ABSTRACT OF THE DISCUSSION OF THE PAPERS READ AT THE PREVIOUS MEETING.

THE THEORY AND PRACTICE OF LAW DIFFERENTIALS—

I. M. RUBINOW.

VOL. IV, PAGE 8.

WRITTEN DISCUSSION.

MR. ALBERT H. MOWBRAY:

This paper covers so much ground in so great detail that it is extremely difficult to discuss it in a small compass. There are, however, a number of points to which it seems to me attention should be directed. The general subject of the actuarial ground work for the next revision of rates is now in the hands of the Actuarial Section of the National Reference Committee and we have had some discussion of the problems brought up in this paper. To some extent, therefore, my remarks may anticipate the work of that section.

Dr. Rubinow opens the paper with the assertion that "the principle of law differentials . . . needs no defence at this time." He does not, however, define precisely what he means by the *principle* of law differentials, although by implication it appears that he considers it to involve a comparison of the cost under two different compensation acts of a common standard set of accidents. It may be that in view of past conditions this method of procedure needs no defense because a better method had not been adopted. For my part I question how far at the present time we are justified in applying this principle if that is precisely what is meant.

At the bottom of page 10 the author refers to the work of Dr. Downey and Mr. Black, then with the Wisconsin Industrial Commission. This work, like my own work, which he refers to in his pamphlet on the standard table, was undertaken not for the purpose of rate determination, but for the purpose of studying comparative benefits under compensation acts. Here I think it is very clear no criticism of the single law differential can be made. As will appear later, I am not so clear as to its justification further.

On page 12 Dr. Rubinow refers to the standard accident table as having saved the situation in New York at the time of the adoption of the New York Compensation Act. It is my recollection that the first New York differential was not determined at all in accordance with the standard accident table. The same general prin-

ciples were used in computations made by Mr. W. W. Greene, then with the New York Insurance Department, and by others whose work was considered at the time the New York multiplier was prepared.

I question whether the author does not claim too much for the standard table particularly in citing the endorsements of it given by the actuarial committee at different rate revisions. At both the conferences the committee was very seriously pressed for time and its recommendations in several respects were not such as might have been expected under more favorable circumstances from the body of men that proposed them.

On page 13 the author takes radical exception to Mr. Ryan's statement in an earlier paper that law differentials would soon have to give way to experience in the determination of rates. The author apparently assumes that the only alternative lies between the determination of single law differentials either on the whole or by classifications, or the use of local experience and the determination of law differentials solely by comparison of pure premiums. I believe there is a third alternative.

The author refers, at the bottom of page 16, to the conclusions of the actuarial committee of the last conference. One factor which was before the committee at that time seems to have escaped his attention, namely, that there have been other factors than law differentials used in passing from basic pure premiums to state pure premiums, and that the use of experience data would tend to eliminate some of these factors. That, to my mind, was one of the greatest advantages which might have accrued from the use of experience differentials.

On page 18 the author presents an exhibit to show the fallacy of the use of experience differentials through a comparison of the pure premium levels between states. I can not conceive of such a state of affairs actually developing in a particular case as is set up hypothetically in this particular table. It seems to me closer adherence to actual fact in the choice of an example would have given the reader more confidence in the fairness of the discussion at this point.

On page 19 the author begins the discussion of some of the features of the standard accident table, which under a certain plan now under consideration becomes of little if any importance. It may perhaps at this time be well to bring to the attention of the Society another method of determining the rates in several different states from combined data.

As we all know, the losses in workmen's compensation naturally divide themselves into certain broad groups—compensation for fatal accidents, compensation for permanent disability accidents, specified indemnity for particular types of injuries in the different compensation acts, temporary total disability compensation, and medical cost. The cost of some of these elements is more or less

closely related with others, and the cost of some is only remotely related to the others, even within the same classification. Our experience data is generally filed so that the losses from the several sources are kept separate and it is, therefore, entirely feasible to determine the relative cost between different states, for example, as respects the fatal accident compensation, or as respects the compensation cost of temporary total disability. It is, therefore, entirely possible to bring together upon a common level the experience of all the states as respects each one of these elements, and to determine a basic pure premium for each of these elements separately. It is also entirely possible to determine a differential by which to proceed from the basic pure premium to the state pure premium for each of these elements separately. I believe this would be much the more logical mode of procedure. It would then be possible to fix upon a basic pure premium in varying groups; determining the medical pure premium, for example, per small group of closely associated classifications; the weekly indemnity pure premium over a somewhat larger group of perhaps less closely related classifications and so on, and determine the death cases from a fairly large group in which the death hazard is, so far as we can ascertain, about equal, but which are not otherwise necessarily closely related. The application of the several differentials to the several elements may be made independently and the state pure premium built up in this way. Under such a system it would be entirely possible to determine the differentials for medical cost directly from experience, and the other differentials from the use of standard tables. Of course, if rates be made by such a scheme the theory of the basic manual will have to be abandoned (except as to uniformity of classifications) as there will be no uniform state multiplier. I believe, however, the gain through the logical soundness in rate-making would more than offset the loss of convenience which might so arise.

It will be at once seen that under such a system of rate-making the relative weight of the several classifications of the standard table as discussed on page 19 and following becomes of little if any importance, the important thing being the correctness of the table within each individual section.

On page 24 the author discusses past methods of valuation according to the standard table. While I have had some work of this kind, others are so much more familiar with the computations referred to by the author that I will not attempt a discussion of this part of the paper, although I might point out that some of the methods referred to at the bottom of page 29 and the top of page 30 seem to me tantamount in many ways to the abandonment of the table as a whole.

On page 35 the author states his objections to the present basis of basic pure premiums. In these I heartily agree with him and it is my personal view that the present New York Act forms a very

much more suitable basis of comparison. The last section of the paper is devoted to a discussion of group differentials as compared with a general differential. While I am of the opinion that in some respects the group differential may be superior to a general differential, I think the methods heretofore suggested in this discussion are superior to either.

While there may be some advantages in the methods of organization suggested by the author I am inclined to believe, until further experiments have been tried, it is hardly necessary to go quite so far.

MR. G. F. MICHELBACHER:

I had hoped to find time to write a discussion of the details of Dr. Rubinow's paper. I find that it will be impossible for me to do so, but I cannot resist the temptation to say a few words on the subject in general.

The problem of law differentials is a big one, for it lies at the basis of most of the actuarial theory in workmen's compensation insurance. The law differential has been used, not only for the combination of state experiences, but also as one of the important factors in the establishment of state rates. In addition, the standard accident table and the law differential calculations have served many valuable purposes in connection with such problems as the valuation of outstanding losses, experience rating and so forth.

Such an important subject cannot be expected to have remained unstudied and undeveloped with the progress which has been made in the establishment of an actuarial theory of casualty insurance. There has come a gradual realization of the difficulties inherent in the theory of law differentials as we have practiced it, and it is certain that at the time of the next manual revision many changes will be made.

From this point of view Dr. Rubinow's paper is historical. He criticizes the old methods of calculation. Assuming that these methods will be largely changed, there is no reason why we should discuss them except as a matter of academic interest. Thus, Dr. Rubinow discusses the selection of a proper basis for a law differential scheme. He presents certain objections against the continuance of the original Massachusetts act as the basic act. No one will take issue with him on this point, for as a matter of fact it already has been decided by the Actuarial Section of the National Reference Committee to recommend a change.

Then again, Dr. Rubinow deals at some length with the problems of valuation which are found in the use of the standard accident table as the basis for the calculation of law differentials. I might take issue with him at certain points of the discussion, but in general I may say that many of his arguments already have been made and that steps are being taken to develop data with which to change the methods of law differential calculation. Thus, the National Workmen's Compensation Service Bureau has inaugurated

as part of its annual statistical program, the filing of wage data. With this information the effect of compensation limits may be more carefully investigated. The Bureau has also called for and will continue to require an analysis of accident experience by classifications. With this analysis of tabulatable accidents by kind of injury, much can be done to correct the standard accident table, and to refine certain methods of valuation which have involved an element of judgment in the past. The Bureau is also studying the cost of medical aid and has developed much interesting information which is being held for the coming manual revision.

So much for the future.

Turning to the past, I should like to point out that the theory of law differentials as it has been practiced has not resulted in any gross inaccuracy.

There has been much controversy concerning the use of a single law differential for the reduction of classification experience. It has been argued that the use of a flat law differential distorts the experience for certain classifications so that the reduced experience is not a proper indication. In answer to these arguments we have the tests which have been made at recent manual revisions which indicate that the established basic pure premiums have closely reproduced the cost of the original Massachusetts act which they were intended to represent. Thus, at the time of the last manual revision it was demonstrated that the basic pure premiums when applied to the Schedule "Z" experience of the Massachusetts Department reproduced the losses of that experience with the remarkably narrow margin of one-eighth of one per cent. Not only was the fit close on the entire experience but it was also satisfactory by schedules.

When the experience for every classification becomes broad enough to serve as the basis for the determination of the pure premium without the use of judgment, the most refined methods of reduction and projection will be necessary. So long, however, as the experience serves as no more than a guide to the pure premium, it is unnecessary to go into refinements so long as the results on the whole are satisfactory.

From the standpoint of projection, the law differential has ceased to be an important factor in those states where experience has been developed in considerable volume. Thus, the general level of rates in the Bureau states has been determined with reference to the actual state experience rather than by a combination of theoretical differential factors. If the theoretical factors produce the desired result, well and good; if not, an experience factor is introduced to force a balance.

One reason why greater progress has not been made in connection with the theory and practice of law differentials is that we have been too busy. In workmen's compensation insurance we always have had more than enough actuarial and statistical work to do.

Revisions of the manual have been made on the average once a year. They have been demanded on short notice, the work has been hurried so that it has not been possible to make extensive investigations of methods and under these conditions, so long as recognized methods produced substantially accurate results, they were continued.

This year, for the first time, we are contemplating a manual revision in the proper manner. We have had almost a year's notice. The Actuarial Section of the National Reference Committee and other committees have been at work for some months and the organization of the work is going on calmly, so that there is plenty of opportunity for study, for discussion and for the investigation of new methods of procedure and new theories of rate determination. The work is not being done by any company or by any interest. It is being done as Dr. Rubinow suggests it should be done, as a public business under the combined auspices of insurance departments, industrial commissions and state rating bureaus.

Under the circumstances it is certain that whatever is done at the next manual revision in the way of the use of law differentials, will be the result of the most careful investigation. This will mark a new page in the history of the subject and for this reason I believe that many of Dr. Rubinow's criticisms and suggestions need not be specifically referred to at this time. They will be answered in what takes place in the development of this subject in the next few months. It is certain, however, that Dr. Rubinow's paper will be valuable in this work and for this reason I for one should like to express my appreciation of it.

ORAL DISCUSSION.

MR. B. D. FLYNN: I should like to take this opportunity to say a few words with regard to Dr. Rubinow's Standard Accident Table. The table has been referred to in a commendatory way at various times during the meetings of the Society, but I have a feeling that many of the members have not appreciated how well this table filled an urgent need in the early days of workmen's compensation in this country. In fact only those men who were engaged in the first actuarial work of projecting rates for the new compensation acts of the various states can appreciate what a useful purpose was served by Dr. Rubinow's table. Founded upon the best of foreign statistics and such American experience as was at that time available it served as a measuring rod of the benefits of the various acts so that "differentials" for the projection of rates could be safely obtained. It is true that in the minds of some there was always a question if the table in certain divisions of injuries was a reliable guide to American compensation experience, but such questions do not affect the usefulness and value of the table for measuring the cost of the scale of benefits of a new compensa-

tion act against the cost of the scale of a compensation act upon which experience had developed. Dr. Rubinow's timely and skillful work in the compilation of this valuable table should receive due recognition.

MR. E. H. DOWNEY: I believe that in the Actuarial Sub-Committee of the Augmented Standing Committee of 1917, several persons suggested a computation of partial differentails in the manner just outlined by Mr. Mowbray. If my memory is not at fault, Mr. Mowbray made such a suggestion at that time, and I believe Mr. Woodward and myself made similar suggestions. As Mr. Mowbray has pointed out, the suggestion was not adopted, partly because of pressure of time and partly because of an inadequate analysis of compensation loss experience. I believe that the method Mr. Mowbray has outlined represents an enormous advance over the crude and inaccurate results of the single law differential. I believe, however, that possibly a still further refinement will give still better results, and such refinement appears to me to be entirely feasible, provided an analysis of statistical experience is made. The difficulty of partial law differentials is that when applied to classifications it introduces a chance variation. A permanent total disability, e. g., is a rare thing. A permanent total disability in New Jersey costs a maximum of $3,000. In New York the maximum would reach $18,000. I think that is within the possibilities. A permanent total disability might as a matter of actual fact have cost only $1,500. From the Rubinow Standard Table we expect one permanent total disability to ten deaths, and one death to one hundred accidents, so that the permanent total disability is very rare, and the classification experience in which there would be an expectancy of five would be a pretty large calssification experience. There are few if any classifications at the present time which show five permanent total disabilities. The mere chance distribution of those permanent disabilities by different states, by ages of the individuals who are injured, by the length of life after the occurrence of the permanent disability—because a man of twenty might die within twelve months—introduces a wide range of pure premium cost for the same number and severity of accidents. The mere chance distribution of those five accidents may produce a variation of 100 per cent. in aggregate cost.

Now I have taken, of course, the extreme case, but something of the same thing happens with regard to death benefits. Under even the Compensation Act of Pennsylvania, the maximum death benefit—or rather the largest amount as yet awarded in any case—is about $8,500. As an opposite extreme there are many cases which cost $100. The variation between states in cost of deaths to persons having the same number of dependents is also wide, so that you have there again a very large chance element. You have a similar chance element in the case of partial permanent disabilities. Permanent partial disabilities are more numerous than

deaths, and their cost varies greatly from causes which have little connection with the nature of the injury itself. In Pennsylvania, e. g., the total loss of the hand would be compensated by 215 weeks, but a 50 per cent. disability of the same hand would in all probability be compensated only as a temporary disability. That is not what the law contemplates, but that is what is done and what appears in your pure premium experience. The same thing holds true in other states. A great many cases of what are actually permanent disabilities are compensated as only temporary disabilities. This is due to administrative defects. You have here a large element of chance deviation.

Now it seems to me that these monetary fluctuations can be largely done away with by basing our pure premiums on the number and severity of accidents rather than the magnitude of monetary loss.

If, e. g., you take the logging experience of the compensation states, you will get a certain large payroll exposure. Against this exposure you will have a large number of deaths; also a considerable number of permanent total disabilities, that being one of the industries in which they concentrate; also a large number of permanent partial disabilities, a large number of temporary compensatable disabilities, and so on. A distribution of these accidents in that industry could be made on a basis similar to Dr. Rubinow's Standard Distribution. To obtain the pure premium for Pennsylvania, value the accidents which have occurred in the logging industry of all states on the Pennsylvania scale of benefits, as determined not by theoretical computation of the law differential, but by the actual experience of the state for similar injuries. This will give you a good basis for valuing deaths, disabilities of all kinds, and medical cost. It seems to me that this method, while at the first glance it appears to involve more work than even Mr. Mowbray's method—in practice I don't think it would involve more work, once the table were constructed—would, I believe, give results which would be more defensible than would ever be arrived at from monetary pure premiums alone.

It should be mentioned, of course, that the problem of law differentials does not arise with respect to a classification which in a given state produces sufficient exposure for sound rate-making. No one, I take it, would wish to combine the Pennsylvania bituminous coal mine experience with the experience of any other state for the purpose of making Pennsylvania rates. No one, I think, can rationally argue that the addition of the California, Maine and New York logging experience adds anything of value to the Wisconsin logging experience. In these cases the gain in volume of exposure is much more than offset by the loss of homogeniety. At most it would be reasonable to combine the logging experience of the Great Lakes states. But the problem of law differentials does arise with respect to the logging industry in Penn-

sylvania, which certainly does not produce a sufficient exposure for rate-making. For these minor industries—and their name is legion—the total experience of the state with respect to the cost of particular injuries is a better basis of valuation than the monetary loss experience of the industry for the country as a whole, while the number and severity of injuries per unit of exposure for the industries of the country as a whole is the most suitable basis —indeed the only possible basis—for predicting the number and character of injuries per unit of exposure in that industry in the given state.

I should like to add that the problem of projecting rates is the problem of predicting the number and severity of industrial accidents per unit of exposure. The probable loss cost is based upon the probable number and severity of accidental injuries, and the past monetary loss cost is a trustworthy basis of prediction only insofar as the corresponding accidental injuries are likely to be reproduced. When we are dealing with very large exposures it may reasonably be assumed that the chance deviations in monetary loss cost will cancel each other, at least to a large extent. But when we are dealing with those classifications in respect to which the problem of law differentials arises, this assumption is unwarranted.

MR. J. H. WOODWARD: Referring to this question of the monetary loss *vs.* the number and kind of accidents as the basis of differential calculations, it seems to me that Dr. Downey's hopes that it will ever be feasible to make a satisfactory analysis of these statistics upon the basis of the number and detailed description of accidents for each classification are not likely to be fulfilled. In practice, what we are finally interested in is the number of dollars that it costs to pay compensation in a certain state and in a certain classification, and if, as now seems probable, we are going to get compensation costs divided into death, permanent disability, temporary disability, and medical, for each classification and each state, we are going to get all that can be reasonably required—certainly for the time being.

I think that this discussion, and also Dr. Rubinow's extremely interesting paper, has shown how far we have progressed since the Rubinow Standard Accident Table was originally promulgated. One of the striking features of the paper, and one which is extremely creditable to the author, is the indication of progressiveness in ideas on his part. Incidentally it seems, in the light of what we have come to realize are the difficulties of the subject, rather odd to think how many persons hailed this table when it was first put out as something that would do for workmen's compensation insurance those things which the American Experience Mortality Table has done for life insurance. Of course, that expectation was bound not to be fulfilled. This is not saying that the table was not as good a table of the kind as could be constructed at that time, or that it did not serve an extremely useful purpose. It simply

means that the problem is entirely different and there is not any analogy at all between the Standard Accident Table and the American Experience Mortality Table—that, in fact, the word "standard" applied to the accident table is to a certain extent a misnomer.

MR. G. F. MICHELBACHER: I must admit that Mr. Downey's proposal to use a combination of accident experience, rather than a combination of loss experience, for the determination of state rates, is one which I have never heard discussed before. I wonder, however, whether the proposed method will cure the evils which it is designed to eliminate.

So far the law differential has been used for two purposes—for reduction and projection. By reduction is meant the process of reducing the losses of the different compensation states to a common level, so that the experience of each classification for the United States may be pooled and thus made available in the greatest volume for rate-making purposes. By projection is meant the process of taking the basic pure premiums established upon the combined experience of all states and translating them into state pure premiums, which are used as the basis for the calculation of state rates.

It is only in the process of reduction that the problem of incomplete experience is found. Naturally there will be but little experience in many classifications in an individual state. The losses in these cases will be incomplete and abnormally distributed to the several types of injury. It follows, therefore, that a method of reduction must be adopted which will give proper weight to the losses by injury divisions, for otherwise there would be some distortion of the experience in the reduction process.

In projection on the other hand, it may be assumed that the basic pure premiums are complete, for if there are any loss elements missing in the basic pure premium experience, they are supplied by underwriting judgment or by actuarial calculation. Thus, if there are no death losses in the experience for a certain classification, this element is supplied by the committee which establishes the basic pure premium. In projection, therefore, there is no problem of incomplete experience or incomplete pure premiums.

If I understand Mr. Downey's proposal correctly, it is designed to avoid the difficulty occasioned by incomplete experience. He would take the accidents by classifications and merely combine the results. In this way the experience for any classification for the United States would be obtained without the necessity of reducing the separate experiences to a common level. For projection, Mr. Downey would employ a method of valuing the accident distribution for each classification upon the basis of experience for the individual state. The method of valuation would undoubtedly involve the accumulation of Schedule "Z" data by kind of injury.

Under this plan the basic pure premium for the sawmill classification would be obtained by taking the standard distribution of

accidents by kind of injury and applying this to the distribution of losses ascertained from Schedule "Z" experience.

I question whether this will solve the difficulties which we find in the present system.

In the first place, what will be done to complete an accident distribution for a classification for which the United States experience is obviously incomplete? Loss elements may be supplied by judgment where the experience is inadequate. If the problem were refined and placed on the basis of accidents, I doubt whether the inadequacies of experience data could be accounted for in this manner.

In the second place, how shall we determine the division of loss cost for the state from Schedule "Z" experience? Shall we take the combined results for all classifications, or shall we rely upon the indicated results for the individual classification? If the first method is adopted, an error undoubtedly will be made, for, as is well known, the cost per case of the various benefits is decidedly different in different industries. Thus, the cost per case of medical treatment varies considerably by classification and the same is true of the cost per case of temporary disability cases, dismemberments, and so on.

If these costs were determined by the combined results for all classifications, they would be wrong for the sawmill classification. The proposed method would require the application of the division of loss cost to the distribution of accidents for the sawmill industry. In this case the result would be fictitious and incorrect.

If the second method of determining the distribution of loss cost were followed, the situation would be as difficult as the present one, for we should be in the position of establishing rates for the majority of classifications upon inadequate experience. Thus, if the Schedule "Z" data for a state for the sawmill classification were incomplete, how could a valuation of compensation cost be made?

Finally, I can see trouble connected with the proposed method from the standpoint of the approval and justification of state rates. The supreme test of the accuracy of a state rate has been the loss experience indication for the state. This has been the reason for the various state Schedules "Z." It would be exceedingly difficult, if not impossible, to apply this test if the division of losses ascertained for the state for all classifications or for a single classification were applied to a hypothetical accident distribution for an individual classification. If the comparison of the hypothetical rate with the actual indicated pure premium for the classification were out of line, the actual experience would be given preference and inasmuch as it is likely that the number of these cases would be considerable, owing to the possibilities of error which I believe to be inherent in the proposed method, it is not probable that rates produced in this manner could be adequately justified and substantiated to the satisfaction of supervising authorities.

MR. I. M. RUBINOW:

(AUTHOR'S REVIEW OF DISCUSSION.)

In reviewing the numerous interesting discussions by several prominent members of the Society, I am unfortunately laboring under a very serious handicap. For over half a year I have been kept away from New York and from personal contact with the compensation insurance business because of my effort "to do my bit." I am, therefore, not sufficiently familiar with the recent developments and plans for the changing of the entire system of compensation rates, so frequently referred to by those who took part in the discussion.

Briefly, these discussions may be classified under the following three heads:

1. General objections to my claiming too much for the method of law differentials and my own services in connection therewith.
2. Detailed criticisms of various technical points discussed in my paper.
3. Statements of the new and better methods of deriving compensation rates, now being elaborated by the various actuarial committees.

The first issue, being largely a personal and insignificant one, can be readily disposed of.

Nowhere in this paper, or any where else have I claimed the virtue of perpetual infallibility, either for the method of law differentials or for the Standard Accident Table. The comparison of this Table with a Mortality Table was but a highly exaggerated compliment paid by others to my work (I believe Prof. A. W. Whitney was the first to suggest that phrase) perhaps because at the time (1914) it did solve a great many vexing problems. It was Prof. Whitney who suggested the advantage of a differential system, but the method of arriving at one had been worked out by me, and in fact suggested as early as 1911 and my files contain very complimentary letters from many students, including Mr. Mowbray, to the effect that my first article on the subject did furnish a method of computing compensation costs (and rates after all are necessarily an expression of costs) which they had been at a loss to formulate.

And whatever elements of inaccuracy the method has developed, no matter how many changes and improvements have since been suggested, I believe an impartial valuation of the services performed by the Differential Method and by the Standard Accident Table cannot afford to disregard the fact that after four years it is still today the official basis of almost all compensation rate computations, and the actual changes introduced since 1914 have been rather slight. The method has not only been approved by actuarial committees, but by most of the state authorities intrusted with the

duty of controlling compensation rates. Even Dr. E. H. Downey in his very severe arraignment of the present methods of calculating compensation rates (*Journal of Political Economy,* December, 1917). "The Making of Rates for Workmen's Compensation Insurance" is forced to admit: "The most that can be said for this method is that no better basis has as yet been devised for estimating the relative cost of compensation laws *in advance of experience.*" That, however, was precisely the problem to be solved in 1914. One is, therefore, somewhat at a loss to understand Dr. Downey's scathing reference to "the crude and inaccurate results of the single law differential."

Mr. Mowbray objects to the claim that the Standard Accident Table "saved the situation in New York at the time of the adoption of the New York Compensation Act," and points out that the first New York Differential was not determined in accordance with that table. In justice to myself, may I state that at no place in my paper was this claim made and the sentence referred to by Mr. Mowbray reads:

"It may be recognized that the Standard Accident Table at that time saved the situation for the entire compensation business."

The Actuarial Committee was appointed and the Table prepared after the New York Act went into effect. It is true, however, that the method of obtaining a differential between the two acts (Massachusetts and New York) was suggested to Mr. W. W. Greene by myself, the Austrian distribution being used, since there was no accepted standard table.

2. As to the substantial points raised by the paper, their discussion, very much to my regret, is not as complete as I might have desired. Nevertheless, in regard to some points, as the valuation of medical costs and the effect of wage influences, I am glad to know that these are being taken into consideration and perhaps my paper was not without some assistance in the matter. I think it extremely significant that such a prominent authority as Mr. Mowbray is on the subject of compensation rates, avoids the discussion of the methods of valuation on the plea that "others are so much more familiar with the computations." Yet all the rates in the past have been based upon differentials, and the differentials necessarily upon the methods of valuation. To me it offers strong evidence that at least in the past the entire organization of differential work was not entirely satisfactory, too much being accepted on faith. At least every time I was forced in the capacity of a consultant, to disagree with personal friends and co-workers of yesterday, in regard to rates, the question most frequently at issue lay in these very details of computation.

Mr. Mowbray makes the point that my illustration of the possible results of the experience differential is artificial, improbable and therefore not convincing, and recommends "closer adherence to actual fact in the choice of an example." This, of course, is a

point well taken. But actual facts of experience have not been published in such detail as to be available to the outsider. Besides, the illustration had to be made simple and schematic. Yet actual experience as far as available does play tricks like that. Here is for instance the pure premiums for different years even within the same state. I am taking the two Wisconsin reports giving pure premiums for 1911–1913, and 1914, picking out the comparative classifications as they come, without any selection.

	1911-13.	1914.
Lead and zinc mining	3.50	2.68
Quarrying	1.44	2.58
Glue manufacturing	1.37	.49
Paint	.45	.72
Soap	.76	.38
Brick	1.46	1.49
Stone cutting	.72	.63
Bakeries	.55	.31
Breweries	.83	.90
Candy	.15	.27

The pure premium is therefore greater in 1914 in five and smaller in the other five classifications, some pure premiums being 80 per cent. greater and others 65 per cent. smaller. And yet only on classifications with a substantial exposure are these comparisons available and it being the same state, the fluctuations in payroll are insignificant.

Classification.	Massachusetts.			Wisconsin.			Differential.
	Payroll.	Losses.	Pure Premium.	Payroll.	Losses.	Pure Premium.	
Stone cutting...	$1,218,242	$88,074	.66	$762,700	$ 5,508	.72	1.09
Bakers	2,357,665	8,187	.35	707,800	3,906	.55	1.57
Brewers	1,572,542	20,229	1.29	3,977,900	32,975	.83	.64
Candy manufacturing	2,178,719	7,344	.34	498,000	764	.15	.45
Cigar manufacturing	652,300	934	.14	665,240	19	.003	.02
Boot and shoe...	42,264,508	50,641	.12	2,891,200	11,014	.38	3.17
Brass goods	759,465	4,188	.55	198,000	286	.14	.25
Elec. apparatus..	1,071,331	2,753	.26	381,100	436	.12	.45
Foundries	1,760,051	9,553	.54	1,409,300	13,563	.96	1.78
Jewelry	4,832,453	7,511	.16	200,700	248	.12	.75
Total of 10 classifications	$ 58,667,276	$ 119,414	.204	$ 11,692,040	$ 68,719	.587	2.88
Total exposure, all classifications	429,739,137	1,295,449	.301	113,498,500	926,323	.816	2.71

In order to test the influences of such accidental fluctuations, I have made a further comparison of the experience of ten comparable classifications for Wisconsin and Massachusetts (experience of 1911–1913 policies inclusive), selecting the first ten in the Wisconsin list for which such a comparison could be made.

The results are startling, to say the least. The general pure premium for Massachusetts was .301 and for Wisconsin .816, indicating a differential of 2.71. Taking these first ten classifications, the Massachusetts pure premium is .204 and the Wisconsin pure premium .587, indicating a differential of 2.88. The fluctuations in the experience differential for individual classifications are from .02 for cigar manufacturing to 3.17 for boots and shoes.

Applying the same methods as used in the table on page 11 of the *Proceedings,* the following results are obtained. The Massachusetts payroll, with the Wisconsin pure premium produces an average pure premium of 1.39 and a Wisconsin Differential of 1.87. The Wisconsin payroll with the Massachusetts pure premiums produces an average pure premium of .641 and the differential for Wisconsin figures at $(.587 \div .641) = .91$.

Finally, if the two payrolls are combined, and either series of pure premiums is applied, the average pure premium for Massachusetts becomes .276 and for Wisconsin .414, producing a differential for Wisconsin of $(.414 \div .276) = 1.50$.

Here then we have the following experience differentials: 2.88, 1.87, .91 and 1.50, showing a variety as great as that quoted in my purely hypothetical illustration. The fact that one of the methods produced 1.50 (the old Wisconsin differential) may point to the fact that this is the best method (adding both payrolls, applying both series of pure premiums independently, and comparing the losses thus computed) but then again this result may have been purely accidental. In any case, Mr. Mowbray's objections to the use of the hypothetical illustration falls to the ground and the illustration from actual experience plainly shows the gross inaccuracy and thorough unreliability of the so-called experience differential.

Furthermore, Mr. Mowbray points out my failure to define the *principle* of law differentials. Formal definitions are proverbially difficult, but surely there must be some agreement as to the nature of this method among all those actuaries who still persist in using it. Else how can they justify the rates computed by them for two score states from one common set of basic pure premiums?

The very purpose of the paper, however, was not to take credit for past performances, or to stand pat in defense of old methods, but to subject both the method and the result to some critical examination. I am extremely gratified that Mr. Woodward has so clearly perceived this purpose.

The table itself should, if it is to continue in use, be carefully revised, and the method of group differentials be made possible by

some simple system of providing for modification of the Standard Table for group purposes.

3. The references to latest developments in rate-making, made by several gentlemen participating in this discussion, are most interesting. Unfortunately, they are not equally clear or sufficiently explicit to permit any judgment by anyone who is not within the inner circle of actuaries working on this problem.

I find Dr. Downey's discussion in this respect least definite. His suggestion for "partial differentials" in no way changes the differential system, except that it introduces a refinement in the method of reduction of experience, a refinement which is on the whole unobjectionable. Dr. Downey is afraid of the "chance element." But after all, if one deals with insufficient experience in any one state, then that experience is all "chance," and the chance element due to the wage conditions or marital relations is no worse than the chance element in accident frequency itself. It is necessary to remember that the cost of compensation (and therefore the rate) does depend upon these wage and marital relations as much as upon accident frequency and that compensation rates are not a fine for accidents. If, for instance, the lumber industry employs a larger proportion of single men, its cost for fatal accidents will be lower, and therefore its rate lower than it otherwise would be though the social cost in accidents and loss of life and suffering may be greater.

I fail to see that Dr. Downey's suggestion of deriving the rate from accident experience direct instead of cost experience will reduce the number of difficulties. To begin with, it will require a volume and excellence of accident experience which will probably remain a utopia in this country for some time to come. And even if that is available, it is, I believe, at least as inaccurate as is the Standard Accident Table, to assume that the "actual experience of the state for similar injuries" would hold true for each classification because it holds true for the state as a whole. In any case, the same logical error is committed, which is charged against the Table.

In his article already cited here Dr. Downey says:

"A flat law differential, in truth, rests upon an assumption which is clearly contrary to fact—the assumption, namely, that the distribution of severity of work injuries is the same for all injuries. . . . Unfortunately, for the hypothesis, the facts are notoriously otherwise."

In justice to all those who contributed to the elaboration of compensation rates, it might perhaps be pointed out that this shortcoming was clearly recognized. Not only is it clearly stated in my paper under discussion, but also in the first paper on the subject published in 1914. (*Proceedings,* Vol. I, p. 10.) The flat differential rests therefore only upon a "working assumption," the inaccuracy of which has been thoroughly recognized all along.

What I have tried, however, to explain in my paper, in definitely advocating "group accident tables" and "group differentials," was that while this assumption does introduce an element of inaccuracy, which can by this time be eliminated, that element of inaccuracy is not as great as might be imagined at first sight (see my discussion, pages 37–40), and that the reason experience differentials of many classifications widely differ from the general law differential is because of the insufficient experience of individual classifications in separate states, introducing a chance element of accident frequency. If in bringing together the experience of many states, the number of accidents were studied as well as the losses, these wide chance fluctuations would become very obvious.

May I conclude by expressing my deep appreciation of the statements made by several speakers, notably Messrs. Flynn and Michelbacher, concerning the value of the differential method and the Table, at least in the past. So much experience has been accumulated during the last five years and so many able minds have labored over the problems of compensation rate-making, especially under the stimulus for scientific research, furnished by this Society, that what constitutes a new branch of science has been created in an amazingly short time. But while temporarily, at least, deprived of an opportunity to contribute to it, I need not be criticized for deriving a certain amount of satisfaction from the knowledge of having contributed something to the foundations of this new science, nor for desiring to cooperate in the further perfection of this structure.

PREMIUMS AND RESERVES OF THE SWISS ACCIDENT INSURANCE INSTITUTION—JOSEPH H. WOODWARD.

VOL. IV, PAGE 45.

WRITTEN DISCUSSION.

MR. BENEDICT D. FLYNN:

We are greatly indebted to Mr. Woodward for bringing to our attention this interesting and instructive outline of the methods used by the Swiss Accident Insurance Institution in the computation of workmen's compensation rates, the rating of risks and the calculation of reserves. To the writer it was particularly interesting to learn how closely the Swiss actuaries have paralleled the best thought in this country in the solution of the many problems of this business. The paper not only gives to the men engaged in the actuarial and underwriting work of workmen's compensation insurance in this country a new realization of the universal character of the problems with which they are dealing, but is also a source of encouragement to them in showing that the solution of these problems reached abroad after many years of experience and study so nearly approach the solutions obtained, or which are being worked out, here.

The method by which the Swiss actuaries worked out their "differential" problem—meaning the problem of relating the experience of one set of compensation benefits to the basis of another—approximates to some extent the actuarial work of this kind carried out in this country. The method of the Swiss actuaries was, briefly, to divide the total claims under a compensation act into certain major divisions of benefits, to weigh the cost of each of these divisions according to the old and the new scale and to obtain a "coefficient of transformation" or differential in order to relate one scale to the other. The divisions of claim costs were made for each industrial classification and by applying the differential for each division, an average differential to cover all of the benefits for the particular classification obtained. In the early work in the United States an estimate of the cost of all divisions of benefits was made, but a single average differential for all classifications was obtained and used. I will not attempt a lengthy explanation or justification of the single differential method as applied in the early work in this country. Dr. Rubinow, in his recent paper upon the subject (*Proceedings,* Vol. IV, p. 8) has gone into this matter fully and has shown that paucity of experience, urgent necessity for quick results and the particular problem of projecting rates for new com-

pensation rates practically necessitated this method of treatment. I believe too that it was brought out clearly at the last general revision of workmen's compensation rates that the system of expense loading which had been in vogue fitted well with a single differential method and that to utilize differentials varying by classifications without changing the method of expense loading would give dangerous results.

The detailed method and the basis of the important work of estimating the cost of the particular benefit under the new act is not given. It is generally known that in this country the Standard Accident Table compiled by Dr. Rubinow served as a most valuable basis for this sort of work. It is to be presumed, however, that adequate statistical data for measuring the cost of the various major divisions of benefits under the new act were available to the Swiss actuaries. It is to be presumed also that the Swiss actuaries were aware of the danger of utilizing experience data which was not fully developed in a plan which called for a division of total compensation cost into major benefit divisions. At the last general revision of compensation rates the danger of dividing the total cost when the claims were still in the temporary total division which later would reach the permanent total division, or possibly death division, was apparent. I believe it is safe to say that the general method pursued by the Swiss actuaries is that which actuaries in this country have felt for some time was the best solution of this troublesome problem.

It might be pointed out at this time that the future differential work in this country should present a much easier problem in view of the fact that not only will experience be available for measuring the weight and average cost of the particular benefit under one act, but will also be available for measuring these factors in connection with the act to which the first experience is to be related. In fact it may possibly be unnecessary in certain large classifications to utilize a differential but simply to use the experience in certain divisions, such as medical, of the state.

The rates of the Swiss Institution are loaded by the method of percentage loading plus a constant. This plan of expense loading so well set forth in Mr. Woodward's paper (*Proceedings,* Vol. III, p. 140) was fully considered at the last general rate revision, but because of the fact that it was a practical necessity at that time to utilize singe law differentials it was not adopted. In Pennsylvania, however, at the present time a method which follows the percentage loading plus a constant plan has been used in calculating the manual rates. At the next general rate revision it is probable that this method will receive favorable consideration together with the method of differentials by industrial classifications.

It is most interesting to note that the manual rules with regard to classification of risks and division of payroll follow so closely the American practice. Some of the rules of payroll division differ

somewhat, as, for example, the much discussed box and container manufacturing classification is considered as not incidental to the general classification by the Swiss and is rated separately, whereas it is now considered as incidental in this country and is not rated separately. On the whole, however, the rules are remarkably similar in the two countries and the underlying principles of classifying risks are practically the same.

The division of enterprises into large and small, i. e., the latter containing ten or less employees, is interesting as regards its bearing on the minimum premium problem in this country. Mr. Woodward does not tell us if the smaller enterprises carry a higher rate than those in the same industrial classification with a larger payroll. Although we can see another distinct purpose in this division of risks into large and small, namely, to indicate whether or not the work should be considered as incidental to the main enterprise, it seems reasonable to believe that this is the manner in which the Swiss Institution meets the undoubtedly heavier cost of the very small risks.

The Swiss method of classifying risks by hazard utilizes 350 classifications, each of which may carry seven different rates representing what is described as "a degree of risk" within the classification. This in a way approaches the American method which employs something like 1,500 classifications. Although in this country almost innumerable variations of the classification rate can be obtained through schedule and experience rating, the principal point of interest in comparing the two methods of rating seems to be that under the Swiss plan a risk in a certain classification may obtain a rate as great as three times that of another risk in the same classification. Under schedule and experience rating in the United States the maximum range is from 40 per cent. below manual rate to 40 per cent. above, although the new experience plan which is now under consideration by various states places no absolute limit upon the range of the rate—although the method itself automatically effects a satisfactory check. The explanation may be that there are certain classifications in the Swiss manual, for example, Machine Shop with Foundry, and that under this classification in the plan of rating by "degree of risk," Typewriter Manufacturing is placed at one extreme and Mining and Milling Machine Manufacturing at the other, while under the American plan each of these risks would be placed in a separate and distinct industrial classification. It is probable that with a full explanation of the Swiss rating plan we would find that the rating given to a particular risk would not differ greatly in the two countries.

Mr. Woodward states in referring to the Swiss method of rating: "In general the first consideration in determining the degree of risk is the previous accident experience for the enterprise," and further: "It is emphasized that for large industries first importance

should be given to the statistical history." The conclusion by Swiss actuaries that experience is the dominating factor in rating risks, particularly in large enterprises, is gratifying to those of us who have for some time felt that experience rating which reflects both the moral and physical hazards should receive due recognition as the most important factor in obtaining the adjusted rating.

The factors considered in fixing the degree of risk are mainly those which are covered under the schedule and experience rating plans of this country. Certain of these, however, such as "nationality of the employees," "the proportionate number of apprentices and young workmen in relation to the total number of workers," "the proportionate number of female workers as related to the total workers," are not considered in this country except as to their effect as shown in the experience rating plan. This statement may be modified, however, by stating that in the schedule rating plan the number of operators on machines might give an idea indirectly of the number of apprentices. Some of the factors mentioned, such as "the proportionate amount of wages of office employees as related to the total wages" and "supplementary insurance"—not referring to boiler insurance or similar lines—are not to my knowledge utilized in connection with the rating scheme in this country.

An inspection of the rates shown in the comparison between those of New York and the Swiss Institution would lead one to think that the New York rates followed more logically the relative hazards of the particular classifications, but conclusions from this would probably be misleading as we are not certain that the industrial conditions are the same in both countries. It is interesting to note that in the majority of classifications New York rates are shown to be lower than would be indicated by a rough differential between the two scales of benefits.

The problem of reserve calculation differs so much from that generally met in this country because of the difference in the type of benefits in the two countries and in method of claim administration, that we cannot obtain much light from the Swiss plan. There are no specific benefits payable for certain types of injuries in Switzerland which can be compared with the benefits for dismemberment or loss of use under most of the acts in this country. All permanent disabilities which are not total are valued according to the judged degree of impairment in earning capacity, and consequently the rate of revision of compensation is the important factor. Incidentally, when we consider that the great majority of permanent disability cases fall into the class of dismemberment or loss of use, we can see the immense amount of work and the responsibility assumed by the Board which adjusts and revises the compensation payable in these cases in Switzerland. The payment of specific benefits in this country, although they may in certain cases not follow the merits of the case closely, certainly simplifies greatly

the adjustment work. So far as those cases which do not fall into dismemberment or loss of use class, the problem of the Swiss actuaries is similar to that which we have at the present time in this country. It is interesting to note that they have not attempted to apply tabular valuations to temporary disability cases. Apparently a case of this class runs along upon temporary disability and at some time is assigned to the permanent disability class. Mr. Woodward does not state if this occurs automatically at the end of a certain period of duration of claim, as, for instance, one or two years. The decision of the Board handling the revision of these cases in Switzerland is a most important help in working out a valuation plan of this kind. The absence of similar authorities in all states in this country for revising the indeterminate claims will, I believe, prove a considerable difficulty in working out a plan of tabular valuation of outstanding workmen's compensation claims. It is clear that the tabular values used must be derived from cases handled in the same manner as the outstanding cases which are to be valued.

The only possible criticism of the method used by the Swiss actuaries in the valuation of outstanding claims which might occur to one is that the change from the present values of disability annuities used during the first three years, when the age of the annuitant is disregarded, following date of injury changes too abruptly from the third to the fourth years of payment. It would seem advisable to arrange the select valuation table by broad groups of ages so that the change from Table A to Table B would be less abrupt. For instance, if the annuitant is age 25 at the date of injury, the valuation factor at the end of the third year is 12.929, but at the end of the fourth year a factor of 16.020 is necessary, an increase of approximately 24 per cent. Then again if the claimant happened to be an old man, say age 55, the valuation factor would be increased approximately 30 per cent.

The use of select tables in the valuation of disability annuities, even disregarding ages of annuitants in the first years of disability, is in the opinion of the writer a wise plan. Taking this lesson from the Swiss work it would be advisable for us to have in mind and to utilize so far as possible in the valuation of outstanding claims the select table idea. For instance, in the valuation of compensation payments to a totally and permanently disabled worker undoubtedly a select table of mortality upon the disabled life should be used. Further, it would be advisable as soon as adequate experience is obtained to make up remarriage tables upon a select basis.

Bearing upon the remarriage rate, the writer agrees completely with Mr. Woodward in his concern regarding the proper remarriage rate to use in the valuation of annuities to dependents which involve this factor. In the valuation of outstanding claims to widows under the New York act the remarriage factor is predominant. The Dutch Royal Insurance Institution (1912) gives

the lowest rates of remarriage, but we should endeavor as soon as possible to obtain light upon the rate of remarriage among the widows of American workmen. This is particularly important in view of the social after effect of the war which will undoubtedly greatly reduce the rate of remarriage among widows.

MR. MARCUS MELTZER:

All who have had some part, however remote, in compensation rate-making will, I am sure, agree with me that we owe a debt of gratitude to Mr. Woodward for giving us such a clear and comprehensive description of the methods followed by the Swiss actuaries in establishing rates for the coverage of compensation under the insurance law of Switzerland. Our Society is fortunate indeed in having as a member one who is not only deeply interested in the solution of our compensation problems but who is so well qualified to study and interpret for our benefit the work of European actuaries.

If I am at all in a position to judge, Mr. Woodward's paper has already had far-reaching results. We all are cognizant of the general dissatisfaction with the hurried work of the last conference on rate revision. The exigencies of the situation required an immediate adjustment of compensation rates. There was no time nor sufficient statistical data available for a scientific treatment of some phases of the work, particularly the actuarial, though the need for improved methods was apparent to all. Certain definite proposals made at the time, which might be said to have anticipated certain features of the Swiss methods, could not be entertained for these reasons. The dissatisfaction grew, but there was lacking a coherent, organized movement to meet the situation. Of late, however, there has been noted a crystallization of effort along definite lines which is certain to eventuate in pronounced improvement in the actuarial methods, and this can be justly attributed in great part to the stimulus furnished by Mr. Woodward's paper.

Mr. Woodward has so admirably attained his aim in this presentation of the Swiss problem and its solution that there is no room left for comment on the subject matter.

However, in justice to the underwriters and actuaries who took part in the last rate revision, I should not pass over without criticism some of the author's comments which appear in the footnotes. To call our methods of computing pure premiums crude, and to make disparaging comments on the arrangement of classifications in our manual without at the same time pointing out how radically different the conditions are in the United States, unjustly places our actuaries in an unfavorable light.

The Swiss actuaries were dealing with a comparatively simple problem—the transformation of one set of rates into only one other. There were only 350 classifications to be dealt with. The Accident Insurance Institution of Switzerland represented one interest—the

state, a very important consideration. The statistical data, though incomplete in some respects, were representative of uniform conditions.

Let us contrast with this the extremely complex situation existing here. In the first place, our rate-making institution has been and still is a purely voluntary organization composed of at least four heterogeneous elements—stock companies, mutuals, state funds and insurance departments. Is it to be wondered at that competitive considerations, under these circumstances, are occasionally permitted to outweigh actuarial hypotheses, the soundness of which cannot be completely demonstrated. And how well it speaks for the scientific spirit which animated the work of the last conference if we consider that the participants were able to set aside their individual interests and work out rates by the best actuarial methods that could be developed with the scant material at hand.

Secondly, we have in our manual about 1,350 classifications as compared with 350 in Switzerland. This, as is well known, is a heritage of employers' liability insurance, and though cognizant that the legacy was not free from taint, the necessity for meeting the multitudinous other problems arising out of the rapid extension of compensation laws compelled us to accept it for the nonce. It might interest the members of the Society to learn that an informal conference which has been at work for the past year on regrouping, consolidation and elimination of manual classifications has recently completed its work and passed it on to the National Reference Committee for approval.

Finally, we have at least forty compensation states and as many different laws. At this late date no one will question the accepted principle that all of the available experience should be used in the establishment of basic rates. But since no two compensation states provide the same benefits, before the basic rates can be determined, the first problem, peculiarly our own, is to perfect the system of reducing the experience of the various states to a common level. Though I have the utmost faith in the ability of our actuaries, this problem in the very nature of it can be only approximately solved. Up to the present a single differential has been used in the reduction of losses. The use of partial or "sectional" differentials computed on the basis of the Standard Accident Table has been proposed. This may not meet the approbation of all as there is even now a strong contingent advocating the use of experience differentials. Suppose this particular problem has been worked out scientifically and to the satisfaction of all. Then arises the question of transforming the basic rates into state rates. Adopting the Swiss method, we determine the percentage distribution of the several elements of loss cost for each classification. We must now apply coefficients of transformation or sectional law differentials to each component of the loss in each classification, for each state separately. The immensity of this undertaking will be easily per-

ceived by any one who has had anything to do with the making of a manual. Even this objection might be waived. But practical considerations should make us hesitate to adopt this method. It will of necessity destroy the principle of the basic manual. Instead of one basic manual which is applicable to all states, insurance carriers will be compelled to issue a separate manual for each state. The slightest revision of a classification in whatever respect will require reprinting the page where it appears in each of the forty-odd manuals instead of one reprint for the basic manual. The resulting confusion and the excessive cost of printing and maintaining such manuals will have to be well considered.

It is extremely doubtful, moreover, whether the rates developed in this manner would be acceptable to all the states. We still have with us jurisdictions where the rate-supervising authorities adhere to the manual of rates established in the early days of compensation and who will permit of no modification except upon the evidence of experience accumulated in their own state, which in most cases is manifestly impossible. With them, actuarial science is hardly a criterion by which to judge the reasonableness or adequacy of rates, and no amount of actuarial theory or reasoning is likely to dislodge them from their attitude.

With this contrast before us, can it be reasonably expected that our compensation rate-making problems can be solved as readily as were those of Switzerland? Or that the use of similar methods will resolve them? I grant that the difficulties mentioned, though numerous, are not insurmountable. A great volume of experience is being accumulated involving an exposure of between eight and ten billion dollars. The Actuarial Section of the National Reference Committee should be able with this material to perfect the method of rate-making. And in this work the methods of the Swiss actuaries made known to us by Mr. Woodward will prove of signal aid.

MR. JOSEPH H. WOODWARD:

(AUTHOR'S REVIEW OF DISCUSSION.)

Mr. Flynn raised one or two points in his discussion which I think perhaps I can say something to further illuminate.

He spoke of the underwriting distinction made between the so-called small enterprises (namely, those employing ten or fewer workmen) and large enterprises. This distinction is illustrated by one or two concrete examples that I can give. Group 18 of the Swiss manual has the general description: "Small mechanical wood-working enterprises (up to ten workmen) without construction work." There are then several sub-classifications which come within the group. For example, sawmills, without accessory industries. The rate for degree of risk V, which is the average rate for small sawmills, is 100 francs per 1,000 francs payroll—equivalent

to our $10. The rate for degree of risk I, which represents the most favorable type of enterprise of this kind, is 50—which is one-half of the average; and the rate for degree of risk X, the highest rate quoted, is 140, about 40 per cent. higher than the average.

Turning to group 19 in the manual, the general description of the group is: "Large mechanical wood-working enterprises (with more than ten workmen) without construction work." There the average rate for sawmills is 70, which is thirty points under the average for small sawmills. For degree of risk I, which is the most favorable degree, the rate is 30 as compared with 50 for the small enterprise; for degree of risk X the rate is 110 as compared with 140. In general then, for wood-working enterprises, it is apparently the opinion of the Swiss actuaries that the rate should be higher for a small enterprise, disregarding all other considerations, and merely on account of its small size.

Mr. Flynn made the statement that he thought that probably it would appear, taking all things together, that the general results under the Swiss system and our system, after taking into account our schedule and experience rating, would not be so very different. Unfortunately, I did not give enough material in the paper to give a correct impression on this point. The probability appears to be that there is a considerably greater variation in rates among enterprises in a particular classification under this system than under our system in this country. Of course, I have no knowledge of the actual practice of the Swiss institution in applying these rates and actual practice is apt to differ from official promulgations. It may be that in operating a system of this kind there would be constant pressure to get risks rated under the superior degree of risk, and, consequently, there would be a good many more risks rated at the lower than the higher rates, so that the average rates would not be true averages. To offset that tendency, however, we should remember that the system is a monopolistic system; the effect of competition in business is not present, and the tendency would not be so great as it would be likely to be in this country.

Mr. Flynn also raised the question of the treatment of temporary cases—the adjustment of temporary cases. Again, I have no knowledge except from the deductions I was able to make from these publications as to what the actual practice may be, but apparently the general principle followed is to make the awards and adjust the claims for temporary cases that are under three years old substantially in the same way as we do and subject to review or revision at any time on request of the injured person, or at the instance of the insurance institution. After three years the rule provides that revision can only be had at the end of the sixth and ninth years, and from what I gathered, revised awards are compulsory at the end of six years and nine years. The cases come up automatically for scrutiny and revision at those times and at the end of nine years the award is made for life and not further disturbed.

Mr. Meltzer seems to think that the basic manual is endangered by some of the radical suggestions that are knocking about these days. Personally, I have felt for some time that the alleged advantages of the basic manual are to a considerable extent illusory and that there is no great objection to a separate manual for separate states in the sense that the rates in the manual need have no systematic mathematical relation from state to state. The advantage of such a relation is purely a matter of office convenience. While it is very important that there should be a complete uniformity of classifications between states, and every effort made to maintain it, yet so far as a uniform basis of rates between one state and another is concerned, my feeling is that in seeking such uniformity it is often impossible to avoid very considerable inequity between the various industries, and that equity should not be sacrificed to a slightly greater convenience in office methods. Where the payroll exposure in a classification is sufficient the rate for a particular state should reflect the experience in that state independent of the experience in other states.

NOTE ON THE CONSTRUCTION OF MORTALITY TABLES BY MEANS OF COMPOUND FREQUENCY CURVES—ARNE FISHER.

VOL. IV, PAGE 65.

WRITTEN DISCUSSION.

MR. EDWIN W. KOPF:

The general results achieved by Mr. Fisher in this paper are certainly as fascinating as any which have come from the laboratories of the modern analytic school in the physical and social sciences. Mr. Fisher's paper is in one sense a challenge. It calls for a statement of what sort of facts social statisticians are trying to discover, what limitations govern the use of such facts, and by what proper means we may bring data thus defined and circumscribed to bear upon the practical problems which face insurance science. Whether we work with the sense data of either the physical or the social sciences, we may agree, before we take up Mr. Fisher's paper in detail that there are four distinct categories of knowledge.

We have first the great mass of facts which we gain through direct observation, and this sort of data we term *empirical* knowledge. If we apply that process in the laws of thought which we call *induction,* by means of which we discover from observed facts the general principles or "law" respecting a given phenomenon, we arrive at the second category of our data, namely, *reasoned* or *generalized* knowledge. This reasoned or generalized knowledge oftentimes consists in bringing observed facts into accordance with others by means of certain reasonable assumptions or connecting hypotheses, which we hold to be acceptable and in accordance with sound common sense for such purposes. This second category of knowledge is perhaps the most advanced stage achieved by insurance and statistical science as we know it to-day. In fact, our known timidity in the employment of reasonable assumptions or hypotheses has on occasion resulted in that crude use of empirical data against which Mr. Fisher and a number of other analysts have so often protested in these *Proceedings.*

There is a third sort of knowledge, of which statisticians and actuaries have but rarely availed themselves—those statistical facts which may be *anticipated* by pure *deduction* from certain assumptions or hypotheses, subject to later verification by reference to empirical facts. The fourth and rather sublimated class of data which we could legitimately use in our everyday statistical work, had we the requisite courage for assumption, and the rigor of analysis, are

the facts which follow deductive analysis from hypotheses, but which are incapable of confirmation in the present feeble state of our technical resources.

Before considering the various elements of Mr. Fisher's paper, I shall ask you first to put away your impressions of the sufficiency or insufficiency of statistical data in the first category of knowledge, with its myriad forms, sheets, tables, pamphlets and volumes, and agree that instead of only one, or at most two, sorts or kinds of facts in insurance science, we may reasonably have four aids to understanding social phenomena.

Let us start off by saying that Mr. Fisher's note is an endeavor to contribute to the third category of legitimate knowledge in casualty and social insurance science, and that our criticism of the paper, if any, must be confined to testing whether Mr. Fisher has rigorously conformed to the criteria of this sort of knowledge or not. Our methods of criticism, which we usually apply to the facts and processes of the first two categories of data in insurance science, cannot help us very much in the present instance. Criticism in the third category of knowledge may be applied, first, to the hypothesis or hypotheses, and second, to the deductive processes, by means of which the several laws and conclusions are established.

Testing of Hypotheses in Insurance Science.

First, the hypotheses: The first few pages of Mr. Fisher's paper are a veritable network of hypotheses, some implied and some expressed, each one of which must be stated in the order in which it affects the entire groundwork of assumption which supports the deductive processes.

(*a.*) *The Implied Hypothesis as to the Nature of Social Statistical Facts. Does it Hold?*

There is first the implied hypothesis as to the *nature of the facts.* The statistical facts of social life are *developmental, dynamic,* or *genetic* in nature. They contrast with those data of the intensive method of research into laws governing particular insentient events or things, where the aim is to discover *constant* characteristics, regularities, recurrent forms, or *static* characters.

Data relating to social phenomena such as mortality, sickness, crime, poverty,—the functioning of society, etc., are developmental, dynamic—and are, therefore, so far as trustworthy observation informs us, decidedly variable, in respect to time, place and the constitution of the social group in which they arise. These phenomena have defied any well-considered effort to establish "types" apart from the severely limited circumstances which characterize a particular group. Professor Pearson in the lecture: "The Chances of Death," read before the Leeds Philosophical and Literary Society, January, 1895, asks us ". . . may we not assume that they (the

laws of frequency of apparently random sizes of things) are essentially the laws of all large numbers, and that even the frequency of death, its distribution by age, will obey the same laws?" The actual observation of deaths, with proper regard for special limiting conditions such as race, sex, occupational, age and other characteristics of populations, has shown all manner of departures from this assumption. The frequency of death, crime, and other social phenomena, is by no means of the same order of facts, i. e., as to whether of *dynamic* or *static* nature, as the frequencies recorded in coin-tossing, card-drawing, measuring fiddler-crabs or cephalic-indices of dolichocephalic German skulls.

Recall the reaction against the mathematical school of social philosophers, which began as far back as 1849. Quetelet had popularized Laplace and Fourier in considerable measure and had been in turn introduced to the English-speaking world through Sir John Herschel's efforts. In his treatise "Sur l'Homme et le Développement de ses Facultés, ou Essai de Physique Sociale," Quetelet had interpreted certain statistics of crime, suicide and disease as revealed by the data of the criminal courts and registration offices of France, The Netherlands and other countries to mean "... we shall be able to *fix* the laws to which Man has been subjected in different nations since their birth—that is to say, we shall be able to follow the centers of gravity of the (social) system." The influence of Laplace is evident throughout the work, especially when he speaks of the "stability of the social system" and compares the new science of society to the mechanics of the Heavens.* The philosophers Drobisch and Lotze first suggested, followed by Rehnisch, that this view was indeed premature, because of the disagreement between the requirements of sound hypothesis and the *nature* of such "knowledge" as: "Society, as it were, exacts a certain proportion of crime as it does of suicide, poverty, physical and mental disease." Mr. Fisher mentions, for instance, that Lexis had analyzed by means of his dispersion theory the extreme right part of the d_x curve of mortality as a "normal" curve with a maximum and mean in the neighborhood of age 70. Dispersion theory or no dispersion theory, Professor Pearson reports having applied this assumption to French data with but moderate success and to English statistics for males with complete failure. No hypothesis in social statistics has ever been proven in nearly a century of controversy to apply beyond the special group, limited to the time and place, to which it relates. The supreme and sufficient test of a hypothesis is whether or not it agrees with observed facts.

Mr. Fisher's first implied hypothesis is that the facts of mortality according to the causes of death are of such *static* nature that we may "analyze the series of deaths at various ages in a system of 8 (or 10) Laplacean-Charlier and Poisson-Charlier frequency curves, *typical* of distinctive groups of causes of death at various stages of life."

* Vol. II, p. 338, "Sur l'Homme, etc."

The lesson of all the eighty years of controversy and criticism of the school of social mechanics is that a supposition of *static* characters—*in material essentially dynamic* and variable according to time, place and social group—is contrary to observed facts. This objection holds, I believe, that the hypotheses underlying Mr. Fisher's deductive analysis are not general in application but must be, according to experience in social statistics, limited to groups of *similar sex, age structure, race and other constitution.* The apparently close agreement of Mr. Fisher's q_x values with those of Professor Glover is probably due to two factors: (1) the practically similar age constitution of males in the 1909–1911 data for the Original Registration States and in the 1910 data for the total Registration Area and (2) the correspondence of high values of $R_B(x)$ with high values of m_x and of low values of $R_B'(x)$ with low values of m_x for certain of the disease groups which weigh heavily in the entire experience. The form and other analytic characters of the curves for the several diseases and conditions may be typical so far as the general population of the registration area, males, of 1910 is concerned. The hypothesis is not sustained, however, by the facts of experience with similar endeavors of the mathematical school of social philosophers, when the attempt is made to employ it in constructing a mortality table for, say, locomotive engineers, textile operatives, males in Newark, New Jersey or in Boston, Massachusetts. The statistical parameters of the d_x column compiled from exposures and deaths for each of these groups may be expected from experience with similar situations in other subjects of social inquiry, to be essentially unlike.

I am not convinced that a mortality table can be constructed validly by means of compound frequency curves without complete reference to the social group to which it applies. I do believe, though, that when crude life table values have been prepared upon the basis of observed exposures and deaths, the compound frequency curve method can be justified as a powerful agent for graduation by sound hypotheses of special application to a particular social group only. Observe, for instance, the smooth character of Mr. Fisher's registration area curve in comparison with Professor Glover's values, graduated by a number of mechanical formulae.

(*b.*) *The Expressed Hypotheses.*

The other hypotheses forming the groundwork for the analytic procedure may be examined regardless of the lack of generality of the implied hypothesis as to the nature of the facts. I have not yet had the opportunity to test by practical calculation from data drawn from various social groups, whether the assumption holds that frequency curves of Types A and B are sufficient to represent the distribution of d_x values for the several diseases and conditions in a life table constructed according to conventional methods.

Limitation of such variable data to two type curves seems to be a somewhat Procrustean procedure. It may be, though, that the use of two types is a commonsense middle-of-the-road measure between a system of many types of curves and the single class of Pearsonian curves derived from the differential equation

$$\frac{1}{y}\cdot\frac{dy}{dx}=\frac{bx-x^2}{a-bx-cx^2},$$

where x represents the magnitude of a given deviation from the mean of a series of measures and y the frequency of such deviation.

Nor have I had the time to apply any tests to Mr. Fisher's grouping of the causes of death to form classes of deaths *typical* of youth, etc. There is a very considerable amount of labor attached to the trying-out process. Mr. Fisher assures me that the work is likely to provide good mental exercise before I discover just what causes of death he has merged in the several groups! Likewise the tests for "goodness of fit" and for errors due to sampling in the figures for locomotive engineers.

There is much in Mr. Fisher's paper that supports the plea for more contributions to the third sort of knowledge in insurance science—the knowledge based upon valid hypotheses, faithful analysis and final reference to actual experience. Results along this line in future will probably be as convincing as James Clerk-Maxwell's exhaustive statistical treatment of the kinetic theory of gases. Clerk-Maxwell's application of the theory of probabilities to the phenomena of gaseous substance placed upon a sound theoretical foundation the laws empirically established by Boyle, Gay-Lussac, Dalton and Avogadro. In insurance science we, too, may some day see harmony between the advocates of the first and third schools of knowledge—that harmony which comes of mutual understanding and helpful criticism.

MR. ALBERT H. MOWBRAY:

This paper presents a new method of constructing mortality tables from consideration of deaths only and causes of death. This proposal is indeed startling to most of us in view of our general training that a correct mortality table cannot be constructed without having regard to the exposures to risk, and we are confirmed in this view from our custom of comparing the deaths and exposures as the equivalent of the occurrences and possibilities of the probability fraction. The fact that it violates our present notions is, however, not valid ground for rejection of the method if it is not found unsound in theory and gives satisfactory results in practice. The fundamental hypothesis of the method is "that the numbers of deaths from specific causes cluster around certain definite ages in such a manner that the frequency distribution according to age from a specified cause or group of causes of death may be represented by a typical skew frequency curve."

Although it is not taken directly, it seems to me that indirectly through this hypothesis an hypothetical exposed to risk is set up from which the probability fraction is derived. Of course, the correctness or incorrectness of the theory hinges upon the truth of this hypothesis.

The examples worked out in Mr. Fisher's paper indicate that the method in skillful hands tends to produce satisfactory results and to the extent of the examples tried indicates the fundamental hypothesis is true. Before this is definitely accepted further practical tests of the hypothesis than those here presented will be necessary. For myself I may say that the fundamental hypothesis seems reasonable.

There are certain advantages which might result from the general adoption of the method here suggested for construction of mortality tables. The volume of data to be considered would be considerably reduced. This would facilitate the taking out of experience and probably speed the time in which the work could be done, as it would not be necessary to handle a large volume of cards relating to exposures which it is now necessary to handle. The method requires the analysis of the fatalities by cause and age, and it is my opinion that careful studies along these lines would be most helpful in increasing our knowledge and probably our ability to control disease. This analysis of cause of death especially in comparison with the elapsed time since the medical examination may throw a great deal of light on the mooted question of the duration of effect of medical selection. The method produces a mortality table that is already graduated and, therefore, the work of graduation it not added to the construction of the table and we avoid the possibility of distortion of results through the use of a graduation formula which may not be entirely suited to the data.

On the other hand the method presents certain difficulties which are by no means insignificant. In the first place it would appear that if cause of death is to be given the importance this method gives it in determination of mortality tables the diagnosis should be very accurate, and the statistical assembly should correspond to this accuracy of diagnosis. Equally careful mathematical analysis is also fundamental. Even upon the basis of these conditions it appears the method leaves a large amount of leeway for the personal equation in determining what causes shall be grouped together in the various frequency curves. It is true, as I understand it, that these curves will be subject to careful tests for stability, and yet, as Mr. Fisher points out, he, himself, is not entirely satisfied with the cause analysis he has made in this way. This cause analysis has been made where it has been possible to compare the material with carefully compiled mortality tables prepared by the methods heretofore used of comparing deaths with the exposures. After the particular frequency curves have been selected there is still some room for the influence of the personal equation in the

selection of constants, but the method of least squares, and other methods of mathematical analysis are probably sufficiently well developed to eliminate any pronounced error from this source. It may be that as we become better trained in the modern mathematical methods of statistical analysis we will be no more afraid of the personal equation affecting the application of this method than we are now of a like influence in graduation by the Makeham formula.

I sincerely hope that Mr. Fisher's method may receive careful consideration and adequate test through application to the construction of mortality tables in this country. Although it is a matter in which this Society is not concerned, it would seem that in the construction of the new mortality table now under way jointly by the Convention of Insurance Commissioners and the actuarial bodies of this country dealing with life insurance there would be an excellent opportunity to test out these methods.

I cannot but regret the implication of Mr. Fisher's remarks at the top of page 84. It may be that "Purely empirical methods . . . have been employed by far too great an extent by actuaries and statisticians." And it may also be true that this is "unfortunately at the loss of sound logic and commonsense reasoning." But human nature is human nature and when a man or a profession has made the best use of the tools it has hitherto had available, there is a very natural resentment at the use of such language when a new method is first put forward and when the profession has had no opportunity to make an adequate study of the new method. Perhaps the actuarial profession of America may be subject to some criticism for not having pursued its probability studies to the point where it has developed these methods, but if the profession is open to this indictment it at least has the defense that in the meantime it has built up on a sound foundation the present huge structure of American life insurance and extended its benefits to a vast number within our population.

MR. JOHN S. THOMPSON:

This recent paper of Mr. Fisher's concerning a proposed new method of analysis of mortality experience is an unusually interesting one and the mode of treatment is very suggestive, especially since the course described is a radical departure from that hitherto taken by actuaries when confronted with the problem of deriving a rate of mortality from a suitable experience.

In case of the treatment of the "Locomotive Engineers" experience there is satisfactory agreement between "actual" and "expected" deaths or between the "observed" and "calculated" deaths (frequency curve ordinates) as they are here called, so far as totals are concerned. Similar figures are not available for the work on the U. S. Life Tables, but the tabulated results indicate that the "fit" is equally good. So far as the distribution among

ages go the results cannot be said to be as good. For example, in case of the "Locomotive Engineers" experience, the two largest groups are C—D and H; in case of the latter the observed deaths are less than the calculated deaths up to age 39 inclusive, greater from 40 to 64 inclusive, and less thereafter. Under group C—D the observed deaths exceed the calculated deaths to age 49 inclusive and are less thereafter. Thus the most obvious test of a satisfactory graduation is not fulfilled but, of course, the extent of the series may be such that better results could not be expected. Indeed the percentage deviation is not great, being very little in excess of 1 per cent. in the total and there is no doubt that a high degree of statistical skill has been applied to the process described in the paper.

In brief, the method is to subdivide the totals of actual deaths classified by ages into corresponding groups by causes of death also classified by ages, the idea being that the subgroups can be more readily graduated by manageable types of frequency curves than can the whole series giving the deaths from all causes at successive ages. If we understand the process, therefore, the result is simply a graduation of d_x, the "actual" deaths, and it is not apparent why a mortality table should not be formed from the unadjusted deaths and some other function of graduation with equally good results. In other words, it is not clear what additional knowledge is furnished by subdividing the deaths by causes and how the excellence of statistical treatment compensates for lack of information covering "exposed to risk." Every actuary is familiar with the unfavorable, not to say disastrous results following upon the attempt to construct the Northampton Table from the record of deaths only. It is conceivable that the numbers of deaths either in total or by separate causes may be the same in two distributions when the "exposed to risk" are different. It is also certain that the rate of mortality in a stationary population is different from that in one in connection with which there is a regular net increment or decrement annually because of migration if the deaths are the same. If there is close adherence between rates of mortality deduced in the ordinary way and those deduced in the proposed way, it can only be because the net variation of the distribution from the stationary condition is not great. Moreover, the high rates of mortality at the very young and very old ages will, as usual, present greater difficulties of treatment because a given rate of variation in the exposures will produce a greater absolute variation in the deduced rate of mortality than at intermediate ages.

Statements regarding "Cause of Death" are considerably affected by errors arising from either (1) looseness of nomenclature, that is, the use of general instead of specific terms, confusion between immediate and contributory causes, etc., or (2) incorrect diagnoses. The execution of the physician's certificate as part of the proof of death may be carried out hurriedly or without the ex-

ercise of the usual professional exactitude and without the realization of the statistical value of the statement. The error, if any, results in the transfer of deaths from one class to another and if the net movement is not great the result or deduced rates will, of course, not be great. But it is reasonable to suppose that the rate of mortality based on the fact of death and the fact of exposure must be less affected by error than a calculation based on the cause of death, changes due to misstatement of ages, to statistical treatment and to the exercise of judgment arising in both.

The process described offers an excellent means of studying the mortality from a given disease or group of diseases (the deaths being more or less subdivided according to the requirements), provided the assumed frequency curve can be made to fit and the exposures can be obtained. There is no doubt that co-operation between the medical director and the actuarial statistician will result in a broader understanding of the incidence and destructive effect of various types of disease.

MR. CHARLES W. JACKSON:

Mr. Fisher's paper is very interesting and the method proposed by him a novel one and a wide departure from the methods customarily pursued by actuaries.

Before accepting Mr. Fisher's hypothesis, however, it will be necessary to submit it to many more tests than those to which it has been subjected at present. While the examples which are given in the paper appear to bear out the truth of the theory, I do not think they are sufficient to justify us in accepting it as established. We know that in the past, many an ingenious hypothesis has appeared to account for various observed facts, only to be overthrown and discredited later.

As stated above, it will be necessary to test it on several experiences, but here practical difficulties arise. Mr. Fisher on page 67 divided the experience into eight typical groups, namely:

B: Deaths typical of youth,
C: Deaths from industrial accident,
D: Deaths from pulmonary tuberculosis, etc.,
E: Deaths typical of middle life,
F: Deaths typical of late middle life,
G: Deaths typical of early old age,
H: Deaths typical of middle old age,
I: Deaths typical of extreme old age.

In the latter portion of the paper, he increases the number of the groups to ten and thus obtains better results. The author has not furnished us with sufficient data to test out his hypothesis on other experiences, for I do not think that any two men would agree as to the deaths which properly belong to groups E, F and G.

Mr. Fisher would be rendering a valuable service to the members

if he would go into greater detail and enumerate more specifically the nature of the different groups. Unless this is done, it is practically impossible to test the truth of the hypothesis, for the time required would be prohibitive.

In constructing tables based upon similar methods, Mr. Pedersen, I believe, made use of four groups; Mr. Pearson of five; Mr. Fisher of eight and later of ten. The question naturally arises whether the last number is sufficient and if it may not prove necessary to increase the number of groups still more.

It would be of great value to all of us to know whether the hypothesis is true or not, for if mortality tables can be constructed from the record of deaths alone, an immense amount of time can be saved, for in the investigation of any mortality experience, by far the greater part of the time of the clerical force is used in calculating the exposed to risk. If we can avoid this labor, a great saving will be effected.

We are greatly indebted to Mr. Fisher for his very interesting paper, which in my opinion would be much more valuable if it contained more detail.

MR. ARNE FISHER:

(AUTHOR'S REVIEW OF DISCUSSION.)

The remarks on my little note by the recent speakers are a striking illustration of the back-wash of a fierce conflict between two different currents of philosophical thought and, scientific method, the battle between the empirical and rationalistic schools of philosophy, represented by the Scotch and English philosophers, as Berkeley, Locke and Hume on the one hand and the continental philosophers, Descartes and Leibnitz on the other hand.

The empiricists maintained that all our knowledge is derived from experience, the rationalists claimed that in addition to what we know by experience, there are "innate ideas" or principles which we know independently of experience. The empiricists denied moreover the existence of logical principles independent of experience. After many years of thought this denial and probably the most important point of the controversy between the two schools has proven to be false, and it has been established that the rationalists were in the right. On the other hand, the empiricists were right in maintaining that nothing can be known to exist except by the help of experience.

Emmanual Kant and Herbert Spencer have attempted to reconcile the two views, and the methods of modern science and scientific research have to a certain extent effected a compromise between the two schools, although somewhat favoring the rationalists. The absolute impossibility to derive the corpus of human knowledge from mere empirical data and observations has been so ably demonstrated that no further proof is required on this particular

point. Yet many of our actuaries of the present day, especially of the Scotch school, seem still to believe in the fallacy perpetrated by John Stuart Mill in his attempt to found a "new logic" and to establish the law of causailty by what he himself termed "an inductio per simplicem enumerationem." This is blind empiricism, almost as narrow in its scope as that of St. Thomas, the apostle, who only would acknowledge the existence of things through immediate sensations. Bias of this or similar character is not uncommon among most Scotch collectors of statistical data, such as actuaries and medical men. I need only to refer to a recent statement of Professor Karl Pearson who informs us how he once pointed out to a Scotch actuary or statistician his prejudice in favor of whole centimeters relating to a series of measurements of human anatomy. The Scot looked at the results he had collected, recognized the bias, and then gravely told Mr. Pearson that it was not due to any personal bias, but that the creator must have designed Scotsmen on the metric scale. It is such paradoxes—more or less due to picayune empiricism—that makes the individual Scot or Irishman so dear to our heart and a source and veritable fountain of wit and humor, but also at the same time tends to make a collection of such individual characters unduly clannish. Every clansman, I am sure, is essentially an empiricist.

In total opposition to our Scotch friends stands the typical German. Most of us have wondered at the often queer antics of the German mind. These antics I feel sure are the outcome of a consistent and stubborn rationalism, ignoring experience. German logic in itself, as a mere formal logic apart from psychological logic, is keen enough, but it is in most cases founded upon a completely wrong hypothesis. In spite of this shortcoming the extreme rationalism of the German mind has in this war proven a tremendous power, although a power for evil only. Happily most normal individuals such as Americans, Englishmen, Frenchmen, Latins and Scandinavians occupy a position between the two extremes of the Scot actuary and German pedagogue.

The philosophical model of modern scientific thought and research work is characteristic of what Jevons called a complete process of induction consisting of four stages, viz., *observation, hypothesis, deduction* and *verification.*

The distinguishsd Italian physician and philosophical critic, Enrico Marselli, has crystallized the content of the philosophy of science in the following extracts taken from the introduction to his "Review of Scientific Philosophy."

"We think the moment has come for professional philosophers to allow themselves to be convinced that the progress of physical and biological sciences has profoundly changed the tendencies of philosophy; so that it is no longer an assemblage of speculative systems, but rather the synthesis of partial scientific doctrines, the expression of the highest general truths, derived *solely* and *imme-*

diately from the study of facts. On the other hand, we hope also that in every student of the separate sciences whether pure or applied, the intimate conviction will take root that no science which applies the method of observation and experiment to the particular class of phenomena which form its subject, can call itself fully developed so long as it is limited to the collection and classification of facts. Scientific dillettantism of this sort must end by sterilizing the human mind, whose natural tendency is to advance from observed phenomena by successive stages to the investigation of their partial laws and from these to the research of more and general truths. But philosophy thus understood, can never confine itself with the dogmatism of a system but rather will leave the individual mind free to make constant new concessions, in the pursuit of the truth."

The same ideas are expressed in even more trenchant form by another Italian, Federigo Enriques, in his "Problemi della Sciensa" ("Problems in Science") as follows:

"To-day they claim that by reasoning we can penetrate the secrets of the universe, which should be mysteriously revealed in the laws of the mind is banished as a chimera. But on the other hand we see that all observations and experiments are of scientific value only so far as they are supported by a reasoning process. Otherwise we should be obliged to wait until nature should be so obliging as to teach us, by answering at random these questions which we should neither know how to ask nor to interpret."

"Watchful waiting" may have its value in diplomacy and politics, but it has no place in science. The whole history of science from the dawn of antiquity to our present day has proven that the human mind is aggressive and does not wait patiently on being taught by experience but hastens ahead with its hypothesis and methods.

But have the so-called "actuarial science" and actuaries in general not exactly adopted such a policy of watchful waiting?

Many people are in the habit to look upon an actuary as a mathematical wizard, a sort of little deity, or oracle, whose authority is unquestioned. For a number of years I held this view myself, but so far as I can judge from recent contributions to assurance mathematics and assurance statistics I feel that most actuaries have been completely dominated by narrow empiricism and that their rather slavish adherence to certain patterned methods of gathering and cataloguing facts have probably had the effect of what Morselli called the sterilization of the mind. As a rather youthful and so to say unknown quantity in statistical and actuarial circles I fully realize that such a sweeping assertion in respect to a body of eminent men can not stand on its face value unless it is supported by historical evidence, which I now shall endeavor to bring forth.

The name of the celebrated Edmund Halley, Astronomer Royal of England, is remembered for two achievements which he was the

first to accomplish: the computation of a comet's orbit and the construction of the first mortality table. Halley thus represented and actually founded one of two at his time essentially empirical branches of human knowledge, astronomy and actuarial methods, none of which at that time (about 1690) could lay claim to the title of a part of science. It is of interest to see how the development of these two branches has been since the time of Halley and how their respective progress compare at the present time.

The originator of the Breslau Life Table was a contemporary of the great Newton who through the development of that particular part of mathematical analysis, known as the infinitesimal calculus, discovered the key that opened the doors for the exploration of the sciences of astronomy and physics and actually became the mathematical foundation of these two sciences.

The genius of Newton was far seeing enough to break with the traditional views of mere empiricism then reigning in astronomy and physics. Like a true rationalist, perhaps the finest type of positive rationalism in the whole history of mankind, he hastened ahead with hypotheses and opened new fields of investigation for the astronomers and physicists. His fertile genius left the sterilized minds of the empirical observers to make additional observations and to gather further data. What was the result? A tremendous development of astronomical and physical sciences. Gallic genius under the leadership of Lagrange, D'Alembert, Legendre, Cauchy, and especially Laplace, took up the heritage of the great Englishman and perfected the splendid structure to which he had laid the foundation. The genius of Laplace is fully equal to that of Newton and shows again the superiority of fertile rationalism over sterile empiricism. In the "Mechanique Celeste" the riddle of the stellar bodies was laid bare by the methods of a rationalistic mind and the empiricist had only to verify by additional observations the results which Laplace had deduced by a mathematical analysis. Astronomy is to-day, thanks to Laplace, one of the most perfect among the exact sciences. It has torn itself away from the clutches of empiricism, which now is its servant instead of its master.

Such has been the development of astronomical and physical sciences since the time of Halley. From mere empirical methods they were elevated to a theory by Newton and perfected into exact sciences by the French mathematicians. Can the same be said to be the case with the actuarial methods as introduced by Halley? I am well aware of the fact that most actuaries are in the habit of speaking of "actuarial science." So false and misleading is this name that even those who more modestly speak of "actuarial theory" are wrong, since all what actuaries can justly speak of are actuarial methods, purely empirical in their conception and application. The mentioning of actuarial science and even of actuarial theory must at the present state of development be regarded as mere phrases.

Have the principles as introduced by Halley in the study of mortality essentially changed from their purely empirical character? I fear I will have to answer this question in the negative. Dr. Milne in the construction of the Carlisle Table took into account the exposed to risk, which were unknown to Halley, but otherwise his method was almost identical to that of Halley. Since the time of Milne a number of ways have been devised to graduate the irregularities of the crude observed data. I can mention you at least twenty-five interpolation or summation formulas used in graduation, and all of which really are special cases of the general Newtonian Interpolation Formula.

Gompertz's and Makeham's hypothesis constitute an attempt to base the investigations on human mortality on a more scientific basis, but unfortunately the Makeham formula can at its best only be called a successful graduation formula.

Let us briefly consider the fundamental features of constructing a mortality table by means of the usual actuarial methods. A certain number, say l_x, persons at age x, are kept under observation for a full calendar year and the number d_x who die among the original l_x entrants during the same year are recorded. The ratio $d_x \div l_x$, is then considered as the crude probability of dying at age x. Similar crude rates are then obtained for all other ages. These crude rates are then subjected to a more or less empirical process of graduation to smoothen out the observed irregularities arising from random sampling. One then chooses an arbitrary radix, say 100,000 persons at age 10, which represents the original cohort of 10-year-old children entering under our observation. This radix is now multiplied by the previously constructed value of q_{10}, and the product represents the number dying at age 10. This number, d_{10}, is subtracted from 10,000 and the difference is the number living at age 11, or l_{11}. This latter number is then multiplied by q_{11} and the result is d_{11}, or the number dying at the age of 11 out of the original cohort of 10,000. In this way one continues for all ages up to 105 or so. The d_x column—or the compound frequency curve—is thus an auxiliary column, a mere by-product of the empirically determined q_x.

Allow me to ask you a simple question. Do these empirically derived numbers of deaths at various ages out of an original cohort of 10,000 entrants at age 10 give us any insight or clue as to the exact nature of the biological phenomenon known as death, and are we by this method enabled to lift the veil and trace the numerous causes which must have been at work and served to produce the total effect, the d_x curve, of which we through the usual conventional methods have a purely empirical representation?

I fear we will have to answer this question in the negative. The usual actuarial methods do not give us a single glance into the relation between cause and effect, which after all is the ultimate object of investigation for all real sciences. Probably many of

my critics would answer that they are not interested in investigating causal relations. An ignoring attitude like this is, however, very dangerous for a statistician, whose very work rests upon the validity of the law of causality.

We shall, however, overlook this evident inconsistency of the empiricists and instead turn our attention to such methods as a rationalistic positivist would employ were he to construct a mortality table.

Such methods we should find to be completely reversed to those of the empiricists, both in respect to points of attack and deduction. In the case of the empiricist the q_x is the initial fundamental function from which the d_x column is computed as a mere by-product. The rationalistic method starts with the d_x column and winds up with the q_x column as a by-product.

The positivist investigator is thus primarily interested in the absolute number of deaths and not in the relative frequencies of deaths at various ages. His very first question is therefore: What is the form of the curve representing the deaths at various ages among the survivors of the original cohort of 10,000? Right here we can, strange to say, apply some purely a priori knowledge. We know a priori that the curve must be finite in extent and assumes only positive values. There can be no negative numbers of deaths unless you were to regard the reported theological miracles of resurrections from the Jewish-Christian religion as such. This information is, however, not sufficient to use as a basis for a purely deductive analysis. We must, therefore, look about for additional information whether of an a priori or an a posteriori character and of such a *general character that it can be adopted as a hypothesis.*

As Poincare once said, every generalization is a hypothesis. Hence we shall look for some *general* characteristics which *all* mortality tables have in common in the age interval under consideration (age 10 and upwards). Now if you take any mortality table, I do not care from what part of the world, you will notice that the numbers of deaths in the d_x column gradually increase from age 10 and until a certain maximum or high crest is reached. After that the numbers begin to decrease quite rapidly, until at age 100 or so only a few numbers are found among the deaths. This high crest with its subsequent rapid decline is found in all existing mortality tables. It does of course appear at various age periods for various localities and populations. In India the high crest for male lives occurs at the age period, 45–50, among most of the Aryan races it falls around the age of 70. The main fact is, however, that this crest is a general characteristic of all mortality tables. We can, therefore, see that the d_x curve is a single valued, positive function with at least one maximum value gradually diminishing towards youth and old age. Such a curve has all the properties of a frequency curve which—as proven by Charlier and Jörgensen—can be represented as the sum of Laplacean-Charlier and Poisson-Charlier

frequency curves of types A and B. So far we have been on perfectly firm ground. We know positively the compound curve is composed of A and B curves. But how shall we construct these separate component curves? No a priori reason will guide us so we must resort to a hypothesis. Now each cause or group of causes of deaths has certain typical characteristics as to its occurrences in various age periods of life. We know for instance that there is a much greater probability that a boy of 5 years will die from measles than that an 85-year-old man will die from measles. On the other hand there is quite a large probability that an 85-year-old man will die from diseases of the prostate gland, while such an occurrence is almost unheard of among younger ages. Similarly deaths from cancer and Bright's disease are very rare in youth but quite frequent in older ages.

Now since we know that the d_x curve is composed of A and B curves and that the probability of death from certain diseases are typical of various age groups it lies close at hand to adopt as a hypothesis that the distribution of deaths from certain causes among the survivors at various ages of the original cohort can be represented by A or B curves. I do not need to state here why such curves necessarily must be skew in appearance as far as the clustering tendency around the mean value is concerned.

The hypothesis does not imply that the curves are fixed or static. Mathematical statistical methods are essentially methods devised to study the dynamic changes in life where a rigid state as known from the theory of static bodies does not exist. Without doubt the curves would group themselves around different ages, in the tables for India than in Europe or America. I have, however, found that a number of statistical data relating to causes of death may be represented by the system of ten curves as given in the examples in the paper, especially in view of the fact that I by this time have obtained better equations for the older age curves.* I have constructed male life tables for England and Wales, Newark, Boston, Detroit and Copenhagen. The English table is especially instructive because we have here a complete registration in the whole country for the years of 1911 and 1912 for which the table was constructed. Moreover, we are able to compare the table thus formed with the table known as English Life Table, No. 8, as constructed

*Another improvement is that the parameters as originally computed have been corrected by means of the "Sheppard Correction Formula," thus allowing for errors of grouping in 5 year intervals. Moreover, it was discovered that there among the deaths in the younger age periods, say up to the age of 50, among the Locomotive Engineers were included a number of permanent disabilities from blindness and amputations of legs and arms. The mortality rate is therefore too high in the younger ages and middle life. A monograph on the mortality among Locomotive Engineers is at present being prepared by Mr. F. S. Crum and will contain the new and corrected frequency curves.

by Mr. King by the usual methods from the deaths by age and the exposed to risk at the same age. The close agreement between the values of q_x is shown in the following table and diagram.

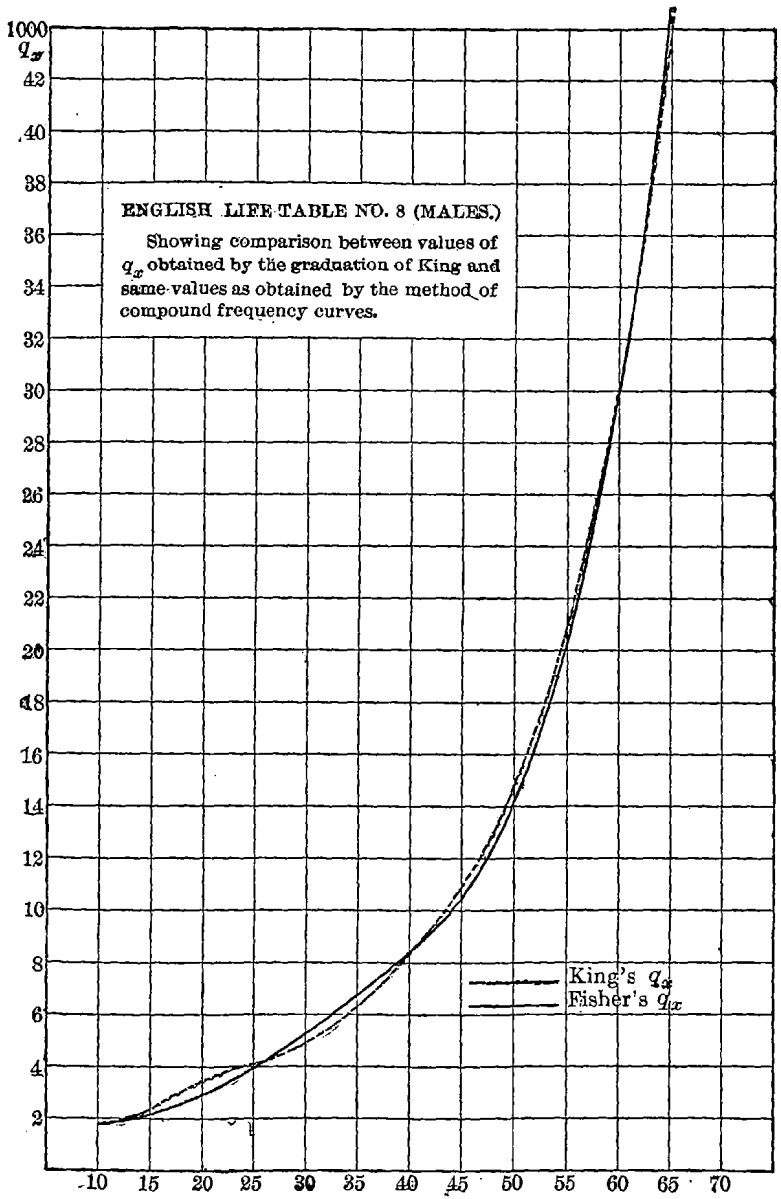

TABLE I.

Comparison between values of 1,000 q_x of English Life Table No. 8 (Males) as constructed by Mr. George King's method of graduation and corresponding values of q_x as computed by Mr. Arne Fisher's method, of compound frequency curves from the mortuary records by age, cause of death and sex of the Register General for England and Wales 1911–1912.

Age.	King's 1,000 q_x.	Fisher's 1,000 q_x
10	1.93	1.79
15	2.35	1.99
20	3.48	2.98
25	3.99	3.93
30	4.78	5.34
35	6.24	6.82
40	8.11	8.41
45	10.89	10.50
50	14.82	14.08
55	21.11	20.41
60	30.42	30.64
65	43.75	44.75
70	64.70	63.08
75	97.51	90.52
80	142.99	133.33
85	199.11	187.95
90	273.95	238.01
95	315.64	348.38
100	416.04	386.84

On the other hand, the data from New York City completely defy any attempt to use the system of ten curves. This is due to a certain reason and a very interesting one indeed, but one which I shall not discuss at this moment.

The successful construction of a mortality table from the absolutely independent material from England and Wales apparently disposes of the criticisms of Mr. Kopf as to the influence of the age distribution of the exposed to risk. This latter distribution is namely quite different in England than in the United States where emigration of younger members makes itself strongly felt. Moreover Mr. Kopf seems to be totally ignorant of the origin and generation of compound frequency curves when he makes the following remark:

"The frequency of death, crime, and other social phenomena, is by no means of the same order of facts, *i. e.*, as to whether of *dynamic* or *static* nature, as the frequencies recorded in coin tossing, card drawing, measuring fiddler crabs or cephalic—indices of doliochocephalic German skulls."

Leaving the German skulls to be taken care of by the bombing squadrons of the Allied airmen and aviators, the first part of this

statement is absolutely erroneous. In regard to the distinction between static and dynamic facts on which Mr. Kopf bestows such great care, I simply wish to state that in all statistical records a static state is unknown. All statisticians deal with variation and the mathematical relation of variates, hence their methods must bear a close relation to the methods of mathematical dynamics. Again, if Mr. Kopf will go to the trouble to investigate the frequency distribution of the frontal breadths of the crab, *Carcinus mœnas,* as measured by Weldon and subsequently analyzed by Pearson as a compound frequency curve with two normal components, he will find that this compound frequency curve is almost identical with the d_x curve of the mortality table. So far as coin tossings or card drawings are concerned I can assure Mr. Kopf that I, from records of such tossings or drawings, can produce a compound frequency curve of exactly the same form as a graduated d_x curve of any mortality table, if he will be obliging enough to make a million individual drawings of cards or differently colored balls from separate urns, such drawings being properly arranged in 8 or 10 sets of simple Lexian series with varying probability from set to set. Taken as a whole, Mr. Kopf's remarks seem to me nothing more than mere verbalisms and vague generalities. I therefore probably might be pardoned for saying that Mr. Kopf's beliefs and doubts are not my reasons.

Let me in conclusion say this: I am a believer in rationalistic positivism in all questions of science and although I by no means wish to underestimate the value of purely negative criticism, I, as a rule, only use criticism in its positive sense.

I fear that the so-called actuarial science of the late years has reached a state of stagnation. The attitude of "watchful waiting" I spoke of before has from a mere habit become a real menace. Mechanical aids of various kinds such as Hollerith and Powers tabulators have so facilitated the mere arithmetical processes of collections and tabulations that we do not need so much as before the introduction of new methods to eliminate the excessive arithmetical work of the old days. This mechanical aid has, however, so dulled the brains of many of our actuaries that they often work as mere mechanical adjuncts of the Hollerith machines, a sort of a superior kind of routine sorter. The mind of man is, however, too worthy a product of progressive evolution that it should be relegated to such a routine state of bookkeeping and calculating. Human thought was by its power of reasoning among the old Greeks justly considered as a divine gift. The ability of man to produce various hypotheses has widened the realm of science and brought modern research away from narrow empiricism. Who among you will deny the power of deductive reasoning as based upon hypothesis when it has the power to arouse debate, when it instigates the utilization of collected statistics which hitherto have been of little value and emphasizes new distinctions of facts, whose objective value, although not impeachable, make us view certain phenomena in a new light?

SOME ESSENTIALS OF SICKNESS STATISTICS—EDWIN W. KOPF.

VOL. IV, PAGE 107.

WRITTEN DISCUSSION.

MR. ALBERT H. MOWBRAY:

This is the first part of a chapter in a text-book which will prove a very valuable manual for students of casualty and social insurance. This part deals with the statistical *description* of sickness, or the collection, editing and tabulation of crude data as distinguished from the critical analysis of tabulated material. The paper in itself will be valuable not only for its own suggestions, but for copious references to the works of others. The writer seems to have taken two propositions as his text, one of which appears in the second paragraph of the paper, "We, in America, must insist upon an impartial thorough search for the facts of sickness if we would build a durable insurance and public health structure," and the other at the top of page 122, "The aim of sickness statistics should be the accurate extensive portrayal of sickness in its fourfold aspects."

The writer does not make it entirely clear what he means by the fourfold aspects of sickness, but it would appear that he has in mind his remark on the preceding page, "Most cases of serious sickness require (*a*) medical, nursing or surgical attention, (*b*) convalescent or after care, (*c*) solution of a social problem, i. e., stresses such as acute or chronic poverty, undesirable home conditions (bad housing, delinquency, or other illness in the home), industrial superannuation, or other situations exist which make for recurrence of illness or retardation of recovery, and (*d*) education of the patient, of members of the family and of the community in the prevention of further sickness." This certainly makes a heavy requisition for social work upon sickness statistics.

It will be noted that the treatment is from a broader viewpoint than most of the papers appearing in our *Proceedings* in that the writer does not confine himself to the insurance aspects of his problem, but also considers sickness statistics as the basis of a public health structure. He has, therefore, brought under review sickness data which only more or less indirectly bear upon the problems of sickness insurance.

The paper discusses the source of data under two general headings

1. General Population Experience,
2. Experience of Special Groups in the Population.

It may be some shock to those of us connected with insurance institutions to note that the place of accident and health companies does not come until subdivision (*c*) of the second group. Besides outlining the general sources of data the writer discusses the best methods of working from each source, including the requirements in order that the best possible data may be obtained. He also takes some notice of the use to which the results may be put. Although within the limits of a paper in the *Proceedings* of this Society it is only possible to discuss briefly many topics about which it is possible to write extensively, the whole field of statistics of sickness seems to be broadly covered in the paper.

Turning to the details, the writer's definition of sickness beginning at the bottom of page 110 seems strikingly broad since it would include "any objectively or subjectively apparent abnormality," either of structure or function of the body or any of its parts, including the brain. No limitation is placed upon the abnormality, and a strict logical following out of the definition might include all cases of unusual genius as cases of sickness! Even leaving out such absurd extensions of the definition, I question whether in his desire for an all-inclusive definition Mr. Kopf has not gone a little too far. In the next paragraph, however, he further limits himself and indicates that for practical purposes statistical consideration of sickness must be limited to what might be termed, although he does not so refer to it, "sickness disability."

On pages 113 and 114 Mr. Kopf sets out very clearly the terminology used in his paper, which seems well adapted to distinguish the various rates deduced from sickness data which, as I understand it, are generally being adopted. Uniformity in this regard is highly desirable as leading to clearness in discussion.

Mr. Kopf lays down on page 116 certain definite principles to be observed in sickness census practice, and the second of these is: "The facts must be recorded only for whole families who express willingness to impart the necessary information."

This principle is laid down because "Compulsion will probably yield no results." The query naturally arises whether there is not thus produced selection which will have some tendency to distort the results. The selection may be unavoidable, but if the rates are to be used as the basis of any financial computations its presence should be known and recognized.

The writer seems to be a bit unfortunate in the presentation of the third point in the summary of the results of sickness inquiries by the census method. In fact there appears to be a contradiction between his statement that sickness rates developed are at a maximum if the enumeration is made in the spring, and his statement that the resulting figures should be qualified as a conservative estimate of at least the average number of days lost per person. If the preparation of the textbook of which this is to be a part has not proceeded too far, it might be advisable to make some slight alteration in the presentation of this point.

In view of the agitation for general sickness insurance legislation in this country, the enumeration on page 125 of the variant factors found in insurance experience is highly interesting and important. In an earlier paper before this Society (*Proceedings*, Vol. III, p. 213) the writer attempted to make some comparisons between the relative effect of three of these, not with a feeling that these three were the only important variants, but rather because suggestions as to organization indicated that even these important variants were not having the careful consideration to which they were probably entitled.

In closing his paper Mr. Kopf alludes to the problem in sickness insurance which is an ever-present problem in all statistical work where comparison between different investigations is of importance, namely, the problem of nomenclature. Fortunately the variations in this regard in different jurisdictions are not as wide as they are in certain other lines of statistical work. In closing Mr. Kopf suggests that he hopes to present in a later paper a treatment of the second part of the general subject of sickness insurance statistics, namely, "Graduation and Higher Critical Analysis of Tabulated Data." We may certainly look forward to some very interesting material being presented to us in that paper.

MR. EDWIN W. KOPF:

(AUTHOR'S REVIEW OF DISCUSSION.)

Somewhere between the perhaps too broad treatment which I gave this introductory essay on the elements of descriptive sickness statistics and the hasty summaries of sickness "statistics" which are served to us in some health insurance discussions, lies the happy mean. As to the necessary breadth of discussion, may I not suggest that our statistical study as well as our social legislation be guided in future more than it has been in the past by the spirit which actuated British Liberalism of the nineteenth century? —the spirit which gave us the Friendly Society and Cooperative movement and the social legislation sponsored by Anthony Ashley-Cooper. Broad in its application to many phases of a single problem; but deep in its hold upon abiding facts. The present world-struggle between the Tory-Junker, with his static, anachronistic view of human life and destiny, and the Liberal with his well-grounded dynamic faith in the self-reliant common man, should lead us to revise what we have thought essential in social data and in social legislation. Would the liberal viewpoint comprise nearly all the statistical data on sickness in order to determine whether we know much about sickness socially or not, or would it center upon sickness only as an insurable incident? Are we so certain that we really know sickness even in its subordinate economic aspects as to say with one of our members: "The value of an investigation of

the cost of a health insurance system in . . . . would be to tell you whether your cost would be 3⅗ per cent. of wages, or 3⅞ per cent. or 4⅒ per cent. You may be sure in advance that your cost will be between 3 and 4½ per cent., probably about 3⅓ per cent." Why this tendency toward the static and dogmatic in social statistics? We need a statistical Declaration of Independence from two vain notions: First, from the all-sufficiency and stability of collected sickness insurance data, especially the German figures, and second, from the Tory policies, reactions and phrase-mouthing at the foundation of German social-welfare insurance. Whatever we may do in America in applying the principles of insurance science to the facts of sickness, should be founded upon broad and impartial study and upon social policies in conformity to those essentials of British and other Liberalism which will eventually triumph in this present War against dynastic, entrenched Toryism of the Potsdam variety.

Mr. Mowbray's point as to selection in family statistics of sickness: The data of sickness censuses of families willing to supply data will always be subject to qualification, as mentioned by Mr. Mowbray. But they supply information on another important issue not discussed in the essay; family sickness statistics show how seriously the functioning of the wage-earner's family is disturbed and to what extent a plan of insurance serves to sustain the institution of the family under the shock of serious or disabling illness. Workmen's insurance problems are very largely family problems. The selective nature of census family statistics of sickness is perhaps more than compensated by the use of such statistics in the study of the social utility of wage-earners' insurance.

The phrase "of at least the average number of days per person" should read "of at *most* that average number of days per person."