Mr. Bailey's exposure distribution results from a merit rating plan which is slightly different from the National Bureau plan in Pennsylvania in that his plan uses the experience period and point system which the National Bureau used in California; therefore, in some respects National Bureau data from California provides a better basis of comparison.

CALIFORNIA

Exposure Distribution

Merit Rating Code	National Bureau Actual	National Bureau Adjusted$^{\phi}$	Dept. of Motor Vehicles*	Relativity
9	68.1%	61%	54.1%	80
1	18.5	22	22.2	90
2	8.7	10	10.8	100
3	2.7	4	5.5	120
4	1.1	1.5	3.0	140
5	.5	1	1.6	170
6	.4	.5	2.8	200
Mean Relativity	86	88.2	93	
Standard Deviation	14	16.3	25	
Coefficient of Variation	.163	.185	.269	

ϕ Adjusted to take into account changes as a result of checking policies with the driver records of the Dept. of Motor Vehicles.

* Based on a driver record study conducted by California Dept. of Motor Vehicles.

DISCUSSION BY L. J. SIMON

Mr. Bailey's paper introduces two advances in actuarial theory that make it another milestone in progress. The first stride forward is in the concept of the coefficient of variation of the rates as a method of measuring the overall effectiveness of a rating plan. This concept is destined to revolutionize our thinking with respect to classification systems because now at last we have the key to comparing two different systems of classification and also a measure which will show us how much increase in precision we will get by superimposing a new rating criterion upon the existing system.

The second advance in actuarial theory made in the paper is almost lost to the reader because it is passed over so quickly. This is the method used for determining the coefficient of variation of the *risks*. Being able to do this from risk distributions, such as the California Driver Record Study, is quite good, but being able to do it from the loss ratio of risks who were claim free the preceding year (which leads to the credibility measure, a value for the parameter "a", a value for "r", and hence to the coefficient of variation of the risks) is of major impact. This same method can be applied under many, many circumstances to determine the coefficient of variation of the risks. This, of course, will provide the measure on which to judge any rate structure in relationship to the absolute maximum that can be achieved.

It must be recalled repeatedly as the paper is read that the coefficient of variation for the *rates* is a valid measurement only if (1) the rates are an accurate reflection of the experience and (2) the exposure distribution is representative of the population. To illustrate the first point, we could arbitrarily set the following class relativities:

1 A	18
1 B Small Cities	18
1 B Large Cities	74
1 C	269
2 A	521
2 Small Cities	1191
2 Large Cities	1471
3	297

Using Mr. Bailey's exposure distribution we would find the mean is the same as his mean (i. e., 118) but the standard deviation = 235.5 and the coefficient of variation = 2.0. However, this would contradict the known fact that the coefficient of variation of the *risks* is close to 1.00. On the other hand, we could just as arbitrarily set all the class relativities equal to 100 which would indicate the class plan was wholly ineffective. Neither of these conclusions would be remotely near the truth because they neglect the fact that the relativities must be based on the experience in order to be valid measures.

To illustrate the second point we see that the Farm and Non-Farm differential has a value of .034 for this company. Assume the differential to be correct, but suppose some other company has its exposure distributed:

Non-Farm	62,912
Farm	89,874
TOTAL	152,786

In this case we would have mean = 82.353, standard deviation = 14.764, coefficient of variation = .179. Hence for this company the farm criterion would be of much more effectiveness. In fact, with the 100-70 differential, this is the *maximum* coefficient of variation that we could have. On the other hand, if a third company refused to write any risk unless he was a farmer, the coefficient of variation would be zero and, *as a rating criterion,* this factor would have no effectiveness. The first coefficient of variation is undoubtedly too high for the population and the second is unquestionably too low. They illustrate that we must guard against being misled by an exposure distribution which is not typical of the population. An atypical distribution may lead us to either overstate or understate the effectiveness of a rating criterion.

We must be very careful when interpreting or comparing coefficients of variation. If two coefficients are equal, it is safe to say that the rating characteristics are equally effective. If coefficient A is .25 and coefficient B is .50, we can say that B is at least twice as effective as A but might be as much as three, four, or more times as effective. This is true because the first .25 is much easier to get from a rating criterion than is the second .25 and so on up the scale. I suspect that raising the coefficient from .90 to .99 would be as difficult as raising the speed of a particle from 185,000 to 185,-400 miles per second.

The most important feature of the paper for this reviewer is the great use these principles should have among those actuaries who must frequently make critical decisions relative to rates and rating plans. Those rating differentials which, after being based on experience representative of the population, show little or no effectiveness can be and should be dropped from the rate structure. Furthermore, the cost of obtaining the information necessary to properly classify a risk under a given rating plan may be weighed against the effectiveness of that plan.

Mr. Bailey has added a new, original and very valuable tool to the actuaries' working procedures and processes. The paper is indeed a significant one.

DISCUSSION BY L. H. ROBERTS

At the seminar in which the paper was discussed, this writer sided with what appeared to be the consensus (although not unanimous): that the coefficient of variation is a good measure of the efficiency of a classification system. He did, however, mention certain reservations with which he believes the author of that excellent little paper to be in agreement.

It should be emphasized that the absolute value of the C.V. of rates is meaningless as a measure of their propriety. What counts, assuming the overall level is correct, is the spread between rates (the C.V. being a measure of this) as compared with the spread between the hazards of individual risks. This, too, has no significance unless rates are closely related to the experience of the respective classes to which rates apply. Since in a perfect rating system there is a one-to-one correspondence between the rate for a given homogeneous class of risks and the hazard of that class (which might include but a single member), it follows that any rate schedule for which the C.V. of rates is less than the C.V. of hazard in the population of risks will be less than 100% efficient, and the C.V. of the rate schedule will decrease with decreasing efficiency in classification.

If, however, rates are based on judgment rather than on credible experience, the C.V. of rates will not necessarily be related to the efficiency of classification. In such cases it may indeed exceed the C.V. of hazard, as where differentials are established for imaginary or exaggerated differences in hazard. It will often be the case, moreover, that the C.V. of hazard is unknown, since knowledge of this statistic requires analysis of experience by individual risk. For these reasons, the most appropriate use of the C.V. will be often only to compare the efficiency of one class plan with that of another, no attempt being made to estimate their absolute efficiency.

Where the C.V. of hazard is known, a measure of absolute efficiency is provided by dividing the square of the C.V. of indicated rates by the square of the C.V. of hazard. (The same result would be obtained if variances are used.) The quotient, called the coefficient of determination, gives the