

# George Box: a model statistician

George Box is the Grand Old Man of Anglo-American statistics. **Ron Wasserstein** heard him deliver an anniversary speech.

"Essentially all models are wrong, but some are useful." The quotation comes from George Box, one of the great statistical minds of the 20th century. He is a particularly apposite choice for this joint issue of *Significance*: British-born, and discovering statistics through the emergencies of war, he moved to the United States in 1960 to found the Statistics Department of the University of Wisconsin. He was also the 73rd, and is the oldest living, President of the American Statistical Association. He has been described as the consummate "Renaissance man" who has made significant and enduring contributions in quality control and allied arts and sciences. He is also genial, humorous – and modest. He calls himself an "accidental statistician". The post-war medal he mentions below was for the sheer quality of the experiments he designed, self-taught, for the army; the book he describes "helping with" at ICI instantly became the classic text on experimental design. He was originally asked simply to proofread it, but made so many changes and improvements that he ended up as co-author. Now aged 92, George Box still lives in Madison, Wisconsin. In June this year I went there to hear him deliver the opening address of the 50th anniversary celebrations of the department he founded. Here is the speech that he gave. It is George Box on George Box.

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I want to tell you how I got to be a statistician. I was, of course, born in England, and in 1939 I was 19 years old. I had been shooting my mouth off throughout my teens about the British government and the fact that they were doing nothing to stop Hitler.

So when war broke out in September of that year, although I was close to getting a degree in Chemistry, I abandoned that and joined the Army. They put me in the Engineers (and when I see a bridge I still catch myself calculating where I would put the charges to blow it up). Before I could actually do any of that I was moved to a highly secret experimental station in the South of England. At the time the Germans were bombing London every night and our job was to help to find out what to do if, one night, they used poisonous gas.

Some of England's best scientists were there. There were a lot of experiments with small animals, and I was a lab assistant making biochemical determinations. My boss was a professor of physiology dressed up as a colonel, and I was dressed up as a staff sergeant.

The results I was getting were very variable and I told my colonel that what we really needed was a statistician.

He said "We can't get one, what do you know about it?" I said "Nothing, I once tried to read a book about it by someone called R. A. Fisher but I didn't understand it". He said "You've read the book so you'd better do it", so I said, "Yes, sir". I asked the Army for some literature about statistics and they duly sent me a number of useful books.

In the next three to four years I designed and analysed hundreds of experiments of many different kinds. In my list of published papers the first two described some of that work.

At one point I was having trouble with a statistical problem. A very senior scientist suggested that I contact R. A. Fisher, who asked me to come and see him. The Army did not know how to send a sergeant to see a

professor, so they made a railway warrant that said I was taking a horse to Cambridge.

When I got there, it was a beautiful day. Fisher said "Let's go and sit under that tree in the orchard. I'll look up the probits and you look up the reciprocals." The specific problem was soon solved and set me thinking about estimating data transformations.

As the War was winding down, it was discovered that the Germans had developed nerve gases: Tabun and other agents that were orders of magnitude more toxic than anything we knew. So I was part of a team that went to study these at their research station in northern Germany. We must have seemed a strange lot. About 50 trucks filled with lab equipment slowly making their way through the ruins of

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Belgium and Germany, manned by all kinds of people: some civilians, some in Army, Navy and Air Force uniforms. I helped to design the field trials.

When I finally got my discharge from the Army, they gave me a medal and paid for me to attend University College London, to study statistics under Professor E. S. Pearson. It took me 18 months to finish my undergraduate degree. I spent the rest of the time doing graduate work.

While I was at University College, my vacations were spent working for a large chemical company, Imperial Chemical Industries (ICI). I helped them to write a book, *Statistical Methods in Research and Production*, edited by O. L. Davies, and they offered me a job and paid my salary for a year of graduate work.

The next eight years were some of the happiest in my life. My division of ICI made, among other things, dyestuffs, synthetic textiles, and waterproofing and mothproofing agents. An expert group of chemists and engineers developed and improved the complicated processes needed. I quickly got myself involved with them and with their experiments, both on the full scale and in the lab. Typically a 1% increase in the yield could give huge profits. To help design effective experiments I had to know all about the processes. I found myself climbing up and down ladders talking and arguing every day with process workers and technical staff.

I enjoyed this, and had no thought of academia, but in the course of solving practical problems, I had come up with a number of ideas for the development of statistical methods and I had written them up and published them.

In 1952 I was surprised to receive a letter from the University of North Carolina at Raleigh: an invitation to come for a year as a "visiting professor". The ICI board of directors gave me a year's leave of absence. They sent me over on the *Queen Mary*, but they wanted me back afterwards. I had a wonderful year at Raleigh where I met Stu Hunter, then a graduate student. We worked on response surface methods.

After that I went back to England and worked for ICI for three more years.

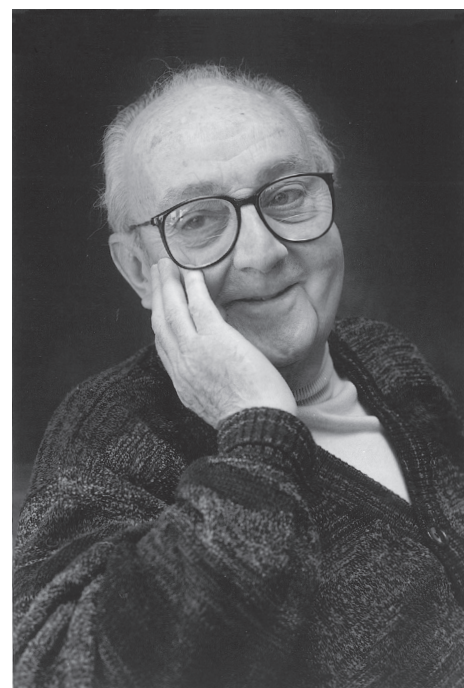
In 1956, John Tukey was calling me from Bell Labs every morning. He wanted me to come to Princeton to be the director of the Statistical Techniques Research Group (STRG) that was being set up. Finally, that year, I went to Princeton and initiated the group with Stu Hunter, Don Behnken, Collin Mallows, Geoff Watson, Henry Scheffé, Mervin Muller, Norman Draper, and others. This group did some excellent research with many publications. That was where I first met Gwilym Jenkins.

We believed that really new ideas in statistics were most likely to come from careful study of specific scientific problems. One idea we had was to design and build an automatic optimiser [a technique for adjusting inputs to pursue and find a possibly moving maximum in an industrial process – ed.], but the faculty at the Chemical Engineering Department at Princeton were not interested.

In 1960 Wisconsin asked me to come and give two seminars: one technical and one on how, if given the opportunity, I would set up a new statistics department. I told them about my ideas and eventually they told me to come and do it.

So I got the department started in the fall of that year in a Nissen hut near the lake. It used to flood from time to time and we would have books floating around the floor.

The distinguished chemical engineer, Olaf Hougen, at Wisconsin, was enthusiastic about our automatic optimisation idea. We got some money from the National Science Foundation. And after three years and many setbacks, we built the optimiser – and it worked. This was where Gwilym Jenkins and I gained the experience in the use of non-stationary models, dynamics and non-linear estimation we needed to write the book, *Time Series Analysis Forecasting and Control*, now in its fourth edition.



The math department was keen to get rid of all its statistics courses, so I found myself teaching what became the "Advanced Theory of Statistics". I had seven students, three of whom were Bill Hunter, George Tiao and Sam Wu. I remember that George Tiao was my bell-wether. Whenever he looked worried, I looked at the blackboard to see what I had done wrong.

Early on it seemed to me that students were learning a great deal about statistical theory but very little about how to use it. So I instituted what came to be called the "Monday night beer session". This was not an official course. It happened in my house and there were no course requirements or grades or anything. You came if you felt like it. Students and faculty were welcomed from all departments. So we had graduate students and often faculty from Statistics, Engineering, and the Business and Medical schools, among others. We also had talent scouts looking for people who had problems they wanted to discuss. Typically in 20 minutes or so the problem was presented and then there was a general discussion about how it might be solved. Decades afterwards, from one-time graduates, I continually hear "the thing I remember best and most helpful was the Monday night beer session". I believe people learned how to solve problems there.

I have been most fortunate in the friendships and all the kindnesses and support I have received from so many different people throughout my life. And to all of them I would like to say "Thank you".