of fixed experience periods. The average future claim frequency by each sub-group is given by the mean of $N(x;t|c;s)$; namely, $t(r+c)/(a+s)$. Hence it is possible to compare the expected claim frequency for risks having had 1 accident in the last 2 years, say, with risks having had 2 accidents in the last 4 years. In these cases, the claim frequencies are $(r+1)/(a+2)$ and $(r+2)/(a+4)$, respectively. Also, by comparing the expected claim frequency for a certain sub-group with the average annual claim frequency for all risks, r/a, one is able to determine debits and credits as previously noted. This procedure was demonstrated by Mr. R. A. Bailey[2] in his discussion of Mr. Dropkin's previous paper.

An important result of the paper being reviewed is the realization that any merit rating plan which recognizes only the length of time since the most recent accident is not using all of the data available. At the same time it must be remembered that the developed formulas assume that each risk does not change from one time interval to the next, which obviously is not correct for long periods of time. Hence one may conclude that the most recent accident is more significant than any prior accident, but still the prior accidents are of some value.

The change in each risk that we know occurs and referred to in the previous paragraph brings to mind another application of the formulas. By comparing the actual with the theoretical we may be able to estimate the change in individual risks which occurs with passage of time. Also from a theoretical point of view, the formulas should be helpful in estimating the effectiveness of proposed changes in merit rating plans before any experience is obtained.

A NEW APPROACH TO INFANT AND JUVENILE MORTALITY

BY

CHARLES C. HEWITT, JR.

Volume XLVII, Page 41

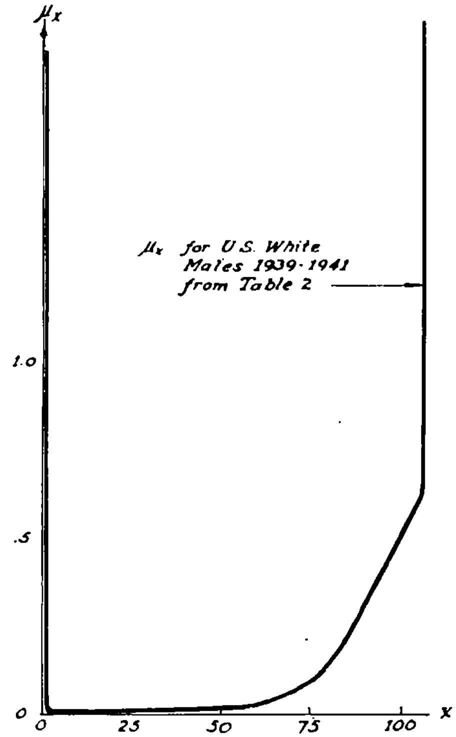
Author's Review of Discussion by

A. L. MAYERSON IN VOLUME XLVII

This is the first time that the writer of the article under discussion has ever had occasion to respond to a review. Frankly, I find the problem of responding more difficult than the original writing of the article itself.

The standard textbook of the Society of Actuaries on this subject is "Life Contingencies" by Professor Jordan of Williams College. In that text[1] the force of mortality is graphed from the beginning to the end of the mortality table and looks something like:

[1] "Life Contingencies", Jordan, C. W. (1952). The Society of Actuaries, p. 16.
[2] CAS XLVII, p.p. 152-154 (Bailey's discussion).



Previous attempts to express this complete curve by analytical means have generally been unsatisfactory, and actuaries have been content with the statement that either the Gompertz or Makeham function produced a good approximation from age 15 to near the end of the mortality table. The existence of the unanalyzed portion of the mortality curve has represented a challenge to actuaries for many years. Admittedly, as stated in my paper, this omission has not caused any great practical inconvenience since life insurers are primarily concerned with the mortality rates of mature individuals.

The advent of a more serious approach to the general subject of probabilities including developments in the theory of stochastic processes supplies what I believe is the missing link in the problem of a complete analytical expression for the *average* force of mortality. The solution of this problem was the primary purpose for the writing of the paper under discussion.

With these thoughts in mind, I now take up Professor Mayerson's review of my paper. Professor Mayerson is kind enough to recognize the originality of the approach and to appreciate the rationale underlying the paper. Because Professor Mayerson has been sympathetic in his review, I find it hard to bring myself to pointing out certain misconceptions on his part. Nevertheless in the interest of a more complete understanding of what I have tried to do, the following comments seem necessary:

(1) Professor Mayerson states that I have assumed that the individual force of mortality has a Pearson Type III distribution. Actually, what I did assume was that one of the three principal elements of the individual force of mortality had a Pearson Type III distribution.

(2) In describing the formula which I have derived for the group or average force of mortality, Professor Mayerson states that the term which I have referred to as the "force of selection" is intended to measure the individual's inherent capacity to survive. Actually, this term in the expression for the average force of mortality measures the effect of the elimination from the group of those individuals who have the least capacity to survive.

(3) Professor Mayerson indicates that he detects an error in the mathematics of my illustrative example. In making this statement Professor Mayerson ignores the context of the illustrative example in which it is assumed that both the individual forces of mortality and the individual rates of mortality for certain infants are constant for the first 4 years of life. Furthermore, although the paper maintains a scrupulous distinction between the individual force of mortality (μ_x) and the average force of mortality ($\bar{\mu}_x$), I am afraid that Professor Mayerson has confused the individual force of mortality with the average force of mortality. The fact is that one of the principal conclusions of the illustrative example is that the average force of mortality decreases throughout the four-year period even though the individual forces of mortality remain constant; the reason being, of course, that those lives least fit to survive are being eliminated by a process of selection.

Also on this same point, Professor Mayerson's statement "μ_0 decreases rapidly during the first year of life is self—contradictory since μ_0 is the value of the force of mortality at only one point, namely age 0. Professor Mayerson suggests that it would be interesting to see a comparison of the theoretical and actual mortality rates at individual ages between 0 and 5. This comparison shows the following:

	Mortality Rates	
Age (x)	*Actual* (q_x)	*Theoretical* (q_x)
1	.00487	.00320
2	.00264	.00205
3	.00189	.00158
4	.00154	.00136

Professor Mayerson correctly points out that the formula for joint life contingencies does not lend itself readily to calculation because the "law of uniform seniority" may not apply. This factor did not bother the writer as much as the fact that in the calculation of joint life contingencies it is customary to assume that the force of mortality with respect to each contingent life is independent of the force of mortality with respect to each of the other contingent lives. In dealing with benefits to survivors under a Workmen's Compensation Law where the survivors normally are the widow and children of a deceased workman, the assumption of the independence of the force of mortality among the members of the same family is open to serious question. While I did not raise this question in the paper itself, I did mention it at the original presentation of the paper in Washington last November. I do think this question of independence or dependence must be resolved before any further practical use is made of the actuarial model created in my paper.

The author of the paper under discussion is unable to resist one further comment which is in the nature of speculation. I believe that we are on the threshold of a major revision in the theoretical approach to the general subject of life contingencies.

I would like to express my appreciation to Professor Mayerson for his time and effort in presenting his review, and I would like to express the hope, which is probably common to every author, that this paper will lead to further study in this field.

THE NEGATIVE BINOMIAL APPLIED TO THE CANADIAN MERIT RATING PLAN FOR INDIVIDUAL AUTOMOBILE RISKS

BY

CHARLES C. HEWITT, JR.

Volume XLVII, Page 55

DISCUSSION BY O. D. DICKERSON

Mr. Hewitt's interesting paper carries on the discussion of automobile rating plans which consider the accident, conviction, claim and/or fault, experience of the auto and its drivers. There is a lack of general agreement whether such plans properly should be classified as individual risk rating plans or as extensions of the classification system.[1] The Canadian plan, to which Mr. Hewitt refers specifically, is designated as a "Merit rating plan"; the European plans are referred to as "no loss bonus" plans; and the bureau plan in the United States bears the hopeful appellation "Safe Driver Insurance Plan". By whatever name called and however categorized, such plans have been the subject of much current discussion and many papers.[2]

Recently the negative binomial distribution has become popular as a model to describe the theoretical distribution of accidents (convictions, claims, or

[1] *See, e.g.:* Kulp, C. A., *Casualty Insurance*, 3rd ed., New York: The Ronald Press Co., 1956, pp. 513 & 515-516; Simon, LeRoy J., "Myths and Mysteries Concerning the Actuarial Soundness of Merit Rating", paper presented to the Casualty Actuaries of Philadelphia, Sept. 7, 1960.

[2] Mr. Hewitt's footnotes cite most of these; the footnotes to this discussion cite a number of others.