

MR. EDWARD S. GOODWIN: Lest there be a misunderstanding regarding individual estimates, it should be stated that death and specific dismemberment cases make up a considerable portion of the total estimates and for that reason the individual amounts are in the main fairly accurate and not merely an adjuster's best guess.

The correcting of last year's outstanding losses in this year's Gain and Loss Exhibit is for the purpose of obtaining in its most accurate form the incurred loss ratio of the most recent year of business. This is what that portion of the schedule should show, as otherwise it would exhibit a result which would misrepresent current conditions. It could be shown both ways but that might easily result in confusion. This procedure would not result in any less careful check being maintained upon previous estimates of outstandings as provided in another portion of the schedule.

THE CLASSIFICATION OF INDUSTRIES FOR WORKMEN'S COMPENSATION INSURANCE—E. H. DOWNEY.

VOL. II, PAGE 10.

WRITTEN DISCUSSION.

MR. W. N. MAGOUN:

Many of you who are in attendance at the meeting of the Society today have probably heard me make the remark that ten years hence, when we look back at the events of today, we shall realize perhaps more than it is possible for us to do right now, that in the year 1916 we were in our infancy in many matters pertaining to workmen's compensation.

True, wonderful progress in this field of insurance has been made during the last five years. The growth and development have been remarkable. It is not surprising, therefore, that although Dr. Downey's paper is yet scarcely a half year old, already history records changes of importance in several directions.

Experience-actual figures based upon real claim records is accumulating like a ball of snow rolling down hill, and that other kind of experience, namely, that which comes to individuals and is gained by everyday constant contact with events transpiring in the whole field of compensation, is accumulating in no less measure.

To discuss the "Classification of Industries for Workmen's Compensation Insurance" means in reality to discuss the Manual of Workmen's Compensation Rules, Classifications and Rates. It is a hobby of mine to put the "rules" first, for unless we have clear cut, workable rules the classifications and rates cannot be equitably applied.

An event which I believe will always be looked back upon as one of the notable landmarks, took place the latter part of the year

1915, namely, the Joint Conference on Workmen's Compensation Insurance Rates.* Although the title does not so indicate, the Conference carefully considered the rules and classifications as well.

The Rules Committee devoted its principal energy to the "Division of Payroll" Rules. As stated by the Committee "this is a subject which has caused a great deal of trouble in the past, it was carefully considered by the Committee and important changes made."

I believe that the rules as revised remove some of the objections raised by Dr. Downey. They attempt to make clear in respect to all classifications the question of whether or not a payroll should be divided. Although permitting somewhat more division than the former rules, they are more satisfactory in that they eliminate the option to "divide" or "not to divide" and specify that if "there are 'distinct enterprises' conducted in a given plant by the same employer and the entire work in each enterprise is conducted either in a separate building or on a separate floor or floors of a building, the employer conducting each of such enterprises as a separate undertaking with separate records of payroll, then such separate undertakings shall each be classified according to the Manual and the proper premium rate applied to each."

This rule is in line with Mr. Mowbray's discussion of Dr. Downey's paper. The Massachusetts Rating and Inspection Bureau has added to the rule for payroll division the words "meaning thereby operations which are specifically classified in the Manual" as defining even more clearly the exact significance of the words "distinct enterprise" and the Pennsylvania Compensation Rating and Inspection Bureau similarly interprets the rule.

At the suggestion of the Rules Committee of the Conference, every page in the Manual whereon appear any classifications, bears the words "See Division of Payroll Rules, pages 7 to 11 inclusive." This very important subject, therefore, would appear to have received the recognition and publicity to which it is entitled.

The new Manual is better than its predecessor in other ways. Dr. Downey criticizes, and properly in my opinion, such an inclusive class as "Electric Apparatus Mfg." A similar case would be "Plumbers' Supplies Mfg." Although the former classification is still retained, a means of dealing with classifications of this type is illustrated by the handling of Plumbers' Supplies Mfg. This classification now appears in the Manual as follows:

- "Plumbers' Supplies Mfg.:
- "Enamelled Iron Ware Mfg.
- Porcelain Ware Mfg.
- Pipe Mfg.—lead
- Pipe Mfg.—cast iron.
- Pipe Mfg.—wrought iron

* See "Proceedings of the Joint Conference on Workmen's Compensation Insurance Rates," published by New York Insurance Department, 1915.

Tanks, Seats and Cabinets (wood)

Valves and Gauges

Rubber Goods (not otherwise classified)

“If the business of the Assured is completely described by one of the foregoing classifications, the risk must be assigned to such classification.

“Plumbers’ Supplies Mfg. (not otherwise classified).”

The use of a general heading, with several classifications thereunder, is further exemplified by such classifications as

Agricultural Machinery Mfg.

Chair Mfg.

Coal Merchants

Foundries

Fuel and Material Dealers, and

Furniture Mfg.

all of which in my opinion tend to improve the Manual for the simple reason that they make it more clear.

Another feature in connection with this Manual, which is growing more common, is the use of explanatory footnotes. The chief objection thereto I assume is in the space taken up, and this is emphasized every time a page is reprinted. The advantages, however, appear to me to more than offset the objections, and I welcome the use of such footnotes. The note pertaining to “Salesmen (outside) Collectors and Messengers” is an excellent illustration as it definitely instructs the user of the Manual how to treat that classification in all its many phases.

That the immediate future will witness the adoption of either of Dr. Downey’s suggestions for the “re-construction” of the existing industry classifications, namely, the “industry-group” or the “operational” or process classification, seems unlikely.

It seems to me reasonably safe to prophesy however that the more extended use of, and improvements in, systems of schedule and experience rating will, if such systems prove their *raison d’etre*, tend materially to modify inequities as between two plants, which, while differing in their operational hazard, fall under the same manual classification.

I referred to this point in my discussion of Mr. Moore’s paper in the *Proceedings*, Vol. II, p. 281. If there are two plants, both manufacturing valves, one producing large, heavy valves, and the other small, light valves, and there exists but one rate for all establishments making valves, is it unreasonable to expect, other things being equal, that the experience of the former will be less favorable than the latter. If this is so, it follows that an experience rating system if properly worked out, will so affect the original base rate for these two manufacturers of valves that the manufacturer of the heavier article will pay a higher rate, and vice versa.

That the future development of rules, classifications and rates for workmen’s compensation insurance will be free from many of the objections raised by Dr. Downey, I believe to be reasonably assured.

Whether this development will proceed too quickly from some points of view, or not quickly enough from others, it is perhaps idle to speculate upon. That a safe and sane middle ground of continuous healthy growth may be the outcome, seems the more likely.

I have already referred to the Joint Conference on Workmen's Compensation Insurance Rates. Before adjourning, this Conference adopted a resolution, advocating the establishment of a permanent conference.

Such an organization is now an accomplished fact. The Insurance Departments of New York, Massachusetts and Pennsylvania appointed a "Standing Committee" consisting of a state insurance department, a state fund, three stock and two mutual insurance companies.

The Standing Committee has met, organized and elected Mr. Harwood E. Ryan of the New York Insurance Department as Chairman and Mr. Leon S. Senior as Secretary, and commenced to hold regular meetings. Representatives of the Insurance Departments of New York, Massachusetts, Maryland and Pennsylvania and of the Workmen's Compensation Service Bureau, Compensation Inspection Rating Board, Massachusetts Rating and Inspection Bureau and the Pennsylvania Compensation Rating and Inspection Bureau may attend its sessions. Each of the aforesaid rating associations has adopted a resolution expressing its willingness to cooperate. The Standing Committee has also invited the California Insurance Department and the California Inspection Rating Bureau to participate.

A central clearing house has therefore been established. The right of each state to make its own compensation rates will not be interfered with. Before adopting a change in Manual rules, classifications or rates, however, each rating association will have the opportunity of presenting such proposed alterations to the Standing Committee and of receiving the valuable advice and help which will come from an organization representing, as the Standing Committee does, all points of view as to what is best for the future, based upon the broad experience of the past.

On the one hand if a change in the Manual of real merit is proposed, all states will benefit. If, on the other hand, an alteration is suggested which may at first appear desirable to the proponent and careful consideration by the Standing Committee discloses a weakness, an undesirable change may be avoided.

Uniformity in phraseology, and substantial agreement as to matters common to all states will be the natural result, while local conditions peculiar to this or that state can readily be handled as "exceptions" as such matters must always of necessity be handled.

We have then at the present moment, just becoming effective, a revised basic manual of rules, classifications and rates for workmen's compensation insurance, and the machinery for effecting im-

provements in the same from time to time, as they may be proposed and after proper investigation and careful consideration be shown to be desirable.

I submit, therefore, that since the presentation of Dr. Downey's paper last October, many of his objections have disappeared, and the handwriting on the wall seems to indicate that even greater achievements are already under way.

NOTE ON THE APPLICATION OF RECENT MATHEMATICAL-STATISTICAL METHODS TO COAL MINE ACCIDENTS, WITH SPECIAL REFERENCE TO CATASTROPHES IN COAL MINES IN THE UNITED STATES—

ARNE FISHER.

VOL. II, PAGE 70.

WRITTEN DISCUSSION.

MR. ALBERT H. MOWBRAY:

Mr. Fisher says in closing his paper, "My chief object in presenting the results was to call the attention of the members of the Society to the practical use of the modern researches on mathematical statistics." It is, however, difficult for the average student to follow this paper because it assumes familiarity with the details of these researches on mathematical statistics. With these the average American student is not familiar because they have been made by German, Swedish and Russian mathematicians whose works have not heretofore been summarized in English and have not been followed very closely in teaching mathematics either in the American universities or to actuarial and other students after leaving universities. Mr. Fisher's recent book "Mathematical Theory of Probabilities" is a first step toward relieving this difficulty, but unfortunately the second volume which, as I understand it, will deal more in detail with the methods used here is not yet available.

Mr. Fisher takes rather serious issue with the teaching of probabilities as they have been heretofore presented to English-speaking actuarial students. As these men have generally heretofore confined their attention to life insurance, where conditions have become relatively more stable the difficulties resulting from this teaching did not strongly present themselves. In our work, however, they are more conspicuous.

The central theme of Mr. Fisher's paper seems to be, "All statistical series are subject to perturbations of various sort of quite different nature than the fluctuations in the ordinary games of chance, which follow the laws of mathematical probabilities. It is one of the paramount duties of the statisticians to try to measure the magnitude or force of such external disturbing influences in a purely quantitative manner." Starting from this point, Mr. Fisher

examines the experience with fatalities in coal mines over a fifteen year period from 1900 to 1914 inclusive, and by the application of his methods reaches the conclusion that there are large "perturbative influences" present. For a clear comprehension of the work given on page 71, a close familiarity with the methods described in Mr. Fisher's book on Probabilities, above referred to, is necessary.

Finding these disturbing elements, he excludes all catastrophes resulting in the loss of five or more lives, and finds by the same method that while the evidence of disturbing influences is reduced, it is strongly present. If I correctly understand Mr. Fisher's theories, this would seem to imply that even ignoring catastrophes involving five or more lives there are yet forces tending to produce great irregularity in the incidence of the isolated fatalities in coal mining work.

Mr. Fisher then takes up a study and comparison of twenty coal mining regions, concluding that there must be great variation from state to state so that it would be improper to apply frequency ratios based upon the experience of the Union as a whole. This study is based upon accidents of one year only, and a brief inspection of the returns would make this apparent without mathematical investigation. Mr. Fisher concludes that because the "coefficient of disturbancy" is imaginary for such states as Michigan and Iowa, there is small fluctuation in frequency from year to year. May not this have been due to the absence of any large accident in the particular region noted during the period under consideration?

Mr. Fisher then takes up the question of frequency of catastrophe hazard, and from a study of catastrophes of what he terms the first magnitude (resulting in the loss of from five to nine lives) he works out a series of probabilities for the number of catastrophes to occur in each year ranging from 0 up to 20. The table on page 77 shows a rather close fit between the computed probabilities and the observed frequency, but the figures on the whole are so small that one is inclined to wonder whether a different series of probabilities could not be determined which would give a considerably different distribution, and yet fit nearly as closely as the figures produced by Mr. Fisher. This is not to be taken in derogation of Mr. Fisher's method. It is a serious question in my mind how far we are safe in relying upon probabilities developed by any mathematical method from limited observations, or intended to apply to rare, infrequent evidence.

In closing his paper Mr. Fisher intimates that the methods used in this paper are more suitable for the solution of the problem, "How Extensive a Payroll is Necessary to Furnish a Dependable Pure Premium," than were the methods used by the writer in his paper, and I am inclined to agree with Mr. Fisher in this regard and trust he may be induced to apply his method to the solution of that problem, which yet remains one of the most important and difficult problems before us.

MR. ARNE FISHER:

AUTHOR'S REVIEW OF DISCUSSION.

Further comment on the remarks by Mr. Mowbray seems rather superfluous except on a few minor points. Mr. Mowbray mentions that the researches of the Danish, Swedish and Russian statisticians are not familiar to the American reader. He might also have added that the biometric methods of the Pearsonian School of England have received far too scant attention amongst assurance statisticians and actuaries in this country. I am further gratified to note that the reviewer has correctly interpreted my ideas of the fundamental principles of the theory of probabilities. In my opinion an *a priori* foundation of the probability theory as given by Laplace is indispensable. Laplace's "Théorie des Probabilités" remains to this very day *the great* work on probabilities. Unfortunately most English actuaries have not recognized its value and often misunderstood its intricate mathematical analysis and have instead followed the Gaussian School of Germany. This deplorable fact may partly be explained by the aversion to everything French prevailing in Great Britain after the Napoleonic Wars, during which period the first editions of Laplace's book appeared. It is to be hoped that the present war may cause a healthy reaction and turn the eyes of the English-speaking actuaries towards the immense wealth of information contained in the work of the immortal Frenchman. It is a fact that the very latest researches on mathematical statistics as carried on by the Englishman, Pearson, and the Dane, Thiele, may be brought to the simple principles found in "Théorie des Probabilités," which has been emphasized by the Swedish astronomer and statistician Charlier.

In regard to Mr. Mowbray's doubt about my coefficient of disturbancy for Iowa and Michigan I may add that all accidents resulting in the loss of five or more lives were eliminated for all the states for which I have computed Lexian Ratios and Charlier Coefficients of Disturbancy. Yet, after reducing all states to similar basis as far as the magnitude—or rather the absence of the magnitude—of catastrophes are concerned, the two states exhibit imaginary coefficients, which can be explained in no other way than by a state of greater stability. This greater stability does not necessarily mean that the average accident frequency is less in these middle states, but it simply indicates a dense clustering about the mean value.

Mr. Mowbray finally states: "It is a serious question in my mind how far we are safe in relying upon probabilities developed by any mathematical method from limited observations, or intended to apply to rare infrequent evidence." If Mr. Mowbray regards observations in the light of exposures the first part of his statement is correct. In regard to the second part I think I can satisfy all doubts by referring to the recent investigations of Bortkiewicz and Charlier, investigations originally started by Laplace and his dis-

ciple, Poisson. When the event is rare, its probability is very small and the ordinary "Law of Large Numbers" does not hold. That is to say, we cannot use the Bernoullian Theorem or the Gaussian exponential. The number of absolute frequencies are on the other hand expressed by the Poisson exponential. Bortkiewicz has shown that statistical frequencies of rare events, following the Poisson Law of Small Numbers, are more stable than the common frequencies. Neither Poisson nor Bortkiewicz use more than one term in the expansion of the B curve series. The parameter γ_2 (the eccentricity) is a measure of the fluctuations from the mean, thus corresponding to the standard deviation, σ , in the A type of curves and gives a closer fit of the curve. Now in regard to rare events, it is a rather paradoxical fact that series of such events present a greater stability than series of more common events. On the other hand, we do not need to know the total exposures to compute the relative frequencies when such frequencies themselves approach zero. The relative frequencies, or probabilities, as determined from the frequency curve may exhibit great deviations when applied in predicting the frequencies of catastrophes for a single year, but the same probabilities will show very small deviations in predictions in reference to a larger period, say a series of twenty or more consecutive years, and this is all that is required.

In regard to the dispersion theory most of the rudiments of this theory are found in the first volume of my book on "Probabilities." A further study of the higher statistical parameters is required in many cases, however.

THE DETERMINATION OF PURE PREMIUMS FOR MINOR CLASSIFICATIONS ON WHICH THE EXPERIENCE DATA IS INSUFFICIENT FOR DIRECT ESTIMATE—ALBERT H. MOWBRAY.

VOL. II, PAGE 124.

WRITTEN DISCUSSION.

MR. HARWOOD E. RYAN:

The method described by Mr. Mowbray for the determination of pure premiums in classifications which, taken singly, do not yield an adequate payroll exposure, contemplates two principal steps. First, the classifications must be grouped in such manner that within any group there will appear only classifications which may be presumed to be closely related in hazard. Second, there must be determined for each classification within each group, its relative hazard as compared with some one classification selected as the standard.

No one can say with certainty how much payroll must be obtained to form a satisfactory group. Nor is there any fixed rule by which hazards of the same kind may be determined. It is clear, therefore,

that the formation of groups and the determination of relative hazard must be, of necessity, a matter of personal judgment. The principal advantage of the method suggested by Mr. Mowbray is that it confines the exercise of personal judgment to comparatively small fields, thus rendering it as accurate as humanly possible. Of the two divisions of the problem the qualitative portion is of course the easier. Less difficulty will be found in the formation of the groups than in the determination of values representing relativity of hazard.

In practice it may not always prove feasible to establish arithmetical factors of relativity but the experience data can be arranged in ascending or descending order of hazard according to the best available judgment. The group pure premium will then furnish a basis for selecting consistent pure premiums for the several constituent classifications.

Turning now to the arithmetical process employed, it appears that while Mr. Mowbray has attacked the problem in a very ingenious manner, the formula which he suggests will not necessarily reproduce the original losses. A slight error has crept into the figures appearing on pages 132 and 133, the total observed losses being there reported as \$108,250. The correct amount is \$108,500. The particular illustration employed is unfortunate in that the payroll for the classification selected as the standard so far outweighs the payroll for the other classifications in the group that the pure premium for the group is practically determined by the pure premium for that classification. In applying a test to the formula, I have made a slight modification in the hypothetical data given in the table which appears at the foot of page 132, preserving, however, the original pure premiums there developed. I have reduced the payroll and losses for classification "C" to \$8,000,000 and \$10,000 respectively, obtaining the following results, the first according to the original formula and the second according to Dr. Rubinow's modification:

CLASSIFICATION "C" MODIFIED.

ORIGINAL FORMULA.

Classification.	Judgment Rating.	Reciprocal.	Observed Losses.	Mod. to Stand. Basis.	Pay Roll.	Pure Prem.		Projected Losses.
						Original Experience.	Adj. by Formula.	
A...	.50	2.00	\$ 2,500	\$ 5,000	\$ 500,000	.50	.0701	\$ 351
B...	.75	1.33	1,000	1,333	1,000,000	.10	.1052	1,052
C...	1.00	1.00	10,000	10,000	8,000,000	.125	.1402	11,216
D...	1.25	.80	5,000	4,000	5,000,000	.10	.1753	8,765
			\$18,500	\$20,333	\$14,500,000	.1276	.1402	\$21,384

Excess Projected over Actual Losses, 15.6 per cent.

MODIFIED FORMULA.

Classi- fica- tion.	Judg- ment Rat- ing.	Observed Pay Roll.	Corresp. P. R. Standard Basis.	Observed Losses.	Pure Prem.		Projected Losses.
					Original Experi- ence.	Adj. by For- mula.	
A50	\$ 500,000	\$ 250,000	\$ 2,500	.50	.0607	\$ 304
B75	1,000,000	750,000	1,000	.10	.0910	910
C . . .	1.00	8,000,000	8,000,000	10,000	.125	.1213	9,704
D . . .	1.25	5,000,000	6,250,000	5,000	.10	.1516	7,580
		\$14,500,000	\$15,250,000	\$18,500	.1276	.1213	\$18,498

Deficiency Projected under Actual Losses, .01 of one percent.

It will be noted that the modified formula reproduces the actual losses of the group to any desired degree of accuracy. The same will be true of the original formula by performing one more operation, viz., applying to the projected losses in each classification the ratio obtained by dividing the total observed losses by the total projected losses. Dr. Rubinow's method is more direct and involves the principle of weighting the observed payrolls according to relative hazard, thus reducing the total payroll of the group to the standard basis where the relativity is unity. The employment of reciprocals is not essential. Once having established the various degrees of relative hazard, it is only necessary to apply the factors of relativity to the observed payrolls, then to derive the hypothetical group pure premium and from it the new pure premiums for the individual classifications.

The proposed method is similar to the more obvious one of finding the actual group pure premium, applying the factors of relativity thereto and then adjusting the projected losses produced by the hypothetical pure premiums so as to reproduce the original losses for the entire group, finally increasing or decreasing the hypothetical pure premiums in the appropriate proportions. The following illustration will explain the point more clearly:

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Clas- sifica- tion.	Judg- ment Rat- ing.	Payroll.	Observed Losses.	Exp. Pure Prem.	Group Pure Prem. Mult. by Col. 2.	Proj. Losses Resulting from Col. 6.	Ratio of Actual to Proj. Losses.	Col. 6 Ad- justed by Col. 8.	Proj. Losses Resulting from Col. 9.
A50	\$ 500,000	\$ 2,500	.50	.0638	\$ 319		.0607	\$ 304
B75	1,000,000	1,000	.10	.0957	957		.0910	910
C . . .	1.00	8,000,000	10,000	.125	.1276	10,208		.1213	9,704
D . . .	1.25	5,000,000	5,000	.10	.1595	7,975		.1516	7,580
		\$14,500,000	\$18,500	.1276		\$19,459	.9508		\$18,498

Error = .0001 per cent.

If any group should contain one or more classifications yielding dependable pure premiums, the pure premiums for the remaining classifications in the group could be established even more directly by simply multiplying the dependable pure premiums by the factors of relativity. In groups which contain a number of minor classifications, no one of which can be said to yield a dependable pure premium, it is, of course, necessary to utilize the entire experience of the group in order to obtain the necessary standard of measurement. A point which Mr. Mowbray brings out and which should be emphasized, is that in determining rates based upon experience, the work should be checked up group by group so that the projected losses derived from the selected pure premiums will reproduce the observed losses.

Mr. Mowbray has rendered a real service in calling attention to the need for an orderly procedure in utilizing experience data as a substitute for the present hit-and-miss practice of selecting classification pure premiums regardless of their possible inconsistency in comparison with others. Even if it should prove impracticable to apply the suggested formula, as modified by Dr. Rubinow, I believe that the fundamental principles involved therein can be utilized by first classifying the data in broad groups and then following the suggestion to further limit the application of judgment by arranging the classifications according to their probable relative hazard in either ascending or descending order.

ORAL DISCUSSION.

MR. LEON S. SENIOR: I just want to refer a moment to Mr. Ryan's discussion. I noticed Mr. Fisher's statement in his paper today, that the underwriter should be entirely eliminated from the study of rate making, and leave it all to the statistician. It was, therefore, rather refreshing to me to note that Mr. Rubinow suggested that the theory which Mr. Mowbray introduced is rather dangerous for the reason that it would leave too much judgment to the safety engineer. Presumably, in the President's opinion, the judgment should be left to the underwriter, as I take it. That is, Mr. Mowbray would leave it to the safety engineer and Mr. Rubinow says it is rather dangerous. I don't believe the President has stated his reasons. Do you think that in the selection of the pure premiums of those classifications, it is preferable to leave the matter to the judgment of the underwriter rather than to the judgment of the safety engineer?

MR. I. M. RUBINOW: It is unfortunate that I should be called upon to answer that question, because I didn't put it up that way. I did not try to draw any comparison between the judgment of the underwriter and the engineer. My criticism was directed to Mr. Mowbray's plan of being only able to assign a difference in value to the hazards between small groups. I think that is a rather danger-

ous procedure as it worked out. As pointed out by Mr. Ryan, it would probably make all the other rates in the several classifications be entirely dependent upon the rates of the governing classifications to which it was approximated, except as rated up or down by the safety engineer, whose judgment would not be much better than that of the underwriter. We ought to eliminate individual judgment as far as possible, anyway.

What I did want to point out is that if we accept Mr. Ryan's idea as expressed in his discussion, we would not use the differential at all. You would have to fall back on the experience in different states, and then we would be confronted with the problem of insufficient payroll exposures, and the only solution offered is Mr. Mowbray's method. Whether the rating is furnished by engineers or underwriters it makes little difference; anyway, it is going to make a rough start in guessing what the hazard is. An engineer will determine whether a machine is hazardous or not, and his engineer's judgment ought to be valuable in preventing particular hazards. I don't trust much to quantitative judgment of engineers' estimates, even on individual machines, and especially on an industry as a whole. I think the safety engineer is not yet used to thinking in quantitative categories. If you put the question up to two or three engineers in different rooms you would get different results.

Let me illustrate the results of this method: you know the United States Government publishes estimates of crops, and their method of getting together estimates of crops is quick. You can imagine about ten million farmers, gathering and counting the amount of cotton each farmer collected, and at the end of December we know more or less how much the cotton crop has been; but you can imagine the great difficulty in saying in June or July what the crop is likely to be; yet the government is called upon to make estimates of probable crops in the future, and all our produce exchange transactions depend upon those estimates. You know that fortunes are made or lost on the day on which the cotton crop report is handed out. The method used is to receive estimates from thousands of people and average them by counties, then by states, and the official average must be made from state averages into one national average. They have four statisticians who sit in a room like little children and they are prohibited from talking to each other. They get estimates and figure and load and guess, each one separately, and after those four estimates are gotten together one estimate is telegraphed to New York.

Now, if some such process was introduced, of four engineers making those estimates, I think we would get an honest picture from those estimates and be able to judge whether those estimates are worth very much. Of course, it is not only a question of averages. It is often a question of who the engineer is, with the strongest personality and ability to compel others to accept his estimates.

MR. JOSEPH H. WOODWARD: Concerning the particular point now under discussion: I don't think the discussion of these details ought to be permitted to cover up the tremendous advance in practice which is represented by the acceptance of the theory that after a scale of pure premiums has been constructed for the classifications as a whole, that scale of pure premiums must be multiplied back into the exposures and adjusted so that the aggregate pure premium produced will agree with the aggregate losses. Although that may seem elementary, it is well known that this view has only been recently accepted. At the time, for instance, of the enactment of the Compensation Law in New York that was not done; and, of course, if that is not done, no one dealing with the problem has any means of estimating the aggregate error which has been introduced by what might be called the graduation of the experience among the different classifications so as to smooth out the effect of accidental fluctuations. The problem is in many ways not unlike the problem of graduating a mortality table. The first test that is applied after a mortality table is graduated is to multiply back for the whole table so as to test the agreement between the graduated and ungraduated data. After that is done, then the same test may be made by sections comprising certain age groups. Similarly, a compensation rate manual should conform with such a test, section by section, so far as the volume of payroll exposed permits.

MR. ALBERT H. MOWBRAY:

AUTHOR'S REVIEW OF DISCUSSIONS.

Mr. Ryan's analysis showing two fundamental requirements of the method I have described is entirely correct, and I am glad he has presented it, as it may make clearer the intention of the method. He is quite right in pointing out that in the selection of groups and the assignment of relative hazard personal judgment is required. The real purpose of the method was to, if possible, combine in a scientific way personal judgment and recorded experience, and I am glad to note that he finds the principal advantage to be that it confines the exercise of personal judgment to a comparatively small field, thus rendering it as accurate as humanly possible. I do not wish to minimize the strain which even this formula places upon those who are attempting to determine relativity in hazard. The problem even in its simplest terms is exceedingly difficult.

I had not found any cases where the formula as originally presented caused quite such a deviation in projected from actual losses as in the case Mr. Ryan cites. It is to be noted, however, that in the example he has set, the clash between the relativity of the indicated pure premiums and the judgment ratings is extremely severe. The reasoning I used led to the original formula and until Dr. Rubinow suggested it the simpler formula did not occur to me,

although the results under the modified formula are very much more satisfactory, and as I attempted to show in the note appended to the paper a slight change in the reasoning develops this formula. The example cited in the paper was only one of many worked out and the primary purpose was not so much to show the difference in accuracy of the two methods as to explain the method itself.

Mr. Ryan calls attention to what might be considered a more obvious method which consists in taking the unweighted average pure premium for the group as the basis, applying the judgment relativities to determine trial pure premiums for the classifications projecting the losses, and then carrying the trial pure premiums by a percentage so as to reproduce the losses incurred. When this method is fully carried out in such detail it arrives ultimately at the same result as by weighted average formula. It, however, clearly lacks the finish and elegance of form of the other formula. Under the weighted average formula, proceeding by an orderly method, the final result is arrived at and it is known in advance that unless an arithmetical error has been made no further adjustment is necessary. The method based upon using the unweighted average pure premium for the group is more a cut and fit process, although in actual operation it may be no more work than the direct formula.

Mr. Ryan suggests that, "If any group should contain one or more classifications yielding dependable pure premiums, the pure premium for the remaining classifications in the group could be established even more directly by simply multiplying the dependable pure premiums by the factors of relativity." While the results obtained by this method may not differ greatly from those obtained by the original formula, there seems to be both a theoretical and practical objection to it. From a theoretical point of view it sets a bad precedent in ignoring absolutely the experience upon the minor classifications in fixing their rates. It would seem that, theoretically at least, all the experience available on any classification should have some weight in determining premiums to be charged. From the practical point of view it may be criticized because we would then be compelled to say (assuming we are to deal frankly with our assured) to a group of employers: "We did not use our experience with your classification in determining your rate, but we did use the experience upon another classification which, in our judgment, presented similar, but not necessarily equal hazard, and which did have sufficient payroll exposure. The rate has been made, therefore, as a proportion of the rate on this classification."

Mr. Ryan has suggested that the illustration was rather unusual in that one classification was assumed to have so much larger volume of experience than all the others. The illustration was chosen in this way to answer the suggestion which had been made earlier orally, that a classification which had sufficient exposure

should determine its own pure premium, and should not be brought into such a combination which might seriously alter the premium on that classification. The illustration shows that under any such circumstances of clear dominance the self-determining classification would not be materially changed. If there were several such classifications in a group and the use of the formula did materially change the pure premiums, it would apparently be an indication that the judgment ratings of relativity were erroneous.