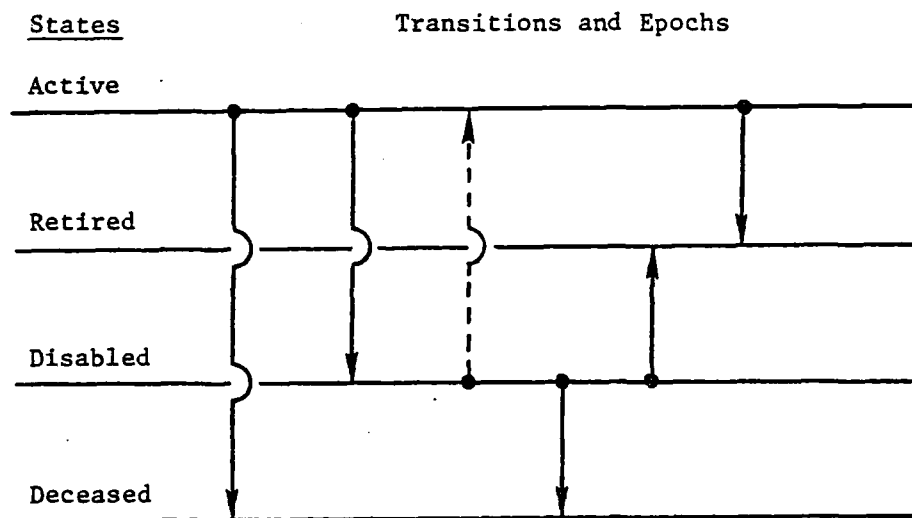The traditional way of describing the associated laws of.

18

States   Transitions and Epochs

FIGURE 1

Pension and Health States, Epochs and Transitions

motion is through _multiple_ _decrement_ _theory_, that is, a failure-rate-oriented competing-risk approach (see, e.g. [H11]). While this approach is useful for simple problems because it can be explained in terms of related single-decrement tables, it supposes that the sequence of states is in extended (tree-like) form. In more complex models, it is often convenient to assume (like the dotted line in Figure 1) that certain states can be recurrent. Then, the more natural formulation is in terms of Markov chains ([C1, H14], and the references in [H17]). With arbitrary continuous duration random variables, the model is, in fact, a Markov-renewal (or semi-Markov) process, whose transition and duration laws can be easily related to an equivalent competing-risks formulation. Economic functions can be tied to both the transitions and to the durations in certain states, but, of course, making certain states recurrent implies equivalence of economic sequences after re-entry.

If the underlying duration law is discrete, as in transitions only at the end of the year, or on birthdays, etc., then the Markov chain approach can have the state space expanded to include age, years of service, etc., and the only law of interest becomes a rather large, special-structure set of transition probabilities determined from the associated decrement tables [B2, H15]. The advantage of this approach is that the only practical calculations involved are the multiplications of large matrices, which can be easily carried out on computers. Numerous other insurance calculations [80.26] can be similarly simplified, and there are many related models in the fields of demography, sociology, educational planning, manpower planning, and so on.

With such a natural and well-understood paradigm, it is
surprising to see that most of today's pension mathematics is
still oriented towards certainty equivalents. Admittedly, the
large numbers of variables and laws makes analytic manipulations
rather formidable; even recent dynamic models [B35, 80.48, 76.5]
require rather heroic mathematics to get expected-value results.
But there are other studies of more specialized topics [B2, S11,
S12, S13] which are beginning to apply a complete random-variable
analysis to pension problems, which, of course, is essential to
answering questions, such as the probability that a given method
of funding will be adequate; this calculation need not be analytic,
but can be easily explored with modern computers. I expect to
see a rather substantial evolution of stochastic pension models
over the next few years.

The field of health insurance seems to be currently oriented
towards data-gathering and empirical investigations, rather than
formal modelling -- in part, because of the explosive growth in
the technology and costs of health-care delivery (see Topic 3 of
this Congress). So I believe it will be many more years before
one can give a balanced survey of this field. [J3] mentions a
qualitative state-space model for monitoring the rehabilitation
process in worker's compensation claims, and there are similar
operations-research-oriented models in the health care field.

Accumulations of Risk

The basic and most successful casualty insurance paradigm is
the accumulated claim, or aggregate claim, or risk model, in which
two random variables of interest are explicity recognized -- the

random <u>number</u> <u>of</u> <u>claims</u> $\tilde{k} = \tilde{k}(t_1,t_2)$ in some exposure interval

(t_1,t_2) , and the <u>random</u> <u>value</u> <u>of</u> <u>each</u> <u>claim</u> $[\tilde{x}_1,\tilde{x}_2,\ldots\tilde{x}_{\tilde{k}}]$ [B37,

S6] . The economic function of interest is the random <u>total</u> <u>of</u>

<u>accumulated</u> <u>claims</u>, $\tilde{s} = \tilde{s}(t_1,t_2)$, which is a random sum of random

variables:

$$\tilde{s} = \tilde{x}_1 + \tilde{x}_2 + \ldots + \tilde{x}_{\tilde{k}} \quad ;$$

the stochastic curve generated by $\tilde{s}$ as t_2 increases, with t_1

fixed, is sometimes called the <u>accumulated</u> <u>claim</u> <u>process</u>, to

distinguish it from the underlying point process of the

<u>claim</u> <u>epochs</u>, which generate the monotone <u>claim</u> <u>counting</u> <u>process</u>,

$\tilde{k}$, as t_2 increases (Figure 2). Usually two assumptions are made:

(1) Given $\tilde{k} = k$, the random variables $[\tilde{x}_1,\tilde{x}_2,\ldots\tilde{x}_k]$ are
     mutually independent and identically distributed;

(2) Further, their common distribution does not depend upon k .

The first assumption is apparently satisfied or is a satisfactory

approximation in practice, for I can find no discussion or verifi-

cation of this in the literature. There are occasionally examples

where (2) is not verified, as when someone who has a large number

of claims turns out to have smaller (or larger) claims, on the

average, than someone with a smaller number of claims; but there

is another explanation for this observation, given below in the

Section on Credibility Theory.

Assuming both (1) and (2), the first two moments are the

important and familiar relations:

$$E\{\tilde{s}\} = E\{\tilde{k}\}\cdot E\{\tilde{x}\} \quad ; \quad V\{\tilde{s}\} = E\{\tilde{k}\}\cdot V\{\tilde{x}\} + V\{\tilde{k}\}\left[E\{\tilde{x}\}\right]^2 \quad ,$$

Accumulated Claim Process, $s(t)$

x_3

Claim Amount

x_2

x_1

t

Claim Counting Process, $n(t)$
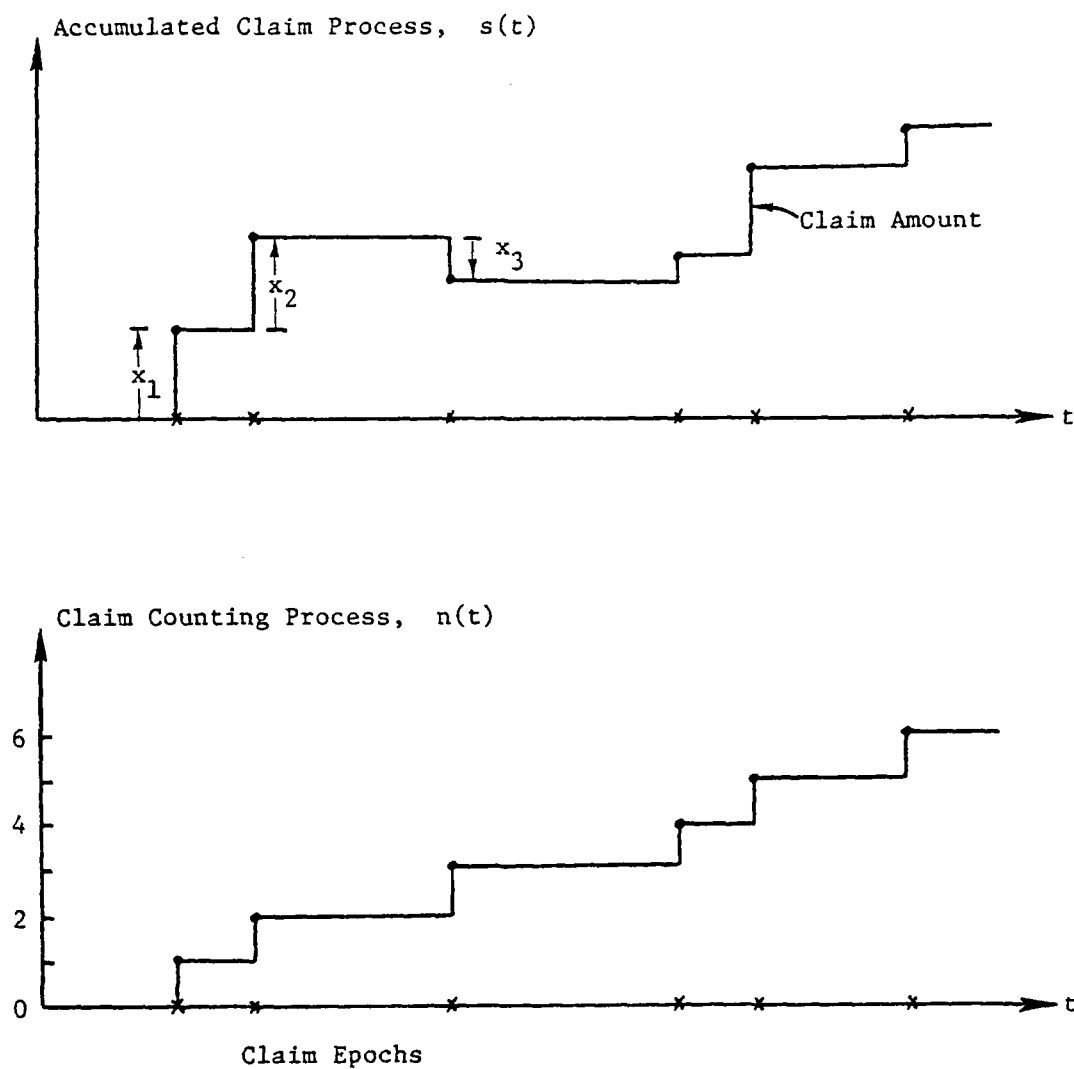
6

4

2

0

t

Claim Epochs

FIGURE 2

Claim Epochs, Claim Counting Process,
Claim Amounts and Accumulated Claim Process

where $\tilde{x}$ stands for a generic claim amount. Notice that, although these assumptions require the claim sizes to be stationary in time, the claim number process may still have a time-varying law. Different formulations, such as underlying birth-and-death, renewal, or time-varying Poisson, have been attempted, but as a practical matter, it is difficult to work with models in which the numbers of claims in adjacent intervals are dependent. So, usually, a stationary, or simple time-varying Poisson assumption is made. The resulting distribution of $\tilde{s}$, after a possible operational time transformation, is the familiar mixture of convolutions of the distribution of $\tilde{x}$.

Surprisingly, in spite of the Central Limit Theorem and the Poisson assumption. it turns out to be difficult to get exact results *or good* approximations to the total claim distribution in the case of general individual claim distributions [B7, S6] . As the controversy over which is the best approximation method is still going on, I will close simply with the remark that all paradigms, including approximations, should be judged in terms of their usefulness for making decisions, or in communicating with other scientists, but, as far as I can tell, this has not yet happened.

Collective Variability

The next model was developed primarily to explicate certain non-obvious variations in observed outcomes from a portfolio, or collective of risks; however, it has, as we shall see, profound implications for model-building and for estimation.

Consider that, for a single risk contract, we are measuring

some outcome random variable, $\tilde{x}$, and that the distribution density

of $\tilde{x}$, $p(x|\theta)$, depends upon one or more parameters which we here

symbolize by θ . Now, if θ were an observable physical quantity,

then we can imagine that it would be easy to assemble a cohort of

M individual risks, i=1,2,...M , each of which had the same

parameter values, $\theta_1 = \theta_2 = \ldots = \theta_M$, but from each of which we

could draw one sample outcome, $x_1, x_2, \ldots x_M$ as M independent,

identically distributed, random samples from the same "urn",

$p(x|\theta)$. But many of the interesting parameters in casualty

insurance, such as "accident-proneness", are difficult to measure,

and one can easily imagine that, no matter how hard one would try

to assemble a homogeneous portfolio, there would be still factors

which could not even be described, and which would lead to unexplain-

able residual variabilities [A3] .

The intuitive leap which makes this a most useful paradigm

is to imagine that, in fact, the abstract parameters θ_i for each

risk i=1,2,...M , are not the same, but are given by selecting

the risk parameters from some structure function density, $u(\theta)$,

which describes the variation of the θ_i as if they were indepen-

dent samples of some random quantity $\tilde{\theta}$ [B37] . This leads to an

"urn of urns" interpretation, shown in Figure 3, in which the

drawing of a particular random outcome, $\tilde{x}_i = x_i$, is a two-stage

process, in which a risk parameter $\tilde{\theta} = \theta_i$ is selected using the

structure density, $u(\theta)$, and then x_i is an independent sample

from the conditional density $p(x|\theta)$. This means that each x_i

has the same marginal mixed density, $p(x) = \int p(x|\theta) u(\theta) d\theta$,

but more importantly, means that the joint density of several

samples from the same risk i , $[\tilde{x}_{i1}, \tilde{x}_{i2}, \ldots \tilde{x}_{in}]$ appears, a priori,
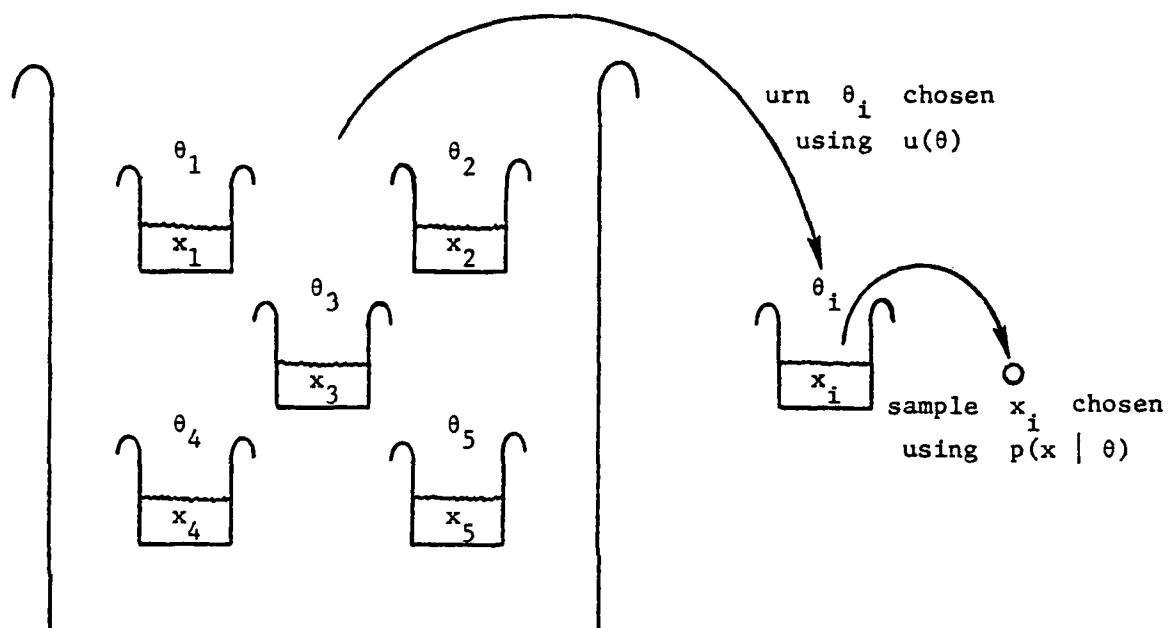
FIGURE 3

Collective Variability and the Urn of Urns Paradigm

(before knowing θ , and averaging over the collective) to be dependent,

$$p(x_{i1}, x_{i2}, \ldots x_{in}) = \int p(x_{i1}|\theta) p(x_{i2}|\theta) \ldots p(x_{in}|\theta) \, u(\theta) \, d\theta \quad,$$

even though, when the particular urn value, θ_i , is given, the successive samples will be independent. This leads to a dependency which makes it possible to succesively "narrow in" on the correct value of θ_i , even though it is unobservable, by taking a large enough number of samples from urn i (see the Section on Bayesian statistics).

The practical importance of the mixing model is that it introduces a new source of variability due to the variation of $\tilde{\theta}$ in the collective. If we let

$$m(\theta) = E\{\tilde{x}|\theta\} \quad; \quad v(\theta) = V\{\tilde{x}|\theta\} \quad;$$

be the first two moments of the observable value $\tilde{x}$, drawn using $p(x|\theta)$ from an urn with a given θ , then it can easily be shown that the result of making a draw from an arbitrary urn in collective (e.g. from $p(x)$) must have first two moments:

$$m = E\tilde{x} = Em(\tilde{\theta}) \quad;$$

and

$$v = V\tilde{x} = E + D \quad; \quad E = Ev(\tilde{\theta}) \quad; \quad D = Vm(\tilde{\theta}) \quad.$$

The first result is expected, since the overall mean is simply the average-over-the-collective mean. But the total variance has two components: the first, E , being the average-over-the-collective of the individual urn variances, and the second, D , representing

the variability-over-the-collective of the mean result from each risk. If the portfolio were homogeneous, this unexpected term D would vanish.

Practically, the mixing of distributions leads to new forms which are often very useful, and match empirical data well. For instance, if $p(x|\theta)$ is Poisson with parameter θ , and the structure density is Gamma distributed, then the mixed density is the useful Negative Binomial.

The only serious challenge to this paradigm seems to be the philosophical point that we perhaps have no business modelling with a quantity which is unobservable, and may not follow the usual laws governing a random variable. This is a fine point which depends upon what one means by "random". But if we believe that the model holds, there are ways to infer the law of $\tilde{\theta}$, if the data set is large enough, which we shall describe in a later Section.

Extreme and Dangerous Values

There is another concept which is part of the model-building tradition in non-life insurance, but as yet is only partly formed -- the idea that, no matter what model we use, actual data will always contain some dangerous "surprises" that can lead to business instability and ruin.

Leaving aside personal views that "Nature is always perverse", we might first of all attempt to describe this behavior by using extreme value theory to estimate the largest observed claims and to set reinsurance treaties [76.9, B19, T15]. See also [80.30, 76.15, R1, R2] for applications where extreme values are part of

the risk process.

Another approach is to develop justifications and estimation methods for long-tail "dangerous" distributions, such as the Pareto [80.44, B14]; these distributions are not often used for regular modelling because higher moments may not exist.

Yet another approach [76.17] is to consider that what is really happening is a mixture of two models, in which the "regular" values are being contaminated by the "abnormal" ones. This, of course, is related to the collective model just analyzed, and to the Bayesian problem of model mixtures (see later).

There is also a well-developed theory of outliers in the statistical literature, but I am not certain if the ideas represented above are simply expressions of this phenomenon, or whether the authors mean that a more structured, as yet imprecise, kind of dangerous risk mechanism is at work. It will be interesting to see if this paradigm develops further.

THE DYNAMIC RISK PROCESS PARADIGM

Reserves versus Stability

The first model which analyzed the effect of random fluctuations upon the risk business of the insurance company as a whole is the venerable <u>collective</u> <u>risk</u> <u>theory</u>, which to avoid confusion with a previous usage, will be referred to as the <u>dynamic</u> <u>risk</u> <u>process</u> <u>paradigm</u>. If we imagine that a company has developed a fixed portfolio of individual risk contracts in a given business line, then its accumulated net premium income in interval $(0,t)$ can be approximated by the straight line Πt, where Π is the total premium rate. Against this pure premium income must be paid out the <u>stochastic</u> <u>accumulated</u> <u>claim</u> <u>process</u>, $\tilde{s}(t) = \tilde{s}(0,t)$, described in the last section, assumed now to be aggregated over all the contracts in the homogeneous portfolio, with the distribution of claim amount, $\tilde{x}$, the same for all risks, and the claim number process $\tilde{k}(t) = \tilde{k}(0,t)$ referring to the counting of all claims from the portfolio. This cohort is said to be "in balance" if $\Pi = E\{\tilde{x}\} \cdot E\{\tilde{k}(t)/t\}$, either for the interval $(0,t)$ under consideration, or for t "large enough", depending upon the underlying claim number process.

The difference between the two accumulating processes, the underwriting gain for the portfolio, will, on the average be zero, but of couse may vary widely into negative and positive values. To provide an element of stability, management furnishes an initial amount of <u>risk</u> (<u>fluctuation</u>, <u>free</u>, <u>technical</u>) <u>reserve</u>, R_0, and then describes its underwriting results in terms of the <u>risk</u> <u>reserve</u> <u>process</u>, $\tilde{R}(t) = R_0 + \Pi t - \tilde{s}(t)$, shown in Figure 4. The

Risk Reserve

Initial Reserve

Slope = premium rate π

Claim amount

"Ruin"

t_*

time

Claim event
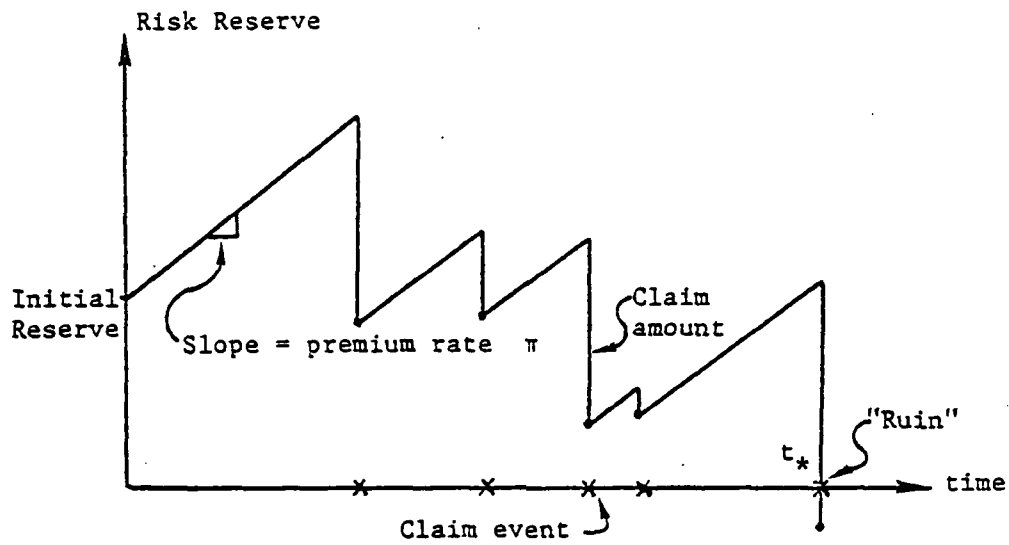
FIGURE 4

The Dynamic Risk Process Paradigm

Virtual Waiting Time

Initial Wait

Slope = -1

Service time of new arrival

End of "busy period"

time

Arrival event
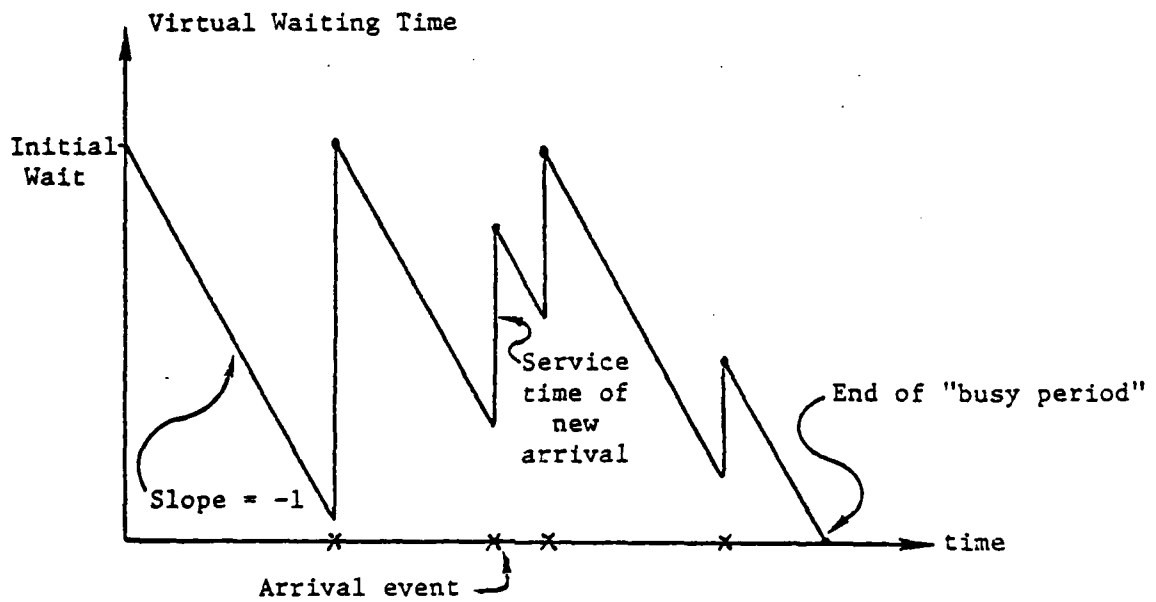
FIGURE 5

Virtual Waiting Time Process in a Single-Server Queue

steadily increasing value due to the net premium income is interrupted at random epochs by random-sized drops due to losses (benefits) paid out to the policyholders. Although this process now fluctuates around R_0 , if balanced, it clearly is possible that, at some random point $\tilde{t}_*$, a large loss when the portfolio is in an already precarious position will cause $\tilde{R}(\tilde{t}_*) < 0$, a condition known as <u>technical</u> <u>insolvency</u>, or to use the more graphic term, <u>ruin</u>, in the portfolio, or in the company. In its classical form, this model does not examine the cost to the company of providing R_0 , nor the economic consequences of ruin, but concentrates simply on the trade-off between stability and ruin, that is, on the effect of R_0 on the <u>probability</u> <u>of</u> <u>ruin</u>, $P_*(t) = \Pr (\tilde{R} < 0$ for some $0 \le t_* \le t)$, or on the <u>mean</u> <u>time</u> <u>to</u> <u>ruin</u>, $E\{\tilde{t}_*\}$, as $t \to \infty$, given that ruin occurs.

This important model was pioneered in the early part of this century by Filip Lundberg, and then developed by Cramér, Segerdahl, Esscher, Ammeter, Philipson, Sparre-Anderson, and many other famous names in a principally all-Scandinavian school of actuaries and academics. It is impossible to provide an overview of this voluminous field in this short space, but there are numerous surveys starting with Cramér's in 1955 [C5], and the papers which appeared in the two symposia which bear Lundberg's name [F6, F7] not to mention actuarial texts [B7, B37, S6], and monographs [B8, S10].

An examination of the literature will reveal that serious study of this paradigm continues unabated. For example, even though the basic theory and methods needed to compute $P_*(t)$ and other measures were known very early, analytical computation is

difficult enough, and has so many intriguing variations and insights, that it has continued to attract actuarial researchers. See, among others, [80.31, 76.12, 76.18, D14, F7, T2, T8]. [B23, S9] mention elementary simulations, and there are no doubt many investigations which have preferred to follow this route for specific empirical assumptions.

Stochastic Evolution

But the dynamic risk process paradigm has been of value to scientists for more than just the analysis of performance measures; this extraordinary activity has also given the basic theory and applications of stochastic processes an important impetus. Indeed, it would not be an exaggeration to say that many of today's standard topics in stochastic processes -- the models of random walks, stationary point and increment processes, renewal processes, Poisson and birth-and-death processes, diffusion processes, martingales, etc. -- would be much less fully developed if Scandinavian actuaries had not been concerned with stability and ruin. For some recent stochastic model generalizations, see [80.8, 80.22, B8, B9, B10, B11, B12, D12, D15, T14, V2].

I see this especially in my own specialty of operations research, where this influence has been felt in a most circuitous way. We know, for example that the early work on telephone traffic problems by A. Erlang was related to and influenced by the early work on risk theory, and that this then lead to the earliest developments of mass service systems -- what we now call queuing theory. For example, Figure 5 shows the virtual waiting process, $\tilde{w}(t)$, in a single-server queue, defined as that time which a hypothetical

customer, just joining the queue at time t , would have to wait until he reached the front of the queue, and began service. As time progresses, the current customer in service is completing his service at a uniform rate of unity, so the residual waiting time of the virtual customer is also decreasing; but then, a new (real) customer joins the queue at some random instant, and adds his random service time to the waiting time of the (bumped) virtual customer. Clearly, this stochastic process is just the mirror image of the risk reserve process, and often has similar underlying assumptions.

In fact, when I explain the risk reserve process to my students in operations research, one of the first things they usually say is that, since there have been so many powerful advances in queuing theory in the last 20 years (many of which they have studied), obviously they can be of tremendous assistance to actuaries faced with a reserve-setting problem! Which only indicates the universality of a good scientific paradigm, and the importance of good communications. I have looked in vain for a comprehensive survey of this insurance-telephone traffic-queuing theory development; perhaps some of you may know of references in addition to [H19, S7, T2].

Dividends versus Ruin and Other Generalizations

Returning to insurance applications, there have been many generalizations of the model to make it more realistic. For example, it was quite early realized that if the risk reserves get too high, the company should declare dividends to its stock-holders, or if ruin is imminent, it should take some corrective

action, such as obtaining new financing; this then leads to a
random walk between two barriers, and various "band strategy"
decision problems. See, e.g. [80.31, 76.8, B37, G4, P3].

Other typical generalizations include a discounted ruin
probability [A4], compounding assets [E5], the use of credibility
theory [D21], and behavior under inflationary conditions [T9].
There is also an interesting body of literature which takes the
most-used approximation forms from the analytic theory, and uses
these forms to suggest general rules for reserve estimation and
regulation [A5, A6, B25, D14, W5].

In reviewing the many contributions to stochastic risk
processes, I have been struck at how closely its development
parallels the normal science concept proposed by Kuhn. In my
opinion, the theory has now far outstripped the actual applications,
in insurance and elsewhere, and is proceeding as an active area of
theoretical activity merely because of the attractive difficulty
and beauty of the model -- in short, it has become a puzzle-solving
activity. In fact, the basic model has apparently not been cali-
brated with real-life experiences; to quote John Wooddy [L2]:

> "...I should like to see more cases where a problem is
> seeking a theory and fewer where a theory is seeking a
> problem. I had an interesting conversation at the recent
> ASTIN Colloquium with Harald Bohman, who has written exten-
> sively on ruin theory. He asked me what the causes of
> actual insurance company insolvencies have been. In the
> United States in the past ten to fifteen years we have had
> a significant number of failures of both life and non-life
> companies. We thus have a large amount of data for studies
> of, say, causes of insurance company insolvency, or stages
> along the road to failure. Here is a prime subject for
> actuarial research whose results would be of intense
> interest to many people."

I do not intend to be critical of the pioneers in this field --

we have already seen the singular importance of their work to actuarial science and to other scientific communities. Furthermore, it is an intrinsically interesting paradigm that will continue to attract attention and generate communications.

> "Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or how quite fascinating such work can prove in the execution" [K7, p.24].

I believe this does mean, however, that for further practical advances in the management of the insurance enterprise, we shall have to look into other models and methods.

INSURANCE OPERATIONS PARADIGMS

Premium Setting

We turn now to the various functional areas of the insurance
company where the skills of the actuary and his basic models are
employed. First and foremost is in underwriting, in setting premiums.

Assuming an adequate data base, then the first step is to
calculate the fair (net, pure) premium which is just the expected
value of the loss under the contract exposure, using one of the
models previously discussed. But then, especially in casualty
applications where the risk is immediate and the variance is
high, comes the idea of adding a security (fluctuation) loading
to the fair premium, which was originally motivated by the
concepts of dynamic risk reserves and protection against stability
just discussed. Three simple ideas come immediately to mind:

   (1) One can simply gross up the fair premium;

   (2) If the risk distribution is approximately normal, then
        we can protect against exceedance of a given loss with
        a certain probability by adding a risk loading propor-
        tional to the standard deviation of random outcome;

   (3) If we require that the premium be additive, in the
        sense that the loaded premium for the coverage of two
        independent risks shall be the same as the sum of the
        individually loaded premiums, then (1) is still appli-
        cable, but (2) is not -- however, we may use a loading
        proportional to the variance of the risk.

This means that the fluctuation-loaded premium Π for a risk $\tilde{x}$
might have the form

$$\Pi = (1 + a)E\tilde{x} + b\sqrt{V\tilde{x}} + cV\tilde{x}$$

with a , b , c constants chosen from stability considerations

[76.6, B17, B20, B37, S6]. The question of desirable premium
calculation principles is still under active discussion, and a
variety of other proposals have been made, such as the use of
the semi-variance [B21] and concepts from capital market theory
[K1]. We shall examine the application of utility theory ideas
in a later Section.

The next step in premium setting is to determine the additional
50-200% increase which determines the commercial premium by adding
expense and profit loadings. Except in life insurance, where
there are specific cost models for sales commissions (in many
cases of regulated form), there seems to be no further modelling
principle used, except the adjustment of the factor a above, or
grossing-up of ∏ itself. This lacuna in the literature is all
the more surprising, as it is in sharp contrast to the fields of
engineering and business management, where extensive and sophisti-
cated cost allocation and modelling are the order of the day.
Are these activities outside of the realm of the actuary, or are
they considered extremely Company Confidential? Perhaps someone
can enlighten me.

Tariff Construction

I am using tariff construction to mean that underwriting
activity in which the entire structure of rates between related
but not identical risk contracts is classified, compared, and
rationalized by further adjustments -- for example, in setting
the physical damage portion of automobile insurance by comparison
across the class of all different vehicles covered, including
their special characteristics such as cost, cost of repair, size,

38

horsepower, etc., plus characteristics of the driver(s) and the
use to which the vehicle is put, where it is garaged, and so on.
It seems to me that this structuring away from purely individual
risk setting is done for two reasons:

   (1)   To provide a smoothness or functional form of premium
         variation across the variation of the risk character-
         istics which approximates some desirable statistical
         or physical law, giving the structure a robust, rational,
         and defensible form; or

   (2)   For competitive reasons in comparison against another
         company's or the industry-average tariff structure.

It should also be admitted that for many lines of insurance, there
are an extremely large number of different rates to be determined
by many small companies who cannot afford the continuing service
of an actuary, and that the underlying tariff structure is essen-
tially furnished by an industry-wide rating bureau.

   The first problem in tariff construction is to select the
risk factors on which the structure will be based. Where these
variables are not given by law, tradition, or public policy, this
search can be rather complex and "artistic", limited only by the
imagination of the actuary (and the company's sales force).
Usually, various proposals are made, and then examined by statis-
tical regression or cluster analysis methods within the preferred
model structure which is usually an additive or multiplicative
model with a certain number of free parameters. The statistical
parameter estimation method chosen provides the mechanism for
ranking the influence of the various factors and for suggesting
which ones should be dropped, although there are usually various
technical problems relating to the independence of the factors
and the validity of the model form chosen [80.43, 80.46, 76.2,

76.3, A2, H6, K2, L8, P10]. This structure is then presented to management, the regulating agencies, and the general public, and further iterations are made as necessary to develop an acceptable risk classification and tariff structure.

Speaking as an engineer-physicist, I must say that I am not very satisfied with these blind, statistically-based, "try-it-and-see" procedures. For one thing, there are few general principles involved [R3, S4] to help guide this search, and to give insight into the business management trade-offs between factors included, and factors dropped. For another, there is usually very little physical or economic motivation behind the choice of an additive or multiplicative model, except possibly the reduction of costs through administrative simplicity, or for ease in statistical estimation. And, there is always the problem of risk factors that are overlooked [A3].

In a few lines of casualty insurance, there are basic physical laws which seem to be guiding tariff construction. In fire insurance, there is not only experimental knowledge about the inflammability of the structure and its contents, there are certain physical dimensions and relationships of volumes, together with experience in using protective devices and sprinkler systems and information about fire-department response times, which enable decomposition of the problem into the probability of ignition, the rate of spreading and "contagion", the ultimate damage potential, and the observed degree of damage [B13, B15, J20, J21, R2, S15, S16]. There is also an obvious motivation for using the theory of extreme values in examining fire losses, and other catastrophic situations, such as earthquakes and floods [80.30, 76.15]. In my opinion,

actuaries could benefit from working more with engineers and
physical scientists who have a lot to say about the physical laws
and risk factor relationships which hold for fire, collisions,
explosions, earthslides and-earthquakes, floods, windstorms and
hail, contamination and pollution, radioactivity effects and
disperal, occupational health and safety, rehabilitation manage-
ment, and so forth. Every large insurance company has specialists
in these areas, of course, but they are busy with contract and
claims administration matters, rather than influencing tariff
construction. This is rather like driving an automobile by
looking out the rear-view window only.

Also, none of these approaches provides the economic and
competitive rationale for moving income and profitability between
different risk classifications for the sake of structural consis-
tency. In [J7], I made a modest proposal that perhaps the under-
writing specialists could express their preferences through simple
inequality relationships between the classes -- e.g., that the
premium should be a non-decreasing function of horsepower; however,
this model has sunk without a trace.

In the next-to-last Section, I shall examine the conflict
between previously acceptable risk classification procedures
and recent shifts in public opi   ı and societal objectives.

Underwriting Exposure and Risk Selection

There appear to be few general models which can be used to
analyze underwriting exposure directly; yet, redesign of risk
contracts is badly needed, for example, in product liability [O1].
A few recent works have appeared on the decision problem of offering
or withdrawing a risk contract [80.29, 76.11, G9].

Bonus/Malus and Consumer Behavior

Most insurance underwriting recognizes the possibility of
rate revision for the individual risk, based upon retrospective
experience rating. In a later Section, we will consider the
Bayesian approach to this problem through Credibility Theory.
The focus of this Section will be on Bonus/Malus systems, where
a policyholder is moved from one premium-rating class to another,
based upon his immediate past experience; for example, in auto-
mobile insurance, it is usually only the number of recent claims
which affects the policyholder's transition from one class to
another. These systems are usually posed empirically, and then
analyzed through Markov chain techniques [S6].

It turns out that most automobile bonus/malus systems currently
in use do not seem to be very good risk discriminators, and various
methods have been proposed to analyze and improve their efficiency
[D5, L16, N3]. Interestingly, recent communications have explicitly
recognized a well-known aspect of consumer behavior -- namely,
that if not reporting or under-reporting a claim will affect future
premiums in a way known to the policyholder, then his "hunger
(thirst) for bonus" will cause him to choose that strategy which
is, in total, least costly. Assuming that the rational consumer
will behave this way, then leads to a new series of actuarial
models in which this effect is taken into account [D6, H2, H3, L5,
L7, N2]. This bonus hunger is also recognized in other insurance
settings where there is a consumer choice of deductibles and the
possibility of "anti-selection" exists [A7, S21].

Claim Reporting Delays

An interesting new model has recently appeared, beginning
essentially with a Boleslaw Monic Fund Competition in 1971 [N1],
to help with a long-recognized problem with certain long-duration
claims, namely, the delays in reporting the numbers of total
amounts of the losses -- the so-called IBNR (Incurred But Not
Reported) problem [80.17, B6, D16, K6, S19, T3, T6].

The situation is depicted in the "Run-Off Triangle" in
Figure 6. If we imagine that all reporting is quantized according
to some common accounting period, say, the calendar year, then
in a given Observation Year the vertical axis represents the
interval in which the claim event was incurred -- the Accident
Year -- and the horizontal axis represents the number of periods
intervening -- the Development Years -- since the claim. Obviously,
earlier events have had more chance to "develop" than later ones;
if we place in the corresponding cells the cumulative claim costs
from a given Accident Year, then we can empirically observe the
"run-off", as the figures mount toward their asymptotic totals
with each Development Year. Note that each additional Observation
Year is represented by a diagonal line in this triangle; the
literature contains many of the 23 other ways of representing the
triangle.

As with all useful paradigms, this model was quickly general-
ized and extended to include many features of interest. The basic
modelling problem is to describe how each year's loss components
are generated, compared with previous experience for the same
Accident Year (e.g., row-wise run-off), and what effects, such as

Development Delay, in Years

$$
\begin{array}{c c c c c c c}
 & 0 & 1 & 2 & . & . & 5 \\
0 & C_{00} & C_{01} & C_{02} & . & . & C_{05} \\
1 & C_{10} & C_{11} & C_{12} & . & & \\
2 & C_{20} & C_{21} & C_{22} & & & \\
. & . & & & & & \\
. & . & & & & & \\
5 & C_{50} & & & & &
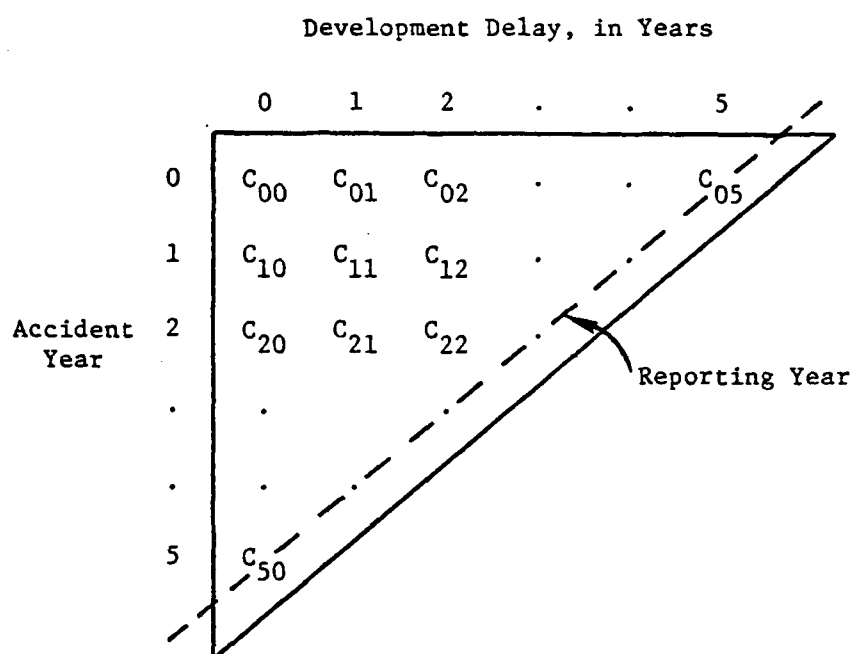\end{array}
$$

Accident Year 2

Reporting Year

FIGURE 6

Run-Off Triangle for Incurred But Not Reported (IBNR) Claims

inflation, may affect losses reported in the same Observation Year
(e.g., diagonal coupling effects). In fact, as Bühlmann, Schnieper,
and Straub [B45] point out, there are two separate delay
effects -- the Incurred But Not Yet Reported (IBNYR) effect due
to initial delays in filing claims with the reporting system [F2, K4],
and the Incurred But Not Fully Reported (IBNFR) effect, due to the
manner in which claim costs are generated by the loss (as in
worker's compensation and health insurance), but also due to the
delays in the claim processing and the reporting system.

Retention and Reinsurance

If a given portfolio of risks has too much variability relative
to the reserves of a company, then that company will normally enter
into a risk-sharing agreement with another carrier, or obtain re-
insurance; we will examine the optimal forms of such contracts
under utility theory assumptions in a later Section.

But relatively early models (see e.g. [B7, S6]) revealed that,
under reasonable assumptions, a company should prefer to "lay off"
the tail $\tilde{x} > M$ of its risk $\tilde{x}$, and retain only the reduced risk
$\tilde{y} = \min(\tilde{x},M)$ -- e.g., it should obtain Stop-Loss reinsurance.
Other authors have studied other objectives and forms of retention
(see, e.g. [B16, B18, B19, L4, S20, W1]).

Determining the properties of $\tilde{y}$ as a function of M when
$\tilde{x}$ is a compound risk sum $\tilde{s}$ turns out to be one of those inter-
esting puzzles which continues to occupy actuarial attention [80.15,
B42, C4, G1, G10, G14, H9, T4, V1]. Recent papers have considered
the connection with utility theory (see later) [80.12, 80.23, G14].

Other Operations Areas

At this point there is a lacuna in the actuarial literature, with the next level of models oriented towards planning, management, and investment problems. Except for some simple models of sales office and agent operations, and an interesting paper on loss prevention [S17], there seems to be little activity devoted towards the daily problems of Underwriting Services, Claims, Data Processing, Records Plant, Inspection, Engineering, Medical, Rehabilitation, Legal, District Offices, and all the other operating divisions of a large modern company. Perhaps it is felt these problems are the purview of other specialists, or that insufficient holistic models of the entire firm are available. Certainly we know that the costs of these departments affect corporate profitability, and that the portfolio of risks supervised by the actuary is what entrains these costs. As a systems engineer, I feel instinctively that the actuary should at least be aware of his influence on all aspects of company operations. Here are some questions to think about: What would be the affect on your company's profitability if claim reporting delays were cut in half? What would be the effect if loss prevention activities were doubled? How much would each of these changes cost?

INSURANCE BUSINESS PLANNING AND MANAGEMENT PARADIGMS

Basic Accounting Models

The past decade has seen a great deal of interest in developing analytic and simulation models for the management of the business enterprise. In fact, a simplified model of the firm has been in use for many years, represented by the accounting equation

$$PI = CP + OE + UP \quad ;$$

that is, premium income balances claims paid plus operating expenses plus underwriting profit, which holds over the long run either by adjusting premiums (rate-making), reducing claims (improved underwriting and loss reduction), or reducing expenses (cost-cutting). This insurance "production function" was assumed to be linear over a wide range of business volume, with the loss ratio CP/PI and the expense ratio OE/PI almost universally accepted as stable corporate objectives and measures of performance.

The simple static accounting model needs, of course, to be expanded to include investment income, and the associated dynamic versions include inflation, changes in reserves, run-off profit, etc. Led by the deceptively simple yet elegant ideas of Harald Bohman, there continue to be fresh insights into model office analysis and understanding of management accounting and control problems through these dynamic, deterministic models [80.6, 80.33, 80.37, 80.38, 76.7, B26, B27, B28, E3]. In many cases, these linear accounting relationships can be used to analyze the associated variances, and then reserve levels and profitability standards can be set by specifying a certainty-equivalent protection level;

we might call this the _safety_ _margin_ or _deterministic-plus_ approach
to modelling.

Solvency and Regulation

If one may judge by the number of papers submitted to Topic 1
of this Congress, the question of appropriate modelling for
reserve setting, solvency margins, financial stability, and
external regulation is one of the hot issues in actuarial circles
[80.4, 80.6, 80.7, 80.11, 80.20, 80.24, 80.25, 80.27, 80.32, 80.35].

As I understand it, the problem is one of managing the rela-
tionship between different types of reserves, especially between
the _technical_ _reserves_ (such as the fluctuation reserve, claim
reserves related to IBNR estimation, etc.) that are specified by
traditional actuarial models, and the _legal_ _reserves_ that are
required to guarantee the solvency of the corporation under
changing conditions which are outside the usual paradigms. To
an increasing extent, these latter reserves are being mandated
by regulatory agencies based upon rather simplistic "maximum
probable loss" or "deterministic-plus" concepts.

Inasmuch as this area is still under active development, and
will be the theme of one of the discussion sessions, I will not
comment further at this time. However, I draw your attention to
some recent work on models for the _surveillance_ of solvency in
the insurance industry using discriminant analysis [C2, G17, P9].

Projection and Simulation

Another rapidly developing area in business management
modelling is in the use of computers for projecting operating

results and for the simulation of the overall operation of an
insurance company, including sales and investment performance.
The papers offered on this topic will form another of the dis-
cussion sessions today [80.2, 80.6, 80.10, 80.14, 80.18, 80.19,
80.34, 80.35, 80.39].

Most of these working programs and simulation proposals
seem to be based on the deterministic, linear accounting models
just discussed, with the addition of random-number generators to
represent mortality or other risk mechanisms, and investment and
inflation variability. While it is important to encourage these
proposals at this early stage of their development, I must say
that they are still relatively unsophisticated by comparison with
other business and engineering management simulations. I am
particularly concerned that there has been insufficient modelling
of the sales function, underwriting risk selection dynamics, claims
management and loss reduction activities, and insurance services
cost components, to be able to rely upon a linear production
function and to use past operating ratios for the projection of
future performance. I would hope that many actuaries would be
challenged to develop appropriate sub-models for each of their
company's operations areas, for use in the overall simulation. A
related problem will be the need to develop simulation-support
data-collection schemes, as many of the traditional data bases
and management reporting systems currently in use are based on
the simple accounting paradigm.

Stochastic-Dynamic Models

A more structured and sophisticated approach to corporate

simulation is taken by T. Pentikäinen and his coworkers [80.25, 76.14, B7, P4, P5, P6]. Based upon the Dynamic Risk paradigm, and using only a stochastic simulation of total claims and an empirical sales response model, their approach permits the rapid exploration of the joint effect of a number of decision-variables, such as reserves, dividends, and sales effort upon the evolution of corporate performance. This approach is an appealing one to me, because it is "top down" decision-oriented modelling, rather than an effort to create a comprehensive simulation "ground up", and to let it run in an unstructured way. Apparently, experience with the simulations has permitted the authors to make certain simplifications in the model structure by appeal to known analytic results, and by using dynamic programming control methods (see also [76.8, F10]).

The counterargument to this approach is that it is relatively sophisticated in concept, and more difficult to explain to managers than a financial-report-oriented simulation. In fact, very few insurance modellers have reported on their successes and failures in selling their results to their management. Is this because most of the work to date has not been supportive of actual business decisions, or is it because the practical details of successful applications do not make interesting communications? An early paper [M4] suggests that management gaming may be an effective communications device, but I have not seen any of these management exercises reported in the insurance field either.

I would also like to call your attention to [76.16], which has some interesting criticisms of the ultimate applicability of purely mathematical methods to insurance management. The study

of how management objectives change in the face of market forces is of course an interesting topic in itself, but about which there has been little discussion. In fact, the important management function of market development and sales strategy is greatly underrepresented in the insurance literature, by comparison with other management science applications. Is this because actuaries do not participate in these decisions, or are these activites considered to be too sensitive for general communication?

We turn now to concepts and methods from other disciplines which have influenced insurance modelling.

THE INFLUENCE OF COMPUTERS

The advent of the modern high-speed digital computer has revolutionized every scientific field, not only eliminating old computational burdens, but obsoleting traditional procedures, greatly extending the sophistication of solvable models, and affecting future attitudes and progress in ways which can only be dimly perceived.

Two examples from actuarial science come to mind. The late David Halmstad was a strong proponent of a high-level computing language called APL, and delighted in pointing out its simplicity and power in computing actuarial functions. As an example, suppose that I is a scalar variable representing the annual interest rate in %, and Q is an arbitrary-length vector variable representing the probability of death at the end of year 0,1,2,.... Then the following two lines of program:

[1] $S \leftarrow \phi + \backslash \phi N \leftarrow \phi + \backslash \phi D \leftarrow Q \times (1 + 0.01 \times I) \star -0, \iota^- 1 + \rho Q$

[2] $R \leftarrow \phi + \backslash \phi M \leftarrow \phi + \backslash \phi C \leftarrow (D \div 1 + 0.01 \times I) - 1 \downarrow D, 0$

will calculate the complete commutation functions D, N, S, C, M, R for this arbitrary mortality table and interest in about 10 seconds on my desk-top computer, about the same time it takes me to open a text and find one value for a given table and interest!

The other example involves calculating the first two moments of the one-year loss distribution in a large life insurance portfolio. While the mean is straightforward, the presence of a large number of different face values and mortality rates makes the calculation of the variance untidy; actuaries have for many

years used an approximation formula based upon grouping of the data. Two years ago, Hans Bühlmann and I talked with an actuary in San Francisco who was going to carry out this calculation on his company's computer, and discovered that he had a few milliseconds of idle central processor unit time while reading the policy master file. So he wrote a small program which "for free" calculated the exact distribution of losses for the entire portfolio through convolution!

Let me pose the following scenario, which I do not think is at all unreasonable; imagine that, by 1985:

(1) All university graduates will be quite sophisticated in the programming and use of computers, including scientific computations, such as large-scale optimization and stochastic simulations, as well as basic data management principles;

(2) The economics of digital computation will have continued to drop to such a point that any of the calculations or simulations that have been proposed at this Congress can be routinely accomplished in a few seconds -- and further, that hand-held calculators or local minicomputers will be able to compute all of the life and many of the non-life actuarial functions instantaneously upon demand;

(3) That extensive computer communications networks will enable the global transmission (via satellite) of insurance experience data banks, economic variables, industrial and trade statistics, etc., at nominal cost.

My questions to you are: How should the actuarial student then be trained? And what will you have him do when he joins your company?

THE INFLUENCE OF ECONOMICS

The Utility Theory Paradigm

Insurance is, we hope, an economic enterprise; and yet the
two fields had surprisingly little to say to each other until
the expected utility hypothesis, first proposed by Daniel Bernoulli
in 1732, was given adequate justification by J. von Neumann and
O. Morgenstern in 1947 (see [B29]); in this important work, they
gave a set of behavioristic assumptions which showed how a
"rational economic man" (REM) would consistently choose between
any two random outcomes with known distributions, say, between
$\tilde{x}$ and $\tilde{y}$. Given these assumptions, together with some technical
fine points, it follows that a REM would behave as if he evaluated
the outcomes by using a personal utility function, $u(.)$, mapping
the outcomes and their associated probabilities into a single
scalar value used for comparison. In other words, (if large values
of outcome are desirable) an REM would consistently prefer a
random outcome $\tilde{x}$ to a random outcome $\tilde{y}$ if and only if

$$U_x = Eu(\tilde{x}) > Eu(\tilde{y}) = U_y ,$$

for some nondecreasing function $u(.)$. Because this paradigm
only purports to rank outcomes, the scale of $u(.)$ is undefined;
a utility function $v(x) = au(x) + b$ $(a > 0)$ will give the
same preference. Therefore one cannot simply say that an individual
should have a nonlinear preference for money.

Although there have been many attacks on this paradigm, based
usually upon experiments in which individuals can be tricked into
violating the REM hypothesis, it has still withstood the test of

time reasonably well. Moreover, it is a useful paradigm in insurance, as has been demonstrated repeatedly by Karl Borch [80.47, 76.8, B29, B30, B31, B32, B33, B34] and many others, in a variety of different applications.

Demand for Insurance

The first success of this paradigm was in satisfactorily "explaining" why anyone would buy insurance against a random loss $\tilde{x}$ and be willing to pay more than the "fair" expected value, $E\tilde{x}$. Suppose an individual with basic wealth W is faced with such a loss, but can purchase an insurance policy to cover the loss at cost Π ; essentially, this is a problem in choosing between a random outcome $W - \tilde{x}$ and a deterministic outcome $W - \Pi$. According to the von Neumann-Morgenstern paradigm, a rational economic man would buy the insurance only if the premium were less than the indifference value Π given by the solution of $u(W - \Pi) = Eu(W - \tilde{x})$. It is a relatively simple matter to show that such an indifference value greater than $E\tilde{x}$ exists for all W only if $u''(x) < 0$, that is, if the individual has a concave-downward utility curve, when he is said to be "risk-avoiding". It is also possible to show that if the range of outcomes is not too large, then the fluctuation loading, $\Pi - E\tilde{x}$, an individual is willing to pay to get insurance is proportional to the variance of the loss and the "risk-aversion coefficient" $-u''/u'$, evaluated at the reduced wealth $W - \Pi$ [P11].

The same approach can of course be applied to a (rational) insurance company to set acceptable premium limits over which it will underwrite a given risk, given its current reserves and

portfolio. Luckily, since the risk aversion coefficient of a
company is usually much less than that of an individual, there
is usually room for negotiating a commercially viable actual
premium.

Premium Calculation Principles

The above result on a REM's permissible fluctuation loading
is, of course, reminiscent of our previous discussion of premium
setting, where a loading proportional to variance was justified
on the basis of additivity. Led by the works of H. Gerber, a
variety of recent papers have explored the variety of abstract
properties which one might require of a premium calculation
principle, including the utility theory approach [80.12, 76.10,
F8, G3, G5, G14, G16, L3].

One interesting result is that, if we require that the
utility theory premium Π be _independent_ of the individual's
wealth (or the company's reserves and current portfolio), the
associated utility function must either be proportional to $u(x) = x$
(the _expected_ _value_ _principle_), or $u(x) = c^{-1}[1 - \exp(-cx)]$,
the _exponential_ _utility_ _principle_. In the latter case, the risk
aversion coefficient $-u''/u'$ is just the constant c over all
values of outcomes, and the utility premium is simply $\Pi = c^{-1} \ln[E \exp(cx)]$. This model is now being applied to a variety
of traditional risk problems, and, like all alternative paradigms,
at least forces the scientist to rethink his basic assumptions;
no doubt we shall see more discussion on this point.

Risk-Sharing and Game Theory

The utility theory paradigm also gives a fresh viewpoint on the problem of risk retention and optimal reinsurance and risk-sharing arrangements. The basic results are due to Borch [B31]. Suppose that we have a group of n insurance companies, indexed by $i=1,2,\ldots n$, each of which is facing a random outcom $\tilde{x}_i$, and behaves as if it has a risk-avoiding utility function $u_i(.)$. The question is: under what circumstances can they agree to form a risk-exchange (REX) or risk pool, in which company i will now assume the random risk $\tilde{y}_i = y_i(\tilde{x}_1, \tilde{x}_2, \ldots \tilde{x}_n)$? We will, of course, require that the treaty (contractual agreement) functions $\{y_i(.)\}$ be such that all claims are paid, i.e. $\tilde{x}_1 + \tilde{x}_2 + \ldots + \tilde{x}_n = \tilde{y}_1 + \tilde{y}_2 + \ldots + \tilde{y}_n$. (Reinsurance arrangements are a variation of this model in which conservative side payments are permitted.)

Two interesting results obtain. The first is that, if there is any treaty which improves the expected utility of all companies, then there are many such treaties, defining a Pareto-optimal set of arrangements over which the companies must bargain for individual advantage. On the other hand, the treaties depend only upon the sum of the pooled outcomes, $\tilde{x}. = \tilde{x}_1 + \tilde{x}_2 + \ldots + \tilde{x}_n$, even if the outcomes are statistically dependent.

Even though the exact REX is not specified by this model, the form of the $y_i(x.)$ are given in terms of the individual $u_i(.)$; for example, if all utilities are exponential, then linear (quota) risk-sharing takes place, so that $\tilde{y}_i = a_i\tilde{x}. + b_i$ with the a_i related to the individual risk-aversion coefficient. The indeterminacy in Borch's result is reflected in the fact that

the side payments b_i $(\Sigma b_i = 0)$ are still open to negotiation, and must be determined in terms of some other model, for example, by reference to the theory of games. A variety of papers have explored the various implications of this Pareto-optimal solution (see, e.g. [76.13, 76.19, B31, B32, B43, L6, M6, P12, R5]).

Gerber [G12] was the first to add inequality side conditions to the REX model, which limits the possibility that the treaty will permit the invasion of reserves of an individual company. This modification leads to the optimality (under the utility paradigm) of the stop-loss contract for simple reinsurance, and gives linear quota-sharing-by-layers for the general REX treaties under exponential utilities; the game-playing arbitrariness is now reflected in the unspecified layer values. Recently, the author and Hans Bühlmann have attempted to remove this element of negotiation by invoking an additional principle, namely, that the REX also be a _fair_ Pareto-optimal risk exchange, that is, the treaties must satisfy $E\tilde{y}_i = E\tilde{x}_i$. This mixture of the utility theory paradigm and a traditional insurance business concept is somewhat confusing at first glance, but it does lead to _unique_ risk sharing treaties, for example the unique determination of the layers in the exponential cases. Unfortunately, if the participants are grossly mismatched in risk-capacity and risk-aversion, then this unique solution may not improve the expected utility of all participants, and the pool will not form; further details may be found in [B44].

The fact that this new model leads to justification of some of the reinsurance and risk pool treaty forms actually used in practice has generated considerable interest, and there will no

doubt be continued development of these ideas [B5, B41].

Notice also that there is a considerable economic literature developing in the area of agent/principal risk-sharing agreements, with emphasis on incentive fee structures, and problems of partial observability (see, e.g. [H18, S14]).

THE INFLUENCE OF OTHER DISCIPLINES

A number of other disciplines have contributed concepts, models, and methods to insurance. For example, I am pleased to see that many papers now reference my own field of operations research, and that many national societies include at least some exposure to O.R. in their training recommendations. However, many actuaries and most of the recent articles seem to equate O.R. with a collection of analytic methods [W6], such as mathematical programming (optimization) [80.16, 80.28, J7, S3, S5], dynamic programming [P6], linear systems [80.16, G15], queuing analysis, reliability theory, decision analysis [S16], and so on. This is not at all what I had in mind when I surveyed O.R. applications in the insurance industry in 1972 [J2] and tried to stress the model-building opportunities in the operations and management areas of the insurance enterprise. Even though operations research/management science societies continue to sponsor sessions on insurance models, they too seem to be mostly methodology-oriented, rather than demonstrating the constructive interaction I had hoped for between the two communities. Perhaps operations research has itself gone too far in its own pursuit of normal science [J18].

Contributions to this Topic also reveal concepts from other disciplines such as information theory [80.5], systems and cybernetics theory [80.3, 80.40], control theory [80.36], and futurism [80.21]. I believe that it is important to keep these dialogues open with other fields, for one is never able to predict where the next successful paradigm will be generated.

Of course, probability and statistics has been a continuing source of techniques and ideas for actuarial science. Because this area is the subject of Topic 2, I will confine my remarks to modelling issues in Bayesian statistics.

MODELS FROM BAYESIAN STATISTICS

A Controversial Paradigm

I would like now to discuss a statistical methodology which
has proven to be an especially rich source of actuarial models
over the past 20 years--I refer of course to the Bayesian revolu-
tion which was foreseen by de Finetti in 1957 [D3], and developed
by L. J. Savage [S1], D. V. Lindley [L11] and many others in-
terested in applications of statistics.

It would take several days of lecturing to cover the philo-
sophical implications of the Bayesian approach and to contrast
it with traditional methods; there are many advocates who are
much more qualified for this task than I--see, e.g. [B1, B3,
B4, C3, E2, E4, L11, L13, L14, S1]. The "controversial" nature
of the Bayesian approach seems, in my opinion, to be related to
a personal worldview of many professional statisticians, namely
that, as specialist consultants, they are not a priori permitted
to have any opinion about the real problem at hand, but "the
data must speak for itself." This attitude leads to a variety
of ingenious theoretical constructions, which unfortunately can
in many cases be shown to have very poor conditional properties
or exhibit a lack of coherence to the laws of probability [B4,
L11, R4].

Fortunately, the issue of whether the analyst has any prior
or collateral information about the problem at hand is hardly
a difficulty for the actuary or the engineer. As Norberg points
out (emphasis added):

"... in class-rating situations the actuary must to some extent rely on subjective judgement since the rate-making decision is forced upon him right here and now and cannot be deferred or put off. If he wants to remain in business, he should not tell the client that, 'Your new gas-tanker is, of course, a most interesting object of insurance, and we look forward to negotiate the terms as soon as the hazard can be assessed from objective facts, say in 10 years or so.'" [N4]

This compulsion to a decision is so embedded in historical insurance underwriting, it seems difficult to imagine a philosophical discussion in London coffeehouses about whether or not to permit subjective judgements. Of course, coherent actuaries—ones who agree to use the laws of probability—may disagree on the probabilities to be associated with certain unknown random quantities, since they do not share the same training and experience in the real world, i.e., their current information states differ.

But the advantage of the Bayesian paradigm is that it provides a mechanism for the orderly sharing and rationalization of this information, both when making the initial underwriting decision, and later, as experimental facts are accumulated. And this is the point I would like to emphasize [L14]:

"The Bayesian approach to statistics is a complete, logical framework for the discussion and solution of problems of inference and noncompetitive decision-making."

Thus, in addition to providing a methodology for estimation, prediction and decision-making [A1], as in Subject 2 of this Congress, it also helps the model-building process, since the scientist is forced to make explicit all of his underlying

assumptions about the influences of one random quantity upon
another, especially which quantities are conditionally independent
of each other, and what is his relative uncertainty about as-
yet-unobserved quantities (his current information state). This
"warts and all" specification of the model is somewhat embarass-
ing when first attempted, since it prevents the subterfuge, tacit
assumptions, and "ad-hocery" which sometimes characterize tradi-
tional estimation methods.

And, as suggested above, the requirement for complete speci-
fication permits scientists who dissent on underlying probabilities
or on the exact form of a model relationship to communicate in
an orderly manner about their differences, to explore the conse-
quences of their differences, and to rationalize these differences
or change their minds, in the face of experimental data. At any
point in the analysis they are free to make approximations or use
appealing empirical methods, and it will be apparent to all
exactly what has or has not been "swept under the rug."

I would like to try and illustrate some of these points with
reference to traditional insurance models, approached from the
Bayesian point of view.

Life Table Analysis under Competing Risks

Consider the classical problem of estimating mortality rates
in a life table with two decrements, death and withdrawal. For
simplicity, consider the single age interval $(0,1]$ in which we
observe that $\tilde{N} = N$ "starters" at time $t = 0$ have resulted in
$\tilde{D} = D$ deaths in service, $\tilde{W} = W$ withdrawals while alive, giving

then $\tilde{E} = E = N - D - W$ "enders" at $t = 1$. Making the usual modelling assumption that mortality and withdrawal are <u>independent competing risk processes</u>, with the (continuous) age cumulative distribution functions:

Pr {age at death $\leq$ t} = $P_D(t)$; Pr {age at withdrawal $\leq$ t} = $P_W(t)$

defined over t in $(0, \omega]$, the problem is to estimate the <u>absolute rates of decrement</u> for this interval,

$$q_D = P_D(1) \; ; \; q_W = P_W(1) \; ,$$

from the data (N, D, W, E) . The estimate of q_D should, of course, be greater than D/N because the observed death data did not include those in W who died after withdrawal.

The traditional approach to this problem is to make some additional special assumptions about the forms of the cdfs $P_D(\cdot)$, $P_W(\cdot)$ (or their associated failure rates); for example, if the decrements are assumed to be uniformly distributed over $(0, 1]$, we obtain the familiar:

$$\hat{q}_D = D/(N - \tfrac{1}{2}W) \; ; \; \hat{q}_W = W/(N - \tfrac{1}{2}D) \; .$$

A difficulty with these estimators is that they are inconsistent, in that they need not approach the true absolute rates, as $N \to \infty$.

Lindley [L12] points out that all of the information contained in the data is given by the likelihood:

$$L(\pi_D, \pi_W) = (\pi_D)^D (\pi_W)^W (1 - \pi_D - \pi_W)^{N-D-W} \; ,$$

where π_D and π_W are the observed probabilities of death and withdrawal under competing risks, respectively:

$$\pi_D = \int_0^1 [1 - P_W(t)]\,dP_D(t) \ ; \ \pi_W = \int_0^1 [1 - P_D(t)]\,dP_W(t) \ ; \ \pi_D + \pi_W = q_D q_W \ .$$

At first glance, it appears from the likelihood that a Bayesian analysis would now require some prior probabilities on π_D and π_W to proceed. However, since the desired estimators are for q_D and q_W , it makes sense to use the above relationships to eliminate the observed probabilities in favor of the absolute rates of decrement insofar as possible. By defining

$$r = \int_0^1 P_D(x)\,dP_W(x)/q_D q_W \ ,$$

the likelihood can be rearranged into:

$$L(q_D, q_W, r) = (q_D)^D (1 - q_D)^E (q_W)^W (1 - q_W)^E (1 - rq_D)^W (1 - (1 - r)q_W)^D \ .$$

The additional "nuisance parameter," r , indicates explicitly what additional (dependent) information is contributed to the model by $P_D(\cdot)$ and $P_W(\cdot)$; or stated another way, the complete forms of the age cdfs are irrelevant, and only the three parameters r , q_D , and q_W , should enter into the estimation process.

Notice that if the Bayesian analyst had well-developed experience regarding these three parameters prior to the experiment, he would now be able to compute a posterior-to-data distribution

for q_D and q_W in the obvious application of Bayes' law, even if he specified that the parameters were dependent.

What about r ? A physical interpretation is somewhat elusive; it is essentially the conditional probability of death "winning out" over withdrawal, given that both death and withdrawal will occur in $(0,1]$. But--and here is the surprising part--it turns out that under many of the usual additional assumptions made in mortality processes, r will have a value at or near 0.5, for example, under constant failure rates or with linear splines used for the cdfs. In other words, after further additional modelling assumptions, r seems to be near-deterministic, in the sense that it has a very "tight" prior around $r_0 = 0.5$, or some other natural value given by valuation calendar assumptions; it is thus hardly relevant whether r is correlated with q_D and q_W , a priori.

Lindley [L12] carries out additional numerical analyses which show that, when N , D , and W are large, most of the information is carried by the likelihood, rather than the prior, and that the mode of the posterior-to-data density of q_D is given by

$$\hat{q}_D = D/(N - (1-r_0)W) \ ,$$

thus validating traditional estimators. But, even more satisfactorily, it is possible to obtain estimates of the variance of the density of q_D , which helps to understand what is a "large" data set. It is also possible to address the question of whether or not this estimator is consistent as $N \to \infty$; it turns out that

it is impossible to eliminate the (admittedly small) original
uncertainty about r , no matter how large the data set.

(The same problem is tackled by classical maximum likelihood
methods in [H14].)

Graduation

If we have only mortality effects (W = 0) , then grouped
data still exhibits significant fluctuation. One possible approach
to the problem of estimating mortality rates is to apply Bayesian
ideas directly in graduating (smoothing) raw observed mortality
rates [J22, H13]. In my opinion, this approach reveals immedi-
ately (through the explicitness of the assumptions required in the
Bayesian methodology) a major modelling difficulty--the graduator
must specify a great deal of prior information about the co-
variance structure of the random parameters associated with each
age interval, assumed to be multinominally distributed. Under
certain additional assumptions, and the use of a clever trans-
formation to obtain an approximately normal likelihood of the
correct conjugate type, [H13] obtains smoothed rate estimates
reminiscent of multidimensional credibility theory [J6, J9].

It should be emphasized that the difficulty here is not in
the use of Bayesian methodology, but rather in the fact that
the King-Whittaker-Henderson free-form graduation and smoothing
techniques are rather too tightly posed; either one believes
specifically in the fit-versus-smoothness objective and accepts
the traditional machinery, or one must make a large number of
supplementary probabilistic assumptions about the way in which

the data roughness is generated. In the latter case, it is clear
that focusing the attention of the modellers upon the dependency
assumptions about roughness in adjacent intervals is extremely
useful in clarifying the model. This structured argumentation
also gives insight into the classical procedures and into other
proposals for automatic graduation (see the discussions in [G15,
H13, S5]). Of course, it does not help much in more artistic
ad hoc approaches.

Mortality Law Models

In many reliability problems [J16] and in the comparison of
certain mortality tables (see, e.g. [80.45, W3]), it may be
possible to make a stronger assumption about the form of mortality
versus age, and leave our uncertainty associated with certain
free parameters. For example, if we assume that the shape of the
failure rate is known as a continuous function of age, except for
a scale parameter, this would give a complementary distribution
function

$$\Pr \{\text{remaining lifetime} > t\} = \exp [-\theta Q(t)] \ ,$$

where θ is the unknown parameter, and $Q(\cdot)$ is the prototype
cumulative hazard function, for example, Gompertz' form. If one
assumes that prior information about values of θ can be expressed
in terms of a Gamma density, this leads to particularly simple
Bayesian updating formulae.

In [J19], the author applies this model to the adaptive
modification of life contingency premium reserves, assuming that

the underwritten cohort consists of lives which have the above lifetime distribution, but in which there is a common value of θ , selected from a larger collective of lives under surveillance by the insurance company. For example, just after underwriting at common age x , the actuary would examine all of his prior knowledge about the variation of θ over previously insured, similar cohorts, and would set an initial reserve at the a priori average value $E\{_oV_x(\tilde\theta)\}$, where $_tV_x(\theta)$ is the deterministic legal reserve at age $t + x$ for the assurance to x , given $\tilde\theta = \theta$. As time passes, expirations in this cohort will occur randomly, changing our posterior estimate of $\tilde\theta$, as more or less expirations than expected occur (this model also has the property that it uses information about the lives still in existence at time t). The correct Bayesian adaptive reserve per contract at time is then $E\{_tV_x(\tilde\theta) \mid \text{Expiration History in } (0,t]\}$, obtained through routine use of the updating formula. This generates a curve which drifts (upward for annuities, downward for assurances) through the family of classical reserve curves when no one dies, then jumps (down or up) at the random instant of death; naturally, with a large number of lives in the cohort, it seeks out and tends to follow the curve corresponding to the correct θ for this cohort. A similar model could of course be useful in other life insurance problems, for example, in the valuation of a portfolio for reinsurance purposes. ([76.3] suggests a credibility approach to group term life insurance.)

There seem to be few other Bayesian models in life assurances; however, it is a natural approach for pension systems studies,

most of which are still dominated by the deterministic-plus
approach. Shapiro studies the adequacy of projected retirement
costs in a pension model in which persistency of participants,
mortality rates and interest factors are random variables whose
parameters are also stochastically distributed [S11, S12, S13].
The projected probability distribution of final accumulation given
could be updated dynamically by Bayesian methods through popula-
tion experience.

Credibility Theory

The most-developed Bayesian model in insurance must be credi-
bility theory; surely you have all heard something about it
already, good or bad. In its simplest form, we imagine a risk
collective, similar to that already described, in which each risk
is characterized both by his outcome random variable, $\tilde{x}$, and by
an unknown risk parameter, θ, which describes his particular
risk variability within the collective; the model for the outcome
is the simple likelihood density $p(x \mid \theta)$ and $u(\theta)$ is the
structure function for the distribution of θ over the collective.

For the selected risk, we cannot observe his risk parameter
θ directly, but we can record his experienced outcomes
$\underline{x} = (x_1, x_2, \ldots, x_n)$ over n years of experience. By a direct
application of Bayes' law, then, it is possible to update our
estimate of the risk's parameter distribution from $u(\theta)$ to
$u(\theta \mid \underline{x})$. In experience-rating applications, the basic problem
is to predict the value of the unknown future outcome $\tilde{x}_{n+1}$,
using both the experience $\underline{x}$ for this risk, together with

information about the collective. This emphasis on future values of observables, rather than on unverifiable values of parameters, is typical of practical applications, but has only recently begun to receive its proper emphasis in Bayesian statistics [A1].

Perhaps the most fascinating part of the credibility story is that the practical importance of this problem was recognized by the American actuaries A. H. Mowbray and A. W. Whitney over 60 years ago, when statistics was in its infancy. They proposed a formula which we would write in the above notation as:

$$\hat{x}_{n+1} = (1 - Z)m + Z\bar{x} \; ; \; \bar{x} = \sum x_t/n \; ; \; Z = n/(n + n_o) \; .$$

$\bar{x}$ is, of course, the average observed experience of this particular risk, and will, as experience accumulates, be a "good" estimator of $\tilde{x}_{n+1}$; however, these pioneers reasoned, with small amounts of experience data, $\bar{x}$ may be a highly variable estimator--why not mix it with the manual premium, m , which is already tabulated for the collective as a whole?

Using heuristic reasoning based upon pooling of data arguments, they argued that the mixing coefficient, Z , which they called the credibility factor, should be of the form indicated above, with the time constant, n_o , chosen from experience.

The next part of the story comes in the 1950's, just at the beginning of the resurgence of interest in the Bayesian approach, when A. L. Bailey [B1], and A. L. Mayerson [M3] showed that the experience rating problem could be posed as the problem

of estimating a posterior-to-data mean, $E\{\tilde{x}_{n+1} \mid \underline{x}\}$, which is already implied in our collective model above. They carried out this calculation for several important likelihoods $p(x \mid \theta)$ and structures $u(\theta)$, and showed that the linear credibility form was <u>exact</u>, even to its dependence on n and the interpretation of the manual premium as $m = EE\{\tilde{x} \mid \tilde{\theta}\}$. The time constant n_o was a function of the (hyper)parameters of the structure function. A good summary of this "American school of partial credibility" can be found in [L18].

The scene now shifts to Switzerland, where Bühlmann showed in 1967 [B36] that, if one choose to <u>approximate</u> $E\{\tilde{x}_{n+1} \mid \underline{x}\}$ by a linear function of the observed data, say $a + b\bar{x}$, then the least-squares estimate was again the familiar credibility form, only now there was an interpretation for n_o as well as m . More precisely, if D and E are the two components of collective variance previously defined, then $n_o = E/D$. This result gave impetus to a number of works from E. Straub and the rest of the "Swiss School."

It also stimulated the author to think in 1973 about various extensions of the basic model, and also to examine the link between the exact linear formulae of Bailey and Mayerson, and the approximate results of Bühlmann. Now, it is known from statistics that if the sample mean is the only sufficient statistic (apart from n) for the parameter θ in a sequence of i.i.d. trials (and if the range of $\bar{x}$ is fixed, and certain technical regularity conditions are met), then the likelihood must be one of the members of the Koopman-Pitman-Darmois <u>exponential</u> <u>family</u>, i.e.,

$$p(x \mid \theta) = \frac{a(x) \exp (-\theta x)}{c(\theta)}$$

over some appropriate range and measure for $\tilde{x}$, making the likelihood of the experience data $\underline{x} = (x_1, x_2, \ldots, x_n)$ equal to

$$L(\theta) = p(\underline{x} \mid \theta) = \frac{\Pi a(x_t) \exp (-\theta n \bar{x})}{[c(\theta)]^n} .$$

This family includes many of the favorite models in insurance, such as the Poisson, the Binomial, the Exponential and the Gamma with fixed shape, the Normal with fixed variance, etc. Now, if we simultaneously assume that the collective structure function (e.g. the Bayesian prior density on θ) is the so-called natural conjugate prior

$$u(\theta) \propto [c(\theta)]^{-n_o} \exp (-\theta x_o) ,$$

(again, over some natural range for $\tilde{\theta}$), then it is extremely easy to form the posterior-to-data density of the structure function for the risk under study, $u(\theta \mid \underline{x})$; essentially, it is of the same form as $u(\theta)$, but with the hyperparameters n_o and x_o replaced by $n_o + n$, and $x_o + \bar{x}$, respectively. This closed-under-sampling property is extremely convenient, and for many of the practically useful likelihoods gives also a convenient prior, such as the Gamma, or Normal [A1, J5]. So far, so good. The bonus comes after making a regularity assumption which is always satisfied in practice, whence we find [J6, J8] that these conjugate families of densities imply that $E\{\tilde{x}_{n+1} \mid \underline{x}\} = (x_o + \bar{x})/(n_o + n)$

exactly! In other words, the hyperparameter n_0 is just the

ratio E/D found by Bühlmann, and the hyperparameter $x_0 = n_0 m$.

So, now we know that the linear credibility rule is exact for a

rather wide class of important distributions, and there are

further indications that it is extremely robust in many other

situations as well.

Well, since that time there has been a virtual explosion

of research into various extensions and mathematical properties

of linearized Bayesian credibility models. I hope that my many

friends in the American-Swiss-Belgian-Australian-Italian-

Portugese-Norwegian-plus School of Credibility will excuse me

if I do not try to enumerate and compare all their various con-

tributions. 1974-1975 saw the first monograph [D8], as well as

the first research conference [K3] on credibility; a 1976 biblio-

graphy [D7] lists 141 items, to which must be added at least

another 30 recent contributions.

Among the types of different models which have been developed,

we might mention:

(i)   the problem of claim frequency and severity

       (see below);

(ii)   the effect of premium volume [B46];

(iii) estimation of extreme values and probabilities

       [B39, D21, F11, J4, P7, S18];

(iv)   minimax estimators [M1, S18];

(v)   other sufficient statistics and "best" estimator

       forms [D10, D17, J6];

(vi)   seasonal and other nonstationary models [G6, J9, K3, S22];

(vii)   hierarchical models [D19, J11, S24, T1, T12, Z4];

(viii)   multidimensional models [J1, J6, J9, J10];

(ix)   conditionally linear models [B46, J9];

(x)   regression and inverse regression models [H1, J12, J13, J14, S24];

(xi)   general, abstract, and "nonparametric" models [H1, L15, S25, T5, Z1, Z2, Z3, Z4]

and many other special topics, such as: the influence of different risk factors [A3]; rate classification [D11, W2]; bonus hunger [N2, W3]; network flows [J15]; reliability [J16, J19], etc. It is difficult to get a perspective on the field at this point; my survey [J17] is already out of date, a more recent one is given by Norberg [N4]. The only obvious trend is that the Scandanavian Actuarial Journal seems to have taken the lead over the ASTIN Bulletin and the Swiss Mitteilungen in publishing articles of this type!

It should also be pointed out that linearized Bayesian formulae are constantly being developed in other fields; for example, in communications theory, similar results arise in Wiener-Kalman filter theory, where the emphasis is on adaptive updating formulae for nonstationary processes. There have also been a number of related articles in statistics, beginning with [E6, H8], see also references in [J17]; unfortunately, these do not often reference or acknowledge priority from credibility, in part,

because of the limited circulation of actuarial journals.   Two
recent statistical works which do reference credibility are
[D20, F1].

The Problem of Severity

The economy and simplicity of Bayesian model-building can
be illustrated by considering the problem of estimating the total
severity of a casualty claim; this problem has previously been
analyzed by Hewitt, Bühlmann, and the author using credibility
theory [B38, H10, J1].   Extending the compound claim process des-
cribed earlier, we suppose that in time period t   (t = 1,2, ..., n)
a random number of claims $\tilde{k}_t$ occur, with random individual claim
amounts $\left[\tilde{x}_{t1}, \tilde{x}_{t2}, \ldots, \tilde{x}_{t\tilde{k}_t}\right]$, whose total, $\tilde{s}_t$, a random sum
of random variables, is the total severity for this time period.
We imagine that the number of claims is governed by an unknown
parameter ϕ , and the claim size by an unknown parameter θ ,
which are fixed for an individual risk, but whose structure dis-
tribution density, $u(\theta, \phi)$ is known over the collective.   As
usual, we assume that, given the parameters, the individual claim
amounts and the number of claims are mutually independent, and
that all the claim amounts are identically distributed.   The prob-
lem is to estimate the total severity next period of this particu-
lar risk, $\tilde{s}_{n+1}$, given the underlying probability laws, and the
observed data $D = \left[k_1, k_2, \ldots, k_n ; x_{11}, x_{12}, \ldots, x_{1k_1} ; \ldots; \right.$
$\left. x_{n1}, x_{n2}, \ldots, x_{nk_n}\right]$.

Suppose we assume that individual claim size, given θ , is
modellable by a density for which the sample mean claim is the

only sufficient statistic and that the claim frequency, given ϕ , is also modellable by a density for which the mean claim rate is the only sufficient statistic; these are strong assumptions, but are satisfied by many of the particular models used in the litera- ture. This means that the two basic model laws for $\tilde{x}$ and $\tilde{k}$ belong to the simple exponential family, say:

$$p(x \mid \theta) = \frac{a(x) \exp (-\theta x)}{c(\theta)} \; ; \; p(k \mid \phi) = \frac{b(k) \exp (-\phi k)}{d(\phi)} \; .$$

(These are _different_ densities; we are using the usual Bayesian trick of letting the arguments describe the form, range, and measure of the (possibly discrete) density, rather than defining new functions.) Now, _only from the assumptions above_, we can show that this implies the likelihood of observing the data $\mathcal{D}$ must be:

$$L(\theta,\psi) = p(\mathcal{D} \mid \theta,\phi) = \left[\prod_{t=1}^{n} a^{k_t *}(s_t) b(k_t) \right] \frac{\exp (-\theta s_{.} - \phi k_{.})}{[c(\theta)]^{k_{.}} [d(\phi)]^{n}}$$

where the asterisk indicates convolution, and

$$k_{.} = \sum k_t \; ; \; s_{.} = \sum s_t = \sum\sum x_{ti} \; ;$$

are the total number of claims and total severity observed over all the time periods.

Clearly the term in square brackets in the data likelihood can be ignored, and it follows that the new data $\mathcal{D}' = [k_{.}, s_{.}, n]$, the total number of claims, the total severity, and the number of time periods, are _sufficient for_ (θ, ϕ) ; in other words, any addi- tional combination or arrangement of the data is noninformative.

Now, if as Bayesians, we were able to completely specify the joint prior density, $u(\theta,\phi)$, then an analytic or numerical application of Bayes' law would update this prior to $u(\theta,\phi \mid \mathcal{D}')$, and this could be used to get a predictive density for $\tilde{s}_{n+1}$, in principle.

But this calculation appears difficult in the general case, and so we look for simplifications. If we consider predicting only the mean severity next period (the experience-rated fair premium), it can be shown that:

$$E\{\tilde{s}_{n+1} \mid \theta,\phi\} = \frac{c'(\theta)d'(\phi + \ln c(\theta))}{c(\theta)d(\phi)} ,$$

and this function might be easier to integrate than the complete predictive density with particular choices for the underlying models.

Notice that we have not yet assumed that the parameters assumed are independent; if they are dependent, this means that observed (mixed) data from the collective will show a correlation between average frequency and average claim size--which is sometimes observed in automobile statistics.

If we are willing to additionally make the assumption that the parameters are independent, $u(\theta,\phi) = u_1(\theta)u_2(\phi)$, say, and that the natural conjugate priors to the basic likelihoods are reasonable choices for u_1 and u_2 , then we get almost immediately that $E\{\tilde{s}_{n+1} \mid \mathcal{D}\}$ is exactly the product of two credibility formulae, one for claim amounts and one for frequency! In fact the independence assumption has often been made in the literature.

If as Bayesians, we feel that these modelling assumptions are too restrictive, we are free to make a credibility approximation to the desired mean, by assuming that it is a linear function of the data. But, from the likelihood, we see that there are only two sample variables of interest, $s_.$ and $k_.$ (or their corresponding sample averages $s_./n$ and $k_./n$). The corresponding 2-dimensional credibility prediction in the correlated case has been carried out in [J1]; [B38] shows the expected result that, in the uncorrelated case, the credibility forecast factors into the product of two independent forecasts for amount and frequency. I freely admit that in using credibility this way we are not obtaining exact results, but only approximations; however, by starting out with a full Bayesian analysis, we at least "know where the cards lie." If the result that only $s_.$ and $k_.$ need be used seems suspicious, we must change the basic likelihoods to more appropriate forms, and not blame dependence of the parameters.

Daboni [76.1] has adopted an interesting approach to the problem of correlation in severity which illustrates the Bayesian approach to model uncertainty through the method of model mixtures [J19, L11, L14]. Essentially, he proposes to use a prior density of the form:

$$v(\theta,\phi) = \pi u_{11}(\theta)u_{21}(\phi) + (1-\pi)u_{12}(\theta)u_{22}(\phi)$$

- ... the initial choice of the mixing coefficient π and the
- ... meters for the four (conjugate) priors, u_{11} and u_{12} ,
- ... are based upon observable means, variances, and

covariances for the collective. The hyperparameters for the
priors are updated in the usual simple fashion, but now there is
a rather complex updating of the mixing coefficient, from π to
a data-dependent $\pi(\mathcal{D})$. This approach is often used when there
is uncertainty about the prior model form. The only philoso-
phically unsatisfactory thing about this particular model is that,
in order to restrict the number of hyperparameters to be estimated,
the shape parameters of the four priors are constrained in a
special way. The paper does show that, as n approaches
infinity, the exact nonlinear-mixed-linear mean forecast does
factor into the product of two independent linear forecasts,
since (aha! now we remember our basic assumption) claim amounts
and frequencies are _independent_, _given_ (θ,ϕ) , and, in the usual
Bayesian way, we may expect, for increasing data sets, to have
the posterior density approach a degenerate density at the true
values, with probability one. Furthermore, there are ways within
the Bayesian paradigm to estimate this convergence!

Prior Estimation and Empiricism

In closing this section on Bayesian modelling, I would like
to respond to Norberg [N4], who implies that I am clearly of
the genuine Bayesian persuasion, even _if I do_ not display the
colours, and wonders whether I would surrender, or use some em-
pirical approach if a prior density $u(\theta)$ were not available.
Well, as a philosopher, I am a good engineer; I find it difficult
to imagine that I would not have _some_ prior opinion, based upon
observation, about almost everything in our real, physical world

of man. But I am also of an empirical rather than abstract bent,
and, heeding Occam's razor, would much prefer a simple model of
known limitations and verifiable dimension, to a more complex,
untested model which may be "more accurate." Thus, when being
empirical, I prefer to make explicit the simplifying assumptions
and computational shortcuts I am taking, in order to be able
to make surprise-free projections. And here, displaying my colours
at last, I would like to discuss what I think is a potential heresy
in implementing credibility theory under certain circumstances:
the problem of justifiable empiricism.

Recall the simplest model for predicting the mean value
$m(\theta) = E\{\tilde{x} \mid \theta\}$ for a particular risk whose parameter θ is
fixed, but unobservable; to use credibility, we need only three
moments from the collective:

$$m = Em(\tilde{\theta}) \; ; \; E = Ev(\tilde{\theta}) \; ; \; D = Vm(\tilde{\theta}) \; ,$$

(in fact, we really only need $n_o = E/D$). The underlying condi-
tional moments $m(\theta)$, $v(\theta)$, are not at issue, since they come
from the model likelihood $p(x \mid \theta)$, whose form is always assumed
to be modellable. But taking the expectation and variance of
these moments to find m, E, D, has bothered many analysts,
since it involves the structure function, $u(\theta)$. They reason
that, as scientists, they would prefer not to have too "personal"
an opinion about this prior, but would like to set to work
immediately, estimating m, E, D from the collective [80.42,
B46, D13, L16, N4, Z3, Z4].

variation of these statistics about the true values, for whatever reasonable range (and distribution?) of θ values he wishes to make explicit, so that I can be scientifically satisfied that both n and M are "large enough." In fact, I expect, from my experience, that "large enough data" implies that all of the unbiased niceties in the above formulae can be eliminated, with $n-1$ and $M-1$ replaced by n and M, respectively, and $\hat{E}/n$ ignored relative to $\hat{D}$. After all, the credibility formula is itself just a point estimate, and is known through experience to be quite robust to choices of m and E/D, in the sense that the data variability noise overwhelms any slight error in the underlying conditional mean, when observing an actual sample path of the credibility statistic. If this variability in credible predictors is of concern, then the modeller should forego credibility theory, and make a full distributional analysis of $\tilde{x}_{s,n+1}$, conditional on X, by whatever methods give him satisfactory results.

The extension of the above heuristic to the case of uneven data record lengths, n_i $(i = 1, 2, \ldots, M)$ is straightforward. Now, to be "confident," we would ask that "almost all" lengths n_i be large in order to use $\hat{m}$, $\hat{E}$, $\hat{D}$ (clearly, for the risk s we are trying to rate, n_s could still be small--in fact, this is region in which the credibility approach is most useful). Incidentally, the classical approach often gets hung up on whether or not it is correct to include $\underline{x}_s$ in the data X used to estimate m, E, D, given that we have already specified a linear form in $\bar{x}_s$ in the remainder of credibility estimator; this seems to me to be a problem in theology!

Suppose that we have data sets of the same record length, n , $\underline{x}_i = [x_{i1}, x_{i2}, \ldots, x_{in}]$ from M members of this collective, indexed by i , each one of which has an unobservable risk parameter θ_i $(i = 1, 2, \ldots, M)$. (Note that our notation is transposed from other authors.) We shall henceforth let index s be the particular risk that we are trying to experience-rate by estimating $E\{m(\tilde{\theta}_s) \mid X\}$, where X is the total collective data file $X = [\underline{x}_i] = [x_{it}]$.

The authors cited above consider the unknown $[\theta_i]$ to be statistically mutually independent, and empirically propose replacing m , E , and D by point estimators of classical form, viz. if

$$\bar{x}_i = \sum_{t=1}^{n} x_{it}/n \; ; \quad \bar{x} = \sum_{i=1}^{M} \bar{x}_i/M \; ;$$

then use:

$$\hat{m} = \bar{x} \; ;$$

$$\hat{E} = \frac{1}{M(n-1)} \sum\sum (x_{it} - \bar{x}_i)^2 \; ;$$

$$\hat{D} + \frac{1}{n}\hat{E} \approx \frac{1}{(M-1)} \sum (\bar{x}_i - \bar{x})^2 \; .$$

As an empiricist, I have no quarrel with this approach, in fact it is probably the one I would use, because I know that these "unbiased, minimum variance" point estimators have certain robust and appealing properties, especially when n and M are both large and the underlying sums are essentially normally distributed. But I would insist that anyone who uses (large) sampling-school results should also be able to bound the possible

The philosophical deviation about which I am most concerned, however, is the implication that estimates like the above, and their generalizations to more complex models, can be used in place of prior moments no matter what the dimension of M or the n_i . The modelling goal seems to be to create a formula in which there is no information needed about $u(\theta)$, only data X (and $\underline{x}_s$). This approach is called empirical Bayes (eB), and the corresponding formula the empirical credibility premium.

The point is further confused by reference to the Empirical-Bayes (EB) school, due to Robbins, Maritz and others, and to the estimator of Stein. Now the EB approach is a schism of a different colour, and I am not enough of a philosopher to either defend it or attack it. Essentially, the EBers would presumably use data X of any size to first estimate a density $\hat{u}(\cdot)$ and then use this in Bayes' law to find $E\{m(\theta) \mid X\}$; but I suppose we might stretch the point and say that an EBer would also estimate m , E , D directly from the data, and use it in a Bayesian formula. I do know that EB computational machinery is complex, and that it can have other serious drawbacks such as incoherence, poor small-sample properties, unknown rate of convergence, or even lack of asymptotic optimality [D2, L11]. I am wary of any help from this quarter.

But, what about the possibility of using the empirically derived eB formula for arbitrary n_i and M--couldn't this also have scme nice, robust properties? Well, yes, I would have to admit--but they haven't been demonstrated yet! In other words,

if you propose to "ad hoc" up a complicated formula involving
both sums, squared sums, and sums of squares of the data by
appeal to two different schools of thought, then I can only be
amazed at your ingenuity. But I suspect that you will have a
difficult time in proving these good properties analytically,
and will have to resort to, say, simulation (that is, to ex-
perience). Note that even Norberg [N4] recommends that, for
small sample sizes, one should use all of the known information
about the particular form of $p(x \mid \theta)$ at hand in setting up
empirical estimators--for example using $\hat{E} = \bar{x}$ if the outcome
is Poisson with parameter θ .

But what, you persist, would I do if I actually did not have
much data from the collective risks either? Fortunately, there
is another approach I could take which would take me back into
the realm of acceptable (to me) empiricism, by referring the
estimation problem to a higher level data source. As Lindley so
often remarks [L14], there are really no completely unconditional
statements in statistics, since our mathematical conversation
can be extended just so far, and there will always remain hypo-
theses, data, beliefs, physical conditions which it is not effi-
cient to include in our model unless our model proves to be
unsatisfactory; contrary to popular belief, even a Bayesian
may change his mind about his model after looking at new data.
In this case, since the Bayesian approach does not use the
collateral data $[x_i \mid i \neq s]$ (because of the independence
assumption on the θ_i), I raise the question as to what tacit
conditions were present when I made this assumption? The answer

is that I assumed that the collective was a relatively homogeneous cohort of customers for the same company. However, I know also from experience that different insurance companies have different collectives with widely different risk characteristics, which I might parametrize by a company parameter, ϕ . Furthermore, I know that there often exist extremely large, albeit highly variable, statewide or nationwide data banks covering many insurance companies. By creating a hierarchical credibility model, I can use not only data from my risk s and my collective, but also the "good" statistics from the collective of collectives. The resulting linear three-part credibility formula mixes $\bar{x}_s$, $\bar{x}$ (including risk s data), and the "manual premium," estimated from nationwide data; also to be estimated by straightforward empirical methods are three components of variance at nationwide, company, and individual levels. Further details may be found in [J11]; a generalization to multiple levels is in [T12]. [D2] generally exorcises the EB spirit, for those of you who are still of uncertain faith.

RISK-CLASSIFICATION: A COMMUNICATIONS BREAKDOWN

In closing this survey of modelling in insurance, I would
like to discuss two recent examples of conflict between the
traditional methods used for risk classification and the goals
of society.

The first issue is whether or not classification by sex
may be legally used in the U.S.A. for defining pension benefits.
The rationale for male-female differentiation is, I am sure,
familiar to all of you. By reference to a life table, such as
the one illustrated in Figure 7, one sees that the mortality
rates are substantially different at all ages, and one calculates,
for example, that:

> "If a male and a female employee reach age 65 with an
> accumulation of, say $100,000 each, and if each elects
> the Single Life Annuity Option, the annuity income is
> $11,450 a year for the man and $10,175 a year for the
> woman" [E1].

In a series of rulings handed down, beginning in 1975, to various
municipal, state, and educational pension systems, various U.S.
District Courts and then our Supreme Court have determined that
sex-classified annuity tables violate Title VII of our Civil Rights
Act of 1964, which makes it unlawful "to discriminate against any
individual with respect to his compensation, terms, conditions or
privileges of employment, because of the individual's race, color,
religion, sex, or national origin." Of course, as specific court
cases, they raised a variety of other issues, such as whether the
mortality tables were representative of the plaintiffs, whether
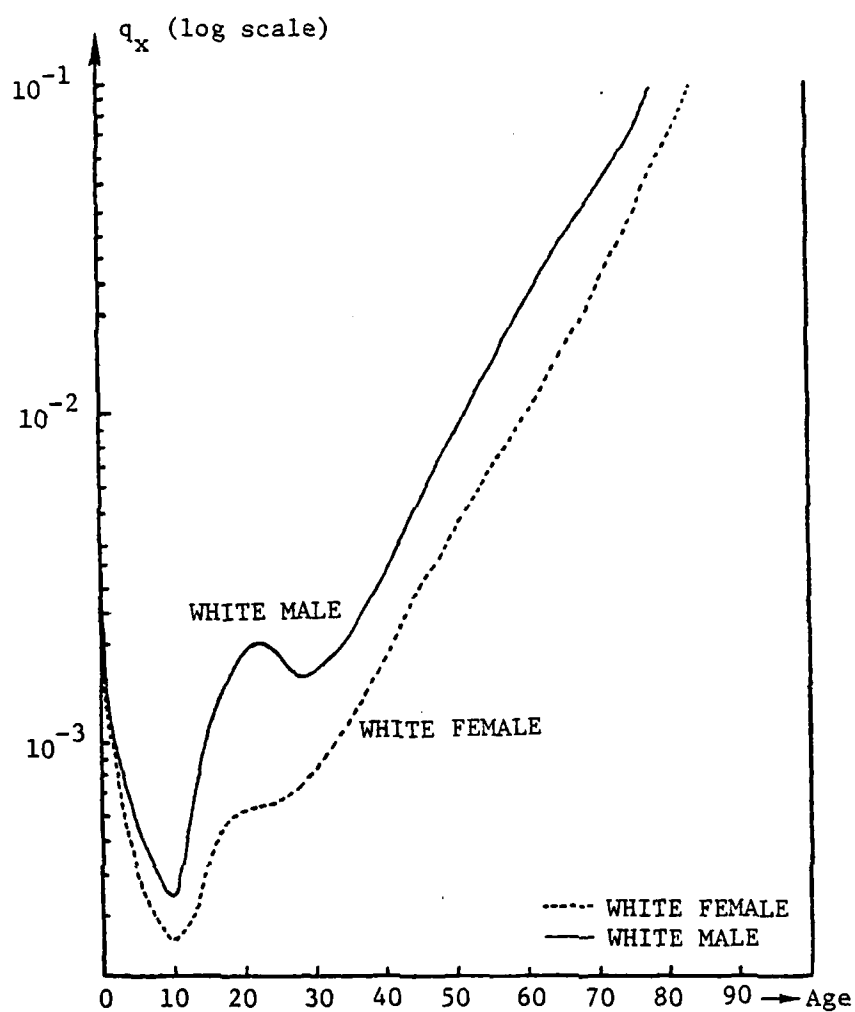graduation and a set-back approximation were equitable, the

FIGURE 7

Mortality Rates – United States Life Tables 1969–71

differences between money purchase and formula benefit plans,
and so on. But the general theme was that collective-based
arguments for "cost-based pricing" must be set aside in favor
of individual considerations of equity:

> "The use of group mortality tables attempts to predict
> only the life expectancy of a group and does not consider
> the individual female. These tables cannot predict the
> life expectancy of any particular individual, regardless
> of sex. Since actuarial tables do not predict the length
> of any individual's life, any claim that such tables may
> be used to assure equal pension benefits to males and
> females over their lifetime, must fail" (quoted in [M2]).

Of course the issue is much broader than retirement annuities,
and includes all employee benefits which might involve discrim-
ination [76.4, E1, H7, K5, L1, M2, ZZ1].

A related social issue concerns classification schemes for
automobile insurance [ZZ2, ZZ3, ZZ4]. As illustrated in Figure 8,
group-average accident rates vary significantly by the age and
the sex of the driver. Although there is no Federal law covering
access to the highways, the freedom to use an automobile has
become almost an inalienable right in the mass-transportation-
limited United States, and the skyrocketing cost of insurance is
viewed as a form of social injustice by many of our working poor,
particularly when ghetto and barrio geographical territory rating
classifications are imposed. The insurance commisioners of
several states have taken a look at this problem, and have come
to rather different conclusions. The basic philosophical issue
is again related to the perceived differences between individuals
in different risk classifications. As shown in Figure 9, there
is a significant overlap between the distributions of losses

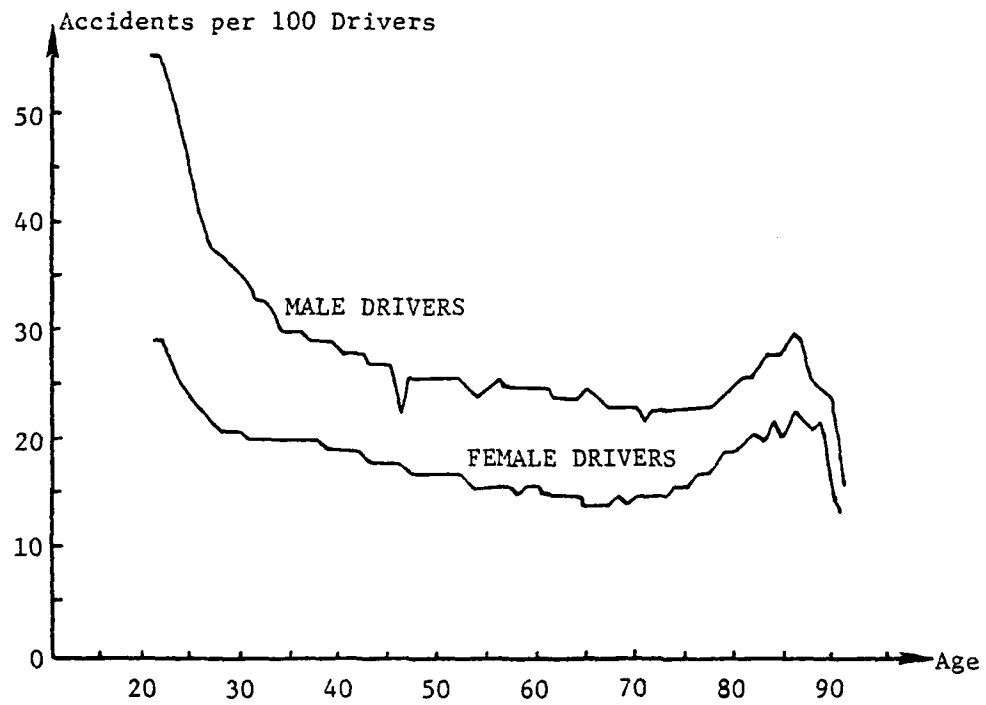Accidents per 100 Drivers

MALE DRIVERS

FEMALE DRIVERS

Age

FIGURE 8

Reported Accident Rate as a Function of Age,
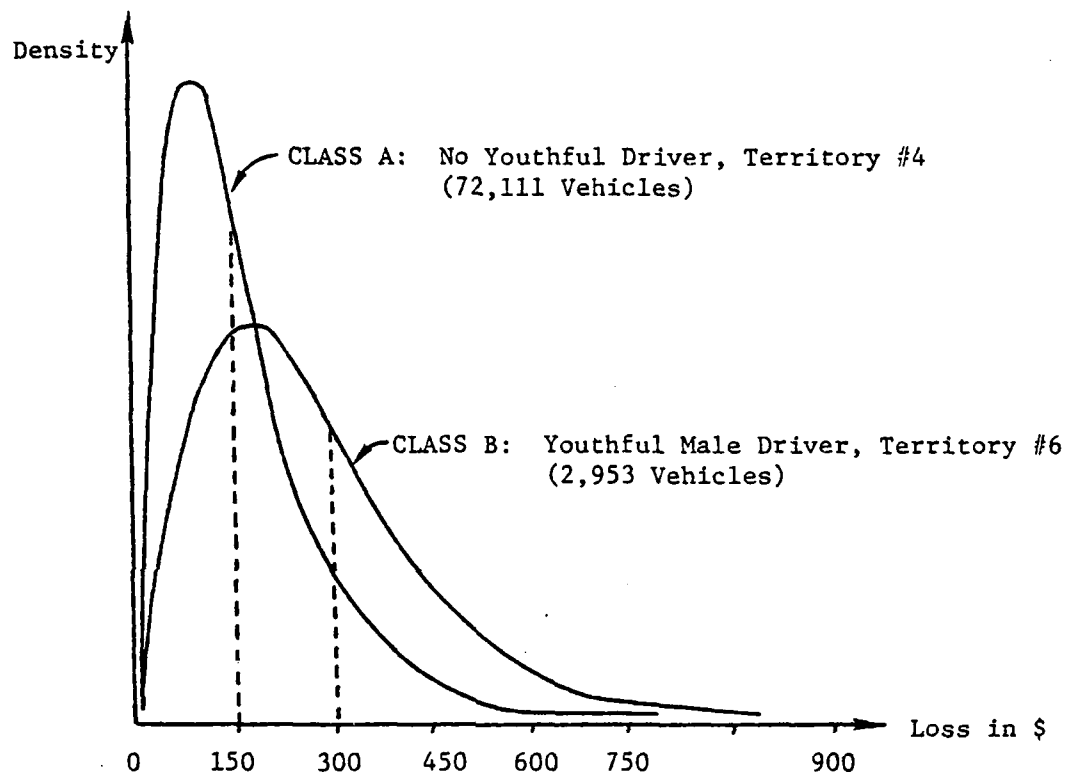California Motor Vehicles Records 1973-8

FIGURE 9

Loss Distribution Densities - Massachusetts Drivers, 1975

between two classification which have significantly different
pure premiums, meaning that a significant fraction of the "worse"
classification will see themselves as better drivers than a
large number of the "better" classification. A related problem
is that the process of classification often produces low-volume,
highly-variable, and heterogeneous high-risk classes, whereas
the low risk classes are much larger and more homogeneous.
J. Ferreira, Jr., has developed an interesting model based on
utility theory which focuses on the individually perceived
inequities in being overcharged; by weighting overcharges more
heavily than undercharges, a certain redistribution of the total
premium takes place which dramatically reduces the perceived
inequities for the high-risk without affecting very much the
more stable classes [F3, F4, L10]. (Of course, individual exper-
ience rating is a possible remedy for high initial automobile
insurance classifications.)

The literature on these problems is fascinating to read,
although the arguments raised on both sides often raise more heat
than light. Insurance professionals, for example, quote medical
studies on the effect of prostaglandins on mortality, comment on
the difficulty of constructing unisex mortality tables, and speak
direly of anti-selection, plan funding instability, unfair benefit
transfers between classes, government interference, and so on.
Lawyers and social advocates point out that current insurance
does not normally discriminate on a racial basis, nor include a
variety of other obvious "causal" factors, and that the principle
that private companies must be responsive to social objectives
has been well established in other areas, such as hiring policy,
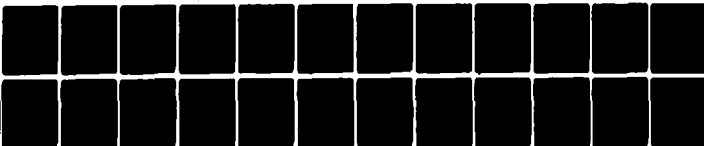
accident prevention, product liability, environmental protection, and so on.

Rather than add to the discussion of these issues -- I am sure you all have strong feelings on them -- I would like to return again to a point made at the beginning: namely, that model-building is not only a tool for predicting and making decisions about natural phenomena, but also has a useful function as a <u>communications</u> <u>medium</u> with other scientists and society at large. And, in this case, I think we have a classical case of a breakdown in communications; neither party is talking about the same issue. In using the risk classification paradigm, actuaries are relying upon the mean value principle to make arguments about equity; on the other hand, societal advocates are recognizing (however imperfectly) that there is a distribution of possible risk outcomes, and that, from the individual point of view, the paradigm gives non-socially-acceptable results.

Now, I would admit that eliminating classifications and regrouping risks may lead to higher variances, possible funding problems, plan terminations, and other systems problems which neither party wishes to happen. But, notice that the analytic argumentation was <u>never</u> <u>extended</u> <u>to</u> <u>this</u> <u>level</u> -- the modelling, analysis, and argumentation was <u>not</u> developed to examine rationally all of the possible product policy changes under discussion, but instead, <u>each</u> <u>community</u> <u>held</u> <u>fast</u> <u>to</u> <u>its</u> <u>own</u> <u>narrow</u> <u>world-view</u>.

As I said earlier, model-building must be primarily a <u>useful</u> activity. When we find that a paradigm no longer serves our needs, then we must modify it to suit, or we will find that its function has been bypassed by other forces in business and society. It is

important to keep the communication channels open to our public,
however painful it may feel to modify or even abandon our
traditional paradigms.

THE EVOLUTIONARY FUTURE OF INSURANCE MODELLING

In summary, we have seen that the current state-of-the-world
in general insurance modelling is complex, characterized on the
one hand by vigorous activity and growth, and on the other hand
by uneven stages of development, a certain tendency towards
puzzle-solving, and examples of communication breakdowns. Does
this mean that a modelling crisis, and a Kuhnsian revolution are
at hand?

For a variety of reasons, I believe that future insurance
modelling will be _evolutionary_, not revolutionary. One very
important reason is reflected in the wide range of communications
offered at the ICA's, at ASTIN Colloquia, and at your own national
meetings; thus, a variety of novel methods and models, often
transposed from other fields, continues to be presented and examined
_on_ _their_ _own_ _merits_, rather than subject to a tradition-oriented
screening process. This _receptiveness_ _to_ _new_ _ideas_ is critical to
the healthy evolution of a field, and it is delightful to see that
it is often the senior statesmen of insurance who are actively
trying out and promoting new ideas.

A community must also invest a portion of its own resources
in the future, which is why _continuing_ _research_ _activity_ is impor-
tant. Here there are some "straws in the wind", such as the forma-
tion of the Geneva Association to study economics of insurance,
and the new interest of American societies in sponsoring research
projects. More research support is needed from industry, in my
opinion.

New ideas are not useful unless _communicated_ to our own

community <u>and</u> to other scientific disciplines; and, here also,
there are good signs of growth, such as the strengthening and
increased dissemination of national journals, and the recent
decision of <u>Mathematical Reviews</u> to abstract interesting papers
from the <u>ASTIN Bulletin</u> and the <u>Swiss Actuarial Association</u>
<u>Bulletin</u>, as well as continuing with the <u>Scandinavian Actuarial</u>
<u>Journal</u>. In America, I would hope to see <u>ARCH</u> grow into a national
research journal which could transcend the traditional actuarial
boundaries, and encourage contributions from other scientists
interested in insurance modelling. Internationally, I believe
we might re-interpret ASTIN to mean simply <u>A</u>ctuarial <u>ST</u>udies in
<u>IN</u>surance, and to develop the membership, the symposia, and the
Bulletin to include all those in our community and from other
disciplines who are interested in modelling and research.

And finally, there must be continuing evolution of the
<u>educational</u> <u>process</u>, for the student of today is the actuary of
tomorrow, and must be trained in the concepts and methods which
will be <u>useful</u> <u>in</u> <u>the</u> <u>future</u>. As indicated earlier, I perceive
a serious mismatch between the abilities of today's graduate
and the demands placed upon him or her by current actuarial
examinations and professional assignments. The number of textbooks
published since 1969 [S6, B7, B37, L9, G13] portends well, and I
understand that various actuarial societies have additional basic
texts under development; the national reports on actuarial
training presented to this Congress also reveal some interesting
and innovative steps. As an educator, I urge you to continue to
devote attention and resources to the formation of young people.

As for yourselves, I urge you all to continue to be receptive

of and tolerant towards new methods, models, and paradigms,
analyzing and testing them, not through reaction, but in terms of
their potential utility to the actuarial community and the insur-
ance enterprise. Change is necessary; as Tennyson says so
beautifully:

> "The old order changeth, yielding place to the new,
> And God fulfils Himself in many ways,
> Lest one good custom should corrupt the world."

The evolution of the '80's will, I believe, make it an
exciting and challenging decade for insurance modelling, and
I look forward to participating in it with you.

Thank you for your kind attention.

98

BIBLIOGRAPHY

I. The following references are from: Transactions, 21st
    International Congress of Actuaries, Zurich & Lausanne, 1980:

Volume 1

[80.1] M.-H. Amsler, "Vers un Modèle Mathématique Commun à
        l'Assurance-vie et à l'Assurance Non-vie," 1-14.

[80.2] V. Andersen, "A Macro-dynamic Model of Life Insurance
        Business," 15-26.

[80.3] A. Banasinski, "Generalized Model of Insurance Business
        in Poland," 27-32.

[80.4] S. Benjamin, "Solvency and Profitability in Insurance,"
        33-46.

[80.5] B. Berliner and B. Lev, "On the Use of the Maximum Entropy
        Concept in Insurance," 47-62.

[80.6] H. Bohman, "Towards a Widening of the Insurance Concept,"
        63-70.

[80.7] G. Buoro, G. Pavesi and G. Zucchiatti, "Osservazioni sul
        Sistema di Calcolo del Margine di Solvabilità," 71-80.

[80.8] J. Calcanis and A. Papatriandafylou, "A Stochastic Model
        for Non-life Group or Social Insurance and its Application
        to a New Product," 81-88.

[80.9] B. Dequal, "Considerazioni su una Particolare Scomposizione
        del Premio per Alcuni Tipi di Assicurazione sulla Vita.
        Conseguenze nella Partecipazione agli Utili," 89-102.

[80.10] J. Engelfriet and H. Wolthuis, "A Simulating Model of a
        Life Office and its Application to an Investment Problem,"
        103-118.

[80.11] M. Feilmeier, "Ein Modell für die Gewinnbeteiligung in der
        Kapitallebensversicherung," 119-136.

[80.12] H. U. Gerber, "Principles of Premium Calculation and
        Reinsurance," 137-142.

[80.13] P. Gessner, "Unternehmensforschung in der Lebensversi-
        cherung," 143-158.

[80.14]   A. B. Goldstein and J. C. Wooddy, "A Stock Life Insurance Company Model," 159-168.

[80.15]   M. J. Goovaerts, K. De Groot and L. D'Hooge, "On an Extension of Some Stop Loss Inequalities Based on Convex Analysis," 169-178.

[80.16]   W. Gruner and C. Stierlin, "Application of Operations Research Methods to the Computation of Premiums in the Area of Employee Benefit Plans," 179-184.

[80.17]   C. A. Hachemeister, "A Stochastic Model for Loss Reserving," 185-194.

[80.18]   F. Harwayne, "A Real World Model of Investment Income - its Position in Workers' Compensation Insurance Ratemaking," 195-168.

[80.19]   G. T. Humphrey, "Modelling Techniques in Life Assurance," 203-210.

[80.20]   Y. Kahane, "Solidity, Leverage and the Regulation of Insurance Companies," 211-228.

[80.21]   W. A. Kraegel, "Insurance Models in Light of Futurism," 229-242.

[80.22]   A. Martinez, "Interpretación del Fenómino Actuarial mediante un Proceso Estocástico Evolutivo de Variables Separables," 243-256.

[80.23]   I. Obregon, "Stop-loss Premiums in Life Reinsurance," 257-262.

[80.24]   A. J. Oddie and J. R. Coomber, "The Stability of Linked Life Assurance," 263-282.

[80.25]   T. Pentikäinen, "A Stochastic-dynamic Model for Insurance Business," 283-294.

[80.26]   J. H. Pollard and M. Sherris, "Application of Matrix Methods to Life Assurance Funds," 295-306.

[80.27]   E. J. Porto, "A Critique of Claim Reserve Models," 307-314.

[80.28]   E. Prieto, "Applicaciones de la Programación Matemática a la Gestión de la Empresa Aseguradora," 315-328.

[80.29]   G. Quario Rondo, "A Risk Selection Model," 329-336.

[80.30]   G. Ramachandran, "Extreme Value Theory and Earthquake
          Insurance," 337-355.

[80.31]   J.-M. Reinhard, "Jeux de Survie Economique: un Modèle
          Semi-markovien," 355-364.

[80.32]   Research Group of Japan, "On Group Term Life Insurance
          in Japan and its Contingency Reserves," 365-376.

[80.33]   J. Royer et M.-H. Boileau, "Méthode Mathématique de
          Détermination Prévisionnelle d'un Résultat d'Equilibre,"
          377-390.

[80.34]   I. L. Rushton, "A General Insurance Forecasting Model,"
          391-402.

[80.35]   J. P. Ryan, "An Application of Model Office Techniques
          to the Solvency Question," 403-410.

[80.36]   J. M. Ryder, "A General Theory of Insurance," 411-418.

[80.37]   W. F. Scott, "On the Full Immunisation of Life Assurance
          Policies Against Changes in Interest Rates," 419-426.

[80.38]   U. Tellenbach, "Ein allgemeines ökonomisches Modell für
          die Entwicklung einer Versicherungsgesellschaft,"
          427-432.

[80.39]   L. W. G. Tutt, "British Pensions Insurance Business in the
          Context of a Generalized Model," 433-442.

[80.40]   D. J. Vegas, "Un Ensayo sobre la Concepción Sistema
          aplicada a la Empresa de Seguros," 443-462.

[80.41]   K. H. Wolff, "Zur numerischen Berechnung der Ruinwahrs-
          cheinlichkeit," 463-472.

[80.42]   B. Zehnwirth, "Applications of Credibility Theory to the
          Estimation of Claim Rate and Claim Size Distribution,"
          473-478.

Volume 2

[80.43]   B. Ajne, "Hypothesis Testing in the Multiplicative
          Ratemaking Model," 1-9.

[80.44]   B. Berliner, "Deriving the Pareto and Exponential Dis-
          tribution Function from Classified Observation Data:
          A New Approach," 31-42.

[80.45] C. Jaumain, "Extra-mortalité multiplicative et applications," 121-127.

[80.46] K. Loimaranta, J. Jacobsson and H. Lonka, "On the Use of Mixture Models in Clustering Multivariate Frequency Data," 147-161.

Volume 4

[80.47] K. Borch, "Insurance and the Theory of Asset Prices," 73-82.

Volume 5

[80.48] M. Watanabe, "The Funding Method of Pension System under Dynamic Conditions," 327-335.

II.   The following references are from: Transactions, 20th International Congress of Actuaries, Tokyo, 1976:

Volume 2

[76.1] L. Daboni, "Some Models of Bayesian Adaptive Processes for Premium Calculation in Non-Life Insurance," 47-52.

[76.2] L. D'Hooge and M. J. Goovaerts, "Numerical Treatment of the Determination of a Structure Function of a Tariff Class, Based Upon the Theory of Continued Fractions," 53-65.

[76.3] P. Pitkänen, "A Theoretical Approach to Premium Rating," 247-253.

[76.4] P. H. Jackson and W. W. Fellers, "Mortality Rates under Non-insured Pension Plans in the United States of America," 443-455.

102

Volume 3

[76.5]   M. Watanabe, "Analytical Representation of the Level Contribution in Case of Introducing the Automatic Adjustment in Pension Benefit," 387-394.

[76.6]   B. Berliner, "On the Choice of Risk Loadings," 413-423.

[76.7]   H. Bohman, "Management Accounting Systems Looked Upon Especially from the Point of View of Inflation," 425-432.

[76.8]   K. Borch, "Objectives and Optimal Decisions in Insurance," 433-441.

[76.9]   E. Ciminelli, "On the Distribution of the Highest Claims and its Application to the Automobile Insurance Liability," 501-517.

[76.10]   H. Clemeur, "Applications Actuarielles d'une Fonction d'Utilité Exponentielle," 519-527.

[76.11]   H. U. Gerber and R. A. Virola, "On Optimal Decisions When the Quality of a Risk is Unknown," 529-538.

[76.12]   D. G. Halmstad, "Exact Numerical Procedures in Discrete Risk Theory," 557-562.

[76.13]   R. Ottaviani, "On Determining the Optimal Partial Coverage of an Insurance Risk," 639-648.

[76.14]   T. Pentikäinen, "Stochastic-Dynamic Prognosis," 659-670.

[76.15]   G. Ramachandran, "Extreme Value Theory and Fire Insurance," 695-707.

[76.16]   A. R. N. Ratcliff, "Some Market Limitations on the Application of Mathematical Research to Insurance Management," 709-718.

[76.17]   E. Straub, "What is an Adequate Dividing Line Between Normal Claims and Large Claims?," 753-762.

[76.18]   O. Thorin and N. Wikstad, "Calculation and Use of Ruin Probabilities," 773-781.

[76.19]   Y. Yasuhara, H. Kubota and Y. Fujii, "The Decision of Retention in Reciprocal Reinsurance," 827-843.

Volume 4

[76.20] H. Bühlmann, "A Probabilistic Approach to Long Term
         Insurance (Typically Life Insurance)," 267-276.

[76.21] E. Franckx, "Introduction to Processes with Convex
         Chaining," 307-334.

[76.22] A. D. Wilkie, "Report on the Special Meeting 'Actuarial
         Notations,'" 337-345.

III. The following general references are organized alphabetic-
      ally, using the following abbreviations:

AB   ASTIN Bulletin

MVsV   Mitteilungen der Vereinigung schweizerischer
        Versicherungsmathematiker

PCAS   Proceedings of the Casualty Actuarial Society

SAJ   Scandinavian Actuarial Journal (formerly
        Skandinavisk Aktuarietidskrift)

TSA   Transactions of the Society of Actuaries

[A1]   J. Aitchison and I. R. Dunsmore, Statistical Prediction
        Analysis, Cambridge University Press, Cambridge, 1975.

[A2]   B. Ajne, "A Note on the Multiplicative Ratemaking Model,"
        AB, 8, 144-153 (1975).

[A3]   M. Amelia Cabral and J. Alfonso Garcia, "Study of Factors
        Influencing the Risk and Their Relation to Credibility
        Theory," AB, 9, 84-104 (1977).

[A4]   M.-H. Amsler, "La Ruine et sa Contre-Utilité," MVsV,
        167-182 (1975).

104

[A5]   _____, "L'Equation Générale d'Equilibre d'un
Risque Collectif," MVsV, 221-246 (1978).

[A6]   _____, "Ruine et Provision de Fluctuation,"
MVsV, 41-56, (1979).

[A7]   K. J. Arrow, "Optimal Insurance and Generalized
Deductibles," SAJ, 1-42 (1974).

[B1]   A. L. Bailey, "Credibility Procedures, LaPlace's Generali-
zation of Bayes' Rule, and the Combination of Collateral
Knowledge with the Observed Data," PCAS, 37, 7-23,
Discussion 94-115 (1950).

[B2]   Y. Balcer and I. Sahin, "Probabilistic Models for Pension
Benefits," Journal of Risk and Insurance, 46, 99-123
(1979).

[B3]   V. Barnett, Comparative Statistical Inference, Wiley,
New York, 1973.

[B4]   D. Basu, "Statistical Information and Likelihood,"
Sankhyā, 37, A, 1-71 (1975).

[B5]   B. Baton and J. Lemaire, "On the Core of a Reinsurance
Market," unpublished manuscript (1980).

[B6]   R. E. Beard, "Verification of Outstanding Claim
Provisions - Separation Technique," AB, 9, 26-32 (1977).

[B7]   _____, T. Pentikäinen and E. Pesonen, Risk Theory -
The Stochastic Basis of Insurance (Second Edition),
Chapman & Hall, London, 1977.

[B8]   J. A. Beekman, Two Stochastic Processes, J. Wiley & Sons,
New York, 1974.

[B9]   _____, "A New Collective Risk Model," TSA, 25,
(1974).

[B10]   _____, "Compound Poisson Processes, as Modified
by Ornstein-Uhlenbeck Processes," Part I, SAJ, 226-232
(1975). Part II, SAJ, 30-36 (1976).

[B11]   _____ and C. P. Fuelling, "Refined Distributions
for a Multi-Risk Stochastic Process," SAJ, 175-183 (1977).

[B12]   _____, "A Multi-Risk Stochas-
tic Process," TSA, 30, 371-405 (1978).

[B13]   L.-G. Benckert and J. Jung, "Statistical Models of Claim
        Distributions in Fire Insurance," AB, 8, 1-25 (1974).

[B14]   G. Benktander, "A Note on the Most 'Dangerous' and
        Skewest Class of Distributions," AB, 2, 387-390 (1963).

[B15]   _____, "Claims Frequency and Risk Premium Rate
        as a Function of the Size of the Risk," AB, 7, 119-136
        (1973).

[B16]   _____, "A Note on Optimal Reinsurance," AB, 8,
        154-163 (1975).

[B17]   _____, "The Calculation of a Fluctuation Loading
        for an Excess of Loss Cover," AB, 8, 272-278 (1975).

[B18]   _____, "On the Rating of a Special Stop-Loss
        Cover," AB, 9, 33-41 (1977).

[B19]   _____, "Largest Claims Reinsurance (LCR), A Quick
        Method to Calculate LCR-Risk Rates from Excess of Loss
        Rates," AB, 10, 54-58 (1978).

[B20]   B. Berliner, "Some Thoughts on (Re)insurance Loadings
        under a Ruin Criterion," SAJ, 74-80 (1974).

[B21]   _____, "A Risk Measure Alternative to the Variance,"
        AB, 9, 42-58 (1977).

[B22]   C. Boehm et al., "Thoughts on the Harmonization of Some
        Proposals for a New International Actuarial Notation,"
        Blätter, DGVM, 12, 99-129 (1975).

[B23]   H. Bohman, "On the Use of Simulation Technique," SAJ,
        175-178 (1974), plus O. Lundberg, "Comments on Harald
        Bohman's Paper, " SAJ, 179-181 (1974).

[B24]   _____, "Rule of Thumb for the Determination of a
        Sufficient Risk Reserve," SAJ, 237-240 (1974).

[B25]   _____, "Sufficient Distance to the Barrier During
        Random Walk," SAJ, 59-61 (1975).

[B26]   _____, "Solvency and Profitability Standards," SAJ,
        111-113 (1976).

[B27]   _____, "How Different Are Life Insurance and General
        Insurance?," SAJ, 54-58.

[B28]   _____, "Insurance Economics," SAJ, 57-74 (1979).

106

[B29]   K. Borch, *The Economics of Uncertainty*, Princeton
        University Press, Princeton, 1968.

[B30]   _____, *The Mathematical Theory of Insurance*,
        D. C. Heath, Lexington, 1974.

[B31]   _____, "Mathematical Models in Insurance," *AB*, 7,
        192-202 (1974).

[B32]   _____, "Optimal Reinsurance Arrangements," *AB*, 8,
        284-291 (1975).

[B33]   _____, "Problems in the Economic Theory of Insurance,"
        AB, 10, 1-11 (1978).

[B34]   _____, "Mathematical Models for Marine Insurance,"
        SAJ, 25-36 (1979).

[B35]   N. L. Bowers, Jr., J. C. Hickman and C. J. Nesbitt,
        "Introduction to the Dynamics of Pension Funding,"
        TSA, 28, 177-214 (1976).

[B36]   H. Bühlmann, "Experience Rating and Credibility," *AB*,
        4, 199-207 (1967).

[B37]   _____, *Mathematical Methods in Risk Theory*,
        Springer-Verlag, New York, 1970.

[B38]   _____, "Comparison of Three Credibility Formulae
        Using Multi-dimensional Techniques," *AB*, 7, 203-207
        (1974).

[B39]   _____, "The Mincing Machine Revisited," *AB*, 8,
        135-143 (1975).

[B40]   _____, "Minimax Credibility," *SAJ*, 65-78 (1976).

[B41]   _____, "An Economic Premium Principle," unpublished
        manuscript (1980).

[B42]   _____, B. Gagliardi, H. U. Gerber and E. Straub,
        "Some Inequalities for Stop-Loss Premiums," *AB*, 9,
        75-83 (1977).

[B43]   _____ and H. U. Gerber, "Risk Bearing and the
        Insurance Market," *AB*, 10, 12-24 (1978).

[B44]   _____ and W. S. Jewell, "Optimal Risk Exchanges,"
        AB, 10, 243-262 (1979).

[B45] _____, R. Schneiper and E. Straub, "Claim Reserves in Casualty Insurance Based on a Probabilistic Model," unpublished manuscript (1980).

[B46] _____ and E. Straub, "Glaubwürdigkeit für Schadensätze," MVsV, 70, 111-133 (1970).

[B47] M. Bunge, Scientific Research, I, The Search for Systems. II, The Search for Truth, Springer-Verlag, New York, 1967.

[C1] R. Consael and J. Sonnenschein, "Théorie Mathématique des Assurances de Personnes-Modèle Markovien," MVsV, 75-93 (1978).

[C2] P. L. Cooley, "Bayesian and Cost Considerations for Optimal Classification with Discriminant Analysis," Journal of Risk and Insurance, 62.2, 277-287 (1975).

[C3] J. Cornfield, "The Bayesian Outlook and its Application," Biometrics, 617-657 (1979).

[C4] F. Covens, M. Van Wouwe and M. Goovaerts, "On the Numerical Evaluation of Stop-Loss Premiums," AB, 10, 318-324 (1979).

[C5] H. Cramér, Collective Risk Theory, reprinted from Jubilee Volume, Skandia Insurance Company, 1955.

[C6] J. D. Cummins, "Risk Management and the Theory of the Firm," Journal of Risk and Insurance, 63.4, 587-609 (1976).

[D1] L. Daboni, "Some Models of Inference in the Risk Theory from a Bayesian Point of View," AB, 8, 38-56 (1974).

[D2] J. J. Deely and D. V. Lindley, "Bayes Empirical Bayes," unpublished manuscript (1980).

[D3] B. De Finetti, "Foresight: Its Logical Laws, Its Subjective Sources," 93-158, Studies in Subjective Probability, M. Kyburg and H. Smokler (eds.), Wiley, New York, 1964.

[D4] _____, "Sulla Teoria della Credibilità," Giornale dell' Istituto degli Attuari, 27, 219-231 (1964).

[D5] N. De Pril, "The Efficiency of a Bonus-Malus System," AB, 10, 59-72 (1978).

108

[D6]   _____, "Optimal Claim Decisions for a Bonus-Malus
System: A Continuous Approach," AB, 10, 215-222 (1979).

[D7]   _____, L. D'Hooge and M. J. Goovaerts, "A Biblio-
graphy on Credibility Theory and its Applications,"
Journal of Computational and Applied Mathematics, 2, No. 1,
55-62 (1976).

[D8]   F. De Vylder, Introduction aux Théories Actuarielles de
Crédibilité, Office des Assureurs de Belgique, 1975.

[D9]   _____, "Geometrical Credibility," SAJ, 121-149
(1976).

[D10]   _____, "Optimal Semilinear Credibility," MVsV,
27-40 (1976).

[D11]   _____, "Iterative Credibility," MVsV, 25-33 (1977).

[D12]   _____, "Martingales and Ruin in a Dynamical Risk
Process," SAJ, 217-225 (1977).

[D13]   _____, "Parameter Estimation in Credibility Theory,"
AB, 10, 99-112 (1978).

[D14]   _____, "A Practical Solution to the Problem of
Ultimate Ruin Probability," SAJ, 114-119 (1978).

[D15]   _____, "A Class of Very Regular Distribution
Functions and Corresponding Ruin Probabilities," SAJ,
177-181 (1978).

[D16]   _____, "Estimation of IBNR Claims by Least Squares,"
MVsV, 249-254 (1978).

[D17]   _____ and Y. Ballegeer, "A Numerical Illustration
of Optimal Semilinear Credibility," AB, 10, 131-148 (1979).

[D18]   _____ and M. Goovaerts, "An Invariance Property
of the Swiss Premium Calculation Principle," MVsV,
105-120 (1979).

[D19]   L. D'Hooge and M. J. Goovaerts, "Bayesian Inference
in Credibility Theory," AB, 8, 164-173 (1975).

[D20]   P. Diaconis and D. Ylvisaker, "Priors with Linear Posterior
Expectation," Annals of Statistics, 7, 269-281 (1979).

[D21]   A Dubey, "Probabilité de Ruine lorsque le Paramètre de
Poisson est ajusté a posteriori," MVsV, 130-141 (1977).

[E1]   T. C. Edwards, "A Benefit Manager's Guide to Sex,"
         Journal of Risk and Insurance, 62.2, 189-200 (1975).

[E2]   B. Efron, "Controversies in the Foundations of
         Statistics," _American Mathematical Monthly_, 231-246
         (April 1978).

[E3]   S. Eisenberg and Y. Kahane, "An Analytic Approach to
         Balance Sheet Optimization and Leverage Problems
         of a Property-Liability Company," _SAJ_, 205-210
         (1978).

[E4]   G. Elfving, "Bayes Statistics," _AB_, 51, 134-155 (1968).

[E5]   D. C. Emanuel, J. M. Harrison and A. J. Taylor,
         "A Diffusion Approximation of the Ruin Function of a
         Risk Process with Compounding Assets," _SAJ_, 240-247
         (1975).

[E6]   W. A. Ericson, "A Note on the Posterior Mean of a Popula-
         tion Mean," _Journal of the Royal Statistical Society (B)_,
         31, 332-334 (1969).

[F1]   S. E. Feinberg, "Linear and Quasi-Linear Bayes Estima-
         tors," _Bayesian Analysis in Econometrics and Statistics_,
         A. Zellner (ed.), North-Holland, Amsterdam, 355-361 (1980).

[F2]   G. Ferrara and G. Quario, "Distribution of the Number of
         Claims in Motor Insurance According to the Lag of
         Settlement," _AB_, 9, 119-124 (1977).

[F3]   J. Ferreira, Jr., "Equitable Insurance Premiums for
         Risk Classes: A Critique of Class Average Pricing,"
         Proceedings, American Statistical Association,
         560-565 (1978).

[F4]   _____, "Identifying Equitable Insurance
         Premiums for Risk Classes: An Alternative to the
         Classical Approach," Chapter IV in _Automobile Insurance
         Risk Classification: Equity and Accuracy_, Massachusetts
         Division of Insurance, Boston, 1978.

[F5]   J. A. Fibiger and S. G. Kellison, "Actuarial Functions
         as Expected Values," _TSA_, 23, 141-150 (1971).

[F6]   Filip Lundberg Foundation, _Symposium on Risk Theory_,
         supplement to _SAJ_, 52 (1969).

[F7]   _____, Methods of Calculating Ruin
              Probabilities, supplement to SAJ (1977):

              O. Thorin, "Ruin Probabilities Prepared for Numerical
              Calculation," 7-17.
              N. Wikstad, "How to Calculate Ruin Probabilities
              According to the Classical Ruin Theory," 19-24.
              H. Bohman, "A Rule of Thumb and its Verification,"
              25-36.
              J. Grandell, "A Class of Approximations of Ruin
              Probabilities," 37-52.

[F8]   L. R. Freifelder, A Decision Theoretic Approach to
              Insurance Rate-making, Monograph No. 4, S. S. Huebner
              Foundation, R. D. Irwin, Homewood, 1976.

[F9]   M. Friedman and L. J. Savage, "The Utility Analysis of
              Choices Involving Risk," Journal of Political Economy,
              56, 279-304 (1948).

[F10]   A. Frisque, "Dynamic Model of Insurance Company's
              Management," AB, 8, 57-65 (1974).

[F11]   D. Fürst, "Formulation Bayesienne du Problème des Valeurs
              Extrêmes en relation à la Réassurance en 'Excédent de
              Sinistres,'" AB, 3, 154-162 (1964).

[G1]   B. Gagliardi and E. Straub, "Eine Obere Grenze für
              Stop-Loss-Prämien," MVsV, 215-231 (1974).

[G2]   H. U. Gerber, "Martingales in Risk Theory," MVsV, 73,
              205-215 (1973).

[G3]   _____, "On Iterative Premium Calculation
              Principles," MVsV, 163-172 (1974).

[G4]   _____, "The Dilemma Between Dividends and Safety
              and Generalization of the Lundberg-Cramer Formulas,"
              SAJ, 46-57 (1974).

[G5]   _____, "On Additive Premium Calculation Principles,"
              AB, 7, 215-222 (1974).

[G6]   _____ and D. A. Jones, "Credibility Formulae with
              Geometric Weights," TSA, 27 (1975).

[G7]   _____, "The Surplus Process as a Fair Game -
              Utilitywise," AB, 8, 307-322 (1975).

[G8] _____, "A Probabilistic Model for (Life) Contingencies and a Delta-Free Approach to Contingency Reserves," TSA, 28, 127-148 (1976).

[G9] _____, "On the Optimal Cancellation of Policies," AB, 9, 125-138 (1977).

[G10] _____, "On the Computation of Stop-Loss Premiums," MVsV, 47-58 (1977).

[G11] _____, "A General Model for Life Contingencies," ARCH 78.2, 35-40 (1978).

[G12] _____, "Pareto-Optimal Risk Exchange and Related Decision Problems," AB, 10, 25-33 (1978).

[G13] _____, Introduction to the Mathematical Theory of Risk, The Huebner Foundation, Philadelphia, 1980.

[G14] _____ and D. A. Jones, "Some Practical Considerations in Connection with the Calculation of Stop-Loss Premiums," TSA, 28, 215-235 (1976).

[G15] _____, "Applications of Linear Algebra in Graduation and Other Disciplines of Actuarial Sciences," ARCH 78.1 (1978).

[G16] M. J. Goovaerts and F. De Vylder, "A Note on Iterative Premium Calculation Principles," AB, 10, 326-329 (1979).

[G17] R. E. Graham and L. W. Johnson, "A Unified Bayesian Approach to Optimal Classification with Discriminant Analysis," Journal of Risk and Insurance, 64.2, 316-319 (1977).

[H1] C. A. Hachemeister, "Credibility for Bayesian Models with Appreciation to Trend," in [K3].

[H2] C. Haehling von Lanzenauer and W. N. Lundberg, "The Propensity to Cause Accidents," AB, 7, 154-164 (1973).

[H3] _____, "Optimal Claim Decisions by Policyholders in Automobile Insurance with Merit-Rating Structures," Operations Research, 22, 979-990 (1974).

[H4] N. Hakansson, "Optimal Investment and Consumption Strategies under Risk for a Class of Utility Functions," Econometrica, 38, 587-607 (1970).

[H5] _____, "Optimal Entrepreneurial Decisions in a Completely Stochastic Environment," Management Science, 17, 428-449 (1971).

[H6] M. Hallin, "Méthodes Statistiques de Construction de Tarif," MVsV, 161-175 (1977).

[H7] D. Halperin, "Should Pension Benefits Depend Upon the Sex of the Recipient?," American Association of the University Professors Bulletin, 61.4, 43-48 (December 1975).

[H8] J. A. Hartigan, "Linear Bayesian Methods," Journal of the Royal Statistical Society (B), 31, 446-454 (1969).

[H9] W.-R. Heilmann, "A Duality Theory for Stop-Loss Distributions," SAJ, 225-228 (1978).

[H10] C. C. Hewitt, Jr., "Credibility for Severity," PCAS, 57, 168-171 (1970), Discussion, 58, 25-31 (1971).

[H11] J. C. Hickman, "A Statistical Approach to Premiums and Reserves in Multiple Decrement Theory," TSA, 16, 1-16 (1964).

[H12] _____, "Introduction and Historical Overview of Credibility," in [K3].

[H13] _____ and R. B. Miller, "Notes on Bayesian Graduations," TSA, 29, 7-49 (1977).

[H14] J. M. Hoem, "Markov Chain Models in Life Insurance," Blätter, DGVM, 9, 91-107 (1969).

[H15] _____, "A Markov Chain Model of Working Life Tables," SAJ, 1-20 (1977).

[H16] _____, "Exposed-to-Risk Considerations Based on the Balducci Assumption and other Assumptions in the Analysis of Mortality," Working Paper #3, Laboratory of Actuarial Mathematics, University of Copenhagen (December 1979).

[H17] _____ and O. O. Aalen, "Actuarial Values of Payment Streams," SAJ, 38-47 (1978).

[H18] B. Holmström, "Moral Hazard and Observability," Bell Journal of Economics, 10, 74-91 (1979).

[H19] K. Hösli, "Grenzwertsätze fur Warteschlangen, Damme, und Risikotheorie," MVsV, 223-238 (1974).

[J1]   W. S. Jewell, "Multi-dimensional Credibility," ORC 73-7, Operations Research Center, University of California, Berkeley (1973).

[J2]   _____, "Operations Research in the Insurance Industry: I. A Survey of Applications," Operations Research, 22, 918-928 (1974).

[J3]   _____, T. L. Johnston and S. S. Leavitt, "Operations Research in the Insurance Industry: II. An Application in Claims Operations of Workmen's Compensation Insurance," Operations Research, 22, 929-941 (1974).

[J4]   _____, "The Credible Distribution," AB, 7, 237-269 (1974).

[J5]   _____, "Credible Means are Exact Bayesian for Simple Exponential Families," AB, 8, 77-90 (1974).

[J6]   _____, "Exact Multidimensional Credibility," MVsV, 74, 193-214 (1974).

[J7]   _____, "Isotonic Optimization and Tariff Construction," AB, 8, 175-203 (1975).

[J8]   _____, "Regularity Conditions for Exact Credibility," AB, 8, 336-341 (1975).

[J9]   _____, "Model Variations in Credibility Theory," in [K3] (1975).

[J10]   _____, "Two Classes of Covariance Matrices Giving Simple Linear Forecasts," RM-75-17, SAJ, 15-29 (1976).

[J11]   _____, "The Use of Collateral Data in Credibility Theory: A Hierarchical Model," Giornale dell' Istituto Italiano degli Attuari, 38, 1-16 (1975).

[J12]   _____ and R. Avenhaus, "Bayesian Inverse Regression and Discrimination: An Application of Credibility Theory," RM-75-27, International Institute for Applied Systems Analysis, Laxenburg, Austria (1975).

[J13]   _____, "Bayesian Regression and Credibility Theory," RM-75-63, International Institute for Applied Systems Analysis, Laxenburg, Austria (1975).

[J14]   _____, "Credibility Regression with Simple Trends," ORC 75-16, Operations Research Center, University of California, Berkeley (1975).

114

[J15]   _____, "Linearized Bayesian Estimation of Network Flows," IEEE Transactions: Circuits and Systems, 23, 823-826 (1976).

[J16]   _____, "Bayesian Life Testing Using the Total Q on Test," The Theory and Applications of Reliability with Emphasis on Bayesian and Non-Parametric Methods, I, 49-66 (1977).

[J17]   _____, "A Survey of Credibility Theory," ORC 76-31, Operations Research Center, University of California, Berkeley (1976).

[J18]   _____, "The Analytic Methods of Operations Research," Philosophical Transactions of the Royal Society of London A, 287, 373-404 (1977).

[J19]   _____, "Bayesians Learn While Waiting," AB, 10, 163-172 (1979).

[J20]   P. Johansen, "Mathematical Models Regarding Fire and Consequential Loss," SAJ, 229-234 (1978).

[J21]   _____, "Early Models Describing the Fire Insurance Risk," AB, 10, 330-334 (1979).

[J22]   D. A. Jones and G. S. Kimeldorf, "Bayesian Graduation," TSA, 19 (1967).

[K1]   Y. Kahane, "The Theory of Insurance Risk Premiums - A Re-examination in the Light of Recent Developments in Capital Market Theory," AB, 10, 223-239 (1979).

[K2]   _____ and H. Levy, "Regulation in the Insurance Industry: Determination of Premiums in Automobile Insurance," Journal of Risk and Insurance, 62.1, 117-132 (1975).

[K3]   P. M. Kahn (ed.), Credibility - Theory and Applications, Proceedings of the Berkeley Actuarial Research Conference on Credibility, September 1974, Academic Press, New York (1975).

[K4]   J.-E. Karlsson, "The Expected Value of IBNR Claims," SAJ, 108-110 (1976).

[K5]   F. P. King, "Men, Women, and Life Annuities," Journal of Risk and Insurance, 63.4, 553-567 (1976).

[K6]   H. Kramreiter and E. Straub, "On the Calculation of IBNR Reserves II," <u>MVsV</u>, <u>73.2</u>, 177-190 (1973).

[K7]   T. S. Kuhn, <u>The Structure of Scientific Revolutions</u> (2nd Edition), University of Chicago Press, 1970.

[L1]   B. J. Lautzenheiser, "Sex and the Single Table: Equal Monthly Retirement Income for the Sexes?," <u>Employee Benefits Journal</u>, <u>2.1</u> (Fall 1976).

[L2]   R. B. Leckie (Moderator), D. D. Bartlett, K. T. Clark and J. C. Wooddy, "Actuarial Research - A Credit to the Profession?," <u>Record - Society of Actuaries</u>, <u>4</u>, 951-969 (1978).

[L3]   P. Leepin, "Über die Wahl von Nutzenfunktionen für die Bestimmung von Versicherungsprämien," <u>MVsV</u>, 27-45 (1975).

[L4]   J. Lemaire, "Sur la Détermination d'un Contrat optimal de Réassurance," <u>AB</u>, <u>7</u>, 165-180 (1973).

[L5]   _____, "Driver versus Company," <u>SAJ</u>, 209-219 (1976).

[L6]   _____, "Exchange de Risques entre Assureurs et Théorie des Jeux," <u>AB</u>, <u>9</u>, 155-180 (1977).

[L7]   _____, "La Soif du Bonus," <u>AB</u>, <u>9</u>, 181-190 (1977).

[L8]   _____, "Selection Procedures of Regression Analysis Applied to Automobile Insurance," <u>MVsV</u>, 143-160 (1977), and <u>MVsV</u>, 65-72 (1979).

[L9]   _____, <u>Théorie Mathématique des Assurances</u>, Presses Universitaires de Bruxelles, 1978-1979.

[L10]   _____, "How to Define a Bonus-Malus System with an Exponential Utility Function," <u>AB</u>, <u>10</u>, 274-282 (1979).

[L11]   D. V. Lindley, <u>Bayesian Statistics - A Review</u>, Society for Industrial and Applied Mathematics, Philadelphia, 1972.

[L12]   _____, "Analysis of Life Tables with Grouping and Withdrawals," <u>Biometrics</u>, <u>35</u>, 605-612 (1979).

[L13]   _____, "The Bayesian Approach," <u>Scandinavian Journal of Statistics</u>, <u>5</u>, 1-26 (1979).

[L14]   _____, "The Bayesian Approach to Statistics," ORC 80-9, Operations Research Center, University of California, Berkeley (May 1980).

116

[L15]   A. C. Lo, "Some Contributions to Bayesian Nonparametric Statistical Inference," Ph.D. Dissertation, University of California, Berkeley (1978).

[L16]   K. Loimaranta, "Some Asymptotic Properties of Bonus Systems," AB, 6, 233-245 (1972).

[L17]   _____, "On the Calculation of Variances and Credibilities by Experience Rating," AB, 9, 203-207 (1977).

[L18]   L. H. Longley-Cook, "An Introduction to Credibility Theory," PCAS, 49, 194-221 (1962).

[M1]   A. Marazzi, "Minimax Credibility," MVsV, 219-229 (1976).

[M2]   G. D. Martin, "Gender Discrimination in Pension Plans," Journal of Risk and Insurance, 63.2, 203-214 (1976).

[M3]   A. L. Mayerson, "A Bayesian View of Credibility," PCAS, 51, 85-104 (1964), Discussion, 52, 121-127 (1965).

[M4]   J. McGuiness, "A Managerial Game for an Insurance Company," Operations Research, 8, 196-209 (1960).

[M5]   D. Moffet, "Risk-Bearing and Consumption Theory," AB, 8, 342-358 (1975).

[M6]   _____, "The Risk-Sharing Problem," Geneva Papers on Risk and Insurance, 11, 5-13 (1979).

[M7]   J. Mossin, "Aspects of Rational Insurance Purchasing," Journal of Political Economy, 76, 553-568 (1976).

[N1]   Nederlandse Reassurantie Groep N. V., IBNR - Prize-Winning Papers in the Boleslaw Monic Fund Competition Held in 1971, NRG, Amsterdam, (1971).

[N2]   R. Norberg, "Credibility Premium Plans which Make Allowance for Bonus Hunger," SAJ, 73-86 (1975).

[N3]   _____, "A Credibility Theory for Automobile Bonus Systems," SAJ, 92-107 (1976).

[N4]   _____, "The Credibility Approach to Experience Rating," SAJ, No. 4, 181-221 (1979).

[O1]   W. Y. Oi, "The Economics of Product Safety," Bell Journal of Economics and Management Science, 4, 3-28 (1973).

[P1]   H. H. Panjer and L. K. Chan, "A Statistical Approach to Graduation by Mathematical Formula," <u>ARCH 79.2</u>, 27-60 (1979).

[P2]   M. Pauley, "Overinsurance and Public Provision of Insurance: The Roles of Moral Hazard and Adverse Selection," <u>Quarterly Journal of Economics</u>, <u>68</u>, 44-62 (1974).

[P3]   P. M. Pechlivanides, "Optimal Reinsurance and Dividend Payment Strategies," <u>AB</u>, <u>10</u>, 34-46 (1978).

[P4]   T. Pentikäinen, "A Model of Stochastic-Dynamic Prognosis," <u>SAJ</u>, 29-53 (1975).

[P5]   _____, "A Solvency Testing Model Building Approach for Business Planning," <u>SAJ</u>, 19-37 (1978).

[P6]   _____, "Dynamic Programming, An Approach for Analyzing Competition Strategies," <u>AB</u>, <u>10</u>, 183-194 (1979).

[P7]   M. A. Pereira Cabral and J. M. Afonso Garcia, "Calculation of Provisions Using Credibility Theory," <u>AB</u>, <u>7</u>, 277-292 (1974).

[P8]   C. Philipson, "A Review of the Collective Theory of Risk," Parts I and II, <u>SAJ</u>, <u>51</u>, 45-68, 117-133 (1968).

[P9]   G. E. Pinches and J. S. Trieschmann, "The Efficiency of Alternative Models for Solvency Surveillance in the Insurance Industry," <u>Journal of Risk and Insurance</u>, <u>61.4</u>, 563-577 (1974).

[P10]   P. Pitkänen, "Tariff Theory," <u>AB</u>, <u>8</u>, 204-228 (1975).

[P11]   J. W. Pratt, "Risk Aversion in the Small and Large," <u>Econometrica</u>, <u>32</u>, 122-136 (1964).

[P12]   F. Pressacco, "Values and Prices in a Reinsurance Market," <u>AB</u>, <u>10</u>, 263-273 (1979).

[R1]   G. Ramachandran, "Extreme Value Theory and Large Fire Losses," <u>AB</u>, <u>7</u>, 293-310.

[R2]   _____, "Factors Affecting Fire Loss-Multiple Regression Models with Extreme Values," <u>AB</u>, <u>8</u>, 229-241 (1975).

118

[R3]   D. H. Reid, "Representations of Claims Arising from a
        Risk Portfolio," AB, 8, 242-256 (1975).

[R4]   G. K. Robinson, "Some Counterexamples to the Theory
        of Confidence Intervals," Biometrika, 62, 155-161
        (1975).

[R5]   M. Ruohonen, "A Note on Pareto Optimality in Mutual
        Reinsurance," SAJ, 231-233 (1979).

[S1]   L. J. Savage, The Foundation of Statistics, Wiley,
        New York, 1954.

[S2]   W. C. Scheel, "Efficient Simulation of Mortality
        Experience for a Closed Cohort of Lives," Journal of
        Risk and Insurance, 64.4, 571-584 (1977).

[S3]   H. Schmitter and E. Straub, "Quadratic Programming
        in Insurance," AB, 7, 311-322 (1974).

[S4]   _____, "How to Find the Right
        Subdivision into Tariff Classes," AB, 8, 257-263 (1975).

[S5]   D. R. Schuette, "A Linear Programming Approach to
        Graduation," TSA, 30, 407-445 (1978).

[S6]   H. L. Seal, Stochastic Theory of a Risk Business,
        John Wiley & Sons, New York, 1969.

[S7]   _____, "Risk Theory and the Single-Server Queue,"
        MVsV, 72, 171-178 (1972).

[S8]   _____, "Numerical Calculation of the Probability
        of Ruin in the Poisson/Exponential Case," MVsV, 72.1,
        77-98 (1972).

[S9]   _____, "The Story of 100 Actuarially Guaranteed
        No-Ruin Casualty Insurance Companies," AB, 8, 364-377
        (1975).

[S10]   _____, Survival Probabilities: The Goal of Risk
        Theory, John Wiley & Sons, New York, 1978.

[S11]   A. F. Shapiro, "A Stochastic Model for Determining
        the Contingency Charge in Group Life Insurance,"
        Journal of Risk and Insurance, 63.3, 463-486
        (1976).

[S12] _____, "The Relevance of Expected Persistency Rates when Projecting Pension Costs," _Journal of Risk and Insurance_, 64.4, 623-638 (1977).

[S13] _____, "A Bayesian Approach to Persistency Rates when Projecting Retirement Costs," _ARCH 78.2_, 194-236 (1978).

[S14] S. Shavell, "Risk Sharing and Incentives in the Principal and Agent Relationship," _Bell Journal of Economics_, 10, 54-73 (1973).

[S15] D. C. Shpilberg, "The Probability Distribution of Fire Loss Amount," _Journal of Risk and Insurance_, 64.1, 103-115 (1977).

[S16] _____ and R. de Neufville, "Use of Decision Analysis for Optimizing Choice of Fire Protection and Insurance: An Airport Study," _Journal of Risk and Insurance_, 62.1, 133-149 (1975).

[S17] H. Simberg, "A Risk Theory Model for Loss Prevention Work," paper presented at 13th ASTIN Colloquium, Washington, D.C. (1977).

[S18] E. Straub, "Estimation of the Number of Excess Claims by Means of the Credibility Theory," _AB_, 5, 388-392, (1971).

[S19] _____, "On the Calculation of IBNR Reserves," in [N1].

[S20] _____, "How to Fix Retention," _MVsV_, 95-104 (1978).

[S21] J. Strauss, "Deductibles in Industrial Fire Insurance," _AB_, 8, 378-393 (1975).

[S22] B. Sundt, "An Insurance Model with Collective Seasonal Random Factors," _MVsV_, 57-64 (1979).

[S23] _____, "Credibility and Relative Exchangeability," _SAJ_, No. 1, 45-51 (1979).

[S24] _____, "A Hierarchical Credibility Regression Model," _SAJ_, Nos. 2-3, 107-114 (1979).

[S25] _____, "On the Choice of Statistics in Credibility Estimation," _SAJ_, Nos. 2-3, 115-123 (1979).

120

[T1]   G. C. Taylor, "Experience Rating with Credibility Adjustment of the Manual Premium," AB, 7, 323-336 (1974).

[T2]   _____, "Use of Differential and Integral Inequalities to Bound Ruin and Queueing Probabilities," SAJ, 197-208 (1976).

[T3]   _____, "Separation of Inflation and Other Effects from Distribution of Non-Life Insurance Claim Delays," AB, 9, 217-230 (1977).

[T4]   _____, "Upper Bounds on Stop-Loss Premiums under Constraints on Claim Size Distribution," SAJ, 94-105 (1977).

[T5]   _____, "Abstract Credibility," SAJ, 149-168 (1977).

[T6]   _____, "Testing Goodness-of-Fit of an Estimated Run-Off Triangle," AB, 10, 78-86 (1978).

[T7]   _____, "An Investigation of the Use of Weighted Averages in the Estimation of the Mean of a Long-Tailed Claim Size Distribution," AB, 10, 87-98 (1978).

[T8]   _____, "Representation and Explicit Calculation of Finite-Time Ruin Probabilities," SAJ, 1-18 (1978).

[T9]   _____, "Probability of Ruin under Inflationary Conditions or under Experience Rating," AB, 10, 149-162 (1979).

[T10]   _____, "Rating the Discount for a Motor Insurance Excess," AB, 10, 295-302 (1979).

[T11]   _____, "The Negative Exponential Distribution and Average Excess Claim Size," AB, 10, 303-304 (1979).

[T12]   _____, "Credibility Analysis of a General Hierarchical Model," SAJ, 1-12 (1979).

[T13]   A. Tenenbein, "Actuarial Functions as Random Variables," ARCH 78.2, 237-245 (1978).

[T14]   O. Thorin, "On the Asymptotic Behavior of the Ruin Probability for an Infinite Period when the Epochs of Claims Form a Renewal Process," SAJ, 81-99 (1974).

[T15]   J. Tiago de Oliveira, "Statistical Methodology for Large Claims," AB, 9, 1-9 (1977).

[V1]   H. G. Verbeek, "A Stop Loss Inequality for Compound
         Poisson Processes with a Unimodal Claimsize Distribu-
         tion," AB, 9, 247-256 (1977).

[V2]   B. von Bahr, "Ruin Probabilities Expressed in Terms
         of Ladder Height Distributions," SAJ, 190-204 (1974).

[W1]   H. R. Waters, "Excess of Loss Reinsurance Limits,"
         SAJ, 37-43 (1979).

[W2]   H. Wenger, "Eine Tarifierungsmethode im Feuer-
         Industriegeschäft," MVsV, 73, 95-111 (1973).

[W3]   W. H. Wetterstrand, "Recent Mortality Experience
         Described by Gompertz's and Makeham's Laws - Including
         a Generalization," ARCH 78.2, 251-271 (1978).

[W4]   D. J. White, Decision Methodology, John Wiley & Sons,
         New York, 1975.

[W5]   N. Wikstad, "A Numerical Investigation of Bohman's
         Rule of Thumb for the Determination of a Sufficient
         Risk Reserve," SAJ, 119-120 (1975).

[W6]   K. H. Wolff, Methoden de Unternehmensforschung im
         Versicherungswesen, Springer-Verlag, Berlin (1966).

[Z1]   B. Zehnwirth, "The Mean Credibility Formula is a
         Bayes Rule," SAJ, 212-216 (1977).

[Z2]   _____, "The Credible Distribution Function is
         an Admissible Bayes Rule," SAJ, 121-127 (1978).

[Z3]   _____, "Credibility and the Dirichlet Process,"
         SAJ, 13-23 (1979).

[Z4]   _____, "A Hierarchical Model for the Estimation
         of Claim Rates in a Motor Car Insurance Portfolio,"
         SAJ, 75-82 (1979).

[ZZ1]   "The Sex Discrimination Issue," Record - Society of
         Actuaries, 123-134 (1975).

[ZZ2]   "Hot Issue for 1979: How to Set Fair Rates for Auto
         Insurance," Journal of American Insurance, 8-12
         (Summer 1978).

122

[ZZ3]   "Risk Classification (23-32); Effect on Plan Design
          (33-75); Classification Areas (77-98); Classification
          Models (115-132)," Record - Society of Actuaries,
          4 (1978).

[ZZ4]   "California Speaks Out on Auto Insurance," Journal of
          American Insurance, 23-32 (Fall 1979).