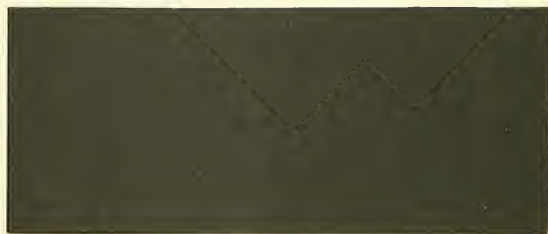WORKING PAPER
ALFRED P. SLOAN SCHOOL OF MANAGEMENT

NOTES ON MARKETING EXPERIMENTATION

99-64

John D. C. Little

MASSACHUSETTS
INSTITUTE OF TECHNOLOGY
50 MEMORIAL DRIVE
CAMBRIDGE, MASSACHUSETTS 02139

NOTES ON MARKETING EXPERIMENTATION

99-64

John D. C. Little

Notes on Marketing Experimentation[*]

1.0 Introduction

The successful conduct of marketing operations requires good information about market behavior. Such information comes from many sources--from historical sales data, personal experiences, surveys, and various special studies. We shall here take up a specialized but valuable method of learning about the market, namely, experimentation.

Principally, we shall consider experiments involving the active intervention in the market by a company to measure the effect on sales of variables under the company's control. For example, quantities such as advertising, price, salesman's calls, packaging, and display might be varied to estimate their effect on sales. Presumably, once the sales effect is estimated, appropriate calculations will convert this into an effect on profit or other relevant measure of effectiveness. Although the discussion will focus on sales experiments, most of the principles discussed apply to any response variable.

The term, experiment, will be used to imply a controlled comparison of alternatives. In other words, two or more experimental treatments are applied in a situation where the experimenter is able to decide which experimental units receive the treatment. Thus, an experiment might compare promotions A and B by using them in different sets of cities. The experimenter decides which city receives which treatment (probably doing this by a random process). A more passive approach is to analyze historical data in which marketing variables have varied in the normal course of company operations. Such data can sometimes be analyzed by econometric methods to estimate the effect of the

---
[*]Based on lectures given in 15.85s, Operations Research in Marketing, Sept. 8-12, 1964.

variables on sales. For example, Telser has used such methods to estimate the effect of advertising on cigarette sales [6] and price on the demand for certain branded goods. [7] Historical data may be quite revealing in the situation that Howard and Roberts have called a try-out. [11] A company may try out a new policy but without benefit of a control or alternative treatment. If the change is drastic enough, say a price cut or the introduction of a new product, there are sometimes special opportunities for analysis. An example of this is Henderson and Brown's study of the effect of a promotional campaign on the sales of frozen orange juice.

Although the analysis of historical data is often very valuable, experiments offer several special advantages. If you make an experimental change and observe an effect, it is frequently more convincing than if a change and an effect occur together in past data. In the latter case, you are likely to worry that a deeper mechanism may be causing both the change and the effect, since changes in marketing variables are usually made for some reason. A serious difficulty in econometric analyses is that the variables of interest may have substantial correlations with each other and with other explanatory variables. This may lead to instability in estimates of the important regression coefficients. In designing an experiment a deliberate effort is made to have small correlation among experimental variables and between them and other explanatory variables. Finally, of course, experiments can be directed at questions of immediate interest and can examine more different alternatives than would normally occur in regular operations.

Experimentation has some important limitations. These will become clearer a little later. Principally, however, the accuracies obtainable, the time required for performance, and in some cases, the cost of the study clearly limit the number of questions which can be turned over to strict empiricism of this sort.

In any case, sales experiments are best supported by studies of intermediate variables (eg. consumer attitude and awareness shifts, and reactions in the distribution system) that may increase understanding of marketing processes and may eventually permit good predictions of sales effectiveness on the basis of intermediate variables.

An interesting potential application of sales experiments is in the role of a monitoring device in the continuous control of marketing operations. At least one company does this in an informal way now. In certain fields, notably chemical engineering, such control is well known under the title of evolutionary operations. Figure 1 traces out the main ideas in marketing terms. Small experiments are made a part of normal marketing operations. Information from the experiments is fed back into the budget allocation process. Although to our knowledge no satisfactory formal theory presently exists for the marketing application, the prospect is sufficiently interesting that we shall discuss some of the problems involved later on.

Many books have been written on the statistical design of experiments. Cox[5] gives a reasonably non-technical exposition of the basic ideas. Cochrane and Cox[3] is something of a handbook. Kempthorne[4] goes deeper into the theory.

2.0 Example: A Two Level Spending Experiment

As a starting example, consider the problem of measuring sales response to promotional spending in order to find a more profitable spending rate.

2.1 Profit Model

We suppose that, under some given set of conditions, sales rate increases with promotional spending, fairly rapidly at low spending levels but with diminishing returns at high levels. The sales response curve might look something as follows:
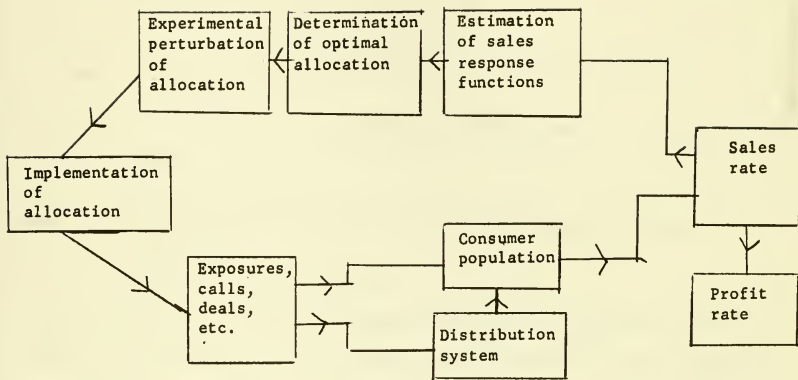
Figure 1. Block diagram for using continuous experiments in the control of marketing effort.
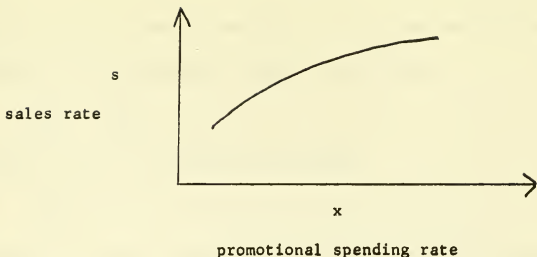
promotional spending rate

Figure 2

Here s = sales rate, say, in units of dollars per household per year
       (dol/hh.yr)

      x = promotion rate (dol/hh.yr)

(More obvious units for sales and promotion might be dollars/year but, if
different sized regions are to be compared, some division by a size variable
is desirable. A good divisor is a quantity that is a measure of potential
sales in the region. Although individual products frequently have their own
special potentials, population and number of households are good all-round
measures of size. The latter will be adopted here.)

To calculate the value of x which maximizes profit rate, we construct
a simple model of company profit. Let

      p = profit rate (dol/hh.yr.)

      m = gross margin, i.e., incremental profit as a fraction of sales
           (dimensionless)

      $= \dfrac{\text{selling price - variable cost}}{\text{selling price}}$

      c = fixed cost rate, exclusive of the promotional spending (dol/hh.yr)

Then

$$p = ms - x - c \tag{2.1}$$

We are presuming that the type of promotion being investigated is a fixed cost
(eg. advertising) not a variable cost (eg. a price deal). A somewhat different
model is appropriate for the latter case.

Given the sales response function $s = s(x)$, the operating point for maximum profit can be determined. Thus, by calculus,

$$\frac{dp}{dx} = m\frac{ds}{dx} - 1 = 0$$

or, the best value of x is the one that makes

$$\frac{ds}{dx} = \frac{1}{m} \qquad (2.2)$$

Suppose that $m = 33\text{-}1/3\%$. Then $1/m = 3$. For a curve of the general shape of Figure 2, the maximum profit value of x can be found by increasing x until the slope of the curve is 3, i.e., until an additional dollar spent returns 3 dollars in sales.

Much more complicated profit models can be constructed. If budgets are limited and good alternative uses exist for the money, higher slopes than $1/m$ may be required for maximum profitability. Time lags could be introduced and future profits discounted.

The simple model suffices, however, to motivate an experiment for estimating sales response curve and to provide a reference point for judging how accurate the experiment must be in order to be meaningful. An experiment with a standard error for the slope of, say, 10 when $1/m = 3$ would probably be useless. A requirement that an experiment have a standard error of .01 might lead to unnecessary expense or perhaps an impossibility.

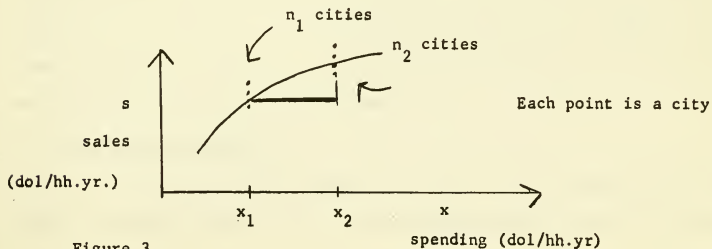## 2.2 An Experimental Setup

Figure 3

Suppose we pick one set of cities and apply a low spending rate and another set and apply a high spending rate. The spending rates are used for a test period of, say, a year and the resulting sales are observed.

Let x_1 = low spending rate (dol/hh.yr.)

x_2 = high spending rate (dol/hh.yr.)

n_1 = number of cities in low group

n_2 = number of cities in high group

s_{1i} = sales rate in the $i^{\underline{th}}$ city of low group during the test period (dol/hh.yr.)

$\bar{s}_1 = \frac{1}{n_1}\Sigma s_{1i}$ = average sales rate in low group (dol/hh.yr.)

s_{2i} = sales rate in $i^{\underline{th}}$ city of high group (dol/hh.yr.)

$\bar{s}_2 = \frac{1}{n_2}\Sigma s_{2i}$ = average sales rate in high group (dol/hh.yr.)

Denote the slope by b and its estimate by $\hat{b}$. A straightforward estimate of b is:

$$\hat{b} = \frac{\bar{s}_2 - \bar{s}_1}{x_2 - x_1} \tag{2.3}$$

The particular slope estimated is that of the chord of the sales response between x_1 and x_2. (Strictly speaking, the experiment can only tell us which of the two points x_1 and x_2 is more profitable. However, if the curve is smooth between the two values, it will take on the slope of the chord somewhere between x_1 and x_2. In a more complicated experiment we might test more than two spending rates, fit a curve through them and obtain a continuously varying slope over a considerable range of x.)

2.3 The Statistical Model

A reasonable statistical model to go with the above setup is as follows. Assume the observed sales rate in a city having promotion rate x is a random

variable, s :

$$s = s(x) + \epsilon$$

where $s(x)$ is the sales response curve and ϵ is a random variable of mean zero and variance σ^2. The variable ϵ is assumed independent from city to city and, for concreteness, will be assumed to be normally distributed.

In other words, we assume that there is an underlying average sales response curve in the class of cities under study and that the cities in each test group will be scattered about the curve with a standard deviation of σ at their particular x value.

2.3 The Standard Error of the Estimated Slope

The following notation will be used. Let X and Y be random variables.

$$E(X) = \text{expected value of } X$$

$$V(X) = \text{variance of } X$$

$$SE(X) = \left[V(X) \right]^{\frac{1}{2}} = \text{standard error of } X$$

$$CV(X) = SE(X)/E(X) = \text{coefficient of variation of } X$$

$$\text{Cov}(X,Y) = \text{covariance of } X \text{ and } Y$$

Let a and b be constants. A useful, standard formula is:

$$V(aX + bY) = a^2 V(X) + b^2 V(Y) + 2ab\, \text{Cov}(X,Y) \tag{2.5}$$

The estimate of slope previously given is

$$\hat{b} = \frac{\bar{s}_2 - \bar{s}_1}{x_2 - x_1}$$

This is, in fact, the maximum likelihood estimate under the assumed statistical model.

We want $SE(\hat{b})$. Using (2.5)

$$V(\hat{b}) = \frac{1}{(x_2 - x_1)^2} V(\bar{s}_2 - \bar{s}_1) .$$

Because of the independence of the individual ϵ's, $\text{Cov}(\bar{s}_2, \bar{s}_1) = 0$ and so

$$V(\bar{s}_2 - \bar{s}_1) = V(\bar{s}_2) + V(\bar{s}_1)$$

$$V(\bar{s}_1) = \frac{\sigma^2}{n_1} \; , \; V(\bar{s}_2) = \frac{\sigma^2}{n_2}$$

Putting things together:

$$SE(\hat{b}) = \frac{\sigma}{(x_2 - x_1)} \left[\frac{1}{n_1} + \frac{1}{n_2} \right]^{\frac{1}{2}} \qquad (2.6)$$

This formula tells the accuracy of the experiment. Suppose for example

$\sigma = .02$ (dol/hh.yr.)

$n_1 = 10$

$n_2 = 10$

$x_1 = .01$ (dol/hh.yr.)

$x_2 = .02$ (dol/hh.yr.)

Then

$$SE(\hat{b}) = .9$$

If, say, $1/m = 3$, we see that we can easily tell the very good from the very bad, but for b close to $1/m$ it will be hard to tell whether we are over or under the best value of x. Of course, if the operation is fairly close to the optimum, we may be reasonably well satisfied, because most maxima are smooth near the top and small deviations may not make much difference.

The formula also shows what quantities affect accuracy. Increasing the sample sizes will decrease the standard error according to the familiar square root rule. Any decrease that can be made in σ decreases the standard error proportionately. The quantity $(x_2 - x_1)$ enters the expression in an extremely sensitive way. Small values will give very high standard errors. The reason is simple: in order to detect a sales difference amid the random variations, one must create a fairly substantial sales difference and this will usually require fairly well separated promotional rates. On the other hand, although large values of $x_2 - x_1$ are desirable, caution is necessary because too large an x_2 may push promotion into the diminishing returns part of the curve and make the slope of the chord between x_1 and x_2 look small, even though the slope of the curve may be large near x_1.

An alternative form of (2.6) is sometimes convenient:

$$SE(\hat{b}) = \left(\frac{\sigma}{s}\right)\left(\frac{s}{x}\right)\left(\frac{x}{x_2 - x_1}\right)\left(\frac{1}{n_1} + \frac{1}{n_2}\right)^{\frac{1}{2}} \tag{2.7}$$

The values of s and x can be anything since they cancel out. Let us suppose they are the national averages of the sales rate and the promotion rate expressed in dol/hh.yr. Then all the terms in parentheses are dimensionless and have the following interpretations: σ/s is the coefficient of variation of sales for cities of the type used in the experiment, s/x is the ratio of sales to promotion nationally; and $x/(x_2 - x_1)$ is the ratio of the national promotion rate to the difference between the two test rates.

It is important to estimate $SE(\hat{b})$ before doing an experiment like the above. Only then can one decide whether the experiment is worth doing. What we principally need to know for this purpose is σ. In the next section we shall present the details of a two level spending experiment and its analysis. In the course of the analysis we shall see one way that σ can be estimated in advance.

2.4 Numerical Example

Next we present and analyze data adapted from a two level advertising experiment conducted a few years ago by the manufacturer of a grocery store product.

The objective of the experiment was to measure the effect of spending rate on sales. The experimental units were the company's sales territories. Out of a national total of 21 territories, 18 were given the normal advertising rate, while 3 were given twice the normal rate. The test ran for a year. Sales were measured by factory shipments.

To portray the statistical characteristics of the original data but still conceal the actual numbers and their ratios, we have taken the following steps: Sales, advertising, and the measure of potential have all been multiplied by

scale factors to standardize them at national values of approximately 10 million
dollars/year for sales, .5 million dollars/year for advertising and 50 million
households for potential. The data in this form have been analyzed to
estimate the parameters of the statistical model. Thereafter a set of new
data has been generated by simulating the statistical model. It is the simu-
lated data that is tabulated here.

Table I presents, for each territory, sales during the test year, sales
the previous year, advertising during the test year, and the measure of
potential. Table II puts the sales and advertising data on a per household
basis. The high spending rate territories are clearly 1, 2, and 3.

Two analyses will be given, the second being neater than the first.
The first is presented because it is the basic intuitive analysis. The
second represents a worthwhile embellishment.

Analysis I: The average values of sales and advertising in the high
and low groups are:

$$\bar{s}_2 = .24796 \text{ dol/hh.yr.} \qquad \bar{x}_2 = .01675 \text{ dol/hh.yr.}$$
$$\bar{s}_1 = .21300 \quad " \qquad \bar{x}_1 = .00892 \quad "$$

and so an estimate of the slope of the sales response curve is:

$$\hat{b} = \frac{\bar{s}_2 - \bar{s}_1}{\bar{x}_2 - \bar{x}_1} = 4.46$$

Thus, it is estimated that in increasing the spending rate from x_1 to x_2,
each dollar of advertising brough back 4.46 dollars of sales during the
experimental period.

Next we want $SE(\hat{b})$ and for this purpose an estimate of σ, the standard
deviation of the dependent variable in the population of experimental units.
Were it not for the presence of the experimental treatment, we could simple find
the standard deviation of the number in the first column of Table II. As it is,
slightly different means should be subtracted from the high and low groups.

EXPERIMENT 1. TABLE I

TERRITØRY NØ.	SALES IN TEST YEAR (DØLLARS)	ADVERTISING IN TEST YEAR (DØLLARS)	SALES IN PREVIØUS YEAR (DØLLARS)	PØTENTIAL (HØUSEHØLDS)
1	586157	45030	521912	2654913
2	612242	37950	424086	2252810
3	555808	36393	490691	2211568
4	1505579	63565	1302596	7645118
5	373427	18664	296753	2185792
6	403184	18623	394744	1979586
7	515501	22763	470941	2721930
8	759243	25743	704022	2917827
9	414064	14926	429551	1696051
10	390888	14941	310568	1696051
11	284007	11598	250085	1376431
12	593965	26892	557926	2634292
13	355453	15936	333822	1819775
14	404030	16301	393962	1752758
15	276128	12873	291158	1464068
16	391713	20151	373694	2005361
17	422276	17283	469825	1912568
18	726657	30097	695916	3283844
19	522147	22328	531114	2453861
20	369957	15379	388078	1747603
21	365622	12564	368556	1587793
TØTALS	10828048	500000	10000000	50000000

EXPERIMENT 1, TABLE II

TERRITØRY NØ.	S(T) SALES IN (DØL/HH.YR.)	X(T) ADVERTISING IN (DØL/HH.YR.)	S(T-1) SALES IN (DØL/HH.YR.)
1	.22078	.01696	.19658
2	.27176	.01684	.18824
3	.25131	.01645	.22187
4	.19693	.00831	.17038
5	.17084	.00853	.13576
6	.20367	.00940	.19940
7	.18938	.00836	.17301
8	.26020	.00882	.24128
9	.24413	.00880	.25326
10	.23046	.00880	.18311
11	.20633	.00842	.18169
12	.22547	.01020	.21179
13	.19532	.00875	.18344
14	.23051	.00930	.22476
15	.18860	.00879	.19886
16	.19533	.01004	.18634
17	.22079	.00903	.24565
18	.22128	.00916	.21192
19	.21278	.00909	.21644
20	.21169	.00880	.22206
21	.23027	.00791	.23211

$$\sum_{i=4}^{21} (s_{1i} - \bar{s}_1)^2 = 8.32 \times 10^{-3}$$

$$\sum_{i=1}^{3} (s_{2i} - \bar{s}_2) = 1.21 \times 10^{-3}$$

An unbiased estimate of σ is:

$$\hat{\sigma} = \left[\frac{95.3}{19} \times 10^{-4}\right]^{\frac{1}{2}} = .0224$$

Therefore, the estimated standard error of the slope is

$$SE(\hat{b}) = \frac{\hat{\sigma}}{\bar{x}_2 - \bar{x}_1} \left[\frac{1}{n_1} + \frac{1}{n_2}\right]^{\frac{1}{2}} = \frac{.0224}{.00783} \left[\frac{1}{18} + \frac{1}{3}\right] = 1.78$$

To summarize: $\hat{b} = 4.46$, $SE(\hat{b}) = 1.78$

Analysis II. If further variables can be found that help to explain the variation in sales from territory to territory, the standard error can be reduced and greater accuracy obtained. As is well known, sales in an area tend to be like previous sales in that area. Previous sales almost always makes a good explanatory variable. Let

$s_i(t)$ = sales rate in the $i\underline{th}$ territory in the test year (dol/hh.yr.)

$s_i(t-1)$ = sales rate in the $i\underline{th}$ territory in the preceding year (dol/hh.yr.)

$x_i(t)$ = advertising rate in the $i\underline{th}$ territories in the test year (dol/hh.yr.)

$\in_i$ = a random error for $i\underline{th}$ territory (dol/hh.yr.)

A simple regression model for sales is then

$$s_i(t) = a_o + a_1 x_i(t) + a_2 s_i(t-1) + \in_i \tag{2.8}$$

where the $\in_i$ are assumed independently and normally distributed with mean zero and variance σ^2.

The model is linear in x even though the sales response curve may not be. This is because we have only two spending levels and seek the slope of the line

connecting them. (Although x values are not exactly alike within each group, there is not nearly enough variation to estimate non-linearity.) The regression coefficient a_1 corresponds to the slope b.

A regression using the data in Table II in the Model (2.8) yields (see Analysis II in Table III):

$$\hat{b} = 4.40$$
$$SE(\hat{b}) = 1.25$$

The standard error of $\hat{b}$ has dropped about 30% due primarily to a drop in $\hat{\sigma}$ from .0224 to .0159 .

Analysis I can also be done as a regression by dropping $s_i(t-1)$ as a variable in (2.8). The results are shown in Table III. Analysis II is obviously better and is taken as the final set of results.

Discussion. What can we say about these results from a managerial point of view? Suppose our criterion is $1/m = 2.5$. We can say the higher level looks pretty good, although there is always a chance it is not best. ($\hat{b}$ and 2.5 are separated by about 1.5 standard errors.)

On the other hand, suppose $1/m = 4.5$ .The estimate does not differentiate between x_1 and x_2. Possibly, however, a manager has prior feelings on advertising effectiveness. These can be taken into account intuitively, or if made explicit as prior probabilities, incorporated in a Bayesian analysis.

The experiment has yielded information but uncertain information. Values far away from 4.40 are now considered improbable. If, as is frequently the case, the company had little previous knowledge on sales response, the increase in information may be considerable.

What was the cost of the experiment? The sales data came from factory shipments and would have been collected anyway,. The cost of the analysis was small. The money spent on the increased advertising was an immediate out-of-pocket cost, which may or may not have been recovered. However, if $1/m < 4.40$ the appearances are that the experiment showed a net profit.

EXPERIMENT 1, TABLE III

MEANS

S(T)	X(T)	S(T-1)
.21800	.01004	.20372

STANDARD DEVIATIONS

S(T)	X(T)	S(T-1)
.0246856	.0027899	.0279975

CORRELATION MATRIX

	S(T)	X(T)	S(T-1)
S(T)	1.000000		
X(T)	.498762	1.000000	
S(T-1)	.627725	.001619	1.000000

ANALYSIS I

MULT. R = .4987,   ST. ERR. OF EST. =   .0224
F(1,19) = 6.2916

TABLE OF RESULTS

VAR	B-COEFF	ST.ERR.B	T
CONSTANT	.1736	.0183	-
X(T)	4.4131	1.7593	2.5083

ANALYSIS II

MULT. R = .8011,   ST. ERR. OF EST. =   .0159
F(2,18) = 16.1249

TABLE OF RESULTS

VAR	B-COEFF	ST.ERR.B	T
CONSTANT	.0611	.0284	-
X(T)	4.4041	1.2482	3.5283
S(T-1)	.5527	.1243	4.4440

The experiment might be improved. Perhaps a better statistical model and analysis can be found. More experimental units in the high group would help, although this would mean more out-of-pocket cost. Possibly a change of experimental unit from sales territory to metropolitan markets would be desirable.

An estimate of σ is vital to the sound planning of an experiment. Analysis II can be adapted to do this by dropping x(t) and running it on historical data. The value of σ is not likely to vary much from year to year. Even though σ is estimated from (2.8) it is the relevant quantity for (2.6) in a two level spending experiment. We may think of the sales data in (2.3) as being adjusted for sales in the previous year.

A convenient way to express σ is as the coefficient of variation; the ratio of standard deviation to the mean. From Table III:

$$CV = \frac{.0159}{.2180} = 7.3\%$$

A revealing way to look at the standard error of the difference between treatments is to express it as a percent of sales. Here

$$\frac{SE(\bar{s}_2 - \bar{s}_1)}{\bar{s}} = \frac{\sigma}{\bar{s}} \left[\frac{1}{n_2} + \frac{1}{n_1} \right]^{\frac{1}{2}}$$

$$= .073 \left[\frac{1}{18} + \frac{1}{3} \right]^{\frac{1}{2}}$$

$$= .045$$

Thus, the advertising increase must produce a percentage increase in sales which will show up against a 4.5% standard error.

3.0 Designing Experiments

3.1 Choice of dependent variable

By "dependent variable" we mean the numerical quantity that the experi-
mental treatments are supposed to affect. The focus here is on sales,
although a variety of other variables are frequently of interest and many
of the remarks made here apply with little change to other variables. An
important dependent variable closely related to sales is market share.

Market share usually has quite a bit less variance than sales itself
and so permits a more sensitive measurement. Seasonal effects tend to be
removed as do some of the effects of national economic conditions. However,
if a company's marketing treatments increase competitive sales as well as
company sales, the percent increase in market share will be less than the
percent increase in sales. Whenever possible, competitive sales should be
examined for evidence of experimental effects.

Obtaining satisfactory sales data is sometimes difficult. An organi-
zation working directly with the final customer, eg. a supermarket or a mail
order house, can audit its own sales. A manufacturer can use factory ship-
ments, but then must accept delays in the pipeline. This is tolerable if
the experiment lasts a considerable length of time. If the product is
shipped to regional warehouses of retail chains, some difficulty may arise
in allocating the sales to individual market areas, although some workable
system can usually be found.

Store audits are an effective but usually costly way to measure sales.
The Department of Agriculture experiments have usually used store audits..
Consumer surveys are another device. Both surveys and audits offer the
possibility of collecting data on competitive sales. Some products have
specialized opportunites for collecting sales data (eg. warranty cards).

The advantage of collecting data on extra dependent and independent variables is worth emphasis. The advantages are illustrated in many of the Department of Agriculture experiments where sales of related products were recorded, as were prices, display space, point-of-sale promotions, and media advertising. Frequently, the results gave information on these added variables, including, from time to time, interesting surprises.

3.2 Choice of experimental unit

The term "experimental unit" is used to refer to the unit that is assigned a treatment independently of the other units. Thus, if a market area is given a "high television" treatment, the individuals in the market may be considered to have received this treatment, but the experimental unit is the market not the individual. This is because each individual is not (and in practice could not be) assigned a high, low or other television treatment independently of the others in the market.

The most desirable experimental unit is probably the individual, or, as is frequently more appropriate, the household. Depending, of course, on the situation, sample sizes in the 1000's or 100,000's may be possible. Although coefficients of variation for individuals are greater than for more aggregated units, the reductions in standard error are likely to be substantial. Individuals or households can be used as experimental units in direct mail promotion, magazine split runs and personal selling, provided that sales can be traced to the individuals. Direct mail sales experimentation has had a long and successful history in such applications.

An individual store can be an experimental unit for treatments directed at individual stores. For advertising treatments involving television, newspapers, or the like, the experimental unit is usually a market area. In some of the Department of Agriculture experiments [1] [2] the unit consisted of a group of stores within the market. Some companies have used their sales territories as units.

For sales measurements an experimental unit must contain as part of its specification some time period of observation. Thus, a market might receive independently assigned treatments in successive 4-week periods. The unit is then a "market for 4 weeks".

3.3 Randomization

The random assignment of treatments to experimental units helps make the assumptions used in most statistical analyses come true. Detailed discussions can be found in [3], [4], and [5].

3.4 What is measurable?

To illustrate the type of marketing variable whose effect on sales might be measured, we list some variables which have been used in past experiments:

- total advertising dollars
- advertising dollars in specific media: for example, television, newspapers, and radio
- salesmen's call rate
- product appearance
- campaign themes
- cooperative advertising
- point of purchase display
- store demonstrations
- dealer contests

The basic question in measurability is whether the experimental treatments will create enough of an effect to be detected amid the statistical variations. More important to profitability, however, is whether the accuracy is sufficient to detect differences that are both economically significant and likely to occur. This question can be posed as a formal problem in decision theory, but we shall here approach it descriptively. Our goal is to find standard errors of appropriate quantities so that the adequacy of an experiment can be judged in advance.

Three categories of experiment will be examined: (1) spending experiments with fixed cost spending rates, (2) spending experiments with variable

cost spending rates, and (3) the general two treatment comparison.

1. Spending experiments involving sales response to fixed cost spending.

   These were introduced in section 2. We would like to answer the following questions about the sales response curve:

   (a) What is the slope near the company's present operating position?

   (b) What is the curvative, i.e., where does diminishing returns set in?

   (c) How would competitors react to changes in company spending and what is the effect of their spending on the company's response curve?

The slope tells whether an increase or a decrease in spending would be profitable. The curvature tells how far such a move could apparently be made with profit.

The problem of competitive action is quite serious and we do not pretend to solve it. It may be that an apparently profitable increase in spending rate would be turned into a loss by a matching increase from competitors. Experiments of the type being considered are unlikely to be of value in second guessing the competition. However, they may sometimes reveal the effect of one company's spending on another company's sales.

The accuracy of estimating slope has been discussed; we found that in a two level experiment

$$\hat{b} = \frac{\bar{s}_2 - \bar{s}_1}{x_2 - x_1} = \text{estimate of slope}$$

$$SE(\hat{b}) = \frac{\sigma}{x_2 - x_1} \left[\frac{1}{n_1} + \frac{1}{n_2} \right]^{\frac{1}{2}} \tag{2.6}$$

Curvature will be viewed as a change in slope. Suppose three spending levels are tested:

	spending rate (dol/hh.yr.)	no. of expt. units	ave. sales observed (dol/hh.yr.)
low	x_1	n_1	$\bar{s}_1$
medium	x_2	n_2	$\bar{s}_2$
high	x_3	n_3	$\bar{s}_3$

For simplicity, we shall specialize to the symmetric case:

$$n_1 = n_3 = n \quad \text{and} \quad x_3 - x_2 = x_2 - x_1 = \Delta$$

Thus a medium rate with n_2 experimental units at x_2, is compared to a high rate, $x_2 + \Delta$, with n units and a low rate, $x_2 - \Delta$, with n units. The same statistical model is assumed as in section 2; sales in a market are given by:

$$s = s(x) + \epsilon \qquad \text{dol/hh.yr.}$$

Let b_{ij} be the slope between the levels x_i and x_j. Then

$$\hat{b}_{12} = \frac{\bar{s}_2 - \bar{s}_1}{x_2 - x_1} \qquad \hat{b}_{23} = \frac{\bar{s}_3 - \bar{s}_2}{x_3 - x_2} \quad .$$

The interesting question is whether the slope is decreasing as x increases.

Let $\quad c = b_{23} - b_{12} =$ change in slope

$$\hat{c} = \hat{b}_{23} - \hat{b}_{12} = \text{estimate of c} \qquad (3.1)$$

Diminishing returns imply a negative c.

We want the standard error of $\hat{c}$. Substituting

$$\hat{c} = \frac{1}{\Delta} \left[\bar{s}_3 - 2\bar{s}_2 + \bar{s}_1 \right]$$

$$V(\hat{c}) = \frac{1}{\Delta^2} \left[V(\bar{s}_3) + 4V(\bar{s}_2) + V(\bar{s}_1) \right]$$

$$= \frac{\sigma^2}{\Delta^2} \left[\frac{1}{n} + \frac{4}{n_2} + \frac{1}{n} \right]$$

$$SE(\hat{c}) = \frac{\sigma}{\Delta} \left[\frac{2}{n} + \frac{4}{n_2} \right]^{\frac{1}{2}} \qquad (3.2)$$

As an example, suppose

$$n_1 = n = 10 \qquad\qquad x_1 = .01 \text{ (dol/hh.yr.)}$$
$$n_2 = 20 \qquad\qquad x_2 = .02 \quad "$$
$$n_3 = 10 \qquad\qquad x_3 = .03 \quad "$$

$$\sigma = .02 \text{ (dol/hh.yr.)}$$

Then $SE(\hat{b}_{12}) = \dfrac{.02}{.01} \left[\dfrac{1}{10} + \dfrac{1}{20}\right]^{\frac{1}{2}} = .78 = SE(\hat{b}_{23})$

$$SE(\hat{c}) = SE(\hat{b}_{23} - \hat{b}_{12}) = \dfrac{.02}{.01} \left[\dfrac{2}{10} + \dfrac{4}{20}\right]^{\frac{1}{2}} = 1.27$$

Comments: (1) As can be seen in the example or by comparing (2.6) with (3.2), measuring changes in slope is harder than measuring the slope itself. Published experiments that have had some success in measuring curvature have used very large differences in x. For example, Benjamin and Maitland's [15] best results are for cases where the ratio between largest and smallest x values goes as high as 9/1 and 15/1 .

(2) The split of experimental units between n and n_2 in the above setup affects $SE(\hat{c})$. For a fixed number of available units, $SE(\hat{c})$ is minimized by setting $n_2 = 2n$. This may be verified by supposing the total available is N, substituting $n_2 = N - 2n$ in (3.2) and minimizing with respect to n_1 .

(3) The $\hat{c}$ calculated above estimates the difference in slope between two chords. With three points on the sales response curve, a smooth curve, say a quadratic, could be fit through them and such a curve would be a convenient representation of the data. However, the standard error of $\hat{c}$ gives a measure of whether such a fit is likely to be meaningful.

2. Spending experiments involving sales response to variable cost spending.

A price deal or dealer contest in which compensation is a percentage of sales requires a slightly different analysis, since in such cases the

promotional treatment affects the incremental profit per unit. Our objective is the same: to calculate in advance a standard error that will tell us whether a proposed experiment has meaningful accuracy.

Here, it will help to change our dimensions somewhat. Let

$$s = \text{sales rate in } \underline{\text{units}}/\text{hh.yr.}$$

$$m = \text{incremental profit in } \underline{\text{dol/unit}}$$

$$p = \text{profit rate in dol/hh.yr.}$$

Our statistical model is that the sales in an experimental unit is

$$s = s(m) + \epsilon \qquad (3.3)$$

where ϵ is normally distributed with mean zero and standard deviation σ units/hh.yr.

Consider a case where the promotional treatments compared have incremental profits m_1 and m_2. These might represent a price deal and normal operations. Suppose n_1 markets receive m_1 and produce average sales $\bar{s}_1$ while n_2 markets receive m_2 and produce average sales $\bar{s}_2$.

Let $\hat{p}_2 - \hat{p}_1 = $ estimated profit difference

$$= m_2 \bar{s}_2 - m_1 \bar{s}_1$$

Applying (2.5) in ways similar to those used previously, we get

$$SE(\hat{p}_2 - \hat{p}_1) = \sigma \left[\frac{m_2^2}{n_2} + \frac{m_1^2}{n_1} \right]^{\frac{1}{2}} \qquad (3.4)$$

The profit differences are perhaps more meaningful when expressed as fractions of sales in dollars. Let

$$s_n = \text{national sales rate (units/hh.yr.)}$$

$$m_n = \text{normal national incremental profit (dol/unit)}$$

then

$$SE\left(\frac{\hat{p}_2 - \hat{p}_1}{m_n s_n} \right) = \frac{\sigma}{s_n} \left[\left(\frac{m_2}{m_n} \right)^2 \frac{1}{n_2} + \left(\frac{m_1}{m_n} \right)^2 \frac{1}{n_1} \right]^{\frac{1}{2}} \qquad (3.5)$$

<u>Example</u>: $\sigma/s_n = CV = .08$

$$m_2 = m_n \qquad\qquad n_2 = 20$$

$$m_1 = .6m_n \qquad\qquad n_1 = 10$$

then $\qquad SE\left(\dfrac{\hat{p}_2 - \hat{p}_1}{m_n s_n}\right) = 2.34\%$

In other words, using these particular numbers, if the difference in profit between the two treatments is on the order of 5% of sales, we stand a very good chance of detecting the better one. If the difference is something like 1% of sales, we will not obtain a reliable measurement.

3. The general two treatment comparison

Marketing alternatives sometimes have little or no cost difference between them, as, for example, different copy themes or different media mixes within the same budget. Sometimes the treatment difference is a single shift in fixed cost rate, as a retooling for a product modification. The omnibus calculation that can be made the basis for further profitability comparisons is the computation of the standard error of the difference in treatment means.

Suppose n_1 experimental units are given treatment 1 and produce an average sales rate $\bar{s}_1$ (say, in dol/hh.yr.) and n_2 units are given treatment 2 and produce an average sales rate $\bar{s}_2$. Let σ be the standard deviation for individual experimental units and s_n the national average sales rate. Then

$$\frac{\bar{s}_2 - \bar{s}_1}{s_n} = \begin{array}{l}\text{estimated difference in sales between treatments} \\ \text{as a fraction of national sales rate.}\end{array}$$

$$SE\left(\frac{\bar{s}_2 - \bar{s}_1}{s_n}\right) = \frac{\sigma}{s_n}\left[\frac{1}{n_2} + \frac{1}{n_1}\right]^{\frac{1}{2}} \qquad (3.6)$$

Example: $\quad \sigma/s_n = CV = .08$

$$n_1 = n_2 = 30$$

$$SE\left(\frac{\bar{s}_2 - \bar{s}_1}{s_n}\right) = 2.02\%$$

Thus the treatments must produce sales differences which will show up against a standard error that is 2.02% of sales in order to be detectably different.

3.5 Formal Experimental Designs

Special arrangements of treatments on experimental units will often increase the efficiency of an experiment. Two or more variables of interest can be examined in the same experiment. Sources of variation that are not of interest but would otherwise obscure the treatment effects can sometimes be measured and removed. The subject is a large one and the reader is referred to the books on the field, e.g. [3] [4] and [5] . We shall confine ourselves with brief descriptions of marketing applications for three standard designs.

Randomized Blocks

Suppose that we wish to compare promotions A and B by using A in 8 cities and B in 8 cities. Instead of assigning the treatments at random to some list of cities, we might break the country up into regions, say, East, South, Midwest, and Far West, and within each region randomly assign A to two cities and B to two cities. The presumption is that sales per household would be more homogeneous within a geographical region than in the country as a whole. A region is a "block". Because of the balanced arrangement any additive sales effect attributable to the region can be estimated and removed.

The statistical model for the experiment is as follows: Sales for a city located in the ith block and receiving the jth treatment are given by:

$$s_{ij} = \mu + b_i + t_j + \epsilon$$

where s_{ij} is sales (say, in dol/hh.yr.), μ is mean sales, b_i is the differential effect of the ith block, t_j is the differential effect of the jth treatment, and ϵ is a random error. The ϵ's are presumed normally and

independently distributed with mean zero and variance σ^2 .

Without the block arrangement, random assignment of treatments would have lumped the b_i into $\in$ and, if $b_i \neq 0$, would have increased σ^2 and decreased accuracy.

Latin Squares

A Latin square is a particularly symmetric design in which several treatments are simultaneously compared and two sources of uncontrolled variation are eliminated. A good example appears in the winter pear study of Hind, Eley, and Twining [14] .

Their Latin Square compared 5 experimental treatments in 5 cities over 5 four-week time periods. The layout was as follows:

	Cleveland	Baltimore	Milwaukee	Houston	Atlanta
1st 4 wks	A	E	D	B	C
2nd "	B	C	E	A	D
3rd "	E	B	C	D	A
4th "	D	A	B	C	E
5th "	C	D	A	E	B

The experimental treatments were:

A. Special point of purchase displays

B. Store demonstrations

C. Dealer contests

D. Media advertising

E. No sponsored promotions by the Pear Bureau

The experimental unit consisted of 15 supermarkets in each city for 4 weeks. The principal dependent variable was sales of winter pears, but sales data was collected on other fruits. A variety of extra independent variables were recorded, including price, display space, and newspaper advertising. Sales data was collected by store audit.

A Latin square can be of any size but the number of columns, and the number of treatments must all be equal. Each treatment appears once and only once in each row and similarly in each column. The design removes uncontrolled variation (to the extent of an additive constant) in the rows and in the columns. In the pear experiment a city effect and a time period effect were removed.

The statistical model assumes that sales for an experimental unit in period i and city j under treatment k are given by

$$s_{ijk} = \mu + r_i + c_j + t_k + \mathcal{E}$$

where μ is the mean sales, r_i a row effect, c_j a column effect, t_k a treatment effect, and $\mathcal{E}$ the usual random error.

In the experiment cited, there were strong city effects. Their removal decreased σ and made the experiment more sensitive.

A Latin square does not estimate "interactions". Thus, suppose store demonstrations are particulary good in some cities and not in others. The experiment contains no means of detecting any difference, but rather measures an average effect over all cities.

We note that a modification of the Latin square, known as a cross-over design, permits estimates of possible carry over effects in which a treatment in one time period affects response in the next. See [1] and [12] .

Factorial Designs

Factorial designs do estimate interactions. Suppose we wish to compare two television treatments, high and low, and also two newspaper treatments, high and low. In such a case we would probably not trust a model without interactions. In other words, if the difference between high and low TV treatments with newspapers at their low level were 5% and the difference between high and low newspapers with TV at its low level were 4%, we might

doubt that high TV combined with high newspapers would be 9%. We might expect some diminishing returns. A deviation from the sum of the single effects would be called an interaction.

The factorial arrangement for this experiment may be depicted as follows:

Each cell would, hopefully, contain several experimental units. In this type of experiment the unit would probably be a metropolitan market. The "factors" here are television and newspapers. Each factor has two or more "levels", in this case, "high" and "low".

The characteristic of a complete factorial experiment is that each level of a factor appears in combination with every level of every other factor.

The statistical model for the above experiment assumes that sales in a unit receiving TV at level i and newspapers at level j is:

$$s_{ij} = \mu + a_i + b_j + c_{ij} + \epsilon$$

where μ is mean sales, a_i the main TV effect, b_j the main newspaper effect, c_{ij} the TV-newspaper interaction, and ϵ the random error term.

4.0 Analysis

An experiment is analyzed according to the statistical model presumed to underlie the data. An formal design has a corresponding model. Adding extra explanatory variables to the model is frequently desirable, as has been remarked.

A good variation on previous period sales as an explanatory variable can be constructed if a considerable time series of post sales is available for each experimental unit. A time series analysis of post sales can be made for each unit separately and an extrapolation of sales into the experimental period made. The extrapolated sales can be used as an added explanatory variable in place of prior period sales.

Sometimes information is available to show that certain experimental units come from populations with higher variance than others. For instance, small cities usually have somewhat higher coefficients of variation than large cities. In such cases, the efficiency of estimation can be increased by weighting each observation inversely proportional to its variance.

4.1 Analysis of Experimental Designs by Regression

Books on experimental design give different computational formulas for each new design. These are of interest for a variety of reasons, and when calculations are to be done on a desk calculator these formulas are almost essential. However, whenever a standard experimental design is viewed as a fixed constants model it becomes a special case of the general linear hypothesis and as such can be analyzed on a computer by a multiple regression program.

The analysis of an experiment by a multiple regression program will give an estimate of each constant in the design and a standard error for it. In addition most programs will give the covariance between each pair of estimated constants or else information from which it can be easily deduced. However, a regression program does not ordinarily produce directly the information required for a table of analysis of variance.

Analysis by regression makes it possible to use one computer program for all different classes of designs. Regression will automatically accept data with missing observations or extra observations and treat designs not found in standard texts. When, as is usual in marketing, there are important explanatory variables outside the design proper (e.g. last year's sales),

their inclusion in the analysis is very simple.

To illustrate the use of regression, we analyze an experiment consisting of a pair of 3 x 3 Latin squares. The data are entirely made up. In fact, the residual error has been made absurdly small in order to show that the analysis does indeed recover the constants used in making up the data.

A pair of 3 x 3 Latin squares

	1	2	3
I	A	B	C
II	B	C	A
III	C	A	B

	4	5	6
I	A	B	C
II	C	A	B
III	B	C	A

Suppose that there are three experimental treatments to be evaluated, A, B, C; six markets in which the test is being conducted, 1, 2, 3, 4, 5, 6; and three successive time periods (perhaps months) I, II, III. In each market in each time period some one of the three treatments is administered as indicated by the cells in the above squares. The dependent variable is sales.

The observations are presumed to have been generated by the market (or, in our example, by us) according to the model

$$y_{ijk} = m + a_i + p_j + d_k + \mathcal{E} \tag{4.1}$$

where y_{ijk} = sales in the ith market area in the jth time period under treatment k.

m = mean sales for all markets

a_i = differential effect on sales of ith market area

p_j = " j^{th} time period

d_k = " k^{th} treatment

$\mathcal{E}$ = random error

All units would be the same, say, thousands of dollars. The random error is assumed to be independent from cell to cell and to be normally

distributed with zero mean and some variance σ^2 .

An experiment is usually oriented toward measuring differences, e.g. the difference between treatments A and B on sales. This would be $d_B - d_A$. A constant added to each d_k would not affect the difference. By convention, it is presumed that a constant has been added to each d_k so that the sum of the d_k is zero. The same remark applies to the a_i and the p_j.

We synthesize some hypothetical data.

$a_1 = -.3$	$p_I = .1$	$d_A = -1.80$
$a_2 = -.2$	$p_{II} = .0$	$d_B = .12$
$a_3 = -.1$	$p_{III} = -.1$	$d_C = 1.92$
$a_4 = .1$		
$a_5 = .2$		
$a_6 = .3$	$m = 3.57$	

From the above table we can construct the expected value of sales for any cell in the squares. For example, the expected value for I, 1, A is

$$E(y_{I,1,A}) = m + p_I + a_1 + d_A$$

$$= 3.57 + .1 - .3 - 1.80 = 1.57$$

Continuing in a like manner, we develop the mean sales in each cell as follows:

	1	2	3		4	5	6
I	1.57^A	3.35^B	5.49^C		1.97^A	3.75^B	5.89^C
II	3.15^B	5.29^C	1.67^A		5.59^C	1.97^A	3.75^B
III	5.09^C	1.47^A	3.25^B		3.45^B	5.59^C	1.97^A

To complete the synthesis, a random normal number of mean zero is added to each cell. A very small standard deviation ($\sigma = .01$) has been used to

make it clear that the analysis does recover the original numbers. The final "observed" sales data are:

	1	2	3
I	1.585^A	3.355^B	5.494^C
II	3.142^B	5.286^C	1.676^A
III	5.089^C	1.471^A	3.249^B

	4	5	6
I	1.989^A	3.765^B	5.899^C
II	5.583^C	1.975^A	3.740^B
III	3.450^B	5.601^C	1.966^A

We proceed to analyze the experiment by multiple regression in the following steps. First we set up a model with dependent variable y and independent variables u_i, v_j, and w_k. Then we change independent variables slightly and rename them x_1, x_2, ... to conform to usual regression program notation. A table of input data for such a program is presented. The output is given, showing means and standard errors for the constants of the model. Finally we show how to compute the standard error of the difference between two of the estimated constants.

Consider first the regression model.

$$
\begin{aligned}
y = m & \\
& + a_1 u_1 + a_2 u_2 + a_3 u_3 + a_4 u_4 + a_5 u_5 + a_6 u_6 \\
& + p_I v_I + p_{II} v_{II} + p_{III} v_{III} \\
& + d_A w_A + d_B w_B + d_C w_C \\
& + e
\end{aligned}
\tag{4.2}
$$

where each data point (cell observation) is to have associated with it a set of values:

$$
\begin{aligned}
y &= \text{sales for the cell observation} \\
u_1 &= \begin{cases} 1 & \text{if cell refers to market 1} \\ 0 & \text{otherwise} \end{cases}
\end{aligned}
$$
.
.
.

$$u_6 = \begin{cases} 1 \text{ if cell refers to market 6} \\ 0 \text{ otherwise} \end{cases}$$

$$v_I = \begin{cases} 1 \text{ if cell refers to time period I} \\ 0 \text{ otherwise} \end{cases}$$

.
.
.

$$v_{III} = \begin{cases} 1 \text{ if cell refers to time period III} \\ 0 \text{ otherwise} \end{cases}$$

$$w_A = \begin{cases} 1 \text{ if cell refers to treatment A} \\ 0 \text{ otherwise} \end{cases}$$

.
.
.

$$w_c = \begin{cases} 1 \text{ if cell refers to treatment B} \\ 0 \text{ otherwise} \end{cases}$$

$$e = \text{random error}$$

A regression on these variables will fail. This is because certain variables can be expressed as linear combinations of other variables and as a result the matrix to be inverted in solving for the coefficients is singular. We can, however, get rid of 3 variables by recalling that

$$\Sigma a_i = 0 \qquad \Sigma p_j = 0 \qquad \Sigma d_k = 0$$

or

$$a_6 = - (a_1 + \ldots + a_5)$$

$$p_{III} = - (p_I + p_{II}) \qquad\qquad (4.3)$$

$$d_c = - (d_A + d_B)$$

Substituting

$$y = m$$
$$+ a_1(u_1 - u_6) + \ldots + a_5 (u_5 - u_6)$$
$$+ p_I(v_I - v_{III}) + p_{II}(v_{II} - v_{III})$$
$$+ d_A(w_A - w_c) + d_B(w_A - w_c)$$
$$+ e$$

Now, letting

$$x_1 = u_1 - u_6, \quad \cdots \quad , \quad x_5 = u_5 - u_6$$

$$x_6 = v_I - v_{III}, \qquad x_7 = v_{II} - v_{III}$$

$$x_8 = w_A - w_C, \qquad x_9 = w_B - w_C$$

We obtain the regression model

$$y = m + a_1 x_1 + \ldots + a_5 x_5 + P_I x_6 + P_{II} x_7 + d_A x_8 + d_B x_9 + e \qquad (4.4)$$

where for each observation

y = sales

$$x_1 = \begin{cases} 1 & \text{if cell refers to market 1} \\ -1 & \text{"} \qquad\qquad\qquad\qquad 6 \\ 0 & \text{otherwise} \end{cases}$$

.
.
.

$$x_5 = \begin{cases} 1 & \text{if cell refers to market 5} \\ -1 & \text{"} \qquad\qquad\qquad\qquad 6 \\ 0 & \text{otherwise} \end{cases}$$

$$x_6 = \begin{cases} 1 & \text{if cell refers to period I} \\ -1 & \text{"} \qquad\qquad\qquad\quad III \\ 0 & \text{otherwise} \end{cases}$$

$$x_7 = \begin{cases} 1 & \text{if cell refers to period II} \\ -1 & \text{"} \qquad\qquad\qquad\quad III \\ 0 & \text{otherwise} \end{cases}$$

$$x_8 = \begin{cases} 1 & \text{if cell refers to treatment A} \\ -1 & \text{"} \qquad\qquad\qquad\qquad C \\ 0 & \text{otherwise} \end{cases}$$

$$x_9 = \begin{cases} 1 & \text{if cell refers to treatment B} \\ -1 & \text{"} \qquad\qquad\qquad\qquad C \\ 0 & \text{otherwise} \end{cases}$$

The regression will not estimate a_6 , P_{III}, d_C directly but they are given by substitution into (4.3).

In this experiment there are 18 observations. The regression input can be tabulated as follows (blanks are zeros):

Obs. No.	y	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9
1	1.585	1					1		1	
2	3.142	1						1		1
3	5.089	1					-1	-1	-1	-1
4	3.355		1				1			1
5	5.286		1					1	-1	-1
6	1.471		1				-1	-1	1	
7	5.494			1			1		-1	-1
8	1.676			1				1	1	
9	3.249			1			-1	-1		1
10	1.989				1		1		1	
11	5.583				1			1	-1	-1
12	3.450				1		-1	-1		1
13	3.765					1	1			1
14	1.975					1		1	1	
15	5.601					1	-1	-1	-1	-1
16	5.899	-1	-1	-1	-1	-1	1		-1	-1
17	3.740	-1	-1	-1	-1	-1		1		1
18	1.966	-1	-1	-1	-1	-1	-1	-1	1	

The output from a typical regression program is shown below.

Analysis of Experimental Design by Regression

```
CØVARIANCE MATRIX
  1   1  3.333333E-01
  2   1  1.666666E-01 3.333333E-01
  3   1  1.666666E-01 1.666666E-01 3.333333E-01
  4   1  1.666666E-01 1.666666E-01 1.666666E-01 3.333333E-01
  5   1  1.666666E-01 1.666666E-01 1.666666E-01 1.666666E-01 3.333333E-01
  6   1  0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99
  7   1  0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99
  8   1  0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99
  9   1  0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99 0.000000E-99
 10   1 -9.938888E-02 -8.294444E-02 -6.588888E-02 -3.238888E-02 -1.466666E-02
  6   6  6.666666E-01
  7   6  3.333333E-01 6.666666E-01
  8   6  0.000000E-99 0.000000E-99 6.666666E-01
  9   6  0.000000E-99 0.000000E-99 -3.333333E-01 6.666666E-01
 10   6  7.005555E-02 -3.200000E-02 -1.238333E-00 -6.806111E-01 2.362135E+00
```

```
MEANS
  1  .000000E-99 .000000E-99 .000000E-99 .000000E-99 .000000F-99
  6  .000000F-99 .000000E-99 .000000E-99 .000000E-99 .357305E+01
```

```
STANDARD DEVIATIØNS
  1  .577350E-00 .577350E-00 .577350E-00 .577350E-00 .577350E-00
  6  .816496E-00 .816496E-00 .816496E-00 .816496E-00 .153692E+01
```

```
CØRRELATIØN MATRIX
  1   1  1.000000
  2   1   .500000  1.000000
  3   1   .500000   .500000  1.000000
  4   1   .500000   .500000   .500000  1.000000
  5   1   .500000   .500000   .500000   .500000  1.000000
  6   1  0.000000  0.000000  0.000000  0.000000  0.000000
  7   1   .000000  0.000000  0.000000  0.000000  0.000000
  8   1  0.000000  0.000000  0.000000  0.000000  0.000000
  9   1   .000000  0.000000  0.000000  0.000000  0.000000
 10   1  -.112007  -.093475  -.074254  -.036500  -.016528
  6   6  1.000000
  7   6   .500000  1.000000
  8   6  0.000000  0.000000  1.000000
  9   6  0.000000  0.000000  -.500000  1.000000
 10   6   .055825   .025500  -.986803  -.542365  1.000000
```

```
MULT. R = .9999,   ST. ERR. ØF EST. =    .0044,   F( 9, 8) =
SUM ØF RESIDUALS =      0.0000,   SUM ØF SQUARED RESIDUALS =      .0001
TABLE ØF RESULTS
VAR    B-CØEFF    ST.ERR.B        T    PARTIAL     BETA

  0     3.5730      .0010      VARIABLE  0 IS A CØNSTANT
  1     -.3010      .0023   -129.4464     -.9997     -.1130
  2     -.2023      .0023    -87.0222     -.9994     -.0760
  3     -.1000      .0023    -43.0214     -.9978     -.0375
  4      .1009      .0023     43.4036      .9978      .0379
  5      .2072      .0023     89.1243      .9994      .0778
  6      .1081      .0014     73.4994      .9992      .0574
  7     -.0060      .0014     -4.1168     -.8242     -.0032
  8    -1.7960      .0014  -1221.0494     -.9999     -.9541
  9     -.1228      .0014    -83.5460     -.9994     -.0652

INVERSE MATRIX
  0  0  5.555555E-02
  1  0  0.000000E-99  2.777777E-01
  2  0  0.000000E-99 -5.555555E-02  2.777777E-01
  3  0  0.000000E-99 -5.555555E-02 -5.555555E-02  2.777777E-01
  4  0  0.000000E-99 -5.555555E-02 -5.555555E-02 -5.555555E-02  2.777777E-01
  5  0  0.000000E-99 -5.555555E-02 -5.555555E-02 -5.555555E-02 -5.555555E-02
  6  0  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99
  7  0  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99
  8  0  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99
  9  0  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99  0.000000E-99
  5  5  2.777777E-01
  6  5  0.000000E-99  1.111111E-01
  7  5  0.000000E-99 -5.555555E-02  1.111111E-01
  8  5  0.000000E-99  0.000000E-99  0.000000E-99  1.111111E-01
  9  5  0.000000E-99  0.000000E-99  0.000000E-99 -5.555555E-02  1.111111E-01
```

Although standard errors are tabulated by the program for the constants of the regression, an additional step is required to find the standard error of a difference between two coefficients. As an example we find

$$SE(\hat{d}_A - \hat{d}_B) = \sqrt{V(\hat{d}_A - \hat{d}_B)}$$

where SE stands for standard error, V for variance and the carat denotes the regression estimate. Using the general formula for the variance of a sum

$$V(\hat{d}_A - \hat{d}_B) = V(\hat{d}_A) + V(\hat{d}_B) - 2 \, Cov(\hat{d}_A, \hat{d}_B)$$

Simply squaring the tabulated standard error, we find that

$$V(\hat{d}_A) = V(\hat{d}_B) = 1.96 \times 10^{-6}$$

The covariance term can be found from an appropriate inverse matrix. The particular inverse printed out varies somewhat from program to program. Here the inverse of the sum of cross products matrix is given. Consider two arbitrary independent variables x_i and x_j with coefficients b_i and b_j. Then

$$Cov(\hat{b}_i, \hat{b}_j) = (i,j \text{ element of inverse})(s^2)$$

where s = the standard error of estimate of the regression. Thus,

$$Cov(\hat{d}_A, \hat{d}_B) = -(.0555)(.0044)^2 = 1.08 \times 10^{-6}$$

and so

$$SE(\hat{d}_A - \hat{d}_B) = .0025$$

If the inverse of the correlation matrix is printed out, the covariance calculation is slightly different. Let r^{ij} = the (i,j) element in the inverse, σ_i be the standard deviation of x_i, and n the number of observations. Then

$$Cov(\hat{b}_i, \hat{b}_j) = \frac{r^{ij}}{n\sigma_i\sigma_j} s^2$$

One task remains. We can find $\hat{a}_6$, $\hat{p}_{III}$, and $\hat{d}_c$ from (4.3) but their standard errors and possibly their covariances are also desired. The problem is trivial here, since all the standard errors for the a's are the same, all the p's the same, etc. In the general case, the regression output can be manipulated to obtain the numbers, but an easier way is to run two more

regressions. A regression with, say, a_5, p_{II}, and d_B as the constants eliminated will produce $\hat{a}_6$, $\hat{p}_{III}$, and $\hat{d}_c$ and their standard errors. The data to calculate all covariances except $Cov(\hat{a}_5, \hat{a}_6)$, $Cov(\hat{p}_{II}, \hat{p}_{III})$, and $Cov(\hat{d}_B, \hat{d}_c)$ is also easily available. Data for the latter can be produced by a regression having a_4, p_I, and d_A eliminated.

Another variation of the regression can be useful. Suppose we are specifically interested in the differences

$$a_1 - a_6$$
$$; a_2 - a_6$$
$$.$$
$$.$$
$$.$$
$$a_5 - a_6$$

Then, instead of eliminating a_6 in (4.2) by converting the u's to the x's, we can resolve the singularity caused by the a's in a more useful way. Let x_o be a variable that takes on the value unity for every observation. Then

$$u_6 = x_o - \left[u_1 + \ldots + u_5\right] .$$

Substitution into (4.2) produces a regression model with a constant term of $m + a_6 x_o$ and the desired differences as coefficients of u variables. In setting up the variables, x_o is not introduced; the constant term will automatically include a_6 . The v and w singularities can be resolved similarly or by the x method. When the regression is run, the standard errors of the differences we have set up can be read off directly as standard errors of the corresponding coefficients.

In summary, multiple regression computer programs provide a general means of analyzing experiments to estimate parameters and their standard errors. To use multiple regression in this way it is necessary to know: (1) the basic statistical model being assumed; (2) how to set the model up in regression form; (3) how to determine covariances of the regression

coefficients from the computer output; (4) how to express variances of sums of random variables in terms of their variances and covariances.

An excellent use of this type of analysis is in planning an experiment. Frequently designs are proposed that differ from standard form. Perhaps some combination of treatments is not operationally possible. What would happen if that combination were omitted? A dry run of the analysis will be helpful in deciding whether important accuracy is lost. Such a dry run could be constructed as above, or, better, live historical data might be used, perhaps adding in effects of the approximate size anticipated.

5.0 Accuracies Obtained in Practice

Quite a variety of sales experiments have been conducted over the past 10 years. As might be expected, most of them are unpublished, although the Department of Agriculture Series is a happy exception. The writer has collected together a certain amount of design and accuracy information from a number of experiments. The data is displayed in Table 5.1

The purpose of the table is to give an indication of what has been done in the past and particularly to show the range of standard errors that have been encountered. The coefficient of variation for an experimental unit is a fundamental design parameter for picking sample size. The standard error of the difference in treatment means shows how big a sales effect the treatments must produce in order to be detected.

The types of experimental design that have been used include: randomized blocks, Latin squares with and without change-over, and factorial designs.

The experimental treatments have included total advertising expenditures, advertising expenditures in specific media (especially television and nespapers), product appearance, point of purchase display, and promotional themes.

Types of sales measurement have included store audits, factory shipments, and consumer surveys.

Direct mail experiments have been excluded from the table.

Table 5.1

Design Data from Past Experiments

Experiment	Type of Expt. Sales Unit	Unit Period	No.of Expt. Units	Duration of Expt.(2)	Dependent Variable	CV for One Expt. Unit(3)	SE(diff.in treatment means) (% of ave. sales)(4)
1	territory	1 yr.	3[1]	1 yr.	sales	8.1%	5.1%
2	territory	6 mos.	6[1]	6 mos.	sales	7.4	4.7
3	market	1 yr.	27[1]	1 yr.	sales	9.2	2.4
					market share	5.7	1.5
					industry sales	7.5	2.0
4	market	8 mos.	30	8 mos.	market share	22.9	13.2
5	market	1 yr.	16[1]	1 yr.	market share	7.4	3.7
6	market	6 wks.	128	6 wks.	market share	7.4	1.3
7	market	1 yr.	27	1 yr.	sales	2.5	1.2
8	market	1 yr.	54	1 yr.	market share	8.0	2.7
9	store group in market	4 wks.	24	16 wks.	sales	8.9	4.4
					industry sales	7.6	3.9
10	store	1 wk.	135	9 wks.	sales	33.3	7.0
11	store group in market	6 wks.	18	18 wks.	sales	10.1	6.4
					rest of industry sales	8.9	5.1
12	store group in market	4 wks.	25	5 mos.	sales	12.6	8.0
					rest of industry sales	8.0	4.2

Notes: (1) In addition, a certain number of units not receiving special treatment were included in the analysis.

(2) Sales data was sometimes collected over a longer period than shown in order to obtain a base period or to check for residual effects.

(3) CV = coefficient of variation = estimated standard deviation for an experiment unit/average sales per experimental unit. Where additional explanatory variables were used in regression or covariance models, the adjusted standard deviation (standard error of residuals) is used.

(4) SE = standard error. Where several different treatments were tested, the standard error of the difference between some important pair of means is given. For continuous variables spread over a range, an attempt has been made to compute the standard error for a comparable two level experiment.

6.0 Adaptive Allocation

We return to the question raised by Figure 1 (page 4): How can we design more effective systems for controlling marketing variables? The elements of a control system should include a model of sales response, a means of allocating market effort on the basis of the model, and devices for keeping the model up to date. Such devices may well include the deliberate perturbation of marketing variables to facilitate response measurements. In addition, data would be collected on such quantities as product availability, consumer attitudes, and competititive activities.

Every company obviously has some procedure for controlling marketing variables, but usually the relationship between the data input and the setting of the marketing variables is not at all formally specified. Our interest is in specifying appropriate relationships and appropriate data inputs and in studying their effect on overall company performance. Our presumption is that by careful systems design we may be able to achieve better performance than is achieved now.

Formal systems design in this sense appears to be a good way off. What we shall do here is set up and solve a simple model embodying some of the ideas that seem relevant to the larger problem.

The model may be described briefly as follows: company sales (and therefore profits) are a function of a marketing variable x, say, a rate of spending money of promotion. The sales response to x changes with time. The change takes the form of a changing parameter in the sales response function. The value of the parameter is estimated in each time period by an experiment.

The promotion rate for each period is picked by using information about the parameter as collected in past experiments. Our problem is to determine how to design the experiments and how to use the resulting information in picking the promotion rate.

6.1 Profit Model

Let s = sales rate (dol/hh.yr.)
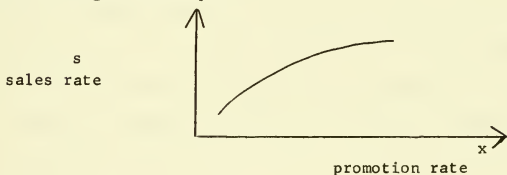
   x = promotion rate   "

   p = profit rate   "

   c = fixed cost rate   "

   m = gross margin, the incremental profit as a fraction of sales

Then

$$p = ms - x - c \qquad (6.1)$$

We shall suppose that at some fixed point in time there is a sales response curve of the general shape:

Presumably, the curve can be approximated, at least near the current operating point, by a quadratic function of x:

$$s = \alpha + \beta x - \gamma x^2 \qquad (6.2)$$

The parameters α, β, and γ are constants at a fixed point in time but some of them may vary with time.

The value of x, say $x*$, that maximizes profit is easily determined:

$$\frac{dp}{dx} = m \left[\beta - 2\gamma x \right] - 1 = 0$$

$$x* = \frac{\beta - 1/m}{2\gamma} \qquad (6.3)$$

Let $p(x*)$ = maximum profit rate (dol/hh.yr)

$p(x)$ = profit rate using x (dol/hh.yr.)

$\ell(x)$ = $p(x^*)$ - $p(x)$ = loss of profit rate if x is used instead of x^* .

$\ell(x)$ = $m\left[\beta(x^* - x) - \gamma(x^{*2} - x^2)\right]$ - $(x^* - x)$ which after manipulation

and use of (6.3) becomes

$$\ell(x) = m\gamma(x - x^*)^2 \tag{6.4}$$

6.2 Model for Changes in Sales Response

If α, β, and γ were known, we would set $x = x^*$ and obtain $\ell(x^*) = 0$.
However, the parameters are presumably fairly difficult to measure and we
ordinarily expect to come up with some non-optimal x and therefore to incur a
relative loss.

If the parameters did not change with time, we would want to put a big
effort into measuring them right away, because the extra profit from increased
accuracy would extend far into the future. However, it is difficult to believe
that in practice the parameters stay constant. Thus, for example, competitive
activity, product changes, and shifts in economic conditions lead us to expect
shifts in response. Consequently, an expensive effort to learn the parameters
immediately cannot be justified. On the other hand, the parameters may change
fairly slowly with time, in which case some effort is worthwhile. In each
time period new information is collected, combined with the old and used to
set operations in the immediate future.

To set up a fairly simple model of changing sales response, we shall
suppose that α and β change with time but γ does not. At a fixed period
in time we assume that national sales rate for the product is:

$$s = \alpha + \beta x - \gamma x^2 \tag{6.5}$$

and that

> α is a random variable with high variance from one time period to the next,
>
> β is a random variable that changes with time as specified below,
>
> α and β are independent

γ is a known constant

(As implied by (6.3) and as will be verified, the information available about α does not directly affect the optimal x. However, information about α may make it possible to learn more about β in an experiment. The assumption of high variance for α, (which may well be fairly realistic), simplifies the statistical analysis by removing α as a contributor to information about β.)

The parameter β will be assumed to be generated by a random walk. One possibility is

$$\beta(t) = \beta(t-1) + \epsilon(t)$$

where $\epsilon(t)$ = a random variable with mean = 0 and variance = σ_β^2.

We shall assume $\epsilon(t)$ is normally distributed and independent of previous values of β and ϵ

The difficulty with the above random walk is that β is likely to wander unrealistically far from its starting value. Therefore we shall hypothesize a long run average value and a tendency for β to return to that value. Specifically, let

β^o = the long run average value of $\beta(t)$

k = a constant, $0 < k < 1$.

We assume

$$\beta(t) = k\beta(t - 1) + (1 - k)\ \beta^o + \epsilon(t) \qquad (6.6)$$

Thus, a plot of $\beta(t)$ vs. t might look as follows:
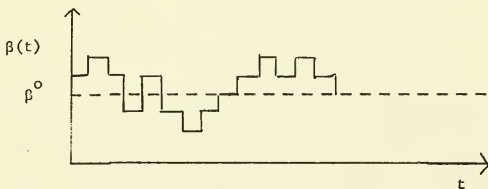
Several properties of the β process are noted:

$$E\big(\beta(t)\big) = k\, E\big(\beta(t-1)\big) + (1-k)\, \beta^o \tag{6.7}$$

This is a difference equation in the expected value of $\beta(t)$. The steady state solution is

$$E\big(\beta(t)\big) = \beta^o \tag{6.8}$$

as implied by our verbal definition of β^o.

$$V\big(\beta(t)\big) = k^2 V\big(\beta(t-1)\big) + \sigma_\beta^2 \tag{6.9}$$

The steady state variance is

$$V\big(\beta(t)\big) = \frac{\sigma_\beta^2}{1-k^2} \tag{6.10}$$

6.3 An experiment on $\beta(t)$

Although national sales rate is given by (6.5), individual markets are assumed to differ from one to another at a fixed t according to

$$s = s(x) + \epsilon \tag{6.11}$$

where $\quad$ s = sales rate in the market (dol/hh.yr.)

$\qquad$ s(x) = sales rate from (6.5) $\qquad$ "

$\qquad$ ϵ = a random variable for the market "

We take ϵ as normally distributed, independent from market to market and as having mean = 0 and variance = σ^2.

An experiment to estimate $\beta(t)$ is presented in the following sketch:
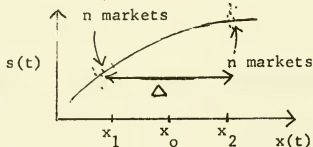
At a given t, suppose that we have picked a promotion rate, $x_o(t)$, that is in some sense best. This is used everwhere except that in n markets a deliberately low value, x_1, is used, and, in another n, a high value, x_2, is used.

We choose

$$x_1 = x_o(t) - \triangle/2$$
$$x_2 = x_o(t) + \triangle/2 \ . \tag{6.11}$$

Let $\bar{s}_1$ and $\bar{s}_2$ be the observed mean sales rates in the markets at x_1 and x_2 respectively. An estimate of $\beta(t)$ can be computed from the experimental data.

Let

$$\hat{\beta}(t) = \frac{\left(\bar{s}_2 + \gamma x_2^2\right) - \left(\bar{s}_1 + \gamma x_1^2\right)}{\triangle} \tag{6.12}$$

This will be called the "experimental mean." Let

$$v = V\left(\hat{\beta}(t)\right)$$

From (6.11), (6.5), and (6.12) we see that, given $\beta(t)$, $\hat{\beta}(t)$ is normally distributed with mean and variance

$$E\left(\hat{\beta}(t)\right) = \beta(t)$$
$$v = \frac{2}{n} \frac{\sigma^2}{\triangle^2} \tag{6.13}$$

Notice that v does not depend on $\beta(t)$ or t.

The experimental result (6.12) does not represent all our information about $\beta(t)$. Even before doing the experiment, we had the information developed in previous experiments. The information will be summarized in a prior distribution of $\beta(t)$. This distribution will be taken to be normal with

$$E'\left(\beta(t)\right) = \text{prior mean of } \beta(t)$$
$$v' = \text{prior variance of } \beta(t)$$

At the beginning of period t, when the promotional rate, $x(t)$, is to be set, we have only the prior distribution of $\beta(t)$. At the end of t, the experimental results are available and we can construct a posterior distribution. The additional information about $\beta(t)$, however, is of no help in t although it will be useful in $t+1$.

6.4 Decision Rule for Setting x(t)

We propose a simple decision rule for setting the promotion rate in each time period.

Choose a number, a, such that $0 < a < 1$. At each t compute $E'\big(\beta(t)\big)$ from

$$E'\big(\beta(t)\big) = a\, E'\big(\beta(t-1)\big) + (1-a)\hat{\beta}(t-1) \tag{6.14}$$

then compute

$$x_o(t) = \frac{E'\big(\beta(t)\big) - 1/m}{2\gamma} \quad . \tag{6.15}$$

The decision rule is to set

$$x(t) = x_o(t) \; . \tag{6.16}$$

Suppose the process starts at $t = 1$. It is necessary to pick a value $E'\big(\beta(0)\big)$ but, thereafter, $x(t)$ is set mechanically by the rule. $\Big($Since (6.14) is an exponential smoothing process, the effect of $E'\big(\beta(0)\big)$ on $E'\big(\beta(t)\big)$ and thence $x(t)$ decreases exponentially with t.$\Big)$

Notice that the rule as stated makes no assumptions about the underlying process generating $\alpha(t)$ and $\beta(t)$ and, if we think of γ as an arbitrary positive constant, the rule is not tied down to any specific sales response process. The rule can be applied to any situation in which the experiment of Section 6.3 is performed, provided that somebody is willing to pick $\underline{a}$ and γ plus $E'\big(\beta(0)\big)$ and the experimental design parameters Δ and n.

The behavior of the decision rule will be clearer if we express it somewhat differently. Putting (6.12) into (6.14) and the latter into (6.15) gives

$$x_o(t) = a\left[\frac{E'\big(\beta(t-1)\big) - 1/m}{2\gamma}\right] + (1-a)\left[\frac{(\bar{s}_2 - \bar{s}_1) + (x_2^2 - x_1^2)}{2\gamma\Delta} - \frac{1}{2\gamma}\right] .$$

$$= a\, x_o(t-1) + \frac{(1-a)}{2\gamma}\left[\frac{\bar{s}_2 - \bar{s}_1}{\Delta} + \frac{\gamma(x_2 + x_1)(x_2 - x_1)}{\Delta} - 1/m\right] .$$

Then, using (6.11) and making the time period explicit for $\bar{s}_2$ and $\bar{s}_1$, we obtain

$$x_o(t) = x_o(t-1) + \frac{(1-a)}{2\gamma}\left[\frac{\bar{s}_2(t-1) - \bar{s}_1(t-1)}{\Delta} - 1/m\right]. \tag{6.17}$$

The quantity $\left[\bar{s}_2(t-1) - \bar{s}_1(t-1)\right]\Big/\Delta$ is an experimental estimate of the slope of the chord of the sales response curve between x_1 and x_2. If the slope

is greater than $1/m$, the implication is that x_2 is more profitable than x_1; if the slope is less, the reverse is true.

Thus the decision rule says to use in t approximately the same promotion rate as in t-1 but adjust it upward or downward depending on whether the experimental evidence from t-1 suggests a slope greater or less than $1/m$. The amount of the adjustment is controlled by the constant $(1-a)/2\gamma$. If the constant is large, the promotion rate will be sensitive to the most recent experimental results; if the constant is small, insensitive.

Although an appropriate choice of constants is critical for reasonable operation, the general form of (6.17) provides an adaptive control system that might be expected to work fairly well for a variety of underlying sales response mechanisms. One might hope that if the constants are chosen with one mechanism in mind, they might work satisfactorily with other mechanisms not too different. Whether or not this is so in a specific case is a question that can be explored by means of simulation.

For the sales response model being assumed we now wish to motivate the decision rule more carefully and then go on to pick values for a, n, and which are optimal or close to optimal.

6.5 Choosing a

Consider first the problem of finding the posterior distribution of $\beta(t)$ given the prior distribution the the experimental results. Let

$$E'\big(\beta(t)\big) = \text{prior mean}$$
$$\hat{\beta}(t) = \text{experimental mean}$$
$$E''\big(\beta(t)\big) = \text{posterior mean}$$

$$v' \qquad = \text{prior variance}$$
$$v \qquad = \text{experimental variance}$$
$$v'' \qquad = \text{posterior variance}$$

Since the prior of $\beta(t)$ is normal and the distribution of $\hat{\beta}(t)$ given $\beta(t)$ is normal, the posterior of $\beta(t)$ is normal and, as can be shown (see [19] p. 294-5), has mean and variance

$$E''\left(\beta(t)\right) = \frac{1/v'}{1/v + 1/v'} E'\left(\beta(t)\right) + \frac{1/v}{1/v + 1/v'}\hat{\beta}(t)$$

$$\frac{1}{v''} = \frac{1}{v} + \frac{1}{v'} \quad . \tag{6.18}$$

The process generating $\beta(t)$ has been specified in (6.6):

$$\beta(t) = k\beta(t-1) + (1-k)\beta^o + \in \beta(t) \tag{6.6}$$

As of the beginning of t, we have

$$E'\left(\beta(t)\right) = k\, E''\left(\beta(t-1)\right) + (1-k)\beta^o \tag{6.19}$$

To simplify notation, let

$$a = \frac{1/v'}{1/v + 1/v'} \tag{6.20}$$

Then, substitution of (6.18) into (6.19) yields as the prior mean

$$E'\left(\beta(t)\right) = k\, a\, E'\left(\beta(t-1)\right) + k(1-a)\hat{\beta}(t-1) + (1-k)\beta^o \tag{6.21}$$

If k is near one, a sufficiently good approximation may perhaps be obtained by setting k = 1 in the above expression. This has the advantage of eliminating k and β^o as parameters in the decision rule and so we shall do it. However, there is no fundamental difficulty in carrying along k and β^o. In that case (6.14) would be replaced by (6.21).

Under the k = 1 approximation, (6.21) reduces to (6.14), thereby justifying an expression that was simply postulated in Section 6.4:

$$E'\left(\beta(t)\right) = a\, E'\left(\beta(t-1)\right) + (1-a)\hat{\beta}(t-1), \tag{6.14}$$

In addition, we now have a value for a, or rather, we will have as soon as we find an expression for v' in terms of the given parameters of the model.

To do this, take the variance of each side of (6.6). Using ' to denote prior and " posterior, we obtain

$$V'\big(\beta(t)\big) = k^2 \, V''\big(\beta(t-1)\big) + \sigma_\beta^{\ 2}$$

Using the $k = 1$ approximation, the above plus (6.18) gives

$$v' = v'' + \sigma_\beta^{\ 2}$$

$$\frac{1}{v''} = \frac{1}{v} + \frac{1}{v'} \ .$$

Solving,
$$v' = \frac{1}{2}\sigma_\beta^{\ 2} \left[1 + \left\{ 1 + 4v/\sigma_\beta^{\ 2} \right\}^{\frac{1}{2}} \right] \ . \tag{6.22}$$

Since
$$v = \frac{2}{n} \frac{\sigma^2}{\Delta^2} \tag{6.23}$$

we can now set $\underline{a}$ by (6.20) .

6.6 Choosing $x_o(t)$

Profit rate is a random variable because $s(t)$ is:

$$p(t) = m \, s(t) - x(t) - c$$

$$s(t) = \alpha(t) + \beta(t)x(t) - \gamma \, x^2(t)$$

Since we cannot maximize true profit we choose to maximize expected profit. Suppressing t for the moment,

$$E'(p) = m \left[E'(\alpha) + E'(\beta)x - \gamma \, x^2 \right] - x - c \ .$$

This is maximized by choosing $x(t)$ to be

$$x_o(t) = \frac{E'\big(\beta(t)\big) - 1/m}{2\gamma}$$

This justifies (6.15) of Section 6.

At this point we have justified the decision rules of Section 6 for our specific sales response model, at least to the extent of the $k = 1$ approximation. It remains to pick the parameters n and Δ of the experiment.

6.7 Designing the Experiment

The experimental parameters will be picked to minimize the sum of two losses: the loss because we do not know $\beta(t)$ exactly and the loss we incur trying

to learn $\beta(t)$ better.

With perfect information we would choose;   Instead we are choosing:

$$x^*(t) = \frac{\beta(t) - 1/m}{2\gamma} \quad . \qquad\qquad x_o(t) = \frac{E'\big(\beta(t)\big) - 1/m}{2\gamma} \quad .$$

The loss compared to perfect information is seen from (6.4) to be

$$\ell = m\gamma \left[x_o(t) - x^*(t) \right]^2 .$$

Notice above that

$$E'\big(x^*(t)\big) = x_o(t)$$

$$\therefore \quad E'(\ell) = m\gamma V'\big(x^*(t)\big)$$

$$= m\gamma \left[\frac{V'\big(\beta(t)\big)}{4\gamma^2} \right]$$

$$E'(\ell) = \frac{m}{4\gamma} v' \tag{6.24}$$

This is the expected loss rate relative to perfect information when we use $x_o(t)$.

In the 2n markets used for the experiment the expected loss rate is higher because the promotion rate is deliberately set to be different from the best available value, $x_o(t)$. Suppressing t for the moment, the experimental promotion rates are

$$x_1 = x_o - \Delta/2$$

$$x_2 = x_o + \Delta/2$$

Consider a market at x_1. Its loss rate compared to perfect information is

$$\ell_1 = m\gamma \left[x_1 - x^* \right]^2$$

$$= m\gamma \left[(x_o - x^*)^2 - \Delta(x_o - x^*) + \Delta^2/4 \right].$$

Therefore $\qquad E'(\ell_1) = m\gamma V(x^*) + m\gamma \Delta^2/4$.

$$= \frac{m}{4\gamma} v' + m\gamma \Delta^2/4 \ .$$

The same expected loss rate will occur for x_2 . We see that the <u>extra</u> cost, ℓ_e, of the experimental deviation is

$$\ell_e = m\gamma \Delta^2/4 \tag{6.25}$$

The total expected loss rate can now be computed. Let

$\qquad\qquad$ N = total number of markets in the country

$\qquad\qquad$ 2n = the number of experimental markets

$\qquad\qquad$ P = the average number of households in a market

$\qquad\qquad$ L = total expected loss (dol/yr)

$$L = N P \frac{m}{4\gamma} v' + \frac{1}{2} P m \gamma n\Delta^2 \tag{6.26}$$

The experimental design parameters n and Δ are to be picked to minimize L. First we observe from (6.26) and (6.22) that n and Δ always appear in L in the combination $n\Delta^2$. We shall find $n\Delta^2$ to minimize L. Then we can more or less trade off n against Δ any way we wish as long as $n\Delta^2$ is kept to its minimizing value. Practically there are limitations (e.g. x_1 must be non-negative and 2n not greater than N, to cite two extremes) but the flexibility implied is interesting and desirable.

Rather than minimize L with respect to $n\Delta^2$, we minimize with respect to the dimensionless quantity

$$v = \frac{2\sigma^2}{n\Delta^2}$$

from which $n\Delta^2$ can immediately be calculated. Substituting $n\Delta^2 = 2\sigma^2/v$ in L, we obtain

$$L = N P \frac{m}{8\gamma} \sigma_\beta^2 \left\{ 1 + \left[1 + \frac{4v}{\sigma_\beta^2} \right]^{\frac{1}{2}} \right\} + P m\gamma \sigma^2/v$$

Setting $dL/dv = 0$ leads to an expression which optimal v must satisfy:

$$v = \frac{2\gamma\sigma}{\sqrt{N}} \left[1 + \frac{4v}{\sigma_\beta^2} \right]^{\frac{1}{4}} \qquad (6.27)$$

Although (6.27) contains v on both sides, it can frequently be solved iteratively by setting $v_1 = 0$ and recursively calculating

$$v_n = \frac{2\gamma\sigma}{\sqrt{N}} \left[1 + \frac{4v_{n-1}}{\sigma_\beta^2} \right]^{\frac{1}{4}}$$

for a few n until v_n converges to v .

6.8 A Numerical Example

To illustrate the behavior of the adaptive control system, we construct a numerical example of the model assumed, determine the optimal experiment and simulate the operation of the system under the proposed decision rule.

In this particular model the data assumed available to the company are:[*]

$\gamma = 100$ = curvature of sales response curve $(dol/hh.yr.)^{-1}$

$\sigma_\beta = 1$ = period to period standard deviation of $\beta(t)$ (dimensionless)

$\sigma = .016$ = standard deviation of sales rate for an individual market (dol/hh.yr.)

$N = 1000$ = number of market areas in the country

$m = 1/3$ = gross margin (dimensionless)

For perspective we note that national sales are in the neighborhood of 10 million dollars/year or .2 dol/hh.yr. and that national promotion rate is on the order of 1 million dollars/yr. or .02 dol/hh.yr. Thus σ implies a coefficient of variation for an experimental unit about 8%.

----

[*] There is little conceptual difficulty in setting up a model in which more parameters are measured (e.g. γ and σ_β), but the computations would be more complex.

To design the experiment, the appropriate data above are substituted into (6.27) which can be solved to give

$$v = .111,$$

$$\text{or } n\Delta^2 = 2\sigma^2/v = 48.0 \times 10^{-4} \text{ (dol/hh.yr.)}^2 .$$

If we choose Δ so that the high and low spending rates are 25% above and below the national rate when the latter is .02, we obtain

$$\Delta = .01 \text{ dol/hh.yr.}$$

Then

$$n = 48 \text{ markets.}$$

This completes the experimental design: In each time period 48 markets will be run at a rate .005 dol/hh.yr. above the national rate $x_o(t)$ and another 48 the same amount below. The resulting experimental standard error of β is .333 $(=v^{\frac{1}{2}})$. This makes the experiment a fairly precise one, especially when compared to the period to period standard error of $\beta(t)$ which is $\sigma_\beta = 1$. Since we are predicting future $\beta(t)$ from past estimates and β is changing fairly fast (standard error = 1), we will tend to rely on the most recent experiment (standard error=.333).

In order to operate we need smoothing constant $\underline{a}$. First v' is found from (6.22) to be 1.10. Then (6.20) gives

$$a = .092$$

and $(1-a)=.908$, so that we do indeed put most reliance on the most recent experiment.

The value of $x_o(t)$ at each t can be calculated from (6.14) and (6.15) or, combining them, from

$$x_o(t) = a \, x_o(t-1) + \frac{1-a}{2\sigma}\left[\hat{\beta}(t-1) - 1/m\right].$$

Specifically,

$$x_o(t) = (.092) \, x_o(t-1) + .00454\left[\hat{\beta}(t-1) - 3.0\right].$$

We are now set to operate the company: Given a starting value $x_o(0)$ and a sequence of experimental results $\hat{\beta}(t)$, we can generate the promotional rate $x_o(t)$.

The underlying market behavior is simulated from (6.6) using the specific parameters

$$k = .9$$

$$\beta_o = 7 \ .$$

This is done by generating a sequence of $\epsilon(t)$ having a normal distribution of mean zero and variance $\sigma_\beta^2 = 1$ and substituting them into

$$\beta(t) = .9 \ \beta(t-1) + .7 + \epsilon(t).$$

For the $\alpha(t)$ process, we take $\alpha(t) = .1$ dol/hh.yr. A varying of $\alpha(t)$ might be more realistic but, since it does not enter into any decisions, we simply make $\alpha(t)$ constant.

Given $\beta(t)$, the experimental results of t are simulated by

$$\hat{\beta}(t) = \beta(t) + \epsilon_{\hat{\beta}}(t) \ .$$

Here $\epsilon_{\hat{\beta}}(t)$ is a random normal number with mean zero and variance v=.111. (We could simulate each of the 2n test markets separately to generate $\hat{\beta}(t)$, but it is obviously easier to simulate the final experiment result directly.)

The company uses the $\hat{\beta}(t)$ to generate $x_o(t)$. The final sales outcome can be calculated from this, $\beta(t)$, α, and γ :

$$s(t) = .1 + \beta(t)x_o(t) - 100 \left[x_o(t)\right]^2 \ \text{dol/hh.yr.}$$

The sales, in turn, can be used to calculate profit. Our principal criterion, however, is the loss relative to profit we could make using perfect information. A side calculation gives the best promotion rate under perfect information:

$$x^*(t) = \frac{\beta(t)-3}{200} \ \text{dol/hh.yr.}$$

The loss rate relative to this is, from (6.4):

$$\ell = 33.3 \left[x^*(t)-x_o(t)\right]^2 \ \text{dol/hh.yr.}$$

The further loss resulting from the experimental deviations has not been simulated, but its expected value can be calculated from (6.25). For one market

$$\ell_e = 8.33 \times 10^{-4} \text{ dol/hh.yr.}$$

Although most quantities fluctuate with time, $\beta(t)$ does have a long-run average: $\beta^o = 7$. If this were always the value of $\beta(t)$, the optimal $x(t)$ would be $x^o = .02$ and the corresponding $s(t)$ would be $s^o = .20$. These values make convenient reference points: Losses can be expressed as a % of s^o and we can also compare performance of the adaptive system with a system that simply sets $x(t) = x^o$.

The simulation results are shown in Figure 6.1. 40 time periods are shown. (The series was started with $x(o) = x^o$ and $\beta(o) = \beta^o$ and ran for 10 periods before the present data was taken.) Plotted are $\beta(t)$, which is driving the system, $x_o(t)$, by which the company responds, and the resulting $s(t)$ and $\ell(t)$. The latter is expressed as a % of s^o.

We see that the response of the adaptive control system is quite good, although, by necessity, changes is $x(t)$ lag changes in $\beta(t)$ by one time period. The losses are generally small, although occasional peaks occur where $\beta(t)$ has changed substantially and $x(t)$ has not yet caught up. The 40 period average is:

$$\frac{\text{average } \ell(t)}{\text{as \% of } s^o} = .473$$

This may be compared with the expected value (under the $k=1$ appoximation) calculated from (6.24):

$$\frac{E'(\ell)}{s^o} = .457\%$$

The extra loss for experimental deviations in the 2n test markets as a % of national sales is
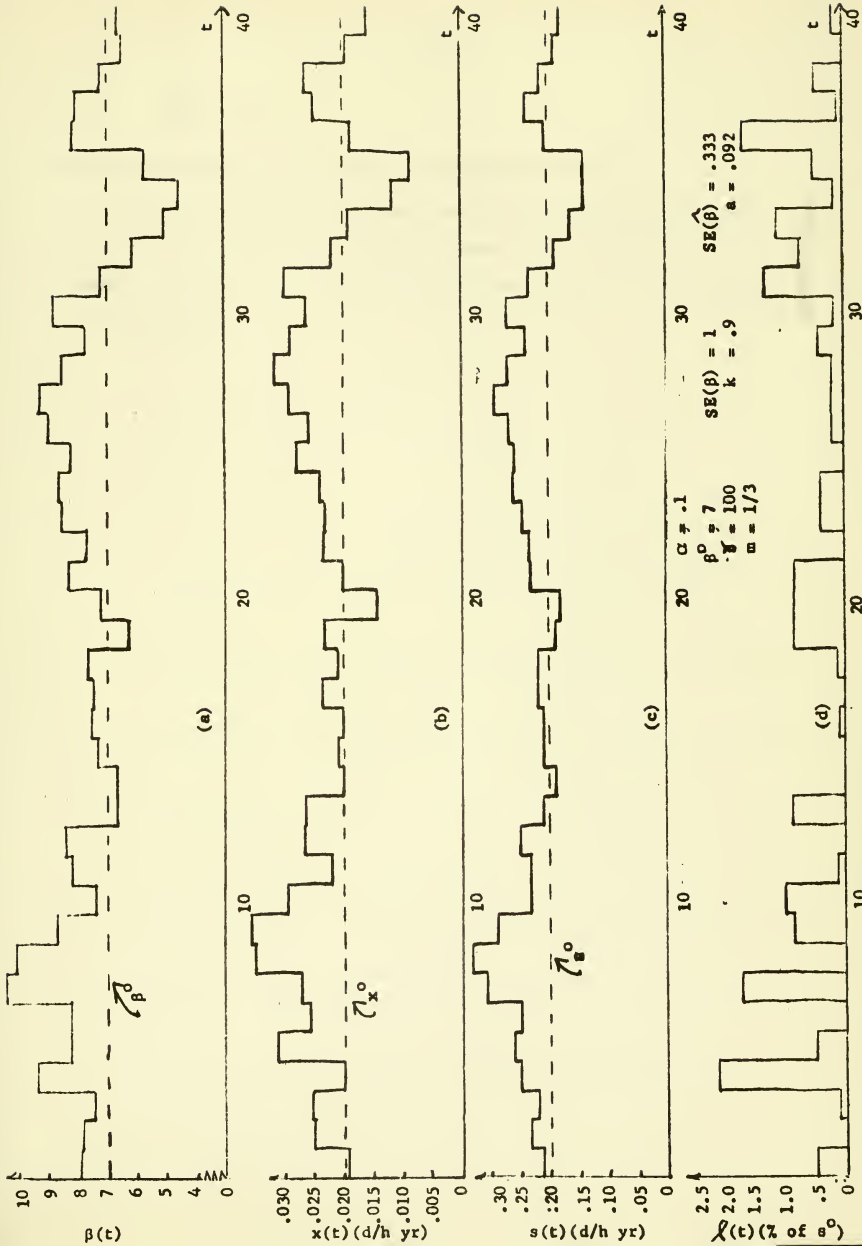
Figure 6.1 Simulation of Adaptive Allocation

$$\frac{2n_\ell e}{Ns^o} = .040\% \ .$$

The total expected loss is therefore .497% of the sales s^o .

By way of contrast, the average loss for a system using a constant $x(t) = x^o = .02$ over the 40 periods of Figure 6.1 is .854 % of s^o demonstrating the value of estimating and following $\beta(t)$ in the particular case considered.

Bibliography

1. P. L. Henderson, J. F. Hind, and S. E. Brown, "Sales Effect of Two Campaign Themes," _Journal of Advertising Research_, 1, 2-11, (December 1961) (also in 2).

2. R. E. Frank, A. A. Kuehn, and W. F. Massy, _Quantitative Techniques in Marketing Analysis_, Richard D. Irwin, Homewood, Illinois, 1962.

3. W. G. Cochrane and G. M. Cox, _Experimental Designs_, John Wiley, New York, 1952.

4. O. Kempthorne, _The Design and Analysis of Experiments_, John Wiley, New York, 1952.

5. D. R. Cox, _Planning of Experiments_, John Wiley, New York, 1958.

6. L. G. Telser, "Advertising and Cigarettes," _Journal of Political Economy_, 70, 471-499, (October 1962).

7. L. G. Telser, "Demand for Branded Goods as Estimated from Consumer Panel Data," _Rev. of Ec. and Stat._, 44, 300-324, (August 1962).

8.* M. Havas, M. G. Van Dress, H. R. Linstrom, and P. Kartalos, "Consumer Acceptance of Florida Oranges with and without Color Added," Marketing Research Report No. 537, U. S. Department of Agriculture, (May 1962).

9. P. L. Henderson, J. F. Hind, and S. E. Brown, "Promotional Programs for Lamb and Their Effects on Sales," Marketing Research Report No. 522, U. S. Department of Agriculture, (January 1962).

10. P. L. Henderson and S. E. Brown, "Effectiveness of a Special Promotional Campaign for Frozen Concentrated Orange Juice," Marketing Research Report No. 457, U. S. Department of Agriculture, (March 1961).

11. J. A. Howard and H. V. Roberts, "Experimentation and Marketing Prediction," unpublished paper.

12. R. J. Jessen, "A Switch-Over Experimental Design to Measure Advertising Effect," _Journal of Advertising Research_, 1, 15-22, (March 1961).

13. M. W. Sasieni, "What Can we Measure in Marketing," _Journal of Advertising Research_, 4, 8-11, (June 1964).

14. J. F. Hind, C. P. Eley, and C. R. Twining, "Special Promotional Programs for Winter Pears," Marketing Research Report No. 611, U. S. Department of Agriculture, (July 1963).

15. B. Benjamin and J. Maitland, "Operational Research and Advertising: Some Experiments in the Use of Analogies," _Operational Research Quarterly_, 9, 207-217, (September 1958).

* Department of Agriculture reports are obtainable from the Division of Information, Office of Management Services, U. S. Department of Agriculture, Washington 25, D. C.

16. A. A. Brown, F. T. Hulswit, and J. D. Kettelle, "A Study of Sales Operations," <u>Operations Research</u>, 4, 296-308, (June 56) (Reprinted in [17]).

17. Bass et. al., editors, <u>Mathematical Models and Methods in Marketing</u>, Richard C. Irwin, Homewood, Illinois, 1961.

18. H. V. Roberts, "Bayesian Statistics in Marketing," <u>Journal of Marketing</u>, 27, 1-4 (Jan. 63)

19. H. Raiffa and R. Schlaiffer, <u>Applied Statistical Decision Theory</u>, Graduate School of Business Administration, Harvard University, Boston, 1961.

20 M. E. Brunk and W. T. Federer, "Experimental designs and probability sampling in marketing research," <u>J. Am. Stat. Assoc</u>, 48, 440, (1953).

1751391
loan