AUTHOR'S REVIEW OF DISCUSSIONS

The perceptive reviews by Messrs. Roberts and Walters take two somewhat dissimilar approaches. It accordingly may be helpful in replying to contrast the two. The reviews also point up the fact that the paper could have been a clearer communicative tool.

Mr. Roberts interjects some very interesting data, accompanied by cogent comments, although with respect to actual ratemaking Mr. Walters' comment about the difficulty of securing long series of consistent data (even series much shorter than Mr. Roberts') is pertinent. Mr. Roberts' random sequences, although they reflect potential difficulties in determining *causes,* can provide excellent evidence of the adaptability of the described methods of analysis. If one takes any complete series of the random data (or any smaller number down to ten) and applies the described methods to them, it appears that the resulting predictions will be quite satisfactory.

One point that should not be missed in this connection is that the methods described, excepting only the combination of the control chart with the other techniques, have been developed and applied by economic statisticians to a very diverse group of non-insurance time series for close to half a century. They therefore reflect both a great deal of theoretical development and a great deal of practical wisdom gained from practical application.

Mr. Roberts' cautions about the high desirability of knowing something about the underlying mechanism, and about the difficulty of establishing cyclic parameters, are well taken. We could well supplement them by citing the cautions against "nonsense correlation" that appear in most text books about economic statistics. It is one of the fruits of the long development and practical experience reflected in the methods described that in using them we do not have to concern ourselves with precise determination of the cyclic parameters or whether the waves we see are true cycles or some type of irregular or random fluctuation. The simple analytical rule given in the paper is specifically designed to avoid the difficulty of estimating complex cyclic parameters.

It is true that better knowledge of the underlying mechanisms and causes of changes in time series can aid us in selecting curves of appropriate shapes,

but the paper demonstrates there is much that can be done, with very scanty knowledge of these parameters, that will still permit us to make accurate and stable predictions and without the introduction of arbitrary judgment. The examples in the paper also show how the methods objectively dampen the wide fluctuations in single-state data that underly the use of multi-state or countrywide data in ratemaking. This dampening has in all cases known to the author been sufficient to make the use of extraneous (out-of-state) data unnecessary.

Both reviewers rightly emphasize the importance of the conflicting needs for stability and accuracy of response. The competitive effect of the interplay between these two factors that is reflected by any one company's or bureau's rates will depend most, however, on when they file new rates in relation to competitors. If they file first, they can always be second guessed. Perhaps most important is to maintain a premium level (and the analysis here is aimed at premium level, not at individual rates) that is profitable. The competition will not for very long try to use rates that are unprofitable. The problem of adequacy has during the past two decades been a much more important one than the problem of being at a competitive disadvantage by not reducing rates quickly enough in line with statistical indications. The examples show that the methods described take well into account *both* the long-term and short-term indications of experience, and react at turning points (which are most critical) faster than the methods now in common use.

An over-all or systems approach was used to achieve the methodological balance between the conflicting needs for both accuracy and stability. One thing which the data in the paper make crystal clear is that, despite the continued presence of inflationary tendencies, there are very definite downward movements that legitimately call for rate decreases from time to time. The actual filings of the rating bureaus have demonstrated the very opposite of the statement that ". . . rate changes which fluctuate to reflect the nonstable movements of inflation and insurance perils are invariably a matter of size of increase rather than a question of increase versus decrease." Any system of time-series analysis must not reflect any such bias — it must be able to reflect such decreases as well as increases — if it is to be truly suitable for ratemaking purposes.

Mr. Roberts clearly recognizes that the paper discusses the whole problem of time-series analysis, not just the one type of time-series movement

called "trend." Failure to grasp this key distinction, covered by the third of the five stated objectives of the paper, creates some difficulties for Mr. Walters. To overcome this communicative failure in the paper we can contrast his interpretation with what the paper should have more clearly implied.

> The reviewer says that the thesis of the paper is that
> "The importance of trend requires the establishment of a method that is actuarially precise, uses a maximum amount of information, is applicable to all lines, and reflects cyclical movements to some extent while at the same time providing stability and removing arbitrary judgment . . .
> "He defines 'trend' as long-term movement, thereby requiring a large number of years to measure it accurately. Hence, the current shorter term method seems doomed at the outset, although the author later recognizes the need to reflect cyclical movements and major irregular fluctuations. Fundamentally, however, his concern is for the long-range growth of pure premiums, excluding the cycles and waves that temporarily mask the ultimate trend."

Had the paper been clear enough for the reviewer correctly to infer what the paper was meant to imply, however, he would probably have written along these lines:

> The importance of all four generally accepted types of movements in time series requires the establishment of a method that is actuarially precise, uses a maximum proportion of the pertinent information available, is applicable to all lines, and reflects trend and cyclical and irregular and (when appropriate) seasonal movements as far as they can practicably be measured while at the same time providing stability and removing arbitrary judgment.
> He states and uses the generally accepted definition of "trend," thereby requiring a large number of years to measure it accurately. Hence the current shorter-term method seems doomed at the outset since it is not designed to reflect, as the author recognizes from the outset is necessary, cyclical movements and major irregular fluctuations. Fundamentally, his concern is for a system that handles equally well all four major types of movements in time series and that masks neither the longer-term nor the shorter-term movements.

The reviewer accurately points up a major disadvantage of the com-

mon substitution of short-term averages by ratemakers in place of the economic statisticians' long-term trend when he says that the author's ". . . 'proof' of the stability of long-term estimates consists of projecting the short-term estimates for more years than they are meant to be projected and comparing them with the long-term projection of long-term data." The short-term trended average gives no perspective of the direction or shape of the long-term movement. The 16th footnote to the paper is in point here.

It would be most informative to compare the forecasts or indicated rate-level adjustments, given by the methods shown in the paper and by the short-term trended-average method now so widely used, with the actual results that were experienced. This could be done for each of a series of years for several of the sets of data presented in the paper (or any other comparable sets). Comparisons of accuracy (via the standard error of estimate or some similar measure) could well be made (1) for several years within each series and (2) for individual years among the group of series.

One can easily agree with Mr. Walters' belief that the true growth of insurance costs is probably exponential, or a modified exponential or logistic shape that eventually tends to flatten. To determine this, however, we need much longer series than any of those available for the paper.

It is apparently lack of clarity in the paper rather than disagreement which underlies Mr. Walters' seventh and eighth paragraphs. The paper does state that the over-all pure premium is independent of the distributions and correlations of the various underlying rating criteria in data *for any one year*. It then goes on to state that these distributions and correlations *could* have an effect on the relationships among over-all average pure premiums for a series of years. The conclusion that these have not so far been of material size in liability insurance is shared by Mr. Walters.

In using the over-all pure premium we are dealing with an average. An average often masks certain details. So far the details here masked have been unimportant. As a means of insuring that, at any time these details do become important, they are properly handled in the rating process, the paper points out that it would be highly desirable to use a pure premium index that reflects in a controlled manner the changing internal mix. This would be an exact parallel to controlling the changing mix among collision deductibles — a change which is definitely of material size — that is illustrated in the paper. Had the needed data been available, an example of

how this index number control could be used for liability insurance premium levels would have been given in the paper.

In short, we agree that there is a potential problem, we agree that the problem has not so far been material in liability insurance, and we agree that it bears watching. An automatic method of doing this watching and at the same time making any necessary adjustments has been proposed. Even without this automatic control mechanism the suggested analytical methods do automatically adjust the rate level for any changes in the distribution by rating criteria. Only if separate time-series analyses are made for two or more individual components of any of the three current major groups (private passenger, commercial, and garage) will there be difficulties. If the suggested index numbers are used, these difficulties will be eliminated.

Mr. Walters is correct that the distance of the guide lines from the trend line, in the figures showing data with the trend removed, should reflect the slope or *"b"* factor of the trend equation. The guide lines in these figures should be *cos arctan b* times the standard error from the trend lines. He also correctly points to one of the banes of the economic statistician's life: changes in the form and classification of the data in time series. Since it was possible to produce all of the charts in the paper, however, one can be reasonably optimistic on this score.

Mr. Walters' last paragraph has been answered above. One of the principal advantages of the system described in the paper over current methods is the fact that it adjusts for cyclical effects in a flexible and reasonably objective manner, and in a way that does not permit continued deficit operations over the long periods that have been experienced in the recent past. It is felt these deficit periods reflect the need for improvements in the present system. Both reviewers are to be complimented on raising important points and for their reasoned and considerate approaches.

Grateful acknowledgement is due personnel of National Bureau of Casualty Underwriters and National Automobile Underwriters Association (now combined as Insurance Rating Board) for help in securing the Illinois and Kentucky data and to Mr. Carl Wilcken, then Actuary of Canadian Underwriters Association, for help in securing the Canadian data, used in the paper. Such acknowledgement is also due unknown critics for pointing out several ambiguities and obscurities in the original draft.