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In This Issue

Readers of *Statistical Science* will know that D. A. Freedman is no stranger to controversy. In the very first issue (Volume 1, Number 1), an article by Freedman and W. C. Navidi expressed views about the potential shortcomings of regression models for adjusting the census that sparked some heated discussion on both sides of the question. Now, in this issue, Freedman and H. Zeisel, Professor Emeritus of Law at the University of Chicago, study the models and methods that have been used to assess the cancer risk to humans from exposure to low doses of chemicals such as the pesticide DDT. Their findings are highly controversial, and once again spark a heated discussion on both sides of the question. Freedman and Zeisel remind us that the assessment of the risk to humans must be based on two extrapolations: A quantitative extrapolation of risk from high doses in laboratory animals to low doses in humans, and a qualitative extrapolation based on the idea that chemicals that are carcinogenic in animals should be considered as carcinogenic in humans. They review the literature and find the evidence for each of these extrapolations to be unsatisfactory.

Several of the discussants disagree with the authors' evaluations and their strongly negative view of risk assessment as a science, but at the same time, as Norman Breslow writes, "Few scientists would dispute their claim that current procedures used for routine risk assessment on the basis of limited animal data lack a solid scientific foundation." J. K. Haseman feels that the authors raise no new points regarding the scientific merits of quantitative risk estimation that have not already been extensively debated. He questions the techniques of the authors and writes, "some would argue that statisticians and lawyers debating science is no more meaningful than biologists debating *p* values." Suresh H. Moolgavkar and Anup Dewanji discuss the biological basis, and the lack of such a basis, for various stochastic models that have been presented in the literature to represent the carcinogenic process. J. Kaldor and L. Tomatis are also in general agreement with the authors that "the quantitative assessment of cancer risk entails a number of biological assumptions which have not been verified empirically," but feel that Freedman and Zeisel have distorted "several important issues concerning the use of animal experiments as indicators of potential human hazard." William DuMouchel seems to sum up the view of many readers when he finds himself agreeing with the authors that much more scientific theory is needed to justify the extrapolations that have been made, but more optimistic than the authors about the potential contribution of statistics in helping to solve the mouse-to-man problem. Together, the article, the discussion, and the rejoinder provide a thought-provoking, stimulating, and, yes, controversial picture of cancer-risk assessment.

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In this issue we present the first articles in an occasional series of "Golden Oldies." In this series, we will republish classic articles from the probability and statistics literature, together with contemporary discussion of the articles and their authors. We begin the series with, not one, but *two* famous articles by Harold Hotelling: "The teaching of statistics," which was originally published in *The Annals of Mathematical Statistics* in 1940 and carried a footnote stating that it was presented as an address at the meeting of the Institute of Mathematical Statistics at Hanover, New Hampshire, September 10, 1940; and "The place of statistics in the university," which was originally published in the *Proceedings of the (First) Berkeley Symposium on Mathematical Statistics and Probability* in 1949. In these famous articles, Hotelling points out that the great increase in the development of statistical theory and the application of statistical methods has resulted in statistics courses being taught in many different departments in the university, by instructors who "are all too often not specialists in the subject." Nine discussants, leaders in the fields of statistics and economics, comment on Hotelling's insight and on the changes in the educational structure that have and, more strikingly, have not occurred since these articles were written. They give a valuable picture of how we currently teach statistics, and they write of Hotelling as a person and of their relationship with him. For example, Kenneth J. Arrow comments that as a graduate student at Columbia University he switched from the Department of Mathematics to the Department of Economics on Hotelling's advice, so he could study mathematical statistics in a "more tolerant" environment. The move was obviously successful. Arrow won the Nobel prize in Economics in 1972. We also take special pleasure in noting that one of the discussants, Harold Hotelling, Jr., is the son of the author of these classic articles, and that another discussant, W. Edwards Deming, also served as a discussant of the 1940 article when it was *originally* published.

These "Golden Oldies" and their discussion are preceded by an article on the life of Hotelling by Adrian C. Darnell, who is presently engaged in a large project examining Hotelling's role in the history of economics. Hotelling, who was born in 1895 and died in 1973, had a fascinating life. Everyone knows that he was a leading mathematical economist and statistician; it is less well-known that he received his Ph.D. from Princeton in 1924 in topology, and was then appointed as a Jùnior Associate at Stanford's Food Research Institute!

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Meta-analysis is a rapidly-growing branch of statistics that deals with the development of quantitative methods for combining the results of different research studies that are pertinent to some particular issue. In practice, a meta-analysis is often carried out by analyzing the relevant results that have been published in the scientific or professional literature. Because there is often a failure to publish studies that do not show "statistically significant" results, these published studies may not be representative of all the relevant studies that have been performed. As Robert Rosenthal of Harvard University wrote in 1979, "The extreme view of this problem, the 'file drawer problem,' is that the journals are filled with the 5% of the studies that show Type I errors, while the file drawers back at the lab are filled with the 95% of the studies that show nonsignificant (e.g., $p > 0.05$) results." In their article entitled "Selection models and the file drawer problem," Satish Iyengar and Joel B. Greenhouse develop statistical models that can be used to carry out a meta-analysis of published results that have been subjected to various types of selection bias. The discussants, who form a selected sample from the leading researchers in the areas of meta-analysis, weighted distributions and selection models, are Larry V. Hedges; Robert Rosenthal and Donald B. Rubin; Nan Laird, G. P. Patil and C. Taillie; M. J. Bayarri; C. Radhakrishna Rao; and William DuMouchel.

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This issue concludes with a conversation between two of the most influential statisticians of our time, Frederick Mosteller of Harvard University and John W. Tukey of Princeton University. In this conversation they discuss their work in statistics, both jointly and separately, from the late 1930s up to the present. This article was adapted from a videotaping that was carried out by the Committee for the Filming of Distinguished Statisticians of the American Statistical Association in May 1987 at the University of Connecticut. The videotaping was sponsored by Pfizer Central Research of Groton, Connecticut, and was moderated by Francis J. Anscombe of Yale University. For those who wish to *see* Mosteller, Tukey and Anscombe, and to *hear* their spoken words, the videotape is available from the offices of the American Statistical Association.