

## Charles Stein: The Invariant, the Direct and the "Pretentious" >>>

An interview of Charles Stein by Y.K. Leong

Charles Stein (b. 1920) is considered to be one of the most original thinkers who made fundamental contributions to probability and statistics. He has received many honors and awards and is a Member of the National Academy of Sciences (USA). He has given many invited lectures, notably as plenary speaker of the International Congress of Mathematicians, and as the Institute of Mathematical Statistics Wald Lecturer, Rietz Lecturer and Neyman Lecturer. He is now Emeritus Professor of Statistics at Stanford University and continues to be active in research in statistics.

The Editor of *Imprints* interviewed him on 26 August 2003 at the Institute when he was the guest-of-honor of the Institute's program, "Stein's Method and Applications: a program in honor of Charles Stein" held from 28 July to 31 August 2003. The following is based on an edited transcript of the interview and subsequent follow-up by electronic mail. Here he reflects on his work and expresses his views on teaching, research and statistics.

**I:** Professor Stein, thank you for agreeing to be interviewed. I'm sure it will be of great value to many people. The first question concerns the statistical work which you did for the Air Force during the War. Presumably statistics was then not yet established as a rigorous discipline. What was it like to do statistics without the benefits of its modern foundations? Was that work instrumental in leading you to think about basic questions in statistics?

**S:** First I should say that I am strongly opposed to war and to military work. Our participation in World War II was necessary in the fight against fascism and, in a way, I am ashamed that I was never close to combat. However, I have opposed all wars by the United States since then and cannot imagine any circumstances that would justify war by the United States at the present time, other than very limited defensive actions.

Statistics was already a well-developed field by 1940, going back to work of Gauss, Laplace, Galton, Karl Pearson, Student, Fisher, Neyman and Pearson, and many others. On the other hand, one can argue that statistics is not yet established as a rigorous discipline. I do not think that my work on the verification of weather forecasts had much influence on my later work.

The actual type of work I did at that time was not really instrumental in leading me to think about basic questions. However, we had a very strong group of people there, Kenneth Arrow, George Forsythe and Gil Hunt among them. Certainly discussions with them helped broaden my understanding of statistics, in particular, with Gil Hunt. Gil Hunt is a very accomplished mathematician

and I profited a great deal from his knowledge of group theory in particular although not as much as I could have.

**I:** You mentioned Gil Hunt ...

**S:** Gil Hunt is a mathematician, Kenneth Arrow is an economist and George Forsyth became a computer scientist and numerical analyst.

**I:** Did you do something on groups with him (Hunt)?

**S:** We considered the question of whether, given a statistical problem invariant under a group of transformations, there exists an invariant procedure possessing desirable properties, such as being minimal or admissible. We showed that if the group is, in an appropriate sense, composed of groups each of which is abelian or compact, there exists a minimax procedure that is invariant under that group. Realistic counter-examples came much later. The full linear group in two or more dimensions does not satisfy this condition, and in fact the conclusion does not hold. Thus, for example, it is usually inappropriate to assume automatically that a sample covariance matrix is essentially the right estimate of the population covariance matrix. The question of admissibility also came later. Unfortunately, both Hunt and I were very slow to publish, but proofs were eventually published by other people, with full acknowledgement of course.

**I:** Often, work done for one's PhD thesis shapes one's future conception of the field. Is this the case for you?

**S:** No, this is not really true of my PhD thesis, which dealt with a topic related to Wald's sequential analysis. Kenneth Arrow had lent me Wald's first work on sequential analysis and asked me whether it provided a sequential test for Student's hypothesis having power depending only on the

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Mind behind the Method

mean, and when I replied that it did not, he indicated that his was a serious shortcoming. A night or two later, I was officer of the day and had to prepare a forecast starting at about two in the morning. After finishing the forecast, I worked out most of the details of a two sample test which did have power depending only on the mean, vaguely similar to earlier work of Dodge and Romig on a different problem. This became my PhD thesis. Neyman was impressed, because George Dantzig, who was a student of his, had proved rigorously the intuitively obvious fact that no single-sample test can accomplish this. However, I think of this work as relatively unimportant, and it did not have much effect on my later work.

However, Wald did have a strong effect on my work. My work with Hunt grew out of Hunt's remark that, in my attempt at generalizing a result of Wald, I was essentially doing group theory, and all of my work on statistics has been in the framework of statistical decision theory, developed by Wald following ideas of Neyman and Pearson, and also von Neumann and Morgenstern. Wald also encouraged me to work on mathematical statistics while I was in the Army Air Force, and to come to Columbia after the war.

**I:** If you were asked to list your three most important contributions to statistics and mathematics, what would you list?

**S:** Certainly the work that is most important is what is called Stein's Method, which was developed further by Louis Chen, whose work inspired much other related work. The second most important is my work on invariant problems, which started with my discussions with Hunt, and continued with my paper in the third Berkeley Symposium on the inadmissibility of the usual estimate of the mean of a multivariate normal distribution in three or more dimensions, and the paper with James in the Fourth Berkeley Symposium, which studied a reasonably good estimate for this problem. Efron and Morris also proposed an important improvement on this estimate. Others, including many of our students, such as Eaton and Loh, did important work involving unknown covariance matrices.

Most people seem to think that my other paper in the Third Berkeley Symposium is my third most important contribution. In response to the problem of estimating the median of an unknown symmetric distribution, Vernon Johns and others proposed a sensible solution, an "adaptive" symmetric average of order statistics. In response to the same problem, I wrote a rather pretentious paper in that symposium, which tried to develop a general theory applicable to this problem. I got nearly everything wrong but the paper is believed to have had considerable

influence on the development of semiparametric statistical methods."

**I:** I think you are modest about that. You mentioned Stein's Method as your most important contribution. Many people would like to know what led you into formulating the method that is now known as Stein's Method?

**S:** Well, Persi Diaconis has already touched on that in his lecture. Briefly, it is that I was teaching a course on non-parametric statistics and I decided to prove what is called the combinatorial Central Limit Theorem. I could have presented the published work of Wald and Wolfowitz and Hoeffding but instead I decided to try my own approach. That involved the idea of exchangeable pairs to approximate the characteristic function. After a while I realized that there was nothing special about the complex exponentials. I introduced an arbitrary function and thereby avoided the need to invert the characteristic function.

**I:** You were looking at it in a different way. Most people would just teach it in the standard way from the books or papers. Not many people would actually try to find a novel way of looking at it. It is quite well-known that teaching does contribute to research, isn't it?

**S:** Many people, including notably David Blackwell, have mentioned that teaching is the most important stimulant to their research.

**I:** Did you foresee the wide applications that your Method brought to other fields some fifteen years after it was introduced?

**S:** No, but I guess that I always thought it ought to have applications. I never really pursued it.

**I:** I understand that you do not have a personal pressure to publish. Don't you think it would be a loss to the community if you don't publish the results you have?

**S:** I have always had great difficulty writing things up and also difficulty in forcing myself to submit something even after it is written. I suppose this has slowed progress somewhat in a few cases, and I regret it.

**I:** Do you have many graduate students?

**S:** No, I had about ten graduate students personally. Louis Chen and Wei-Liem Loh are among them.

**I:** Your students will invariably pick up your ideas and extend them. In some sense, they are doing what you are doing.

**S:** Yes.

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**I:** In research, which is more important: conceptual foundations or technical perfection?

**S:** That is a hard question to answer. I find it hard to get all the details right, and yet that is important. But I am stronger on the conceptual aspects than on the technical aspects.

**I:** Would you say that your approach to a problem is intuitive?

**S:** To some extent. I would look at problems which are not always very well clarified at first, and then I go on to clarify them, I hope.

**I:** What is your view on breadth versus depth in research? For example, some people work deeply on a small topic while some people have a wider interest.

**S:** I must say that I have not solved a wide range of problems though the problems I studied are formulated over a broad range, like the invariance problems and the question of using direct elementary methods rather than complex variable methods in probability theory. Of course this was not new, going back at least to Lindeberg in the modern period, but elementary methods had become unfashionable and my approach is much more widely applicable.

**I:** The next question is about the explosion in information and knowledge that we are faced with nowadays. Is it necessary to keep up with this explosion in information and knowledge?

**S:** For young people it is important and, to some extent, it may be possible for them. At my age it is not important. I am incapable of keeping up with, for example, the Fermat conjecture. I have tried reading books on it but have not made much progress in understanding it.

Everyone is exposed to some aspects of computational theory and practice, but that is more important for young people.

**I:** Do you use the computer in your work?

**S:** I have done some computing although my colleagues tend to discourage me. In a recent work on simulation, my co-authors did most of the computing.

**I:** I suppose you give them the ideas and they do the computing.

**S:** They may be stronger in computing than I am but they are also strong theoretically.

**I:** Could you give us your projections for statistics in this new century?

**S:** Not really. Clearly the field continues to develop. The computational aspects will be perhaps even more important than they are today. There will probably be a mixture of good work and bad work. There will be a lot of statistical packages to enable people to solve problems, often without understanding them. But I am unable to anticipate the directions of the important changes.

**I:** What do you think of the importance of statistics in computational biology?

**S:** Statistics is, of course, important and people are interested in applying it.

**I:** In some sense, what you have done plays an important role in computational biology.

**S:** I have not followed the field of computational biology enough to know whether my ideas have really been useful.

**I:** You mentioned the computational aspects of statistics. So we are going to be faced with a lot of data generated by computational statistics and then the theory has to keep up with it. We seem to be like in physics where there is more information than theory. Do you think there will be a revolution in statistical theory?

**S:** I cannot anticipate that. There is one big question on which I am not really competent, and that is the extent to which elaborate models are useful in applied statistics. Elaborate models can give an impression of providing results that are in fact not justified whereas with simpler models, it is more apparent if the results are not justified. The elaborate models are like black boxes which are supposed to give you the answers. And you do not know if anything goes wrong.

**I:** Do you find it surprising that statistics can do so many things and solve many problems in real life?

**S:** No, it is not clear that it does. It certainly plays an important role but one should not put too much confidence in this claim.

**I:** But statistics takes the guess work out of solving problems. In the old days, you did not know what is going on and you did it by trial and error. Now statistics gives you a way of doing things.

**S:** It gives you a way of thinking about things, but you may not come out with correct conclusions.

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**I:** You have given us a very good view of your philosophy of statistics and other subjects. We would like to thank you for the time you have given us.

