CHAPTER 7

SHORTAGE IN RELATION TO SYSTEM INADEQUACY: AN A PRIORI MODEL AND THE EMPIRICAL RESULTS

Thus far, in discussing the various components of our index of system inadequacy and in introducing our methods of measuring shortage, we have avoided any but qualitative statements about the relation between the demand-supply balance and the level of shortage suffered under a particular climatic event. We must now, however, proceed with the task of attempting to quantify this relation. It is appropriate to begin by reviewing briefly some of the ground we have covered.

In Chapter 5 we defined the percentage shortage suffered by town i in year t as:

$$S_{it} = \frac{D_{it} - V_{it}}{D_{it}} \times 100 \tag{7-1}$$

where D_{it} = projected demand (annual total) in town i for year t; and V_{it} = the amount of water (also annual total) available from the system of town i in year t without emergency augmentation.

If we divide through on the right of Equation 7-1 in both numerator and denominator by Y_{it}, the safe yield of town i's system in year t, we obtain:

$$S_{it} = \frac{D_{it}/Y_{it} - V_{it}/Y_{it}}{D_{it}/Y_{it}} \times 100 \tag{7-2}$$

which we may write for convenience as:

$$S_{it} = \frac{\alpha_{it} - \alpha^*_{it}}{\alpha_{it}} \times 100 \tag{7-3}$$

where, obviously, $\alpha \equiv D/Y$ and $\alpha^* \equiv V/Y$. This expression emphasizes that there are two determinants of the size of shortage: the chosen level of system inadequacy (where we concentrate on choices of safe yield relative to given levels of projected demand); and the percentage of the safe-yield flow available from the normal system (without emergency augmentation). It is the second determinant which reflects climatic variation. We would, for example, expect that if town i experienced in year t an event worse than the safe-yield event (the design drought), the ratio α^*_t would be less than 1. Clearly, the definition of safe yield implies that in a repetition of the design drought, α^*_t would equal 1. On the other hand, any stress less severe than the design drought, from a slightly less severe drought to the largest imaginable annual rainfall, will result in some α^*_t more than 1. For a particular α^*, shortages will be greater, the greater the relative inadequacy of the system.

Now, it is our aim to construct a model of the impact of drought which can be applied to planning for future supply increments. We need, then, in essence, a probability distribution of α^*_t events. But direct evidence on this point is lacking, so we are forced to turn to other measures of climatic variation. Specifically, we wish to use available information from the record of some climatic variable to develop a surrogate distribution of α^*_t events. Hence our concern in the last chapter with various indicators of climatic variation.[1]

It is clear that the "actual" distribution of α^*_t events would reflect the nature of the particular watershed being looked at, the intrayear and over-year storage provided by the system, and the distribution of precipitation events, not only on an annual but also on a seasonal basis. We have already mentioned the implications of ignoring, as we do, the intrayear variations in precipitation. We are, of course, also abstracting from differences in watershed types, for we are attempting to construct a single, relatively simple model of drought impact applicable across a climatic region. As a final simplification, we ignore storage effects.

This may seem too drastic a bit of model-building sleight of hand, but several comments may help to put it in perspective. First, because we are not dealing with seasonal rainfall variations, overseason storage would not, in any case, be of concern. Second, as the genesis of this study demonstrates, "drought" becomes a phenomenon of general interest when several dry years follow consecutively. A single dry year, or the first of

[1] We emphasize that our work is directly applicable only to surface supply systems. While the same principles probably apply to groundwater systems, the practical matter of estimating the frequency of occurrence of various α^*_t events, the fraction of the maximum dependable draft available in year t, seems extremely difficult.

several dry years, creates the situation which overyear storage is designed to handle. But it seems reasonable to suppose that for most systems the cushion of overyear storage will be eliminated in the first year of a drought period. In subsequent years, the system will be more directly dependent on streamflow levels. The bias introduced by ignoring storage effects will be in the direction of overstating the expected shortages and losses over the long run. This will be so because some fraction of the α^*_t events giving rise in the model to shortages will, in fact, represent isolated dry years in which no shortages (or only very small shortages) need occur as overyear storage is drawn down.[2]

In the absence of storage effects, α^*_t will be approximated by the ratio of the year t streamflow to the streamflow associated with the safe-yield event, for the stream serving as the source of the system's supply. We may think of Equation 7-3, with streamflows used to predict α^*_t, as the most satisfactory version, a priori, of our model of drought impact. Unfortunately, however, there are not sufficient streamflow data available to permit us to test this model using the appropriate record for each system in estimating α^*_{it}. Because the characters of the streams used for water supply vary so much across the state, it seemed particularly dangerous to choose one or two long records as the basis of a streamflow variable to be applied to every system. Accordingly, the model described by Equation 7-3 was actually tested with variously transformed versions of our rainfall series,[3] based either on the individual sites or on the pooled record. These attempts to show that we could explain the observed shortages on the basis of Equation 7-3 were uniformly unsuccessful. In particular, significant shortages were observed in several cities for which the model predicted no shortage; and relatively small shortages (on the order of 20 to 30 percent) were observed in cities for which the model predicted very large ones (on the order of 50 percent).[4]

Although we cannot show that our a priori model is an accurate guide to the world, we can suggest two considerations which tend to explain the significant departures from it in the data. First, while it is true that if the available amount of water for delivery is smaller than that demanded,

[2] This bias originally seemed particularly harmless because our work with the implications of the empirical results, presented below, indicated that drought losses were a very much less serious problem than generally believed. The a priori model developed here, however, casts these losses in a far more serious light and suggests that a more intensive research effort, one including storage effects and intrayear precipitation patterns, would be of benefit in more accurately diagnosing the degree of danger.

[3] One of these versions was based on our attempts to relate rainfall and streamflows and thus was intended to be a good surrogate for streamflow. This version did not perform significantly better than any of the others tested.

[4] See Appendix B for a description of the data and sample used.

shortage (by our definition) *must* occur, it is not true that if the amount of water is adequate, shortage must *not* occur. Interposed between the supply system and the consumer is the judgment of the system manager. Thus, in the midst of worsening drought the system manager may act to conserve water—perhaps, intially, by refusing to use his overyear storage cushion, later, by cutting back demands he could satisfy from current watershed yields—and, by so acting, create shortages where the model would predict none. There is no check except local political pressure on such a conservative policy, on forcing a famine in the midst of plenty. And, of course, except in retrospect, one can never be at all sure that there is "plenty." The system manager who tried, through demand restriction, to build an overyear storage cushion between 1965 and 1966, would have been a hero in the summer of 1966. Trying to repeat his coup in 1966, he would have wound up a scapegoat in those areas for which the drought ended in late 1966.

Second, a complementary explanation of the failure of our first model is the possibility that most, if not all, municipal supply systems may have significant safety factors built into their estimates of safe yield. For example, standard engineering texts recommend that water systems be designed with a "25 percent reserve for the drought that occurs once every 20 years."[5] Another source of a safe-yield cushion may be the bank storage capability of the reservoir system. In the proper soils such storage may be large, and as reservoir levels drop, the system would tap more and more of it. The rate of inflow of stored water would increase with the fall in the surface of the reservoir.[6]

If we tentatively accept the argument that there are, for one reason or another, safe-yield reserves built into most systems, why do *any* systems suffer as large shortages as those predicted by the model? We note that if such reserves are the result of the factors we have discussed, their purpose would be compromised if town officials were made aware of them. We may guess that some towns become aware of them of political necessity. If, for example, a town with a relatively inadequate system is faced with what appears to be a steadily worsening drought, it will be apparent that very large shortages might be in store. (For example, if $\alpha_{it} = 2$ and $\alpha^*_t = 1$,

[5] Gordon M. Fair, John C. Geyer, and Daniel A. Okun, *Water and Waste Water Engineering* (New York: Wiley, 1966), I, Chap. 8, p. 6.

[6] The motives of consulting engineers, conscious or unconscious, probably work in the direction of inclusion of cushions in safe-yield estimates. First, particularly given the state of public and managerial attitudes towards water shortage, the reputations of firms and specific engineers might be endangered by the occurrence of shortage in a client system which had built the recommended improvements. Second, consulting fees are tied to the size of the project undertaken; such projects might be significantly smaller if, for example, bank storage were taken into account in determining safe yield.

then the potential shortage is 50 percent. We observed no shortages larger than 30 percent in any town in any year of the drought.) It may, indeed, seem that there is a limit of political feasibility to the stringency of restrictive measures which may be enacted and to the emergency purchases which can be funded. The response to this situation may often be for the officials, after some point, to shut their eyes and keep pumping, hoping that the weather will break. Towns with lower potential shortages may, on the other hand, feel the necessary measures feasible. If this argument is accurate, the larger the potential shortage, the more likely the town would be to use some of its safe-yield reserve. Hence our model would tend to overestimate the shortages to be observed in relatively inadequate systems.

EMPIRICAL RELATIONS AMONG SHORTAGES, INADEQUACIES, AND CLIMATIC VARIATIONS

Having attempted to explain why our a priori model does not fit the available data well, we now seek some relation which does. We intend in subsequent chapters to follow-up the implications for the expected costs of system inadequacy of both the a priori model and the relation fitted to the actual data. At this point we include a scatter diagram of shortage against system adequacy for 1964 for 15 towns (Figure 13). On this diagram we show the function relating shortage to adequacy under 1964 conditions as implied by the a priori model.

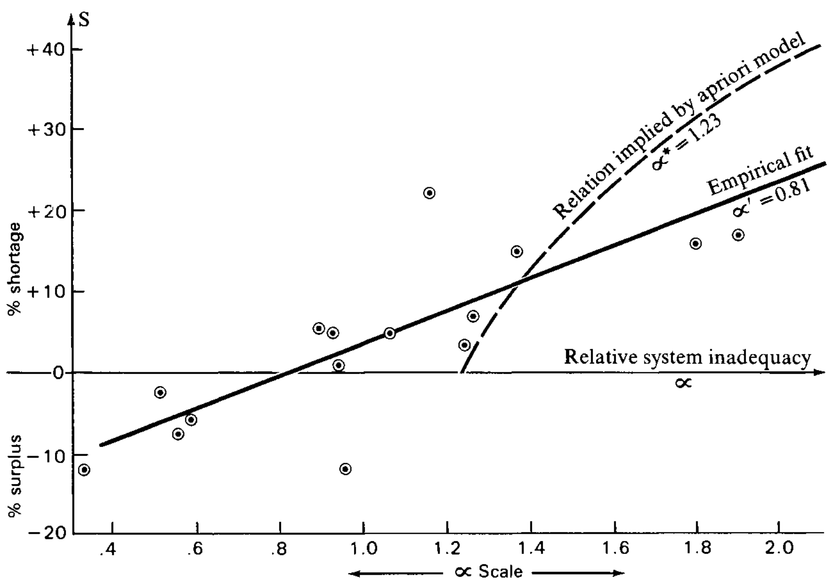

Figure 13. Scatter for 1964: 15 observations of shortage and system inadequacy.

We may, however, experiment by fitting to the data the regression:

$$S_{it} = k_t + \beta(\alpha_{it}) + \epsilon_{it} \tag{7-4}$$

for each year (so that climatic conditions will be invariant for regression). We hypothesize that k_t will be different for each year, reflecting climatic variation, but that β will be the same for every year. The results of this experiment indicate that for 1964–66, β is approximately equal to 20, and that k_t varies in the proper direction to be reflecting the worsening of the drought.

We next consider an equation of the form:

$$S_{it} = \zeta + \beta(\alpha_{it} - \alpha_t') \tag{7-5}$$

where α_t' represents the adequacy ratio at which shortages would be zero in the empirical formulation of Equation 7-4, that is, the α-axis intercept. We use the cumulated rainfall deviation information discussed in the last section to obtain estimates of α_t' for use in the regressions.[7] Specifically, we estimate α_t' from the relation:

$$\alpha_t' = \frac{\Delta_{(o)}}{\Delta_{(t)}} = \frac{\text{cumulative rainfall deviation (1908–11)}}{\text{cumulative rainfall deviation } (t - 3 \text{ to } t)} \tag{7-6}$$

The null hypotheses are:

$$\zeta = 0 \text{ and } \beta = 20.$$

The results of this analysis are summarized in Table 15. The following observations are relevant to the propositions being tested:

1. Based on r^2's (mean for the 4 years = 0.60) and on the F-ratio tests (all highly significant, well over the requirements for significance at the 1 percent level), we may conclude that a simple linear relation of the form we have postulated is a good description of the empirical connection between degree of shortage and the existing D/Y ratio for a town experiencing a given climatic event.

2. We note that the estimates of β ($\hat{\beta}$) are quite close for the years 1964–66 and that the null hypothesis that $\beta = 20$ cannot be rejected in those years. The estimate for 1963 is, on the other hand, significantly different from 20. The mean $\hat{\beta}$ for the years 1964–66 is 18.78, and the value of $\hat{\beta}$ from a pooled regression for 1964–66 is 19.37.[8] It is probably the

[8] For a discussion of pooled cross-section regressions, see E. Kuh, *Capital Stock Growth: A Microeconometric Approach* (Amsterdam: North-Holland, 1963), Chs. 5 and 6.

[7] A confusion developed because, by looking originally only at the role of α_t' when shortages were zero, we came to identify it with the fraction of safe-yield flow available, or α_t^* in the a priori model. If this were true, then β would have to equal $1/\alpha_{it}$ and could not be constant.

TABLE 15. RESULTS OF TEST OF EMPIRICAL MODEL HYPOTHESES

Regression Equation:

$S_i = \zeta + \beta(\alpha_{it} - \alpha_t') + \epsilon_i$ $\quad i = 1, \ldots, 15$

For each of the years (t), 1963–66.

Null Hypotheses:

$\zeta = 0 \qquad \beta = 20$

1963 (Let $[\alpha - \alpha'] = x$) $\qquad \alpha'_{63} = 2.77$

$S = 23.66 + 13.00\,(x)$
(5.25) (2.98)

F-test sig. at 1 percent; $r^2 = 0.59$

t-tests ζ—significantly different from 0 at 1 percent
β—significantly different from 20 at 5 percent

1964 $\qquad \alpha'_{64} = 0.81$

$S = -0.0192 + 18.79\,(x)$
(2.01) (4.13)

F-test sig. at 1 percent; $r^2 = 0.61$

t-tests ζ—*not* significantly different from 0 at 90 percent
β—*not* significantly different from 20 at 70 percent

1965 $\qquad \alpha'_{65} = 0.76$

$S = 3.895 + 17.88\,(x)$
(2.24) (4.07)

F-test sig. at 1 percent; $r^2 = 0.60$

t-tests ζ—*not* significantly different from 0 at 20 percent
β—*not* significantly different from 20 at 70 percent

1966 $\qquad \alpha'_{66} = 0.70$

$S = 3.594 + 19.68\,(x)$
(2.73) (4.39)

F-test sig. at 1 percent; $r^2 = 0.61$

t-tests ζ—*not* significantly different from 0 at 30 percent
β—*not* significantly different from 20 at 90 percent

Pooled—1964–66

$S = 2.33 + 19.37\,(x)$
(1.31) (2.36)

F-test sig. at 1 percent; $r^2 = 0.61$

t-tests ζ—*not* significantly different from 0 at 5 percent
β—*not* significantly different from 20 at 70 percent

previously mentioned opportunities for reservoir drawdown in the first of a series of dry years which explain the 1963 slope estimate. The lower β for 1963 than for later years indicates that shortages increased less rapidly in that year with increases in system inadequacy.

3. The null hypothesis that $\zeta = 0$ must be rejected for the 1963 regression. For 1964 it may be accepted with a high degree of confidence. For 1965 and 1966 we are only able to say that it cannot be rejected with any assurance.

In summary, our empirical model seems to be fundamentally valid for the *later* years of a series of dry years. This limitation does not seem too great a handicap for the reasons already discussed. We must, however, note that there are real dangers in extrapolating purely empirical relations into the future.[9]

[9] It may have occurred to the reader to question these regression results because of the underlying link between both the dependent and independent variable and the level of projected demand. Thus, expressing S_{it} in terms of its components, we may write Equation 7-7 as:

$$\frac{D_{it} - V_{it}}{D_{it}} = \zeta + \beta(\alpha_{it} - \alpha'_t) + \epsilon_i$$

Both sides of this equation vary directly with the size of D_{it} for given values of the other variables. There is, then, certainly at least initial reason to suspect the presence of a spurious correlation bias.

To test for the presence of such spurious correlation, we calculated correlation coefficients between D_{it} and S_{it} and between D_{it} and $(\alpha_{it} - \alpha'_t)$. These were insignificant even at the 50 percent confidence level for every year between 1963 and 1966. In addition, we calculated the partial correlation coefficient of S and $(\alpha_{it} - \alpha'_t)$ (netting out the influence of D explicitly), and found that it is virtually equal in each year to the simple correlation coefficient for S and $(\alpha_{it} - \alpha'_t)$.

The evidence thus indicates that in fact the correlation between S_{it} and $(\alpha - \alpha'_t)$ attributable to their common link with D_{it} is negligible.