LONG-RANGE FORECASTING
From Crystal Ball to Computer
Four RESEARCH STRATEGIES

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Sherlock Holmes was wrong. He should have listened to JEVONS (1877). Research over the past century has supported Jevons. This research has much to say about when to theorize. The first part of the chapter discusses the use of theory in forecasting.

Another strategy discussed here is breaking down a problem to solve it by its parts. You will get some ideas about how, why, and when to decompose.

Finally, the strategy of eclectic research$^G$ will be described. What is it and when should it be used?

Each of these three strategies is used at various places in this book. They are all relevant for developing and evaluating forecasting models.

**USING THEORY**

Actually, Sherlock Holmes was not completely wrong. The development of a theory does bias the judgment (Chapman and Chapman, 1969; Wason, 1960, 1968a,b; Pruitt, 1961; Rosenthal and Rosnow, 1969; Geller and Pitz, 1968), but biases can be reduced by specifying alternative theories. (I'll have more to say about this in Chapter 17.) But, my dear fellow Sherlock, what data are to be collected? How do I purge my mind of that half-baked theory I have? How do I incorporate my ideas into the picture after I have the data? (Sherlock obviously had no idea what he was getting into when he made that remark.)

There are few cases in the social sciences where we have "no data yet." Researchers have experience, they have done experiments, they have read the literature, and they feel that this background is relevant to the new situations they encounter. Most people feel that this information should be used in some way. The question is how to use these subjective data most effectively. Should subjective information be used before analyzing the problem or after? This question has been debated in many fields. Currently, for example, the conclusion in psychology is to use the theory before the data. In economics there is a bit of
Using Theory

hypocrisy, students are taught to use theory first, but practicing economists insist on using it afterward.

Here is what has happened in economics. Theoretical econometricians have advocated a prior use of theory. In fact, often they never get around to using any data at all. Those working on practical economic problems initially had little faith in prior theory; they used the exploratory approach. Such an approach was used in the 1930s and 1940s. For example, Burns and Mitchell (1946) conducted a famous study that was almost devoid of prior theory. Koopmans (1947) criticized this work—rightly so, I believe. The Rayco Case indicates the low regard that some analysts held for the use of prior theory:

In the case of the Rayco Seat Cover Company (Hummel, 1961), 300 variables were selected to “explain” variations in automobile seat cover sales per square mile. Simple plots of each variable against the sales measures for 150 sales offices eliminated 226 variables that appeared to be unrelated to sales. A stepwise regression then reduced the remaining 74 variables down to the best 37. This model was shown to produce an excellent fit to the data, but there was no evaluation of its usefulness in a predictive situation.

What happens when researchers fail to use their information before examining the data? They show great creativity afterward in using their subjective information. Economists use a fancy name for this, a posteriori analysis, but that does not help much. See Lucy; she is in Exhibit 4-1. Then see Tom Swift . . . and Solow:

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Exhibit 4-1  A POSTERIORI ANALYSIS: LUCY’S VIEWPOINT

By Schulz

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In this adventure of Tom Swift (see Armstrong, 1970b), we find Tom explaining differences in sales of caribou chips among 31 countries. An excellent explanation of the data was obtained by using a stepwise regression and selecting 8 predictors from a possible set of 30. The adjusted $R^2$ was 85%, . . . which is interesting because the data were all drawn from a table of random numbers. The data produced a theory, but it was useless.

Solow (1957) analyzed data from 1909 to 1949. These data fell along a straight line with the exception of those for the years 1943–1949. The 1943–1949 data were parallel to the other data but shifted above them. Solow devoted about one-half page to a hypothesis about this “structural shift.” He even drew upon other literature to support his hypothesis (he did, however, regard the hypothesis with a healthy amount of suspicion). A reply to Solow’s paper by Hogan (1958) indicated that, rather than a “structural shift,” the results were due to mistakes in arithmetic.

These studies show what can go wrong. What about studies that contrast theory and no theory? I examined this issue:

In Armstrong (1968a, pp. 160–165), two models were developed to forecast camera sales per capita in each of 11 countries. Each of these models was developed using data from 19 other countries. The exploratory model used little prior theory. Stepwise regressions were run, drawing from a set of 15 variables, and the model with the highest $R^2$ was selected as the forecasting model. A theory-based model was also developed by selecting seven variables, by putting a priori constraints on the signs, and by incorporating prior estimates of magnitudes. Although the exploratory model provided the best fit to the 19-country analysis data ($R^2$ of 99.8% vs. 99.6%), its performance in forecasting for the 11-country validation sample was inferior; the mean absolute percentage error ($MAPE^G$) was 52% vs. 31% for the theory-based model. The average percentage error ($APE^G$) of the theory-based model was also lower, $-5\%$ vs. 38%.
The evidence cited was drawn from economics and closely related fields. What about other areas? Sociologists and psychologists have also examined this issue.

I was impressed by the theoretical arguments of Francis (1957), a sociologist. He concluded that the theory should precede the data. It was difficult, however, to find empirical studies on this issue. Reiss (1951) provided indirect evidence. His evidence is consistent with the hypothesis that one should use prior theory:

Reiss (1951) studied whether juvenile delinquents would commit crimes again after they were released. Although a larger number of variables improved the fit of a model to the analysis sample, the predictive power dropped for the validation sample when the number of predictors was increased above four. Presumably, one could improve accuracy by using prior theory to reduce the number of predictor variables.

Three studies were found in psychology. These studies provide indirect evidence that is consistent with the greater value of *a priori* analysis relative to *a posteriori* analysis. In these studies, the judges used their implicit subjective theories after they analyzed the data. The result was a loss of predictive power:

Kelly and Fiske (1950) used objective tests to predict the success of students in a training program in clinical psychology. When the test data were examined by analysts who had interviewed the candidates, the resulting predictions were inferior to those provided by tests alone.

Harris (1963) examined the effects of using subjective methods, after using objective methods, in predicting football scores. The judges, who were sportswriters and football coaches, could not improve upon the forecasts made by an objective extrapolation known as the Litkenhous formula. The judges toned down the objective forecasts, and the forecasts from the "formula followed by subjective analysis" were less accurate than the "formula only" forecasts.
MILSTEIN [1980, 1981] found personal interviews to be worthless for predicting which applicants would be successful at the Yale School of Medicine.

Einhorn (1972b) discussed the dangers of proceeding without a theory and illustrated these hazards by reviewing the literature and by using simulations based upon random data. Stone and Brosseau illustrated these dangers without even trying to do so:

Stone and Brosseau (1973), in a study of student success in a medical training program, used 115 variables in a stepwise regression to explain differences among 19 subjects. This exploratory model was then used to predict success for 18 new subjects, and an $R^2$ of 76% was obtained. For this they won the “Tom Swift Award for Data Abuse” (Armstrong, 1975a); if one assumed that the data were random, the expected $R^2$ would have been higher than 76%. (For the 1978 Award, see BUHMeyer and Johnson [1978])

This evidence, although uniformly favoring the a priori over the a posteriori analysis, is limited. One study examined the question directly; one study found that a priori analysis made things better; and four studies found that a posteriori analysis made things worse. Practicing econometricians will continue to be skeptical. I think that they are wrong, and I'll tell you why . . . but I won't tell you until Chapter 8 (on econometric methods).

The researcher can use theory in many ways to develop a forecasting model. Many resources and sources of information exist that can be used in this a priori analysis. The details will be presented in Part II of LRF. Three of the important resources, however, are illustrated in Exhibit 4-2; they are (1) paper, (2) pencils, and (3) books. Another tool is time.

It would be nice to provide additional studies on the value of a priori analysis, but I have been unable to find any. Perhaps they do not exist; more likely, I have been looking in the wrong places.

Does this evidence mean that researchers should not use subjective inputs after analyzing the data? I think that it does. However, another good rule is never to say “always” or “never.” Always remember the story about the young economist who analyzed U.S. economic
data from 1930 to 1978. He found some dramatic deviations of the data from the model during the years 1940–1945. On reading the literature, he discovered that World War II occurred during this period.* In such a case, it is reasonable to revise the model.

Don’t laugh. The WW II example is not so strange as it may sound. Evans (1974, p. 174) cited the thesis written by a Ph.D. candidate at the University of Pennsylvania. The model failed to show any economic downturn during the Great Depression.

**DECOMPOSITION**

The spirit of decision analysis is to divide and conquer: Decompose a complex problem into simpler problems, get one’s thinking straight in these simpler problems, paste these analyses together with logical glue...  

Raiffa (1968, p. 271)

*This example and the Lucy cartoon (Exhibit 4-1) were passed on to me by Franklin Fisher at M.I.T.
Raiffa tells us to use decomposition for decision analysis. This section of Chapter 4 suggests that decomposition is also applicable in forecasting problems. It is useful in structuring forecasting models, obtaining forecasts, assessing uncertainty, and evaluating forecasting models.

Decomposition has a number of advantages. It allows the forecaster to use information in a more efficient manner. It helps to spread the risk; errors in one part of the problem may be offset by errors in another part. It allows the researchers to split the problem among different members of a research team. It makes it possible for expert advice to be obtained on each part. Finally, it permits the use of different methods on different parts of the problem.

The advantages of decomposition are impressive but probably a bit vague. When I was lecturing to a group of students about the benefits of decomposition, someone asked me, “How do you know?” After thinking about this, I concluded that I knew because other people had told me. I was once an industrial engineer and, as such, made much use of the strategy of decomposition. But I was surprised that I could not find empirical support for my belief. As a result, we conducted our own study. It supported two hypotheses:

1. Decomposition will improve judgmental predictions
2. Decomposition is of greatest value when uncertainty is high

Armstrong, Denniston, and Gordon (1975) randomly split 151 subjects into two groups. One group received some questions directly; the other group received a decomposed version. The groups were asked to make predictions for five problems ranging from areas where uncertainty was low to others where it was high. These questions were asked (paraphrased here):

1. How many families were there in the United States in 1970?
2. How many high school dropouts were there in 1969?
3. How many packs of Polaroid color film were used in the United States in 1970?
4. How many pounds of tobacco were produced in the United States in 1969?
5. In 1972, a Philadelphia radio station asked high school students to submit postcards with the message “Carefree Sugarless Gum.” There was no limit to the number of cards per
The school that submitted the most postcards would have a free rock concert by the Grass Roots band. The contest was heavily promoted. How many cards were submitted?

The decomposed versions contained a series of questions on parts of the problem. These were used by the researchers to compute the overall prediction. For example, the Polaroid questions were:

1. How many people were living in the United States in 1970?
2. What was the average size of a family?
3. What percentage of families owned cameras?
4. Of those families owning cameras, what percentage owned Polaroid cameras?
5. What was the average number of packs of film used per Polaroid camera owner?
6. What percentage of Polaroid film sales were color film?

The decomposed version yielded more accurate forecasts for all questions, and the improvements were much greater for the questions where uncertainty was high, especially for the chewing gum contest. (The answers to the questions on the preceding page can be found in Appendix F.)

ASCHENBRENNER and KASUBEK [1978] provided additional support for decomposition. However, more research is needed. For example, omissions of key elements seem to be difficult to detect [FISCHHOFF, SLOVIC, and LICHTENSTEIN, 1978]. Also causal reasoning would seem helpful in decomposition, yet in BURNS and PEARL [1981] it did not lead to improved accuracy.

A most useful forecasting application has been to consider separately the current status and the change. This split is illustrated in Exhibit 4-3.

Forecasting errors are often caused by failure to know the starting point, that is, the current status. Morgenstern (1963) provided evidence on the difficulties that economists have in measuring current status. Thompson (1961) cited this as a major problem in weather forecasting. PALMORE [1979] showed its importance in predicting health changes for elder citizens. Hedlund et al. (1973) provided evidence about the analogous problem of the base rate in trying to predict which mental patients are dangerous.
Some specific examples are cited to show how serious the problems can be in measuring current status:

A problem with the estimation of the base rate was found in Bakwin's (1945) study of tonsillectomies. A sample of 1000 eleven-year-old children was examined by a group of physicians, and 61% were selected to have their tonsils removed. The remaining 39% were then examined by another group of physicians, and 45% were selected for tonsillectomies. The remaining children were examined by still another group of doctors, and 46% were selected. Notice that the base rate depended on the physicians, not on the children in the sample. (Incidentally, in case you don’t already know this, children should rarely have their tonsils removed. The operation is dangerous, and it often does nothing worthwhile for the child.)

Bell (1962, pp. 152–153) discussed problems in measuring the current crime rate in New York City. The “crime rate” increased drastically when all complaints were phoned into a central switchboard. Before this time, the calls had gone to local precincts, and not all calls had been recorded there. This caused the crime rate to appear lower, which, in turn, made the police chiefs happy.

The U.S. Bureau of the Census (1967) found that the number of dilapidated housing units in 1960 was probably closer to 3.4 million than to the 1.9 million previously reported. This led the
Bureau to change its original conclusion that 1950 to 1960 was a period of progress in housing developments.

Cole (1969) estimated that about 40% of the errors in 1-year forecasts of the GNP are due to errors in measuring the current GNP.

Breaking the problem down into current status and change can alert the researcher to serious difficulties in the measurement of current status. This aspect of the problem is often overlooked.

Different methods might be used for estimating current status than for predicting change. For example, judgment might be used to assess the current status of a person's health, and segmentation methods to forecast the change in this person's health over the next 20 years. Another example, judgment methods might be used to assess current sales of breakfast cereals and extrapolation methods to forecast change.

Other types of decomposition have also been employed with apparent success in forecasting problems. For example, WARSHAW [1980] obtained improved predictions of actual purchase behavior for branded products by asking detailed questions about the purchase situation:

In WARSHAW [1980], sixty housewives were asked about their intentions to purchase various brands of soft drinks over a five-day period. Global intentions were asked: "What is the probability that you will purchase brand X between now and next Monday morning?" Also, a decomposed version was asked of the same subjects with questions about location and about the purchase of multiple brands. The decomposed version (which Warshaw refers to as a "derived intention") was significantly more accurate in predicting actual purchase behavior.

Of particular importance is the breakdown between industry sales and market share. Many organizations follow this practice. (Such a breakdown proved useful in the reanalysis of Hacke's transistor forecast from LRF p. 20.) For economic problems, this breakdown can be used in conjunction with the breakdown between current status and change, yielding four separate parts of the forecasting problem.
It was six men of Indostan
To learning much inclined,
Who went to see the Elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind.
The First approached the Elephant
And, happening to fall
Against his broad and sturdy side,
At once began to bawl:
"God bless me, but the Elephant
Is very like a wall!"
The Second, feeling the tusk,
Cried, "Ho! what have we here
So very round and smooth and sharp?
To me 'tis very clear
This wonder of an Elephant
Is very like a spear!"
The Third approached the animal
And, happening to take
The squirming trunk within his hands,
Thus boldly up he spake:
"I see," quoth he, "the Elephant
Is very like a snake!"
The fourth reached out an eager hand,
And felt about the knee:
"What most the wondrous beast is like
Is very plain," quoth he;
"'Tis clear enough the Elephant
Is very like a tree!"
The Fifth, who chanced to touch the ear,
Said, "E'en the blindest man
Can tell what this resembles most;
Deny the fact who can:
This marvel of an Elephant
Is very like a fan!"
The Sixth no sooner had begun
About the beast to grope
Than, seizing on the swinging tail
That fell within his scope,
"I see," quoth he, "the Elephant
Is very like a rope!"
And so these men of Indostan
    Disputed loud and long,
Each in his own opinion
    Exceeding stiff and strong.
Though each was partly in the right,
    They all were in the wrong!

"The Parable of the Blind Men and the Elephant"
John Godfrey Saxe

Each blind man of Indostan used only one approach. In contrast, eclectic research requires the researcher to utilize a set of very different methods in solving a problem. In other words, the research budget should be split so that a variety of approaches can be used. This is the strategy used by my boyhood friend, Perry Mason (you know, the lawyer).

Eclectic research can be applied in a variety of ways in forecasting. It can be used to measure variables in a forecasting model... or relationships in a forecasting model... or it can be used to assess the validity of a relationship... or the uncertainty involved with a forecast. An illustration of how eclectic research might be used to measure a relationship is provided in Exhibit 4-4.

Using a combination of all five approaches rather than a single approach increases the likelihood of obtaining a valid estimate of the relationship shown in Exhibit 4-4. For a given research budget, it may be better to use a number of different approaches, even though crudely done, than a single approach, done well. This is especially true in situations where measurement is difficult.

An analogy may help to clarify this argument. Assume that a hunter is about to shoot at a bird. Unfortunately for him, he cannot see his target directly. However, he does have some idea as to the general location of the target because he saw the bird go into the bush, he can hear the bird, and he can see some branches moving. Since it is getting late, he decides at this time to try to shoot the bird. The question he now faces is whether to use his rifle or his shotgun.

If he uses the rifle, he is likely to miss the target altogether, and the bird will fly away. If he does hit the bird, it is likely that the shot will kill the bird. If he uses the shotgun, he is more likely to hit the bird with some of the buckshot. In this case, he has a higher likelihood of hitting the bird, although the shot may not kill it. By wounding the bird, however, he will get further information as to its location and can then move in for the kill.
The rifle is analogous to intensive research. When aimed in exactly the right direction, it does the job and does it well. If not aimed correctly, however, it does little good. The shotgun, analogous to eclectic research, is likely to do some good if aimed in the general direction of the target. It may down the bird or, at least, maim the bird and allow for a second shot.

**Importance of Eclectic Research**

Social scientists often refer to the physical sciences for their research methods. However, two major differences affect the transfer of methods from the physical to the social sciences. First, it is generally more difficult to develop the desired controls when running experiments in the social sciences. And, second, there is more uncertainty about measurement in the social sciences. Of course, it is all a matter of degree.

Sometimes the differences are illusory. People often reject experimental findings in the social sciences because the results challenge their current beliefs. On the other hand, we do not often get upset about an experimental finding on the strength of materials because a belief in this area is not an important part of our attitudes. However,
an expert in the strength of materials may get upset and react emotionally. (I'll show you an example soon of how physical scientists can do this.) Furthermore, the presumed accuracy of the physical sciences is often an illusion. Why, we did not even know where the sun was until 1961! (Maybe we still don't).

Macdonald (1972) examined studies by W. J. Youden on the distance from the earth to the sun. Of 15 observations published from 1895 to 1961, each worker's estimated value was outside the certainty limits set by his immediate predecessor.

Macdonald (1972) also had a recommendation for the physical sciences—eclectic research! He suggested that the quantity in question should be measured with a different technique, one that operates on a different principle. This contrasts with the earlier position of Bridgman (1927), a well-known physical scientist who said, "If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations."

Numerous researchers have recommended eclectic research for the social sciences. These include Cronbach and Meehl (1955) and Campbell and Fiske (1959) in psychology; Webb et al. (1973) in the social sciences; and Cook and Selltiz (1964) and Curtis and Jackson (1962) in sociology. These researchers have used different names such as "multiple operationalism," "methodological triangulation," "convergent operationalism," "operational delineation," and "convergent validation." This confusion in terms is understandable because they had not read LRF; the correct term is "eclectic research."

Although many researchers have argued for eclectic research, social scientists still favor intensive research. Let me explain why.

Intensive research calls for the specialist ("one who knows more and more about less and less until eventually he knows everything about nothing"). Typically he has expensive equipment, conducts elaborate experiments, and collects large sample sizes. His team is composed of specialists in the same field. The specialist is the person who understands and can argue for expensive equipment or large-scale studies. He finds it easier to publish his results because journals are organized by narrow technical areas and because editors appear to value the reliability of a finding (the ability to replicate under the same conditions) more highly than the validity of a finding.
Eclectic research calls for the generalist ("one who knows less and less about more and more until eventually he knows nothing about everything"). The generalist has an interdisciplinary staff and spreads his budget out so that he must get by with makeshift equipment, crude experiments, and small sample sizes.

So you can see the difficulties faced by the eclectic researcher. Eclectic research is accepted by most researchers as being good for science ... but it is bad for the scientist. How unfortunate.

Evidence on Eclectic Research

Although evidence can be cited that demonstrates the shortcomings of intensive research (e.g., the example of Youden on the distance to the sun), it is difficult to find research that contrasts eclectic and intensive strategies. The most relevant evidence that I found was the case of Immanuel Velikovsky. His works provide a critical test between eclectic and intensive research for the following reasons:

1. Many predictions were made
2. The predictions from the two strategies differed radically
3. It has been possible to test many of the predictions
4. The case is well documented.

Velikovsky used historical writings (Velikovsky, 1950) and cultural artifacts (Velikovsky, 1955) to develop theories about the history of the earth. These pieces of evidence, such as the Bible, are regarded by most scientists as being of such poor reliability as to be unworthy of use in the development of scientific theories. For example, any given passage in the Bible is subject to many interpretations. Instead of regarding events in the Bible as analogies and parables, Velikovsky assumed that real events were being discussed. This reduced the number of interpretations. More importantly, Velikovsky assumed that the events were so important that they were observed by others on earth. Therefore writings from other cultures, such as Greek mythology, were also taken at face value. Furthermore, because the events were so dramatic, physical evidence should also exist, and so artifacts and geological findings were examined.

These various sources fit into a pattern, according to Velikovsky. He concluded that catastrophic events occurred in the world. These events (e.g., the near collision of Venus and Earth around 1500 B.C.) had such a great effect upon the earth that there was no need for highly reliable instruments to record the events. Everyone on earth was aware
of what was happening. Velikovsky summarized his argument (p. 308 in Velikovsky, 1950; see also the last three paragraphs of Chapter 12 in Velikovsky, 1955, for a similar argument) as follows:

If a phenomenon had been similarly described by many peoples, we might suspect that a tale, originating with one people, had spread around the world, and consequently there is no proof of the authenticity of the event related. But just because one and the same event is embodied in traditions that are very different indeed, its authenticity becomes highly probable, especially if the records of history, ancient charts, sundials, and the physical evidence of natural history testify to the same effect.

Velikovsky's use of extremely different approaches to test his theory did not impress the scientific community. He used "unreliable data" and covered many different scientific disciplines in which he was not a "recognized expert." It was all so outrageous that prominent people from the scientific community mounted an active, and partially successful, campaign to suppress the publication of Velikovsky's theories. The stated reasons for this attack upon Velikovsky were that the method used was not scientific and that he was not an expert in the fields affected by his theories.

Velikovsky made a series of different forecasts based on his theory. Most of these forecasts were labeled as impossible in the light of existing knowledge. Yet evidence from space probes and other sources has confirmed a large number of these predictions, and there has not yet been an incorrect prediction. Much of this evidence is summarized by De Grazia et al. (1966). The forecasts that have been verified by additional evidence have varied greatly in nature. They involve the temperature of Venus, the existence of electromagnetic fields in the solar system, the variations in the length of a day, radio noises from Jupiter, hydrocarbons in the atmosphere of Venus, the age of oil deposits in the Gulf of Mexico, the previous existence of advanced human culture in areas of northeastern Siberia that are not currently inhabited, and the age of the ancient civilization of Mexico.

Multiple Methods and Multiple Hypotheses

Velikovsky's research emphasized the use of multiple and different methods. This strategy will be stressed in Part II of LRF in the discussion of forecasting methods. For an interesting application of eclectic research to forecasting, see the study of offshore nuclear plants by BAKER et al. [1980]. It will also be used in Part III in describing
methods for analyzing forecasting models. A third use of eclectic research is discussed in Part V, where it is suggested that researchers should test multiple hypotheses rather than advocate a single hypothesis. Though it is not terribly popular, research based on multiple hypotheses has provided us with most of the evidence that is summarized in this book.

**SUMMARY**

Subjective inputs by researchers are necessary and important. The proper time to use this information is before the analysis of the data. The researcher should provide an explicit summary of his prior knowledge. These are his hypotheses or theories, and the process is referred to as *a priori* analysis. Although this is a difficult and time-consuming process, it is superior to *a posteriori* analysis, which uses subjective inputs after analyzing the data. A good rule to follow about *a posteriori* analysis is this: Avoid it!

Decomposition offers numerous advantages. It is useful in judgmental forecasting. Decomposition is expected to be especially valuable in structuring forecasting models. The distinction between current status and change is of particular value. Also useful are breakdowns between base rate and individual variation, and between total market and market share. A good rule to follow about decomposition is this: Do it! It can often help, and will seldom hurt.

I suggest eclectic research for situations where uncertainty is high and measurement is difficult. In such cases, one should use approaches that are designed to measure a given concept but do so in different ways.