**TRANSACTIONS OF SOCIETY OF ACTUARIES
1969 VOL. 21 PT. 1 NO. 60**

DISCUSSION OF PAPERS PRESENTED AT EARLIER REGIONAL MEETINGS

AN EMPIRICAL APPROACH TO THE DETERMINATION OF CREDIBILITY FACTORS

RALPH D. MAGUIRE

SEE PAGE 1 OF THIS VOLUME

PAUL H. JACKSON:

Mr. Maguire's paper presents the rationale underlying the determination of credibility factors on the basis of the relative stability, from period to period, in the sign (+ or −) of the divergence of actual loss ratios from expected. One of the assumptions underlying the approach is that the credibility factor for a given risk will depend entirely on certain aspects relating to that risk and that it is "equally applicable to all deviations of actual from expected claim levels, whether large or small, positive or negative." This suggests that the numerical value of the credibility factor is independent of these deviations, but formula (7) shows that the numerical value of the credibility factor can be estimated from the proportion of the total deviations which have changed in sign from one period to the next.

Estimating credibility on the basis of deviations between actual and expected loss ratios seems just a little too easy and, in my judgment, takes into account too few of the pertinent facts. For example, two group life cases could cover the same number of lives and have the same premium and the same expected loss ratio. If one has an insurance schedule of one and one-half times pay for active employees only, while the second provides one times pay for actives with a flat $2,000 for retireds, the dispersions about the mean in the latter case should be much smaller and the credibility accorded actual experience should be greater. Such differences might not show up in actual loss ratios, expected loss ratios, or their difference.

Perhaps my chief objection to an assumption that the credibility factor is equally applicable to all deviations of actual from expected is that as the deviation increases the possibility of that deviation being due to chance fluctuation about the expected value diminishes so that the probability that the true loss ratio is significantly different from the expected must therefore increase. To illustrate this, suppose that $f(x)$ is the distribution of true loss ratios about the mean e, and $g[(a - x)/x]$ represents the probability of actual loss ratio, a, occurring for a case having a true loss ratio of x. The value of x for which the product $f(x)g[(a - x)/x]$ is maximized is the most probable value for the true loss ratio t. If actual losses were identical to expected losses in a given year, the value $x = e$ will clearly maximize $f(x)$; and, equally clearly, the value $(a - x)/x = 0$ will maximize $g[(a - x)/x]$. When $(a - e)$ is small, the maximum for $f(x)$ $g[(a - x)/x]$ is well defined, the range of likely values for t is small, and considerable confidence can be placed on the value for t. On the other hand, as $(a - e)$ increases, the product $f(x)$ $g[(a - x)/x]$ becomes quite small and the range of likely values for t is quite extensive; thus the process of maximizing the product and of finding a single most probable t becomes far less certain. Ultimately, say, for $(a - e) > 6\sigma$, the product is equal to zero for all practical purposes, so the determination of the most probable value for t becomes impossible, even though in this case it is also a near certainty that t is greater than $e + 3\sigma$. Clearly, the absolute size of $(a - e)$ has some bearing on the credibility factor.

Unknown to the author, the clerk who assembled the data underlying Table 1 wanted to be sure that everything was right. In reviewing the data, he discovered that an error had been made, but it was far too late to correct the galley proofs and so the corrections were not made. Actually, several large claims on Risk 2 were miscoded as falling in Period A when they should have been included in Period B. The result does not change the two period totals, but Risk 2 is changed so that its loss ratio becomes 59 per cent in Period A and 76 per cent in Period B (the miscoded claims representing approximately 21 per cent of the premium, which was roughly constant for the two periods). Anyway, I know the author will be pleased to learn that I have corrected Table 1 for this error, and now formula (7) indicates that the credibility factor, k, is approximately equal to 103 per cent (for the five risks with positive deviations in Period A, the estimate for the credibility factor is 156 per cent). At this point, an experienced actuary would probably fire the helpful clerk and re-miscode the claims. This does illustrate, however, that the estimating process is heavily dependent on the numerical size of the deviations, particularly of those deviations changing sign. Apparently, a substantial number of cases has to be included in the calculations before formula (7) will produce a reasonable estimate of the credibility.

On balance, I strongly favor empirical approaches to credibility. After all theoretical niceties have been laid aside, the question remains whether the particular experience-rating approach selected will really work. One good, though not conclusive, test is whether it would have worked in the past. For such testing, I prefer to use the dividend formula itself rather than to test or develop values for the credibility factor. By way of example, a 25,000 life case with 100 deaths expected each year may well have a theoretical credibility factor on actual insurance amounts in the neighborhood of 60–70 per cent. Yet, if the billing rate plus accumulated claim fluctuation reserves covers 125 per cent of expected claims for the next year plus expense and profit margins (including some charge to cover catastrophic losses), it may be perfectly reasonable to give 100 per cent credibility to the actual experience on the grounds that there is only a negligible chance that the incurred claims will exceed the available funds arising from the case itself and require recourse to the insurance company's surplus.

WALTER SHUR:

Mr. Maguire has written a very interesting and stimulating paper on an important subject that seems to have been quite neglected in our actuarial literature. The essence of his paper is as follows:

1. The formula $t = ka + (1 - k)e$ is commonly used to estimate the true claims level. It is used notwithstanding the fact that there appears to be no theoretical justification for estimating the true claims level as a linear combination of actual and expected claims levels or for a credibility factor (k) which is independent of the actual claims level.
2. Even if the formula $t = ka + (1 - k)e$ is appropriate, it follows from the lack of a theoretical basis that k cannot be calculated by formula but must be estimated empirically. Maguire then goes on to develop an ingenious method of using the actual results of two periods of observation to estimate k. The split of cases in the first period into those with positive deviations of actual over expected and those where such deviations are negative was particularly clever.

The purpose of my discussion is to set down some fairly simple assumptions which lead directly, on a theoretical basis, to the formula $t = ka + (1 - k)e$, where k is independent of the actual claims level. The formula for k turns out to be $k = n/(n + c)$, where n is the number of observation periods, and the constant c, which depends on the size and other characteristics of the case but not on the actual claims level, has a very simple and logical interpretation.

Suppose that we are dealing with a very large block of cases for which the following assumptions are reasonable:

1. The true claims levels τ for the various cases in this large block are normally distributed about the known average claims level for the block, e, with a variance σ_T^2 (throughout this discussion the term

"claims level" means loss ratio relative to manual premiums). Hence, the distribution of the true claims level is given by

$$f(\tau) = \frac{1}{\sigma_T\sqrt{2\pi}}\, e^{-\frac{1}{2}[(\tau-e)/\sigma_T]^2}\,. \tag{1}$$

2. The actual claims level (a), which emerges *on a particular case* in the block, during a single observation period is normally distributed about the true claims level (τ) for that case, with variance σ_A^2. (Note that σ_A^2 is assumed to be the same regardless of the value of τ. And to be consistent with that assumption, it is also assumed that the cases in the block are all the same size.) Hence, the conditional distribution of (a), given a true claims level (τ), is given by

$$f(a/\tau) = \frac{1}{\sigma_A\sqrt{2\pi}}\, e^{-\frac{1}{2}[(a-\tau)/\sigma_A]^2}\,. \tag{2}$$

3. The characteristics of a particular case do not change over the periods of observation. Hence, the actual average claims level $\bar{a}$, determined from actual claims results on a particular case for n different observation periods, is also normally distributed with mean τ, and variance σ_A^2/n (i.e., the variance of a sample mean). The conditional distribution of $\bar{a}$, given a true claims level τ, is given by

$$f(\bar{a}/\tau) = \frac{\sqrt{n}}{\sigma_A\sqrt{2\pi}}\, e^{-\frac{1}{2}[(\bar{a}-\tau)/(\sigma_A/\sqrt{n})]^2}\,. \tag{3}$$

It follows that the joint distribution of $\bar{a}$ and τ is given by

$$\begin{aligned} f(\bar{a},\tau) &= f(\tau)f(\bar{a}/\tau) \\ &= \frac{\sqrt{n}}{2\pi\sigma_A\sigma_T}\, e^{-\frac{1}{2}\{[(\tau-e)/\sigma_T]^2 + [(\bar{a}-\tau)/(\sigma_A/\sqrt{n})]^2\}}\,. \end{aligned} \tag{4}$$

With some effort, purely algebraic, equation (4) can be rewritten as

$$\begin{aligned} f(\bar{a},\tau) &= \frac{1}{\sqrt{\sigma_T^2 + \sigma_A^2/n}\cdot\sqrt{2\pi}}\, e^{-\frac{1}{2}[(\bar{a}-e)/\sqrt{\sigma_T^2+\sigma_A^2/n}]^2} \\ &\times \left[\!\!\left[\frac{1}{\sqrt{(\sigma_T^2\sigma_A^2)/(n\sigma_T^2+\sigma_A^2)}\cdot\sqrt{2\pi}}\, e^{-\frac{1}{2}\left\{\frac{\tau-[(n\sigma_T^2\bar{a}+\sigma_A^2 e)/(n\sigma_T^2+\sigma_A^2)]}{\sqrt{\sigma_T^2\sigma_A^2/(n\sigma_T^2+\sigma_A^2)}}\right\}^2} \right]\!\!\right]. \end{aligned} \tag{5}$$

Integrating equation (5) with respect to τ and observing that the integral from $-\infty$ to $+\infty$ of the expression in brackets is equal to unity (since it is the integral of a normal density function), we obtain the distribution of $\bar{a}$, namely,

$$f(\bar{a}) = \frac{1}{\sqrt{\sigma_T^2 + \sigma_A^2/n}\cdot\sqrt{2\pi}}\, e^{-\frac{1}{2}[(\bar{a}-e)/\sqrt{\sigma_T^2+\sigma_A^2/n}]^2} . \tag{6}$$

(This interim result is worth a comment; it indicates that $\bar{a}$, which is the result of two normal processes, the second of which uses a mean equal to the result of the first, is also a normal variable. Furthermore, its mean is equal to the mean of the first normal process, and its variance is the sum of the variances of the two normal processes.)

What we are really after is the conditional distribution of τ, given a particular claims level $\bar{a}$.

Hence,

$$f(\tau/\bar{a}) = \frac{f(\bar{a},\tau)}{f(\bar{a})} = \frac{1}{\sqrt{\sigma_T^2\sigma_A^2/(n\sigma_T^2+\sigma_A^2)}\cdot\sqrt{2\pi}} \times e^{-\frac{1}{2}\left\{\frac{\tau-[(n\sigma_T^2\bar{a}+\sigma_A^2 e)/(n\sigma_T^2+\sigma_A^2)]}{\sqrt{\sigma_T^2\sigma_A^2/(n\sigma_T^2+\sigma_A^2)}}\right\}^2} . \tag{7}$$

From equation (7) we see that the conditional distribution of τ, given a particular value of $\bar{a}$, is also normally distributed. It is clear that the best estimate we can make of the true claims level is the mean of this distribution (which is also its mode). Hence, if t is our estimate of the true claims level, we have

$$t = \frac{n\sigma_T^2\bar{a} + \sigma_A^2 e}{n\sigma_T^2 + \sigma_A^2} = \frac{n}{n + \sigma_A^2/\sigma_T^2}\cdot\bar{a} + \frac{\sigma_A^2/\sigma_T^2}{n + \sigma_A^2/\sigma_T^2}\cdot e . \tag{8}$$

Thus, on the basis of our assumptions, the estimate of the true claims level is a linear combination of the actual and expected claims levels, and the credibility factor k is given by

$$k = \frac{n}{n + \sigma_A^2/\sigma_T^2} \tag{9}$$

(which is independent of the actual claims level), where n is the number of observation periods.

Thus, the constant c in the theoretically derived formula $k = n/(n + c)$ is simply the ratio of the variance of the distribution of actual claims, σ_A^2 (an index of the credibility of the actual claims level), to the variance of the distribution of true claims levels, σ_T^2 (an index of the credibility of the manual rate structure within the large block of cases we are concerned with).

Large values of σ_A relative to σ_T tend to reduce the value of k; in such instances we should pay relatively less attention to the actual claims and put more faith in the manual rate structure. Small values of σ_A relative to σ_T tend to increase the value of k; here we should pay relatively less attention to the manual rate structure and put more faith in the actual claims level. If $\sigma_A = \sigma_T$, our a priori knowledge (i.e., the expected loss ratio relative to manual rates) is about as credible as the actual experience of one observation period, and we simply weight the actual and expected claims levels on the basis of the number of "observation periods" (i.e., $n/[n + 1]$ and $1/[n + 1]$).

This discussion is purely theoretical and makes no comment on the applicability of its assumptions to the real world. Nor does it comment on the appropriateness of assuming that the constant c is invariant by size of case in the commonly used formula $k = n/(n + c)$, where n is a measure of exposure.

This last formula can be traced to a paper published in 1918, where it emerges after the author makes several approximations to more complex formulas derived on a theoretical basis (Albert W. Whitney, "The Theory of Experience Rating," *PCAS*, IV [1918], 274). Another important background paper on the subject of estimating true claims levels is Mr. Keffer's paper "An Experience Rating Formula" (*TASA*, XXX [1929], 130).

WILLIAM J. SCHREINER:

Mr. Maguire has presented a very neat, practical formula for obtaining k, the credibility factor, in the formula

$$t = ka + (1 - k)e .$$

At the same time, however, the paper stimulates doubts with respect to the validity of the classical assumption that values of k are restricted to the interval between zero and unity.

The accompanying table illustrates data for which values of k greater than unity and less than zero are obtained with Maguire's technique: for Set A, $k = 2$; for Set B, $k = -1$.

Rather than dismissing Maguire's approach out of hand as permitting a result inconsistent with our preconceptions, let us re-examine our preconceptions. If we require k to lie between zero and unity, the value of the true claim rate, t, is restricted to the interval between a and e. General reasoning and experience tell us, however, that it is quite possible for the true claim rate to lie outside that interval. Indeed, the less our confidence

Risk No.	Actual Loss Ratio		Expected Loss Ratio	Deviation (Actual-Expected)	
	Period A	Period B		Period A	Period B
			Set A		
1	55	60	50	+ 5%	+10%
2	60	65	55	+ 5	+10
3	65	70	60	+ 5	+10
4	70	75	65	+ 5	+10
5	75	80	70	+ 5	+10
Total	....	....	....	+25%	+50%
			Set B		
1	55	45	50	+ 5%	− 5%
2	60	50	55	+ 5	− 5
3	65	55	60	+ 5	− 5
4	70	60	65	+ 5	− 5
5	75	65	70	+ 5	− 5
Total	....	....	....	+25%	−25%

in the predictive value of the expected result, e, the more likely it is that the true claim rate lies outside the interval between a and e.

It seems evident, therefore, that the requirement that k lie between zero and unity, by eliminating potentially correct solutions for t, casts doubt on the ability of the classical credibility formula to attain its objective with respect to predicting the true claim rate. However, in spite of this theoretical defect, it is possible that the classical credibility formula has value on practical grounds. Let us investigate this possibility.

Paradoxically, it is only in situations in which one believes that the expected result may *not* be the proper measure of the true claim rate that the credibility formula has any potential utility. As Maguire points out, if one believes that the expected result is the proper measure of the true rate, one would assign zero credibility to the actual experience and the formula would serve no useful purpose.

For values of k greater than zero and less than unity the classical formula suggests the following actions:

1. $a > e$: Charge a premium lower than actual results but greater than the prior estimate.
2. $a < e$: Charge a premium greater than actual results but less than the prior estimate.

In the first case, the indicated prospective premium is less than the amount that actual past experience indicates was required. Such a course of action, obviously, invites additional loss. (It also reminds me of the Chinese proverb: Fool me once—shame on you. Fool me twice—shame on me.) Conversely, when the actual result is less than the expected, do we wish to reduce the premium as long as the experience refund device exists to permit the policyholder's net cost to be adjusted to reflect his actual experience? Although the credibility formula indicates this course of action, to do so clearly increases the chance of future loss. Therefore, it seems that, in each instance, the classical formula dictates a course of action that is in disagreement with sound business judgment.

An additional practical problem must be considered with respect to the application of credibility formulas to health insurance coverages. How does one determine the level of credibility for particular coverages? Surely the level of credibility applicable to the experience obtained with respect to a $20 room-and-board benefit for a group of steelworkers in Pittsburgh is different from that applicable to a $50 benefit available to accountants in Los Angeles. In view of the wide variation in benefits, location, industry, size, and the like, found from case to case, it would appear that such an effort holds little reward to reliable success.

I feel that one is compelled to conclude, on both theoretical and practical grounds, that the classical credibility formula is inappropriate for its task. While the jury is still out with respect to the efficacy of the more general credibility approach in solving for the true claim rate, I believe that we are indebted to Maguire for presenting us with a tool that gives every indication of permitting more satisfactory solutions than those to which we have become accustomed. I join with him in looking forward to the benefit of additional comments on this subject, which, after all, is at the very heart of the insurance enterprise.

GEORGE J. VARGA:

It may be of interest to make a comparison of the credibility factors obtained by two other methods with those set forth by Mr. Maguire in his Table 2 for uniform amounts of insurance.

Method 1

$$z = aY^b ,$$

where z is the credibility factor, a and b are constants, whose values are to be determined, and Y is the life years of exposure.

To obtain the values of the constants a and b, the following equations were solved:

$$10.1 = a(250)^b ,$$

$$51.9 = a(8{,}750)^b ,$$

where 10.1 and 51.9 represent the credibility percentage factors for the midpoints 250 and 8,750 of the ranges 0–499 and 7,500–9,999 in Maguire's paper. This produced the following values: $a = 0.795$; $b = 0.460$. The formula can now be written as $z = 0.475\ (Y^{0.460})$.

TABLE 1

No. Life Years in Period A	Midpoint	Credibility Factors		
		Maguire's Factors	Method 1 ($z=aY^b$)	Method 2 ($z=c\sqrt{Y}$)
0–499	250	10.1%	10.1%	10.1%
500–749	625	16.3	15.4	16.0
750–999	875	20.8	18.0	18.9
1,000–1,499	1,250	17.4	21.2	22.6
1,500–2,499	2,000	22.6	26.3	28.5
2,500–3,499	3,000	31.7	31.7	35.0
3,500–4,999	4,250	44.5	37.2	41.7
5,000–7,499	6,250	49.4	44.5	50.5
7,500–9,999	8,750	51.9	51.9	59.7
10,000–24,999	17,500	44.0	71.4	84.5
....	24,460	N.A.	83.3	100.0
....	36,366	N.A.	100.0	....

Note.—N.A. = Not available.

Method 2

$$z = c\sqrt{Y} = cY^{1/2} .$$

This is a special case of Method 1 found by letting b have the value of $\frac{1}{2}$ and where c is a constant whose value has to be determined.

To obtain the value of c, the following equation was solved:

$$10.1 = c\sqrt{250} ,$$

where 10.1 represents the credibility percentage factor for the midpoint 250 of the range 0–499. This gave c a value of 0.639. The formula can now be written as $z = 0.639\ (Y^{0.500})$.

The results obtained are shown in Table 1. As can be seen from Table 1, Method 1, by requiring the use of two constants whose values are determined from credibility factors desired for specified upper and lower midpoints, will reproduce the desired credibility factors for the upper and lower midpoints selected and, in addition, will give a consistent set of credibility factors for the midpoints that fall between the selected upper and lower limits. This property of the formula becomes a useful tool when, for competitive or other reasons, it is desired to reach full credibility at a specific upper limit.

The same approach was used for obtaining credibility factors for actual amounts of insurance, with the results shown in Table 2. Inasmuch as the constants in Method 1 were determined from the credibility factors obtained by Maguire for midpoints 250 and 8,750, it follows that selection of credibility factors for other midpoints could produce different values and, therefore, different credibility factors. The column labeled

TABLE 2

Midpoint	Maguire's Factors	Credibility Factors		
		Method 1 ($s=aY^b$)	Method 2 ($s=c\sqrt{Y}$)	Alternate Method 1
250	13.8%	13.8%	13.8%	9.6%
625	15.6	20.4	21.8	15.6
875	18.3	23.5	25.8	18.6
1,250	19.0	27.3	30.9	22.5
2,000	21.6	33.4	39.0	28.8
3,000	30.6	39.7	47.8	35.6
4,250	37.5	46.0	56.9	42.8
6,250	54.2	54.2	69.1	52.4
8,750	62.5	62.5	81.6	62.5
13,110	N.A.	74.2	100.0	77.3
17,500	58.6	83.9	....	90.0
26,450	N.A.	100.0	....	Over 100.0

NOTE.—N.A. = Not available.

"Alternate Method 1" is the result of substituting the credibility factor 15.6 per cent of midpoint 625 in place of the 10.10 per cent credibility factor of midpoint 250 for calculating the value of the constants a and b. The credibility factors produced by this substitution appear to be closer to those found by Maguire than those produced by Method 1.

ALLEN L. MAYERSON:

In his conclusion, Mr. Maguire states that "any determination of credibility factors must be based on an empirical approach." It is distressing that this rather dubious conclusion has been reached just at the time that most members of the Casualty Actuarial Society, after using a semi-empirical approach to credibility for more than fifty years, are beginning to build a solid and consistent theoretical framework for credibility theory, using the statistical insights provided by the Savage de Finetti school of statistics.

Maguire states that "there is reason for skepticism regarding the assumption that credibility factors vary by risk size according to the simple relationships that have gained rather wide acceptance, such as $k = N/(N + C)$." I used to share his skepticism, until I began to study the work of Arthur Bailey, particularly his 1950 landmark paper entitled "Credibility Procedures" (*Proceedings of the Casualty Actuarial Society*, Vol. XXXVII). In my 1964 paper, "A Bayesian View of Credibility" (*PCAS*, Vol. LI), I was able to combine some of the modern statistical techniques with Arthur Bailey's ideas on credibility to show that the formula $Z = N/(N + C)$ does give the credibility of the claim frequency, on the assumption of a linear relationship between what Maguire calls the "true claim level" and the actual claim level. C turns out, however, not to be a constant, but a function of the mean and variance of the prior distribution which describes the "expected claim level." (It may be petty to quibble about notation, but I cannot help mentioning that the use of Z, rather than k, for credibility, is as well accepted by casualty actuaries as the use of A_x by life actuaries for the whole life single premium.)

The usual formulas for credibility are, in fact, deficient, but Maguire's approach does not appear to confront that problem, namely, the application of a credibility formula based on the distribution of the number of claims to the credibility of the net premium or claim cost. F. S. Perryman, in his 1932 paper, "Some Notes on Credibility" (*PCAS*, Vol. XIX), showed how to avoid this difficulty on the assumption that the net premium, the product of claim frequency and average claim cost, is normally distributed. A new paper, "On the Credibility of the Pure Premium" (*PCAS*, Vol. LV) by Mayerson, Jones, and Bowers, discards the assumption of a normal distribution and derives the number of claims required for full credibility in terms of the moments of the claim frequency distribution and the moments of the distribution of claim amounts.

If no mathematical theory were available, either in the statistical or actuarial literature, an empirical approach to credibility might be justified. Maguire's paper fails to convince me, however, that his method is better than the theory painstakingly built up over the past sixty years, summarized in Longley-Cook's "Introduction to Credibility Theory" (*PCAS*, Vol. XLIX), discussed at the 1967 Yale Conference sponsored by the Society of Actuaries Research Committee and under constant study by those concerned with improving the mathematical foundations of actuarial practice.

JAMES C. HICKMAN:

Most actuarial topics may be discussed on one of three levels—the practical, the theoretical, and the philosophic. Credibility is not an exception to this statement. Indeed, it would be hard to select an actuarial topic which can be discussed with greater profit on each of these levels.

Credibility was developed to fill an imperative business requirement. Although the reasoning may seem at times a bit obscure, one cannot read the pioneering papers on credibility by Whitney,[1] Perryman,[2] and Bailey[3] without a degree of admiration. In these papers the reader will witness a valiant struggle to develop a practical method, and a supporting philosophy, for blending expected claims levels with observed claims levels for the purpose of forecasting future claims levels. The development of the concept of credibility may well be the most significant contributions of North Americans to actuarial thought.

One of the most exciting intellectual developments in actuarial science in recent years has been the construction of a mathematical foundation under existing credibility procedures. With the development of acceptable foundations for personal, or subjective, probability, it has become possible to recast most credibility procedures as linear approximations, in the sense of least-squares, to the mean of a posterior distribution, given observations of claims levels and a prior distribution which summarizes the initial knowledge and opinion about the claims level. The papers by Mayerson[4] and Buhlmann[5] provide excellent introductions to this line of thought.

The author of this paper has set for himself the task of developing an empirical approach to the determination of credibility factors. This task seems to be in keeping with the motto of the Society of Actuaries and is in line with the viewpoint of the British school of empirical philosophers. Nevertheless, when it comes to the determination of credibility factors, it seems fair to ask if a completely empirical approach is possible.

The trouble starts, I suppose, with the definition of the term "true claims level." The notion of a true claims level is certainly a useful actuarial abstraction. In the Bayesian approach to credibility theory, the actuary is required to quantify his knowledge and opinion about the claims level in the form of a distribution of probability. Then this distribu-

[1] A. W. Whitney, "The Theory of Experience Rating," *PCAS*, V (1918), 4.

[2] F. S. Perryman, "Some Notes on Credibility," *PCAS*, V (1932), 29.

[3] A. L. Bailey, "Credibility Procedures," *PCAS*, V (1950), 38.

[4] A. L. Mayerson, "A Bayesian View of Credibility," *PCAS*, V (1964), 5.

[5] H. Buhlmann, "Experience Rating and Credibility," *ASTIN Bulletin*, V (1967), 4.

tion is modified, through the mechanics of Bayes's theorem, as claims experience is revealed. Within the classical, pre-Bayesian approach to credibility, the concept of "full credibility," rather than a prior distribution, plays a key role. In this approach the actuary is required, with the help of a mathematical model which he constructs, to state the number, or perhaps the amount of claims, which he will require to be observed before the true claims level may be estimated, with "practical certainty," based on the observed claims alone. Partial credibility is then assigned to volumes of claims data less than that which deserves full credibility by some reasonable and convenient formula. In both these models it is recognized that in this finite world we can observe only a limited amount of claims information; the true claims level will always be discerned only through a haze of random error. The haunting possibility that significant shifts may have occurred in the basic claims distribution, and as yet may not have been detected, also clouds the estimation of the true claims level.

The author's approach has great initial appeal because these troublesome subjective elements (the prior distribution of the Bayesian approach and the assignment of full credibility in the non-Bayesian approach) are apparently avoided. The remaining question is whether, after extracting the mathematical risk models and the subjective elements from credibility, there remains a viable concept.

Perhaps it would be wise to pinpoint some steps in the author's development which make his approach distinctive and which in turn create questions about whether he is discussing credibility or an index-of-time trend in claims levels.

1. It is stated that equation (2) may be useful in estimating the true claims level. It appears that this would be true only if the credibility factor had previously been estimated correctly. Yet the estimation of k is, in fact, the objective of the development.

2. Equation (5) is said to be useful in estimating the credibility factor k, yet equation (5) depends on knowing the values of t_i and there is no way within the usual probability models for insurance systems to *know* the value of true claims level with *certainty*. If, in fact, it were possible to estimate satisfactorily the value of t, the motivation for estimating k would be gone. If what is sought is only an estimate of the average deviation between t and e, for n risks, when it is acknowledged that the sample average of such deviations will have a rather large variance, there can be no objection to the computation. This interpretation does not stress, however, the problem of setting the criterion by which different risks may be classified with precision into groups with characteristics which will produce equal credibility factors.

3. Comment (*b*) following equation (5) relates to the behavior of the estimate of the credibility factor if there is "no correlation between expected and true claims levels." This appears to be an interesting remark, and it would be helpful in appreciating the full force of the remark to learn the assumptions about the distributions of t, e, and a which lead to the indicated conclusion.

4. In discussing equation (6) the author acknowledges that it would be desirable for a credibility factor to increase with the size of the risk. Since it is less apparent that this will be true for the estimate of the credibility factor that he proposes than for the traditional type of factors which are listed, it is difficult to appreciate the expression of skepticism about the traditional partial credibility formulas.

5. In commenting on equation (7) the author states, quite correctly, that D_i^B is an unbiased estimate of $(t_i - e_i)$. However, one is moved to ask if unbiasedness is all that we seek in an estimate of $(t_i - e_i)$. It appears that equation (7) might also be described as an index of the deviations in actual and expected claims levels between two time periods.

The author's view is summarized in his statement, "It appears to the author that measurement of the imperfection in the expected claim level determination can only be accomplished empirically." Certainly no one who places faith in a scientific approach to insurance problems can disagree with this statement. Yet because of the inherent incompleteness of our observations and the dynamic nature of the insurance world, it may be that these observed deviations can be understood only insofar as they tend to support or refute some mathematical model.

The operational use of these observed results to adjust future price-benefit structure may be more coherent if the observations are used to adjust and alter a consistent model rather than employed directly in making such adjustments.

DONALD A. JONES:

A discussion of any paper can only be the discussant's reaction to his own interpretation of the author's paper. I hope that this discussion is a reaction to something near the author's intention.

The author starts from the generally accepted view of credibility theory as that set of principles which cover the blending of a risk's own experience with insurer's prior knowledge of this risk and of others with common characteristics. Within this theory there have been two formulations of principles. The older of these, as described by Whitney,[1] Bailey,[2]

[1] A. W. Whitney, "The Theory of Experience Rating," *PCAS*, IV (1918), 274.

[2] A. L. Bailey, "Credibility Procedures," *PCAS*, XXXVII (1950), 7.

and Mayerson,[3] was developed by methods of inverse probability and has received sound statistical foundations by the development of Bayesian statistics in the past decade. The other formulation is built around the "standard" equation given by the author as estimate of $t = ka + (1 - k)e$ (equation 1).

Here, the credibility factor, k, is read from a curve which has been determined to give desired values at two different amounts of exposure. Descriptions of some aspects of this formulation may be found in Perryman,[4] Longley-Cook,[5] and Mayerson, Jones, and Bowers.[6] To this existing theory the author has added another formulation and, by implication, has suggested that an adequate formulation of credibility should have a feedback property to describe fully the dynamic aspect of the insurer's operations. I think that the author's suggestion of a more dynamic formulation of credibility theory is timely and important; however, I find his formulation too narrow and arbitrary to be preferred to that given by Bayesian statistics.

My preference for the Bayesian credibility theory is based on its following three advantages. First, the Bayesian estimate of the (true) claim level is given by the regression curve, that is, $E[t \mid a]$, the expected value of t, given the observed claim level a, which may be—or may be approximated by—but is not restricted to linear expressions. The author's formulation cannot accommodate nonlinear estimates of the claim level. Second, the Bayesian formulation has its natural setting in a general statistical theory where the developmental work of research statisticians is available. I do not see how the author's formulation can be classified as a method in any of the existing schools of statistics. Third, I believe that the Bayesian formulation is more objective than that of the author. This comparison of the two formulations may seem contrary to the vernacular of "subjective probability" and "empirical approach" which are associated with the formulations. However, as is generally true, the Bayesian method, which requires the prior specification of subjective input, can be more objective than a method which consists of the insertion of data into a formula.

As an illustration of the subjective elements which are submerged in the application of the author's formula (7), consider his second example with the modification that the experience is to be obtained by simulation

[3] Allen L. Mayerson, "A Bayesian View of Credibility," *PCAS*, LI (1964), 85.

[4] F. S. Perryman, "Some Notes on Credibility," *PCAS*, XIX (1932), 65.

[5] L. H. Longley-Cook, "An Introduction to Credibility Theory," *PCAS*, XLIX (1962), 194.

[6] A. L. Mayerson, D. A. Jones, and N. L. Bowers, "On the Credibility of the Pure Premium," *PCAS*, Vol. LV (1968).

rather than from actual group experience. (1) For what numbers of life years, say E, shall we determine the credibility factor k? (2) How many groups (author's n) should we observe at each selected value of E? (3) For each group what should be the expected claim level, e_i, or, for this group life example, the age distribution? (4) What should be the distribution of expected claim levels for the collection of groups at a given number of life-years exposure? (5) What should be the relationship between the exposure amounts in the A and B experience periods? All five of these questions, which would be settled subjectively for simulated data, are settled automatically as the experience is gathered on actual groups. In other words, when the actuary accepts the real groups' experience as adequate to determine credibility factors, he is settling the five questions listed above.

There are two more decisions which must be made regardless of the data source. For each level of exposure the derived credibility factor must be acceptable (another subjective criterion!). It may seem that the only objective action is to accept the factor as derived, but what if this factor is greater than 1? Or negative? In addition to each factor being acceptable alone, the aggregate of factors for all exposure levels must be acceptable. As the author indicates with regard to Table 2, there is an "erratic pattern for large groups" which is not acceptable. While the author suggests smoothing this set of factors by combining the experience of several companies, other actuaries would have their favorite smoothing techniques to employ here.

Let us return to the author's idea of a feedback property in our credibility theory. By definition, credibility theory is concerned with the feedback of a group's past experience into the determination of its expected future claim level. The author has suggested that we might also use the past experience of several groups to determine *how* a single group's past experience should affect the estimate of its expected future claim level.

To illustrate the meaning of this in the Bayesian formulation, we will consider the following history. A company issues a group with expected claims in the first period of $E[X_1]$. The group's total claims in the first period are x, so the expected claims in the second period are $E[X_2|X = x_1]$. Now assume that a second group with expected claims $E[Y_1] = E[X_1]$ is insured. If this second group's first period claim amount is y, the usual Bayesian formulation says that the expected claim amount for the group's second period is $E[Y_2|Y_1 = y_1]$. The author has suggested that the relationship between the claim amounts in the first and second periods for the first group is information relevant to the determination of the expected claim level in second period for the second group. In symbols we seek $E[Y_2 | Y_1 = y_1, X_1 = x_1, X_2 = x_2]$.

The author has placed an important, but difficult, challenge before us when he calls for an explicit algorithm covering the input of group experience to the structure of credibility theory.

(AUTHOR'S REVIEW OF DISCUSSION)

RALPH D. MAGUIRE:

I wish to thank all those who have contributed discussions on this paper. They have both added valuable material and brought out differing viewpoints.

In his discussion Mr. Jackson raises a question on whether the estimation of credibility factors on the basis of actual and expected loss ratios takes into account enough of the pertinent facts. He cites as an example two group life cases with the same number of lives, premium volumes, and expected loss ratios but with insurance schedules which would be expected to produce differing fluctuations in actual claim results. Jackson correctly points out that the credibility accorded actual experience should be greater on the risk with the insurance schedule with the inherently lower fluctuation in actual claim results and that the dispersions about the mean should be much smaller on that risk. However, the only basis that I can find for his conclusion that "such differences might not show up in actual loss ratios, expected loss ratios, or their differences" is that the samples of risks with the two types of schedules might be too limited to bring out the appropriate differences in credibility factors. This is a practical problem inherent in any estimation that is based on a limited volume of data. I would agree that the insurance schedule is very clearly a characteristic of the insurance coverage which should be considered in the determination of credibility factors, depending on the volume of available data and on other practical considerations.

Jackson also indicates reservations about the validity of the assumption that the credibility factor for a given risk depends on certain aspects relating to that risk and that it is "equally applicable to all deviations of actual from expected claim levels, whether large or small, positive or negative." He offers an illustration to support his statement that "as the deviation increases, the possibility of that deviation's being due to chance fluctuation about the expected value diminishes, so that the probability that the true loss ratio is significantly different from the expected must therefore increase." I agree with this statement and find it consistent with the results of applying formula (1) to a group of risks with the same credibility factor; we would estimate the largest deviations of true from expected claim levels on those risks with the largest deviations of actual from expected claim levels. I therefore do not believe that Jackson proves his contention that "the absolute size of $(a - e)$ has some bearing on the credibility factor." I might add that the assumption that the credibility factor for a given risk is equally applicable to all deviations of actual from expected claim levels is a property of credibility factors implied by the standard application of formula (1) and that my formula (7) is intended to produce credibility factors which on the average satisfy this condition.

Jackson expands on my hypothetical example, Table 1, with some hypothetical background which aptly illustrates his point that "a substantial number of cases has to be included in the calculations before formula (7) will produce a reasonable estimate of the credibility."

Mr. Shur has presented a particularly valuable and interesting discussion. His derivation of the formulas $t \cong Ka + (1 - K)e$ and $K = n/(n + \sigma_A^2/\sigma_T^2)$, according to some simple but not unreasonable assumptions, is both significant and enlightening. I was particularly pleased that his formula for K enabled Shur to reach the same general conclusions that I expressed in my paper about the effects on credibility of chance fluctuation in actual claim levels and of imperfection in expected claim level determination.

Mr. Schreiner's discussion centers around the traditional restriction that the credibility factor must be in the interval between zero and unity. His alteration of my hypothetical example illustrates that formula (7) can produce values on either side of this interval. From a theoretical point of view I believe that such a result would be perfectly proper under certain special circumstances. As an example, consider an employer with a number of divisions of equal size. Suppose that each year the employer concentrates on improving the mortality of those divisions which had worse-than-average group life claim experience in the preceding year, through free medical care, periodic medical examinations, careful screening of new applicants, and the like, and that the employer goes to the opposite extreme on those divisions which had better-than-average experience. If these measures are sufficiently influential, there would be a tendency for each division to swing back and forth each year, from better-than-average to worse-than-average to better-than-average, and so on. Under these conditions the "true" credibility factor for each division would be negative, and formula (7) would tend to produce a negative result. It is possible also to concoct an example in which the "true" credibility factors would exceed unity, but I believe that we shall rarely, if ever, encounter situations in actual practice where credibility is outside the traditional interval.

Schreiner also discusses the limitation of credibility factors to the interval between zero and unity from the point of view of setting a rate level for the future. Although I agree with him that practical considerations might lead to the establishment of renewal rate levels which imply credibility factors outside the traditional range, or at least different from the "true" factors, I believe that the most meaningful approach to renewal rating is to estimate separately the future claim level, expenses, and contingencies and then to consider the competition and other practical aspects.

Mr. Varga has developed various alternative tables of credibility factors by fitting two different formulas to my results at various risk sizes. This is one of several available techniques for both smoothing and extrapolating on the actual results.

Before commenting on the specific points raised by Messrs. Hickman, Jones, and Mayerson, it would seem appropriate to deal first with the two rather general and interrelated objections that appear to run through their discussions. One is based on an apparent belief that, in developing a means of determining credibility factors empirically, I have proposed a concept of credibility which is opposed to Bayesian theory. The second is an objection to the use of an empirical approach to the determination of credibility factors and to my suggestion that any determination must have an empirical basis. In responding to the first objection, I must confess in all modesty that it was never my intention to formulate a new theory of credibility. My purpose was merely to develop a formula which would produce factors that would on the average satisfy standard credibility procedures in actual practice. Incidentally, the fact that the starting point in my derivation is a formula which has been validated by Shur in his discussion of my paper according to certain assumptions and through application of Bayes's theorem suggests at least some initial consistency with Bayesian theory. In dealing with the objection to an empirical determination, I would point out that the Bayesian approach to credibility requires the actuary "to quantify his knowledge and opinion about the claims level in the form of a distribution of probability." Whether this quantification is performed by an actuary or a lay person, it should be based on some knowledge. In my paper I point out that this probability distribution (or dispersion) depends on the degree of imperfection in expected claim level determination and that this imperfection reflects a failure to adjust completely and accurately for all the exposure characteristics which affect claim levels. When one considers the myriad and potential interaction of complex characteristics which could influence the claim level on a particular risk, I believe that one is forced to conclude that the only practical means of measuring the degree of imperfection in expected claim level determination is empirical. The approach to credibility factor determination set forth in my paper is based on an implicit measurement of this imperfection.

Mr. Mayerson states that he used to share my skepticism about the formula $K = N/(N + C)$, where N is a measure of risk size and C is a constant. He then points out that in his 1964 paper, "A Bayesian View of Credibility," he developed this same formula according to certain assumptions. Since he also points out that C turns out *not* to be a constant, Mayerson presumably shares once again my skepticism, if not disbelief.

Mr. Hickman raises the question of whether my paper deals with credibility or an index-of-time trend in claim levels. I believe that the answer is, and should properly be, both. The credibility that should be attached to past experience in estimating the current or future true claim level of a particular risk should reflect the extent to which the true claim level varies from year to year.

Hickman appears to question the validity of deriving equations which express one unknown function in terms of other unknown functions. I believe that this is a standard algebraic technique often used to develop an expression for an unknown function in terms of either known, ascertainable, or estimatable functions.

Hickman expresses interest in my comment about the behavior of the estimate of the credibility factor if there is absolutely no correlation between expected and true claim levels. I concluded that equation (5) would tend to produce a credibility factor of 1 in this situation because the dispersion of the true claim levels about the expected would tend to approximate the dispersion of the actual claim levels about the expected.

Hickman states that "the author acknowledges that it would be desirable for a credibility factor to increase with the size of the risk." Even after careful rereading of my paper I do not find any such acknowledgment; I do state that "there should be little doubt . . . that credibility increases with increasing risk size."

In discussing equation (7) Hickman asks if the unbiasedness of D_i^B as an estimate of $(t_i - e_i)$ is sufficient. I believe that this estimate is sufficient and that the resulting formula produces an unbiased estimate of K.

Mr. Jones states that my approach to credibility factor determination implies a "feedback property to fully describe the dynamic aspect of the insurer's operations." I believe that he is referring to my concept of using the past experience of several groups to determine how a single group's past experience should affect the estimate of its expected future claim level. This approach is quite consistent with an accepted actuarial technique—the use of previous mortality experience of a group of persons to estimate future mortality on a group of similar persons.

In listing his reasons for preferring the Bayesian approach over my empirical approach, Jones suggests that a number of subjective elements are submerged in the application of my formula (7). He illustrates this contention by supposing that the experience data to be entered into the formula are to be obtained by simulation rather than from actual experience, and then lists five questions which would have to be settled subjectively rather than automatically settled in the data-gathering process. I do not believe that simulation is a feasible alternative to gathering actual data, because simulation would require a knowledge of the degree of imperfection in expected claim level determination, and I have attempted to show both in my paper and in my review of discussions that this knowledge can only be based on actual experience. To use the mortality table analogy again, I do not believe that simulation can replace the data-gathering process in developing a mortality table.

RETURN ON STOCKHOLDER EQUITY—ACTUARIAL NOTE

THOMAS P. BOWLES, JR.

SEE PAGE 9 OF THIS VOLUME

JAMES G. BRUCE:

Mr. Bowles's Actuarial Note is a very potent package, so lucid that it can be studied and appreciated by those who will learn his definitions and can follow the algebraic rearrangements of his formulas. It will surely be extremely useful to actuaries, but, in addition, key life insurance executives without an actuarial background and financial analysts interested in stock of life insurance companies should be able to master the concepts of this Actuarial Note and to use its definitions and formulas in a practical way.

It is hard to quarrel with the underlying theoretical concepts set out in this paper by Bowles. His stated purposes of (1) aiding communication between actuary and nonactuarial businessmen and (2) recognizing the corporate "whole" in price structure rather than just the plan/age cell "part" should be heartily welcomed, especially by those who have experienced the stress of trying to convince generally capable executives of fundamental truths that are admittedly hard to get across to some of those who have the influence and the power to make decisions and may do so without due reflection on the definitions and relationships among the factors Bowles has so capably set forth.

The author's intent to stimulate further thinking creates a tantalizing wish to see some points pursued further by one who has his broad experience. For example, the five ways of offsetting unfavorable trends totally or in part invite intensive discussion as to the practical means of accomplishing each. Hopefully, by pointing to troublesome blocks to ready adoption of the suggestions, further ideas will be elicited either in the discussion to follow or in the author's reply.

It has not been argued that return on stockholder equity is the sole or even the most compelling force in bringing about a change in price structure, even when the demonstrations permitted by this paper are highly convincing. Indeed, it is given as "one appropriate guideline for pricing and fiscal planning." There is always that specter of competitive position that can be so difficult to put into perspective, especially when vocal and articulate agency executives are on the defensive. The sales executive who is primarily interested in his record and sees price increases as an unjust handicap to his assignment is very likely to resist them with all the arguments and influence that he can muster. Nor can actuaries or other executives respond coldly to his objections, for the sales organization plays a vital role in key items with which this paper deals, such as premiums earned and accelerated investment in new business.

It takes twenty-two words to suggest use of part of capital funds in subsidiary operations in order to achieve a higher rate than the investment portfolio yields. It is difficult to evaluate this solution. It is extremely hard to refute the logic of declaring that, if r_n, the return on stockholder equity, is less than the stated objective, some of the capital funds might be used to yield the stated objective. Since the objective for r_n is clearly to be much higher than i_n, the difference being the reward for risk-taking, how many words would it take to cover the deliberations that must take place before any part of stockholder's equity is put into a new venture? Is it unfair to mention that the decisions are rougher because it has been fairly common practice to proclaim publicly that capital and surplus constitute the evidence of financial strength and the degree of protection for the policyholder? To answer these questions, probably only a treatise on the exercise of sound business judgment and the responsibility of executives to both stockholders and policyholders would suffice. This would make a considerable appendix to an actuarial note.

The solution involving accelerated investment in new business at the higher price levels begs for a revelation of just how to do this successfully. It must be assumed that a strong sales force has been pushing toward higher and higher goals each successive year. The very practical question arises of how much extra production can be effected at the higher price level without a damaging increase in the unit acquisition cost. Higher premiums could be an automatic alibi for poor sales and a justification for higher compensation in order to attract the talent and drive needed to overcome the deteriorating competitive position.

The magnitude and the nature of deliberations and considerations revolving about the possible ways of reducing stockholder equity make this approach fascinating to contemplate but deserving of a separate treatise reaching beyond the realm of purely actuarial aspects.

To increase the interest yield by an aggressively creative investment policy would seem to be an objective that would not have to be stimulated by a demonstration of trends of return on stockholder's equity, because management presumably demands efficiency in the performance of its investment department in line with a philosophy and policy established by the board of directors. Nevertheless, the revelations of this paper should be useful in emphasizing the role that competent investment performance plays in the prosperity of the company and in illustrating the relation between the investment function and other phases of the business.

The foregoing comments, which reflect on the practical aspects of accomplishing what the author suggests, lead to the conclusion that this brief Actuarial Note opens the door to a tremendously broad discussion of what makes a life insurance company tick. It also points to specific areas where much light needs to be shed for analysts who have not grasped realistically the significance of a decline in the gain from "insurance operations," especially if such gain has been determined by the fallacious or inconsistent method mentioned in the fourth paragraph of the Introduction. Pointing out the weaknesses in that method, as Bowles does, might bring a vigorous reaction from those whose own interests would tend to suffer from more realistic assumptions as to where interest credits belong and in what proportion. It is quite startling, for example, to see the change in gains from insurance operations and their ratio to total gains as set forth in Table 1.

Human nature being what it is, can you imagine the sparks that could fly in executive circles when a demonstration apparently shows that the ability of those in charge of insurance operations is responsible for the lion's share of total gains rather than the genius of those handling investments? Does this not mean that the utmost tact and goodwill must be used in making a presentation such as that which is provided by this paper?

Bowles has done an excellent job of marshaling and presenting to actuaries some very helpful ideas concerning return on stockholder equity and its relation to price structure and other elements that he has defined. It now remains for actuaries to detect and employ the most effective means of selling those concepts to nonactuarial executives who will have the power to inaugurate and execute the corrective methods suggested after the trends have been analyzed by the formulas provided.

GEORGE D. CHESTER:

Mr. Bowles's paper presents a very important principle that an actuary must consider when evaluating the future financial prospects of a life insurance company stockholder—the return on stockholder equity. His definition of stockholder equity as stated in the Actuarial Note, however, does not include certain redundancies in actuarial reserves held by some companies, as would be the case if a block of life reserves were valued by the net level premium method at a low rate of interest. I assume that this was done intentionally in order to simplify the mathematics of the paper.

Although five different procedures for improving the return on stockholder equity were listed, it appears that the paper places emphasis on the pricing of insurance products. It states, for example, "What is important to the actuary, however, is the need to introduce these concepts into pricing philosophy." Although this is an extremely important requirement in the operation of any life insurance company, I would question how practical it would be to attempt a significant additional return on equity merely by pricing the products. In my own experience I have found that after a great deal of research into the subject of gross premiums for specific policies the final rates adopted were generally a function of the rates charged by competitors.

In any business, and life insurance companies are no exception, the stockholder is interested in obtaining a reasonable return on his investment. From the insurance company management's point of view, this is no simple task. In the first place, the determination of the gross premiums for the various products, as well as the corresponding cash values, maturity values, settlement options, and the like, must be consistent with the profit objective of the company. In addition to proper price tags, however, each company must have effective corporate planning covering all the phases of the business. No matter how carefully a company may have arrived at its gross premiums, it is necessary to control (*a*) acquisition costs; (*b*) quality of business; (*c*) underwriting; and (*d*) investment objectives, since the lack of proper control in any one of these areas may mean the difference between meeting the necessary profit objective and realizing no profit at all.

My own definition of stockholder equity of an insurance company includes the value of the business in force on the reinsurance basis, that is, on the assumption that no further business would be added in the future. Under this procedure significant ratios are the following:

1. Surplus funds to total stockholder equity
2. Growth rate of earnings on the insurance portion where earnings are determined in a manner similar to the method used in Table 1 of the paper
3. Interest leverage factor defined as the ratio of 1 per cent of total liabilities to total adjusted earnings
4. Ratio of market value of common stocks owned to total market value of the common stock of the company
5. Ratio of invested real estate at its probable market value to total of market value of common stock of the company

From the stockholder's point of view, each of the above ratios is important. If surplus funds represent a high percentage of total stockholder equity and, further, if the company's investment objectives are conservative, the return on the stockholder's investment is apt to be low in spite of good earnings on the business. Also, the appreciation in value of the stock of such a company will be held down by an overabundance of surplus funds.

In one case that I studied, the growth rate of business was about average, and the ratio of surplus funds to total stockholder equity was high. The value of the company was determined on the "going concern" basis, that is, the total earnings were projected for the next ten years, and both the present value of such earnings and the equity ten years hence were discounted to the present by use of a reasonable current interest rate. The results indicated that the prospective stockholder could not afford to pay a price for the stock equivalent to the current stockholder equity.

I might state at this point that many companies have been highly successful financially, not because of the profitability of their insurance business but because of aggressive investment policies. In other words, everything else being equal, those companies which have consistently invested a greater-than-average percentage in common stock have generally shown the greatest growth in stockholder equity. In a few such cases stockholder equity accumulated at an amazingly high rate.

In a situation in which a life company has in its portfolio common stocks equal to the total market value of its own stock, in effect, the stockholder purchases a no-load mutual fund (except for the tax aspects) plus possible growth in earnings on the business. If, additionally, this company owns some invested real estate and has a reasonably good interest leverage factor, the stockholders' prospects may be excellent for the near future.

ROBERT G. ESPIE:

Mr. Bowles's Actuarial Note, "Return on Stockholder Equity," is a very useful first step toward the design of a management tool that should be available to life insurance companies and that has not, unfortunately, been available in the past. In the determination of alternative uses of investable funds, the traditional attitude of life insurance management has simply been that the company should write as much profitable business as its surplus can stand. Apparently little thought seems to have been given in the past to the return on investment in such operations, in comparison with the return from alternative uses of funds available for investment. Under circumstances where the alternatives have been limited to the return on bonds, mortgages, and stocks suitable for the investment of insurance funds and insurance surplus funds and where it has been assumed that expansion of life insurance operations is more profitable than investment in securities, management's decisions appeared to have been based on impressions rather than demonstrations; the strategy has been to write as much profitable business as the company can afford, without inquiring too carefully into the economic implications.

Currently, the advent of diversification and the recognition that profit can be obtained from leveraging capital funds mean that more precise measurements are needed. The decision to invest in a diversification can be intelligently made only if one can determine whether diversification yields a better return on investment than investment in expanded life insurance. The decision to finance expansion of life insurance operations through the use of "borrowed" capital can be made intelligently only if one can compare the cost of "borrowing," whether in the form of debt securities or of preferred stock, with the return on investment in insurance operations. And this same philosophy pertains to the problem of securing equity capital in the capital market, since success in getting and keeping such equity capital depends on the investor's judgment on whether he will get a better return from insurance stocks than he can get from alternative avenues of investment.

Bowles has demonstrated that return on investment in a life insurance company is a composite of one kind of return from insurance operations and another kind of return from investment of surplus funds in securities, and he has developed some useful algebra to demonstrate the interrelationships of the components of the composite return. He has, however, avoided the problem of determination of the data on which the insurance-operations components depend. Until this determination is made, his algebra cannot be effectively utilized.

Bowles has assumed, at least implicitly, that the unamortized investment in new business can be equated with the adjustment to earnings for which investment analysts and others are currently seeking a rational and practical formula. I wish to comment on this assumption.

Earnings adjustments may involve different concepts from those of determining investment in new business. For example, earnings adjustments necessarily must differ between a net level reserve operation and a preliminary term reserve operation, but the investment tied up and the return on such investment should be independent of the reserve assumptions. The adjustment of reported earnings is dependent on the reserve assumptions—the return on investment depends on cash flow.

If the adjustment to reported earnings does not exactly reproduce the investment determined on a cash flow basis—and most of the current and proposed bases of adjustment would not do so except by coincidence—the earnings adjustment is not satisfactory for the purpose intended by Bowles.

The typical earnings adjustment is designed to correct mismatching of revenue and charges against revenue. This is a different concept from determination of the ultimate profitability, or the present value of future profits, involved when we divert capital into the sale of new business.

To illustrate, let us consider a manufacturing company for which a comparatively reliable calculation may be made that an investment of $10,000,000 in a new operation will yield a return of 15 per cent per annum. Neither that company nor its accountants would contemplate adjusting reported earnings of the operation to bring them to $1,500,000 each year, regardless of the degree of reliability of the forecast of return on investment.

It would therefore appear to me that the determination of unamortized investment in new business should not start with reported earnings and be modified by adjustments to reported earnings but rather that it should start from considerations of cash flow.

A typical nonparticipating ordinary life premium calculation is essentially a discounted cash flow calculation. The present value of future income is equated with the present value of future outgo. Either explicitly, or as a residual, there is a calculation of the present value of future profits. The negative cash flow under the policy is a measure of the investment in it. The later positive cash flows constitute the return on that investment.

To illustrate at the risk of oversimplifying, a $100 annual premium ordinary life policy may involve a negative cash flow of $50 at date of issue. The profit built into the premium calculation may be $5 per year. The return on investment would then be determined from the equation $50 = 5.00\, a_x$, where the mortality and lapse rates are those used in the premium calculation and we must solve for the interest rate, which is the rate of return on investment. But the proper reported earnings may be quite different from those obtained by the deduction of $50 from current expenses and the addition of $5 per year during the lifetime of the policy, even if actual results are completely consistent with initial forecasts.

Where actual results differ from initial forecasts, it becomes even clearer that there will be a difference between proper adjustment of current reported earnings and, in effect, substitution of return-on-investment calculations for that adjustment.

I do not quarrel with Bowles's decision to restrict his discussion of the problem to his main theme, and I do not suggest that my approach eliminates the very thorny problem of how to distinguish between those expenses which are linked to production of new business from those which are continuing overhead or renewal expenses. I simply suggest that the objectives be more carefully defined in areas where such definition may help to clarify the development of appropriate measurements.

It is a corollary of my suggestion that the problem is really twofold. First, we must make as careful a forecast as we can of the return on investment. Later we may very well want to make a separate calculation of what return on investment actually has been, with the advantage of hindsight.

In conclusion, I share in the implicit attitude of Bowles that a start should be made in the investigation of these techniques in order that the managements of life insurance companies may in the future have yet another tool at their disposal for making useful management decisions.

CARLTON HARKER:

Mr. Bowles is to be congratulated for his fine Actuarial Note. Its clarity is perhaps exceeded only by its timeliness.

The concept of return on stockholder equity has found applicability at Piedmont Life for a number of years. Several significant points, however, in which this company's approach differs from that approach discussed in Bowles's paper might be of some general interest:

1. For purposes of internal financial discipline, the company's rather sizable common stock portfolio (or investment fund) is treated as a separate operating division of the company. Piedmont Life, it is to be noted, has about 58 per cent of its assets in common stocks, all of which are matched by surplus or Mandatory Securities Valuation Reserves.
2. The capital funds (as defined in the paper) less the investment fund are allocated to the various operating divisions of the company (ordinary, group, credit, and individual A & S) by the usual carryforward fund accounting techniques.
3. For the ordinary line the desired return on stockholder equity is set at 10 per cent of the ordinary capital fund, where such desired return can be achieved by either
    a) earnings on an annual statement basis, or
    b) specially calculated earnings defined as the capitalized value at the time of issue of all the earnings of the new business issued during the calendar year in question. Earnings which are capitalized are those which emerge from asset share calculations done in the traditional manner. The asset shares are calculated on both a "liberal" and "conservative" basis to provide a range of the most probable result. Appropriate year, age, and policy amount groupings facilitate the calculations using model office techniques.

    The "either/or" approach is used in lieu of the adjusted earnings method presented in Bowles's paper.

The method of Piedmont Life measures the return to the stockholder as a function of his equity, but from a slightly different point of view from that in Bowles's paper. The rationale of Piedmont Life is to treat the sale of a life policy in a manner similar to the sale of a consumer product by recognizing the profit to the stockholder at the time of sale. The long-term aspect of the life insurance contract, which is very often a serious stumbling block to the nonlife insurance person when comparing life insurance operations with mercantile operations, is bridged by this approach. Also such rationale could actually be put into practice by some forms of reinsurance agreements wherein the current year's issues are totally reinsured or sold off for a lump-sum profit.

The present-value approach in some instances might be more readily understood than the adjusted-earnings method. The present-value approach (incidentally, done every other year) has a wealth of by-products. When such asset shares are completely computerized, the production efforts have not been found to be too burdensome.

ABRAHAM HAZELCORN:

Mr. Bowles has focused on the very same area of a life insurance company's operations as have businessmen who seek to acquire life insurance companies. It is, therefore, not only as an Actuarial Note but as good training and business thinking that his paper represents a valuable contribution to the Society. Many who seek to acquire a life company make some assumption on the return on stockholder equity for that portion of the life insurance companies' funds put to work in the business of selling life insurance; they then concentrate on funds which they consider surplus to that life insurance operation. From the mergers and acquisitions and the formation of holding companies with life insurance components in recent years, evidently, many of them have concluded that they can do well with what they consider as funds available currently only to earn the general portfolio rate of the life insurance company.

Therefore, the concept and the mathematics to which Bowles exposes us help us in this basic understanding. While his purpose is to make us aware of the basic financial concept of return on stockholder equity, he does open up the panorama of other general economic concepts which would have to follow logically. The beneficial effect should go beyond that of improving the actuary's pricing technique and philosophy. I believe that he cannot help but be successful in stimulating further thinking in the areas that he has carved out for himself in this paper.

It will no doubt still be difficult to communicate many of the concepts to board members and successful businessmen in the nonlife insurance financial community. We have before us, however, something which will go a long way in reducing that difficulty.

The inbreeding, which has existed in some older insurance companies, has had to give way in the recent past because of the over-all economic and business pressures. This paper goes beyond adjusted earnings—assuming that problem can be satisfactorily solved. It views the whole ball of wax in trying to measure the economic performance of a life insurance company. If as actuaries we use our competence in a narrower field, which Bowles refers to as the "plan/age cell," we will be remiss in many of our responsibilities.

We would, of course, be disturbed if this concentration would, in any way, endanger the fiduciary obligations and proper functioning of a life insurance company. It is clearly assumed that this will not be the case in this consideration. The thought is how to operate better or more efficiently to the benefit of stockholders, and policyholders in the case of participating policies.

The New York State legislation affecting holding companies, to be effective later this year, and all the activity at the NAIC sessions concerning holding companies are again another aspect of the needed concern with what Bowles is actually, in my opinion, treating.

It will, however, be difficult to even guess at the application of these concepts in regard to a new life insurance company venture which is to stand on its own. G_n will be less than zero in this case. It would be necessary to have close control of production and some of the noncontrollable costs when entering a new life insurance company venture, as well as a good measurement of start-up costs. Perhaps this is why we will not see such pure life insurance company ventures in the future as we have in the recent past.

STEPHEN G. KELLISON:

Mr. Bowles is to be congratulated on an excellent paper that points out the desirability in structuring prices for life insurance to recognize the corporate "whole" as well as the plan/age cell "part." Moreover, this paper is a significant contribution in the attempt to make the analysis of stock life insurance company financial results more similar to that of other industries.

My discussion touches upon two interesting aspects of using the return-on-investment method in setting rates for nonparticipating life insurance. This method was originally discussed in Anderson's 1959 paper "Gross Premium Calculations and Profit Measurement for Nonparticipating Insurance."

The approach used in Anderson's paper can be briefly summarized with the following definitions and formulas:

Definitions

${}_tB$ = Book profit for the tth policy year

${}_tV$ = Terminal reserve at the end of the tth policy year

q_t = Probability of termination of the policy during the tth policy year, including all causes (deaths, lapses, surrenders, term conversions, etc.)

i_t = Rate of interest earned during the tth policy year

j_t = Rate of interest used for the tth policy year in discounting book profits to the date of issue

F_t = Persistency and discount factor at the beginning of the tth policy year

Z = Present value of book profits over an n-year period, discounted at rate j_t for $1 \leq t \leq n$

Formulas

${}_tB = {}_{t-1}V - {}_tV(1 - q_t)/(1 + i_t) + {}_tK$, where ${}_tK$ is the sum of several terms, none of which involve reserves or rate j_t.

$F_1 = 1$, $F_{t+1} = [(1 - q_t)/(1 + j_t)] \cdot F_t$ for $t \geq 1$.

$$Z = \sum_{t=1}^{n} {}_tB \cdot F_t \, .$$

The first point was alluded to by the author in his reference to the rather unusual case of a company with issue, underwriting, and sales expenses spread more evenly over the first ten policy years. The return-on-investment method employs the principle that the present value of book profits, Z, is zero when discounted at the yield, or return, rate. In the typical case the book profit is negative for one or two years and then becomes positive. If the book profits are all positive, however, it is impossible to find a yield rate which will make the present value of book profits equal to zero.

Actually, this problem is more prevalent than the rather extreme example cited above might indicate. For example, a high premium form of insurance, such as endowment or retirement income, sold with a fairly large minimum-sized policy will often produce positive book profits in every year, especially at the higher ages. This is true even when it is sold with typical issue, underwriting, and sales expenses.

One does not have to go even this far to find cases in which the return-on-investment method produces unreasonable results. For example, a policy with a "small" initial investment will often produce a very high return on investment and still have very small dollar profit margins, which are probably insufficient.

In view of these problems, several companies have felt compelled to look at alternate profit measures in obtaining consistent rates for a portfolio of plans. One of the most significant aspects of this paper is that it retains the basic philosophy of the return-on-investment approach to setting rates but applies it on a corporate basis, thus at least partially circumventing these plan/age problems.

The second point concerns the impact of reserve bases in the return-on-investment method to setting rates. To the extent that the actual dollar inflow or outflow to the company is not affected by the reserve basis chosen, the ultimate profitability of a policy is independent of the reserve basis chosen, since reserves are merely an internal bookkeeping device which affect only the incidence of profits. This assumption is generally satisfied, except possibly for federal income tax.

It is possible to show that the present value of profits over an n-year period is dependent only upon the beginning and ending reserves, if the book profits are discounted at the same rate as the company is earning, that is, if $j_t = i_t$ for $1 \leq t \leq n$. In this case we have

$$Z = \sum_{t=1}^{n} {}_tB \cdot F_t$$

$$= \sum_{t=1}^{n} \left({}_{t-1}V - {}_tV \frac{1-q_t}{1+i_t} + {}_tK \right) \prod_{r=0}^{t-1} \frac{1-q_r}{1+i_r} ,$$

where $(1 - q_0)/(1 + i_0)$ is defined to be 1,

$$= {}_0V - {}_nV \cdot \prod_{r=0}^{n} \frac{1-q_r}{1+i_r} + \sum_{t=1}^{n} \left({}_tK \cdot \prod_{r=0}^{t-1} \frac{1-q_r}{1+i_r} \right) .$$

Thus the present value of profits is dependent only upon ${}_0V$ and ${}_nV$, not upon ${}_tV$ for $1 \leq t \leq n - 1$.

However, if the book profits are discounted at any rate other than the earned rate, i_t, this result will not hold. To test this result, I considered a 20-pay life policy issued at age 45 with four different reserve bases, as follows:

1. 1958 CSO $2\frac{1}{2}$ per cent—net level
2. 1958 CSO $2\frac{1}{2}$ per cent—CRVM
3. 1958 CSO $3\frac{1}{2}$ per cent for 20 years, $2\frac{1}{2}$ per cent thereafter—net level
4. 1958 CSO $3\frac{1}{2}$ per cent for 20 years, $2\frac{1}{2}$ per cent thereafter—CRVM

In all cases cash values were minimum values on 1958 CSO $3\frac{1}{2}$ per cent for twenty years, $2\frac{1}{2}$ per cent thereafter.

An arbitrary gross premium of $39 per thousand was assumed in all four cases. The present value of profit in each case was $10.26 per thousand. This illustrates the result proved above, since the terminal reserves at duration 0 and duration 20 are equal for the four cases, even though terminal reserves for durations 1–19 vary.

The return on investment differed remarkably in the four cases as follows: (1) 8.7 per cent; (2) 12.0 per cent; (3) 9.7 per cent; and (4) 14.8 per cent. Thus, in the use of the return-on-investment method in setting rates, the choice of reserve basis is extremely significant whenever discounting is done at a different rate than the earned rate.

In summary, my remarks are not directed at the main thrust of the paper, but, hopefully, they will be enlightening in utilizing the return-on-investment method in setting rates. Again the author is to be congratulated for extending this concept so that it will not only be more useful in setting rates but will be of significance in over-all corporate planning as well.

MENO T. LAKE:

With every morning's paper reporting another acquisition of a stock life insurance company by another life or casualty company, by a conglomerate, by a congeneric, or even, more recently, by a bank holding company, it would be difficult to imagine a more appropriate time for the presentation of Mr. Bowles's Actuarial Note. His approach to determining the return on stockholder equity in a life company seems so basic that one wonders why this subject has not received more attention in our actuarial literature. Certainly none of us would invest our personal funds in a venture without a clear fix on what rate we expect to earn; yet, I wonder how many of us in a stock company have ever calculated the return on stockholder equity in our own organizations?

In his introduction Bowles points up a very important distinction between underwriting and investment earnings. With the recent rise in interest rates there has been a strong tendency to attribute all the "excess" of actual investment income over required interest to the investment operations without realizing that the actuary has assumed much of this extra income in setting current premium rates so that it is an essential part of meeting the underwriting earnings goal. Without competitive premiums there may be nothing new to invest, so it becomes difficult to distinguish clearly between investment and insurance earnings.

If any actuary of a stock life insurance company has not attempted to determine the rate of return on his stockholder equity, I urge him to apply Bowles's general approach to his company. He should, however, be prepared for some sleepless nights after he does so, as I venture that, in the majority of cases, he will find that the rate of return has been declining in recent years. The results may well raise more questions than they answer. I guarantee that the experience will be very worthwhile and that he will learn many things about his company's operations.

My own efforts in this direction have raised a number of questions. It is my hope that Bowles's beginning will result in further efforts to analyze critically life companies' earnings, and, with this aim in mind, I would conclude this discussion by raising a number of questions that I feel justify further study:

1. What are the earnings of a life company? I hope that our attempts to answer this question will eventually lead us to a fairly uniform method of determining "adjusted earnings." Certainly our author has been in the forefront in this effort in recent years.

2. What is the "stockholder equity"? Bowles suggests, for simplicity, that this be taken, primarily, to be capital, surplus, and the security valuation reserve. But, if the security valuation reserve is "released," should we not exclude gross unrealized capital gains from the equity? If a "market" position is desirable, should not mortgages and bonds be revalued to current market?

3. When realized capital gains *do* arise, should they be included with other earnings in determining the return on equity? If a company has a regular practice of realizing such gains, there seem to be strong arguments for including them with underwriting earnings—particularly since the stock investments have had a depressing effect upon the over-all yield rate.

4. If the return on equity *is* declining, what steps can be considered to correct this? Bowles lists several excellent alternatives, but each of these seems to open up a whole new field for further research. For example, increasing premiums should help the return on equity. But will it? Or might it result in less business being sold, and hence less funds invested in new business, and actually aggravate the decline in return on equity? Another solution is the reduction in equity through increased dividends to the stockholders. But this presupposes that the stockholders can exceed the company's return on equity elsewhere. Certainly the reduction of the equity should help increase the return shown on the remaining equity. But what is the line beyond which such reduction in equity should not go lest the company's contingency margin be endangered? Does this not require us to determine some better criteria for "adequate" capital funds, in order that we may be able to maximize return on equity without endangering the interests of our policyholders? If we actuaries do not determine them, I fear that others who are less knowledgeable about our business may make these determinations for us.

In concluding these rambling comments, I want to say that I feel that Bowles has given us a start in a direction of critical analysis that we should have taken long ago. I sincerely hope that others will follow his lead, so that we may greatly improve our techniques in determining what I feel to be the most important single measure of a stock life insurance company's performance—the return on stockholder equity.

RICHARD A. LEGGETT:

Mr. Bowles's paper is interesting because it applies to stock life insurance companies a concept commonly used in investment circles but not generally used in our business. The profitability of the company is a responsibility of the actuary, and any new tools that we can obtain for managing this complex problem are welcome. Although all actuaries recognize that it is essential for the premium-rate structure to produce adequate earnings for the company, this paper describes another way of measuring the adequacies of these earnings. Not only is this concept an impressive way to describe company operations to directors, but it also reveals more clearly to the management what is going on.

Bowles refers to a method of showing "gain from operations" as contrasted with "gain from investments" where the latter consists of investment income in excess of that required to maintain reserves. I am surprised that this method is still in use and that many companies are concerned with the decline in "gain from insurance operations" relative to this "gain from investment operations." I had thought that this misleading comparison had been laid to rest long ago; if not, it should be.

There are other ways of analyzing a company's earnings. It seems likely that most companies look at gain from insurance operations separately from earnings on capital funds and that the projected level of this operating gain *is* considered by the actuary in setting premium levels. However, perhaps there has been a tendency for us to look at operating earnings and to be satisfied if they are increasing at a good rate from year to year. This is important but not enough. As company actuaries, we have traditionally been primarily concerned with the operating gain and perhaps have felt that earnings on capital funds were someone else's problem. This attitude has changed in recent years, however, perhaps because we have heard that some people outside the insurance business are aware of our large capital funds and have felt that they could produce a greater return on it than we do. I am sure that the managements of many stock life companies are now well aware of this situation and are considering what can be done to maximize the return on stockholder equity. Perhaps return on stockholder equity is the best measure of how good a job management is doing for the stockholder, because it reveals how successful management is in putting the stockholder's money to work.

If we intend to use such tools to illuminate the inner workings of the insurance business, we should be prepared to advise on questions that are revealed. Bowles rightly points out that the answers to questions on inadequate return depend on more than the classic solution of increasing premium rates. Actually, I have not noticed that this particular solution has been used much in recent years, and I suspect that it may be the last to be tried. Perhaps I am influenced too much by my associates in the marketing side of the business, but it is my impression that, unless a company has a well-controlled agency force, it cannot increase premium rates substantially under today's conditions without suffering corresponding decreases in production that ultimately result in lower gains from operations.

Also, because of the nature of nonparticipating premiums and the effect of old business, earnings are stable and are not quickly changed. For a mature company with a substantial capital fund, it may be impossible to increase the gain from insurance operations by increasing premiums fast enough to maintain a satisfactory return on stockholder equity. Although the premium-rate level or the rate of sales can be increased, both these steps take time and are slow to increase profits. Even if a company does increase premium rates by 5 per cent on new business, this will not produce a substantial change in the gain from operations for several years in a mature company. In Bowles's case history, I would estimate that a 5 per cent increase in premium rate levels would be reflected in about a 10 per cent increase in gain from operations only after five years, or only about 1 per cent of stockholder equity. This is only if you do not lose many of your agents to another company and have decreasing sales as a result. I doubt that we can regulate our pricing to produce a desired return on stockholder equity with much accuracy without other modifications in our business as well. Then the problem becomes one of how else the management can increase the return on stockholder equity more rapidly. There must also be consideration for the prospective policyholder and recognition that he cannot be expected to pay higher premiums to make up for overcapitalization of the company. But, in spite of these considerations, in setting rates we should certainly consider the effect they will have on the return on stockholder equity.

The paper starts with the statement that "traditionally, stock life companies have been considered somewhat unique in relation to other corporate enterprises." I do not think that this precludes our using the concept of return on stockholder equity, but I do think that it is a mistake not to recognize that nonparticipating life insurance is a unique product. In pricing, we face all the forces of competition that exist in any industry, and perhaps more, when it is considered that a substantial part of our competition is from nonprofit organizations. Yet, in our product we guarantee that the insured can continue his insurance at the same price as long as he lives. In a mature company more than half of the premium income five years from now is from business already on the books today. Although it is a desirable goal to try to analyze insurance operations by the same methods that are used for financial analysis of other businesses, it is a mistake not to recognize that there are differences which make it impossible to analyze and control our companies as one would a manufacturing company. (I think that this statement also applies to problems of adjusting company earnings or comparing costs of life insurance policies.)

Although I realize that the paper intentionally omits the question of how to adjust a statutory gain from operations, it seems to me that any statement of return on stockholder equity must include provision for capital gains or losses as part of the return. Not only may capital gains contribute an important part of the return even now in some companies, but use of this concept (return on equity) may encourage the investment of surplus funds in ventures where capital gains become a more significant part of investment income. The inclusion of capital gains in earnings has had more attention recently, and it cannot be ignored in considering the return on stockholder equity.

The problem of producing a satisfactory return on equity is an important one for actuaries of stock companies, and Bowles's paper is very timely. We may do well to apply the method of his case history to our own companies. We have been somewhat fortunate in the last two decades that favorable mortality and particularly favorable investment income have enabled us to reduce premium levels substantially and still produce favorable earnings. I will confess, however, which side of the generation gap I am on by suggesting that interest rates will not keep rising forever and that, when this stops, the ratio of gain from operations to premium income will turn down as in his case history.

I think that Bowles suggested in another paper a few years ago that we might be at the top of the hill on investment income. Although this has not yet proved to be true, sooner or later we will all face the situation of the company in his case history—the ratio of insurance gain to premium income will turn down perhaps in spite of our efforts, which, as he says, compounds the problem of maintaining a good return. It is not too early to start considering the solutions to that problem.

It seems to me that the management of the company in his case history might rightfully infer that they were doing very well in insurance operations. Although they may have been aware of good dollar earnings, perhaps a return on equity of 15 per cent will make a stronger impression. They may conclude that the best way to improve the stockholder return is to use excess capital funds more productively, possibly by expanding insurance operations more rapidly. This may be even more apparent when the gain after taxes is considered, with the relatively higher tax rate on the investment income earned on capital funds. In the long run it looks as if investment in greater sales might be a good move for the case-history company. But the important thing is that this concept focuses the attention of the management on matters which perhaps have been neglected, namely, the management of capital funds. This requires answers to questions of whether assets representing capital funds or a portion of them should be in more speculative investments than those backing the reserves. There is also a question of what portion of surplus it is really necessary to keep in relatively low-yielding liquid investments in order to support insurance operations, or, in other words, what the relation of surplus to premium income should be for a stock life insurance company.

For me, a principal value of Bowles's paper is that it stimulates our thinking on a matter of importance to stock life insurance companies today, a subject on which we have spent too little time.

EDWARD L. ROBBINS:

For the United Life and Accident Insurance Company, Mr. Bowles's Actuarial Note is both interesting and timely. We have recently perfected a method of achieving the goal that Bowles describes.

Our company now has an operational seriatim gross premium valuation program for our direct ordinary in force. The difference between the appropriate statutory reserve and the "gross premium reserve" is the present value of future profits, that is, the value of the business on the books. Substituting this for Bowles's "unamortized investment in new business," we obtain his S_{n-1}. Substituting the "increase in present value of future profits" for his "increase in unamortized investment made in acquiring new business," we obtain G_n.

This substitution is not the intent of his paper, unfortunately, which states in footnote 2 that the value of the business on the books is *not* equal to the unamortized investment in new business. Any excess of the former over the latter must then only be countable once it shows up in the Convention Blank as surplus. This, however, is a matter of accounting philosophy, and there is an argument here on both sides. I personally favor the argument that the stockholder decision should be affected by the future profitability of the lines of insurance above and beyond what has already shown up in surplus.

The two questions which this paper attacks are as follows: (1) How do we calculate the return on stockholder equity? and (2) With what criteria do we act so that we get the desired return?

Question 1 is resolved elegantly. The sample history shown by Bowles's analysis is quite revealing. The answer to question 2 is not so apparent. In the following analysis, I hope to show a method of resolving question 2, at least for insurance operations.

For any given year, to achieve $r_n = (S_n + \text{Shareholder dividends})/S_{n-1} - 1$ as the rate of return on stockholder's equity, you must satisfy the equation

$$S_{n-1} + i_n S_{n-1} + G_n - \text{Shareholder dividends} = S_n$$

where

$$G_n = R_n - i_n S_{n-1}{}^{1} - \text{Dividends on preferred stock}$$

$$+ \begin{Bmatrix} \text{St. Res.}_n - \text{GPV}_n \\ \text{Renewal business} \end{Bmatrix} - \begin{Bmatrix} \text{St. Res.}_{n-1} - \text{GPV}_{n-1} \\ \text{New + Renewal} \end{Bmatrix}$$

$$+ \begin{Bmatrix} \text{St. Res.}_n - \text{GPV}_n \\ \text{New business} \end{Bmatrix}, \text{ less increase in provision for deferred taxes}$$

(St. Res._n is appropriate statutory reserve, end of year n, and GPV_n is gross premium reserve, end of year n).

If you have good historical factors developed with regard to the positive effect of $1,000 of renewal business on R_n, the negative effect of $1,000 of new business sold on R_n, and gross premium valuations broken down by first year and renewal, it is only a problem of analyzing the last item,

$$\begin{Bmatrix} \text{St. Res.}_n - \text{GPV}_n \\ \text{New business} \end{Bmatrix},$$

to figure what amount of insurance needs to be sold, and/or how to price it. All other items in the equation are pretty well either estimable or matters of history. Projecting this into the future should give a reasonable guideline as to volume and pricing expected.

Substituting everywhere the unamortized investment in new business for "value of business on the books," we can obtain a method more consistent with Bowles's paper.

[1] Defined using "value of business on the books."

STUART A. ROBERTSON:

Mr. Bowles earns our thanks for this stimulating discussion of a matter that is of great importance in establishing sound pricing theory. Among the particularly troublesome questions for the actuary responsible for setting his company's nonparticipating premium rates are those relating to the proper level of profit and to the most suitable means for distributing an over-all profit objective among the plan/age cells. Examination of the concepts so ably explored in this paper may lead to solutions involving formulation of profits in terms of return on stockholder equity—an approach which is, in my view, entirely reasonable.

When it is to be applied to the practical problem of pricing, some improvement in the concepts would appear to be achieved by recognizing some secondary principles, including (1) the fact that stockholder equity is the sum of several distinct and different elements; (2) the fact that the rate of return that may be deemed suitable for one element of the stockholder equity will not necessarily be the same for each of the other elements; and (3) the fact that the business in particular plan/age cells will be dependent upon the several stockholder equity elements in differing degrees.

The most important pieces of the stockholder equity are (*a*) the adjusted capital funds, (*b*) the unamortized investment in acquisition of new business, and (*c*) an important and typically large item that has not been included by the author—the investment in agency plant. In a typical company, this last item is a large one, and provision for a fair rate of return on it will have a significant effect upon price. This is consistent with the view that expenditure for acquisition of new business and expenditure for expansion of agency plant, while related and sometimes difficult to distinguish, should be recognized independently when formulating pricing theory. It is perfectly proper to include as acquisition expense the cost of *maintaining* an agency plant at its existing stage of development; in addition, each segment of new business should pay its fair share toward providing a return on the investment in that plant.

What is a suitable rate of return for each of the major elements of the shareholder equity? There is no unique answer, and the scope of the paper clearly precludes the need to pursue specific values. My point in suggesting principle 2 is that the rate of return may reasonably differ for the different elements. In each case a proper rate of return will be related to the risk of loss. Specifically, consider the unamortized investment in new business; here the rate of return that is reasonable will be a function of the confidence we can place in the assumptions underlying the establishment of the premium. Our profit analysis or asset share studies show that the investment in a block of new business will be restored—along with interest on the unrestored balance at a rate equal to the desired rate of return—when the last policy in this segment matures (or at the end of some shorter study period chosen arbitrarily). If confidence in the profit projection could be at the 100 per cent level, we should be well satisfied to look for a return on that investment at a rate consistent with high grade, risk-free investments. If, as is almost certain to be the case, we have some doubt about the future mortality, lapse rates, investment yields, expenses, and the like, then it would be unsound to accept such a low yield; instead, we would ask for a rate of return consistent with the greater risk. The reserve basis is a factor too. If the investment in new business is measured by use of net level reserves, we will settle for a lower rate of return than for an otherwise identical block of business reserved for on a preliminary term basis. This is consistent, for the risk of failure to recover a given dollar of investment is correspondingly diminished.

Consider, on the other hand, the rate of return that we should seek on the capital funds. In most cases we can consider the risk of invasion of these funds for the purpose of meeting policy obligations to be minimal; that is, we can expect that for a given segment of business the reserves held plus the future premiums and investment income will almost certainly cover claims and expenses. If we could go the full way and attach *no* risk of loss, we could justify a return on this part of the stockholder equity that includes no more risk premium than the investment return on the securities in which the company has invested the funds. Most companies, we may suspect, have premium rates, reserves, and the prospect of future experience such that this is about all that need be demanded as a yield on the free capital and surplus.

My third suggestion is that any particular plan/age cell may be more or less dependent than another on the various elements making up stockholder equity. Experimental coverages, for example, will generally involve greater risk and thus justify a higher rate of return on the investment made in acquisition of business and a higher-than-normal requirement for the return on capital and surplus as well. Another example involves the writing of stop-loss reinsurance contracts. Here there may be little, if any, investment for acquisition and possibly no use made of the agency plant. However, the combination of low claim frequency and high potential liability calls for large amounts of capital funds to back up such business, and, with even a moderate rate of return to compensate for the risk, the dollar effect on premium will be large in relation to the pure premium. For a final example, consider single-premium life insurance, as typically issued. This is similar to an illustration cited by Bowles, wherein there is no investment in acquisition of business. While there is no profit to be realized from the return on the unamortized investment in acquisition, the activity still did use the agency plant and should make its contribution to the yield on that element of the stockholder equity. Further, to the extent that the single-premium business requires capital funds to back it up, there should be a suitable yield on such funds consistent, again, with the risk.

My comments are not intended to argue, in any sense, against establishing some return on total stockholder equity as a company's over-all profit objective. I wish merely to direct your attention to the need to distribute that profit objective among the various segments of the company's business and to suggest that a division of the stockholder equity into its major elements will be required when translating the over-all profit objective into terms that can be applied to specific plan/age cells. For further pursuit of the subject, I recommend a reading of Anderson's excellent paper on gross premium calculations (*TSA*, XI, 357), with particular reference to the section that bears the caption "Profit Objectives."

MEL STEIN:

Mr. Bowles is to be congratulated for having written an original and interesting paper on a topic of major importance.

In the abstract there is set forth a list of five ways by which a company may improve its projected return on stockholder equity. The third way, "Accelerated investment in new business at the higher price levels," is too limited. As long as the "yield" earned on money invested in new business is greater than the net rate of interest earned on invested assets, this should be generalized to read "Accelerated investment in new business." An average higher price is not necessary under this (the normal) condition.

Bowles refers to the change in "unamortized investment in new business" as an adjustment to Convention Blank earnings. A number of people think of adjusted earnings and adjusted net worth as follows: (1) adjusted earnings is equal to (*a*) Convention Blank earnings plus (*b*) change in unamortized investment in new business; (2) adjusted net worth is equal to (*a*) Convention Blank capital and surplus, increased by items such as the MSVR, plus (*b*) value of insurance in force. The following item is sometimes also included in adjusted net worth: (*c*) value of agency force.

Unfortunately, the use of unamortized investment in new business violates the old accounting rule: Net worth at the beginning of the year plus net income (from all sources) during the year is equal to net worth at the end of the year.

Because of this, the use of unamortized investment in new business in the determination of adjusted earnings allows easy manipulation of both adjusted earnings and adjusted net worth (in the same direction). This is but one of the reasons why I am against the use of the unamortized investment in new business concept in the determination of adjusted earnings.

Adjusted earnings, as well as adjusted net worth, should be based on the value of insurance in force. Any change in the value of insurance in force due to changes in assumptions would, of course, be deleted from adjusted earnings from operations.

Bowles's criticism of the Statement Blank's allocation of interest earnings to insurance operations is indeed justified. A far more equitable method would be to allocate to insurance operations, gross (less a proportionate share of investment expenses) interest earnings directly related to insurance operations. This would differ in substance from Bowles's proposed method by allocating interest on deficiency reserves to insurance operations. This is a result of the fact that deficiency reserves and regular policy reserves have the following points in common:

1. They are required by law.
2. They are calculated on an individual policy basis under formulas set forth by state insurance laws.
3. They are released as individual policies are deleted from the insurance in force.
4. They are calculated by the use of mortality tables.
5. The amounts of both are, to some degree, arbitrary; while deficiency reserves are considered more arbitrary, neither type of reserve represents a true actuarial liability.

While deficiency reserves do not receive the same favorable federal income tax treatment as regular reserves, the rulings of the IRS hardly qualify as a model of logic and equity. In fact, some people even go so far as to call the IRS and its rulings somewhat arbitrary.

The allocation of interest earned on deficiency reserves to insurance operations is consistent with the approach which utilizes a gross premium valuation to project (*a*) the insurance operations portion of the Convention Blank gain from operations and (*b*) the increase in the value of insurance in force.

Bowles points out that, if a premium is computed to earn 15 per cent on the first-year, new-business surplus drain, the return on stockholder equity will be 15 per cent only if the funds not so invested in new business are also earning 15 per cent. In addition, the return on the first-year, new-business surplus drain will be 15 per cent only if *all* the renewal policy year profits from this business are rapidly reinvested in new business also earning 15 per cent.

Bowles's comparison of a 15 per cent yield on new business as opposed to a 15 per cent return on stockholder equity hits the bull's-eye of an area misunderstood by far too many people in the insurance industry (hopefully, this group does not include very many actuaries).

Before going further, it should be pointed out that the approach and mathematics contained in this paper seem to presuppose the lack of an adequate gross premium valuation program. This is made obvious by the use of unamortized investment in new business as a part of both adjusted earnings and stockholder equity. Even though the previously mentioned old accounting rule is not brazenly violated, the use of unamortized investment in new business to help determine both adjusted earnings and stockholder equity is *so* open to manipulation that it is an invitation to deliberate distortion of a company's performance. To distort substantially both adjusted earnings and stockholder equity in the *same* direction is dismayingly easy. To make it worse, if this is done consistently (for the successive calendar years) and the concept of unamortized investment in new business is accepted, such a distortion could be defended on actuarial grounds.

Stockholder equity and adjusted earnings should also include a value for *net* value of future sales and *net* increase in value of future sales. The *net* value of future sales is more commonly referred to as the "value of the agency force." This term, however, does not include sales through media such as brokers and mass marketing. While I can think of several logical, defendable methods of determining such values, this discussion will not go into a detailed description of these methods, as they are outside the scope of this fine paper.

It would be interesting to see what effect the replacement of unamortized investment in new business by the value of insurance in force would have on the figures in Tables 1 and 2. Offhand, I can see no reason why the resulting redefinitions of S_n and G_n would affect the validity of Bowles's novel approach to the return on stockholder equity.

If it is assumed that the use of unamortized investment in new business and the exclusion of the value of future sales in determining adjusted earnings and stockholder equity are acceptable, a few minor adjustments to this interesting, well-written, and pioneering addition to actuarial literature would be recommended. These would include deleting increase in deficiency reserves and adding certain nonadmitted assets to R_n (the gain from insurance operations, before federal income tax).

Bowles's concise, well-written paper on this important and timely subject has enriched actuarial literature by its excellent presentation of the concept of taking deliberate actions to influence a company's return on stockholder equity.

GATHINGS STEWART:

We can all be thankful to Mr. Bowles for stimulating our thinking about "return on stockholder equity." His Actuarial Note is most timely for two reasons: (1) Long-range planning is becoming increasingly important in life insurance operations, and he has provided some very interesting tools in this regard. (2) With the recent trend toward diversification in our industry, careful thought must be given to the use of capital funds and what will produce the best long-term results for both policyholders and stockholders. For this reason it seems to me that Bowles's Note should be just as significant to mutual company management as to the management of stock companies.

I found the section on "Relationships" very interesting from a strategy viewpoint. Perhaps some readers may feel that the ratio Z_n is not completely appropriate for their operations. This being the case, different formulas could be devised relating G to insurance in force or some other parameter. Being short of time and having a touch of spring fever, I have not yet attempted this.

Throughout the Note, the author has flirted with the concept of marginality without calling it by that name. In effect, he has assigned the interest on the assets which match the reserves to the "gain from insurance operations" and has considered the interest on stockholder equity as a separate item. He has then dealt with "stockholder equity" on a marginal basis by considering how this equity should be used (for affiliate investments, cash dividends, investment in new business, etc.). Another way to look at the question of marginality is to assume that a certain level of minimum capital and surplus is necessary to run the insurance operations and that these funds should be appropriately invested. Any capital and surplus beyond this amount can then be dealt with on a marginal basis. It is at this level that the constructive use of equity for diversification and investment in new business can be measured.

The use of marginal concepts in federal income tax planning is essential. Such a concept should prove helpful in considering "return on equity." This leads me to what I hope is a constructive comment on this excellent Actuarial Note. Might not the paper be expanded to consider the return on equity after the deduction of federal income taxes, since tax implications on a marginal basis are so very different from tax considerations on a total-company basis? This would lead to much more complicated formulas but should prove helpful, since tax implications for investment earnings on equity may be materially different from the tax implications on the gain from insurance operations.

WILSON H. TAYLOR:

Mr. Bowles is to be congratulated for his contribution to this interesting and timely topic. In the current atmosphere of great emphasis upon performance, his keynote sentence regarding the continued attractiveness of insurance companies as a vehicle for stockholder investment highlights a fact worthy of consideration. With the form of most of the financial reporting of an insurance company determined by the needs of regulatory authorities, there is some need for performance measures relating to the shareholders' best interests. Return on stockholder investment is one, if not the best, performance measure.

While I agree with Bowles that consideration of the return on stockholder equity should be an integral part of pricing, particularly with respect to providing an offset for the generally low return available on capital funds, it would seem that, as a practical matter, combining the return on investment in new business with the return on total capital funds is too vague to be of benefit in the pricing process. What effect, for example, will a new product have on the ratio of investment in new business to capital funds? Will pricing decisions be distorted by an inappropriate level of accumulated capital funds? A more direct and appropriate method would be to allocate to each piece of business under consideration the total investment that it requires, including the capital funds needed to support it. Premium levels could then be set to achieve the desired return on the total investment by the internal-rate-of-return method. If all pricing decisions were thus analyzed (and recognizing that "underpriced" products in needed but highly competitive lines must be balanced by high-return lines), the company would be well positioned to meet its over-all return goal. It is recognized, of course, that in actual practice desired returns should vary with the anticipated risk level of the product being considered.

This approach to the return-on-investment problem confers a threefold benefit. First, it is goal-oriented and permits the company to co-ordinate its pricing both to be consistent and to obtain an adequate return on stockholder funds. Second, it provides a very good project-evaluation tool to assist in the allocation of limited resources, both manpower and capital, toward the most desirable uses and away from those areas where an adequate return cannot be obtained. Third, it provides a broad framework for an effective performance measure. Actual experience can be compared to that upon which the expected return calculations were based. If the company is not attaining its goals, such comparisons will give an immediate indication of the source of the trouble and so complete the first step necessary to the resolution of the problem.

It should be remembered that return-on-investment theory deals with only one constraint, that of limited capital. Insurance companies, as do most other firms, face other and often more severe constraints, such as limited manpower in both field and home office, the dictates of social necessity, the need to protect markets from government intervention, and so on. Under the above-expanded definition of investment, which includes required capital, the problem that Bowles mentions of the return-on-investment principle implying no loading where there is no investment is not very likely to arise. Should it do so, however, consideration of these other constraints will determine the price. The product in question should be priced, and effort expended to sell it, until one of the constraints, probably manpower, indicates that the total return to stockholders would be lowered by further effort at the expense of other lines.

It is always possible, of course, that capital is not a true constraint in a given situation, that is, that a company has more capital than it needs. If such is the case, funds are not being handled in the best interest of stockholders, and some action should be taken to assure a more efficient use of capital. Determining at precisely what point there is too much capital is, of course, impossible. It is influenced by the requirements of the regulatory authorities and the dictates of prudent management. Each company must determine the required levels for its own situation, taking into consideration such factors as the type of assets held and the diversification of the investment portfolio, underwriting and marketing methods, reserve and premium levels, the concentration of risk, and so on. A great deal of work is still needed on this largely untouched aspect of the problem.

I would like to make several comments on the calculation methods described by Bowles. Despite the greater complexity introduced, it is necessary to use after-tax values for the return calculation. Pretax figures will produce different weightings for return on investment in new business and return on capital funds because of different tax rates, with the latter taxed at essentially the full corporate rate and the former at lesser rates (or even at zero for underwriting gain when the company is not paying a Phase II tax). It is the after-tax results that ultimately matter to the stockholders.

The actual calculation of the return on stockholder equity raises some interesting questions. The deduction of the redemption value of preferred stock and the amount of deferred tax charges give a leveraged return value. The result is not fully a measure of how well the company uses the funds available to it but depends in part on how much nonequity capital is held. It would seem that either these items should not be deducted or both calculation methods should be used, so that performance can be evaluated without distortion from leverage. It is assumed that the liquidation value of convertible preferred issues would not be deducted in any event, since such stock is at least partially in the nature of equity. For an actual return determination it would seem necessary to include a subsidiary line showing total realized and unrealized capital gains net of actual and deferred taxes. They are an integral part of investment performance, since gains often represent compensatory return for the low dividend yield on common stocks and losses are often the result of money market bond transactions which have served to increase net yield.

The use of premium profit margins, the Z_n's of the paper, might be misleading for a life insurance company. Profit margins may well be of more direct value in casualty insurance or casualty-type coverages (such as group accident and health), but, for permanent life insurance, profits depend on mortality, termination, expense, and interest experience, which show varying incidence by duration so that no given level of Z_n should be expected to continue. An analysis of variations in actual experience from that used in the expected return calculation to determine the source of unexpected increases or decreases in net gains would be more meaningful. If there is to be useful feedback to the ratemakers, it is necessary to have information on differences between actual experience and past assumptions. Even for casualty-type coverages it is necessary to subdivide actual results into fairly small groups, such as line of business and year of issue (or rate edition), in order that favorable experience on one line of business does not obscure unfavorable results elsewhere or that large gains on old business do not hide inadequate returns on recent issues or deteriorating experience on some segment of the business.

I would like to touch briefly on the possible solutions to the problem of a low return on stockholder investment. Bowles has listed the major possible actions available to improve upon an inadequate return. At the risk of some repetition, I will categorize several solutions to the problem under four primary causes of a low return. While these categories are admittedly somewhat artificial, in that they are at least partially overlapping, their direct statement may shed some further light on the problem. The causes, and the applicable solutions, follow:

1. Insufficient profit margins in the premium rates.
   a) Raise premiums to an acceptable level.
   b) Improve performance through more aggressive investment practices, more careful underwriting, tighter expense control, and so on.
2. Unfavorable product mix.
   a) Put greater effort and more investment into the most profitable lines.
   b) Withdraw from or de-emphasize the least profitable lines.
3. Too much idle or underutilized capital.
   a) Invest funds in a subsidiary or other undertaking where capital may be more efficiently used and a better return obtained.
   b) Pay greater dividends to stockholders, if it is felt that they can better use the funds, or purchase and retire part of the outstanding stock.
4. Required funds too large.
   a) Maintain lower reserves in order to reduce stockholder investment if it is ascertained that the reserves held or contemplated are redundant.

Finally, I would like to thank Mr. Bowles for his effort in presenting a thought-provoking paper on a relatively new topic, one that will be of increasing interest, at least to those of us employed by stock insurance companies.

JOHN C. WOODDY:

Mr. Bowles's Actuarial Note is a useful compilation of concepts, definitions, and relationships pertaining to a matter which, I think, most stock company actuaries have in mind when analyzing their companies' results but which has not been set forth so explicitly before. For instance, my company's internal reports first credit the stockholders with interest on their investment and then attempt to assess the return from the risk-bearing activity, something similar to G_n in the notation of the paper.

Bowles discusses the return on stockholder equity in two contexts—fiscal analysis and pricing. The two are, of course, related but separate, and the Note does not make clear the different considerations which apply.

In fiscal analysis we are trying to learn something from experience in the broad sense. We want to know whether management's policies were sound or should be changed. Were conservation practices effective? What judgment can we make of our underwriting? How efficient are our administrative procedures? Certainly we must not be led to think that everything is satisfactory because the company had a good over-all profit, if this is mainly due to interest on capital and surplus. If we are primarily concerned with answering questions for various lines of business considered separately, it will probably be most useful to look at G_n for each line. A judgment on the whole operation, which, as Bowles points out, is of primary concern to the board of directors, would be best derived from consideration of r_n.

Pricing is something else again. In the first place, we cannot be indifferent to what others are charging for the same or similar products, so the actuary operates under constraints. The Note refers, somewhat obliquely, to the difficulties inherent in relating profit on a particular element of business to the capital funds invested to acquire and maintain said business. This concept seems useful only when surplus is the scarce factor and the company faces competing demands from various actual and potential sources of business. When to the necessarily complex conceptual structure of most life companies is added the bizarre relationship between actual gain and taxable gain, one is driven to question whether the whole *is*, in fact, the sum of its parts.

The Note states as a premise that "an adequate return on stockholder equity is one appropriate guideline for . . . pricing . . ." and envisions some study of the over-all price structure as related to over-all return on total stockholder equity. No specifics are given. I tend to think of some kind of return akin to interest on stockholder equity, with the pricing of different elements of insurance, ideally, then being done so that the *expected* additional profit is related to the differing risks involved.

The Note deals with return on stockholder equity *before* federal income tax. I must concede that inclusion of tax complications would have made the discussion impossibly complex; yet application of Bowles's ideas must bring in tax effects in order to have any value for a particular company. More specifically, I concur with the strictures expressed in the list of weaknesses of the "method sometimes used" appearing in the Introduction, but this method does have its relevance to income taxes and thus cannot be ignored.

The purpose of the Note is to present a structure which will facilitate the analysis of an enterprise without encumbering the presentation by what could have become extended discussion of difficult and controversial subsidiary points, for instance, the method of calculating i_n. Thus it is obviously unfair to point out that no method of determining the *unamortized investment in new business* is given and that this element is the key to any actual determination of stockholder equity and the return thereon. If we do not get some kind of number for this probably sizable chunk of stockholder equity, we have no use for the Note. Bowles states categorically that this "is not the 'increase in the value of the business on the books,' " but a study undertaken to determine the unamortized investment, and so on, would hardly ignore the value of the business. For one thing, it would not seem reasonable to include in stockholder equity an amount of unamortized investment on a particular category of business with a present value *less* than its unamortized investment.

But, now, having done our study—and I am sure that no one here believes that this was anything but a long and costly endeavor involving some of our brightest actuarial talent—what do we do with it? Do the officers and directors immediately proceed to the stock market and buy heavily—or sell short? Have all officers and directors carefully divested themselves of any interest in their companies' stock before undertaking the study, and do they now proclaim the results to all the media of public communication—before taking any action themselves—or are they debarred from action even then, if it might appear that they are acting as touts?

As the author points out in his opening sentence, "Traditionally stock life insurance companies have been considered somewhat unique in relation to other corporate enterprises." Now we find life companies as subsidiary elements in larger corporate empires of less arcane enterprises or as the masters of such enterprises. For this and/or other reasons there is a demand that investors be able to appraise life companies in terms appropriate to other publicly held entities. If we point to our fiduciary responsibilities to rebut this presumption, we are told that banks seem to manage to discharge their obligations to the public while fitting into a general corporate pattern. Reference to risk brings the retort that (common) stockholders expect to take a risk when investing; they simply want better information as a basis for judging their risk. I think, perhaps, we have not hammered hard enough to drive home the fact that *risk* is an insurance company's *business* and that an investment in such a company's stock involves not merely risk but $(\text{risk})^2$. In fact we could go even further and say that if $(\text{risk})^2$ characterizes the usual short-term exposure of the nonlife company, then investment in a life company involves $(\text{risk})^3$.

RICHARD W. ZIOCK:

Mr. Bowles is to be congratulated for uniting profit concepts based on plan/age to the broader financial measuring rod of return on stockholder equity. His Actuarial Note is ever so much more timely because of the increased popularity of return on stockholder equity, which popularity has paralleled that of the holding company. Return on stockholder equity as a financial measuring rod possesses an equalizer quality which enables such diverse entities as pickle factories and insurance companies to be compared in their value to the holding company management.

It should be pointed out, however, that the smallest unit to which return on stockholder equity is usually applied is the company. It should be apparent that, for example, to compute return on stockholder equity by line of business would require assumptions on the allocation of surplus to each line of business. Such assumptions would be meaningless, since all of surplus stands behind any line of business which might find itself in trouble. Therefore, for internal management of insurance operations in an insurance company, yield on capital invested in new business, yield on capital invested in agency plant, value of good will generated by good company-policyholder relations, value of retaining less than optimal profit lines of business in order to retain agents, and the like, still determine the proper course of action as before. Return on stockholder equity can only measure a company's performance relative to other companies or some standard. It cannot provide clues to the best decisions for internal management.

Bowles brushes over the question of adjusting earnings, adjusting only for increase in unamortized investment in new business and increase in deficiency reserves. If the conclusion is to be reached that unfavorable experience or declining price level is the cause of a declining Z_n (which is, in my opinion, strongly implied in the paper), the method of adjusting earnings is of utmost importance.

If the adjustment to earnings is not proper, an aging or decrease in age of the business will cause a change in the adjusted earnings. If we consider an individual policy of $1,000, the adjusted earnings less interest on capital invested per $1 of premium income in force would have to be level regardless of duration. On a practical basis this would require adjusted earnings which rise slightly with duration as the capital invested approaches zero. Whether or not a particular method of adjusting statutory earnings will achieve this result will depend on the size of the reserve, the experience factors, and the capital invested in the policy. If the experience interest rate is expected to be much in excess of the valuation rate over the life of the policy, however, statutory earnings will rise substantially at the later durations when the reserve has been built up. An adjustment to the statutory reserves for interest will produce less rapidly rising earnings by duration under this condition.

The analysis of return on stockholder equity into insurance and investment elements as presented in the paper can be affected considerably in the case where a company retains a substantial portion of earnings, invests all or a large percentage of its capital funds (as defined in the paper) in common stocks, desires to regard the rate of return on common stocks as including the capital gains (long-term viewpoint), and includes capital gains (on a smoothed basis) in its adjusted earnings. Suppose that the rate of return on common stock investments is 10 per cent, the rate on all other assets being 4 per cent (with negligible capital appreciation) and on all assets (common stock and other) 5 per cent.

Table 1 shows the data for a hypothetical company which increases its capital funds by \$10,000 over a five-year period. The amount of capital invested in insurance, that is, $(S_{x-1} - F_{x-1})$, remains constant at \$5,000, there being no preferred stock outstanding at any time. Z_x was 20 per cent during both years. Table 2 illustrates a different analysis of r_xS_{x-1}. Here the total adjusted earnings of the company are regarded as gain from insurance operations plus the common stock rate of return on capital

TABLE 1

EXAMPLE OF CALCULATION OF Z BY THE METHOD PRESENTED IN THE PAPER

x	F_{x-1}	S_{x-1}	$r_xS_{x-1}=G_x+0.05\,S_{x-1}$	$0.05\,S_{x-1}$	G_x	P_x	Z_x
n........	10,000	15,000	5,750	750	5,000	25,000	0.20
$n+5$.....	20,000	25,000	6,250	1,250	5,000	25,000	0.20

TABLE 2

EXAMPLE OF CALCULATION OF Z BY THE METHOD OF THE DISCUSSION

x	F_{x-1}	S_{x-1}	$r_xS_{x-1}=G_x+0.1\,F_{x-1}+0.04\,(S_{x-1}-F_{x-1})$	$0.1\,F_{x-1}$	$0.04\,(S_{x-1}-F_{x-1})$	G_x	Z_x
n.......	10,000	15,000	5,750	1,000	200	4,550	0.18
$n+5$....	20,000	25,000	6,250	2,000	200	4,050	0.16

funds plus the other assets rate on the capital invested in insurance operations. Under these circumstances Z_x declined from 18 to 16 per cent. The difference from Table 1 is due to the fact that the higher rate of return on common stocks was credited to insurance operations in Table 1 and not in Table 2. In my view the profit due to higher return on assets invested in common stocks should not be considered a result of insurance operations. This view would be enforced if common stock investments are limited by state law to capital funds or policyowner surplus, as is often the case. Also, one might question the utility of taking surplus earning 10 per cent out of an insurance company and investing in a subsidiary earning 10 per cent, even though the interest on assets is only 5 per cent.

(AUTHOR'S REVIEW OF DISCUSSION)

THOMAS P. BOWLES, JR.:

I wish to express my thanks to the fifteen gentlemen who contributed provocative discussions of what appears to be a controversial subject. Perhaps the controversy arises principally because life insurance managers have either not done their homework or are not satisfied with the results. Certainly the life insurance company cannot find protective cover in the shadows of the cliché, "The life insurance business is really different, you know." I wonder.

Some of the discussants seemed to have overlooked the statement made in the Note that related controversial subjects, such as adjusted earnings, adjusted book values, and the like, were purposefully avoided in order to concentrate on the basic concept and logic of return on stockholder equity.

The discussions at least suggest that we are just beginning to build a structure which can be useful to the life industry.

A RUIN FUNCTION APPROXIMATION

JOHN A. BEEKMAN

SEE PAGE 41 OF THIS VOLUME

NEWTON L. BOWERS, JR.:

Professor Beekman is to be congratulated for presenting a highly interesting paper to the Society. His Theorems 1 and 2 give very attractive results for the mean and variance of the random variable

$$Z = \max_{0 \leq t < \infty} \left[\sum_{i=1}^{N(t)} X_i - t(p_1 + \lambda) \right].$$

The ruin probability can be evaluated from the distribution of Z since $\psi(u) = \Pr(Z > u)$. Beekman then suggests in his Theorem 3 a method for approximating $\psi(u) = \Pr(Z > u)$ by the use of a gamma distribution. Before the publication of this paper, it was easy to despair of using ruin theory. Most of the methods for evaluating the ruin function, including those of Beekman in his paper "Collective Risk Results" (*TSA*, XX, 182), are difficult to apply except in special cases. This paper seems to offer great promise for the extended practical use of ruin theory.

The purpose of this discussion is to suggest an alternative approximation procedure, again based on Beekman's Theorems 1 and 2. This alternative seems to offer certain advantages which will be illustrated by the first example of the Beekman paper.

As Beekman points out, a third fact is known about the distribution of Z. This is that $\Pr(Z = 0) = \lambda/(p_1 + \lambda)$, where $p_1 = E(X)$. The procedure that I propose is to use a mixed approximating distribution for Z consisting of a "lump" of probability of amount $\lambda/(p_1 + \lambda)$ at the origin plus a continuous portion consisting of a multiple of a gamma density. Therefore, we approximate the ruin function by the expression

$$\Pr(Z > u) = \frac{p_1}{p_1 + \lambda} \int_u^\infty \frac{t^{a-1} e^{-t/\beta}}{\Gamma(a)\beta^a} dt \qquad \text{for} \qquad u \geq 0 .$$

To illustrate the procedure, let us examine Beekman's first example, where the density for the size of the individual claim was $p(x) = e^{-x}$, $x > 0$. Let us leave λ arbitrary. Since $E(X^k) = k!$, we have from Theorems 1 and 2 that $E(Z) = 1/\lambda$ and $\mathrm{Var}(Z) = 2/\lambda + 1/\lambda^2$. Therefore, $E(Z^2) = \mathrm{Var}(Z) + [E(Z)]^2 = 2(1 + \lambda)/\lambda^2$. We now choose the parameters, α and β, so that the first two moments of the approximating distribution are equal to those just evaluated. For our mixed distribution

$$E(Z)=\frac{\lambda}{1+\lambda}(0)+\frac{1}{1+\lambda}\int_0^\infty t\cdot\frac{t^{\alpha-1}e^{-t/\beta}dt}{\Gamma(\alpha)\beta^\alpha}=\frac{1}{1+\lambda}\alpha\beta\,.$$

Similarly $E(Z^2)=[1/(1+\lambda)][\alpha(\alpha+1)\beta^2]$. Therefore, the equations that α and β must satisfy are

$$\frac{1}{\lambda}=\frac{1}{1+\lambda}\alpha\beta \quad \text{and} \quad \frac{2(1+\lambda)}{\lambda^2}=\frac{1}{1+\lambda}\alpha(\alpha+1)\beta^2\,.$$

From the solution that $\alpha=1$ and $\beta=(1+\lambda)/\lambda$, we obtain the approximation that

$$\Pr(Z>u)\doteq\frac{1}{1+\lambda}\int_u^\infty\frac{e^{-t/\beta}}{\beta}dt=\frac{1}{1+\lambda}e^{-u/\beta}$$
$$=\frac{1}{1+\lambda}e^{-u\lambda/(1+\lambda)}$$

for $u\geq 0$. As can be seen in Beekman's paper (in *TSA*, XX, 182), the proposed approximation gives the exact answer in this important case.

We summarize the results of the calculation for Beekman's second example:

$$E(X)=8.6 \qquad \lambda=0.3E(X)=2.58$$
$$E(X^2)=116.2$$
$$E(X^3)=1927.4 \qquad \Pr(Z=0)=\tfrac{3}{13}$$
$$E(Z)=22.52$$
$$\mathrm{Var}(Z)=756.14 \qquad E(Z^2)=1263.26$$

We now choose α and β. The equations are

$$\tfrac{10}{13}\alpha\beta=22.52\,;\qquad \tfrac{10}{13}\alpha(\alpha+1)\beta^2=1263.26\,.$$

The solution is $\alpha=1.0916$, $\beta=26.8256$. To find the point u such that $\Pr(Z>u)=0.01$, we seek u such that

$$\tfrac{10}{13}\int_u^\infty\frac{e^{-t/\beta}t^{\alpha-1}}{\beta^\alpha\Gamma(\alpha)}dt=0.01\,.$$

Thus we look for the 0.987 point on the distribution of a gamma distribution with $\alpha=1.0916$. For α values near 1, such that the gamma distribution is highly skewed, the handiest tables seem to be those in K. Pearson's

Tables of the Incomplete Gamma Function (London, 1922). Using these tables, we obtain

$$u = 4.351\sqrt{1.0916}\,(26.8256) = 121.95\,.$$

CECIL J. NESBITT:

I find myself in a quandary concerning this paper and the discussion prepared by my colleague, Professor Bowers. My mathematical soul appreciates the skill that the author and the discussant have shown in approximating a long-term ruin function. But my actuarial soul rebels against some of the underlying assumptions of the model considered; in particular, that no adjustments will be made to accommodate to unfolding experience, that only constant risk factors will be considered for the indefinite future, that interest is ignored, and that little account is taken of growth or other variations in the insured group over the long term. If one does accept the long-term ruin function of the paper, however, he may well find the approximations suggested there and in the discussion to be superior to the standard asymptotic approximation.

I make these remarks in the spirit of inquiry and in the hope that the author will further argue the virtue of his ruin function approximation.

ROBERT C. TOOKEY:

One of the most interesting observations that one might make after studying this paper is how handy the gamma function approximation is in "threading the needle" in various collective-risk problems. I have had the feeling in recent years that, with the development of high-speed computers and their application to many of our mathematical problems today, there might be less use for such mathematical tools as the gamma function, the beta function, and the Bessel function.

In the application of ruin theory, it is important to keep in mind that ruination can result not only from purely adverse statistical fluctuation but also from the occurrence of catastrophies, concentration of risk, and errors in underwriting, just to name a few. While the paper points out that the $250,000 fund and the 30 per cent security loading in the rate structure would enable a company retaining $50,000 of insurance on any one life to avoid ruin, 99 times out of 100, I cannot conceive of a case in which the fund would be so low. I personally would not like to see it drop below $1,000,000 because of the other uncertainties involved.

(AUTHOR'S REVIEW OF DISCUSSION)

JOHN A. BEEKMAN:

Professor Bowers' version of Theorem 3 is more accurate, as I unwittingly built a discontinuity into $\psi^*(u)$ at $u = 0$. Formulas for the a

and β in Bowers' formula for $\Pr(Z > u)$ can be calculated as they are in his first example; they are

$$\beta = \frac{2}{3}\frac{E(X^3)}{E(X^2)} + \frac{E(X^2)}{2\lambda}\left(1 - \frac{\lambda}{p_1}\right);$$

$$\alpha = \frac{E(X^2)}{2\lambda}\cdot\left(\frac{p_1 + \lambda}{p_1}\right) \div (\text{above } \beta) .$$

The author urges that this version of Theorem 3 by Bowers be used.

Professor Nesbitt raises some interesting questions about the hypotheses of the long-term ruin function which will require lengthy consideration. At the moment, I would say that the long-term ruin function provides approximations to answers to questions about retention limits and initial capital for new lines of business and that this approximation of the ruin function is easy to apply.

In answer to Mr. Tookey's discussion, I would say that it is correct that simulation techniques offer much potential for solving collective-risk problems. However, analytical solutions are much cheaper to use, when available, since simulation involves considerable computer expense. Furthermore, analytical solutions have the potential of being exact rather than approximate. Relative to the example, I agree that the actuary should use his professional judgment in any application of this theory, and this may lead to a higher initial reserve than that required by the theory. At least the theory can guide the actuary in reaching a conclusion.

In addition to these formal discussions, I received two letters which I would like to comment on briefly. Bowers has obtained an alternate derivation of the moments of Z which has the advantages of (1) being more direct than mine and (2) giving the third and fourth moments fairly easily. Professor Hilary Seal asked for a better comparison of the Beekman-Bowers approximation of $\psi(u)$ with Lundberg's 1926 approximation:

$$\psi(u) = \frac{\lambda}{q_1^* - p_1 - \lambda} e^{-Ru} ,$$

where

$$q_j^* = \int_0^\infty y^j e^{Ry} dP(y) \qquad (j = 0, 1)$$

and R is given by $q_0^* = 1 + R(p_1 + \lambda)$.

Professor Harald Cramér's book (reference 7 in the bibliography of my reference 3) contains just the example for comparison on pages 43–45.

The improvement is marked at $u = 20$ and 100. Furthermore, the approximation is consistently conservative, as one would want it to be.

FIRE INSURANCE DISTRIBUTION: VALUES OF THE RUIN PROBABILITY $\psi(u)$ FOR $\lambda=0.3$

u	$\Psi(u)$				
	Exact	Lundberg's Approximation	Ratio Lundberg/ Exact	Beekman-Bowers Approximation	Ratio B.B./Exact
20	0.5039	0.4524	0.898	0.5141	1.020
40	.3985	.3904	0.980	.4098	1.028
60	.3280	.3370	1.027	.3367	1.027
80	.2757	.2909	1.055	.2811	1.020
100	0.2346	0.2511	1.070	0.2370	1.010

This comparison is very valuable, as my comparisons in Examples 2 and 3 of this paper were in error. My formula for C on page 186 of reference 3 should have been $\lambda/[q'(R) - p_1 - \lambda]$, not $\lambda/[q'(R) - 1 - \lambda]$. Therefore, Examples 1 and 2 of that paper should have read $C = 0.8823$, $\psi(\$128,000) \fallingdotseq 0.01$ and $C = 0.8424$, $\psi(\$253,143) \fallingdotseq 0.01$. Examples 2 and 3, therefore, show little difference in the two methods.

I wish to thank Professors Bowers, Nesbitt, and Seal and Mr. R. C. Tookey for their very helpful discussions.

A LOGICAL APPROACH TO POPULATION PROBLEMS

ROBERT W. BATTEN

SEE PAGE 49 OF THIS VOLUME

GEOFFREY CROFTS:

Professor Batten has provided students with a powerful method of handling stationary population problems. I am in full accord with his approach. I would like, however, to make a few additional comments.

I feel that the student will achieve greater clarity and facility if the aggregates l_x, d_x, L_x, T_x, and Y_x can be related through a two-dimensional diagram showing both the passage of time and the increase in age. It may be difficult or cumbersome to incorporate such a diagram into a written presentation, but it is of tremendous value in oral discussions.

Batten's logical approach would be used in a pedagogical situation following (*a*) the definition of the stationary population model and (*b*) a careful development of the meanings of the elementary aggregates l_x, d_x, L_x, T_x, and Y_x stemming from the above definition. Both the paper and this discussion assume these steps to be taken. These symbols are then used meaningfully to deal with more complicated problems demonstrated in the paper.

The author gives two meanings to T_x, which might be numbered 1 and 2. In teaching this subject, I develop three additional meanings which serve as elementary quantities for solving the more complex examination-type problems:

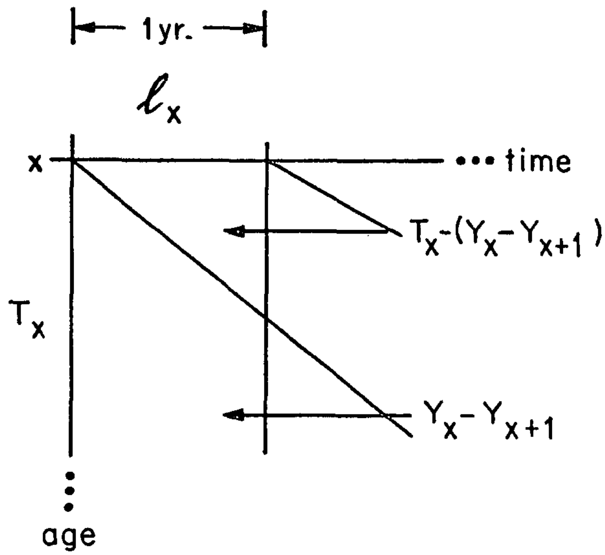
3. The aggregate future lifetime since age x of *any* l_x people alive at age x (subject to mortality according to the underlying table). This is not much different from Batten's second meaning and could be regarded as including his meaning as a special case.

4. The total past lifetime since age x of the l_x persons who die during a one-year period at ages x and over. Another way of saying this is that T_x is the total of the difference between the age at death and x for each of the previously mentioned l_x deaths.

5. The total lifetime lived by the population during a one-year period at ages in excess of x. This can be verified using Batten's methods by adding two parts:

a) The total lifetime lived during the year by T_x persons alive at the beginning of the year, namely, $Y_x - Y_{x+1}$; and

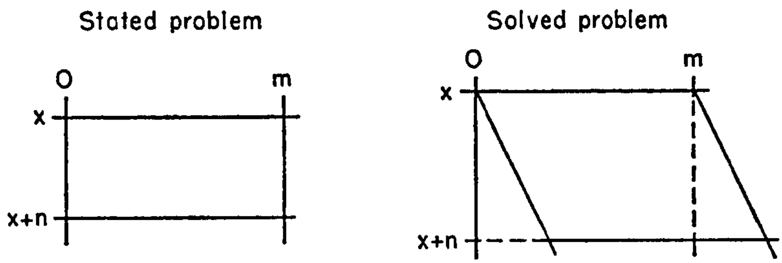
b) The number of years lived after attaining age x during the year but before the end of the year by those l_x persons who attain age x during the year, namely, $T_x - (Y_x - Y_{x+1})$. The diagram which I would have before me in explaining this concept would be:



In a similar vein we learn that there are two useful meanings to l_x: (1) the number attaining age x during a year and (2) the number dying at ages x and over during the year, being the sum of $d_x + d_{x+1} + d_{x+2} + \ldots$.

In his first illustration in Section IV, Batten explains that $l_x - l_{x+n}$ is the number dying during a given year between ages x and $x + n$ by using the first meaning of l_x above. To grasp the validity of this meaning of $l_x - l_{x+n}$ requires the slightly sophisticated line of thought through the implications of the stationary population model as given in the paper. If the student is convinced of the truth of meaning 2 for l_x, then it is obvious that $l_x - l_{x+n}$ is the number dying between ages x and $x + n$ during the year.

This first illustration in Section IV presents a certain amount of difficulty to the student. Rather than dealing with the aggregate lifetime of the $m(l_x - l_{x+n})$ persons who die in the m-year period, Batten seems to have dealt with the aggregate lifetime of those *ml_x persons who attain age* x *during the* m-*year period* and who die during the n years following the attainment of their xth birthday. The difference between the two problems can be demonstrated diagramatically. The internal area in each figure represents the time and age combinations at which deaths can occur:



It is the conditions of the stationary population model which make the answer to the latter problem the same as that of the former. If certain of the conditions are altered, the answers to these two problems are different. The automatic assumption that the answer to one question gives the answer to the other is the cause of considerable difficulty.

If resort is made to my meaning 4 for T_x, the aggregate lifetime of those $l_x - l_{x+n}$ persons dying during a given year between age x and $x + n$ can be developed as follows:

a) Each person dying lived at least x years or a total of $x(l_x - l_{x+n})$ years of life.
b) The total lifetime since age x would be T_x including the lifetime since age x of the l_{x+n} persons who died during the year at age $x + n$ and over.
c) But the deaths at ages over $x + n$ (whose lifetime since age x has been included in item *b*) must be eliminated. The removal of T_{x+n} removes their lifetime from age $x + n$ (not from age x). Each of the l_{x+n} deaths must have another n years removed from T_x for a total removal of $T_{x+n} + n \cdot l_{x+n}$.

Thus the aggregate lifetime sought is

$$x(l_x - l_{x+n}) + T_x - T_{x+n} - n \cdot l_{x+n} .$$

There seems to be no particular value to the use of an m-year period.

The illustration of increasing populations given in Section VI can be used to demonstrate that the five different questions, for which T_x is the answer under stationary population conditions, do not yield the same answer under other conditions.

I expect to urge all Part 4 students at Northeastern University to pay close attention to Professor Batten's paper.

JAMES C. HICKMAN:

Professor Batten is to be thanked for contributing to the growing body of actuarial literature devoted to providing insights into stationary population problems. There are few topics in actuarial mathematics that have spawned more test questions, along with associated notes and hints directed to students, than has stationary population theory. Without minimizing the importance of the delight that these problems have brought to successful students and the frustration that they have brought to unsuccessful students, it would be fair to ask if stationary population theory is of interest to actuaries solely because it supplies challenging questions for the professional examinations. Despite the fact that a stationary population requires a delicate balance between increments and decrements that is only crudely approximated by real world populations, the answer to this question is an emphatic "No!"

In particular, stationary populations are important tools in the study of funding methods. By the assumption of a stationary population, the characteristics of the funding methods under study may be examined without being obscured by fluctuations that inevitably occur in real world populations.

When a stationary population is assumed, many funding methods may be studied through the use of the following equation of equilibrium:

$$\int_0^\infty l_x C(x)dx + \delta \int_0^\infty l_x V(x)dx = \int_0^\infty l_x B(x)dx\,, \qquad C + \delta F = B\,.$$

In this equation $C(x)$ is the annual rate of contribution into the fund for members aged x; $V(x)$ is the individual fund for lives aged x; and $B(x)$ is the annual rate of benefit payment for participants aged x. The symbols δ, the force of interest, and l_x, the population density at age x, have their usual actuarial meaning. C denotes the constant annual contribution rate; F, the constant fund; and B, the constant annual rate of benefit payments.

In Table 1 certain well-known examples of the application of this equation of equilibrium are listed. The confirmation of the equation of equilibrium will be found in the indicated references.

The stationary population concept also plays a role in the construction of mortality tables from population data. In the past, a key assumption in this construction process was that the observed death rate provided an estimate of the central death rate for the corresponding interval of ages for the stationary population defined by the life table. That is, ${}_nD_x/{}_nP_x$ (where ${}_nP_x$ is the population between age x and $x + n$ on the census date and ${}_nD_x$ is the number of deaths between ages x and $x + n$ in the population under study in the year centered on the census date) is an estimate of the central death rate for ages x to $x + n$ for the life table, ${}_nm_x = {}_nd_x/{}_nL_x$. The problem then becomes that of estimating ${}_nq_x$, given an estimate of ${}_nm_x$. Much of chapter V of Spiegelman's textbook [6] may be thought of as being devoted to the development of alternative methods for estimating ${}_nq_x$, given estimates of ${}_nm_x$. The uniform distribution of

TABLE 1

EXAMPLES OF THE EQUATION OF EQUILIBRIUM FOR VARIOUS FUNDING METHODS

	$C(x)$	$V(x)$	$B(x)$	Domain	Reference
Pension funding:					
Unit credit	0	0	0	$0 \le x < a$	[1, 7]
	$\frac{{}_{r-x\|}\bar{a}_x}{(r-a)}$	$\frac{(x-a)\,{}_{r-x\|}\bar{a}_x}{(r-a)}$	0	$a \le x < r$	
	0	$\bar{a}_x$	1	$r \le x$	
Entry age normal	0	0	0	$0 \le x < a$	[1, 7]
	$\frac{{}_{r-a\|}\bar{a}_a}{\bar{a}_{a:\overline{r-a}\|}}$	$\frac{\bar{a}_{a:\overline{x-a}\|}\,{}_{r-x\|}\bar{a}_x}{a_{a:\overline{r-a}\|}}$	0	$a \le x < r$	
	0	$\bar{a}_x$	1	$r \le x$	
Insurance funding:					
Whole life	0	0	0	$0 \le x < a$	[3, p. 171]
	$\bar{P}(\bar{A}_a)$	${}_t\bar{V}(\bar{A}_a)$	μ_x	$a \le x$	
Current cost	0	0	0	$0 \le x < a$	
	μ_x	0	μ_x	$a \le x$	

deaths assumption over the interval x to $x + n$, the Reed-Merrill method, Greville's method, and the various methods of using standard tables may be viewed as providing alternative solutions to this problem. In recent years a considerable amount of research has been directed to solving the problem of adjusting the observed central death rates for continuing changes, such as population growth and mortality improvement, which force the observed population away from a stationary position and therefore tend to invalidate the assumption that the observed central death rates can be used to approximate the central death rates of the life table [2, 4].

Open group model office computations are familiar to most actuaries. In these computations the expected progress of an insurance system is traced by the use of a family of demographic and economic assumptions. For example, dynamic population assumptions have long been used in projecting the progress of the OASI and DI trust funds, under separate collections of assumptions which tend to produce high and low costs.

Another example of the employment of population theory of both the stationary and dynamic types in actuarial practice is provided by the application of the theory of immunization in selecting maturity dates of assets, so as to minimize the effect of changes in the interest rate on the balance between the assets and liabilities of an insurance system. This theory was developed primarily by British actuaries [5, 8] during the period following World War II when interest rates were held artificially low by government action. Now, after twenty years of almost continually increasing interest rates, it may be difficult to remember when there was once some concern about the ability of life insurance companies to earn interest income sufficient to fulfill the guarantees in their outstanding contracts. At that time this theory provided a measure of comfort to those responsible for reducing the impact of declining interest rates on the ability of insurance companies to honor their promises.

Immunization is a continuing process which requires scheduling the inflow from invested assets to satisfy two simple rules: (1) the weighted average of the time of investment inflow must equal the weighted average time of liability outflow and (2) the second moment of the weighted average of the time of cash inflow from invested assets should exceed the weighted average time of the liability outflow. In summary, this theory provides support for the investment rule "Invest long."

In this theory, liability flows are claims, withdrawal, and expense payments less gross premiums, and asset flows include both interest payments and maturities. The weights applied to the expected cash flow streams are provided by v^t, where the interest rate is that for fixed unit investments at the time the valuation and the time matching are done. It is generally assumed in this theory that the present expected value of the asset flow is equal to the present expected value of the liability flow. Any assets in excess of those which match the liabilities may be invested independent of the immunization rules. However, to maintain immunization for a block of business, it may be necessary to shift assets into and out of the immunized assets as changes in the interest rate and in the expected volume of liability flows disturb the original balance.

Table 2 is directly related to a similar table in a paper by Wallas [8]. The table gives formulas for computing mean liability terms and a numerical example. The example is concerned with the immunization of a block of whole life policies of amount one, issued at age 40, and assuming, rather arbitrarily, that the gross premium less expenses is the net premium. We made the computations assuming a continuous model. At first a stationary population was assumed, and then populations of insured lives that have grown at annual rates of 5 and 10 per cent were assumed. In order to simplify comparisons, the amount and present value results have been stated per life at the current rate of entry; that is, expected payments have been computed using $l_{40+t}/l_{40} = {}_tp_{40}$.

Of course, as is usual in the application of stationary and dynamic population theory to actuarial problems, these results may be viewed only as illustrations. The practical problems in implementing a program of immunization and the cost of foregoing possible gains as a result of interest rate changes are ignored in the example. Nevertheless, certain serious practical problems are evident even in this simple example. The mean time of the liability payments has been overstated by ignoring withdrawal payments and by an unrealistic expense assumption. However, it appears that, even after mental adjustments are made for these biases, it would be difficult to plan a schedule of asset maturities which would produce a mean time of asset payments which would equal the mean time of liability payments.

Mr. Richard Maurer, a student of the Society, prepared Table 2, using the computing facilities of the University of Iowa. The trapezoidal rule was used to evaluate all integrals.

REFERENCES

1. COOPER, S. L., AND HICKMAN, J. C. "A Family of Accrued Benefit Actuarial Cost Methods," *TSA*, XIX (1967), 53–59.
2. GREVILLE, T. N. E. *Methodology of the National, Regional, and State Life Tables for the United States, 1959–61* (United States Department of Health, Education, and Welfare: Public Health Service Publications No. 1252, Vol. I, No. 4, 1967).

TABLE 2

IMMUNIZATION TABLE, 1958 C.S.O., 3 PER CENT INTEREST

Annual Rate of Increase in Entrants	$r=0$	$r=0.05$	$r=0.10$
	Sums Assured		
Amount: $\int_0^\infty {}_tp_{40}(1+r)^{-t}dt$	32.179	15.379	9.505
Present value: $\int_0^\infty {}_tp_{40}(1+r)^{-t}\bar{A}_{40+t}\,dt$	19.869	8.463	4.836
Mean term: $\dfrac{\int_0^\infty {}_tp_{40}(1+r)^{-t}(\bar{I}\bar{A})_{40+t}\,dt}{(\text{Present value})}$	13.918	17.143	19.210
	Premiums		
Amount: $0.02\int_0^\infty {}_tp_{40}(1+r)^{-t}dt$	0.642	0.307	0.190
Present value: $0.02\int_0^\infty {}_tp_{40}(1+r)^{-t}\bar{a}_{40+t}\,dt$	8.956	4.977	3.343
Mean term: $\dfrac{0.02\int_0^\infty {}_tp_{40}(1+r)^{-t}\bar{I}\bar{a}_{40+t}\,dt}{(\text{Present value}}$	10.420	11.334	11.759
	Net Liabilities		
Present value: (Sum assured−premiums).......	10.914	3.487	1.494
Mean term*	16.788	25.435	35.888

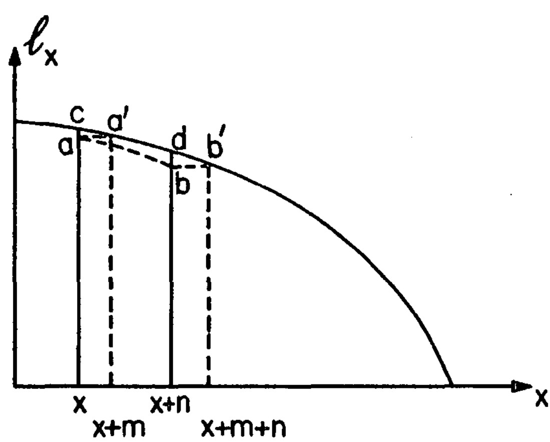
* Mean term net liabilities=[(Present value sum assured) (Mean term sum assured)−(Present value premiums) (Mean term premiums)]/[(Present value sum assured)−(Present value premiums)].

3. HOOKER, P. F., AND LONGLEY-COOK, L. H. *Life and Other Contingencies*, Vol. I (Cambridge, Mass.: Cambridge University Press, 1953).
4. KEYFITZ, N. "A Life Table That Agrees with the Data," *Journal of the American Statistical Association*, LXI (1966), 305–11.
5. REDINGTON, F. M. "A Review of the Principles of Life Office Valuation," *JIA*, LXXVIII (1952), 286–315.
6. SPIEGELMAN, M. *Introduction to Demography* (1st ed.; Chicago: Society of Actuaries, 1955).
7. TROWBRIDGE, C. L. "Fundamentals of Pension Funding," *TSA*, IV (1952), 17–43.
8. WALLAS, G. S. "Immunization," *JSS*, XV (1959), 345–57.

PAULETTE TINO:

More than fifty proofs have been given of the law of quadratic reciprocity. Happy is the present-day student in the theory of numbers, because he cannot fail to find the proof congenial to him. The time is approaching when the student in stationary population will be in the same enviable position: Professor Batten has just added his own approach to past literature, and reading him led me to write this discussion.

I propose to solve the problems in Batten's paper using basically the graphical representation of the l_x curve. The idea here is that, since the population is stationary, its past is known and its future is predetermined, and all this can be read off the graph even though it is essentially the picture of the population at the present time. Let us take, for example, the problem of locating on the graph those lives in the population now between ages x and $x + n$ who will die in the next m years.

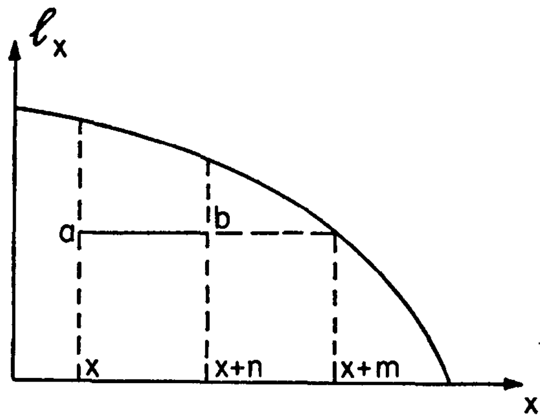


We know that the present population represented by area $(x, c, d, x + n)$ will dwindle after m years to $(x + m, a', b', x + n + m)$. If we translate the $\widehat{a'b'}$ curve m years on the left, we get in area (a, c, d, b) those persons who will die within the next m years.

Once the group in question has been located on the graph, problems of future lifetime–past lifetime can be tackled through integrals or by use of the diagrammatic approach. I chose here the integral method; with that method, a past lifetime is of the form $\int l_{x+t}\cdot(x+t)dt$, and a future lifetime is of the form $\int T_{x+t}dt$, the form being adjusted to the population located on the graph. This statement is illustrated in the solution of the following problems.

Solution of the Problems Proposed in the Text

1. The average present attained age of those persons now living between ages x and $x+n$ who will ultimately attain age $x+m$.



The population involved is in the rectangle $(a, b, x+n, x)$. The average age is read to be $x+n/2$. (Equal density of the population between ages x and $x+n$.)

If the student wants to confirm his geometrical intuition, he can write:

$$\text{Average age} = \frac{\text{Total past lifetime}}{\text{Total population}}$$

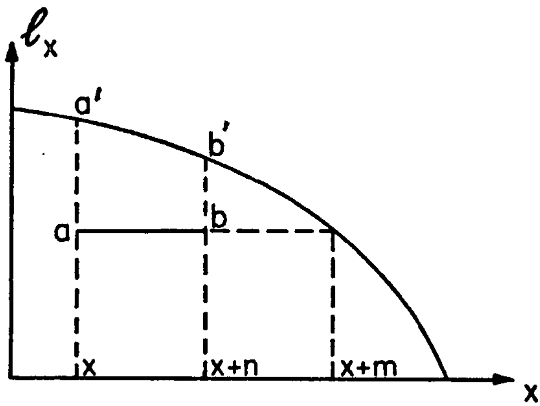
$$= \frac{\int_0^n (x+t)\cdot l_{x+m}dt}{\int_0^n l_{x+m}dt}$$

$$= \frac{nxl_{x+m} + (n^2/2)l_{x+m}}{n\cdot l_{x+m}}$$

$$= x + \frac{n}{2}\ldots$$

and smile.

2. The average present age of the $T_x - T_{x+n} - nl_{x+m}$ who will not survive to age $x+m$.



The population involved is in the area (a, b, b', a').

$$\text{Average age} = \frac{\text{Total past lifetime}}{\text{Total population}}$$

$$= \frac{\int_0^n (l_{x+t} - l_{x+m})\cdot(x+t)dt}{\int_0^n (l_{x+t} - l_{x+m})dt} .$$

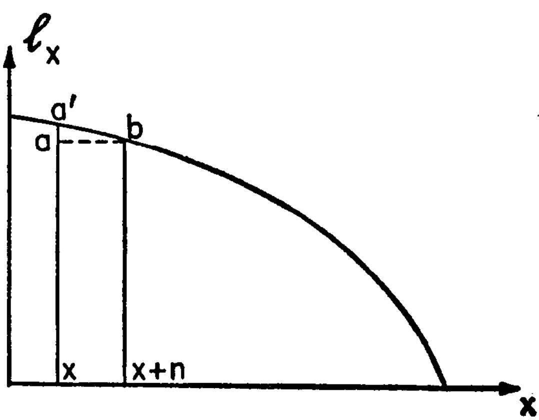
This result is immediate. There is only one integral by parts, namely,

$$\int_0^n l_{x+t} t dt = -tT_{x+n}\Big|_0^n + \int_0^n T_{x+t}dt$$

$$= -nT_{x+n} + y_x - y_{x+n} .$$

$$\text{Average age} = [x(T_x - T_{x+n} - nl_{x+m}) - nT_{x+n} + y_x - y_{x+n} - (n^2/2)l_{x+m}]/(T_x - T_{x+n} - nl_{x+m}) .$$

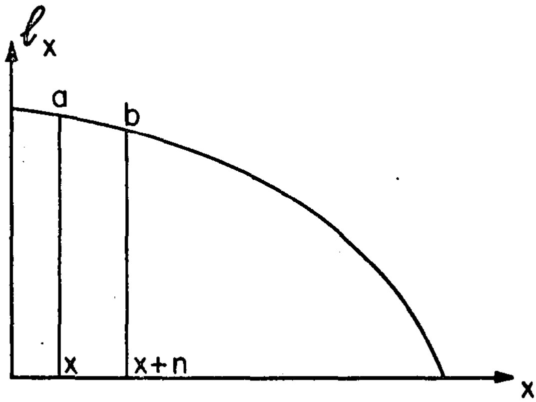
3. The average age at death of the persons who will die between age x and $x + n$.



The persons involved are represented by the segment (a, a'). To their aggregate past years before x they will add, in aggregate, the aggregate years represented by the area (a, a', b).

$$\text{Average age} = x + \frac{T_x - T_{x+n} - nl_{x+n}}{l_x - l_{x+n}}.$$

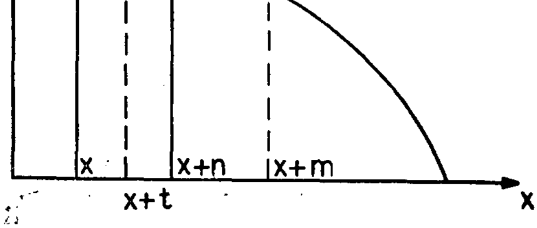
4. The total lifetime of those persons now between ages x and $x + n$.



Past lifetime: $$\int_0^n l_{x+t} \cdot (x + t)dt = x(T_x - T_{x+n}) - nT_{x+n} + y_x - y_{x+n};$$

Future lifetime: $$\int_0^n T_{x+t}dt = y_x - y_{x+n}.$$

5. The average future lifetime of those persons now between ages x and $x + n$, before attainment of age $x + m$.

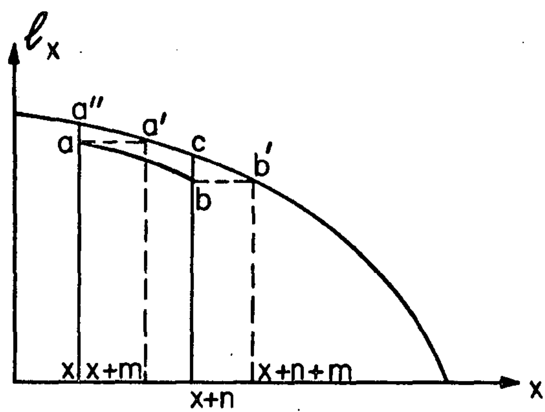


$$\text{Average future lifetime} = \frac{\int_0^n (T_{x+t} - T_{x+m})dt}{T_x - T_{x+n}}$$

$$= \frac{y_x - y_{x+n} - nT_{x+m}}{T_x - T_{x+n}} .$$

(The future lifetime of the l_{x+t} lives is represented by area $[x + t, a, b, x + m] = T_{x+t} - T_{x+m}$.)

6. Average age of those persons now between age x and $x + n$ who will die in the next m years.

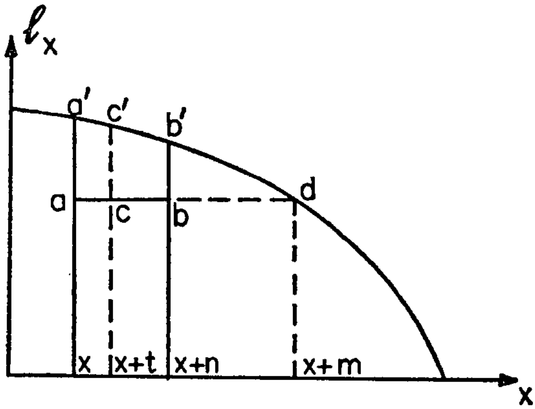


The population involved is in the area (a, a'', c, b) where the curve $\widehat{ab}$ is the curve $\widehat{a'b'}$ translated for m years, toward the left.

$$\text{Average age} = \frac{\int_0^n (l_{x+t} - l_{x+m+t})\cdot(x + t)dt}{\int_0^n (l_{x+t} - l_{x+m+t})dt}$$

$$\begin{aligned} &= [x(T_x - T_{x+n}) - x(T_{x+m} - T_{x+m+n}) \\ &\quad + (- nT_{x+n} + y_x - y_{x+n}) \\ &\quad - (- nT_{x+m+n} + y_{x+m} - y_{x+m+n})]/[T_x - T_{x+n} \\ &\quad - (T_{x+m} - T_{x+m+n})] \\ &= x + \{[y_x - y_{x+n} - (y_{x+n} - y_{x+m+n}) \\ &\quad - n(T_{x+n} - T_{x+m+n})]/[T_x - T_{x+n} \\ &\quad - (T_{x+m} - T_{x+m+n})]\} . \end{aligned}$$

7. Total lifetime of those persons now between ages x and $x + n$ who will not survive to age $x + m$.



The lives involved are in the area (a, a', b', b). The future lifetime of those lives now age $x + t$ is

$$T_{x+t} - T_{x+m} - l_{x+m}(m - t) ,$$

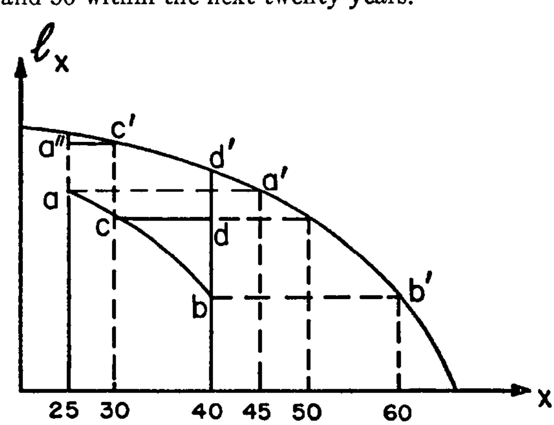
represented by area (c, c', d).

Future lifetime: $$\int_0^n [T_{x+t} - T_{x+m} - l_{x+m}(m - t)]dt = y_x - y_{x+n} - nT_{x+m} - mnl_{x+m} + \frac{n^2}{2} l_{x+m} .$$

Past lifetime: $$\int_0^n (l_{x+t} - l_{x+m})(x + t)dt = x(T_x - T_{x+n} - nl_{x+m}) + \left(- nT_{x+n} + y_x - y_{x+n} - \frac{n^2}{2} l_{x+m}\right) .$$

Total lifetime: $$x(T_x - T_{x+n} - nl_{x+m}) + 2(y_x - y_{x+n}) - n(T_{x+m} + T_{x+n}) - mnl_{x+m} .$$

8. Find an expression for the average attained age of those persons in a stationary population now between 25 and 40 who will die between the ages of 30 and 50 within the next twenty years.



The curve $\widehat{ab}$ is the curve $\widehat{a'b'}$ translated twenty years on the left. Area (a, a'', c', d', d, c) represents all those who will die meeting the conditions of the problem.

Average attained age

$$= \frac{\int_0^5 (l_{30} - l_{45+t})(25 + t)dt + \int_0^{10} (l_{30+t} - l_{50})(30 + t)dt}{\int_0^5 (l_{30} - l_{45+t})dt + \int_0^{10} (l_{30+t} - l_{50})dt}$$

$$= [25\cdot 5\cdot l_{30} - 25(T_{45} - T_{50}) + (5^2/2)l_{30} - (-5T_{50} + y_{45} - y_{50})$$

$$+ 30(T_{30} - T_{40}) - 30\cdot 10l_{50} + (-10T_{40} + y_{30} - y_{40})$$

$$- (10^2/2)l_{50}]/[5l_{30} - (T_{45} - T_{50}) + T_{30} - T_{40} - 10l_{50}]\ .$$

S. DAVID PROMISLOW:

Inspired by the work of Professor Batten and previous writers, we have managed to produce a few simple general formulas which will give solutions to all the stationary population problems that have been considered in the literature.

We consider for a fixed interval, x to $x + n$, the group of people in a stationary population now living in this interval, that is, at an age y where $x \leq y \leq x + n$, who will die between the ages of $f_1(y)$ and $f_2(y)$, where $y \leq f_1(y) \leq f_2(y) \leq \omega$. We want to know the number of such people and the total lifetime lived by the group between the ages of $f_3(y)$ and $f_4(y)$ for those now aged y. We make the following assumptions:

a) For $i = 1, 2, 3, 4$, $f_i(y) = a_i y + b_i$, where $a_i = 0$ or 1 and b_i is any constant.

b) One of the following three cases holds:

CASE 1: $f_1(y) \leq f_3(y) \leq f_4(y) \leq f_2(y)$, for all y;
CASE 2: $f_3(y) \leq f_4(y) \leq f_1(y) \leq f_2(y)$, for all y;
CASE 3: $f_3(y) \leq f_1(y) \leq f_4(y) \leq f_2(y)$, for all y.

These assumptions may not hold in a given interval. But, in any of the problems that have been considered, one can break up the interval into a finite number of subintervals in which they do hold and consider each separately.

For $i = 1, 2, 3, 4$ we define the quantities

$$A(i) = a_i(T_{x+b_i} - T_{x+n+b_i}) + (1 - a_i)n\cdot l_{b_i}\ ;^1$$

$$B(i) = a_i(Y_{x+b_i} - Y_{x+n+b_i}) + (1 - a_i)n\cdot T_{b_i}\ ;$$

[1] We can define l_z and T_z arbitrarily for negative z, since, if b_i is negative, a_i must equal 1.

$$C(i) = a_i[Y_{x+b_i} - Y_{x+n+b_i} + xT_{x+b_i} - (x+n)T_{x+n+b_i}] + (1-a_i)\left[\left(nx+\frac{n^2}{2}\right)l_{b_i}\right].$$

The following results are then easily derived.

$$\text{Total number of people} = [A(1) - A(2)].$$

In total lifetime we must consider separately the three cases listed above, in order to distinguish between lifetime lived before $f_1(y)$, which is certain, and lifetime lived after $f_1(y)$, which is contingent upon survival.

Total lifetime equals:

CASE 1: $[B(3) - B(4)] - (a_4 - a_3)C(2) - (b_4 - b_3)A(2)$;
CASE 2: $(a_4 - a_3)[C(1) - C(2)] + (b_4 - b_3)[A(1) - A(2)]$;
CASE 3: $[B(1) - B(4)] + (a_1 - a_3)C(1) - (a_4 - a_3)C(2) + (b_1 - b_3)A(1) - (b_4 - b_3)A(2)$.

I will illustrate with a few examples.

a) Finding total ages at death.—In this general type of problem, $f_3 = 0$ and $f_4 = f_2$. Note that

$$B(i) + a_iC(i) + b_iA(i) = a_i(G_{x+b_i} - G_{x+n+b_i}) + (1-a_i)n\cdot F_{b_i},$$

where F and G are as defined by Grace and Nesbitt (*TSA*, Vol. II).

Substituting in the Case 3 formula and comparing with $A(1) - A(2)$, we obtain the form of answer given by Grace and Nesbitt.

b) Batten's example, Section V.—Here we must consider two separate intervals, as Batten does. We have the following data:

	a_1	a_2	a_3	a_4	b_1	b_2	b_3	b_4	Case
$25\leq y\leq 30$......	0	1	0	1	30	20	0	0	2
$30\leq y\leq 40$......	1	0	0	1	0	50	0	0	2

c) Batten's example 1, Section IV.—This familiar problem is, of course, a special case of example *a*. It is commonly believed to be a different type of problem from example *b*, since it does not appear to deal with a closed group of people at the beginning. It really does, however, if we look at it in the right way. Take $m = 1$ for simplicity. We must consider three separate intervals with the following data:

	a_1	a_2	b_1	b_2
$x-1\leq y\leq x$..............	0	1	x	1
$x\leq y\leq x+n-1$..........	1	1	0	1
$x+n-1\leq y\leq x+n$........	1	0	0	$x+n$

One can also easily conceive of problems in which the a_i's are constants other than 0 or 1. For example, consider groups of people who will or will not double their present ages. The definitions of A, B, and C above can easily be modified to cover such cases.

Finally, I would like to add that, in my opinion, none of the proposed methods for doing population problems will enable the student to obtain a complete grasp of the subject. The main difficulty is due to the fact that one is dealing with a continuous model which has no actual physical realization. In teaching this subject and in learning it myself, I have found that the best approach is to view the continuous model as a limiting case of discrete ones. Accordingly, one should first study for an arbitrary integer n a population generated by $(1/n) \cdot l_0$ births at the beginning of each $1/n$ years. The resulting population is, of course, not stationary, but it is what we may call $1/n$-stationary; that is, the distribution of ages at any time t is the same as that at time $t + 1/n$. The $1/n$-stationary population is an easily visualized object, and any questions asked about it can be answered by a straightforward counting procedure. The answer to any question asked about the stationary population is the limit as n tends to infinity, of the corresponding answers for the $1/n$-stationary populations.

GARY N. SEE:

Professor Batten has made a noteworthy contribution to the literature on stationary population theory. Through the use of some basic concepts, he shows us how it is possible by general reasoning to solve relatively complex problems. Aside from this desirable result, the student's understanding and insight should be increased thereby. For reasons discussed later, however, I would caution students studying for the Part 4 examination against neglecting to master the "traditional" method and putting too much reliance on Batten's approach.

Batten contends that "any problem involving a stationary population concept can be solved, without integrals, diagrams, mnemonic devices, in a comparatively short time." This is true in many cases but in reference to problems posed on the examination it does not necessarily follow, because a problem can be stated in such a fashion as to effectively exclude methods other than the integral method, which presumably is the one on which the student is being tested. While there exist a number of past examination questions which illustrate this, it seems apropos to examine the problem which has just appeared on the May 15, 1969, examination:

Find an expression in integral form for the average future lifetime of those in the stationary population now living between ages 18 and 25, who will die between ages 21 and 30 and within 10 years from the present time.

The requirement that the answer be "in integral form" would appear to preclude methods other than the integral approach. It is for this reason that I feel that Batten's approach does not remove the necessity for the student to master the traditional method.

If one removes the required form of the solution, the solution for this particular problem which requires the least thought, it seems to me, is Mr. Veit's method, since, in using his approach, one does not consciously have to make required subgroupings of the population to solve the problem, as appears to be necessary in both Batten's method and the integral method.

If the student recognizes the subgroupings which should be made, the traditional method is not difficult, as the integrals may be written directly as follows:

$$\frac{\int_{18}^{20}\int_{21-y}^{10} f(y,t)\,dt\,dy + \int_{20}^{21}\int_{21-y}^{30-y} f(y,t)\,dt\,dy + \int_{21}^{25}\int_{0}^{30-y} f(y,t)\,dt\,dy}{\int_{18}^{20}\int_{21-y}^{10} g(y,t)\,dt\,dy + \int_{20}^{21}\int_{21-y}^{30-y} g(y,t)\,dt\,dy + \int_{21}^{25}\int_{0}^{30-y} g(y,t)\,dt\,dy},$$

where $f(y,t) = t \cdot l_{y+t}\mu_{y+t}$ and $g(y,t) = l_{y+t}\mu_{y+t}$.

While the above is all that would be required for the solution to the 1969 problem, the evaluation of the above expression, if it were required, is not too time-consuming because of the simplicity of the functions involved. Batten's point of the possibility of a careless error is valid, but a minor error in calculus would probably not result in much loss in grade. On the other hand, if one goes astray in a general reasoning approach, the error might be more costly unless the student very carefully documents his reasoning step by step. If he wisely does this, there is some question as to any appreciable difference in time required to solve the problem. Another possible drawback of Batten's method for some students is whether, under the stress of examination conditions, reliance solely on general reasoning would be wise.

The foregoing problem is of the type illustrated in Batten's Section V. It seems to me that if one has mastered his approach, which involves a thorough understanding of concepts, it is just as easy to write the integral expression directly.

In conclusion, while Batten's paper contributes a great deal to this subject, I do not think that it should be regarded as a panacea by students studying for the Part 4 examination.

(AUTHOR'S REVIEW OF DISCUSSION)

ROBERT W. BATTEN:

I owe a debt of gratitude to the five discussants of my paper, as do all present and future students of life contingencies. Doubtless, some actuaries feel that the topic of population theory has received an inordinate amount of attention in recent *Transactions*, but this contention seems to be refuted by the quality of the discussion which such papers provoke.

Professor Crofts has pursued and extended the logical approach by considering other verbal descriptions of the basic symbols. I am particularly gratified by his tacit agreement that general reasoning approaches are to be preferred to sophisticated mathematics in the presentation of such problems to students.

Quite understandably, it is often the tendency of students to invest little effort in pursuing concepts, such as that of the stationary population, which are seemingly at odds with reality. It is along these lines that Dr. Hickman's comments have made a very significant contribution. His examples effectively rebut those who favor removal of population theory from the syllabus on grounds of nonapplicability to "real life" actuarial problems.

By concentrating on the l_x curve, Mrs. Tino has suggested a general method of solution by which the original form of the answer may be read directly from a simple diagram. For examination purposes it appears that this method may be rather time-consuming, as it necessitates both construction of diagrams and integration. Nonetheless, she has made a contribution to the literature in the light of the basic concept upon which her method rests.

Professor Promislow's generalized formulas are elegantly presented. Particularly intriguing was his comment concerning a "$1/n$-stationary population," an entity which many students would do well to investigate.

Mr. See has forwarded the contention that a logical approach to population problems should not be the only weapon in the student's arsenal, an argument which he substantiates by quoting the 1969 Part 4 Examination problem covering this material. I certainly concur that a student has not mastered any concept which he can handle in only one fashion. The serious student tries to comprehend and use as many approaches as possible, and this certainly includes the expression of stationary populations in integral form. I strongly disagree, however, with the implication that a student who is able to present a complete and correct answer to a given problem should be ordered to express a solution by a specified method in a specified form and, in fact, to leave that answer in a form which is as difficult to evaluate as the problem originally posed.

I would like to express my appreciation to Mr. Francisco Bayo, who brought to my attention an error in the original draft of this paper.

My purpose in writing this paper was to add a totally logical approach to the many other devices available to the actuarial student of life contingencies population theory, while at the same time enhancing his basic understanding of the symbols and concepts involved. Hopefully, with the insights added by this discussion, progress has been made in this direction.

MORTALITY AND REMARRIAGE EXPERIENCE FOR WIDOW BENEFICIARIES UNDER OASDI

FRANCISCO BAYO

SEE PAGE 59 OF THIS VOLUME

JAMES L. COWEN:

Mr. Bayo's fine paper provides valuable information on the remarriage and mortality experience of widows in the United States. The data used to derive the tables are the most extensive ever assembled to prepare remarriage rates. I can only hope that, in spite of the changes in the eligibility conditions introduced by the 1965 social security amendments, Mr. Bayo will be able to continue developing information on remarriage patterns of older widows that will not be blurred.

The OASDI remarriage rates contained in this paper have been compared with the 1955–62 railroad retirement (RRB) experience that I presented in Volume XVII of *TSA*. In the technical supplement to our tenth actuarial valuation, we presented the 1958–65 RRB experience. As has been the case in the past, the OASDI remarriage rates were considerably higher than those experienced by the widows of railroad employees. Ratios of actual to expected remarriages of railroad widows for the years 1958–65 on the basis of the OASDI rates are given in the accompanying table.

The lower remarriage rates for widows of railroad employees are a puzzle to us, especially since the eligibility conditions under OASDI and railroad retirement are virtually the same. There should be little or no effect due to the procedural differences in the way in which the studies were made. (The RRB studies use a policy-year observation period based on anniversaries of widowhood and measure durations from onset of widowhood, while OASDI studies use a calendar-year observation period and measure duration from entitlement to benefits.) The only effect should be at ages around 62, the previous earliest age of entitlement to benefits under OASDI when the widow has no dependent children in her care. There is no reason to believe that the RRB records are incomplete. Records are made up for mothers otherwise eligible who do not receive benefits because they are working full time, and the Board is generally notified of their remarriage because the checks are usually made out in their names.

Age*	Per Cent Ratios of Actual to Expected Remarriages for Duration					
	0	1	2	3	4	5 and Over
	Mother Beneficiaries					
Under 30....	66.7%	86.4%	94.1%	70.8%	78.9%	76.9%
30–34.......	76.4	90.9	84.8	76.0	95.0	70.5
35–39.......	77.2	91.2	92.2	77.1	87.3	74.7
40–44.......	60.5	83.9	81.2	69.2	98.2	67.3
45–49.......	45.8	67.1	57.8	72.3	66.7	80.2
50–54.......	66.7	53.6	62.5	70.6	†	68.3
55–59.......	†	†	†	†	†	60.9
Total.....	66.2%	82.4%	80.0%	73.0%	84.8%	72.2%
	Widow Beneficiaries					
62–64.......	71.4%	103.8%	89.9%	109.1%	117.9%	†
65–69.......	55.6	109.9	82.2	95.3	106.3	103.7%
70–74.......	76.7	96.8	84.9	65.8	96.3	83.8
75–79.......	†	52.4	37.5	50.0	†	84.2
80–84.......	†	†	†	†	†	88.2
Total.....	61.0%	100.9%	83.0%	92.5%	107.3%	91.5%

* Age last birthday at onset of widowhood.

† Less than five actual remarriages in the experience.

Note.—Ages at onset of widowhood 60 and 61 have been omitted in this table because they would be classified as widows under the railroad retirement program but the OASDI table for widows does not include these ages.

As Mr. Bayo states, the RRB rates, although lower, showed the same general trends as the OASDI experience. This includes the discontinuity between the experience of mothers and that for aged widows and the decreases in the remarriage rates since the 1956 RRB and OASDI studies.

The graduated rates, however, do contain one significant difference. Even though there was no continuity between the experience of mothers and the experience of widows, the 1962 RRB table shown in Volume XVII of *TSA* did merge the two sets of rates by ignoring the fit at ages 45–59. As explained in Volume XVII of *TSA*, it was felt that this approach was more appropriate for deriving a table to be used for valuation purposes. As we understand it, the OASDI actuaries do not use their remarriage tables for valuation purposes, so Mr. Bayo had no need to merge the experiences of the mothers and widows.

The mortality experience of OASDI widows and mothers is not significantly different from that of the comparable classes of beneficiaries covered by the railroad retirement program. Using Mr. Bayo's graduated rates against our experience for calendar years 1962–64, we obtained ratios of actual to expected deaths of 108.1 per cent for mothers and of 99.9 per cent for widows.

WALTER RIESE:

In publishing this paper, Mr. Bayo provides a welcome addition to the stock of the more exotic types of decrement tables for which actuaries have an insatiable appetite.

A rather interesting point arises in connection with Table 3, in which married female mortality and widowed female mortality are compared. The author notes that differentials in mortality decrease with age. While this is true in terms of multiple mortality, the reverse is true in terms of extra deaths per thousand, as shown in the accompanying table, which is Table 3 in the paper, with one column added.

COMPARISON OF MARRIED FEMALE MORTALITY AND WIDOWED FEMALE MORTALITY, UNITED STATES POPULATION, 1959–61

Age	Central Death Rate per Thousand		Ratio of Col. (2) to Col. (3)	Extra Deaths per Thousand Col. (2) − Col. (3)
	Widowed	Married		
(1)	(2)	(3)	(4)	(5)
20–24	2.47	0.58	4.26	1.89
25–29	2.40	0.74	3.24	1.66
30–34	3.30	1.04	3.17	2.26
35–39	3.88	1.54	2.52	2.34
40–44	5.16	2.41	2.14	2.75
45–49	6.81	3.70	1.84	3.11
50–54	9.45	5.60	1.69	3.85
55–59	12.17	8.02	1.52	4.15
60–64	18.19	12.91	1.41	5.28
65–69	25.97	19.99	1.30	5.98
70–74	39.85	32.52	1.23	7.33
75–79	64.57	53.30	1.21	11.27
80–84	110.23	93.96	1.17	16.27
85 and over	205.71	134.78	1.53	70.93

Column 5 of the table may provide the kind of support that the author was seeking for his fascinating hypothesis regarding a matching process in marriage on the basis of health. It is not clear to me, however, why extra mortality associated with widowhood would have to be increasing with age, either in terms of percentage or in terms of extra deaths per thousand. It does not seem unreasonable to expect that, as the basis force of mortality increases with age, the extra mortality from some particular source might gradually decline.

With regard to the mortality differential between married females and widowed females, however, the theory that widowhood affects mortality ought not to be rejected too quickly. Notwithstanding the suggestion of questionable validity of population statistics by marital status, it is interesting to speculate to what extent the psychological factor of "feeling needed" may affect longevity. There seems to be some support for this theory in Table 7, which shows lower mortality for mother beneficiaries than for widows generally.

It may be of interest to record a few statistics indicative of the remarriage experience among widows of active and retired contributors under the Public Service Superannuation Act of Canada for the five years ending December 31, 1967 (see accompanying table).

REMARRIAGE EXPERIENCE, 1963-67, OF WIDOWS OF CANADIAN FEDERAL GOVERNMENT EMPLOYEES

Duration	Number of Remarriages		Ratio of Col. (2) to Col. (3)
	Actual	Expected*	
(1)	(2)	(3)	(4)
0	26	57.8	0.45
1	79	85.7	0.92
2	90	78.0	1.15
3	69	60.2	1.15
4	56	43.8	1.28
0-4	320	325.5	0.98
5 and up	154	149.9	1.03
Total	474	475.4	1.00

* The expected number of remarriages was calculated on the basis of the graduated 1960-62 OASDI experience, assuming that Tables 15 and 16 made up one continuous table and assuming a rate of 3.1 per thousand for those ages at widowhood below 62 at attained ages below 67 which are beyond the range of Table 15.

Although the ratio of actual to expected remarriages seems low at duration 0 and high at duration 4, the experience is rather small for definite conclusions to be drawn. There is a surprising degree of agreement between the actual and expected number of remarriages for durations 0-4 combined as well as for durations 5 and over combined, and there is almost perfect agreement in total.

Apparently in contrast to the trend under OASDI, the experience of widows of Canadian public servants or retired public servants indicates a general level of remarriage rates during the 1963–67 period about 45 per cent higher than that in the preceding five years. However, here again the size of the data indicates caution.

(AUTHOR'S REVIEW OF DISCUSSION)

FRANCISCO BAYO:

I thank Mr. James L. Cowen for his comments. We are all aware of his excellent work on the same subject dealing with railroad retirement beneficiaries.

With regard to his hope for similar analyses in the future, an investigation is being conducted currently to determine the feasibility of further OASDI widow remarriage studies. Since the 1965 amendments to the Social Security Act eliminated the termination of widow's benefits because of remarriage, it is now necessary to determine whether there is a simple way to obtain data on the totality of remarriages that occur after the amendments and, if so, the cost involved.

It is of interest to see Mr. Cowen's table on the ratio of actual to expected marriages based on the 1958–65 RRB experience. Since the periods of observation (1960–62 for OASDI vs. 1958–65 for RRB) now conform more closely, there should be little doubt that more remarriages are being observed in OASDI than there are in the railroad retirement system. We do not yet know why this is the case.

The ratios of actual to expected deaths presented by Mr. Cowen seem to indicate that OASDI and RRB widow's mortality and mother's mortality are similar, but it is possible to infer that RRB mortality is slightly higher, since for widows, where they are almost equal, a larger portion of the healthier lives are excluded from OASDI than from RRB because of the entitlement to old age benefits.

In regard to Mr. Walter Riese's comments, I find of interest his observation that the differential in mortality between widows and married females increases with age in absolute terms although it decreases in terms of excess ratios. I believe that we could conceive of an increasing absolute differential if we accept the idea that mortality, except perhaps when due to violence (say, homicides and accidents), tends to be due to several causes operating at the same time, particularly at the older ages, although in general only the ultimate cause is recorded. In this sense, an intrinsic level absolute excess could be observed as an excess that increases with age because of interactions with increasing mortality from other causes.

I entirely agree with Mr. Riese that there is some room for speculation about psychological factors in the mortality of human beings. The "feeling needed" state of mind should improve the chances of survival. In a study published in the August 31, 1963, issue of the *Lancet*, Mr. Michael Young *et al.* found that widowers have a 40 per cent excess mortality in the six-month period immediately after bereavement and that the differential decreases asymptotically thereafter. On the other hand, in a study prepared by Messrs. Peter R. Cox and John R. Cox, published in the January 18, 1964, issue of the *Lancet*, it was found that there is a delay effect in mortality after the loss of a husband. The mortality of widow beneficiaries in the British National Contributory Pension System was found to be higher in the second and third year after bereavement than in any other year. We have also observed slightly lower mortality for OASDI widows in the early durations, but at this moment it is not known what causes the delay in the effect.

AN ANALYSIS OF CONTRIBUTIONS TO SURPLUS

ROBERT H. JORDAN

SEE PAGE 81 OF THIS VOLUME

JOSEPH A. SCHWARZ:

This is a very interesting analysis of the interplay of asset share and dividend formulas. Many an actuarial student has been quite shocked in his first contact with asset shares to find on a whole life policy that the asset share run-out to the terminal age can reach such seemingly impossible results as \$3,000 per \$1,000 of insurance, plus or minus! The author's method pinpoints very neatly the reasons that this occurs—sometimes legitimately as a result of the powerful effect of interest and survivorship, sometimes augmented by disparities between dividend and asset share assumptions. In this connection it might be mentioned that in this machine age, where asset shares are so readily computed, it is handy to introduce a special function which represents the present value at issue of the excess of the asset share over the cash value or similar goal. Once the situation is attained that asset share assumptions are the same as those for dividends with respect to interest, mortality, and expenses, this function will become constant. Considered over a one-year period and using formulas (7) and (8) in the article with all but ${}_t\bar{C}^s$ equal to zero, we have

$$
{}_t\bar{C} \doteq \frac{1}{1 - q_{t-1}^{AS} - wq_{t-1}^{AS}} \cdot {}_t\bar{C}^s ,
$$

$$
{}_t\bar{S} = {}_{t-1}\bar{S} + {}_t\bar{C} = {}_{t-1}\bar{S}\left(1 + \frac{i^{AS} + q_{t-1}^{AS} + wq_{t-1}^{AS}}{1 - q_{t-1}^{AS} - wq_{t-1}^{AS}}\right)
$$

$$
= {}_{t-1}\bar{S}\,\frac{1 + i^{AS}}{1 - q_{t-1}^{AS} - wq_{t-1}^{AS}}
$$

or

$$
v(1 - q_{t-1}^{AS} - wq_{t-1}^{AS})\,{}_t\bar{S} = {}_{t-1}\bar{S} \,.
$$

Extending this back to the time of issue does not disturb this relationship. Actually, even when its value is changing with duration, it readily assesses whether what would appear to be a large change in ${}_t\bar{S}$ is really significant.

The author chose to analyze the situation after the initial period during which initial expenses are being amortized and select mortality gains are being credited. It is instructive to carry the analysis into this period, especially if the dividend formula is modified in keeping with the article, so that a simplified fund formula effectively results. Thus, let

$$\begin{aligned} {}_tD = {} & (\bar{L} - {}_tE^D)(1 + i^D) + (i^D - i^v)[{}_{t-1}\bar{V}(\bar{A}) + \bar{P}^d] \\ & + (q^v_{t-1} - q^D_{t-1})[1{,}000 - {}_t\bar{V}(\bar{A})] - q^D[{}_t\bar{V}(\bar{A}) - {}_t\mathrm{CV}] \\ & - i^D[{}_{t-1}\bar{V}(\bar{A}) - {}_{t-1}\mathrm{CV}] - [{}_{t-1}\bar{V}(\bar{A}) - {}_{t-1}\mathrm{CV}] \\ & + [{}_t\bar{V}(\bar{A}) - {}_t\mathrm{CV}]\,. \end{aligned}$$

The additional terms compared with formula (2) are necessary to reflect the fact that the dividend fund or cash value in this case is initially lower than the reserves, giving rise to a greater amount at risk, a smaller interest-bearing base, and a greater required build-up of funds compared with the reserve assumptions.

Then, if surplus is defined as ${}_t\bar{S} = {}_t\mathrm{AS} - {}_t\bar{V}(\bar{A})$ and, as in formula (5), ${}_t\bar{C} = {}_t\mathrm{AS} - {}_t\bar{V}(\bar{A}) - {}_{t-1}\bar{S}$, ${}_t\bar{C}$ then becomes with the additional items for ${}_tD$,

$${}_t\bar{C} \doteq \frac{1}{1 - q^{\mathrm{AS}}_{t-1} - wq^{\mathrm{AS}}_{t-1}} \left\{ \begin{aligned} & (i^{\mathrm{AS}} - i^D)[{}_{t-1}\bar{V}(\bar{A}) + \bar{P}^d + \bar{L}] \\ & \quad + {}_tE^D \cdot i^D - {}_tE^{\mathrm{AS}} \cdot i^{\mathrm{AS}} + i^D \cdot {}_{t-1}N \\ & + (q^D_{t-1} - q^{\mathrm{AS}}_{t-1})[1{,}000 - {}_t\bar{V}(\bar{A})] \\ & \quad + q^V_{t-1} \cdot F^V - q^{\mathrm{AS}}_{t-1} \cdot F^{\mathrm{AS}} + q^D_{t-1} \cdot {}_tN \\ & + {}_tE^D - {}_tE^{\mathrm{AS}} + {}_{t-1}N - {}_tN \\ & + wq^{\mathrm{AS}}_{t-1} \cdot {}_tN \\ & + {}_{t-1}\bar{S}(i^{\mathrm{AS}} + q^{\mathrm{AS}}_{t-1} + wq^{\mathrm{AS}}_{t-1})\,. \end{aligned} \right.$$

In the above, ${}_tN = {}_t\bar{V}(\bar{A}) - {}_t\mathrm{CV}$.

Furthermore, if we include the provision for termination dividends given in formula (15), eliminate the fraction in the above formula, and use the relationship between ${}_t\bar{S}$, ${}_t\mathrm{AS}$, ${}_t\bar{V}(\bar{A})$, ${}_t\mathrm{CV}$, and ${}_tN$, we have

$${}_t\bar{C} = \left\{ \begin{aligned} & (i^{\mathrm{AS}} - i^D)({}_{t-1}\mathrm{CV} + \bar{P}^D + \bar{L}) + {}_tE^D \cdot i^D - {}_tE^{\mathrm{AS}} \cdot i^{\mathrm{AS}} && \text{(a)} \\ & \quad + i^{\mathrm{AS}} \cdot ({}_{t-1}\bar{S} + {}_{t-1}N)\,; \\ & + (q^D_{t-1} - q^{\mathrm{AS}}_{t-1})(1{,}000 - {}_t\mathrm{CV}) + q^V_{t-1} \cdot F^V - q^{\mathrm{AS}}_{t-1} \cdot F^{\mathrm{AS}} && \text{(b)} \\ & \quad + q^{\mathrm{AS}}_{t-1}\left[{}_t\bar{S} + {}_tN - \mathrm{TD}\left(1 + \frac{i^{\mathrm{AS}}}{2}\right)\right]; \\ & + wq^{\mathrm{AS}}_{t-1}({}_t\bar{S} + {}_tN - \mathrm{TD})\,; && \text{(c)} \\ & + ({}_tE^D - {}_tE^{\mathrm{AS}}) + ({}_{t-1}N - {}_tN)\,. && \text{(d)} \end{aligned} \right.$$

Portion (a) indicates that any interest earned on funds on hand in excess of dividend formula requirements increases surplus. In later years, when ${}_{t-1}N$ would be zero, there would also be a significant increase in surplus because of interest earned thereon. In the early years, when the surplus would generally be negative, ${}_{t-1}N$ would tend to be of about the same magnitude as this deficit, and the last term would generally not be significant.

Of course, i^D could be set larger than the experience rate i^{AS}, so that the resulting loss in the first term is offset in the over all by the profit from the last.

Portion (b) similarly indicates that profits arise from any excess mortality, based on the amount at risk in excess of funds at hand, augmented by surplus released at death. In this connection, surplus would include also the excess of reserves over cash values; in other words, the funds released are the excess of ${}_t\text{AS}$ over ${}_t\text{CV}$. Any terminal dividends paid would derive from these funds released. Depending on how they are applied in the dividend formula, select mortality profits could be an important element here.

Portion (c) is similar to portion (b) in that funds released on withdrawals in excess of cash values are a source of profit, or of terminal dividends. In a co-ordinated dividend fund system, all terminal dividends would be basically derived directly from $({}_t\bar{S} + {}_tN)$ or the equivalent thereof.

Portion (d) shows that differences between dividend formula assumptions and actual experience in the expense area also affect profits. It also shows that surplus is increased by the reduction in the investment in new business or, put otherwise, by the amortization of initial expenses. In a dividend fund formula this would result from the definition of dividend funds with respect to unamortized expenses; in the more usual dividend formulas, ${}_tE^D$ would be increased to include such an element.

J. ALAN LAUER:

In Appendix B, Mr. Jordan develops an asset share formula in which the working of the Phase I portion of the federal income tax law is reflected in the interest credited in the accumulation. He states that his development is a "rough" approach in which refinement has been ignored. The following development, which is based on the marginal tax rates described by John C. Fraser (in *TSA*, XIV, 51) may be of interest. This development is applicable only to companies which are in situation B described by Fraser, although it could be modified to apply to companies in other situations.

Jordan's formula (3), with subscript $[x]$ omitted, is

$$
\begin{aligned}
{}_t\text{AS} = \Big[& ({}_{t-1}\text{AS} + \text{GP} - {}_tE^{\text{AS}})(1 + i^{\text{AS}}) \\
& - q_{t-1}^{\text{AS}}\left(1{,}000 + \frac{\text{GP}}{2}\right)\left(1 + \frac{i^{\text{AS}}}{2}\right) \\
& - {}_tD - {}_t\text{CV}\cdot wq_{t-1}^{\text{AS}}\Big] \div (1 - q_{t-1}^{\text{AS}} - wq_{t-1}^{\text{AS}}) .
\end{aligned}
\tag{3}
$$

It can be restated as

$$
\begin{aligned}
{}_t\text{AS} = \Big[& ({}_{t-1}\text{AS} + \text{GP} - {}_tE^{\text{AS}}) \\
& - q_{t-1}^{\text{AS}}\left(1{,}000 + \frac{\text{GP}}{2}\right) + ({}_tI^{\text{BT}} - {}_tT) \\
& - {}_tD - {}_t\text{CV}\cdot wq_{t-1}^{\text{AS}}\Big] \div (1 - q_{t-1}^{\text{AS}} - wq_{t-1}^{\text{AS}}) ,
\end{aligned}
\tag{L1}
$$

where ${}_tI^{\text{BT}}$ is the net investment income attributable to the particular asset share cell in policy year t after investment expenses but before federal income tax, and ${}_tT$ is the amount of federal income tax attributable to the same cell in policy year t. It can be seen that $({}_tI^{\text{BT}} - {}_tT)$ is the net investment income after federal income tax.

Because most of the income tax must be paid during the calendar year in which it accrues, interest for approximately one-half year is lost on the amount of tax paid. Thus the term ${}_tI^{\text{BT}}$ can be determined as

$$
{}_tI^{\text{BT}} = i^{\text{BT}}({}_tB - \tfrac{1}{2}{}_tT) , \tag{L2}
$$

where i^{BT} is the net earned interest rate after investment expenses but before federal income tax and

$$
{}_tB = ({}_{t-1}\text{AS} + \text{GP} - {}_tE^{\text{AS}}) - \tfrac{1}{2}q_{t-1}^{\text{AS}}\left(1{,}000 + \frac{\text{GP}}{2}\right) . \tag{L3}
$$

The term ${}_tB$ represents the amount at interest (unadjusted for the amount of income tax) in the asset share formula and will be recognized as the same amount that is multiplied by i^{BT} to obtain I^{BT} in Jordan's development.

Before investigating T, we must define some terms:

m^A = Marginal tax rate applicable to assets.
m^T = Marginal tax rate applicable to fully taxable investment income.
m^{NT} = Marginal tax rate applicable to wholly tax-exempt investment income.
$m^{\text{NP},k}$ = Marginal tax rate applicable to nonpension plan reserves valued at interest rate k.
h = Ratio of fully taxable investment income to total investment income.
${}_tA$ = Mean assets in policy year t.
${}_t\text{MV}$ = Valuation mean reserve in policy year t.

${}_tT$ can now be expressed as follows:

$$ {}_tT = m^A \cdot {}_tA + h \cdot m^T \cdot {}_tI^{\text{BT}} + (1-h) m^{\text{NT}} \cdot {}_tI^{\text{BT}} - m^{\text{NP},k} \cdot {}_t\text{MV} \,. \quad \text{(L4)} $$

Note that, for qualified pension plans, the marginal tax rate applicable to pension plan reserves would be substituted for $m^{\text{NP},k}$.

The mean assets in policy year t can be stated as the initial funds minus one-half of the year's death claims and minus one-half of the income tax for the year, all increased by one-half of a year's interest. That is,

$$ \begin{aligned} {}_tA = {} & ({}_{t-1}\text{AS} + \text{GP} - {}_tE^{\text{AS}}) \left(1 + \frac{i^{\text{BT}}}{2}\right) \\ & - \tfrac{1}{2} q_{t-1}^{\text{AS}} \left(1{,}000 + \frac{\text{GP}}{2}\right)\left(1 + \frac{i^{\text{BT}}}{2}\right) - \tfrac{1}{2}{}_tT \left(1 + \frac{i^{\text{BT}}}{2}\right). \end{aligned} \quad \text{(L5)} $$

Formula (L5) was derived intuitively and is not the only possible formula for ${}_tA$, but this formula has a theoretical and a practical advantage. The theoretical advantage is that

$$ \begin{aligned} {}_tA = {} & \tfrac{1}{2}[({}_{t-1}\text{AS} + \text{GP} - {}_tE^{\text{AS}}) + (1 - q_{t-1}^{\text{AS}} - wq_{t-1}^{\text{AS}})_t\text{AS} \\ & + {}_tD + {}_t\text{CV} \cdot wq_{t-1}^{\text{AS}}] \,. \end{aligned} \quad \text{(L6)} $$

That is to say, ${}_tA$ is the mean of the funds at the beginning and end of the policy year. Formula (L6) can be derived from formulas (3) and (L5) if formula (3) is first adjusted by the substitution of i^{BT} for i^{AS} and the addition of the term $-{}_tT(1 + i^{\text{BT}}/2)$.

The practical advantage of formula (L5) is that, in combination with formula (L3), it leads to

$$ {}_tA = ({}_tB - \tfrac{1}{2}{}_tT)\left(1 + \frac{i^{\text{BT}}}{2}\right). \quad \text{(L7)} $$

Now, substituting formulas (L2) and (L7) into formula (L4), we have

$$ \begin{aligned} {}_tT = {} & m^A({}_tB - \tfrac{1}{2}{}_tT)\left(1 + \frac{i^{\text{BT}}}{2}\right) + h \cdot m^T \cdot i^{\text{BT}}({}_tB - \tfrac{1}{2}{}_tT) \\ & + (1-h) m^{\text{NT}} \cdot i^{\text{BT}}({}_tB - \tfrac{1}{2}{}_tT) - m^{\text{NP},k} \cdot {}_t\text{MV} \,. \end{aligned} \quad \text{(L8)} $$

A little algebraic manipulation gives us

$$ \begin{aligned} {}_tT = {} & \left\{ i^{\text{BT}} \cdot {}_tB\left[\frac{m^A}{2} + h \cdot m^T + (1-h) m^{\text{NT}}\right] \right. \\ & \left. + m^A \cdot {}_tB - m^{\text{NP},k} \cdot {}_t\text{MV} \right\} \\ & \div \left\{ 1 + \tfrac{1}{2} i^{\text{BT}}\left[\frac{m^A}{2} + h \cdot m^T + (1-h) m^{\text{NT}}\right] + \tfrac{1}{2} m^A \right\} . \end{aligned} \quad \text{(L9)} $$

From formulas (L2) and (L9)

$$I^{\mathrm{BT}} - {}_tT = i^{\mathrm{BT}} \cdot {}_tB - {}_tT\left(1 + \frac{i^{\mathrm{BT}}}{2}\right) \tag{L10}$$

$$= R^B \cdot {}_tB + R^V \cdot {}_t\mathrm{MV} \, , \tag{L11}$$

where

$$R^B = i^{\mathrm{BT}} - \frac{1 + i^{\mathrm{BT}}/2}{1 + \frac{1}{2}(W \cdot i^{\mathrm{BT}} + m^A)} (W \cdot i^{\mathrm{BT}} + m^A) \, , \tag{L12}$$

$$R^V = \frac{1 + i^{\mathrm{BT}}/2}{1 + \frac{1}{2}(W \cdot i^{\mathrm{BT}} + m^A)} m^{\mathrm{NP},k} \, , \tag{L13}$$

and

$$W = \frac{m^A}{2} + h \cdot m^T + (1 - h) m^{\mathrm{NT}} \, . \tag{L14}$$

The asset share formula can now be restated as

$$\begin{aligned} {}_t\mathrm{AS} = \Big[& ({}_{t-1}\mathrm{AS} + \mathrm{GP} - {}_tE^{\mathrm{AS}})(1 + R^B) \\ & - q_{t-1}^{\mathrm{AS}}\left(1{,}000 + \frac{\mathrm{GP}}{2}\right)\left(1 + \frac{R^B}{2}\right) + R^V \cdot {}_t\mathrm{MV} \\ & - {}_tD - {}_t\mathrm{CV} \cdot wq_{t-1}^{\mathrm{AS}} \Big] - (1 - q_{t-1}^{\mathrm{AS}} - wq_{t-1}^{\mathrm{AS}}) \, . \end{aligned} \tag{L15}$$

In the foregoing development, ${}_t\mathrm{MV}$ has been used as the factor to which R^V should be applied. Jordan used ${}_t\bar{V}(\bar{A})$ in his development. The mean reserve has more theoretical merit, because the mean reserve is the function which is entered in the income tax return. On the other hand, there is a practical reason for using the terminal reserve, which is usually an adequate approximation to the mean reserve. It is common when making asset share calculations to compare the resulting asset shares with the respective terminal reserves. The calculation is simplified slightly if the asset share formula involves the function with which the asset share is being compared rather than some other function.

It is difficult to compare the numerical results of my formula with those of Jordan's formula, because the numerical values of the marginal tax rates are different for each company and even in the same company are apt to change over a period of years. In short, the numerical values of the marginal tax rates are dependent on a rather formidable set of assumptions which are unique to a particular company at any given time. These assumptions are easily determined for a particular past year, given a company's actual income tax return for that year. In some situations it may be desired to extrapolate the marginal tax rates into the future in order to approximate the conditions that will prevail while a particular scale of premiums or dividends is in use. At any rate, Table 1 is presented to provide at least some comparison of the two methods.

The values of R^B and R^V for Lauer's method are based on marginal tax rates applicable in Provident Mutual in 1967 and are calculated by formulas (L12) and (L13), respectively. The values of R^B and R^V for Jordan's method are $0.52i^{BT}$ and $0.48i^{BT}(1 + 10i^V - 10i^{BT})$, respectively. In both cases a 48 per cent corporate tax rate with no surcharge has been assumed. It can be seen that there is some difference between the values of R^B and R^V under the two methods, but the combined values

TABLE 1

	Jordan	Lauer	Jordan	Lauer
	$i^{BT}=4.5\%$; $i^V=2.5\%$		$i^{BT}=5.5\%$; $i^V=2.5\%$	
R^B	2.34%	2.46%	2.86%	3.14%
R^V	1.73	1.58	1.85	1.59
R^B+R^V	4.07	4.04	4.71	4.73
	$i^{BT}=4.5\%$; $i^V=3.5\%$		$i^{BT}=5.5\%$; $i^V=3.5\%$	
R^B	2.34%	2.46%	2.86%	3.14%
R^V	1.94	1.77	2.11	1.78
R^B+R^V	4.28	4.23	4.97	4.92

$R^B + R^V$ are very close in this example. In the asset share formula, R^B is applied to ${}_tB$ and R^V to the reserve (either mean or terminal); but it can also be considered that $R^B + R^V$ is applied to the reserve and R^B is applied to the excess (positive or negative) of ${}_tB$ over the reserve. In cases where the difference between ${}_tB$ and the reserve is relatively small, the combined value $R^B + R^V$ is much more significant than either of the separate values R^B and R^V. From this, it may be concluded that Jordan's method should give reasonable results in most situations where marginal tax factors are not available.

(AUTHOR'S REVIEW OF DISCUSSION)

ROBERT H. JORDAN:

Mr. Schwarz has expanded the value of the paper by examining the relationships that apply during the period when initial expenses are being amortized. In the course of his development, he gave the one-year accumulation formula for ${}_t\bar{S}$ that applies when ${}_t\bar{C}^I + {}_t\bar{C}^M + {}_t\bar{C}^E = 0$. If the actuary arrives at a dividend formula and dividend experience factors such that, from some duration on, this sum is always zero, the accumulation formula can be used to calculate the surplus at any future duration without calculating the asset shares themselves. I am very grateful for Schwarz's fine contribution to the paper.

We are indebted to Mr. Lauer for his development of a more refined formula for asset shares reflecting Phase I federal income tax directly. My development was admittedly rough but has at least served as a means of encouraging Lauer's more comprehensive study of the subject. The values of R^B and R^V shown in Table 1 of Lauer's discussion are especially interesting. If there has been any doubt that a higher valuation interest rate creates a meaningful reduction in the tax rate, this table should dispel it.

A FAST, MORE MEANINGFUL TWENTY-YEAR NET COST FORMULA

PETER L. J. RYALL

SEE PAGE 101 OF THIS VOLUME

JOSEPH M. BELTH:*

It is an honor and a privilege to appear before this Society. I was delighted to receive an invitation to present these comments after I had indicated to Mr. Moorhead my interest in Professor Ryall's work.

I learned of Ryall's work less than three months ago, and I did not see his papers until about two months ago. During this period I have not been able to allocate an amount of time sufficient to do justice to his work. My comments are of a general nature, therefore, and represent only my initial reaction to his work. Moreover, they pertain only to the paper entitled "A Fast, More Meaningful Twenty-Year Net Cost Formula." Since Ryall's method utilizes the sum of the first ten years' dividends and the sum of the first twenty years' dividends, I will refer to it as the "ten-twenty method."

Perhaps my most fundamental reaction to Ryall's work is one of relief. I believe that these are the first full-length papers on the subject of life insurance price measurement to appear in the actuarial literature. I have often been asked why actuaries have not written on this subject, and I speculated on this in the preface of my book.[1] I am glad that I will no longer have to wrestle with that question.

In one important respect, the ten-twenty method differs from all other methods of life insurance price measurement that have been developed thus far. It is the only method that utilizes tables of computed factors that are applied to certain policy data to arrive at a price figure. The traditional net cost method uses no factors at all, unless dividing a twenty-year net cost by 20 to obtain a yearly net cost is to be construed as the application of a factor. The one-thirtieth method uses a factor of 30, but this is a rough approximation rather than a computed factor. The level-price method[2] and the benefits-premiums method[3] involve extensive

* Dr. Belth is Professor of Insurance in the Graduate School of Business at Indiana University.

[1] *The Retail Price Structure in American Life Insurance* (Bloomington, Ind.: Bureau of Business Research, Graduate School of Business, Indiana University, 1966), p. xi.

[2] *Ibid.*, pp. 33–43.

[3] Joseph M. Belth, "The Relationship between Benefits and Premiums in Life Insurance," *Journal of Risk and Insurance*, XXXVI, No. 1 (March, 1969), 19–39.

computations that are performed directly on the policy data, but no factors are developed. In a sense, then, the ten-twenty method lies between those methods that involve no detailed computations and those methods that involve extensive computations.

In his paper Ryall discusses the relationships among the traditional net cost method, the one-thirtieth method, and the ten-twenty method. He uses the ten-twenty method as "control" and shows the "misrankings" produced by the other two methods. I felt that it might be useful to examine the relationships among the methods by using the level-price method as "control."

In connection with a recent paper (not yet published), I assembled twenty years of policy data for the $10,000 participating straight life policies issued in 1968 to standard males aged 35 by fifteen large United States companies. For the purpose of this discussion, I computed prices for these policies under the traditional net cost method, the one-thirtieth method, the ten-twenty method (using factors based on 4 per cent interest and the 1958 C.S.O. Mortality Table), and the level-price method (using 4 per cent interest, the 1958 C.S.O. Mortality Table, and Moorhead's R lapse table). In each case, the premium used in the calculations included the cost of the waiver-of-premium clause, and an adjustment was made to compensate for the fact that two of the fifteen companies classify applicants by age last birthday rather than age nearest birthday. To parallel Ryall's work, I took into account post-mortem dividends and what he refers to as "apportionable premiums."

Kendall's coefficient of rank correlation between the ten-twenty prices and the level prices was 0.71. Similarly, Kendall's coefficient between the one-thirtieth prices and the level prices was 0.73. (Kendall's coefficient for the one-thirtieth prices was slightly higher than the corresponding coefficient for the ten-twenty prices; however, when the ordinary coefficient of correlation was used, the ten-twenty prices showed a slightly higher correlation to the level prices than did the one-thirtieth prices.) Since Kendall's coefficient between the traditional net costs and the level prices was 0.56, these results suggest that both the ten-twenty and one-thirtieth methods are substantial improvements on the traditional net cost method.

The results also suggest that there is very little difference between the ten-twenty and one-thirtieth methods in terms of the relative positions of the companies. Indeed, Kendall's coefficient between the ten-twenty prices and the one-thirtieth prices was 0.98. Admittedly, this analysis dealt only with issue age 35, and the results might be different at higher issue ages, as suggested in Ryall's paper. My initial view, however, is that the ten-twenty method may not be sufficiently superior to the one-thirtieth method to compensate for its additional complexities.

Two other points arose from my analysis. The first is the rather substantial importance of lapse assumptions. Certainly there is room for debate about the suitability of incorporating lapse assumptions in price calculations from the buyer's point of view, as well as debate about what would constitute an appropriate lapse table if it were decided to incorporate lapse assumptions. However, my view is that the lapse factor should be included in a sound price-measurement system.

The second item is the treatment of terminal or settlement dividends payable on surrender. On the basis of earlier studies I have made, there are at least two ways in which surrender dividends may be handled in a sound price-measurement system.[4] These two approaches may be mathematically identical if the calculations are carried to the end of the policy. However, when the calculations are stopped at a point such as the end of the twentieth policy year, I feel that neither approach does full justice to all the companies.

I attempted to test the importance of the lapse assumptions and the treatment of surrender dividends by computing level prices using zero lapse rates and the alternative approach to surrender dividends described in my book. The combined effect of these two changes in the level-price calculations produced rankings virtually identical to those produced by the ten-twenty and one-thirtieth methods. I hope that the Moorhead Committee and the Bittel Special Subcommittee will give careful consideration to these two closely-related areas in their deliberations. Anyone interested in the details of my analysis is welcome to a copy of my worksheets.

One of the main features of the ten-twenty method is that it utilizes data that are generally available in the trade publications, at least with respect to many of the companies included in those publications. This may be a legitimate or even desirable criterion for a price-measurement technique. My suggestion, however, would be to develop a truly sound technique—avoiding approximations wherever possible—and then to decide what data are needed for the calculations. Surely there is no particular magic in the kind of data typically included in the trade publications, and presumably the publishers would be receptive to suggestions on how their material might be made more useful.

Another important characteristic of the ten-twenty method is that, given the necessary policy data and the various computed factors, the price figures may be obtained quickly with a desk calculator. Apparently

[4] *The Retail Price Structure . . .*, pp. 45–55.

the development of such a method was Ryall's objective, as indicated by the word "fast" in the title of the paper. I submit, however, that the wide and increasing availability of computers suggests that attention might better be focused on direct price-measurement techniques—those that involve extensive computations performed directly on the policy data.

The ten-twenty method, like the one-thirtieth method, represents an attempt to approximate certain "level equivalents" that can be subtracted from the annual premium to arrive at a price figure. Along this line, the following comment was made several years ago by Mr. Irwin T. Vanderhoof during an informal discussion of net cost formulas:

> A simple method of including interest in net payment illustrations is to obtain a "level dividend" equivalent in value to actual dividends where such value is based on interest at the rate used in dividend accumulations. Probably some companies would change competitive positions slightly using this method.[5]

In the recently published paper cited in footnote 3, I illustrated the notion of an E-value. It is the excess of what the policyholder pays over what he receives in the form of dividends and benefits, with everything expressed in present-value terms. The E-value is also the present value of the expenses, contingency margins, and profit of the company from the buyer's point of view. Reading Ryall's paper and recollecting Vanderhoof's comment have reminded me that it would be quite feasible to express the E-value in level annual terms rather than in present-value terms. I hope to develop this approach as an addendum to the paper.

Vanderhoof's comment, to the effect that some companies might change positions "slightly," may well have been the understatement of 1962. The fact is that some companies change positions rather markedly when a sound price-measurement technique is substituted for the traditional net cost method. To focus on this point, I have developed a simple ratio that might be described as a "steepness index." The numerator of the ratio is one-twentieth of the simple total of the annual dividends for the first twenty policy years plus one-twentieth of the twentieth-year surrender dividend, if any. The denominator is the level equivalent of the annual dividends for the first twenty policy years plus the level equivalent of any surrender dividends payable in the first twenty policy years.

I calculated such steepness indexes for the fifteen policies mentioned earlier, using 4 per cent interest, the 1958 C.S.O. Mortality Table, and

[5] *TSA*, XIV (1962), D354.

Moorhead's R lapse table in the calculation of the denominators. The ratio varied from approximately 1.2 to approximately 1.6. As might be expected, the companies with relatively high indexes (in this case, those above approximately 1.4) were the ones whose relative positions worsened substantially when shifting from the traditional net cost method to a more sound price-measurement technique. Those with relatively low indexes were the ones that maintained or substantially improved their relative positions under a sound price-measurement technique. Several other formulations of such an index are possible, and I may elaborate on this subject in a future article.

In conclusion, I would like to congratulate Professor Ryall on two extremely provocative papers. Also, I would like to express the sincere hope that these first papers on life insurance price measurement before this Society will not be the last.

JOHN M. BRAGG:

Professor Ryall is to be congratulated for producing this new formula for determining twenty-year net costs. As the title of the paper implies, the new formula is both "fast" and "more meaningful."

It is more meaningful than earlier methods in that it takes direct and accurate account of interest and survivorship. Efforts, such as this, to find an acceptable method for making cost comparisons are commendable. There are other considerations, however, and a reference to them seems in order as a part of the discussion of this fine paper.

First of all, we should note that the attention being devoted these days to what is described as "the cost of life insurance" arises mainly from legislative sources and is motivated by consumer-oriented considerations. We should also observe that other investigations and disclosures have *not* resulted in the ranking of the prices of competitors. The automobile investigation resulted in higher safety standards; the tobacco investigation in warnings about health hazards; the equity product furor in elaborate requirements to prevent sales misrepresentation; and the welfare plan investigation in disclosure requirements which are elaborate but relate only to the particular plan itself.

Net cost methods which have been used, and proposed, give absolute numerical results which seem meaningless in themselves. They seem of value only as a ranking device. But, in my opinion, ranking is not good enough. What is needed is a method to determine a *fair price* for life insurance.

In considering the question of "fairness" for life insurance, the whole product-price-servicing complex must be examined. Briefly, here are some major elements in the matter of "fairness":

a) The buyer must receive the product (and servicing of it) which he thinks he has bought. This is a major point in the life insurance business, because receipt of the benefit and service typically occurs after the sale and, in many cases, long after it.

b) The product (and service) must contain the qualities and safety features which experts know are essential but which unsophisticated buyers might ignore.

c) There are intangible "utility" elements which can cause a buyer to believe that his price is fair. These include his feelings toward the company and agent, the nature of the service rendered to him, and the feelings he has about the benefits he has bought.

The theory of "utility" is something that we, as actuaries, could study more than we do. Our business is based on a belief that a dollar in benefits is worth more than a dollar in premiums—measured in terms of utility to the buyer.

It might be worth noting that state regulatory authorities, over the years, have done much to bring fairness to the product-price-servicing complex of our business. One recent example is the quite widespread prohibition of coupon, founders, and other "gimmick" policies. This type of regulation strikes me as parallel to the new federal safety standards for automobiles.

Fairness in the product-price-service complex is not easy to define, but its existence can probably be inferred if (*a*) new customers are attracted to the product in reasonable numbers over an extended period of time (i.e., a period long enough for the experiences of early buyers to become known to later prospects), (*b*) old customers tend to become repeat buyers, and (*c*) persistency is relatively good.

There is an opposite side to the fairness coin; the price should also be fair to the company and the agent. A price which is fair to all parties is probably in the vicinity of the highest price which is considered fair by the buyer; such a price could be called the "optimum price."

There may be some need for a simple "cost of insurance" formula for use in special situations, such as those involving replacement or "twisting." This, however, is only part of the answer to the question of fairness in the entire product-price-servicing complex.

GEORGE H. DAVIS:

The subject of the cost of life insurance has received attention from a number of different quarters recently, including the United States Congress. Much of this attention has been critical, with the allegation made that it is impossible for a policyholder to learn the true cost of his life insurance coverage. For many years companies and agents have used what we have come to call conventional net cost figures for a policy surrendered after twenty years as a measure of the cost of life insurance that is supposed to be comprehensible and reasonable for the policyholder's information. In view of the deficiencies of this method, which include the complete ignoring of the effect of interest, it has to be admitted that there is some validity in the criticism.

In the light of these developments, Professor Ryall's paper is a very timely contribution to the *Transactions*. It may be well for life insurance companies to concede that the calculation method involved in conventional net cost figures is obsolete and inadequate, and they may have to suffer some embarrassment for not having taken action based on this point of view long ago. A special committee of three of the life insurance trade associations is currently giving intensive study to the subject of life insurance costs and is attempting to develop indices or means of measuring the cost of a life insurance policy which will be reasonably simple and at the same time not subject to the shortcomings of the conventional net cost method. The work Ryall has done in preparing his two papers has already been of significant assistance to this committee, which is attempting to study all proposals for better methods of measuring and expressing life insurance costs.

Ryall's paper is not intended to be an analysis of the entire problem of the determination of meaningful measures of life insurance costs. As its title indicates, its purpose is to develop an improvement on the conventional method of determining net costs. It corrects the serious defect of the ignoring of the effect of interest and also introduces the effect of mortality. It shows how this can be accomplished with reasonable accuracy by approximate methods and also provides for adjustments for various items which may be added to the face amount at death. It seems to accomplish its intended purpose reasonably well.

A thorough study of the whole problem of life insurance cost figures will need to inquire into other details of the conventional method which this paper does not attempt to examine. One of these is the assumption of surrender after twenty years. This is an unrealistic assumption and seems to be justified only if the effect of making it rather than other possible assumptions as to final termination has little effect upon the result. This is probably not true for early ages at issue. Rather than make any assumption as to surrender at a particular duration, it may be more reasonable to put lapse rates into the calculation, although this adds a substantial complication.

Others who have studied the life insurance cost question have attacked it by breaking the policy down into its insurance and investment elements and determining the value of one of these elements. If the insurance element is valued, this is done after the cost of the investment element has been deducted based on an arbitrary rate of interest. If the investment element is valued, the cost of the insurance element is deducted based upon an arbitrary mortality standard, and the interest rate is determined which produces the benefits of the investment element. Several variations of these two approaches have been suggested. Although they may have some merit and may be useful for some situations, these approaches seem to me essentially unrealistic, since a life insurance contract in toto has to be regarded as containing insurance and investment features which are not separable. Furthermore, either method puts all the loading into the element being valued, and this does not reflect a reasonable assumption as to the actual incidence of expense. For a basic method of determining the real cost of a life insurance policy, the approach of treating it as a combination of insurance and investment elements providing benefits in event of both death and surrender seems to be preferable. Ryall's method is one which follows this approach.

JACOB S. LANDIS:

Professor Ryall's ingenious approximation takes us a giant step forward in the all but unending quest for an optimum measure of two elusive magnitudes, the "net cost of insurance" and the "yield" on savings channeled into life insurance. The optimum, of course, would be a combination of maximum precision, minimum complexity, and minimum of "input" data.

The search for such optimum indices is, of course, a legitimate problem in actuarial science. To the extent that such indices compress a premium, a death benefit, a scale of cash-surrender values, and, in the case of participating insurance, a dividend scale into a series of annual costs or a single level cost, or yield, figure, their precision depends on that of the mortality, interest, and, possibly, lapse assumptions used in the calculations. This being understood, the annual or level costs, or the yield, are merely "illustrative" to the extent that, in the case of participating insurance, they incorporate an illustrative dividend scale. Perhaps, following Ryall's usage in his second paper, we should speak not only of an *illustrative* yield but also of an *illustrative* net cost of insurance.

As an actuary of an insurance regulatory agency I am deeply concerned about a developing trend to make "price disclosure" in the field of life insurance a concern of the public authorities and, for this purpose, to adopt a particular "net cost" formula as a "true" measure of this cost, or price. Where an illustrated dividend scale enters into the calculation, I feel that it would be wrong for a regulatory authority to put a stamp of official approval on *any* formula or method calculation, no matter how precise. By merging a definite premium, a definite death benefit, and a definite scale of cash-surrender values with a merely illustrative dividend scale, the "net cost" actually discloses less, not more, than a separate statement of each of these items does.

Consider, if you please, a very young and very aggressive stock company issuing participating insurance. It is a fair assumption that its dividend scale would be dictated, to a large extent, by competitive considerations. If a particular net cost formula is officially adopted, it would not be too difficult to devise a dividend scale so as to produce a favorable net cost if calculated by that formula. What assurance does the prospective policyholder, or the regulatory authority, or, for that matter, the company itself have that the scale can be maintained over the next twenty years?

Are we not hypnotized by the current slogan of "truth in lending" into pursuing the unattainable goal of "truth in (participating) life insurance"? It is a simple matter to calculate the true cost of credit. All the ingredients are there, well determined and absolutely fixed. The prospective borrower may be confused, but any arithmetician can put him straight. Life insurance is an entirely different matter. It is a matter of numerous contingencies, and, in the case of participating insurance, also of future management decisions. So much so that, I submit, it would be a disservice, not a service, to the life insurance buying public to sanction any annual or level net cost figure as being "proper price disclosure."

The opinions expressed above are my own and do not reflect or prejudge the position of the New York State Insurance Department in this matter.

MEL STEIN:

Professor Ryall is to be congratulated for writing an imaginative and interesting paper.

While the methods presented in the paper are sound from an actuarial viewpoint, I believe that they will, unfortunately, see far too little use by agents. This is a result of the buying attitude of the public. When the average person buys insurance and compares ten- or twenty-year "net costs" or variations thereof, he is interested in what will happen *if he lives*. If a person is in reasonably good health, he believes that he will live. The life insurance is *just in case*. Thus he thinks of the insurance in the following two-fold way: (1) the "protection" element, which will provide for his family *in case* he happens to die, and (2) the "investment" element, which is what his "net cost" will be after he survives the specified period (as he fully intends, and expects, to). This is the element on which his comparisons are based.

When comparing the "investment" element of the policies of two or more companies, the insured wants *exact* figures, not approximate factors whose derivation he does not understand. The less educated buyers of insurance will swallow comparisons based on total premiums less total dividends less the cash value at the end of the comparison period. The more educated buyers will replace total dividends by accumulated dividends. The smartest buyers will also replace total premiums by premiums accumulated with interest.

Another item that buyers are often interested in is each year's premium less dividends less increase in cash value.

The above discussion on the attitude of buyers of life insurance should not, in any way, be considered a criticism of this original, resourceful, and well-done paper.

LOUIS M. WEISZ:

Professor Ryall is to be congratulated for presenting a method for determining net costs which is both accurate and relatively easy to use.

As another measurement of net cost, I have considered the expense and profit which the policyholder has paid for. This differs from Ryall's method in that it deducts out the cost of mortality. The n-year level cost is just the premium less the level dividend less the level cost of funding the nth-year cash value less the level cost of mortality. For policies where the premium varies by duration, a level premium would be determined. Each element in the cost would be discounted for mortality and interest, with the mortality being based on a modern select table and the interest rate being that which could be earned by an alternative investment having the same degree of safety. Thus the level cost of mortality would just be a level term premium. The method is particularly applicable for cases where the premium or death benefit varies by duration. Net costs could easily be calculated by means of a computer.

The method is useful as a guide in pricing the product. It shows how much money the company has held back over the n-year period, that is, how much has gone other than to provide benefits for the policyholders. It also gives the relative cost by plan. In making comparisons between two companies, an assumption would have to be made as to the mortality applicable. An obvious choice would be the 1955–60 Basic Select and Ultimate Table, though this might be adjusted for the company's relative level of mortality.

I would use Ryall's method for a policyholder who intends to surrender his policy at the end of the period for which the net cost is made. Here it is assumed that the policyholder survives the period. He has paid premiums and in return has received dividends and the cash value at the end of the period. The mortality cost which he has paid has not been used for his benefit but has gone to pay other claims.

(AUTHOR'S REVIEW OF DISCUSSION)

PETER L. J. RYALL:

I wish to thank Messrs. Belth, Bragg, Davis, Landis, Stein, and Weisz for their discussions of my paper. These discussions are especially valuable in that they represent a broad spectrum of opinion. I comment below only on those views with which I disagree.

Professor Belth advocates the incorporation of lapse assumptions in policyholder cost calculations. An alternative approach is to calculate, in addition to the twenty-year cost, the surrendered net cost (ignoring interest and mortality) for the three-year period following issue. Since cash values and dividends beyond the third year generally increase quite smoothly, it is to be expected that, in comparisons between two companies' policies, a marked difference in three-year costs will be associated with similar differences over somewhat longer periods.

If there is a marked difference in costs at early durations in favor of one company, and an appreciable difference in twenty-year costs in favor of the other company, the prospect can make his choice in the light of his appraisal of the possibility that he might terminate his contract prematurely. Such a choice—between alternatives expressed in financial terms—is much more intelligible to the prospect than the selection of (to the prospect) elusive termination rates from the wide range in use.

As evidence that the ten-twenty method is not appreciably superior to the one-thirtieth method, Belth cites a rank coefficient for issue age 35 based on policies of fifteen companies vaguely described as "large." This is no substitute for the analysis that I make with respect to policies of twenty-four companies, including *all* the largest according to a clearly defined criterion. I do not "suggest" that the results "might" be different at higher ages. I give statistical evidence, namely, that the Kendall coefficients of rank correlation between costs obtained by the two methods (for $10,000 participating whole life policies) for issue ages 25, 35, 45, and 55 are 0.957, 0.946, 0.855, and 0.797, respectively.

Mr. Bragg states that "other investigations and disclosures have *not* resulted in the ranking of the prices of competitors" (emphasis in original). In view of past legislative action with regard to "truth in lending" and "truth in packaging," this is an extraordinary assertion. Moreover, the concern is a continuing one, as is exemplified by the recent filing of bills in the Senate and House that would require the price per ounce, pint, quart, pound, or other unit to be printed on labels of packages containing foods, drugs, cosmetics, and other household commodities.

It is cold comfort for us policyholders to be told that, in the case of a product as essential as insurance against catastrophic financial loss, the price is "fair" as long as it is less than its utility to us. Yet, without price competition, this is just what is implied by Bragg's definition of fairness (see his third from the last paragraph). Furthermore, as if to add insult to injury, he suggests that "a price which is fair to all parties is probably in the vicinity of the highest price which is considered fair by the buyer."

To argue that the vendor in a business transaction can, unrestrained by competition, unilaterally determine a "fair" price is cant. The greater the sophistication of the techniques used in determining prices to extract the last dollar of profit for the agent-company coalition, the greater is the prospect's need to protect *his* interests by obtaining quotations from different companies.