Letter to the Editor

YOUR REVIEWER of the Proceedings of the 1973 NATO Conference on the Role and Effectiveness of Decision Theories in Practice (Op! Res. Q. 28, 2, i, 360–361) has offended us. Needless to say, as scientific directors of the Conference and as editors of the book, we think we did a sound professional job. Some will inevitably disagree, and we do not argue with people's right to say what they think. We do, however, think that a review should be accurate in statements made or implied, and that opinions should be supported by relevant references to the material reviewed.

No attention is drawn to the date (1972–1973) of the material. Any Proceedings have to report on how a subject stands, as seen by the participants of the Conference, at a particular moment in time. This book is no exception. It is not a text-book, and its coherence and uniformity of quality cannot be judged on the same basis. Editors have a duty to report clearly and factually, with a minimum of alteration to what was written and said: we might add that, in this book, the discussions reported have been carefully summarized and, while they may contain fewer words, offer more information than most such reports. It is also not surprising that some authors have updated (slightly) their ideas, and published additionally elsewhere; they retain copyright and are free to do this, but it in no way detracts from the value of their material in the full context of the conference.

Your reviewer implies that he does not like Proceedings, and admits that he has only read the book in part. This is no excuse for ill-judged comment. His quotation from the preface is incomplete, and he thereby gives a wrong impression of what the book is about and what it claims to achieve. He is correct that the majority of papers were written by academics, but they were written for and appreciated by a very broad spectrum of people interested in application. Although he calls for first-hand experience, he does not offer any source references.

However, our main concern is that he insults all authors by not indicating what he considers to be poor material. Authors are now all suspect (rather like the peers who may or may not have been considered unworthy by the scrutinizers of a recent honours list). Your reviewer seems to have had little confidence in his opinions, since he offers no evidence and makes no specific references to the offending papers. Smears are becoming fashionable, but they are still unprofessional. We would like to express our regret that such an attack on our authors has been made in the pages of this journal, by someone who has not necessarily even read what they have written.

Our opinion is that the review is both uninformative and prejudiced. We suspect that your reviewer has little serious interest in Decision Theory, and has not been able to understand the total material put before him. We hope serious readers will do justice to both ourselves and our authors by ignoring the review and using their own judgements. They will at least find out, which they will not do from the review, what the Conference was all about.

Finally, since your reviewer thinks so badly of the book (which incidentally has had sales above the initial optimistic forecasts), we suggest that he return his review copy to the Society's office. He has certainly not earned it. But we hope that he will read it first.

D. J. WHITE
K. C. BOWEN

REPLY TO COMMENTS BY BOWEN AND WHITE

WHEN FACED with the sort of comments as those of Bowen and White, there seem to be two alternative reactions—one may maintain a (hopefully) dignified silence, or
take up the gauntlet which has been metaphorically flung down. I intend to take this latter course and respond to their criticisms, as failure to do so may, in the minds of some, imply tacit acceptance of the criticisms levelled. I would wish to leave no-one in any doubt that I find their comments both unjustified and unreasonable. Needless to say, I also think that I did a good job and I too am offended!

It is quite correct that people should have the right to say what they think and I maintain that I was doing just that in my review. It is clear that I did not think much of their book—I still do not—and I surely have the right to say so, without being accused of attacking people or casting the smears which are apparently “becoming fashionable”. If anyone has been vilified then I must claim that distinction as I have been labelled as uninformative, prejudiced, having little serious interest in Decision Theory, and not being able to understand the material put before me—to my mind, a very comprehensive set of smears, or perhaps insults would be a better description. Furthermore, I should very much like to know which of my statements (actual or implied) was inaccurate.

Let me turn to their specific criticisms. It is quite correct that the date of the material was 1972–1973 and that the papers that I had already seen were, by and large, published subsequently. However, if it is so important to have a record of how a subject stands at a particular point in time, why did the book not appear before 1976? There is no disputing the fact that by the time the book was published I had seen a number of the contributions in substantially the same form elsewhere. Anyone with an interest in Decision Theory must surely have been in a similar position and is there, therefore, any need to publish them again? I am not suggesting that these contributions should have been edited out but merely questioning whether or not it is worthwhile publishing conference proceedings at all if it takes so long to do it. After all, the better contributions can always be submitted for publication individually in journals or elsewhere, as in this case has happened.

The editors also take up my admission of not having read the book from cover to cover. (I invite readers to speculate from my comments why this was so!) In fact, I did read something like 75% of the contributions which were selected on a somewhat random basis so as to include, for example, at least one paper from each of the book’s six sections. In particular, I tended to avoid the papers which I was fairly sure that I had seen previously elsewhere. I accept that this process may well have biased my opinion slightly as it is possible to argue that those papers which find their way into print subsequent to being presented at a conference are probably the better ones by virtue of the fact. However, I should also point out that those papers which I did not read in detail were nevertheless given a cursory appraisal and I am sure that this practice is by no means uncommon with books running to several hundred pages. Furthermore, I would suggest that one is quite entitled to comment on the uneven and disappointing nature of the papers as a result of so doing. After all, it is surely possible to state that something is not uniformly good (or bad) as a result of such a sample whereas one could not come to the opposite conclusion—which is something I did not do. My comments were not ill-judged for that or any other reason.

Regarding my quotation from the preface: of course it is incomplete—perhaps I should have reproduced all three pages of it! Having quoted completely the stated aims of the conference, how can this possibly be misleading? I would suggest that the editors themselves perhaps recognize the failure to achieve the aims stated and are over-reacting as a consequence (one suspects that they “protest too much”). On the same theme, I am apparently insulting all authors by not indicating which papers I found disappointing. The reasons for this I would have thought were reasonably obvious. First, the book as a whole contains 31 papers and merely to reproduce their titles and authors would have taken almost 300 of the 400 words available for the review—in fact, to mention by name many more than just a few would have severely curtailed the scope for the more general comments which I consider to be more important. There were, in fact, so many papers that I would have wanted to comment on in detail that limi-