Vol XXXV
No. 2

APRIL-
JUNE
1980

ISSN 0019-5014

INDIAN JOURNAL OF AGRICULTURAL ECONOMICS

INDIAN SOCIETY OF
AGRICULTURAL ECONOMICS,
BOMBAY

MEASURING AGRICULTURAL GROWTH

N. Krishnaji*

Growth rates are usually computed by fitting functions of time to time-series data on variables such as agricultural output. Sometimes a best-fitting trend is chosen from a pre-specified set of functions of time so as to answer the question whether deceleration has or has not taken place over a given period. A somewhat rarer but procedurally similar practice is to estimate summary measures of fluctuations around the trend to see if fluctuations in one period have been wider than in another.

This paper argues that these procedures have no theoretical basis, either economic or statistical, and that consequently inferences on patterns of growth or magnitude of fluctuations drawn from fitted trends are not valid. Much of the controversy about growth and fluctuations in output is rooted in the lack of conceptual clarity about the underlying terms: trend and fluctuations.

I

THE CONCEPTUAL PROBLEM

If our concern is about changes in actual output there will be no room for controversy: given the index numbers we can compute an unique rate of increase (or decrease) in the actual output from one year to another. But we know that variations in weather induce—to an unknown extent—fluctuations in output and hence a rate of change in actual output contains a weather-induced component which can exaggerate the rate for certain choices of the terminal years. So what is usually sought to be measured is the change in not actual output but a notional output which, other things remaining the same, corresponds to 'normal' weather conditions. No ready-made and unique answer can be given to the question how normal weather is to be defined and measured. In respect of a single weather variable such as rainfall, normal rainfall is usually defined as the mean of a distribution; in respect of temperature not only the mean but also the minimum and maximum during a time period are taken into account. There is no difficulty in generalising this concept and regarding normal weather as a characteristic of the joint distribution of the relevant weather variables. The underlying measurement problems are no doubt difficult to solve but for our purpose here it is sufficient if we assume that deviations from normal weather—so conceptualised—cause deviations in the level of output realisable for given levels of input use. This leads to the decomposition:

$$Y_t = Y_t^* + U_t \qquad \ldots.(1)$$

* Centre for Studies in Social Sciences, Calcutta. On leave from the Centre for Development Studies, Trivandrum. The author has benefited a great deal from many discussions on the subject with Dr. A. Vaidyanathan and from comments made on an earlier version of the paper by participants in the Lonavla Seminar. Chandan Mukherjee carried out the simulations reported in section II.

where Y_t, Y_t^* and U_t stand for the actual output, the notional output corresponding to normal weather, and the contribution of weather variables respectively in the year t. The weather-induced component can be positive or negative depending on the weather conditions in a given year. The problem then is to separate U_t from the actually realised output Y_t so that the characteristics in the movement of Y_t^* can be studied and questions concerning the pattern of growth answered unambiguously.

We shall argue that trend-fitting does not yield a satisfactory solution to the problem. As a prelude to the argument let us begin with an attempt to conceptualise 'trend' and 'fluctuations' within the framework implied in (1). If by fluctuations we mean only weather-induced fluctuations they then correspond to U_t in (1); but in this case it is not meaningful to talk about a 'trend' in output as a smooth function of time. The level of output that would be realised under normal weather conditions, Y_t^*, depends on the extent of input use, and the technical and social conditions of production; the extent of input use itself depends on the economic environment. Thus a fall in the notional output resulting from, say, changes in the price regime is not inconceivable. There is no reason then, why, after the removal of the effects of weather, output levels should fall along a smooth curve. In other words, while 'growth' (in some vague sense) in Y_t^* may be present, 'fluctuations' arising out of changes in the economic environment may also be present. What we then have is a series (Y_1^*, Y_2^* .... Y_T^*) with which neither an unique growth rate (except as an average) nor a mathematical trend (except as an approximation) can be associated.

It is clear now that fitting a function of time of a given functional form f (t) (such as the linear) to the data Y_t is equivalent to the formulation:

$$Y_t = f(t) + V_t + U_t \qquad \ldots.(2)$$

where U_t is the same as in (1) and V_t represents the error in approximating Y_t^* by the given function f(t). The errors in approximation may be small or big depending on the (fortuitous) closeness of Y_t^* to a curve of the given functional form and nothing can be said about their distribution *a priori.*

In contrast to (2), conventional trend-fitting is based on the model

$$Y_t = f(t) + W_t \qquad \ldots.(3)$$

where f(t) is, as before, a specified function of time (with unknown parameters), and W_t are assumed to be independently and identically distributed with mean zero and a constant variance. Users of this model compute growth rates on the basis of the estimated parameters of f(t) and identify the magnitude of fluctuations with the estimated variance of W_t, *i.e.*, the estimated residual variance. From (1), (2) and (3) it is obvious that this procedure of introducing a smooth trend (where none need be present) in Y_t^* leads to a confounding between the weather-induced fluctuations and the errors in approximation (discussed above) which depend not only on the notional Y_t^* but also on the specified functional form. In other words, deviations from a given trend ($W_t = U_t + V_t$) cannot be equated to weather-induced fluctuations (U_t) except in the unlikely case $V_t = 0$ (*i.e.*, when notional output describes exactly the prescribed trend).

Is the probem solved by fitting not one but several specified functions and choosing the one that fits best? No; on the contrary, this is the most efficient way of sweeping under the carpet the weather-induced fluctuations and minimizing their importance. The following graphical example sharply illustrates the point.

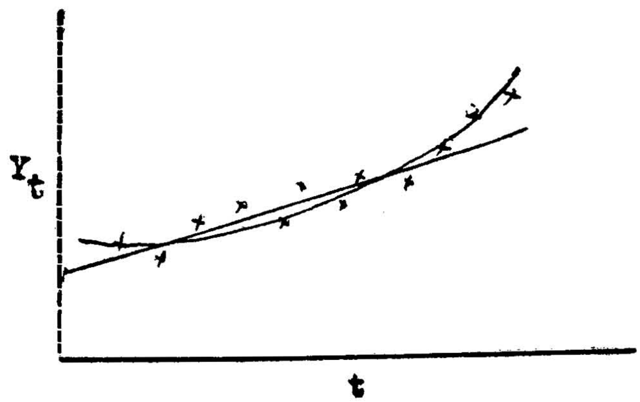

Figure 1

In Figure 1 it is assumed that notional output is describable by a linear trend. In all but the last two years weather conditions are nearly normal so that actual output falls close to the 'true trend'. The last two years are 'good' so that output levels far higher than the corresponding notional levels are realised. In such a case a quadratic function of time (exhibiting acceleration when none is actually present) may fit the data better than does the 'true' linear form. Choosing the best-fitting form can thus misrepresent the pattern of growth; but what is more serious is that the magnitude of weather-induced fluctuations can be grossly under-estimated.

What if we know that the last two years were 'good'? Such prior knowledge is not very helpful for two reasons: (*a*) unlike in the hypothetical case the behaviour of Y_t^* is not known, and (*b*) we have very little knowledge on how precisely weather conditions influence the level of output.

Growth rates computed from one year to another are not accepted since the terminal years could be 'abnormal'. The example discussed above shows that growth rates based on trend-fitting can suffer from the same defect as do the point-to-point rates of change. But the illustration suggests another major, but little known, defect of mechanically choosing the best-fitting regression form for computing growth rates and drawing inferences on the pattern of growth. This is discussed in the next section.

II

CHOOSING THE BEST-FITTING TREND

It is not surprising that the mechanical use of model (3), which is inappropriate, leads to ambiguities and controversies. The seriousness of the problem has already been demonstrated through statistical exercises based on agricultural production data for the Indian economy. These exercises show in particular that trend-fitting does not lead to unambiguous inferences on the behaviour of rates of growth (see, for example, Dey,[1] and Reddy[2]). This is essentially because the same data can usually be approximated equally well by trends exhibiting, say, both constant and decelerating rates of growth. Elements of this difficulty lead to the Vaidyanathan-Srinivasan type of controversy.[3]

A common procedure for resolving this problem is to choose the form of regression that gives the highest value of R^2 from among a set of pre-specified trend functions.

How reliable is this procedure ? In other words, what is the guarantee that the form yielding the highest value of R^2 and the true form coincide? We must remember that this question arises only in the case when there is actually a smooth trend which describes the behaviour of Y_t^*. Let us assume this to be the case and try to answer the question.

We shall show that R^2 is not a very reliable guide for choosing the correct form. This has nothing to do with the agricultural production data for the Indian economy; it is a feature of R^2 in a more general theoretical setting.

For this purpose we have conducted a set of limited simulation experiments. These consist in deriving data under an assumed form and seeing how often R^2 discriminates in favour of the true form among a set of pre-specified forms of trend.

The set consists of

$$Y_t = a + bt + E_t \qquad \ldots.(4.1)$$

$$Y_t = a + b \log t + E_t \qquad \ldots.(4.2)$$

$$\log Y_t = a + bt + E_t \qquad \ldots.(4.3)$$

$$\text{and } \log Y_t = a + b \log t + E_t \qquad \ldots.(4.4)$$

where in each case, E_t are independently and identically distributed normal variables with zero mean and constant specified variance.

There are two sets of experiments. The first experiment—(A)—consists in deriving the data from a linear trend [*i.e.*, (4.1)] and seeing how often R^2 discriminates in favour of the true trend when all the four forms of trend are fitted. More precisely, we have estimated

$$\text{Prob}\,[\,R_1^2 > R_j^2,\ j = 2, 3, 4 \mid (4.1) \text{ is true}] \qquad \ldots.(5)$$

where R^2_i stands for the R^2 statistic in respect of the ith form of trend in the order listed in (4.1) to (4.4).

1. Amal Krishna Dey, "Rates of Growth of Agriculture and Industry", *Economic and Political Weekly*, Vol. X, Nos. 25 and 26, June 21 and 28, 1975.
2. V. N. Reddy, "Growth Rates", *Economic and Political Weekly*, Vol. XIII, No. 19, May 13, 1978.
3. T. N. Srinivasan, "Constraints on Growth and Policy Options: A Comment", *Economic and Political Weekly*, Vol. XII, No. 48, November 26, 1977; A. Vaidyanathan, "Constraints on Growth and Policy Options", *Economic and Political Weekly*, Vol. XII, No. 38, September 17, 1977; and "Constraints on Growth and Policy Options: Reply", Vol. XII, No. 51, December 17, 1977.

In the second set of experiments the data are derived from (4.3), *i.e.*, an exponential (constant growth) trend to estimate the probability of R^2 discriminating in favour of the true trend (4.3), *i.e.*,

$$\text{Prob}\,[R^2_3 > R^2_j,\ j = 1, 2, 4 \mid (4.3) \text{ is true}] \quad \ldots.(6)$$

The probabilities (5) and (6) are estimated for various configurations of data with growth rates ranging from 2.75 to 3.25 per cent. In each case the variance of E_t is specified in such a way that E (R^2) ranges between 0.75 to 0.9. This is done in the following way: For $Y = a + bt + E$, with Var (E) $= \sigma^2$, it is easy to show that

$$E(R^2) = b^2V^2 / V(b^2V + \sigma^2) = b^2/(b^2 + M) \quad \ldots.(7)$$

where $V = \text{Var}(t)$ and $M = \sigma^2/V$.

For a given b and Var (t) which depends on the sample size, M and hence σ^2 can be chosen such that the desired value of $E(R^2)$ is realised. Actual values of R^2 are expected to vary around the latter.

For each configuration of parameters 50 regressions each of forms (4.1) to (4.4) are estimated with the sample size n=10 and also n=15. The data are generated from the true model independently for each regression. The probabilities (5) and (6) are estimated on the basis of these 50 regressions for each data set.

The parametric specifications are given below:

(A) True regression: $Y_t = a + bt + E_t$,

$a = 100$

$b = 2.75, 3.00$ and 3.25

$M = \text{Var}(E)\ \text{Var}\ (t) = 1.0, 1.5, 2.0$ and 2.5

Sample size, $n = 10, 15$

TABLE I—VALUES OF $E(R^2)$

b	M =	1·0	1·5	2·0	2·5
2·75		0·88	0·83	0·79	0·75
3·00		0·90	0·86	0·82	0·78
3·25		0·91	0·88	0·84	0·81

(B) True regression: $\log Y_t = a + bt + E_t$

$a = \log 100$

$b = 0.0275, 0.0300$ and 0.0325

$M = 0.00010, 0.00015, 0.00020$ and 0.00025

$n = 10, 15$

$E(R^2)$ will in this case also be given by the entries of Table I. For example, for $b = 0.0275$ and $M = 0.00010$, $E(R^2) = 0.88$ as given at the top left of Table I. This follows from equation (7).

The results of the simulations are presented in Tables II and III. More research needs to be done, especially on the precise values of R^2 at which it becomes a reliable guide to the true trend. Some incomplete computations (not reported here) show that when R^2 approximates the order of 0.99, it is quite reliable from this point of view. The results in Tables II and III show conclusively, however, that when R^2 values range between 0.75 and 0.9 the probability of a wrong form yielding a higher value of R^2 than that of the true trend is quite high. We may note that in our computations it is only (4.1) and (4.3), *i.e.*, the linear and the exponential trends which compete with each other in respect of higher R^2 values: it is only rarely that the other two forms, (4.2) and (4.4), yield values of R^2 higher than those of the true trends, the linear in case (A) and the exponential in case (B).

TABLE II—ESTIMATES OF PROBABILITY OF R^2 DISCRIMINATING IN FAVOUR OF A TRUE LINEAR TREND (CASE A)

	b	M: 1·0	1·5	2·0	2·5
n = 10	2·75	0·46	0·40	0·44	0·38
	3·00	0·44	0·38	0·36	0·36
	3·25	0·54	0·40	0·44	0·42
n = 15	2·75	0·60	0·42	0·46	0·48
	3·00	0·48	0·58	0·56	0·44
	3·25	0·58	0·52	0·36	0·52

TABLE III—ESTIMATES OF PROBABILITY OF R^2 DISCRIMINATING IN FAVOUR OF A TRUE EXPONENTIAL TREND (CASE B)

	b	M: 0·00010	0·00015	0·00020	0·00025
n = 10	0·0275	0·54	0·46	0·50	0·56
	0·0300	0·50	0·54	0·60	0·52
	0·0325	0·62	0·62	0·50	0·64
n = 15	0·0275	0·60	0·58	0·56	0·56
	0·0300	0·76	0·62	0·62	0·54
	0·0325	0·62	0·64	0·70	0·58

Note:— Each of the entries in Tables II and III are estimated on the basis of 50 independent replications of the relevant model.

The linear and the exponential forms are the most commonly used trend functions. There are no prior grounds for believing that when trends do exist (in the sense that Y_t^* is describable by a smooth function of time) they have to be of these forms. Some workers have, on the other hand, used the Gompertz function as an alternative.[4] Apart from (4.2) and (4.4) which are never used, one can think of forms such as $Y_t = (a + bt)^{-1}$ or log $Y_t = (a + bt)^{-1}$, which can be shown to yield good fits. If we consider all these different trend forms as possible candidates, it is clear from our simulations that the probability of picking up the correct trend by looking at R^2 values would be even smaller than the values presented in Tables II and III.

4. See Dey, and Reddy, *op. cit.*

In the light of the behaviour of R^2 let us consider some actual data. The data are the indices of agricultural production (all-India) for the period 1955-56 to 1964-65 (with 1950-51 = 100). These are 122.2, 130.0, 121.2, 139.6, 136.3, 148.7, 151.5, 146.0, 149.7 and 166.7 for these years in succession.[5] With Y_t standing for the index in time t, the following results are obtained:

$$\text{(A)}\quad Y_t = 121.81 + 4.31\, t, \qquad R^2 = 0.8401$$

$$\text{(B)}\quad \log Y_t = 4.81 + 0.0306\, t, \qquad R^2 = 0.8392$$

It is easy to verify that (A) implies a decelerating rate of growth while (B) implies a constant rate of growth. In terms of approximation there is very little to choose between the two.

Growth rates computed from (A) would decline from 3.54 to 2.68 per cent with an average of roughly 3.11 per cent. On the other hand, (B) yields a constant rate of growth of 3.06 per cent. In the light of our discussion concerning the unreliability of R^2, if we are willing to ignore questions about deceleration (since they cannot be answered by this method), we may then be tempted to say that the growth rate is roughly 3.1 per cent. However, this growth rate does not refer to the output series purged of the weather-induced component and hence has to be interpreted with caution as no more than a poorly-defined summary measure: for example, one cannot claim on this basis that output will increase by 3.1 per cent provided normal weather prevails (since the procedure does not remove the effects of weather variables).

III

ANALYSING FLUCTUATIONS

While it may appear reasonable to assume that the contribution of weather variables, *i.e.*, the residuals U_t in (1), are independently and identically distributed normal variables, such is not the case with $W_t = U_t + V_t$ since, as noted earlier, V_t depends on the closeness of Y_t^* to the artificially introduced function of time. However, there are valid reasons to doubt the independence assumption even in respect of U_t: the presence of long period cycles in rainfall may induce serial correlation in U_t which in turn may give rise to short period 'trends' in output attributable to rainfall alone.[6] Apart from this, changes in the proportion of rainfed crops, which are susceptible to wide fluctuations, may invalidate the assumption that U_t are identically distributed over time. Commenting upon Sen's finding that fluctuations in foodgrain output have widened during the era of planned development,[7] Raj[8] says, "...... it is clear enough from recent experience that fluctua-

5. Source of data: Basic Statistics Relating to the Indian Economy, Statistics and Surveys Division, Planning Commission, Government of India, December 1969.
6. This argument (communicated privately) is due to A. Vaidyanathan; needless to say, it requires investigation.
7. S. R. Sen, "Growth and Instability in Indian Agriculture", Address delivered at the Twentieth Annual Conference of the Indian Society of Agricultural Statistics, January 10-12, 1967.
8. K. N. Raj: Planning and Prices in India, Bangalore University, Bangalore, 1974.

tions of a fairly large order and possibly of greater frequency must now be allowed for than might be considered on the basis of data relating to the earlier decades of this century. This is because not only has cultivation got extended to areas less well-endowed with the supply of water but the high-yielding varieties are generally more demanding in respect of water requirements." These are hypotheses which need to be tested rigorously but they give sufficient scope to question the assumption that weather-induced fluctuations are independently and identically distributed over time. Needless to say, there is hardly any justification for a similar assumption in respect of W_t.

Yet this is what is assumed by those who use model (3) and with the additional assumption of normality, inferences are drawn on the growth pattern (deceleration, etc.). Since the distribution of W_t depends not only on Y_t^* but also on the arbitrarily specified function f(t), it is obvious that such inferences are not valid.

For the same reason Var (W_t) does not measure the characteristic of the distribution of fluctuations caused by weather and hence cannot be used for calculating the probability of occurrence of 'bad' or 'good' years. For one such calculation which estimated that years as bad or worse than 1965-66 and 1966-67 will occur with a probability of roughly 1 in 200, we may refer to Minhas and Srinivasan.[9] Our arguments show that this estimate is not valid. We now give a numerical example based on the Indian data discussed in the last section (for the period 1955-56 to 1964-65) to show that such estimates can be grossly misleading.

It will be seen that in 1957-58 the index number of production fell to 121.2 from 130.0 in the previous year. If we assume that 1957-58 was a 'bad' year and do precisely the same calculations which Minhas and Srinivasan did for 1965-66 and 1966-67, we get the following results:

Omitting the bad year (1957-58) results in the linear trend:

$$\text{(C)}\quad Y_t = 120.49 + 3.9669\, t, \qquad R^2 = 0.8635$$

The residual standard deviation is 5.18 and the 'trend value' for 1957-58 is 132.39. Thus the actual value for 1957-58 (121.2) is 11.19 index points below the trend value. Assuming normality of residual it is easy to estimate that the probability of occurrence of such a bad (or worse) year as 1957-58 is roughly 1/65 (*i.e.*, such bad years are expected to occur only once in 65 years). These calculations are based on the period 1955-56 to 1964-65; in the decade which followed, years as bad as or worse than 1957-58 occurred three times (1965-66, 1966-67 and 1972-73).

The error, as already noted, lies in identifying the deviations from a specified trend with fluctuations arising out of variations in rainfall, etc.[10]

9. B. S. Minhas and T. N. Srinivasan, "Food Production Trends and Buffer Stock Policy", *The Statesman*, November 14, 1968.

10. Those who fit trends believe, however, that the procedure is scientific. Commenting on Ashok Mitra's remark that, "there are ways and ways of interpreting the trend of foodgrain production in the country in more recent years" and that "several different versions can emerge, depending upon the subjective biases at work" ("Bumper Harvest Has Created Some Dangerous Illusions", *The Statesman*, October 14-15, 1968), Minhas and Srinivasan, *op. cit.* say: "This clever remark will certainly get high marks as a debating point. But no respectable statistician will concede that trend-fitting is as arbitrary as Dr. Mitra suggests."

IV

CONCLUDING REMARKS

From the common sense—and statistical—point of view the failure of trend-fitting as a satisfactory tool of analysis arises from the fact that it ignores the sources of variation in the underlying variables. What thus passes off as a scientific approach is nothing but the pure empiricism of goodness of fit. It is worthwhile quoting Feller[11] on the subject:

> "An unbelievably huge literature tried to establish a transcendental 'law of logistic growth'; measured in appropriate units, practically all growth processes were supposed to be represented by a function of the [logistic] form .... Lengthy tables, complete with chi-square tests, supported this thesis for human populations, for bacterial colonies, development of railroads, etc.... Population theory relied on logistic extrapolations (even though they were demonstrably unreliable). The only trouble with the theory is that not only the logistic distribution but also the normal, the Cauchy, and the other distributions can be fitted to the *same material with the same or better goodness of fit.* In this competition the logistic distribution plays no distinguished role whatever; most contradictory theoretical models can be supported by the same observational material.
>
> Theories of this nature are short-lived because they open no new ways, and new confirmations of the same old thing soon grow boring. But the naive reasoning as such has not been superseded by commonsense, and so it may be useful to have an explicit demonstration of how misleading a mere goodness of fit can be."

It is true that economists are not searching for a transcendental law of growth but the criticism still applies since the effort is to discover a law of growth without asking the question what causes growth.

A sound theory must begin by asking this question. Since observed variation is attributed to two sets of factors, *viz.*, rates of input use, etc., on the one hand, and rainfall, etc., on the other, the seemingly modest aim of measuring growth in output, purged of the weather-induced component, cannot be realised without an adequate knowledge of the working of at least one set of factors.

Let us consider two possible approaches to generate such knowledge: (*a*) estimating production functions, and (*b*) estimating rainfall-output (or crop-weather, more generally) relationships.

A production function, supposedly an inputs-to-output causal relationship, can tell us how much of the observed variation can be attributed to changes in input use; similarly crop-weather relationships will enable us to isolate that part of the variation arising out of weather fluctuations. Both

11. William Feller: An Introduction to Probability Theory and Its Applications, John Wiley & Sons, New York, 1966.

these approaches are logically sound and can lead to the sort of decomposition of output variation implied in (1); they are crippled, however, by theoretical and practical difficulties which appear to be insurmountable in the present state of knowledge.

Much has been written on the production function approach; we can do no better than refer to Krishna Bharadwaj[12] for a theoretical critique and to Vaidyanathan[13] for practical difficulties attending the estimation of technical relationships. For the problem on hand, *viz*., variations in aggregate output (which is usually a value-weighted average over crops and regions), the difficulties listed by these authors get multiplied. For, even if technical relationships are valid for individual crops in specified regions under specified technical conditions of production, it is not clear how to aggregate such relationships over crops and regions. At any rate, the use, in this context, of smooth mathematical functions which allow for mutual substitution between all inputs (such as land, labour and fertilizer) all along the input scale is meaningless.

Turning now to crop-weather relationships,[14] let us leave aside the relationships between meteorological variables like rainfall, on the one hand, and areas sown to crops, on the other (since these relationships are complicated by the fact that areas sown are determined to a certain extent by the relative profitability of different crops), and consider the relationship between yield per acre and rainfall (and other weather variables) in respect of purely rainfed crops. If one can isolate such relationships the job is more than half done. However, the research work done (as far as the author is aware) in this respect is not very satisfactory. The reliance is on multiple regressions (of the linear or non-linear type) of the yield on variables such as total rainfall (or the number of rainy days) during specified periods, maximum, minimum and mean temperatures, and humidity (see, for example, Sreenivasan and Banerjee,[15] and Das and Vidhate.[16] As Vaidyanathan points out, there is in such attempts no underlying agronomic theory which identifies the critical phases of plant growth, the particular meteorological factors which are important in each phase and the manner in which they influence growth, and the nature of interaction between different weather variables. In the absence of an agronomic theory which can yield quantitative relationships (of the type sought after) correlation analysis can produce misleading results.

12. Krishna Bharadwaj, "A Sceptical Note on the So-called Technical Relations in Agriculture", Working Paper No. 35, Centre for Development Studies, Trivandrum, June 1976.

13. A. Vaidyanathan, "Labour Use in Indian Agriculture: An Analysis Based on the Farm Management Survey Data", in P. K. Bardhan, A. Vaidyanatha, Y. K. Alagh, G. S. Bhalla and A. Bhaduri, (Eds.): "Labour Absorption in Indian Agriculture: Some Exploratory Investigations", Asian Regional Team for Employment Promotion, International Labour Organisation, Bangkok, 1978.

14. Vaidyanathan has reviewed a part of the literature on the subject. See his paper in Part III of this issue. (Ed.)

15. P. R. Sreenivasan and J. R. Banerjee, "Studies on the Forecasting of Yield by Curvilinear Technique—Rabi Jowar (Sorghum) at Raichur", *Indian Journal of Meteorology and Geophysics*, Vol. 24, No. 1, January 1973.

16. J. C. Das and S. G. Vidhate, "Forecasting Wheat Yield with the help of Weather Parameters—Part II—Uttar Pradesh", Scientific Report No. 160, Indian Meteorological Department, August 1971.

Since it is difficult to establish crop-weather relationships for specified crops and regions, it follows that it is even more difficult to establish the relationship between output aggregated over several crops and spatial units, on the one hand, and aggregate indices of weather, on the other. The fact that precipitation of rainfall exhibits a very large variation over time and space compounds the difficulty.

We may now raise the question: why do we need a 'trend' rate of growth? If actual output has fallen by ten per cent over the last year, it is no comfort to be told that it would have risen by three per cent had normal weather prevailed.[17] Some questions concerning growth and fluctuations can be answered by looking at variations in actual levels of output: the acceleration in the growth of wheat output in Punjab following the so-called green revolution and its subsequent tapering off is a case in point. True, simple arithmetic and graphical aids do not always provide unambiguous answers to questions which bother economists. But that does not justify the application of inappropriate tools of analysis.

17. Ashok Mitra, *op. cit.*, has written in the same vein: "It is with the actual end product of the efforts invested in agriculture that the makers of food policy have to concern themselves. It is neither here nor there if those entrusted with the responsibility for managing the nation's food over the next five years are told that a certain potential for agricultural expansion is being built. The nation cannot be fed with the potential, it is the actual realised output which will matter."