Statistical Analysis of Accident Data as a Basis for Planning Selective Enforcement

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This paper deals with the application of quality control techniques to the analysis of traffic accident information, and the adaptation of the techniques to operational decision-making processes. The underlying theory of control charts and its application to motor vehicle accident data is presented in detail. Several new theoretical conclusions relating to the statistical sensitivity of accident control charts are presented.

A generalized system of control chart computer programs, designed to reduce the reported statistical techniques to operational practice, was developed and subsequently applied to a sample of accident data. Results suggest that for many accident data uses, a high alpha-error probability should be tolerated in order to realize a low beta-error probability concomitant with operationally defined lengths of roadway, realistic control chart time periods, and reasonable sensitivity to changes in accident-producing potential. Such control charts would rarely fail to detect a small change in accident potential at the cost of having many of these change indications be spurious. The principle is that for many accident data applications, it is appropriate to tolerate many false indications of change in order to reduce the likelihood of failing to detect a real change.

•THE overall accident information system is comprised of on-the-scene investigation, report preparation, encoding, and subsequent statistical analysis of the recorded data. The system potentially can yield important information for operational decision-making in engineering, enforcement, licensing, and other governmental functions directed toward controlling the motor vehicle accident problem.

The data may be utilized on a single accident basis, as in judging whether to issue a criminal complaint against an offending motorist, or on an aggregated basis as in calculating accident rates. The latter aspect is the subject of this work, with the presumption that information on the individual accident is complete and correct. The study is concerned with combining such information from a multiplicity of accidents, and making valid interpretations of the results.

The most difficult problem in interpreting such aggregated accident information is to identify stable patterns out of the continuing fluctuations in the data from day to day, week to week, month to month, and year to year. This inherent, seemingly unexplainable, fluctuation forces increasing levels of aggregation, such as combining daytime and nighttime accidents to obtain 24-hour totals, because with the resulting larger accident totals it generally becomes easier to extract a stable pattern out of fluctuations that otherwise could not be reliably interpreted.

Cursory examination, however, discloses that the simplest form of accident information aggregation, namely, raw tabulation of numbers of accidents, in itself can pose

Paper sponsored by Committee on Highway Safety and presented at the 46th Annual Meeting.

substantial questions in logic of interpretation. Should numbers of daytime accidents be combined with nighttime accidents? Winter with summer accidents? Good weather with bad weather accidents? Freeway with surface street accidents? Rural with urban accidents? There obviously is a point beyond which such aggregations no longer are useful or, at best, are of limited help in the specifics of allocation of enforcement manpower, selection of appropriate accident countermeasures, establishment of priorities for maintenance or capital improvements, or other operational purposes.

To be useful, sufficient accident data should be available to meet the needs of a given operational problem. Thus, for example, if a commander has to allocate his manpower between daytime and nighttime shifts, he requires data that are collected on a shift basis; he will not be materially helped by a 24-hour accident total. However, he also is not helped very much if the data collected on a shift basis do not include enough information to enable him to detect any stable pattern of differences between daytime and nighttime accident experience, since he does not wish to continuously respond to what might be random differences.

All uses of aggregated accident information for operational decision-making of one form or another pose the same problem of detecting stable patterns of accident experience out of a broad array of random fluctuations of statistical data.

SELECTED TOPICS ON CONTROL CHART THEORY

Several ways of adapting control chart techniques to accident information have been reported by a number of investigators [Mathewson, Brenner, and Hulbert (1954), Mathewson and Brenner (1956), Norden, Orlansky, and Jacobs (1956), Littauer (1957), Blindauer and Michael (1959), Rudy (1962)].* All are essentially variations of the classical Shewhart quality control chart techniques originally designed to assist in maintaining product quality during the course of manufacture [Shewhart (1931), Grant (1952)]. The Shewhart control charts, in turn, are based on well-known probability concepts and the associated theory of runs in sequential events.

When these control chart techniques are applied to accident data, a number of matters have to be considered more carefully than is usually necessary in the manufactured product applications for which they were originally conceived. To highlight some of these matters, a brief review is presented of overall control chart logic, starting with the statistical concept of control chart limits as applied to accident analysis. Formal derivations are in Appendix B. More detailed background information on probability theory and its applications to control charts can be found in the referenced works.

STATISTICAL CONTROL CHART LIMITS ON ACCIDENT EXPECTATION

The network of surface streets and freeways will be considered to be made up of a group of "elements," the definition of which is left open here. For enforcement purposes, the most convenient definition might be the beat. For traffic engineering, it might be the intersection as distinguished from the intervening mid-block lengths; for highway engineering, it might be design features such as a horizontal curve between the points of tangency.

Each element has an accident history. This history can be described in terms of the number of accident events without regard to the number of injuries or extent of property damage accompanying each event. Or it may be described in terms of the aggregated losses, such as the total number of fatalities, without regard to the number of events that produced the totals. Or it may be described in various combinations of events and accompanying severity, such as the total number of accidents in which one or more persons were killed. It is convenient at the outset to consider accident experience solely in the context of the number of accident events, although, with minor mathematical adjustments and assumptions, the same logic can be applied to the other definitions.

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*References are cited in Appendix A, Bibliography, under appropriate headings.

Implicit in all operational decisions directed toward the accident problem is some estimate of accident expectation, regardless of whether or not the estimate is explicitly identified. A decision to improve horizontal sight distance around a curve generally implies a judgment (by someone) of a high accident expectation for the unimproved roadway coupled with a reduction in expectation with the improvement. A decision to allocate additional enforcement manpower to a given beat implies a judgment of high expectation on that beat.

Underlying all statistical usage of accident data for operational decision-making is the assumption that these data reflect in some manner the accident causation process. The same assumption is explicitly employed here.

Another important assumption is the commonly accepted belief that even when all factors that conceivably could relate to accident causation remain unchanged (condition of the road surfaces, traffic control devices, weather, condition of vehicles, composition of motorists and their driving habits), accident experience will nonetheless vary. In some periods there will be no accidents, while in others the number will be high. Therefore, any observed deviation in accident experience from what it was expected to be might be reflecting nothing more than inherent variability of accident experience in an unchanging accident causation process. On the other hand, an observed deviation might be indicating some change in the accident causation process. Generally speaking, the closer accident experience is to what we expected it to be, the more likely are we to conclude that we correctly assessed the accident causation process. Similarly, with greater deviations we are more likely to conclude that there was some (unanticipated) change in the accident causation process.

In order to make such judgments more precise, it is necessary to express quantitatively the inherent variability of accident experience. The control chart technique is one statistical method for doing so. Specifically, it is a pair of accident values having a selected probability of bracketing the value of accident expectation. To cite one example, a two-sided 95 percent control chart interval, $\Pr\{A < \eta < B\} = 0.95$, is read as a 95 percent probability that the range between A and B will bracket the number of accidents.

Stated otherwise, we expect that in any 95 of 100 periods of observation in which the accident causation process remains unchanged, the number of accidents that occur will be bracketed within the range A to B. At the same time, we expect that in the remaining 5 observation periods the number of accidents will be greater than B or less than A. Depending on the application, broader or narrower control chart limits may be specified. A 90 percent control chart interval will be narrower than the 95 percent interval for the same expectation; a 99 percent interval will be broader. As shown in Figure 1, (a) when the observed accident losses fall within the 95 percent control chart interval, we are most indecisive as to the occurrence of a change in accident causation; (b) we are less indecisive when it falls outside the 95 percent interval, although still within the 99 percent interval; and (c) we are least indecisive when it falls outside the 99 percent interval.

Bearing in mind that the purpose in interpreting accident experience is to decide whether or not there has been some change in the underlying accident causation process, we "play the odds" and draw conclusions according to the following basic set of rules:

Rule 1. If the observed number of accidents falls outside the control chart interval, we conclude that there was a change in the underlying accident causation process (during the time period and on the road element for which the control chart interval was established).

Rule 2. If the observed number of accidents falls inside the control chart interval, we conclude that there was no change in the accident causation process.

For a 95 percent control chart interval, on the average 5 out of 100 (Rule 1) decisions that a change has occurred in accident causation, when in fact it has not, will be made. Or, the likelihood that Rule 1 will lead to an erroneous conclusion is 5 percent. We shall refer to this decision error as the Type I error, or the alpha (α) error, or the error of commission (stating that a process change occurred when in fact it did not).

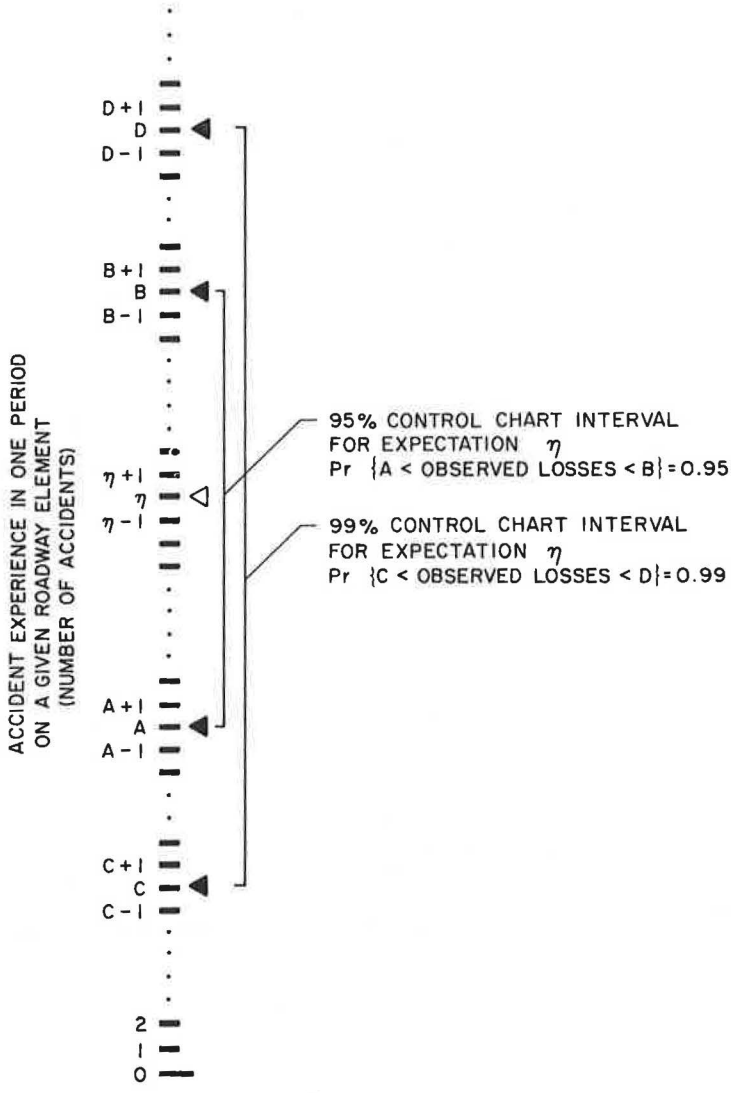

Figure 1. Representation of 95 percent and 99 percent control chart intervals for interpreting accident experience.

Rule 2 decisions as to no change in the accident causation process will also be correct most of the time, but nevertheless will be wrong occasionally. We shall refer to this error as the Type II error, or the beta (β) error, or the error of omission (failure to recognize that a process change occurred).

A method for establishing the magnitude of the beta error is described in Appendix B. It suffices here to state that it depends on the accident expectation.

The various possible decisions and decision errors in interpreting observed accident experience in relation to the control chart interval on its expectation are shown in Figure 2. The magnitudes of the Type I and Type II errors determine the sensitivity of the control chart interval for judging that changes in the accident causation process have or have not occurred. Operational personnel have to establish these values in advance, that is, to decide what chances they are willing to take on making the Type I error and

Event	Decision	Actual Status of Accident Causation Process	
		Changed	Unchanged
Value of observed losses is above upper bound of control chart interval	Accident causation has changed (i.e., worsened)	Decision is correct	Decision is Wrong Type I Error
Value of observed losses is within the control chart interval	No change in Accident Causation	Decision is Wrong Type II Error	Decision is Correct
Value of observed losses is below lower bound of control chart interval	Accident causation has changed (i.e., improved)	Decision is Correct	Decision is Wrong Type I Error

Figure 2. Correct and incorrect decisions inherent in use of control chart intervals.

Type II error. In general, this will depend on the operational problem itself. It will, however, also be linked to choices on other matters that have to be set properly to derive full benefit from the control chart logic, including (a) the length of the time period (should control chart intervals be established for daily losses? weekly? monthly?); and (b) the length of the roadway element (a one-mile length? five? ten?).

In addition to these matters relating to choice of the error probabilities and the domain (length of time period and length of roadway element) for the control chart interval, there is the all-important choice of how to estimate the expectation for which the control chart interval is to be computed. And when the expectation is estimated for the unchanging accident causation process, the choice has to be made as to the degree of change to which the control chart interval is to be sensitive. An interval that rarely will lead to the Type II error of failing to detect, say, a 10 percent change in accident causation, will frequently permit the Type I error of asserting that a change of this magnitude occurred when in fact it did not.

In effect, there is not any single optimum control chart interval for interpreting accident experience, but rather whole familes of intervals involving various combinations of (a) Type I error, (b) Type II error, (c) length of time period, (d) length of roadway element, (e) accident expectation, and (f) degree of sensitivity to process change. The optimum combination of these factors will depend on the particular application. Various criteria for selecting a combination will be discussed later. However, the underlying theory and interpretation procedures are the same for all combinations.

In the discussion so far, the control chart interval for each time period is developed around the expectation for that time period. The accidents in the period are then compared with the predetermined control chart interval, and a decision is made as to whether or not a process change occurred in that period. By combining this information (observed accidents in the present period in relation to the control chart interval around the expectation) with similar information for the prior observation periods, it becomes possible to establish the presence (or absence) of process changes extending over a

sequence of observation periods. Up to the present time, the basis for combining successive sets of information in this manner has arisen from the theory of extreme runs (i.e., Grant, 1952).

An allowable rule for interpreting information in successive control chart intervals depends on the selected value for the Type I error associated with an arbitrary interval. However, for such rules to fit into the framework we are developing here, the Type I and Type II errors associated with the use of these rules must be known. To the authors' knowledge, there has been no systematic study performed to assess the magnitude of these errors. A more detailed discussion of methods of generating such rules and of assessing the magnitude of the Type I and Type II errors associated with their use is beyond the scope of this paper but is, nevertheless, mentioned as a possible avenue of future investigation.

SOME ISSUES RELATED TO ESTIMATING ACCIDENT EXPECTATION

The manner in which accident expectation is estimated can substantially alter the validity and effectiveness of control chart methods. Several matters related to this problem will be discussed.

Apart from its important role in accident control chart technology, the procedure for estimating accident expectation is of itself important for operational decision-making. Valid estimates of accident expectation may be of major operational importance even if control chart technology is not being used. For example, knowledge that the expected number of accidents on Saturadays is higher than on Mondays may be sufficient for a proportionately greater allocation of manpower to Saturday duty. The discussion here, however, will not treat this important use of accident expectation as an operational tool of itself, but instead is limited to its use in control charts.

As stated earlier, underlying all statistical usage of accident experience for operational decision-making is the assumption that accident data in some manner reflect the underlying accident causation process. For purposes of exposition, we shall define p^* as the measure of this accident causation process. Let us then posit that the accident expectation m is a barometer of p^*.

Accident expectation can be estimated in any number of different ways, each of which may produce a different value for m. However, since all are supposed to be barometers of the same accident causation process measure p^*, the question must be raised as to how good the selected value m is as a barometer of p^*.

It is convenient at this point to introduce the Poisson distribution for accidents which arises from the assumptions that (a) any given accident observed on a roadway is independent of any other traffic accident, and (b) as the length of the time period in which any given section of roadway is observed approaches zero, the probability of observing one or more accidents approaches zero. One property of the Poisson distribution is that its mean and variance are equal. Denoting by Y_t the number of accidents and by m the mean, then

$$P[Y_t = y_t] = \frac{e^{-m} m^{y_t}}{y_t!} \tag{1}$$

and an estimate $\hat{m}$ of m based on the accident data over the preceding N unit time periods is

$$\hat{m} = \frac{1}{N} \sum_{i=t-N}^{t-1} Y_i \tag{2}$$

The estimate (Eq. 2) assigns equal weights to the accident experiences of all of the preceding N unit time periods. In contrast to this, a variable weighting scheme could be used. For example, the scheme

$$\frac{Y_{t-3} + 2Y_{t-2} + 5Y_{t-1}}{8} \triangleq \hat{m}$$

assigns variable weighting 1:2:5 to the accident data of the third, second, and first periods, respectively, immediately preceding the period t. The general concept is that the longer away in time, the lower the correlation in accident losses. In the example, the losses in t - 1 carry 5 times the weight of those in t - 3 for estimating losses in period t. Of course, selecting the appropriate weights in schemes such as this also poses a problem. For the present, however, we shall consider only the equal weighting approach.

We can increase the inherent stability of our estimate $\hat{m}$ by increasing the number of data points in our sample. Stability of this nature makes $\hat{m}$ relatively insensitive to short-term transient changes in p^*, and can be desirable for some applications, as, for example, in deciding whether or not to make some costly spot capital improvement, or in making some basic policy change in licensing. In these kinds of decisions, one would not wish to react to some transient change in the accident causation process which might readily correct itself.

On the other hand, other classes of operational decisions specifically require sensitivity to short-term transients in accident causation. Selective enforcement is in this category. One may even suspect that its primary use of control chart techniques would be only in response to short-term effects, although for some enforcement decisions more stable effects might be the important factors. Thus, a single method of estimating accident expectation will not be optimum for all uses of control charts.

SOME ISSUES RELATED TO THE CHOICE OF ONE-SIDED OR TWO-SIDED CONTROL CHART SYSTEMS

The preceding discussion of control charts has dealt with two-sided control chart limits. With no a priori information concerning the direction of change of the causation process, the two-sided limits are appropriate when we expect either an improvement or a worsening in the causation process. However, if we have a priori reasons to believe that the causation process will change in a particular direction (for instance, due to diurnal variation) it is wasteful of information to employ control charts based on two-sided control limits. We are thus led to propose three separate control chart systems: Control Chart System A based on an upper one-sided control chart limit, Control Chart System B based on a lower one-sided control chart limit, and Control Chart System C based on two-sided limits. The designation of A, B, and C for the three types of control charts is arbitrary, and is used solely to simplify the presentation, here and later in the report.

Control Chart System A

When on the basis of a priori information we expect the causation process to worsen, or when an operational decision is to be made only when the accident causation process appears to have worsened (based upon a measure of p^*), it is appropriate to use a control chart based on an upper one-sided control limit. With such charts, a decision that a worsening in the causation process has occurred in time period t is reached only when the measure of p^* falls above this limit. It is not possible with this type of control chart system to make any decisions regarding process improvement even if improvement actually occurs in period t.

Control Chart System B

When we expect the causation process to improve on a priori grounds, or when an operational decision is to be made only when the process appears to have improved, it is appropriate to use a lower one-sided control limit on the measure of p^*. With these charts, the decision that an improvement has occurred in the causation process in period t is reached when the measure of p^* falls below this limit. With this type of

control chart system it is not possible to reach the conclusion that the process has worsened in period t even if in actuality it has.

Control Chart System C

When we have no a priori information concerning the direction of change of the causation process, or when operational decisions concerning improvement and worsening must be made, it is appropriate to use two-sided control limits on the measure of p^*.

A mathematical description of each of the three control chart systems together with their associated decision rules is presented in Appendix B, Topic 1.

Conceivably, an investigation might be initiated using Control Chart System C; when sufficient indication of direction of change of the causation process is available, the investigation might then be continued with System B or System A (whichever is appropriate). A complete discussion of interplay among the three control chart systems and guidelines for their choice will be postponed to future papers.

SOME ISSUES RELATED TO SPECIFYING CONTROL CHART PARAMETERS

Once a user has selected either an A, B, or C control chart system as the most appropriate for a particular operational problem, he must specify its relevant parameters, some of which relate to the nature of the application while others are largely statistical. The specification of the statistical parameters will be discussed here.

The statistical group of factors that must be taken into account in establishing a control chart system for a particular application relates to the property of "control chart sensitivity" and includes (a) the acceptable alpha (α), i.e., probability of the Type I error; and (b) the acceptable beta (β), i.e., probability of the Type II error pursuant to failing to detect a K percent change in expectation.

A control chart utilizing a low value for the alpha error will have correspondingly low probability of suggesting that a change in p^* took place when in fact there was no change. However, this kind of a chart might also have a correspondingly high beta error, i.e., a high probability of failing to suggest a change in p^* when such a change in fact occurred.

In effect, it is not possible to reduce the probability of making a Type I decision error without increasing the probability of making a Type II decision error. The user must accordingly decide, in establishing a control chart system, the kind of error that he is most anxious to avoid making. In some applications, it might be especially important to avoid the Type I error; in others, avoidance of the Type II error might dictate the choice. The following are examples of these situations:

1. Choose a low value of alpha (e.g., 0.01 instead of, say, 0.05). The decision on a proposed major expenditure for a given capital improvement is to be geared to a worsened accident causation process. Accordingly, one would wish to guard against concluding that a worsening had occurred when it did not, because such an erroneous conclusion would lead to the money being spent needlessly.

2. Choose a low value of beta (e.g., 0.60 instead of 0.90). A decision on a proposed operational practice is to be geared to correcting a worsening risk situation. Accordingly, one would wish to guard against not recognizing a high-risk situation, and having failed in this regard, thereby deciding against a needed operational practice. A control chart in this form would have reduced likelihood (i.e., 0.60 instead of 0.90) of failing to detect a bona fide worsening of risk, but would more often come up with a spurious indication of worsening risk.

The selected level of the beta error must be linked to a selected change in accident expectation. For example, with Control Chart System A and a fixed value of the alpha error of 0.05, a beta error of approximately 0.28 is associated with a failure to detect an 80 percent increase over an accident expectation of 10 accidents per time period. For the same control chart system and alpha error, a beta error of 0.16 is associated with failure to detect a 100 percent increase over an accident expectation of 10 accidents per time period. Stated otherwise, we may sizably decrease the beta error for

fixed system, alpha error, and expectation if we are willing to sacrifice detection sensitivity.

A UNIFIED PROCEDURE FOR ESTABLISHING A CONTROL CHART SYSTEM FOR A PARTICULAR APPLICATION

In this section we shall be concerned with describing briefly the manner in which the parametric values of a control chart are specified once the choice of the control chart system has been made. This shall be illustrated with actual data.

Consider that a user decides that a particular control chart application requires an alpha error of magnitude α^*, and a beta error of magnitude β^* for (probability of failing to detect) a change of magnitude K* (percent) of accident expectation. As derived theoretically in the Appendix, in order for this triplet to produce a reliable control chart system, the accident expectation must be as great as a mathematically calculable value that will be referred to as λ_0^*. If the estimate of the expectation is less than λ_0^*, a different control chart system must be designed according to one or more of the following alternatives: (a) change the value of the tolerable alpha error (α^*), (b) change the value of the tolerable beta error (β^*), or (c) change the value of K*. Or, to maintain the desired α^*, β^*, K* triplet, we have the following alternatives: (a) increase the period of measurement, e.g., from a one-week to a one-month period, as a means of increasing the accident expectation for the same roadway length up to λ_0^*; (b) increase the length of roadway, e.g., from a one-mile up to a five-mile length, as a means of increasing the accident expectation up to λ_0^* in the same period of measurement; or (c) increase both the period of measurement and roadway length so that accident expectation for the new combination is up to λ_0^*.

Guidelines for selecting the appropriate alpha error, beta error and associated K value have already been discussed. These decisions are common to all control chart designs. The new method, however, goes beyond the (α, β, K) choices, and deals as well with changing either or both the roadway lengths and the period of measurement, so that a control chart system of selected (α^*, β^*, K*) sensitivity can be used.

The mathematical bases underlying the proposed method are presented in Appendix B, and deal with the following topics, under various assumed operational constraints:

1. Topic 2, determination of the control chart time period for a fixed length of roadway;
2. Topic 3, determination of roadway length for a fixed time period control chart; and
3. Topic 4, determination of optimum trade-offs between length of time period and length of roadway.

In the interest of simplifying the presentation, the discussion-demonstration to follow is limited to the Topic 2 treatment, since a similar procedure could be followed in dealing with Topic 3. In dealing with the Topic 4 problem, graphical and/or computer techniques are required for determining optimum trade-offs between period length and roadway length. These trade-off procedures become necessary if, to realize a selected control chart sensitivity, either the required time period or roadway length turn out to be greater than would be useful for a given operational application. A demonstration of the Topic 4 problem will be deferred to future papers.

TABLE 1

MILEPOST DESIGNATIONS AND LENGTHS OF FIVE OCEANSIDE BEATS

Item	Beat				
	1	3	5	43	45
Length (miles)	17.93	5.28	4.84	4.35	2.85
Beginning milepost	53.65	40.68	35.84	45.96	32.99
End milepost	71.58	45.96	40.68	50.31	35.84

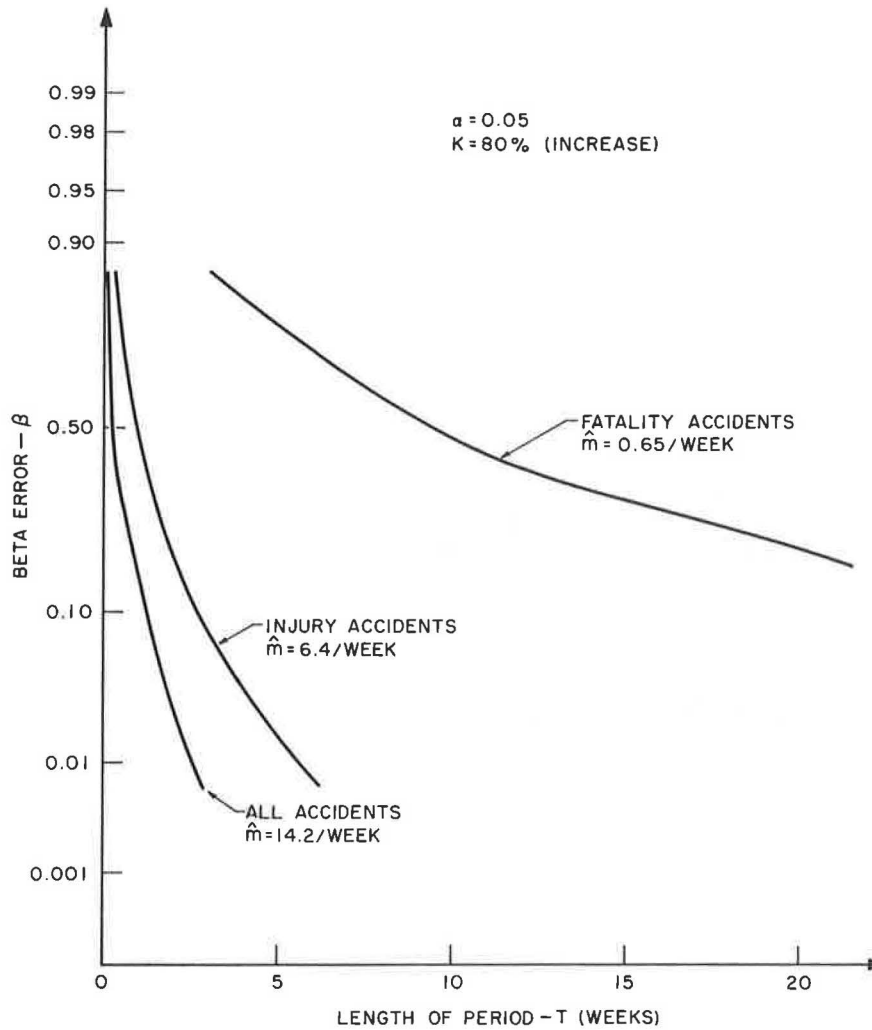

Figure 3. Control Chart System A: β vs T (based on 1962 Oceanside data).

In order to develop a demonstration of the overall method, actual accident data were used. These data were provided by the California Highway Patrol in an IBM tape data file for accidents that occurred on Highway 101 in the Oceanside area (near San Diego) between the years 1961 and 1964. This roadway is divided into five CHP line beats. The beginning and ending milepost designation, together with the length of each beat, are shown in Table 1.

The overall process is shown in Figures 3 and 4 in which the variation of β with control chart time period (T) is plotted for fixed values of α, K, and $\hat{m}$. Two grossly different values of α are used (α = 0.05 in Fig. 3, and α = 0.30 in Fig. 4) to demonstrate the effect that the choice of α has on control chart time period. Three values of $\hat{m}$ are used; the reasons for selecting these values will be explained presently. The value of K is arbitrarily set at 80 percent (increase). These figures apply to Control Chart System A.

A number of general properties of the mathematical formulation are directly illustrated. Some of these are:

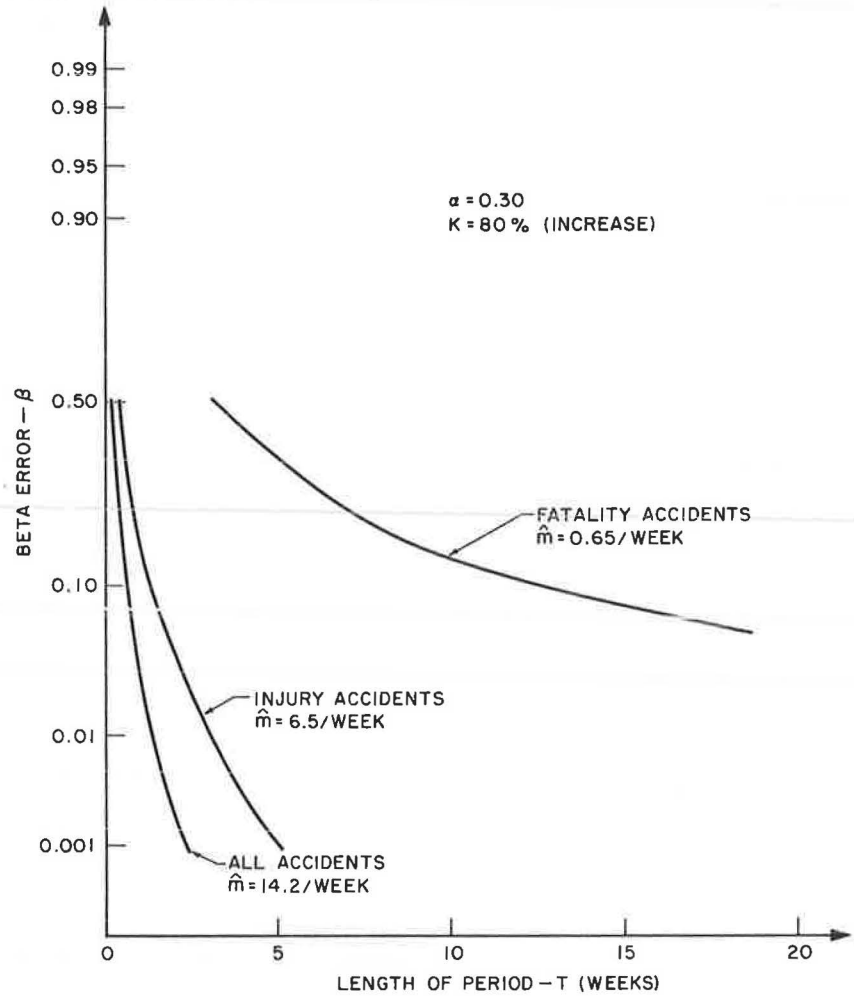

Figure 4. Control Chart System A: β vs T (based on 1962 Oceanside data).

1. By increasing the α error that we are willing to tolerate, we can reduce the control chart time period. For example, with β held constant at 0.3 for an $\hat{m}$ of 6.5 accidents/week, the control chart time period changes from 1.5 weeks to 0.8 weeks by increasing the α error probability from 0.05 to 0.30.

2. By increasing the β error that we are willing to tolerate, we can reduce the control chart time period. For example, with α held constant at 0.30, for an $\hat{m}$ of 6.5 accidents/week, the control chart period changes from 2.5 weeks to less than 3 days by increasing the β error from 0.02 to 0.50.

3. The control chart period varies sharply with changing values of accident expectation. For example, with β held constant at 0.1 and α held constant at 0.30, the control chart period is 12.5 weeks for an $\hat{m}$ of 0.65 accidents/week, and 4 days for an $\hat{m}$ of 14.2 accidents/week.

Figures 3 and 4 pertain only to the three selected values of $\hat{m}$ (0.65, 6.5 and 14.2) and the two arbitrarily chosen values of α (0.05 and 0.30). Similar functional plots, or other forms of nomographs, can be readily developed for the complete array of α and $\hat{m}$ values.

The $\hat{m}$ values in Figures 3 and 4 were deliberately selected to coincide with estimates of accident expectation on the Oceanside length of roadway. This is to say that, using the accident history of this roadway (the 1962 record in this case), estimates of $\hat{m}$ for the expected numbers of several categories of accidents were computed. The functional variations of β with the control chart period were then generated only for these values of $\hat{m}$, to satisfy the demonstration purposes of this work. In practice the user would estimate the accident expectation from past history and then, with these data, enter an array of ($\hat{m}$, α, K) charts to determine the smallest control chart period that he could use and still have the resulting control chart produce meaningful conclusions.

In the example illustrated in Figures 3 and 4, the $\hat{m}$ values for which the functions are plotted coincide with the following estimates of accident expectation for the full length of Oceanside roadway (the beat distinctions have been suppressed to facilitate the demonstration of the method):

1. $\hat{m}$ = 0.65 accidents/week coincides with fatality accidents,
2. $\hat{m}$ = 6.5 accidents/week coincides with personal injury accidents, and
3. $\hat{m}$ = 14.2 accidents/week coincides with total accidents.

A control chart on fatality experience having an α error probability of 30 percent and a 10 percent (β error) probability of failing to detect an 80 percent (K) increase in fatality-producing potential would require a control chart time period at least 12.5 weeks long. In other words, there would be one control chart point every 12.5 weeks. If the period were reduced to 3 weeks, the β error would be increased to about 50 percent. Thus, even a bimonthly control chart would be essentially insensitive to as much as an 80 percent shift in fatality-producing potential. The user would probably conclude that control charts on fatality experience would not be useful.

On the other hand, a control chart on total accident experience on this roadway could use a period as short as 4 days for β = 10 percent and α = 30 percent. With this degree of sensitivity, it would be a highly useful operational tool.

Several additional sets of α, β, T values for fixed K on the Oceanside roadway are given in Table 2. This illustrates the frame of reference within which a user could select a control chart system for this roadway. His choice of a particular α, β, T combination would depend almost entirely on his operational judgment as to how he might utilize control chart results.

TABLE 2

SELECTED RESULTS UNDER CONTROL CHART SYSTEM A USING TIME PERIOD, T, AS FUNCTION OF α, β, K = 80 PERCENT, AND ACCIDENT CLASSIFICATION FOR 1962 OCEANSIDE ACCIDENT EXPERIENCE

Alpha	Beta	Accident Classification		
		Total T (weeks)	Personal Injury T (weeks)	Fatal T (weeks)
0.05	0.80	0.20	0.50	4.5
	0.60	0.25	0.75	7.8
	0.50	0.30	0.90	8.8
	0.30	0.65	1.5	15.2
	0.20	0.85	2.0	20.0
	0.10	1.25	2.8	>20
	0.05	1.7	3.6	>20
	0.01	2.5	5.7	>20
0.30	0.50	0.12	0.37	3.2
	0.40	0.20	0.45	4.3
	0.20	0.32	0.80	7.8
	0.10	0.6	1.3	12.5
	0.05	0.8	1.8	18.8
	0.03	1.0	2.3	>20
	0.01	1.35	3.0	>20
	0.005	1.6	3.6	>20
	0.001	2.2	5.0	>20

To increase further the operational utility of these control chart systems, it undoubtedly would be desirable to reduce the K value as much as possible, i.e., to make the overall control chart process sensitive to as little as, say, a 10 percent increase in accident potential instead of the 80 percent value used in the Oceanside example. A good way of doing this would be to substantially increase the α error probability, possibly to as much as 40 percent. The resulting control chart would rarely fail to detect a small change in accident potential, but at the price of having its indications of changes be more frequently spurious.

The principle suggested here is that for certain applications of control chart techniques to accident data, it will be appropriate to tolerate more false indications of a change in accident-potential in order to reduce the likelihood of failing to detect a real change.

Each of the three variations of the method (fixed roadway length-minimum time period, fixed time period-minimum roadway length, trade-off between roadway length and time period) can be programmed for computer solution, either for weighted or unweighted estimates of accident expectation. It thereby would become practical to implement control chart techniques in which the control chart is continuously being adjusted to maintain a preset sensitivity. To the best knowledge of the authors, a dynamically adjusting control chart system of this nature has not as yet been applied to accident rate processes.

Computer solutions will also make practical another new and potentially important control chart methodology, namely, to maintain concurrently within the same enforcement or engineering jurisdiction a number of control charts of different sensitivities. One control chart of given sensitivity might be directed toward short-range operational decisions, another of different sensitivity might be for long-range decisions. In one sense, this is tantamount to treating continuing accident data from a number of different, although possibly overlapping, directions. Some of the resulting indications might be contradictory, but an overall gross indication should nonetheless emerge if there is some reason for such an indication. One must always bear in mind that control chart techniques serve virtually no useful purpose for sharply defined changes in accident causation processes. Instead, their primary justification is for hazy situations in which gradual changes in accident causation are suspected. In such hazy situations, with no sharply defined yeses or noes, it becomes necessary to glean hints and suggestions from as many directions as possible.

The authors would strongly emphasize here that any control chart system for interpreting successive variations in accident experience is, at best, an inexact process, although much more exacting than simple "seat of the pants" analyses of such data. In the same vein, it almost would be idle to hope that any kind of statistical treatment would come up with precise yes or no answers to specific operational problems. Instead, control charts will produce hints and indications of underlying changes in accident causation and the associated appropriateness or inappropriateness of given operational decision alternatives. They thereby produce additional inputs to operational decision-making, but nevertheless do not comprise the whole of the process.

CONTROL CHARTS ON ACCIDENT RATES

The discussion thus far has dealt with control charts based on raw numbers of accidents, and has not considered charts based on accident rates. The overall logic is essentially the same; nevertheless, several problems of control chart application arise according to the nature of the exposure and procedures used to estimate it.

Let us define an accident rate, R_t, in time period t as the number of observed accidents of a particular classification, Y_t, per unit of accident exposure, E_t:

$$R_t = Y_t/E_t \tag{3}$$

When the exposure is constant from time period to time period, the distribution of R_t (when Y_t is Poisson) has mean m/e and variance m/e^2. When m is large (say, greater

than 25 accidents/period), it is possible to approximate the rate control chart limits by asymptotic expressions. In virtually all prior applications of control chart techniques to accident data, asymptotic expressions have been used for both rate and raw accident limits. These expressions, however, do not hold for small m; in such cases in order to determine rate control limits, we must know the form of the distribution of R_t (which is not Poisson even when E_t is constant from period to period).

With E_t a random variable (i.e., traffic density, vehicle miles), we are faced with other problems. For instance, what is the distribution of E_t? Are Y_t and E_t statistically independent? What then is the distribution of R_t? For many of the widely used exposure measures, questions such as these are largely unanswered.

We ask these questions because it is our contention that in many selective enforcement problems operational constraints dictate that we deal with small expectations—thus precluding the use of asymptotic expressions for either rate or raw accident control limits. For example, with a desired (high) control chart sensitivity, we will usually find that in order to use such expressions to determine control limits, we need either inordinately long roadway sections, long control chart time periods, or both. Furthermore, the results from the ensuing analyses would prove virtually useless for basing operational decisions. These remarks become particularly pertinent in dealing with control charts based on finely classified accident data.

Remarks can also be made concerning the exposure measure itself. There still is no measure that cannot be validly criticized on some account. Even if mileage is obtained by direct odometer readings, one could question the absence of normative observations of the time of day or day of week in which the odometer values were generated. If these normative data were available, one could question the absence of additional descriptions of the type of driving, e.g., the relative demands on freeways vs surface streets.

Without deprecating the importance of detailed multidimensional descriptions of exposure data, a somewhat different point of view is suggested for operational purposes. This is to base all initial statistical descriptions of accident causation process on raw totals of the accident events, with no attempts to operate on these data with any form of randomly varying exposure information such as mileage. Subsequently, exposure information could be advanced as possible explanations of fluctuations of the raw accident data.

Several factors support this logic. First, the mathematical theory is clearly developed for treating raw accident data, but not for treating accident rates having exposure as a random variable. Second, manipulating relatively exact raw accident totals with less exact, if not totally spurious, exposure data can readily confound interpretations without producing any reliable new insights.

Finally, it appears that most operational countermeasures ultimately reduce to responses to raw accident experience anyway. Consider the example of a given enforcement beat on which the raw numbers of accidents are high. Even if the accompanying traffic volumes were also high, so that a low mileage-based rate resulted, substantial manpower would nonetheless be allocated to that beat. This is to say that high accident losses will produce operational action regardless of whether or not, with exposure manipulations, the losses are represented by statistically low rates.

This leads to the very challenging proposition that the "accident causation process" is the totality of all exposure. The raw accident losses should be considered as measuring the multidimensional quantity and quality of exposure. Personal characteristics of drivers as well as physical characteristics of vehicles using a roadway become a part of that roadway's exposure, along with the inventory of weather conditions encountered by the motorists. The amount of drunkenness in the set of drivers using a particular roadway is as much a part of that roadway's exposure measure as the raw volumes of vehicles. (It should be noted that Eq. 3 implies that the multidimensional space of factors which contribute to the exposure have been reduced to a single random variable E_t.)

A high accident experience becomes an indication of something amiss in the antecedent exposure hyperspace; a statistically significant change in accident experience is an indication of change in some aspect of the exposure hyperspace. The problem reduces

to isolating where the exposure change is occurring and directing operational action to it. One may even argue that all operational actions can be considered to be directed toward influencing or changing some aspect of the exposure hyperspace.

The implications of this proposition are more far-reaching than may be immediately apparent. It leads to replacing present rate-based accident statistics with raw accident data, unmodified by any exposure data, as the primary statistical methodology for operational purposes. It precludes any deliberate or inadvertent overlooking of inherently dangerous environments as a result of operating on this accident data with sound or unsound exposure data. It identifies accident experience as such, clearly and unambiguously.

Speculations as to how these losses relate to the antecedent exposure base are a secondary aspect of the analytical process. Correlations between the losses and the exposure (hyperspace) presumably, of course, would strongly dictate appropriate operational measures. However, failure to isolate such correlations nonetheless would not preclude proper recognition of accident loss magnitudes as such, and changing patterns in these magnitudes.

In scientific history, many phenomena have been observed well before acceptable theories were developed to explain the phenomena. This is somewhat analogous to the situation here. Regardless of whether or not some exposure argument reduces a high accident loss experience to a low rate, operational personnel will take appropriate corrective actions. The proposition of developing raw accident data methods reflects this highly rational operational practice.

CONCLUSION

The use of control chart techniques for accident data is not new. Such usage has been suggested by a number of authors, and on the surface, is almost a natural for operational personnel whose decision-making requires them to be able to detect stable patterns—if such patterns exist—out of what otherwise are largely random fluctuations of accident experience from one period to the next, or from one roadway length to the next. This is precisely the primary purpose of control charts.

However, these techniques are not in widespread use today, if they are being used at all. We may offer several possible explanations. One is that the underlying mathematical logic has not been fully translated into the rather immediate operational needs, such as how the user should select the length of roadway and the period of time of his control chart system.

A more important reason is that the precise rules of the α and β errors in accident control chart applications have never been fully enunciated. In fact, at the outset of this effort we were utilizing the conventional 5 percent α error, and were finding that the resulting control chart systems were virtually worthless for the roadway under study. The charts would show the obvious, sharp changes in accident experience, but would be insensitive to the more subtle changes. Clearly, operational personnel do not need, nor have the time to bother with, statistical methods that do no better than they can do by simple examination.

The key to practical usefulness of these techniques for accident data analysis is, in our opinion, the β error. Conventional statistical experiments with low α error generally must have large sample sizes to realize low β error. Economic constraints will generally dictate the sample size and in most cases rule out the experiment with low β error. As a result, it is rather common for the scientist, rather than reporting a spurious discovery, to say that he has discovered nothing. This approach is questionable in many areas of accident data analysis where the cost of false detection is relatively insignificant when compared to the cost of failing to detect a problem. Our work to date strongly suggests that for many accident data uses a high α error probability, even 40 percent or larger, should be tolerated, if necessary, to realize a low β error concomitant with realistic values of T, L, and K.

The question of optimum trade-off between α and β error leads directly into another important area of inquiry, namely, that of determination of the value or cost associated with making each type of error. An accompanying issue is the value or cost of each

possible countermeasure that might be implemented following a (control chart) indication of need. Combining such value systems with the statistical measure implicit in the α, β, K choice is immediately suggested as the next major area of study. This combination can lead to important new cost/effectiveness methods.

Another related line of inquiry is illustrated in this example: A unit commander has to allocate his manpower to the beats and shifts under his jurisdiction, regardless of whether or not stable statistical patterns can be discovered. At present, he does this without the benefit of control chart methods. When he starts to use control chart methods, he undoubtedly often will be in the same situation, namely, no statistically significant indications of causation changes. Here the line of inquiry would deal with how the operational decision-making process should function in the face of no evidence from the control chart methods.

ACKNOWLEDGMENTS

This is a summary of a report on one of a number of projects that comprise a continuing five-year research program on highway safety of the California Transportation Agency, Robert B. Bradford, Administrator.

Justin J. DuCray provided needed administrative liaison throughout this course of the work. Charles G. Beer of the California Division of Highways and William H. Oliver of the U.S. Bureau of Public Roads also assisted with administrative matters.

The work was performed in cooperation with the California Highway Patrol, Bradford M. Crittenden, Commissioner. Robert A. Bieber materially assisted, both in describing statistical practices of the Patrol to project staff, and in making certain data available.

The authors also wish to express their appreciation to A. V. Gafarian, System Development Corporation, for his critical review and suggestions.

Appendix A

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The bibliography is partitioned into nine categories according to the major emphasis of the references as they pertain to the work on this project.

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Appendix B

MATHEMATICAL DERIVATIONS

Topic One: Hypotheses and Decision Rules for the Three Control Chart Systems

Let the number of accidents Y_t observed on a fixed-length roadway in the t th time period of length T be Poisson distributed with parameter λ. We shall consider three separate control chart systems, each based on hypotheses concerning the parameter λ. Systems A and B are based on a one-sided argument; System C, on a two-sided argument.

1.1 The Systems

The null hypothesis H_0 and alternative H_1 for each system are as follows:

System A: Detection of a "worsening" in the causation process

$$H_0: \lambda = \lambda_0$$

$$H_1: \lambda = \lambda_1, \ (\lambda_1 > \lambda_0)$$

where

$$\lambda_1 \triangleq \lambda_0 + K\lambda_0, \ 0 < K < \infty$$

System B: Detection of an "improvement" in the causation process

$$H_0: \lambda = \lambda_0$$

$$H_1: \lambda = \lambda_1, \ (\lambda_1 < \lambda_0)$$

where

$$\lambda_1 \triangleq \lambda_0 - K\lambda_0, \ 0 < K < 1$$

System C: Detection of either "improvement" or "worsening" in the causation process

$$H_0: \lambda = \lambda_0$$

$$H_1: \lambda = \lambda_1 \ (\lambda_1 \neq \lambda_0)$$

where

$$\lambda_1 \triangleq \lambda_0 \pm K\lambda_0$$

For each of the three control chart systems we shall insist that the following probability statements hold:

$$\Pr\{\text{accept } H_0 \mid H_0\} = 1 - \alpha \tag{1.1.1}$$

$$\Pr\{\text{accept } H_0 \mid H_1\} = \beta \tag{1.1.2}$$

1.2 The Decision Rules

The decision rules for each control chart system are as follows:

System A

If the observed value of Y_t in period t of length T is:

$$y_t < a \begin{cases} \text{accept } H_0 \text{ and conclude no change} \\ \text{in the causation process in period } t \end{cases}$$

$$y_t \geq a \begin{cases} \text{reject } H_0 \text{ and conclude a worsening} \\ \text{in the causation process in period } t \end{cases}$$

where a is the least integer such that

$$\sum_{y=a}^{\infty} \frac{e^{-\lambda_0} \lambda_0^y}{y!} \leq \alpha \tag{1.2.1}$$

is satisfied.

System B

If the observed value of Y_t in period t is:

$$y_t > b \begin{cases} \text{accept } H_0 \text{ and conclude no change} \\ \text{in the causation process in period } t \end{cases}$$

$$y_t \leq b \begin{cases} \text{reject } H_0 \text{ and conclude an improvement} \\ \text{in the causation process in period } t \end{cases}$$

where b is the largest integer such that

$$\sum_{y=0}^{b} \frac{e^{-\lambda_0} \lambda_0^y}{y!} \leq \alpha \tag{1.2.2}$$

is satisfied.

System C

If the observed value of Y_t in period t is:

$$c < y_t < d \begin{cases} \text{accept } H_0 \text{ and conclude no change} \\ \text{in the causation process in period } t \end{cases}$$

$$y_t \geq d \begin{cases} \text{reject } H_0 \text{ and conclude a worsening} \\ \text{in the causation process in period t} \end{cases}$$

$$y_t \leq c \begin{cases} \text{reject } H_0 \text{ and conclude an improvement} \\ \text{in the causation process in period t} \end{cases}$$

where c and d are the largest and smallest integers, respectively, such that

$$\left[\sum_{y=0}^{c} \frac{e^{-\lambda_0} \lambda_0^y}{y!} \leq \frac{\alpha}{2}, \sum_{y=d}^{\infty} \frac{e^{-\lambda_0} \lambda_0^y}{y!} \leq \frac{\alpha}{2} \right] \tag{1.2.3}$$

are satisfied.

For each control chart system the probability β of accepting H_0 when H_1 is true (1.1.2) is given by:

System A

$$\beta_A = \sum_{y=0}^{a-1} \frac{e^{-\lambda_1} \lambda_1^y}{y!} \tag{1.2.4}$$

System B

$$\beta_B = \sum_{y=b+1}^{\infty} \frac{e^{-\lambda_1} \lambda_1^y}{y!} \tag{1.2.5}$$

System C

$$\beta_C = \sum_{y=c+1}^{d-1} \frac{e^{-\lambda_1} \lambda_1^y}{y!} \tag{1.2.6}$$

Topic Two: The Determination of the Control Chart Time Period for a Fixed-Length-Roadway Chart

In the following discussions we shall only consider System A, as similar reasoning applies for the other systems.

Consider the space $\{\lambda_0, \alpha, \beta, K\}$ obtained by solution of Eqs. 1.2.1 and 1.2.4, where β is a dependent variable and λ_0, K and α are allowed to vary over a respective set of values. Entering the space with a particular set of values of α, β, and K (say, α^*, β^*, K^*) we may find a λ_0^* which can be interpreted geometrically as the value of λ_0 at which the lines $\alpha = \alpha^*$, $\beta = \beta^*$, $K = K^*$ intersect the surface of λ_0. In terms of the accident causation process, λ_0^* is the expected number of accidents in time period of length T that is necessary to insure a Type I error of magnitude no greater than α^*, and a Type II error of magnitude β^* associated with a K^* increase in the parameter λ.

Thus, if m is the expected number of accidents per unit time with estimate $\hat{m}$, the control chart time period, T, that is necessary to insure a chart of strength at least (α^*, β^*) is given by:

$$T_{\alpha^*, \beta^*, K^*} = \frac{\lambda_0^*}{\hat{m}} \tag{2.0.1}$$

2.1 Graphical Representation of the $\{\lambda_0, \alpha, \beta, K\}$ Space for a Particular Control Chart System

Graphical representation of the $\{\lambda_0, \alpha, \beta, K\}$ space presents a convenient medium for determining a λ_0^* associated with a given (α^*, β^*, K^*) triplet. With λ_0^* determined it is then a simple matter to determine the control chart time period $T_{\alpha^*, \beta^*, K^*}$.

Consider a graphical representation in which α is taken as a page parameter and β vs K is plotted for set values of λ_0. Rewriting Eq. 1.2.1 as an equality relationship, we have

$$\sum_{y=a}^{\infty} \frac{e^{-\lambda_0} \lambda_0^y}{y!} = \alpha - \epsilon \tag{2.1.1}$$

where α is fixed, ϵ is a function of λ_0, and a is the least integer such that Eq. 1.2.1 holds.

As λ_0 tends to infinity, ϵ in Eq. 2.1.1 tends to zero. On the other hand, for small values of λ_0, ϵ can become appreciable in comparison to α. In practical terms, this behavior means that for a preset alpha and λ_0, a value y = a cannot be found that will make the summation in Eq. 2.1.1 exactly equal α. One effect of this behavior is that when α in Eq. 1.2.1 is taken as a page parameter, given values of K, α, β (say, K^*, α^*, β^*) do not necessarily lead to a unique value of λ_0^* on the graphical plots. Another effect is that a determined value of λ_0^* might be larger than is necessary to maintain a preset control chart strength. Practically, this means that the resulting control chart time period will tend to be overly conservative.

One way of circumventing these problems is to develop the graphs using an exact alpha test. This development is as follows.

In Eq. 1.2.1 with fixed values of α and λ_0, determine the value y = a, where a is the least integer such that Eq. 1.2.1 holds. The value of ϵ in Eq. 2.1.1 can then be determined by directly evaluating the summation in Eq. 2.1.1 between the limits (a, ∞) for the fixed values of α and λ_0. Thus, an observed value of Y_t that is equal to or greater than a will always fall within the region of rejection of H_0. To bring the size of the critical region exactly to alpha we include the term for y = a - 1 with probability p, where:

$$p\left[\frac{e^{-\lambda_0} \lambda_0^{a-1}}{(a-1)!}\right] = \epsilon \tag{2.1.2}$$

Thus, beta in Eq. 1.2.4 for the exact alpha test becomes:

$$\beta_A = \sum_{y=0}^{a-2} \frac{e^{-\lambda_1} \lambda_1^y}{y!} + \left[\frac{e^{-\lambda_1} \lambda_1^{a-1}}{(a-1)!}\right] \tag{2.1.3}$$

The decision rules for System A in Section 1.2 are then modified for the exact alpha test as follows:

1. An observed value $y_t \geq a$ always leads to rejection of H_0.
2. An observed value $y_t \leq a - 2$ always leads to acceptance of H_0.
3. An observed value $y_t = a - 1$ leads to rejection of H_0 with probability p and acceptance of H_0 with probability $(1 - p)$, where p is determined in Eq. 2.1.2. This process can easily be carried out with a table of random numbers.

If the value system associated with the choice of α^* and β^* is quantified, the determination of λ_0^* based on an exact alpha test procedure can be accomplished solely by an iterative computer technique, thus eliminating graphical representation for all but instructional purposes. Otherwise, the use of a graphical representation appears to be a more practical approach to the determination of λ_0^*.

It is noted that in the descriptive examples in the main body of this report, a graphical representation based on Eqs. 1.2.1 and 1.2.4 was used in the determination of λ_0^*. The results of the examples, while correct, tend to be overly conservative; i.e., the time periods as calculated yield control charts of strengths exceeding the requirements of the specified (α^*, β^*, K^*) triplet. This, in addition to the non-uniqueness problem associated with the direct use of Eq. 1.2.1, lead to the exact alpha test procedure subsequently presented.

Topic Three: The Determination of the Roadway Length for a Fixed-Time-Period Control Chart

The problem of determining the roadway length for a fixed-time-period control chart is essentially similar to the problem discussed under Topic Two.

Let the number of accidents observed in fixed time period on a roadway of length L be Poisson with parameter λ. With this definition of λ in mind, the control chart systems discussed in Section 1.1 of Topic One apply directly. The decision rules of Section 1.2 apply with the modification that acceptance of H_0 leads to the conclusion that no change occurred in the causation process for the roadway of length L during fixed observation period T, and rejection of H_0 leads to the conclusion that a change in the causation process occurred for the roadway of length L during fixed time period T.

By thus specifying the (α^*, β^*, K^*) triplet together with the appropriate control chart system, it is possible to find a λ_0^* associated with the triplet (α^*, β^*, K^*). The exact alpha test procedure discussed in Section 2.1 applies directly to this case.

Thus, if m', with estimate $\hat{m}'$, is the expected number of accidents per unit length of roadway observed from actual accident experience on a time period of length T basis, the roadway length necessary to insure a fixed-time-period control chart of strength (α^*, β^*) for a K^* change in λ is

$$L_{\alpha^*, \beta^*, K^*} = \frac{\lambda_0^*}{\hat{m}'} \tag{3.0.1}$$

Topic Four: Optimum Trade-Off Between the Time Period and Roadway Length Subject to a Given Set of Constraints

Let us define the following value functions:

$U_T \triangleq f_1(T_i) \triangleq$ the value as a function of T_i of a control chart system of strength (α^*, β^*) when the control chart time period is T_i (4.0.1)

$U_L \triangleq f_2(L_i) \triangleq$ the value as a function of L_i of a control chart system of strength (α^*, β^*) when the roadway length is L_i (4.0.2)

Additionally, we define the following constraints on L_i and T_i:

$$T_i \leq T_{max} \tag{4.0.3}$$

$$L_i \leq L_{max} \tag{4.0.4}$$

If both constraints in Eqs. 4.0.3 and 4.0.4 can be satisfied for at least one paired value of T_i and L_i, the optimum trade-off between T and L for a control chart of strength (α^*, β^*) may be found by optimizing the function

$$f(T, L) = U_T T_i + U_L L_i \tag{4.0.5}$$

subject to the constraints of Eqs. 4.0.3 and 4.0.4.

T_i and L_i in Eq. 4.0.5 are paired-values of time period and roadway length that insure a given control chart strength (α^*, β^*) associated with a K^* change in λ. A pair (T_i, L_i) is determined by evaluation of either Eq. 2.0.1 or 3.0.1 depending on whether T or L is considered fixed. The value $T_i = T^*$ and $L_i = L^*$ that maximize Eq. 4.0.5 thus yield the optimum trade-off of T and L for a control chart system of fixed strength (α^*, β^*). It is beyond the scope of this Appendix to discuss methods of maximizing Eq. 4.0.5.

The case might arise where T_{max} and L_{max} are so chosen that constraints in Eqs. 4.0.3 and 4.0.4 cannot be simultaneously satisfied for any paired values of T_i and L_i. In this case it is still possible to obtain "pseudo-optimum" trade-offs if we relax the appropriate constraint equation.

The regions of relaxation are depicted on an (L, T) space in Figure 5.

Topic Five: A Variable Weighting Scheme for Estimating the Accident Expectation

Consider the following weighting scheme (Brown, 1963, p. 101) for estimating the parameter λ of the accident process:

$$m = W \sum_{j=0}^{N-2} \left[(1 - W)^j y_{t-j-1}\right] + (1 - W)^{N-1} y_{t-N} \tag{5.0.1}$$

where

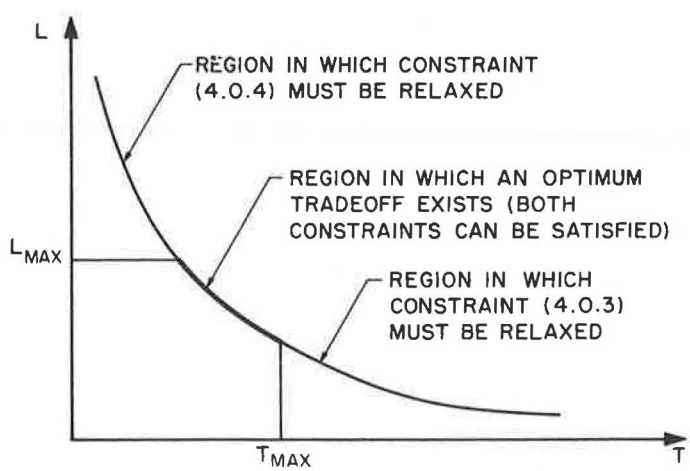

Figure 5. Regions of optimum and pseudo-optimum trade-offs in the {L, T} space.

$W \triangleq$ a weighting constant,

$N \triangleq$ the number of past time periods in estimating λ,

$t \triangleq$ the time period of interest,

$j \triangleq$ a dummy variable, and

$y_i \triangleq$ number of accidents observed in the i th time period.

The weight given to historical accident data decreases geometrically with the age of the data. With large values of W, data in the near past are strongly weighed in favor of further removed accident data, thus causing the estimate m to be highly sensitive to rapid fluctuations in the barometer of the accident process.

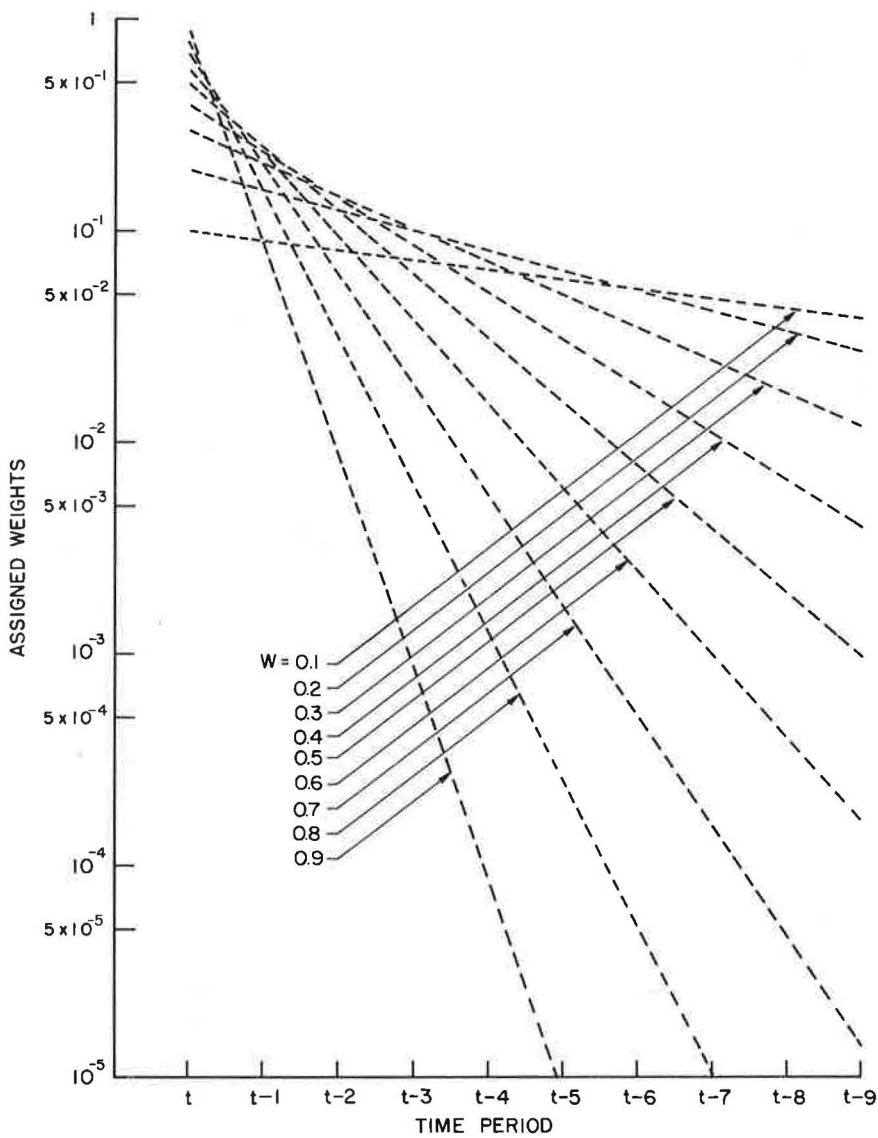

Figure 6. Assigned weightings as a function of weighting constant W and time period (age of data).

With small values of W, the estimate m behaves like the average of a large amount of historical accident data, and thus is less highly sensitive to rapidly developing changes in the barometer of the accident process than it would be if a large value of W were used. Figure 6 shows the variation of weights assigned to historical accident data as a function of age for weighting constants of 0.1(0.1)0.9.

Discussion

B. J. CAMPBELL, Director, The University of North Carolina Highway Safety Research Center—I appreciate the opportunity to comment on this attack on the problem of bringing statistical techniques to bear on the operational problems of accident control. I do not feel competent to judge the adequacy of the derivations and the statistical theory underlying this paper, but I have confidence in the authors' excellent reputations and am sure that they have buttressed their arguments with care. I hope that their work will continue to be supported.

Translating the best that science and mathematics can offer into a useful operational tool is not easy. In the present paper, it is clear that selection of values of alpha, beta, time, beat length and loss expectation (and any weighting system used) must be optimized, and I daresay that this can be accomplished only after extensive trial analysis of many data sets.

The authors mention a weighting scheme, one in which recent accident events count more heavily in the chart performance than more distant events. This weighting scheme applies to past accident experience as a means of calculating loss expectation. The procedure recognizes that some events are more relevant than others to detecting the system being "out of control." Other examples could be considered. If the chart is being used to guide police enforcement, then one might consider that accidents precipitated by flagrant violation of the law perhaps should count for more than other accidents, and thus entry of such an accident in the chart should have more effect in bringing the situation to an "out of control" state. The idea of dealing with sequential events in a way that takes account of degree of relevancy to the total situation is being studied at the Cornell Aeronautical Laboratory, and I suggest that the authors might contact Dr. Kihlberg of the Transportation Research Department there. The work of Dr. Kihlberg and the work of the present authors might harmonize usefully.

I would like to question whether there is a danger that this control chart system may give a false indication that an improvement has been effected. For example, if the accident situation on a given enforcement beat increases and the accidents go "out of control," and if an operational change in level of enforcement is made to bring the situation under control, is there not a likelihood that the subsequent time period has a high probability of showing an "improvement" because of a "regression to the mean" phenomena. If this is a theoretical problem, perhaps it is not serious in an operational context using proven countermeasures. However, in our field we have so few proven countermeasures and often we are in the process of evaluating rather than simply applying a countermeasure.

Finally, I would like to comment on the appealing possibility that we might after all be able to use accident frequency to make program decisions rather than having to deal with the troublesome problem of obtaining exposure data so as to generate accident rates. The reasoning goes something like this. Suppose that we have one road segment that causes ten deaths during a given time despite a low rate (but of course a high volume) on that road. We ask if this segment is more worthy of action than a second road segment that causes five deaths during the same period at a higher rate but a much lower volume. After all, we are trying to prevent accidents. There is, I agree, much appeal to this but I would like to express some doubts. For one thing, the frequency of accidents in a given place at a given time is just one quick look at a dynamic moving phenomena. For example, five fatal accidents per unit time on a high-rate facility with

a low volume is a transient characteristic, and in a period of rapidly growing vehicle exposure the situation can change almost before there is time to impose effective countermeasures.

Another consideration has to do with the effect of the countermeasures not only on accidents, but on the whole stream of traffic. Suppose we have ten accidents on one facility, a low-rate high-volume facility, and eight accidents on another facility, a high-rate but low-volume facility. If we decided that our countermeasure was to be a very substantial reduction in allowable speed, then application of this countermeasure to the low-rate high-volume road would aim at somewhat more accidents but would also inconvenience large numbers of travelers. In this particular example, it seems that considering not only accident suppression but minimizing inconvenience to the non-accident traffic stream is necessary, and would dictate working on the high-rate but lower-volume facility.

As I have said, this paper represents an important and productive project that has already shown good results, and will no doubt produce more through the further exercise of theories being developed therein.

R. BRENNER, G. R. FISHER and W. W. MOSHER, JR., Closure—The authors would like to take this opportunity to thank B. J. Campbell for his pertinent remarks and suggestions concerning this paper. In his discussion, Campbell raised the question as to whether there would be, following an "out of control" situation, a high probability of the control chart system falsely indicating an "improvement" in the subsequent time period due to (a) operational changes in level of enforcement, and (b) "the regression to the mean" phenomena. We can briefly reply to his question in two steps.

Regarding the effects of operational changes in level of enforcement, two assumptions are implicit: that level of enforcement directionally affects accident generation and that the time lag associated with the effect of a change in enforcement is comparable to the control chart time period. Assuming these two assumptions hold, an "improvement," if detected by the system, would be an actual improvement.

If the term "regression to the mean" is interpreted as the tendency of an estimate to lag behind the true mean in a changing process, then there would be an increased likelihood of falsely detecting an "improvement," if the data point for the "out of control" time period were included in the set of sample values used to generate the estimate. However, since we do not include this "out of control" data point, "regression to the mean," as we have interpreted it, should not pose a problem in control chart applications.

Campbell also commented on the use of weighting schemes to estimate accident expectation. It should be pointed out that the choice of a particular weighting scheme generally arises from statistical considerations. This should not be confused with other measures which, for example, can be used to differentially weigh separate types of accidents as to their degree of importance or relevance in particular operational problems.

Campbell's final remarks are pertinent to the manpower allocation process in general and point out the necessity of evaluating trade-offs between sometimes conflicting goals and the necessity of considering value criteria in a rational approach to manpower deployment. It should be noted that the authors are presently engaged in the development of optimum allocation models utilizing the statistical techniques reported in this paper.