Comment

C. L. Mallows and D. Pregibon

How refreshing to see a paper such as this! The real world is much more complicated and interesting than the artificial one in the textbooks. We are sure that everyone engaged in serious statistical consulting has his or her own set of horror stories. It used to be that statisticians were exhorted to learn their trade by apprenticeship; fortunately there seem to be some general principles, now being identified, that suggest how a consultant may reasonably proceed. Dr. Chatfield has been a leader in identifying these principles. We must hope that teachers and the authors of the next generation of textbooks will give these matters the attention they deserve.

We would like to mention some related work that bears on the subject. Chatfield argues that *strategy*, a largely neglected topic, is at least as important as *techniques* of data analysis. Indeed, as vocal proponents of SPC, the statistical community owns a process that is not in control, the *process* of data analysis. Sometimes it works and produces good quality analyses, and sometimes it does not. Unfortunately we do not understand how to ensure good quality. Part of the problem is that there does not exist a "theory of data analysis" on which to base measurements of quality. Mallows and Walley (1980) and Mallows and Pregibon (1987) attempt to get at the underlying principles of data analysis, with limited success. These articles discuss statistical concepts without appealing to probability models that, in much applied work, are wholly contrived.

A fundamental notion that we have been struggling with concerns "judgments of exchangeability." In its most basic form, this concerns knowing when data may be aggregated and when data may be ignored. Aggregation, even as simple as averaging, requires that the units being aggregated are similar or "exchangeable"; if they are not, aggregation is either meaningless or misleading. In collaboration with Draper and Hodges of RAND, we believe that we have made progress in elucidating the components of exchangeability judgments in data analysis (Draper, Hodges, Mallows and Pregibon, 1991).

Another reason that the process of data analysis is ill-understood is that it is difficult to think in general terms; every problem seems to have special aspects. Or is it that many scholars do not get involved in real-world (messy) problems, so that thinking and writing about strategy is completely alien? Chatfield provides a few references to discussions of strategic issues, and to them we add Daniel and Wood (1971) for regression (note that a flowchart appears on the page facing page 1!) and Nair and Pregibon (1986) for quality improvement experiments.

Another bibliographical note concerns Chatfield's general guidelines in Section 3. In his wonderful book, Polya (1957) describes the qualitative steps in the mathematical problem solving process. These are (1) understanding the problem; (2) devising a plan; (3) carrying out the plan; and (4) looking back. These four steps translate roughly into Chatfield's guidelines (subsection headings in Section 3). What this indicates to us is that solving problems using statistical methods is not very different from problem solving in general. Thus there is commonality to be exploited and articles such as Chatfield's contribute to understanding the problem solving process.

Our final comments concern Chatfield's remarks on software and its role in data analysis. It almost appears that he is blaming the computer for misleading scientific investigators, who are otherwise thoughtful and thorough. Our view is that software can help rather than hinder the data analysis process. This can happen and is happening in two distinct and complementary ways.

The first focuses on techniques and, in particular, interactive graphical techniques. Indeed, in the quotation attributed to Cox, surely graphical methods are better suited to draw our attention to nonstandard features than are nongraphical methods. Tukey has said "numerical summaries focus on expected values, graphical summaries on unexpected values." The challenge is to harness the computer to work for us. A specific exmaple might help to highlight the issue: In Chatfield's first example concerning "perfectly correlated" variables and the detective work required to explain the anomaly, we envisage a simple interface such that the entries of a correlation matrix are "mouse

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sensitive" (a mouse is a graphics input device on PCs and modern workstations), so that clicking on an entry will cause the corresponding scatterplot to be displayed. Thus the convenient numerical summary that we have all come to know and love and suspect, is complemented by a graphical display that is available as needed. This is computing power working for us—it is what we should require software vendors to supply.

The second opportunity concerns so-called expert systems and how they attempt to embody statistical problem solving strategy. Our experience (Gale, 1986; Pregibon, 1986) with such systems is restricted to Polya's third step—carrying out the plan. (The first two steps involve the problem context to a sufficiently high degree that we do not expect rapid progress in bringing such systems to fruition.) Even this third step is challenging. Once we have the ability to encode a sequence of analysis steps into a software representation, we have a testing ground for strategies that use different sequences of steps or different techniques at each step. This suggests the following specific problem: Characterize the variability in the *process* of regression analysis. How might one go about solving the problem? Assuming that analytic solutions are intractable or not useful (overly simplified!), the only viable alternative is to appeal to computing technology. This includes both hardware to perform computations rapidly and software in which to represent the sequence of analysis steps and their associated techniques. Apart from our own attempt to bring computer power to bear on the problem (Lubinsky and Pregibon, 1988), we know of only one other serious attempt (Adams, 1990). Our journals and our textbooks are filled with an excessive amount of material on the *techniques* of data analysis. This energy should be applied to the *process* of data analysis. This poses an interesting challenge for the field, and computing technology provides a means to address it—who will heed the call?

Comment

Douglas A. Zahn

1. INTRODUCTION

This article is an important contribution to the literature on improving the quality of the services provided by the specialist statistician. The checklists and cases are useful to me; I will incorporate them in my practice and in the statistical consulting course my colleagues and I teach. I am confident that many others will also do this. I like the article's focus on avoiding trouble; it is reminiscent of old sayings such as "A stitch in time saves nine" or "An ounce of prevention is worth a pound of cure." In the language of the quality movement, the author is encouraging us to move upstream in our process as we seek to improve its quality.

I have two concerns about this article. I agree that avoiding trouble deserves more attention as a strategy for improving the quality of the statistician's services. However, this article addresses only the statistical aspects of avoiding trouble. It does not address how the relationship between the statistician and scientist relates to avoiding trouble. It also does not address how one might go about systematically improving the quality of one's services. In the words of one client from whom I have learned much, "Mere knowledge itself will not change behavior." What, in addition to checklists and good advice, will it take to change a statistician's behavior so as to produce improved services?

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2. PITFALLS AND RELATIONSHIPS

I propose that the most important step for the statistician to take for avoiding trouble is to establish a working relationship with the scientist. A key part of developing this cooperative relationship is remembering that generally the statistician is involved in a project as a guest of the scientist. Other aspects of developing this relationship include aligning on goals with the scientist, being honest and not putting down, deriding or denigrating the scientist in any way, overtly or covertly, consciously or unconsciously.

Reflecting on this and rereading the article has led me to be concerned that the article is sending the wrong message to its audience, less experienced specialist statistician practitioners. To my ears, the article has the flavor of post-dinner conversations over drinks about how I saved science from the onslaught of those poor clients. I may be overly