

## GWILYM JENKINS, EXPERIMENTAL DESIGN AND THE TIME SERIES

GEORGE E. P. BOX

UNIVERSITY OF WISCONSIN-MADISON

*EDITOR'S NOTE : This paper was presented at the opening session of the First Catalan International Symposium on Statistics, held in Barcelona, September 1983, in honour to the late Gwilym M. Jenkins. It is therefore a special paper in its format. It is a great honour for our Journal to publish it.*

We are here to honor this remarkable man - Gwilym Meirion Jenkins. He was my friend; and the times when we worked together were some of the happiest and most exciting in my life.

Gwilym was Welsh. Wales, you may remember is that bit of Britain on the left looking North that sticks out into the Irish Sea. People sometimes think about Britain in much the same way as they think about Spain, and I am ashamed to confess that many people do not know that Catalonia is a distinct entity in itself with its own customs, its own proud history and its own beautiful language that is very much alive. The same is true of course for Wales. In fact Gwilym Jenkins spoke only Welsh up until the time he was seven years old. His grandmother who lived to be 106 did not learn English until she was in her 60's because it was not until then that English was much spoken in her village. Gwilym told me that he sometimes thought in Welsh. Once, for example, when he lived in London he went on an errand for his wife Meg to buy a colander but returned empty handed because he could only remember the Welsh but not the English word - for "colander".

The Welsh are famous for their poetry and their song. It is said that a Welsh congregation sings automatically, not only in tune, but also in four-part harmony. Certainly you will find throughout British history that all minstrels that are mentioned - in Shakes

peare and elsewhere - are all Welsh. I take that to mean, in modern terms, that they are a race with a very active right brain certainly Gwilym possessed unusual inventiveness and intuition as well as remarkable analytical power.

He took a batchelors degree in Mathematics in 1953 at University College London with first class honors and completed his Ph. D. there in 1956. His dissertation was on Time Series Analysis.

After this he served for two years as a junior fellow at the Royal Aircraft Establishment at Farnborough where he helped to design aircraft. One of the problems he told me about which illustrates the kind of work he did there, concerned the design of an aircraft undercarriage. He explained how by running a little wheel along the runway its ups and downs could be recorded as a time series and the spectrum estimated. Since it was also possible to compute transfer functions for any design of undercarriage, a design could be selected for which low transmission occurred at frequencies where the spectral power was high - so ensuring that the plane did not shake to pieces on take-off. If was this kind of useful application of theory that interested Gwilym. He was equally at home with time domain as with frequency domain analysis and he was sometimes asked when one or the other was appropriate.

One reply he gave was simply that when, as

- George E.P. Box - Department of Statistics - University of Wisconsin - 1210 West Dayton Street - Madison, Madison WI, 53706 - U.S.A.
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in the aircraft undercarriage problem, you are directly interested in frequencies it is natural to work with frequencies but when you are concerned with matters such as forecasts occurring in real time then you would usually want to work in real time. There would be cases, of course, where switching from one to the other would greatly assist mathematical reasoning.

It seems that strong forces are always at work which try to divide the world of Statistics into two non-overlapping territories - theory and application. Gwilym fought constantly against those forces. He believed that not much was to be expected from theory without practice, or from practice without theory, and that the two were inextricably wedded with most useful theory arising from sensible practice and most useful practice based on sound theory in a never-ending iteration. The results of this mode of thinking come through strongly for example in his book with Don Watts on Spectral Analysis /1/ which draws on practical experience and is genuinely concerned with the problem of how do you do Spectral Analysis.

It was at the Royal Aircraft Establishment that he met his wife Margaret Bellingham. From that day onwards Meg's loving enterprise and devotion were to be his unfailing support. George Barnard quickly recognized Gwilym's ability and in 1957 he was appointed lecturer at Imperial College. His publications on time series also brought rapid recognition on the other side of the Atlantic and in 1959 he came as a visitor to Stanford University and later to Princeton.

I remember very well his visit to the Statistical Techniques Research Group at Princeton. My friend George Barnard had written me a letter by way of introduction to Gwilym in which he said something to the effect that - "on matters concerning time series he would value Gwilym's judgement before anyone, even before John Tukey". I was almost as intrigued by this letter as was John Tukey, and we quickly invited him to come.

His arrival in Princeton marked the beginning of a long and happy collaboration between us which later resulted in much visiting to and from between England and the United States .

It was during one such visit to Madison in 1964-5 that the seriousness of his medical condition was first realized. From now on he would fight a slowly losing battle against Hodgkin's disease.

For the next seventeen years his condition fluctuated unpredictably and disappointingly between moderately well and desperately ill. In circumstances that would have undermined the courage of a hero I have seen him continue to work at a pace which many a healthy person would have found impossible, and to somehow still maintain his buoyant optimism and sense of humor.

I want now to say something about the work we did together. Before I met Gwilym I regarded time series as a very boring subject. I'm sure this was because I had never really used it. Indeed I think that Gwilym was the first person I'd ever met who talked coherently about time series in terms of actually doing something with it.

But in the beginning our discussions weren't about time series at all, but about a problem in experimental design.

It can be interesting to consider by what strange routes real investigations sometimes proceed - starting off where you would not expect them to - making detours which later turn out to be profitless and so forth. Certainly what is finally published seldom provides much idea of the rather haphazard and messy route which has been taken. So I thought it might be of interest to try to reconstruct how, so far as I can remember, some of our work progressed.

Let me first explain about the experimental design problem that we began with. It concerned an idea for making an industrial process track a moving optimum. For example, suppose for some chemical catalytic process yield  $y$  was a function of temperature  $x$ . Then as the catalyst decayed with time the process curve representing the relation between  $y$  and  $x$  might drift as indicated in Figure 1 in a manner not knowable in advance. One way to make the process continuously adapt to such a moving optimum was to perturb temperature  $x$  sinusoidally and to use the observed variation in response -

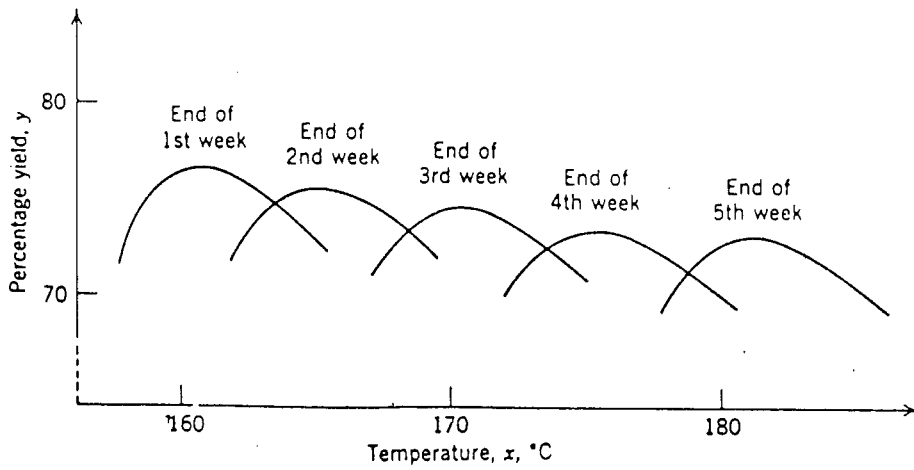


Figure 1. Typical drift in process curve caused by decaying catalyst.

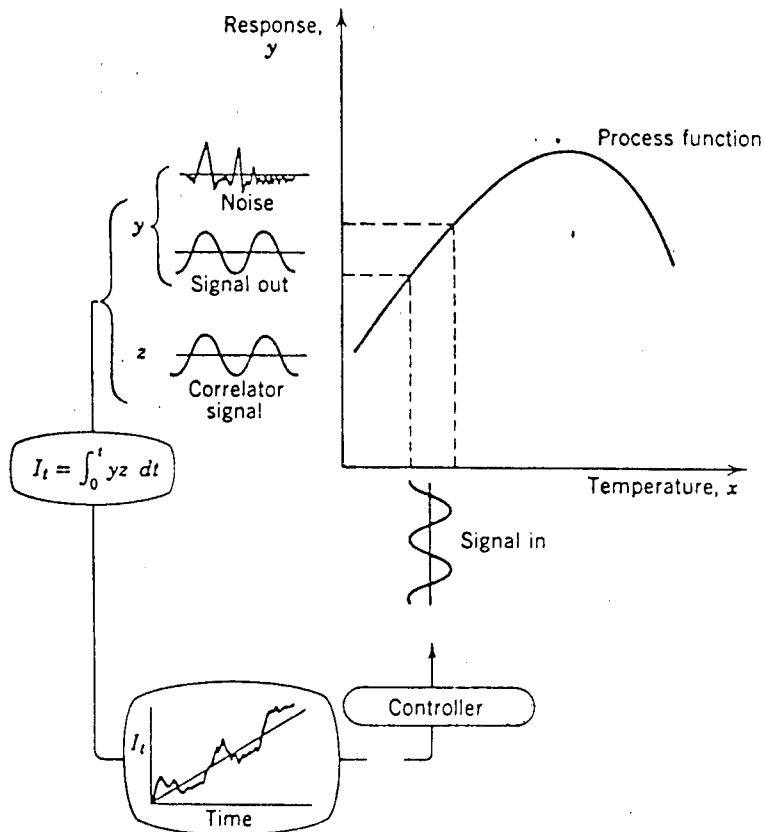


Figure 2. Automatic optimisation for a continuous chemical process.

to apply a correction to the mean level of temperature as in Figure 2.

Gwilym agreed that the thing to do was to try to get such an apparatus actually working where we could see it, run it, and get data from it. We didn't have any luck with the Chemical Engineers at Princeton. However, a few months later I moved to Wisconsin and the chemical engineers there, especially Olaf Hougen, greeted the project with enthusiasm and an apparatus of this kind was indeed eventually built/6/. (In particular, the "gas-furnace data" in our book /7/ came from that investigation).

The analogy with standard experimental design and analysis and with the philosophy of evolutionary operation/8/ was obvious (Figure 3). But Gwilym emphasized that if the continuous scheme of Figure 2 was going to work properly then we should need to pay careful attention to the dynamics of the system that was being perturbed and to the correlation structure of the noise we encountered. So we began to work on this. Also

he found considerable entertainment/9/ working out appropriate frequencies for sine wave designs when several factors were to be perturbed and optimized. In particular for second order designs you had to watch for confounding arising from harmonics produced by second and possible higher order effects. In addition, it was necessary to worry about these frequencies in relation to the power spectrum of the noise.

The work which eventually resulted in our book took place roughly between 1960 and 1970. It evolved in this way: Having started off thinking about automatic optimization and the importance of dynamics, we realized that what we were really involved with was a control problem of a very special kind/10/. This meant that we started to work on more general problems on discrete control. In particular, we tried to better understand the relation of what we were doing to other kinds of statistical control.

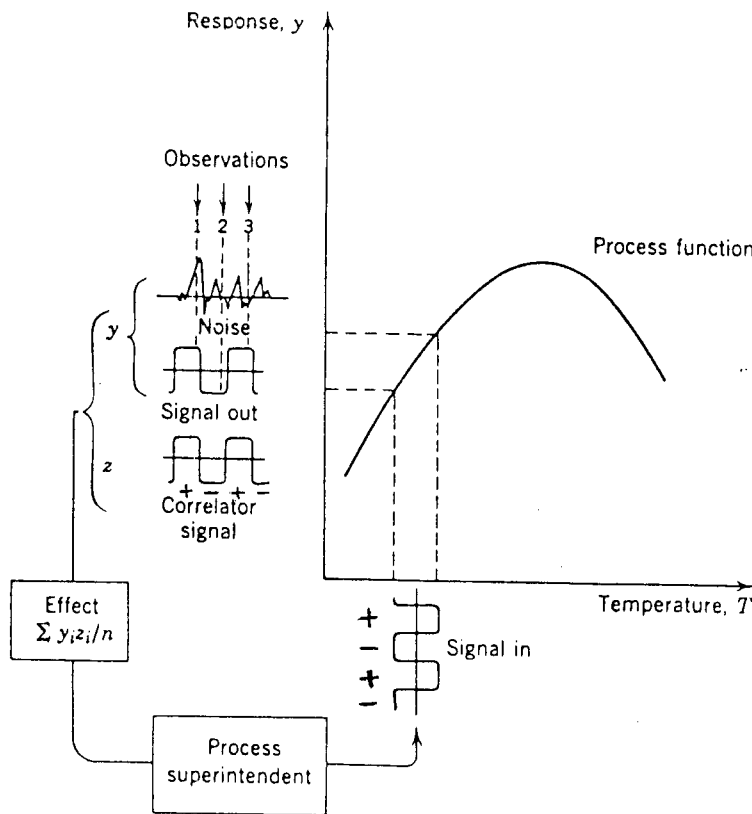


Figure 3. Evolutionary operation.

A study that didn't appear in our book/11/ but kept us intrigued for some time was nicknamed the "machine tool" problem. The idea was that in, say, a machining operation the natural tendency was for the measured characteristic not to vary about a fixed mean but to drift away from target in a manner which might be represented by a non-stationary process such a (0,1,1) ARIMA process. We further supposed that the loss incurred by being a distance  $\delta$  off-target was  $(a\delta^2)$  dollars. When this deviation became sufficiently serious the machine could be stopped and the mean level readjusted, but this would cost C dollars. The problem was to design a strategy which minimized overall loss. We were a little surprised when the answer was equivalent to plotting deviations from target on a chart with two parallel action lines. This was like a Shewhart chart - but with a totally different justification and a totally different basis for setting the "action lines". Another difference, but often in practice not an important one was that it was one-step-ahead forecast ( an exponential average of past data ) that was actually plotted. Such a chart using exponential averages had earlier been recommended on empirical grounds by Roberts/12/. An example is shown in Figure 4.

Our early papers together emphasized the iterative and adaptive nature of experimentation itself and pointed parallels to evolutionary operation, adaptive optimization and to feedback control. But certain problems of feedback control can be thought of in terms of forecasting - one can control by acting in such a manner that a forecasted discrepancy is cancelled. So we became interested in forecasting also. Interwoven with all of this was the question of the type of models that were appropriate.

A general principle that Gwilym found very appealing was that if something worked and had withstood the test of time then there must be a good reason for it. One entity that qualified in the area of control was the proportional plus integral controller which in one form or another had been used successfully in industry for over a hundred years. For discrete data such a device requires that compensatory action  $X_t$  at time t is of the form

$$X_t = k_1 e_t + k_2 \sum_{i=1}^t e_i \quad (a)$$

where  $e_i$  is the deviation from target of the controlled variable at time i and  $k_1$  and  $k_2$  are constants.

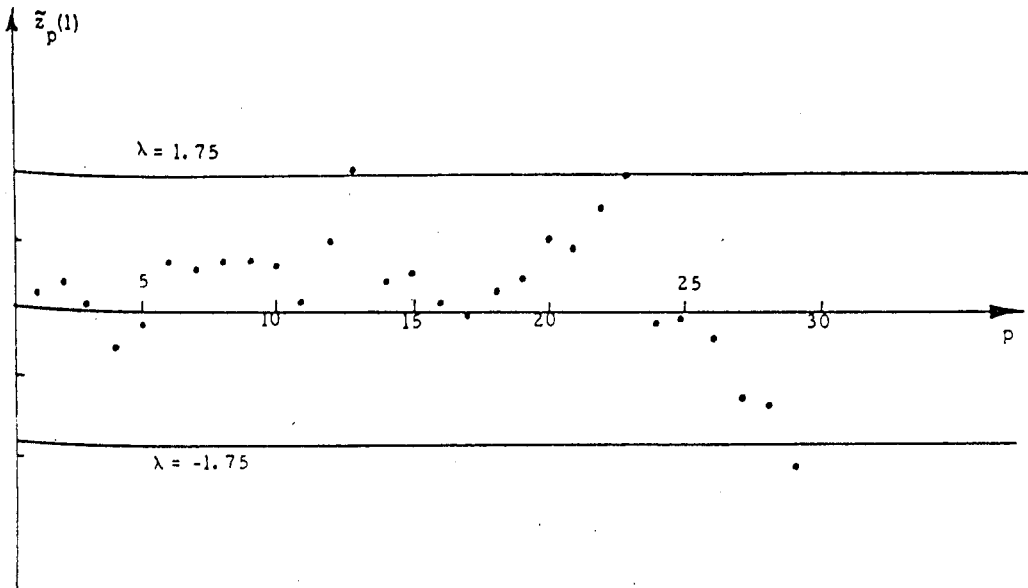


Figure 4. Predictions  $\tilde{z}_p(1)$  of process deviations with action lines.

Another such device that qualified in the area of forecasting a discrete series  $\{z_t\}$ , was the exponentially weighted moving average of past data

$$\hat{z}_{t+1} = (1 - \theta) \sum_{j=0}^{\infty} \theta^j z_{t-j} \quad (b)$$

which had been introduced more or less on empirical grounds by Holt/13/, Winters /14/ and others. For many business and economic series this kind of forecast worked surprisingly well. At least it was capable of tracking non stationary phenomena.

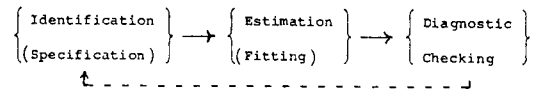
It can be shown that both equations (a) and (b) are optimal if the disturbance to be controlled in the first case, and the series to be forecast in the second/15/, are both members of non-stationary autoregressive-moving average (ARMA) processes whose autoregressive polynomial has one or more zeros on the unit circle. Some time later it became clear that seasonal processes with period  $s$  could often be parsimoniously accommodated by employing models which included a backshift operator which related items  $s$  intervals apart /16/.

When models of this kind were appropriate simple linear operations such as differencing could produce stationary autoregressive-moving average processes the properties of which had been extensively studied /17,18,19,20,21, 22,23,24,25/.

We soon became adherents of the view that whatever you wanted to do with a time series, whether it was to control it, forecast it or (later) to seasonally adjust it, you first needed to build a model for it using available data and some common sense. Once a satisfactory model had been built then the right way of doing whatever you wanted to do would be made manifest. The model-building process was thus of central importance. While potential ingredients for model building were available such as spectral analysis, autocorrelation analysis, various procedures for estimating parameters and various tests of fit, we were not sure how, or if at all, these fit ted together into a coherent system.

Gwilym's inclination was to try things and see if they worked, letting success or failure indicate in which direction we should go. I liked this way of working too.

Modern time series analysis is possible because of the electronic computer. Gwilym had already realized this before the 1960's, and worked very hard to have programs written that would enable us to experiment easily, Sam Weller said something to the effect that "nothing clarifies the thoughts of a man so much as the knowledge that he is going to be hung tomorrow morning". With this in mind we set about attempting to model as many real time series as possible. By trial and error this led to a three stage system of model building that seemed to work and which we eventually adopted. This was of the form



The three ingredients were

Identification: - getting an idea of what the general form of the model might be, using visual displays of the data and of minimally parametric identifiers such as the autocorrelation and partial autocorrelation functions.

Estimation: - having got an idea of what kind of model might be worth trying, to act temporarily as if we believed it and to estimate its parameters using likelihood.

Diagnostic checks: criticizing the fitted model by visual checks on residuals, their autocorrelations etc. and by more formal checks of fit; leading in some cases to modification (re-identification) of the model.

We thought that it was particularly important not to try to make the model building process automatic and entirely controlled by the computer, but to ensure that the human brain intervened particularly at the identification and the -diagnostic checking/model modification - stages. Subsequent experience has, I believe, demonstrated the rightness of this idea.

We found the problem of model-building philosophically puzzling because on the one hand you have a model which is very specific and on the other you know that it must be false, since models are, at best, approximations. When, for example, you write down the likelihood, the model must necessarily be treated as true - not nearly true but exactly true. When you are checking fit, however you are -

obviously acting as if you no longer believed in the model's necessary truth. To be a good model builder it seemed essential to be a bit schizophrenic. One must be prepared to be a whole-hearted sponsor for the model on one leg of the iteration and its wholehearted critic on the other. One of the ideas that later came, at least partially, out of this was the implied need for two different kinds of inference. /34/.

We came to think (a) of the model building process as the iterative building of a filter which transformed data to white noise which appeared to independent of any known input; (b) that this could be accomplished by the mind and the computer appropriately combining their talents in an iteration involving identification, estimation and diagnostic checking; (c) that, probably, this was the kind or procedure by which all statistical models ought to be built whether time series or not.

Concerning estimation; at the time when we were writing our book, Gwilym favored the likelihood approach /26/ although he was happy to indulge my wish to introduce a little Bayes as well. In fact, for samples of the size we usually encountered, the two approaches usually gave results that were essentially identical.

When you argued with Gwilym you argued with someone who was constructive, friendly, sympathetic but firm and I found our discussions highly educational. They were our chief way of "sorting things out" as Gwilym used to say. When we were together and Gwilym was well enough, we used to go on walks where we discussed things. I remember, for example, a problem we called the "golf course" problem because it was while walking on the golf course that we first thought of it. Another problem involved the "jam jar" model because we had an analog about a jam jar filling up with water. Discussion of a problem could be taken up at any time and we would see if, by kicking it around between us a bit, some further progress could be made. During the long periods when we weren't together we would send tapes to each other usually wrapped in a piece of paper with the equations written on it. I saved some of the tapes and one devastating thing I discovered was how idiotic I sounded as I listened to an old tape of myself discussing

twelve months previously a problem to which we now knew the answer.

It was Gwilym's idea to write the book. I remember his saying to me something like "Look, George we can go on writing papers about this and that aspect, but we seemed to have got ourselves involved in trying to sort out a philosophy of how to go about building and actually using time series models. We've got a lot of explaining to do - so let's write a book".

Gwilym's work with me was, of course, but a small part of his contribution to Statistics and still less of his overall scientific contribution. By 1964 he had been promoted Reader in Statistics at Imperial College but he was beginning to see that Statistics, even when properly applied, could not of itself solve the problems he wanted to tackle. His consulting work concerned systems which, whether they were hospitals, government departments, or chemical plants, contained many interdependent sub-systems each of which has to be viewed in relation to the other. A neat statistical solution of one particular aspect of a problem might result in no benefit overall.

When he became Professor of Systems Engineering at Lancaster in 1965 he was anxious to study further how statistical methods fitted into a wider system, and also to train his students so that they should be able to go out into the world and do useful things. The Master of Science degree which he devised required not only knowledge of Systems and of Statistics, but also the successful undertaking, under faculty supervision, of a major project in industry, government or some other suitable field. This plan guaranteed close contact between the University and outside areas of application, wedding theory to practice, and tended to ensure the relevance and originality of the Department's research.

I wish that more schemes of this sort could be instituted for training statisticians. This would certainly improve the students' knowledge of Statistics. But it might have other desirable consequences. It might, indirectly and in time, improve the curriculum, and the teaching and competence of the professors. Perhaps paramount, in a few rare but important instances it would inevitably lead to genuinely new

and exciting research in Statistics - experience certainly shows that fresh theoretical innovation frequently originates from thoughtful practice.

Many of the cooperative projects at Lancaster were highly successful and industrial and government clients became anxious to support further joint endeavour. It was to meet this need that in 1974 Gwilym Jenkins founded and became Managing Director of ISCOL, a consulting enterprise wholly owned by the University of Lancaster. In these days, when the mutual benefit which can flow from interdependence between University and Industry is perhaps better accepted, such an initiative might be welcomed unreservedly. In 1974, however, it generated political problems which finally proved unmanageable and Jenkins left the University to form his own Company and at the same time to take up a visiting professorial appointment at the London Business School.

The satisfaction of the absorbing work in which he was soon engaged and the international success of this company might have been enough for most men. But he saw his projects, whether they concerned coping with pollution of the Rhine, planning electricity generation, or forecasting employment for the European Economic Community, as important not only in themselves, but also as case studies from which others could learn. We are told that students and teachers of Medicine in the Middle Ages did no dissections and had little practical knowledge of the functioning of the human body but instead formulated their art entirely in terms of theory. Statistics is still sometimes taught in a similar way. Perhaps because of this, although hundreds of thousands of words are written each year in books and journals devoted to Statistics, few of these concern the course of actual investigations in which Statistics has been employed. For this reason the two volumes of case studies which he gave us "Practical Experiences in Modelling and Forecasting Time Series/27/" and "Case Studies in Time Series Analysis/28/", are gems of especial value for researcher, student and teacher alike.

In 1969 Jenkins had founded, and was coordinating editor for, the Journal of Systems Engineering. Furthermore, in 1971 with his friend and long time collaborator Philip

Youle, he wrote an excellent book for the layman on Systems Engineering/29/. These authors believed that systems methods were important not just to the chemical industry, the hospital, and the local bus company, but, for example, to secretaries, housewives, and clergymen. The uninstructed might confuse systems analysis with regimentation. They showed how the ideas of systems analysis, far from restricting his options, could liberate the worker from the common frustrations and discouragements which arose from lack of a rational approach.

In recent years the effectiveness of foreign industrial competition has all too clearly demonstrated the superiority of this kind of thinking over traditional European and American management practice, which seems to have adopted accounting rather than scientific method as its inspiration. In particular the United States and the countries of Western Europe have found it increasingly difficult to match the quality and price of products from Japan. It is well known that an important reason for Japanese superiority is their application of statistical methods to the management of production. The methods employed are not new and indeed were mostly invented in the west. What they have done is essentially to use the Systems Approach so that these methods are appropriately integrated and made to suffuse the whole structure of management.

Experimental Design, particularly using orthogonal arrays/30,31,32/, is a statistical technique that is used extensively both in raising the mean level of quality and in reducing its variation. The real problems of experimental design in quality improvement cannot be appreciated if we adopt a model that assumes that errors are random drawings from some distribution having fixed mean and variance. For, if statistical design is to be used to move an unsatisfactory process to new operating conditions where it will be in a state of control we should not, assume that it is there already. Fortunately the basis of statistical design was laid by a very practical man - R. A. Fisher/33/ who worked in an industry - Agriculture where his experimental material was never in a state of control. His ideas using replication, randomization and confounding to produce small blocks were fashioned for systems with disturbances that could be auto



rrelated and even non-stationary. This is illustrated in Figure 5 which shows a nonrandom disturbance such as might be encountered in relation to a  $2^3$  design randomized in small blocks of four. With this we come full circle to the link between time series models and experimental design which Gwilym and I found so intriguing.

Gwilym's life was an inspiring one, his boundless optimism and cheerfulness made possible his great achievements even in the face of pain and recurring disappointment. I count myself especially fortunate to have been his friend.

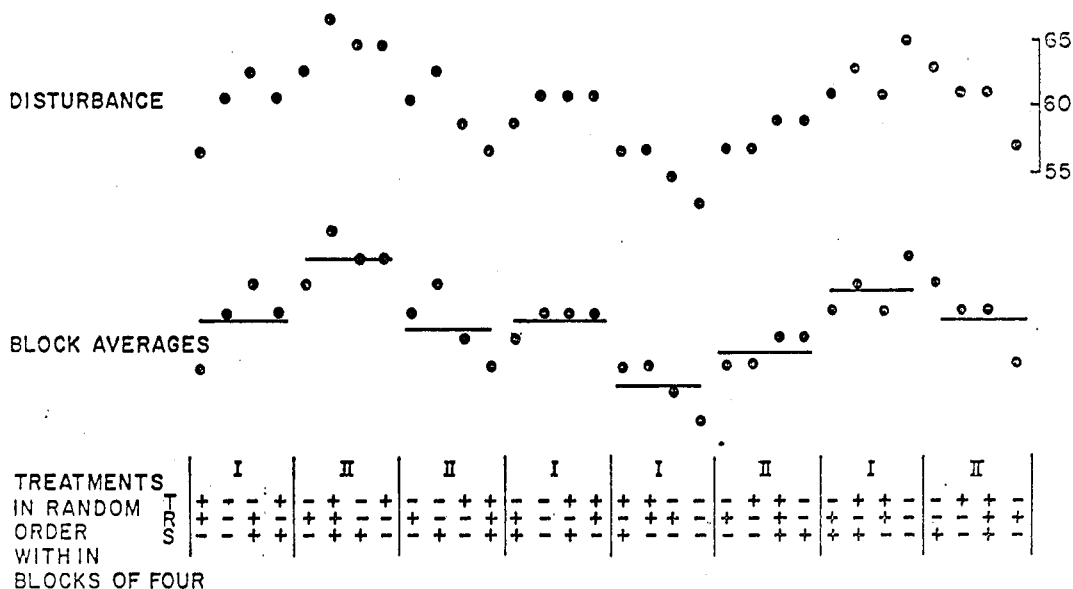


Figure 5. Autocorrelated disturbance. Averages for blocks of four. Replicated  $2^3$  design, randomized within blocks with TRS interaction confounded.

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