the statistician who usually is a guest invited by the scientist. This implies to me that the statistician has the job of selling the idea to the scientist that showing the data to the statistician will be useful. When this sale is made, the scientist is enrolled in thinking this action will be useful and does it from this point of view rather than from the point of view that he/she was forced to do it. Most people I have talked with have vivid memories of how differently they do a task when they feel forced to do it rather than when they are enrolled in doing it.

4. SYSTEMATIC QUALITY IMPROVEMENT

Another important step to take to avoid trouble is to implement a process for systematically learning from one's past troubles. I have described such a process in Zahn (1988).

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I am grateful to John Fox, Duane Meeter, Gayle Muenchow and Andrea Zahn for extensive conversations on the Chatfield article which were most helpful to me as I prepared this discussion.

Rejoinder

Christopher Chatfield

I would like to thank all the discussants for their encouraging comments and for the additional guidelines, instructive examples and references. (I would add one more reference, namely Feynman's, 1988, illuminating account of the space shuttle catastrophe discussed in Example 8.) Taken as a whole, the discussion contributions provide a valuable commentary on the article and should be read in conjunction with it. I am very grateful to Jim Zidek for organizing this discussion.

Most of the comments require no reply from me and the absence of any response to a particular point implies no value judgment on that point.

The role of IDA deserves brief comment. Let me emphasize that its relative importance varies considerably from problem to problem, depending on the background information, etc., and I agree with Bailey that subsequent sophistications in the "statistical" analysis are often less important than the IDA phase, while also agreeing that one should generally not let the final analysis be completely dictated by the IDA. I also agree with Andrews that it is best to keep the analysis simple wherever possible.

I am pleased that Mallows and Pregibon agree that the *process* of data analysis needs more attention and welcome their additional references, particularly Polya (1957), which I nearly included myself. Andrews' comments on the scientific process are also relevant here.

Clayton and Nordheim provide wide-ranging insightful remarks on problem formulation, design and analysis, and I particularly concur with their comment that an analysis may be "optimal" in a different way to that usually assumed in textbooks.

Zahn has provided much helpful comment on consulting skills based on his wide experience therein. The only note of discord comes when he interprets some of my remarks as reflecting an "adversarial or condescending attitude towards scientists." Let me make it clear that I entirely agree with Clayton and Nordheim that "the active involvement of the investigator is essential in a successful statistical investigation," and with Zahn that "the most important step for the statistician to take for avoiding trouble is to establish a working relationship with the scientist." Either I have expressed myself poorly, or Zahn has detected undertones to my paper that were not meant to be there (although, like other statisticians, I would be less than honest if I pretend that I never get exasperated with a "client"). As regards the "time is pressing" comment, let me explain that in that particular case the pressure was not being applied by the people doing the work, but by upper management who had imposed unrealistic deadlines. As for the Prelude, paragraph 3, I still think that a default value of 999 million is ridiculous whether or not one spots it straight away. The default value was not selected by the person I was working with, and he thought that it was ridiculous too! However I do agree that the statistician is at least partly (mostly?) to blame when he fails to extract crucial information from the client, and that we must be careful not to make statements that "put down" the scientist (although I expect most of us will do it unconsciously from time to time).

Glick's entertaining remarks touch on many sensitive issues and will strike a chord with those of us who have to handle "difficult clients." Even so I

entirely agree that consulting and data analysis can be *fun*. In response to his "10-Step Hands-off Guide for Researchers," I also can give a 7-step guide on how to be a "good" statistician (with tongue very much in cheek!!!). The rules are:

1. Use a complicated ritual like a war dance or factor analysis—much more impressive than a sensible descriptive analysis.
2. If a 2-tailed test is not significant, try a 1-tailed!
3. Give *P*-values to 5 decimal places!!!
4. If you fiddle the axes on a graph, then don't label them.
5. Only use a nice straightforward set of data to demonstrate a new method.
6. If you can't understand the computer output, then BLUFF. No one else will know what the Durbin-Watson statistic is either!
7. Give the client the answer he or she wants. Try several approaches if necessary (e.g., with or without an outlier; parametric or nonparametric). If you still can't get the answer you want, blame (a) the computer; (b) outliers; (c) lack of homoscedasticity; (d) the weather; etc . . . .

Andrews concludes by saying that "Statistical collaborating and consulting is not a slippery path with disasters lurking at every turn, but a constructive, stimulating and satisfying activity". I heartily agree with the latter part of this statement, but my experience suggests that the earlier part is overly optimistic. There are indeed pitfalls waiting for us at every stage of a statistical investigation!

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