old-world manner that was entirely natural to him and free from guile."

We can always be thankful for scholars whose influence, foresight and inventiveness are combined with such a character.

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Comment

Robert V. Hogg

I welcome the opportunity to comment on Harold Hotelling's articles on statistical education, particularly because many of us are concerned about our directions in this area, even to the point of asking seriously "Are we really doing it right?" Certainly, as statisticians, we know that we must be willing to experiment and make changes (at least small ones) in striving for optimality. Hotelling recognized this and states that "no syllabus in use today can be expected to survive a few more years of research." As new statistical methods and ideas develop, changes *must* be made even though we know that the optimum will never be achieved. However, we must continue to chase it for if we do not, our programs will dry up and fossilize.

One major issue he addresses in both articles is "What sort of persons should be appointed to teach statistics?" He makes it clear that it should be someone who has "a profound and thorough knowledge of statistical methods" and "a genuine sympathy and understanding for applications." At the university level, he emphasizes that publication of scholarly research has always been accepted as the best proof of an understanding of your field. Because a good teacher of statistics must be familiar with recent advances (even if outside his or her specialty), we need even more good expository articles today (as compared to the 1940s) written by some of the leaders in research in those areas.

To illustrate the importance of research to the teaching of statistics, I will use myself as an example (not that I am a great researcher). I like to think I know *a little* about *M*, *R* and *L* estimation, and this knowledge helps me add a little excitement to teaching a nonparametrics course, either beginning or advanced. For example, at a very early stage of an elementary course (not necessarily nonparametrics), many textbooks (including my own) suggest that $\bar{x} \pm 2s/\sqrt{n}$ serves as an approximate 95% confidence interval for the population mean μ provided the sample size n is reasonably large. Of course, if the underlying distribution is normal, there is no problem; really there is no problem if the underlying distribution is almost symmetric with reasonable tails. But suppose we have a very skewed, heavy-tailed underlying distribution. Then we have a different story and, in some cases, that confidence coefficient might be as small as 65 or 70% because $\bar{x}$ and s are then highly correlated (they are uncorrelated in symmetric cases). How many teachers of statistics really know that? In particular, at that point in the course, the students really should be informed that something else should be done in this case (e.g., transforming the data or using robust methods) even if the details cannot be explained at the level of the course.

Hotelling argues (and rightfully so) that most mathematicians will not be very good teachers of statistics; and those that are asked to do it should be given "a furlough for a year or two" to obtain proper training. That is, mathematicians *and* statisticians should experience some good applications before being asked to teach statistics. I can really speak with some experience here because I earned my PhD in mathematics in 1950. Although I wrote my thesis on a statistical topic under the direction of Allen Craig, I really knew very little in the way of statistical applications. I taught some great courses in mathematical statistics in the 50s; but is wasn't until the 60s—or even the 70s—that I truly saw the importance of some of the methods, like those in design of experiments. That is, while I knew all that theory about those quadratic forms, I really could not design a good experiment. A

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