design. The notion of variation reduction as the main objective of experimentation and the issues of planning and modeling strategies for achieving this objective have raised many novel and fundamental questions not considered in the literature. For readings on parameter design, I recommend the excellent panel discussion in *Technometrics* edited by Nair (1992) instead of the author's subsection 3.3, which misses this important reference. There are also important advances made in "traditional" design. Let me mention a few examples here. Use of the minimum aberration criterion for factor assignment in factorial design poses a very difficult and still unsolved problem in coding theory (see Chen and Wu, 1991). Construction of supersaturated designs poses another combinatorial challenge (see Lin, 1993; Wu, 1993) and can benefit from work on search designs due to Srivastava (1975). Design and analysis of computer experiments present new challenges not encountered in physical experiments. Before making such a statement the author should have thoroughly researched a basis for it!

An unforgivable mistake he makes is in judging the recent advances in experimental design based on a book edited by S. Ghosh. The author should know well that most original ideas appear in refereed journals (like *Technometrics*). I suggest that the author read the last seven years of *Technometrics* before making such a grandoise statement. If referring to these "conventional" topics, the author says "[they] are either intellectually stale, or quickly becoming so." It is amusing to hear this from someone who is apparently unfamiliar with the recent advances in experimental design.

The author lists four "new" topics which in his opinion "would provide enormous profit potential to industry." These new tools are interesting and may have the potential to be used by research statisticians in industry. However, it is premature to make such a statement. Can he point to many (or even a few) case studies to support his speculation? By comparison "conventional" tools such as experimental design and statistical process control have already helped industry reap enormous benefits. Partial least squares is being used by some researchers in the chemical industry but widespread use of them is still far off. High-dimensional response surface analysis and spherical regression are still in their infancy in industry.

A DISTORTED VIEW OF ACADEMIC STATISTICS AND STATISTICIANS

Let me choose to refute only the more blatant statements made by the author. At the end of Section 2 he says, "chucking money at university statisticians is a laudable charity, but may be a breach of fiducial responsibility. . . ." In Section 6 he says, "To academics the economic progress of an industry . . . is a distant consideration, except . . . rationalizing a proposal for funding." This cynical view is ill-founded. In fact academic statisticians (unlike our colleagues in computer science) have not received much funding from industry. Those who are good or lucky enough to initially get funded have to work extremely hard to continue the funding. I invite the author to supply evidence to support his claims. Another incorrect statement at the end of the paper is that "It is probably past time for university researchers to drop stale pseudo-applied activities (such as control charts and oddly balanced designs)." Both are real applied activities and are not stale.

Let me conclude my discussion on a more positive note. I do share the author's concerns with the overemphasis on theory in academic training and the tendency of some professors to place weaker students in industry (or government). Again one cannot be too pessimistic. There are schools which show intellectual respect for work in industrial statistics and I like to think that Carnegie Mellon is one of them.

Comment

H. P. Wynn

The divisions which Professor Banks highlights between his three groups of statisticians pale into insignificance compared to the other divisions which hold back the use of statistics and related methods in industry. The most serious of these also haunts the hallways of academia. This is the professional division between engineering as a discipline and statistics. The strength of the separation is not uniform. For example, electrical engineering has led the way in areas such as automatic

H. P. Wynn is Professor of Statistics at the School of Mathematics, Actuarial Science and Statistics, City University, Northampton Square, London EC1V OHB, United Kingdom.

control and signal processing. Even here, the division between control theorists and time series people still persists, as does the division between classical statistical process control and automatic control.

In mechanical engineering the use of statistical ideas is much sparser, although it does have a firm hold in several areas such as reliability, particularly structural reliability, and statistical modelling in materials science. Although the fashionability of Total Quality Management may have peaked as more and more companies have been through some kind of culture change programme, I am one of those who feel that it has several important messages. My own favourite for top of the list is teamwork. The idea that good engineering, good science, good product development and so on, can only take place in a team environment seems of very great importance, although not entirely new. One of the important roles of teams is to provide an intimate platform on which many of the barriers can be broken down and in which scientists and engineers, managers, computer experts and so on can communicate and interact in a spirit of mutual respect. This mutual respect does not mean that the statistician is forced to use the most naive and unsophisticated methods just so the engineer can understand them, nor should it mean that the engineer should stop to explain in absolute detail some important technical aspect of metal fatigue. The team environment should be one in which all professionals can operate at a high technical level. This does mean, of course, that there are certain amounts of common methodology, shared technical knowledge and, obviously, common goals. The role of management is to provide these goals but also to provide generalist glue to cement the team together. Other disciplines which have a more generalist approach are also critical, such as engineering design and mechatronics.

My main remarks concern experimental design which does not get a particularly good press from Professor Banks. To some extent I agree with him about the use of classical factorial fractions, although there is still considerable work to be done to understand the relationship between both "regular" fractions (for which there are now many computer algorithms), classical irregular fractions (Plackett-Burman and others), useful but ad hoc fractions (Wu and others), optimum experimental design (again for which there are now many different algorithms) and the whole area of designs for computer experiments such as Latin hypercube sampling and its close relative the low discrepancy sequences used in numerical analysis.

But there is a more profound message than just "use a good experimental design" which the statisticians can convey to engineers and industry in general, namely, some understanding of the scientific method. It is easy to go along with the methodological criticisms of Genichi Taguchi's work and not gain from the impact it is having and may continue to have on the use of experimentation generally. Both statisticians and their colleagues in other disciplines have suffered for too long from the demise of what used to be called the scientific method. Despite the massive amount on the philosophy of induction and dialogues between Bayesians, non-Bayesians and so on, very little attention has been paid to the philosophy of experimentation. It should be of considerable interest to people that the "Taguchi revolution" in the use of experimentation in industry has coincided with a similar growth in scholarly work on the study of experimentation in the philosophy of science (e.g., Franklin, 1986; Hacking, 1983). This tradition, which really dates back to Francis Bacon, submerged in the nineteenth century by criticism by people like W. S. Jevons and in the twentieth century by the growth of analytic philosophy generally and the somewhat unconstructive approach of the falsificationist school of Karl Popper. What we are now finding in the philosophical literature is a return to the primacy of experimental methods with lengthy studies of experimental protocols, particularly in nuclear physics. There has been less neglect in the social sciences and medical literature (see Kish, 1987).

One particular thread of this material I should like to pull out is the spectrum between passive observation and controlled experimentation. This is something which Taguchi has emphasised indirectly with his differentiation between control variables and noise variables. Teaching Total Quality Management in our own M.Sc. course, I spend the first day trying to relate the modern quality revolution to some of the classical ideas about experimentation and have come up with a brief list which tries to classify this active/passive spectrum, fleshed out with examples. I am nervous about presenting this list to a wide audience but it has certainly proved useful in discussions with several "generations" of students. I have called them "modes of intervention" in reference to the importance that Hacking attaches to experimentation as intervention:

1. Invention, fabrication
2. Controlled experimentation
3. Control
4. Search
5. Passive observation
6. Forced attention
7. Non-epistemic observation

The game is to try and fit well-known procedures into these categories. For example, Taguchi-type experiments are a combination of controlled experimentation (2) and passive observation (5). Control and search are dynamic versions of controlled experimentation in which the system must respond adaptively to the outside world. Forced attention is the classical imperative from data analysis: be on the lookout for unforeseen effects. Non-epistemic observation is a phrase that philosophers have used for the phenomenon that one may have observed something without knowing it at the time. The top category, which is the most active category, in which one performs some act of making or doing is sometimes referred to in philosophy as *verum factum*, knowing through doing.

It is instructive to take what is now commonly accepted as the iterative and cyclical nature of the scientific method and quality improvement (see the Shewart–Deming chart) and trace back the ideas several hundred years. Here are two of my favourite quotations. Claude Bernard (1865), in his book on experimental medicine, states, "An experiment differs from observation in this, that knowledge gained through observation seems to appear of itself while that which an experiment brings us is the fruit of an effort that we make with the object of knowing whether something exists or does not exist." And a very Baconian quote from Jevons (1874), a principal critic of Bacon himself, reads, "It may readily be seen that we pass upwards by insensible gradations from pure observation to the determinant experiment. . . . The successful investigator must combine diverse qualities: he must have clear notions of the results he expects and have confidence in the truth of his theories and yet must have that candour and flexibility of mind which enable him to accept unfavourable results and abandon mistaken views." It is the responsibility of statisticians of whatever type to rescue the practical side of the philosophy of science, particularly as embodied in the design and analysis of experiments, and deliver it to the world. This leaves an admirable role for the academic statistician, who may have a theoretical bent, which is to try to continue to understand the dynamic nature of the whole process.

Rejoinder

David Banks

I thank all the discussants and am pleased that their spectrum of opinion confirms my belief that important controversies underlie the current practice of industrial statistics. From the comments, my sense is that statisticians with experience in manufacturing industries find broad agreement with the paper, in contrast to academic statisticians whose research is in industrial statistics. Between these extremes, traditional academic statisticians tend to be favorable, but industry statisticians in quasi-academic environments (the Bell group) are less so. If there is a latent variable that explains this ordering, it may reflect the degree to which these researchers are expected to simultaneously justify their work on two criteria: statistical merit and practical value.

There was strong consensus on statistical education. I think most agree that the best single thing academic statisticians can do for industry is to revise program requirements in the terminal M.S. degree. The current curricula contain enormously too much inference and probability, but insufficient exposure to a breadth of toolkit topics. The precise content of an ideal curriculum is debatable, but the discussants have persuaded me that it should give conceptual coverage of survey sampling and categorical data.

TQM evoked more diversity. Many discussants were uncomfortable with my claim that the TQMperor has no clothes. I concede that the ubiquitous expression of the contrary view may have seduced me into overstatement. A possible compromise is that the TQMperor has really nice underwear. Specifically, I can agree that TQM has useful but staggeringly simple ideas. Their connection to statistics is generally slight. A manager who does not incorporate what is good is doomed. Managers should assess each TQM precept thoughtfully, in the context of their own situation, and not expect that adoption of TQM is sufficient, or even primary, for their survival.

Stitching together several of the discussants' comments, TQM may be successful because it creates a corporate climate in which statistical reasoning can flourish. Insofar as statistical thinking replaces wishful thinking by factual evaluation, it can only benefit industry. In particular, decision makers should have a grasp of the principles of experimentation, the critical weighing of evidence and random variation.

Some discussants felt that the overview of industrial statistics was narrow and incomplete. Doubtless, it was incomplete. And I agree that it spoke more to conventional manufacturing than to the R&D environ-