ABSTRACT OF THE DISCUSSION OF PAPERS READ AT THE PREVIOUS MEETING

THE MULTI-SPLIT EXPERIENCE RATING PLAN IN NEW YORK

ROGER A. JOHNSON, JR.

VOLUME XXVIII, PAGE 15

WRITTEN DISCUSSION

MR. CHARLES M. GRAHAM:

Mr. Johnson's paper on the Multi-Split Experience Rating Plan in New York is particularly well timed. The Plan has been in effect less than a year but certain results of its application have provoked considerable criticism. Some of these have been covered by Mr. Johnson and some have not. I shall comment not only on the points raised by Mr. Johnson, but also on certain other criticisms of the Plan, and will endeavor to offer suggestions for the amelioration of the conditions giving rise to these criticisms.

Mr. Johnson has pointed out the fact that expected loss rates have had to be recalculated once each six months for all policy years which might enter into the rating period. He has mentioned three proposals, all of which aim at eliminating the semi-annual recalculation of the expected loss rates. It is interesting to note that the Actuarial Committee of the Compensation Insurance Rating Board has recognized this situation by adopting the second proposal outlined by Mr. Johnson, i.e., a single expected loss rate for all policy years to be effective during the fiscal year, which means in effect, a single expected loss rate to apply to all ratings effective from July 1st of a given year, to June 30th of the succeeding year. It was pointed out that this procedure is practicable only when no substantial law amendments are encountered. At the present time, it does not appear likely that substantial law amendments will be encountered in New York State in the immediate future.

I was particularly glad to note that Mr. Johnson stressed the point that for risks below the Q point, the risk modification is not determined solely on a comparison of the adjusted primary losses with the expected primary losses but rather on a comparison of adjusted primary losses plus expected excess losses with total expected losses. On this point there has been a misunderstanding difficult to dispel due to the fact that the Multi-Split Rating formula buries the credibility of such losses, whereas the credibility stood out clearly in the rating computation under the old plan.

Mr. Johnson has made an excellent point in advocating the calculation of W and B values for each individual risk. The case cited by him in which a reaudit produces higher payrolls on a risk is not merely a possibility as the writer has encountered such a case in actual practice. Therefore, from the standpoint of practical as well as theoretical considerations, Mr. Johnson's recommendation that the values of W and B be individually computed for each risk to which W values apply, should be given the most careful consideration.

Mr. Johnson's second suggestion for lowering the Q point is, in my opinion, a very good one. Even if Q is reduced from 12,000 to 8,500, it will still be possible to encounter a risk which would have primary credibility greater than unity. This, however, would occur only on risks having extremely low D ratios and may possibly not occur even in these cases if certain adjustments which will be hereinafter discussed, are made in the present method of computing D ratios. It is expected that an adjustment of the Q point will be made in connection with the 1942 revision of New York rates.

Mr. Johnson's third suggestion, which is to increase the value of g from .4 to .53, is also a good one. I anticipate an increase in this value in connection with the rating factors effective 7/1/42, not, however, to the full extent recommended by Mr. Johnson. It would seem that Mr. Johnson's recommendation is fully in order but it may be thought advisable to make the adjustment gradually rather than all at once.

One objection to the present rating values used in conjunction with the Multi-Split rating plan which I consider very serious has not been mentioned by Mr. Johnson. This objection concerns the computation of the D ratios used in determining the primary expected losses for rating purposes. At the present time these D ratios are determined by using a state-wide distribution of claims according to size of loss separately for serious, non-serious, and medical other than that assigned to individual claims. From these state-wide distributions, factors are calculated measuring the relationship of the discounted costs (indemnity plus medical) of serious cases to the total serious indemnity cost undiscounted; similar calculations being made for non-serious and medical unassigned to individual claims. Although the partial D ratios so determined are applied to the separate partial pure premiums by classification, it must be obvious that the original distribution of cases by loss size groups should vary widely by classification according to the severity hazard of the classification involved. While it is recognized that it would be truly a formidable task to compile such distributions by classification and further, that having such distributions by classification, few, if any, classifications would provide sufficient exposure for the determination of partial D ratios, surely such distributions could, in time, be prepared for related groups of classifications or perhaps by industry schedules. Statistics now available compiled from actual ratings covering almost the entire first year of operation of the Multi-Split Plan, indicate that actual primary losses for the Manufacturing and All Other groups, exceed the expected primary losses by from 7 to 9%, while the expected primary losses for the Contracting industry group exceed the actual primary losses by about 9%. These figures support the views expressed above and indicate the necessity of a more refined calculation of the D ratios. It will probably be necessary to apply average correction factors by industry group in connection with the rate revision effective July 1, 1942, due to the lack of time available in which to make the extensive tabulations necessary for a more accurate computation of the D ratios. The application of such correction factors, however, will be a step in the right direction and will, undoubtedly, serve to soften the penalty which the Muti-Split Plan has exerted on many risks having low severity and high frequency rates, at the same time equalizing the effect of the Plan by reducing D ratios applied of risks having high severity but low frequency rates.

In conclusion, it is my belief that the Multi-Split Plan represents a distinct improvement over the plan which it superseded and that the criticisms set forth by Mr. Johnson, and also the criticism of the D ratios set forth above, are minor obstacles which can be remedied without undue difficulty as more experience under the Plan is accumulated.

DISCUSSION OF THE RATEMAKING PROCEDURE IN WORKMEN'S COMPENSATION INSURANCE—A METHOD OF TESTING CLASSIFICATION RELATIVITIES

STEFAN PETERS

VOLUME XXVIII, PAGE 105

WRITTEN DISCUSSION

MR. R. M. MARSHALL:

At a time when the compensation ratemaking procedure is the subject of considerable study and investigation, Mr. Peters' paper, "A Method of Testing Classification Relativities" is a welcome addition to the literature of the Society.

The reader cannot fail to be impressed by the immense number of calculations made by Mr. Peters in applying the test. With regard to this, a number of questions occur to me which may be worth while to set down.

It is noted that Mr. Peters has illustrated his method of testing classification relativity by a comparison of the selected pure premiums underlying the New York compensation rate revision effective July 1, 1938, with the corresponding pure premiums indicated by the New York policy year 1938 experience when it became available at a date considerably later. The first question relates to this time lag between the effective date of the rates and the time when these rates can be tested by this method.

The basis of the compensation ratemaking procedure is that the past will repeat itself. In our selection of pure premiums we endeavor to select those which correspond the closest to the relativity shown by past experience. Mr. Peters' hypothesis is that there is an underlying set of "true" pure premiums from which the indicated pure premiums of a policy year deviate in a normal frequency distribution, the deviations being due entirely to chance. Our ratemaking procedure assumes that the relativity of these "true" pure premiums will not vary greatly over a short length of time, so that the relativity during the period when the proposed rates are to apply will be the same as for the period just completed. It might be expected that the pure premium indications of the five latest policy years combined, would also deviate from the "true" pure premiums in a normal frequency distribution, with smaller deviations than shown by a single policy year. Furthermore, it is noted that the test is concerned only with classification relativity, as Mr. Peters has introduced factors in his calculations designed to eliminate any differences in rate level.

All this is by way of leading up to the question, "Could not the test have been based entirely on the data shown in the exhibits of classification experience which are regularly prepared for a rate revision?" For example the exhibits prepared for the July 1, 1941 revision of New York compensation rates show the pure premium indications of the five latest policy years, formula pure premiums derived from National experience, and pure premiums underlying the present rates. It would be a relatively simple matter to calculate formula pure premiums by rating against the underlying pure premiums. Furthermore, the data are all exhibited on the proposed rate level, so the corrections for differences in rate level which Mr. Peters makes would not be necessary.

The use of the actual indicated pure premiums for a five year period would also reduce the number of classifications with no serious incurred losses. It is noted that wherever the amount of the incurred loss is zero, Mr. Peters' calculations give a value of minus infinity, which he has had to exclude from his results. A review of New York experience indicates that we would expect a serious case for approximately each $35,000 of premium, or roughly a little more frequently than one every other year for classes with 50% credibility, and less often for classes with lower credibility. It is therefore not surprising that, when only one year of experience is considered, approximately 40% of the classifications had a serious indicated pure premium of zero.

Mr. Peters' difficulties with minus infinity arise because he has adopted the expression "$\log \frac{\text{actual losses}}{\text{expected losses}}$" to measure the relationship between selected pure premiums and actual pure premiums. In searching for a simpler relationship, the suggestion arises, "Could the relationship be expressed by the algebraic difference of selected pure premiums minus indicated pure premiums?" It would seem that these values should also have a normal distribution with zero as a mean.

The goal of all our ratemaking calculations is to arrive at a single manual rate for each classification. Therefore, as a practical question, "Could the test be applied to the total pure premium instead of testing serious, non-serious and medical pure premiums separately?" If the separate tests should indicate one method of selection gave best results for serious pure premiums, and a different method of selection gave best results for non-serious or medical pure premiums, the proper course for a best over-all result would still be in doubt.

The premise that the best fitting pure premiums selections are those which show the smallest deviations from the pure premiums indicated by the actual experience suggests the final question, "Why not adopt the indicated pure premiums as the selected pure premiums?" We would probably not care to adopt the pure premium indications of a single policy year, or even of five years for classes with low credibility. However, if we could adopt a variable experience period, for example two or three years for classes with 100% credibility and a longer period as the credibility decreases, satisfactory results might be obtained. It has been pointed out in the discussion of the ratemaking procedure by the Actuarial Committee of the National Council, that to determine formula pure premiums by weighting the state indications against the present underlying pure premium is really equivalent to extending the experience period of the particular classification beyond the standard period which has been selected as the basis of the current "indicated pure premiums." In this connection the attached exhibit which was prepared by Mr. H. T. Barber for the information of the Actuarial Committee of the Council may be of interest. (Grateful acknowledgment of Mr. Barber's permission to reproduce this table is made herewith). This table shows, for various classification credibilities, the weight accorded the pure premium indications of the various policy years represented in the formula pure premium, the formula pure premiums being obtained by weighting the indications of the two latest years against the formula pure premium from the previous revision (assumed to be the present underlying). This table assumes that any distortion due to use of National experience has been eliminated. (Page 580).

MR. A. L. BAILEY:

Mr. Peters' suggestion that casualty actuaries would do well to put to a rigorous test some of their obviously sound methods and naturally following assumptions is one of the important contributions of his paper. To newcomers to the casualty insurance field—both Mr. Peters and myself being

PROPORTION OF STATE POLICY YEAR EXPERIENCE IN PURE PREMIUMS DERIVED BY SUGGESTED FORMULA:
Formula Pure Premium = Z (Indications of Two Latest Policy Years) + $(1-Z)$ (Formula of Previous Revision)

Class Credibility:	1.00	.90	.80	.70	.60	.50	.40	.30	.20	.10	.05
Policy Year											
1 (Latest)	.5000	.4500	.4000	.3500	.3000	.2500	.2000	.1500	.1000	.0500	.0250
2	.5000	.4950	.4800	.4550	.4200	.3750	.3200	.2550	.1800	.0950	.0488
3	*	.0495	.0960	.1365	.1680	.1875	.1920	.1785	.1440	.0855	.0463
4	*	.0050	.0192	.0410	.0672	.0938	.1152	.1250	.1152	.0770	.0440
5	*	.0005	.0039	.0123	.0269	.0468	.0691	.0874	.0922	.0692	.0418
6	*	*	.0008	.0037	.0108	.0235	.0415	.0613	.0737	.0624	.0397
7	*	*	.0001	.0011	.0043	.0117	.0249	.0428	.0590	.0561	.0377
8	*	*	*	.0003	.0017	.0059	.0149	.0300	.0472	.0504	.0358
9	*	*	*	.0001	.0006	.0029	.0090	.0210	.0377	.0455	.0340
10	*	*	*	*	.0003	.0015	.0054	.0147	.0302	.0409	.0323
11	*	*	*	*	.0002	.0007	.0033	.0103	.0242	.0368	.0307
12	*	*	*	*	*	.0004	.0019	.0072	.0193	.0331	.0292
13	*	*	*	*	*	.0002	.0011	.0051	.0155	.0298	.0277
14	*	*	*	*	*	.0001	.0007	.0035	.0123	.0268	.0263
15	*	*	*	*	*	*	.0004	.0025	.0099	.0242	.0250
16	*	*	*	*	*	*	.0003	.0017	.0080	.0217	.0238
17	*	*	*	*	*	*	.0002	.0012	.0064	.0196	.0226
18	*	*	*	*	*	*	.0001	.0009	.0050	.0176	.0215
19	*	*	*	*	*	*	*	.0006	.0041	.0158	.0204
20	*	*	*	*	*	*	*	.0004	.0032	.0143	.0194
Total (20 yrs.)	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	.9991	.9871	.8717	.6320

NOTE: Policy Year (n) Weight $= \dfrac{(1-Z)^{n-2}-(1-Z)^{n}}{Z}$ except that $(1=Z)^{-1}$ is taken equal to 1.

of that order—it is rather disturbing to find the complete disregard of twentieth century developments in statistical methods of testing such assumptions and methods.

In practically all other fields dealing with numerical data—Mr. Peters mentions research workers, but the same is true of cost accountants, time study men, economists, sales analysts, and many others—the standard procedure is to analyze the data, make an assumption, and then test the assumption before applying it. Many statistical tools have been developed and tables prepared in recent decades to permit a production line processing of statistical data in making both the original analyses and the later tests of hypotheses. Mr. Peters' paper is a first step in the retooling of the casualty actuarial industry.

The analysis of variance, made use of by Mr. Peters, is one of the most outstanding and widely useful statistical tools which have become available since 1900. The general usefulness of the method and its simplicity of application have also unfortunately had the result of making it one of the most widely misused procedures. In each case to which it is applied a very careful review is necessary of the assumptions made by its application. This requirement is not brought out sufficiently clearly in most of the texts presenting the method.

The analysis of variance results in one or more pairs of variances, which are then tested to determine the probability of obtaining two variances differing from each other by as much as the observed difference under the assumption that they represent two independent estimates of the variance of a single homogeneous distribution (homogeneous in so far as the sampling process is concerned) If this probability is very small (Mr. Peters used less than .02), then the assumption that they are two estimates of the same value is ruled out as improbable and they are interpreted as being estimates of the variances of two different distributions.

The fundamental criticism which I shall make of Mr. Peters' paper is based on the belief that the variable he used:

$$x = \log_{10} \frac{\text{actual losses}}{\text{expected losses}}$$

is not one from which an estimate can be made of the variance of any *homogeneous* distribution. This is in fact admitted by Mr. Peters in his statement on page 111:

> "Since, however, that part of the deviation of actual losses from expected losses which is caused by the chance fluctuation of actual losses will obviously be distributed with larger dispersion for classifications with small exposure than for classifications with a larger exposure . . ."

It is, however, not only the differences in exposure which will cause differences in the resulting dispersion of x. The differences in accident frequencies expected for serious, non-serious, and medical losses will produce a considerable difference in the dispersions of the corresponding values of x. The same result will arise from the difference between the dispersions of the amounts of losses for individual claims for serious, non-serious, and medical losses.

Thus, in comparing the variances of the x's for serious, non-serious, and medical pure premiums, the necessary condition that they are three estimates of the same homogeneous distribution does not exist; nor is this condition met in comparing the variances of the x's for 50%-and-over-credibilities with the variances for under-50%-credibility. Actually the variances are estimates of quite widely different distributions, each of which is heterogeneous because of differences in the exposures.

The same lack of the necessary conditions is found in the tests made from Table B for the significance of $\overline{x_1} - \overline{x_2}$ and of Δ. Here the implicit assumption has been made that the ratemaking procedure is equally accurate for all sizes of classifications having less than 50% credibility. We might wish it were, but we all very much doubt it. It may be impossible to make an accurate adjustment for such differences in accuracy; but they should at least be recognized as existing.

Because of the failure to meet these conditions necessary for the application of the analysis of variance, the writer believes that the analyses presented and the conclusions drawn by Mr. Peters from these analyses are without foundation in fact. Not that they are necessarily wrong; but only that they have not been demonstrated to be true or even probable.

If we wanted to determine the variation in the salaries of our employees, some paid by the year, some by the month, and some by the week, we would undoubtedly reduce them all to a single standard basis before calculating the variance. It is suggested that such a procedure be applied to the material at hand before proceeding with the analysis of variance.

As I will show in a paper to be presented at an early date, the standard deviation of the ratio of actual to expected losses is approximately:

$$\frac{\sigma A}{E} = \sqrt{\frac{V_{2:L}}{C \cdot V^2_{1:L}}} = \sqrt{\frac{M \cdot V_{2:L}}{E \cdot V^2_{1:L}}}$$

where $V_{1:L}$ *and* $V_{2:L}$ are the first and second moments about the origin of the distribution of the losses by size of loss per accident, C is the expected number of accidents, A and E are the actual and expected losses respectively, and M is the average loss per accident. In applying this the greatest accuracy would be obtained by using the best available estimate of M for each classification. The value of $\frac{V_{2:L}}{V^2_{1:L}}$ depends on the form of the distribution of the amounts of individual losses and has been found to differ only slightly between classifications of groups of similar classifications.

Somewhat less accuracy would be obtained by using the average value of M, which is $V_{1:L}$ for all classifications within each of the three groups: serious, non-serious and medical. Such a procedure would, at least, eliminate the heterogeneity caused by the differences in exposures. Similarly, the use of the proper values of $V_{2:L}/V^2_{1:L}$ for each of the three types of losses would correct for the differences in the forms of these distributions of losses.

If we subtract the average value of the ratio of actual to expected losses, unity, from each such ratio and then divide by the standard deviation of such ratios, we shall have a function with a mean of 0 and a variance due to chance fluctuation of unity irrespective of the amount of exposure, the expected frequency, or the variation in individual losses per accident. This would also assume that the variation caused by the inaccuracy of the manual rates was proportional to the variation expected from chance alone—a fair first approximation to the actual condition. This procedure would give:

$$y = \frac{\frac{A}{E} - 1}{\sqrt{\frac{MV_{2:L}}{E \cdot V^2_{1:L}}}} = \frac{A-E}{\sqrt{E}}\sqrt{\frac{V^2_{1:L}}{MV_{2:L}}} \text{ or } \frac{A-E}{\sqrt{E}}\sqrt{\frac{V_{2:L}}{V_{1:L}}}$$

as a variable satisfactory for the analysis of its variance.

For actual losses equal to zero, this latter value would become:

$$y_{(A=0)} = -\sqrt{E} \cdot \sqrt{\frac{V_{1:L}}{V_{2:L}}}$$

and the difficulties encountered by Mr. Peters in handling the $-\infty$ values of x would not be encountered.

The separation of classifications into four groups as to credibility, such as:

a. credibility of 0%
b. credibility of 10%, 15%, or 25%
c. credibility of 50% or 75%
d. credibility of 100%

might be considered. The 0 and 100% credibility groups would seem to be of particular interest.

It is hoped that Mr. Peters will continue his efforts to find out just what the existing ratemaking procedures produce in the way of results. Knowing Mr. Peters as I do, I know that he will take this discussion as it is intended: not as criticism for its own sake, but as a possible second approximation to the end we are all seeking—the facts.

AUTHOR'S REVIEW OF DISCUSSIONS

MR. S. PETERS:

In the introduction to my paper I emphasized that the new method of testing classification relativities proposed therein was undoubtedly subject to faults and susceptible to improvement and I asked for criticisms and suggestions. The discussions presented by Messrs. Bailey and Marshall show that this prediction was correct and that the method proposed can be substantially improved. I am very grateful for the criticisms offered and hope that they will help to develop a workable and conclusive method of testing classification relativities. The criticisms and suggestions fall into two groups; Mr. Marshall is chiefly concerned with the object and application of the method, while Mr. Bailey deals mainly with its technical aspects.

The purpose of the method developed in my paper is to test which of two different sets of pure premiums is more accurate. This is done by ascertaining which of them fits better the actual experience of the period during which these pure premiums are to be applied. This is the reason why, in the illustrations, I compared the selected pure premiums of the July 1, 1938 rate revision with the experience for policy year 1938. If the comparison were made, instead, with the indicated pure premiums for the five year period used in computing the selected pure premiums these pure premiums would not have been connected with the actual experience of the period to which they are to be applied and, besides, it is evident that the closest fit would be furnished by the set of selected pure premiums which is identical with the indicated pure premiums, a result which obviously has not much meaning.

Mr. Marshall asked whether it is not possible to devise a test of the pure premiums which applies to the total pure premiums rather than to the pure premium parts. While it may very well be debatable whether the division of the pure premium into serious, non-serious and medical portions is the most appropriate division, all experience presently available is based on these three parts and it therefore seems advisable to use this subdivision of the pure premium. It would not be possible to apply the test method to the total pure premium because the deviations of the three pure premium parts are distributed with greatly different dispersion and it is therefore impossible to apply the analysis of variance without modification to a distribution of the deviations of total expected losses from total actual losses.

Mr. Marshall's proposal to base selected pure premiums solely on actual state experience and to extend the length of the period of this experience in accordance with the credibility of the classification deserves attention, in my opinion. I intend, in the second part of my paper, to develop a set of selected pure premiums which will be based on a procedure similar to that suggested by Mr. Marshall.

Mr. Bailey's technical criticisms deal mainly with two points of the proposed test method: (1) the occurrence of values $-\infty$ and the consequent necessity of excluding these values from the computation of means and variances and (2) the heterogeneity of the distributions studied. As for the occurrence of the values $-\infty$, I agree that the method has to be revised in order to avoid this difficulty and I believe that this can be done by adding the same constant to both actual and expected losses, which constant can conveniently be chosen as a quantity proportional to the payroll exposure of the classification. While the occurrence of $-\infty$ will thus be eliminated, the problem will not be entirely solved inasmuch as the distributions of actual losses have a discontinuity where actual losses are zero and no modification of the variable used can avoid that the approximation of the actual distribution by a continuous distribution involves a certain amount of error.

There is no such thing as a homogeneous distribution of data gathered from actual experience. Homogeneity is a matter of degree and depends on where one judges it appropriate to draw a line, that is, which influences causing heterogeneity one wants to consider as negligible. For instance, even the distribution of the results obtained by throwing a die is not homogeneous in a strict sense since the effect of wear if it does not affect all faces of the die in equal degree, may change the probabilities in the later throws and, thus, may cause a slight heterogeneity of the distribution. It is conceded that the sampling distribution of the losses of a classification is strongly influenced by the size of its exposure. It had been my intention to use in any actual application of the test method relatively small exposure groups in order to avoid this influence. For the merely illustrative application of the test method given in the paper itself, I had believed that the subdivision of classifications into two groups, one with credibility of 50% and over and one with credibility under 50%, was sufficient, but I have been convinced by Mr. Bailey that as far as many of the qualitative and quantitative statements on the properties of the distributions of deviations of actual from expected losses are concerned, this grouping is too crude and is likely to distort the results. The exposure element is, in any case, of great importance only for the part of the variable called x_a in my paper, that is, the portion of the variable which is due solely to the chance fluctuations of the experience. Its influence on the part called x_n, that is, the variable due to the method of selecting pure premiums, is certainly much smaller and, besides, difficult to measure since it is the theory underlying the selection of formula pure premiums that the inclusion of a portion of national pure premiums will correct the possible unreliability of the indicated pure premium which is due to the small volume of experience in the state for which the rates are being made. I therefore believe that the omission of any correction for exposure does not invalidate the statements made in the latter part of my paper relating to the comparison of two sets of selected pure premiums since this comparison is made only for the variable x_n. However, as the portion x_a of the observed variable x constitutes merely an undesirable ballast as far as the test method is concerned, I agree with Mr. Bailey that, if possible, it would be good to reduce this ballast to the same size for all classifications irrespective of their volume of exposure in order to make the method more sensitive.

I do not believe, however, that it is suitable to use for this purpose the variable mentioned in his discussion of my paper. The variable which I used was chosen for two reasons. First, it had a symmetric distribution which was sufficiently similar to a normal distribution, although more peaked, to permit the assumption that means and variances of large samples are distributed approximately like means and variances of large samples with a normal parent distribution. The symmetry has besides the advantage that one can use the variance of the distributions as a characteristic statistic. The second reason was that by choosing the variable proposed in my paper, the observed variable x can be split additively into a portion due to sampling variations and a portion due to the method of selecting pure premiums. As a consequence the variance of the distribution could be equally split into the corresponding two portions and this in turn permits judging from the relative size of the observed variances σ^2 the relative size of the variances σ_n^2 which are due to the method of selecting pure premiums. In Mr. Bailey's variable the influence of sampling fluctuations and of the method of selecting pure premiums are mixed up in a manner in which it is not possible to segregate the one from the other and, particularly, it is not possible to judge from the size of the observed variances σ^2 for two sets of selected pure premiums which σ_n^2 is the greater.

I believe that it is possible to modify the variable use in my paper so as to avoid the occurrence of values $-\infty$ as well as to eliminate to a large extent the influence of the size of exposure of the classification involved without giving up the advantages of the original variable. I hope to present this modified and improved test method in a second part of my paper.

ON GRADUATING EXCESS PURE PREMIUM RATIOS

PAUL DORWEILER

VOLUME XXVIII, PAGE 132

WRITTEN DISCUSSION

MR. SEYMOUR E. SMITH:

Mr. Dorweiler's paper, explaining the method adopted by the Actuarial Committee of the New York Compensation Insurance Rating Board for graduating excess pure premium ratios by size of risk, serves two important functions. The first is to give a thorough exposition of a practical and sound method for smoothing a complicated tabulation of raw statistics in precise conformity with the pattern of behaviorism of the underlying data. This exposition should be very valuable both to students and members of our Society. It is this writer's opinion that those text-books of his acquaintance on the subject of statistics leave the student quite unprepared to handle the smoothing and gradation of much of the statistical data encountered in casualty insurance. It is true that these texts provide valuable material on such points as arithmetic and geometric means, index numbers, the method of least squares, etc., but the main emphasis is placed on the "normal" frequency distribution. This "normal" distribution is used as the base, on which most of the mathematics of statistics are built, and after the student has studied several chapters of the text he runs into the statement that if the underlying data does not conform fairly closely to this "normal" distribution the formulae and methods which he has so laboriously learned are not applicable. At this point the text-book leaves him stranded. Thus Mr. Dorweiler's paper is a welcome addition to the information available to students on practical methods of smoothing statistics to their underlying pattern.

At the time that this data and its graduation was being studied by the Actuarial Committee, two alternative methods received consideration. The first method consisted of the development of an excess pure premium formula of the form

$$E(y) = 10^{-ay-by^2}, \text{ where}$$

$E(y) =$ excess pure premium ratio
$y =$ ratio of actual to expected losses
$a =$ a constant
$b =$ a linear coefficient expressed as a function of the premium size.

This method produced quite satisfactory results, its main advantage being that values could be determined directly from the formula without the use of charts, and that values could be obtained for any intermediate premium size.

The second method was by the graduation of the excess pure premiums by loss ratio group by the use of a second degree curve of the form $y = a + bx + cx^2$. The curves so obtained were modified by the use of minimum limitations, and the values plotted on a chart. From this chart the values were determined for each risk size and plotted on a final chart similar to Chart IV in Mr. Dorweiler's paper.

The second important function which this paper has performed, is in the development of a more intimate knowledge of the behavior of excess pure premium ratios. Five years ago this would have been of only academic interest, but the development and growth of retrospective rating over the last few years has made this subject one of prime importance. The provisions in the basic premium for losses over the maximum and savings on minimum premium risks comprise the entire insurance element in this widely used form of rating. As a result the accuracy of the excess pure premium ratios for these risks is as fully important as the standard premium rate.

The general trend in casualty insurance rating is toward closer conformity with the actual hazards and experience of the individual risk. This is as it should be, particularly so in Workmen's Compensation insurance, where the self-interests of the assured, his workmen, the insurance carrier and society as a whole all lie in the same direction—accident prevention and the speedy return to productive usefulness of injured workers. Rating methods fostering industrial emphasis on accident prevention must provide material incentive to this end through the individual risk rate. To accomplish this, in addition to the base or average rate, there must be a scientific knowledge of the individual risk's divergence from the average. Thus the importance of a sound understanding of excess pure premium ratios, which, in this writer's opinion, will assume an increasingly significant role in future casualty rating methods.

MR. STEFAN PETERS:

Mr. Dorweiler's method of graduating excess pure premium ratios constitutes the latest and, so far, the best achievement in this field. It furnishes excess pure premium ratios which, except perhaps for high loss ratios and also for large premium sizes, are most likely very close to the theoretical values.

The reviewer agrees with the opinion expressed by the author at the end of his paper that questions such as whether different sets of excess pure premium ratios should apply for risks whose hazards are substantially different or whether or not the experience before its graduation should be keyed to the permissible loss ratio for each premium size group are of relatively much greater importance than the refinements of the graduation method itself. Yet, the excess pure premium ratios are so closely linked with the distributions of risks of a given premium size by size of loss ratio and, ultimately, with the basic concepts of accident frequency and severity that it is desirable that these relationships be reflected in the graduation method or be used to test its accuracy. This possible approach to the problem, which will be illustrated below, has the added advantage of being free of the main theoretical imperfection inherent in Mr. Dorweiler's method.

The author points out in the beginning of his paper that the excess pure premium ratios y, viewed as a function of the loss ratio r and the premium size x, can be represented as a surface in a space of three dimensions and he discusses certain geometrical properties of this surface. The actual experience furnishes us with a number of isolated points which can be arranged in groups which are located in planes parallel to the r-y-plane (ungraduated excess pure premium ratios for risks of a given premium size) or in groups which are located in planes parallel to the x-y-plane (ungraduated excess pure premium ratios for a given loss ratio). The latter points were, in reality, not the direct result of the underlying experience, but obtained by linear interpolation between neighboring loss ratios. The graduation process consists in fitting to these isolated points a smooth surface with the geometrical properties mentioned by the author. Instead of attempting a graduation of the surface as a whole by a mathematical procedure, Mr. Dorweiler has limited himself to graduating separately in this manner each group of points located in parallels to the x-y plane which correspond to a number of selected loss ratios. He thus obtained a set of parallel smoothed "ribs" of his surface and removed the few remaining unevenesses between the "ribs" by graphic adjustment. This type of procedure appears to be sufficient from a practical viewpoint as long as the graduation of the "ribs" is sufficiently reliable, that is for low and moderate loss ratios for which the volume of experience is fairly large. Mr. Dorweiler gives good reasons why, if this procedure is followed, the set of "ribs" selected by him for graduation is preferable to the set of "ribs" corresponding to a constant premium size which had been chosen by others as the basis for a graduation of the surface. From a theoretical viewpoint, however, it can be objected that there exists a theoretical relationship between excess pure premium ratios for different loss ratios and a constant premium size as well as for excess premium ratios for different premium sizes and a constant loss ratio. An ideal graduation method would reflect both kinds of relationship. A general outline of how this may possibly be achieved is given in the following.

The starting point is the distribution of risks of a given premium size x by size of loss ratio r. The relative frequency of such risks with loss ratios between r and $r + dr$ be $f(r;x)dr$. A good estimate of the function $f(r;x)$ can be easily obtained from experience, but it can also be derived from basic data for which there exists a larger volume of experience as will be explained later. Since r varies from 0 to ∞ we have obviously

$$\int_0^\infty f(r;x)dr = 1$$

The expected total losses of a risk of size x are

$$E = \int_0^\infty (x \cdot r) \cdot f(r;x)dr = x \cdot \text{permissible loss ratio} = x \cdot .581 \text{ (in New York)},$$

hence $\int_0^\infty rf(r;x)dr = .581.$

The expected losses in excess of a selected loss ratio r_0 are

$$\int_{r_0}^{\infty} x(r-r_0)\, f(r;x)dr = x(r-r_0) \int_{\infty}^{r} f(r;x)dr \Big|_{r=r_0}^{r=\infty} - x \int_{r_0}^{\infty} \left(\int_{\infty}^{r} f(r;x)dr \right) dr = x \int_{r_0}^{\infty} dr \int_{r}^{\infty} f(r;x)dr$$

The middle terms have been obtained by integration by parts and it can be shown that the expression immediately to the right of the first equality sign is 0 not only for the lower limit but also for the upper limit for functions $f(r;x)$ representing frequencies of risks by loss ratio. We have therefore:

$$\text{excess pure premium ratio } y(r_0;x) = \frac{1}{.581} \int_{r_0}^{\infty} dr \int_{r}^{\infty} f(r;x)dr$$

Excess pure premium ratios can, consequently, be obtained by integrating twice the frequency function $f(r;x)$ and can be graduated by graduating first the function $f(r;x)$.

The function $f(r;x)$ itself can mathematically be obtained from a distribution of the accident frequency for risks of premium size x and from the distribution of accidents by size of loss. The former can be obtained from purely theoretical considerations and a knowledge of the average number of accidents which is about proportional to the size of the risk. The latter does not depend on the size of the risk and can be obtained from the experience of risks of all sizes combined which increases its reliability. If need be, recognition can be given to the variation of this distribution for types of risks essentially different in hazard. The reviewer understands that an attempt on these lines is being made by a member of this Society.

With respect to the technical details of Mr. Dorweiler's method the reviewer thinks that it would perhaps have been preferable to assign smaller weights to the experience for large premium sizes before applying the method of least squares so as to avoid having the shape of the graduated curves determined to a large extent by the experience with the smallest volume.

At a certain point the author was compelled to substitute for some vanishing excess pure premium ratios small positive quantities in order to avoid logarithms of 0. It would have been desirable to test the admissibility of this step by substituting various small quantities such as .001, .002, .003 in order to ascertain whether or not the arbitrary selection of these quantities has only a minor influence on the shape of the curve. It is evident that, if the influence should be a major one, the otherwise systematic and objective method would be invalidated.