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LETTERS TO THE EDITOR

Letters should be addressed to the Editor, Graduate School of Business, 401 Uris Hall, Columbia University, New York, New York 10027.

A Statement Concerning the Evaluative Function in Government

I would like to comment on "Social Change and the 'Evaluation Function' in Government," guest editorial by Mr. Nicholas E. Golovin, *Management Science*, Volume 15, Number 10, dated June 1969.

Mr. Golovin makes an excellent case for an objective "evaluation function" or "long range think" organization in government. However, I would like to question the desirability of a new 'fourth' branch of government.

From my limited knowledge of English History it would appear that their House of Lords did serve such an evaluative function for a number of years. Granted that it is not now, nor has it served this objective evaluative function for quite some time. Perhaps the U. S. Senate, with appointments by state legislators, longer terms of office, etc. was set up initially to fulfill a similar objective evaluative function. Certainly Senator J. William Fulbright is now performing in the fashion outlined by Mr. Golovin.

A unicameral legislature is serving now, effectively, the State of Nebraska as well as countries utilizing the parlimentary system.

Rather than creating a fourth branch of government it may be desirable to, through constitutional amendment(s) if desirable, change the United States Senate to an evaluative function. Points to consider would be longer terms of office, perhaps some criteria of accomplishment and/or ability for membership, expansion of its information collection capabilities, etc.

Finally, my experience supports Mr. Golovin's point in §8 that such a basic approach can be applied to the management of large enterprises of any kind. Better managed business organizations have in the past, and do now, use such functional groups (not necessarily so identified), e.g., financial committees of boards of directors.

In summary, the arguments for an objective, evaluative function in government are strong. The only question is how the organization can be accomplished in order to maintain the function's objectivity, hopefully permanently.

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A Note on the Determination of Optimal Forecasting Strategy

In management science literature and in managerial practice, the problem of comparing competing alternative techniques for forecasting such variables as sales, earnings per share etc. has become of major interest. In recent years a myriad of alternative techniques have been proposed to forecast the same variable. For example, sales forecasting procedures for forecasting distributor orders range from exponential smoothing techniques, spectral analysis approaches, and simulation models, to retail and wholesale demand sampling schemes. Each of these techniques costs a different amount to operate, provides a different amount of ancillary information[1] (aside from the forecast) and results in different forecast error distributions.

The current state of the art is reflected in a study by Schussel ("Sales Forecasting with a Human Simulator," *Management Science*, Vol. 13, (June 1967)) to compare alternative methods for forecasting sales. Along with many others, Schussel compares forecasting procedures "intuitively," using some commonly accepted statistics based on forecast errors made in the past. I use the word "intuitively," in the sentence above, to point out that an approach such as Schussel's can make no claims about the statistical significance of the statistics used. Furthermore, although the statistics used imply definite assumptions about the cost of forecasting errors, the *expected* costs of competing forecasting procedures cannot be calculated.

The basic premise of Schussel's approach (which selects a single procedure as best) is as follows:

> If one forecast method is best as judged by each of several different statistics that were derived from different (cost) assumptions, then we can feel fairly confident about its superiority under general circumstances (p. B606).

But *only in exceptional cases will a single forecast method be judged best by* all *statistics used to measure forecast effectiveness*. In adopting such an approach, the investigator assumes erroneously that the best forecasting strategy always consists of selecting a single forecasting procedure.[2] He also leaves, in effect, the evaluation of his experimental results up to the individual reader.

In practice, it often becomes difficult to achieve consensus as to what results mean. Rational men can easily disagree; witness the recent series of papers arguing the predictive power of alternative methods for forecasting quarterly earnings of corporations (The First of this series appeared in the *Journal of Business*, Vol. 40 (January 1967), pp. 44–45). Admittedly only part of the disagreement in the above papers resulted from the fact that the type of criterion as suggested by Schussel failed to work. But two of the authors, Green and Segall, do explicitly admit in their study that

> We do not regard the differences in accuracy (between alternative forecasting methods) as particularly clear cut or dramatic, *yet we remain uncertain about how the differences can be evaluated* (p. 30).

Schussel also falls victim to this difficulty in his paper. The statistics he uses do not provide the unanimity he had hoped for.

In sum, there is a need for a comprehensive methodology for determining optimal forecasting strategy. In particular, we need a methodology that allows us to make probabilistic statements about the economic worth of alternative forecasting procedures.

A Decision-Theoretic Formulation of the Problem. The problem of constructing optimal forecasting procedures can be formulated symbolically as follows. If we characterize each forecasting procedure as being made up of the sum of two random variables, one representing actual sales (A) and the other representing forecast error

[1] If the manufacturer uses an extensive information gathering system, such as a sampling scheme, to collect data on inventories, sales, etc. of products in the distribution system, then aside from being helpful in forecasting, the data also can be invaluable to marketing.

[2] In (Peterson, Rein, *An Optimal Control Model for Smoothing Distributor Orders: An Extension of the H.M.M.S. Aggregate Production—Work Force Scheduling Theory*, unpublished, Ph.D. Dissertation, Cornell University, 1969) we show that in general the best forecasting is *some combination* of alternative forecasting procedures.

$(Y^{(s)})$, then the forecasts $(F^{(s)})$ generated by a set of p alternative forecasting procedures (s) can be written as follows:

$$F^{(s)} = A + Y^{(s)}; \qquad s = 1, 2, \cdots, p.$$

Allowing for the possibility that the optimal action may be to choose some *linear combination* of alternative forecasting methods, we can denote the optimal procedure, $F^{(D)}$ as follows:

$$F^{(D)} = \sum_{s=1}^{p} F^{(s)}\alpha_s; \qquad \sum_{s=1}^{p} \alpha_s = 1, \qquad \alpha_s \geqq 0.$$

Note that if the optimal procedure consists of a single forecasting procedure then $\alpha_s = 0$ for all s but one. We will assume that $E(Y^{(s)}) = 0$ for all s. Presumably any forecast error bias, once it is detected, can be effectively removed by increasing or decreasing the forecasts made by the amount of the bias. The unbiasedness assumption for each procedure along with the restriction $\sum_1^p \alpha_s = 1$ gives us the desirable property that the optimal forecast will also be unbiased:

$$\begin{aligned} E(F^{(D)}) &= E(\textstyle\sum_{s=1}^{p} F^{(s)}\alpha_s) \\ &= \textstyle\sum_{s=1}^{p} (E(A\alpha_s) + E(Y^{(s)}\alpha_s) \\ &= E(A). \end{aligned}$$

The problem of constructing optimal forecasting procedures can then be stated succinctly as follows. Let $F^{(1)}, F^{(2)}, \cdots, F^{(p)}, A$ be a set of $p + 1$ random variables, where $F^{(s)}$ represents $s = 1, 2, \cdots, p$ alternative forecasting procedures and A the actual value of the variable being forecasted. Let X be the matrix of observations on the set of $p + 1$ random variables over m time periods in the past.

$$X = \begin{bmatrix} F_1^{(1)} & F_1^{(2)} & \cdots & F_1^{(p)}, A_1 \\ F_2^{(1)} & F_2^{(2)} & \cdots & F_2^{(p)}, A_2 \\ \vdots & & & \vdots \\ F_m^{(1)} & F_m^{(2)} & & F_m^{(p)}, A_m \end{bmatrix}$$

Similarly, let Y be the matrix of future (unknown at present) observations on the set of $p + 1$ random variables $F^{(1)}, F^{(2)}, \cdots, F^{(p)}, A$ up to a decision horizon n. Then the decision problem can be stated as one of constructing $F^{(D)}$ by specifying the set $P = \{\alpha_s; s = 1, 2, \cdots, p\}$ after having observed X, but before observing Y, given a joint probability function $F(Y)$ and a loss function $W(Y, P)$.

The selection of an optimal forecasting procedure $F^{(D)}$ in such a way is in theory a straightforward problem, but operationally it remains difficult. In theory, to establish procedure $F^{(D)}$ as "best" we must demonstrate that the *optimal use* of the procedure will result in the *lowest expected total cost in use* up to some future decision period n. Total cost in this context is defined usually to include both the cost of obtaining a forecast and *the cost in use* of the forecasting errors made. In practice, however, the application of this apparently simple total expected cost criterion becomes complex, as we will see below. The cost of operating a forecasting procedure has not been considered explicitly in most formulations in the past.

Symbolically, the decision-problem can now be stated as follows. Choose the set $P = \{\alpha_s; s = 1, 2, \cdots, p\}$ which minimizes the following expectation:

$$E\text{ (Total Cost)} = \int_y W(Y, P)f(Y)\,dy + E\text{ (Cost of operating } P\text{)}.$$

The minimization of the above equation with respect to P is in general intractible.

Some Operational Problems. Basically three methodological difficulties arise. First, in many practical applications, we lack well-defined loss functions, $W(Y, P)$. As a result, it is often assumed (as a simplification) that the cost of forecast errors varies directly with some statistic like the average percentage error of forecast, the average squared percentage error, the error variance, etc. But even in these cases the actual cost in terms of dollars and cents is seldom specified. Nevertheless, it is imperative that all costs be made explicit. To trade off the cost of obtaining forecasts by alternative methods, versus lower or higher expected costs of errors in use, we must be able to evaluate explicitly the expected cost of forecast errors.

Secondly, competing forecasting procedurés will generate forecasts over the same period of time, using as input, information that is identical for all procedures. As a result, the characteristics of the error distributions for each of the procedures will be dependent and usually positively correlated. Unless we are willing to make some very specific a priori assumptions, we lack at present appropriate powerful statistical tools to test whether two error distributions are statistically significantly different, *when the error distributions are dependent to an unknown extent.* Specifically in the case of comparing alternative forecasting procedures the following relationships usually exist among forecasting errors:

(1) $Y_i^{(s)}$ is not independent of $Y_i^{(t)}$ for all s, t.

(2) $Y_i^{(s)}$ is independent of $Y_{i+1}^{(s)}$ for all i.

Note that if we let the forecast errors made by procedures 1 and 2 be represented by $X^{(1)}$ and $X^{(2)}$, then we cannot compare the Var $(X^{(1)})$ and Var $(X^{(2)})$ by the usual analysis of variance techniques, such as the F-test, because the two variances are dependent.[3]

Goodness of fit techniques such as the Chi-Square test and the Kolmogorov-Smirnov statistic cannot be used either because they also require the assumption that the two distributions of forecast errors compared be independent. Specifically these tests asume that:

(1) $X_i^{(1)}$ is independent of $X_j^{(2)}$ for all i, j.

(2) $X_i^{(s)}$ is independent of $X_{i+1}^{(s)}$ for all i; $s = 1, 2$.

The latter assumption, that the samples are independently drawn from the population is an assumption which underlies most statistical tests, parametric or nonparametric. There are also appropriate tests available (for example the t-test) if we are willing to assume that the differences $\{X_i^{(1)} - X_i^{(2)}; i = 1, 2, \cdots, n\}$ are normally distributed. But such an assumption is generally difficult to justify a priori.

Thirdly, we usually do not know the exact form of the joint probability function $F(Y)$. Note that if the set of alternative procedures is large, $F(Y)$ is a very complex multivariate distribution. Probably the only tractable assumption, that would give us some helpful results, would be to assume $F(Y)$ to be multivariate normal. Using past data we could attempt to fit a multivariate normal distribution to the data. But once again, the a priori reasoning for a normal distribution of alternative *forecasting procedures* along with the variable being forecasted, is in general, not clear at present.

[3] Some testing procedures have been developed recently to test homogeneity of a set of p correlated variances under restricted assumptions. For example, Chien-Pai Han (*Biometrica*, Vol. 55, (1968), p. 317) suggests a test procedure if one is willing to assume that a common correlation coefficient exists between *all* pairs of the p variances. He also assumes that the $p \times 1$ vector of sample variances has a multivariate normal distribution with mean vector μ and a nonsingular covariance matrix.

In summary, I have argued in this note that the determination of optimal forecasting strategy is in practice a difficult problem, even though theoretically it is clear how we should proceed.

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Some Comments on Information Systems

I am glad that the "Letters to the Editor" section of *Management Science* provides additional communication links between the readers and I would like to comment on Professor Stanfel's letter "On Information Systems" in the June 1969 issue.

My contribution to Mr. Stern's column "Information Systems in Management Science" are excerpts selected by Mr. Stern from a paper called "A Tentative Definition of Management Information Systems" which was submitted to *Management Science* in January 1968 and which has since been revised and resubmitted under the title "Management Information Systems: Concepts and Pragmatism." The excerpts published in February 1968 were not intended as a definition of MIS but were selected as some thoughts leading to such a definition in the intuitive general sense. (In 1968, the prospects of an article in a prestigeous journal had to have priority over a "mere" column contribution in the same Journal).

I agree with Professor Stanfel when he says: "One tends to be pessimistic about the formulation of a precise definition of a management information system which is at once consistent and sensible and, simultaneously satisfies everyone." I sympathize with his suggestion not to be concerned with finding good definitions for MIS, because I share his discontent and disenchantment with all of the MIS definitions known to me, but as a student, computer systems man, management consultant, teacher, and academician I feel a need for at least an intuitive and general definition of management, information, and systems which can lead to a hopefully meaningful definition of management information systems. After all, science is only refined common sense. Even if this reasoning process does not provide a precise definition of MIS, it may allow us to pinpoint the problems encountered in defining MIS as a system designed to provide management with information. What is common to the general notions of management, information, and systems that can be utilized for defining MIS such that neither one of these notions is violated nor the fact that today we can apply machines like computers to provide management with information? An answer to this simple question most certainly does not provide revolutionary insights but it may help us in selecting important concepts from the social, behavioral, systems, and computer sciences under the unifying notion of MIS.

Any effort to define concepts is to be seen as an attempt to provide conceptual clarity, even if this definition provides only problem definitions rather than solutions. It appears especially important to this writer that we gain conceptual clarity of the concepts reflected in the words management, information, and systems, before any attempt is made to combine these into a new concept. If the notion of management is immediately related to such intelligence tasks as goal formulation (consisting of goal planning and goal setting), and goal (achievement) control, such insight ought to be related to the concept of information.

I would like to support Professor Stanfel's suggestion that we look at practical examples of information systems; however, we should make sure that all of these examples are presented in the proper conceptual context, i.e. the problem definition