Editorial

'If you were stuck in a lift with someone from the following list, in which order would you *least* like their professions to be?'. This question was asked in a recent survey of 400 people; the respondents were given a list of professions to choose from. 'Nutritionist' did not feature in the list; 'biologist' was possibily the closest, coming near to the middle of the final ranking. 'Statistician', however, was included and came out in first place.

Does this result surprise you? Presumably the reason people would not want to be stuck in a lift with a statistician is because they think the job a statistician does is boring. They see statisticians as people who sit at desks (or computers) all day compiling tables of figures. In the scientific area, however, a statistician's life is not like that at all, but the role of statistics in scientific research is still widely misunderstood, even by the scientists themselves.

It occasionally falls to my lot to give an introductory training session on statistics to an intake of new science graduates. I ask them 'Why do we need statistics?'. 'To tell us whether our results are significant', or 'To confirm what we can already see from the data' are common responses. If that were all there were to it, I wouldn't want to be stuck in a lift with a statistician either.

The *British Journal of Nutrition* has for some time now had a policy of including on its Editorial Board a group of statisticians (of which I am currently one) whose function it is to help the editors ensure that published papers are statistically sound. This leads to authors having to answer questions about their experimental design and analysis, which I am sure some regard as nit-picking. But if the design is wrong, or the analysis inappropriate for the design used, the conclusions could be incorrect.

My answer to 'Why do we need statistics?' is primarily for efficiency. Any study has limited resources, be they time, money, animals, materials or any combination of these, and we need to carry out the study in such a way that we get the maximum amount of (correct) information from those resources. This involves both the design (the choice of treatments, the choice of measurements, the allocation of experimental units to treatments) and the analysis. Faulty design can lead not only to inefficient studies but also to the possibility of bias, and incorrect conclusions.

Another question I sometimes ask our new graduates, most of whom profess to be trained as experimental scientists, is 'What are the three basic principles of experimental design?' Sometimes I get one back; randomization. The second, replication, is only seldom mentioned, perhaps because the need for it is seen as self-evident. The third, blocking, is never mentioned. Many of them don't know what it is when I tell them the answer. But for increasing efficiency, blocking is perhaps the most important of the three principles, and when studies involve animals, or people, inefficient design is not only wasteful of resources; it is unethical as well.

It is my belief that a large proportion of studies carried out today could be made substantially more efficient by the proper application of statistical ideas, and most of the papers I review for the Journal serve to strengthen this belief. Of thirty-five papers I have seen in the past 2 or 3 years, I found it necessary to criticize the design in seventeen (49%) and the analysis in thirty-one (89%). Of course, the papers we see are selected by the editors as needing some statistical input. Even if we assume that the manuscripts we do not see are all statistically sound, which is unlikely, the proportion of papers with statistical

weaknesses is surprisingly large. It is not as though professional statistical help were expensive, especially when compared with the real cost of doing the experiment.

It is interesting to speculate why this state of affairs has arisen. For many experimentalists, their first contact with statistics is a short course at university, given by the lecturer who drew the short straw when the courses were assigned. Such courses typically deal with a few elementary significance tests, but contain little about design. Also, they are usually given during the first year of a 3 year degree course, when most students have no appreciation of the need for statistics in experimental work. Not surprisingly, many people get turned off statistics for good at this stage.

After the university course, a typical experimenter's next contact with statistics is the first professional consultation with a real statistician. If the statistician is inexperienced too, or just having an off day, this can be an unfruitful or even painful experience for both, and they might not try again. In the consulting room, statisticians are a bit like doctors; some will just provide the service requested (which is probably not what you really need at all), whereas others will ask questions designed to find out what you actually want to know. When you next visit a statistician, hopefully before you start the experiment, be prepared to take time to explain the background to your problem, the resource limitations, and what you are trying to find out. If there are any variables you could measure which are not of direct interest, but which might affect the measures you *are* interested in, mention them. Leave him or her time to think about the problems involved, and you should end up with a more efficient experiment as a result. Finally, show the statistician a draft of your paper before you submit it for publication.

All this, however, will not help the *British Journal of Nutrition*'s statistician when he or she next gets a paper to review from an author who has not read this editorial and who has clearly not consulted a statistician effectively (or at all). When I first started reviewing papers for the Journal, I was told not to insist on statistical perfection, because many authors would not have access to professional statistical help. All I was to do was to check that what had been done was appropriate. Often, the design and analysis are so briefly described that it is not possible to be sure exactly what has been done in any case. But papers about badly designed studies, inefficiently analysed, cause me a considerable dilemma, even if there is nothing actually wrong. In my view, experimenters, particularly those who are paid out of the public purse, have a duty to do their experiments efficiently. Ignorance of the basic principles of design and analysis ought not to be a defence. But if a poorly designed experiment has been done, tax payers' money spent and animals sacrificed, any information which can be salvaged ought to be made publicly available. To publish the study in the Journal, though, would give it respectability it does not deserve, and would encourage others to think that such studies were scientifically acceptable. When they come from research establishments where I know there are competent statisticians willing to help, if only they were asked, I feel justified in insisting that caveats are entered acknowledging weaknesses in the design, and in asking for more elaborate analyses if I think they might yield a bit more information. But what should I do if I don't know the establishment in question, or if I know there are no statisticians readily available? Are you content to read papers which still have room for improvement? Why not enliven the correspondence column with your views?

DENNIS O. CHANTER