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A Conversation with John Nelder

Stephen Senn

Abstract. John Ashworth Nelder was born in 1924 in Dulverton, Somerset, England. He received his secondary education in nearby Tiverton at Blundell's, a "public" [that is to say, privately funded] school that he attended as a day pupil. In 1942, he entered Sidney Sussex College, Cambridge University, to read mathematics. His studies were interrupted after one year by war service and he trained as an RAF navigator in South Africa. He returned to Cambridge in 1946 and completed his studies, graduating a "wrangler" [the Cambridge University term for one who graduates with a first in mathematics] in 1948. He stayed on at Cambridge for a further year and completed the diploma in statistics in 1949.

In 1950, he was appointed head of the statistics section at the National Vegetable Research Station at Wellesbourne. In 1955, he married Mary Hawkes. They have a son and daughter. He spent one year (1965–1966) on leave of absence from Wellesbourne in Adelaide as a research fellow at the Waite Institute. During this time, he started to develop the computer program, Genstat, which incorporated ideas of "general balance" that he had developed for the design and analysis of experiments. In 1968, John succeeded Frank Yates as head of statistics at Rothamsted Experimental Station, Harpenden. At Rothamsted, developing ideas he had earlier published in *Biometrics*, he started a collaboration with Wedderburn that resulted in a paper on generalized linear models that was to revolutionize statistical analysis. This modeling approach formed the sole *raison d'être* of the statistical package GLIM and is now incorporated in other major statistical packages such as Genstat, Splus and SAS. During his time at Rothamsted, he was appointed as a visiting professor at Imperial College London (1972), which led to his collaboration with Peter McCullagh in writing a book, *Generalized Linear Models*. Since his retirement in 1984, he has continued as a visiting professor in the Department of Mathematics at Imperial. He is a familiar figure at Harpenden Station, waiting to catch the train to London.

John Nelder has received many honors for his statistical work. He was awarded the Guy Medal in Silver of the Royal Statistical Society in 1977 and elected a Fellow of the Royal Society in 1981. He has served both the International Biometrics Society (1978–1979) and the Royal Statistical Society (1985–1986) as president. In 1981, the Université Paul Sabatier, Toulouse, granted him an honorary D.Sc. He has published two books and over a hundred papers, but these numbers do not do justice to his influence on statistical modeling, which is enormous. A hallmark of his work is the way that statistical and mathematical insight is combined with deep numerical, algorithmic and computational understanding to forge original analytic tools

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of great generality and high applicability. He is unusual among theoreticians in the practical interest he has shown in planning experiments and analyzing data. Since his retirement, he has continued to be active in research and has started a collaboration on hierarchical generalized linear models with Youngjo Lee. In this work, he hopes to do for random-effect models of the exponential class what he has already achieved for fixed-effect models.

The following conversation took place at John and Mary Nelder's home in Redbourn after dinner on Friday, November 17, 2000. Redbourn is a village in Hertfordshire near Harpenden, a small town in which Rothamsted is situated. John Nelder and Stephen Senn often take the same train into London from Harpenden and this conversation is the result of many others that have been held during these journeys.

EARLY YEARS

Senn: Tell me about your family background.

Nelder: I was born in a hotel in West Somerset called the Carnarvon Arms, which had been in the family for three generations. My great-grandfather was made the first publican for the hotel when the railway was put through from Taunton to Barnstaple. His son, my grandfather, bought the property from the local landowner and developed it greatly as a hotel. My father took it on, and I was born there in 1924. I have one brother who is 2 years younger than I am. Neither of us wanted to take on the hotel so it did not survive into a fourth generation.

Senn: Were there any mathematicians in your family?

Nelder: Not specifically. My father was a very good arithmetician in the sense that in the days when we had pounds, shillings and pence, where you had to carry 12 from pence to shillings and 20 from shillings to pounds, he could add the three columns in parallel, something which I never learned to do; but he was in no sense a mathematician otherwise.

Senn: My recollection of Dulverton is that it is on the edge of Exmoor, which is a tract of relatively open country in Britain. What was it like growing up on the edge of Exmoor?

Nelder: I am doubtful whether you ought to describe your childhood as idyllic, because everybody would like to think that perhaps theirs was, but I find it very difficult to imagine a better place to grow up in. The hotel had a large garden; it also had an attached farm with quite a lot of farmland that went down to the river Barle. We swam in the river. We flooded a piece of a field in the winter in case there was enough ice to skate on, which was not very often. I went everywhere on my bicycle, up to the edge of the Moor, and into the woodlands that bordered the rivers Exe and Barle. I think it's hard to imagine a nicer place with no restrictions on where we could go. We collected plants and mounted them in books. I loved the long names of the families—Caryophyllaceae, Scrophulariaceae and so on. We collected birds' eggs, something that, of course, would be absolutely forbidden today. We collected butterflies. We learned a lot about natural history from what we simply discovered in our movements around.

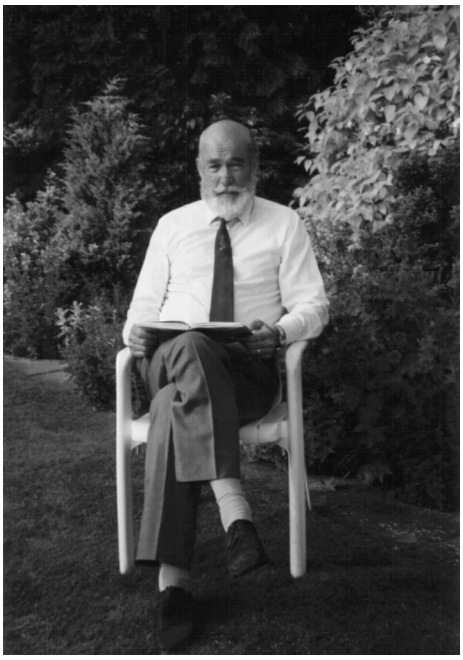

FIG. 1. *John Nelder in the garden of his house in Redbourn.*

Senn: So is this where your interest in bird watching started?

Nelder: Yes. I think I was always more interested in birds than I was in flowers or butterflies. I kept detailed records of nests that I found throughout the year. I learned a lot about clutch size and nest sites, etc. and the interest has continued throughout my life.

Senn: I know that you have applied statistics to ornithology. Tell me about that.

Nelder: The great ornithological work of the prewar years was the *Handbook of British Birds* and in it were detailed all the individual records of the great rarities that had occurred. I was always interested in the fact that huge numbers of rarities occurred in the neighborhood of Hastings, a port in southeast England, over the period about 1900 to 1920. I began to wonder whether these were genuine or not, as did an ornithologist who was doing his national service in the army locally. We started extracting all these records and comparing them with records of a different area, but the same period, and then continued comparing the same two areas in the following period. It became rapidly clear that this particular combination of the Hastings area and the 1900–1920 era were totally different from the other three combinations, which agreed extraordinarily well among themselves. We investigated the occurrence of multiple records and the time of the year in which they occurred, and this simply made this particular area and time stand out. I then found that some ornithologists were also interested in this and they had investigated how these records had appeared and they concluded that a local taxidermist was possibly the chief villain. Then it was found that he could have imported these birds on ice from ships and sold them off as British records to people like Lord Rothschild for very large sums. He probably made about £7000 out of this, which in 1910 was something worth having. So two papers were published, mine saying the records do not agree with anything in the neighborhood and the following periods, and theirs describing how it might have been done. Some of the locals were incensed about this. One of them put up a statistician who claimed that I was a Bayesian who had tried to assign a probability that these results were false, which, in fact, I hadn't done at all. We had to write another article explaining what the argument was. All the doubtful records were subsequently deleted from the British list. This occurred at a time when another man had just finished his five-volume *Birds of the British Isles* in which he had accepted all these records as genuine.

Senn: You made yourself rather unpopular with certain members of the ornithology fraternity.

Nelder: I think that is true of the locals, but others were delighted that we had exposed this because they had believed for some time that these records were fraudulent.

SCHOOL AND CAMBRIDGE: ROUND 1

Senn: Your interest in music also started young. Was this something you were pushed into?

Nelder: No. On the contrary, I am told that at age five and a half I demanded to learn the piano. I was sent to a local teacher in Dulverton and went on from there. I think my parents were incredibly long-suffering, hearing me for long periods in the evenings in the family sitting room bashing my way through anything I could lay hands on, including the Beethoven sonatas. It started young and it has stayed with me to this day.

Senn: I believe you also won a gold medal for music when you were a youngster. What did that involve?

Nelder: The Royal Academy of Music had eight grades. For their exams up to grade 5, you just went ahead. At that point, you had to take a theoretical exam in harmony. Then you were allowed to go to 6, 7 and 8. For those latter grades, they gave a gold medal for people who won the highest marks in the country and I won for grade 6 with 146 out of 150. I do not think I really deserved it, but at any rate I got one of the gold medals and I had to go to St. James Palace, where it was presented to me by the then Duke of Kent.

Senn: Where did you go to school?

Nelder: Initially, I was taught by my mother. She had been trained as a Froebel teacher and ran a small nursery school for my brother and myself and three or four local children of both sexes. [Froebel 1782–1852 was an educational theorist who founded the kindergarten and influenced Montessori and the nursery school movement.] Then I went to a private primary school, where I did not get on very well. At one point, she withdrew me and had me coached privately. Then I went back again and I won a scholarship to Blundell's school, which was a public—Americans would say private—school in Tiverton where it was possible in those days to go as a day boy for the princely sum of £11 (equivalent to $16 at the current exchange rate) per term and that's where I did my main schooling.

Senn: So what subjects did you enjoy at school?

Nelder: I always enjoyed maths, of course. I enjoyed geography, but this was partly because we had a quite outstanding geography teacher. I was interested in languages, but not tremendously so, and the sciences were not very well taught but I still enjoyed them. I remember to this day that when we were given an experiment to measure the specific heat of copper, we were then given the "right answer." Nobody ever

explained to us why that figure was the right answer and ours was wrong, and thus a great opportunity was lost to introduce the ideas of error and bias into the work that we did.

Senn: Which subjects did you not enjoy?

Nelder: I was never mad keen on Latin, I must say, but then in those days you had to keep Latin up to take your entrance exam to Cambridge, where they gave you an extremely difficult and complicated translation passage (which I thought I did nothing on at all). In general, I enjoyed school. The things I did not like were very often simply attached to the teacher rather than the subject. If the teacher was boring, I found the subject boring. I was taught pure maths very much better than I was taught applied maths.

Senn: So what made you apply to Cambridge?

Nelder: This was entirely the result of people who came to stay at the hotel. These included the senior tutor and the bursar of one of the Cambridge colleges. They used to play bridge with my parents, and that's how we made contact with Cambridge. I was the first person in my male line to go to university and it came about in that way. Also, my school had a private (closed) scholarship to, Sidney Sussex College. Eventually I went to Sidney Sussex, although I did not get a scholarship.

Senn: So, just to get some dates straight here, when did you go up to Cambridge the first time?

Nelder: I went up in 1942 for one year in the middle of the war. You were allowed to study provided you did your service training, which I did in the air squadron. Most afternoons you did that and you had lectures in the morning. We took part 1 of the mathematics tripos at the end of that year.

Senn: What sort of things were on the syllabus?

Nelder: Mechanics, electricity and magnetism, analysis, matrix algebra, geometry. We learned things like projective geometry that, as far as I can see, nobody learns at all nowadays.

Senn: Do you remember any good teachers in particular from that time?

Nelder: Yes, but I am inclined to forget in which years they occurred. I was taught by R. A. Lyttleton, the famous astronomer. I did supervision with Shaun Wylie, who was one of the people who decoded stuff during the war. ["Supervision" is the Cambridge University term for teaching through small group tutorials and is organized on a college rather than university basis.] There is a story that he actually managed to read a paper in Russian by looking at the mathematics and decoding the text from scratch (without, presumably, a dictionary). I went to lectures by Fred Hoyle and Herman Bondi and a wonderful Russian called Besicovitch [Abram Samoilovitch 1891–1970], an expert on almost-periodic functions, whose language was always somewhat Russian. He would pose a nasty question. He would get half of the class to say that the answer was yes and the other half to say that it was no. Then he would look up and say: "Is it or is it not?" and this, I suppose, is a very Socratic manner of teaching. If you wanted to learn Hardy, which is what he was supposed to be lecturing on, you had to read the book in your spare time because most of his examples were taken from his current research. I don't think this was a bad thing.

FIG. 2. *John and Mary Nelder's house in Redbourn.*

Senn: Did you find that your musical interests flourished at Cambridge?

Nelder: Oh yes, because I joined the music club and I performed there. There were practice rooms and I learned to play chamber music. I didn't join the choir, but I used to go to their concerts.

Senn: And what sort of other things were you involved in socially at Cambridge?

Nelder: I went to a Scottish country dance club, although not being in the slightest bit Scottish, and I used to go to Mathematical Society talks. I remember one by Littlewood on the probability of a snowball's chance in hell and sundry things like that. I never found any lack of things to do. I belonged to the Bird Club of course. The great place to go birding was the sewage farm, which was an old-fashioned one with big settling tanks and many interesting migrants [that is to say, birds!] came through there.

Senn: Was there a lot of disruption during the war?

Nelder: Well, it was untypical in the sense that we had this large element of service training. There wasn't

a lot of food. The local café had a few cakes and buns for sale but unless you were there at 10:30 in the morning you had no chance of getting any of them. Thus to have a lecture at 10 meant that you did not get any that day. We survived and put up with it.

THE WAR AND CAMBRIDGE: ROUND 2

Senn: At what stage in your studies were you when you left Cambridge to join the armed forces?

Nelder: I had just done one year of the three years of the maths Tripos [The Cambridge degree]. I joined the RAF in July 1943 and I was sent out to South Africa to train as a navigator. I spent time in several stations along the south coast, East London, Port Elizabeth, ending up in Cape Town and then getting a boat home. The journey out was memorable. We went in a convoy. It took us 19 days to get to Gibraltar! It took us 6 weeks to get to Durban and it was the only stage in my life when I have ever been hungry. I lost about 20 pounds on the voyage. When we got to South Africa we ate enormously and we were all extremely ill.

Senn: Did you manage to use your mathematical knowledge at all in South Africa?

Nelder: Yes, in the sense that I knew enough to give lectures to people on how to navigate to find ships moving on the ocean. I was able to do that. I took some of my maths textbooks with me and read them on the troop ship (mostly geometry, as I remember). I found navigation very interesting. There was, of course, no instant latitude–longitude. You took sextant measurements on the stars and the moon, you took radio directions and you did your best to join these up and find your position.

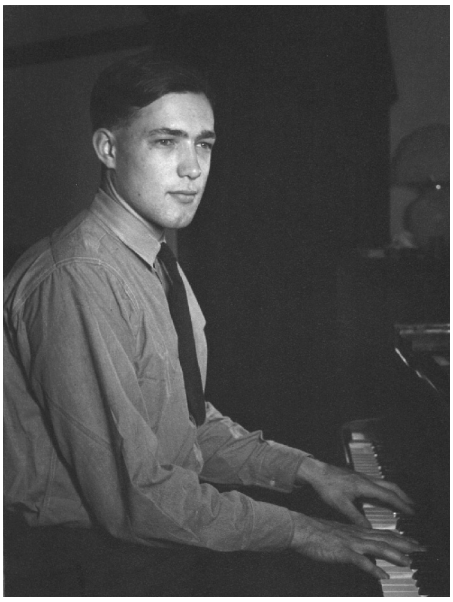

FIG. 3. *John Nelder at the piano during war service in South Africa.*

Senn: So when did you return to Cambridge?

Nelder: I was de-mobbed in 1946 in time to go back to Cambridge. It was a great advantage to have done that previous year at Cambridge because I got my de-mob much earlier than I would have if I had not.

Senn: Did you find that Cambridge had changed on your return?

Nelder: It had changed enormously in the sense that the place was full of naval commanders and lieutenant colonels who had come back from the services to finish their degrees. Hence, it was quite unlike, I suppose, almost any other university generation of the century.

Senn: By that time, Fisher must have been there. Did you attend any lectures by him when you were at Cambridge?

Nelder: Yes, I went to a seminar of his where he dealt with the tuberculin data (Fisher, 1949). Later Peter McCullagh and I used these in our book on generalized linear models. I found Fisher's seminar very difficult. I wondered at the end why he didn't take logarithms. Many years later, Chester Bliss at a Biometric Society conference at Cambridge did exactly what I had wondered about and got almost exactly the same answers as Fisher. I don't remember attending any other talks by Fisher because, of course, he was professor of genetics, and I wasn't studying genetics.

Senn: About 10 years ago, Henry Bennett edited Fisher's letters to statisticians. What must be one of the rudest letters in the whole collection is one which Fisher wrote to you (Bennett, 1990, pages 282–283). What exactly was the subject of Fisher's anger in that case?

Nelder: I could not understand an extraordinary result in the end, I think, of *The Design of Experiments* (Fisher, 1935, page 244), which contains the expression $(n+1)/(n+3)s^2$. This had to do with the loss of information from estimating the variance. I pointed out to him that there were two integrations involved in the production of this expression and that if you did them in the opposite order you got a different answer. I asked why one was right and the other one was wrong. This was typically me, I suppose. I always say what I think. That was what provoked his ire. I have since then asked a number of people about this issue and nobody has ever been able to explain to me why the order of integration that he used was in a sense the right one and the other one was the wrong one.

Senn: What about Harold Jeffreys? He must have been at Cambridge also.

Nelder: I went to his lectures and they were the most infuriating ones I think I ever attended. Cambridge lecturing standards were not good at the best of times. Jeffreys spoke rather softly, so that only those in the first three or four rows could hear him well. When he came to the punch line of a theorem, he would turn and face the blackboard and then nobody could hear him. This was so frustrating that most people dropped out by the end of about the fourth lecture. This left a small nucleus who felt they had to go on to the end. I am afraid that I was one of the dropouts. I decided to read Jeffreys' book instead.

Senn: You stayed on for a further year after you graduated to do the diploma in statistics. What was the reason for that?

Nelder: I wanted a job that would connect mathematics and either biology or natural history. In those days, it certainly looked as though statistics was that connecting link, so I decided to stay on for the year and do the diploma in statistics.

Senn: Who were your teachers on the diploma?

Nelder: Wishart was there, Henry Daniels, who has recently died, Frank Anscombe and Lindley too, although Lindley did not lecture to me. It was a very interesting period in the history of the Cambridge Statistical Laboratory, as it then was.

WELLESBOURNE, GENERAL BALANCE AND GENSTAT

Senn: Your first employment was at the National Vegetable Research Station in Wellesbourne. How did you come to get the job at Wellesbourne?

Nelder: The director was Jim Philp, who was appointed to build the station from scratch. He came to Cambridge and interviewed me for the job of statistician and I got the job. There was a site for the station, but there were only some ex-army huts at the end of a muddy lane into a field. There was no electricity. I had a splendid Monroe electric calculating machine but nothing to run it from. We were indeed starting from scratch!

Senn: I believe that you also had some initial training in Versailles and Rothamsted. What was that?

Nelder: Well, because Wellesbourne did not exist, I was sent off to Rothamsted to learn some statistics from Yates and his department. At that time, there was an exchange scheme with the French. Yates said, "Nelder has got some time to spare so I will send him." They sent me to a plant breeding station at Versailles for about six months where I learned quite a lot of genetics. In turn, I taught them, I hope, a bit of statistics (because there wasn't much in France in those days). Then I came back to Rothamsted. I moved to Wellesbourne when there was somewhere for me to work.

Senn: Did you make any statistical acquaintances that you regarded as being important at either Versailles or Rothamsted?

Nelder: Certainly at Rothamsted: people like Michael Healy, Mike Grundy, George Dyke and others, many of whom I have kept in touch with ever since. Versailles less so because it was, as I said, primarily a plant breeding station rather than any kind of statistical center.

Senn: When you got to Wellesbourne, you were the first statistician there, but you must have appointed a number during the time that you were there. Who do you regard as being your principal collaborators?

Nelder: The first person we appointed was Roger Mead, and he and I wrote a paper on the simplex minimization algorithm, now called the polytope algorithm (Nelder and Mead, 1965). Then we appointed a third statistician, Geoffrey Berry, who is now in Australia and well known in medical circles. The department developed from that base.

Senn: You must have met your wife Mary during the time you were at Wellesbourne.

Nelder: I did. She was brought up in Stratford-on-Avon and was keen on birds. She had a postcard from the chairman or secretary of the Bird Club saying "there is a new man come to Stratford called John Nelder, and I think you ought to meet," so we did.

Senn: I found it rather surprising when I looked back at your early papers that there seem to be a good many on genetics. Was that a particular interest at the time?

Nelder: I suppose it was, and it may have arisen partly from the work at Versailles. I used to be an avid reader of *Heredity,* which was edited by Darlington and Mather at the time, and I published a few things in there. It was obvious there was a lot of scope for mathematics, particularly in biometrical genetics.

Senn: I notice you also have an early publication with John Hammersley (Nelder and Hammersley, 1955). What was that about?

Nelder: He and I used to go together to supervision in Cambridge and he knew far more mathematics than I did. I came upon a problem that I wanted to solve by simulation. I wanted to generate samples with a certain spatial covariance structure and I had an idea

of throwing circles at random on a plane with different frequencies and radii to achieve this. I gave this to Hammersley who really did all the mathematics for me, including the use of Schlömilch's lemma (which I think you will find in Whitaker and Watson and which was entirely new to me). Nowadays, of course, what we would do is invert a large matrix. Looking back it's interesting to see how things have changed.

Senn: Tell me about "general balance." That must have been something you developed during your time at Wellesbourne.

Nelder: During my first employment at Rothamsted, I was given the job of analyzing some relatively complex structured experiments on trace elements. There were crossed and nested classifications with confounding and all the rest of it, and I could produce analyses of variance for these designs. I then began to wonder how I knew what the proper analyses were and I thought that there must be some general principles that would allow one to deduce the form of the analysis from the structure of the design. The idea went underground for about 10 years. I finally resurrected it and constructed the theory of generally balanced designs, which took in virtually all the work of Fisher and Yates and Finney and put them into a single framework so that any design could be described in terms of two formulas. The first was for the block structure, which was the structure of the experimental units before you inserted the treatments. The second was the treatment structure—the treatments that were put on these units. The specification was completed by the data matrix showing which treatments went on to which unit. I published two papers in 1965, in the *Proceedings of the Royal Society of London*, at the end of which I postulated that it should be possible to write a general computer program, which, given these two structures, could deduce the form of the analysis of variance, the number of different kinds of contrasts of treatments in terms of their standard errors and so on.

Senn: Is this actually incorporated into Genstat, this particular approach of yours?

Nelder: Yes. As far as I know, Genstat is the only statistical package that does all these analyses by this single algorithm.

Senn: I must confess I have been very impressed, as a relatively recent Genstat user, by the way in which you are invited to declare the blocking structure and the treatment structure.

Nelder: Yes, this approach is almost unknown in the U.S., for example. It does seem to me to synthesize a lot of apparently different things and to put them into a single framework, which is what I'm usually trying to do.

Senn: But do you think that some of your own later work on modeling has reduced the primacy, if you like, of the randomization approach to inference and designed experiments and has perhaps moved analysis further away from this.

Nelder: I think it is perhaps of its time. Of course, nowadays one has spatial covariance structures and all the rest of it. Nevertheless, this approach has its uses—in particular when one is analyzing experiments from many sites. The variation between sites is usually much more important than the variation within sites, so that getting the last 10% of extra information from within a site is relatively unimportant given the variation between sites. To that extent, I think my general balance algorithm still has a part to play.

Senn: I think you have an example of a famous experiment where you yourself were surprised by the particular results that came out of applying your general balance algorithm. Can you tell us something about that?

Nelder: Yes. I went to Adelaide at the invitation of Graham Wilkinson, specifically to work on the general balance algorithm. He did most of the actual implementation of this. One of the first examples that came along was an experiment with a Greco–Latin square that had balanced incomplete blocks in its cells. I thought the design was a misplaced piece of ingenuity. Anyway, we gave this design to the algorithm, which produced correctly the analysis of variance, and in one case one of the interactions came out in a higher stratum of error than the main effects that contributed to it. If you had asked me previously, I would have said that this would never happen, but

FIG. 4. *A field trip.*

that was what the algorithm produced and when we looked at it, it was entirely right. We regarded that as something of a triumph.

Senn: What were your relations with Rothamsted during the time you were at Wellesbourne? Was there a sense in which Wellesbourne was regarded as a junior partner and looked down upon; were there any problems at all?

Nelder: I suppose we were always thought not to be at the center if you like. I never programmed the original computer, the Elliott 401, because it seemed to me too much like hard work, so I got other people to do it. When the next computer came along, I started to use it and, in fact, we began to put programs together in sequences comprising input, analysis and output programs. This was really the first attempt to produce a statistical system, though I have to say that the people at Rothamsted were never really interested in what we were doing.

Senn: So this is an area in which Wellesbourne was forging ahead of Rothamsted.

Nelder: I think we were, yes.

Senn: This seems a good point to discuss computing because that has certainly been a very important theme in your work and I myself have been amazed to witness you still driving a laptop very rapidly when you give your lectures and switch from one screen to another and so forth. When did you first start using computers?

Nelder: As I told you, we sent data in to be analyzed on the very first Elliott 401, but I did not write programs. When the next machine came along, the Ferranti Orion, which was a 48-bit, three-address machine with a very interesting instruction set and an autocode, we began writing programs. This is where we started to try to develop systems that put various stages of an analysis into one framework. It was hard work because the machine wasn't really designed to do work in this way.

Senn: You have already mentioned your simplex, or perhaps now it should be called polytope, paper with Roger Mead. This paper, in fact, is a citation classic. Do you know how many times it has been cited?

Nelder: No. I would have to go and look it up. I would think upwards of 2,000, maybe more. [Well over 4,000 times by April 2002.]

Senn: How did this work come about?

Nelder: I went to hear a lecture by a man at ICI (Imperial Chemical Industries) who was exploring response surfaces on real-time systems, and they did this by choosing three points to form a triangle say in two dimensions and then reflecting the one with the worst response in the mean of the other two. The problem with this, of course, is that if you get accidentally good results somewhere, then that point gets stuck. We thought we might do this with functions. We said we shall have to make this thing flexible and self-adapting, so we developed this method whereby the simplex enlarged and contracted itself and so on as it went over the surface.

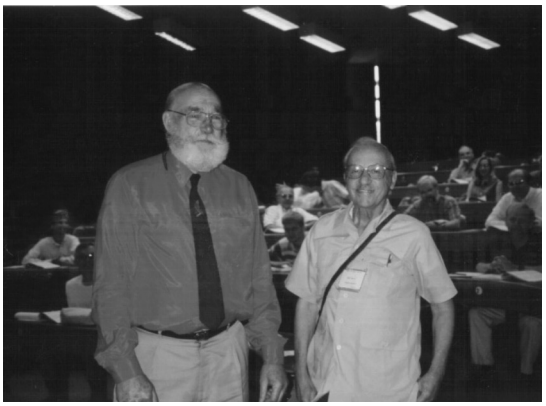

FIG. 5. *SIAM meeting Stanford, July 1997. With George Dantzig.*

Senn: And does this iteration converge very well in practice?

Nelder: You can easily construct an example in two dimensions where it never converges. There are occasions where it has been spectacularly good; particularly on functions with jumps in them. Mathematicians hate it because you can't prove convergence; engineers seem to love it because it often works.

Senn: What was the genesis of Genstat?

Nelder: It began partly, as I said, from our attempts to write a very elementary system for the Orion machine. Then I had a chance to work in Adelaide, where they had a CDC machine that was the next generation as far as I was concerned. I learned FORTRAN for the first time and we began to write this system as a support for the analysis of variance algorithm which Graham Wilkinson was developing. It had things like derived variates, formation of tables and so on. That was the beginning of it.

ROTHAMSTED AND GENERALIZED LINEAR MODELS

Senn: Were you involved in the development of GLIM at all?

Nelder: Very much so, because I was the chairman of the GLIM working party. When the paper on generalized linear models came out in 1972, we decided

to develop a small package that would implement the fitting of this class of models.

Senn: Surprising that you did not simply decide to develop some suitable separate procedure within the Genstat package. What was your reason for pushing for a separate package?

Nelder: I think we were much concerned in GLIM to have something that worked on the smaller and slower machines of the period. Looking back at it, I see that we should have joined them together. What actually happened was that we took the know-how we got from GLIM and put generalized linear models into Genstat.

Senn: Tell me about GLIMPSE.

Nelder: This was a project funded by the EEC (European Economic Community) and organized by NAG, the Numerical Algorithm Group, to try to put a front end to GLIM (using Prolog as the language for defining the rules) so that people could analyze data with some help from the "abstract statistician" as we called it. There were interesting ideas in this, but it turned out that perhaps Prolog was not the ideal language in which to write complicated rule systems full of uncertainties, but it had its points. The trouble was that Prolog was not sufficiently standardized. Thus, when we came to move from one machine to another, there were problems making a version for the general public. Nonetheless, it did contain a lot of useful ideas about how to analyze data using GLM's. I am sorry that we did not have enough manpower to carry the thing through in the time that we were allowed.

Senn: There is a paper by the late Bernd Streitberg in which he claims that expert systems are impossible (Streitberg, 1988). He got frustrated by people claiming that such systems are possible. I think he cites yours as being the closest he ever saw to a true working example.

Nelder: I think he was expecting too much. He was expecting something in which you could prove in some sense that something was going to work. We never imagined ourselves doing any such thing. We wanted to be helpful to statisticians who were involved in analyzing data, but did not know as much as we did; we could help find the best model for them and so we had a very much more restricted aim than he seemed to want for artificial intelligence systems. I didn't agree with him for that reason. We were not trying to do what he was wanting us to do.

Senn: What is the K-system?

Nelder: Genstat has a great many more facilities than GLIM, so I wanted to try and persuade people to move from one to the other. I also wanted a system in Genstat which looked in many ways like GLIM, so I wrote a set of procedures which makes Genstat look very much like GLIM. I use it almost all the time for my own computing.

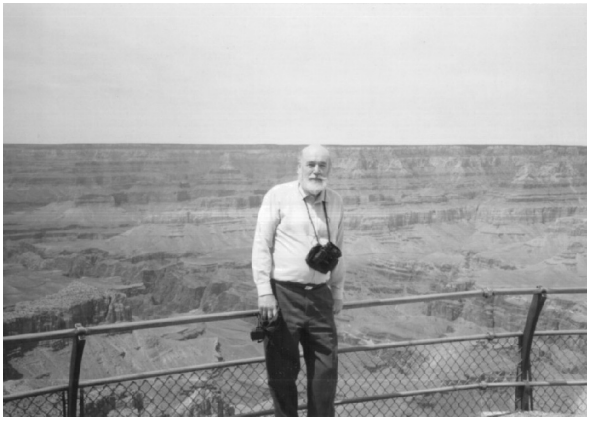

FIG. 6. *On a visit to the Grand Canyon.*

Senn: You have been critical of developments in modern statistical computing. What is it you dislike in these developments?

Nelder: There are two aspects to this. There is the statistical side and there is the computing side. Some of the statistics seems to me to be simply wrong. For example, I have quoted Type III sums of squares in SAS as being of no interest in making inferences. I am also critical of what I call linear programming, in which procedures are simply added in sequence to an existing set without any attempt to integrate them into a proper framework, such that where the same thing occurs in different places it is done in the same way everywhere.

Senn: You mean this is linear programming as a philosophy of programming rather than a branch of mathematics?

Nelder: Yes. It is nothing to do with mathematics at all. It's simply this idea of adding yet another procedure on when you want something else, instead of looking to see how it could be integrated with what you have already. Although there have been many criticisms, no doubt justified, of Genstat, one of the things that it does have is this idea that when you need a certain facility, it's the same everywhere in the system.

Senn: You are now working with Youngjo Lee on a program to implement HGLM's (Lee and Nelder, 1996). When and how will this be available?

Nelder: For those who do not know about them, HGLM's are a way of integrating generalized linear models with random effects and allowing models for

the dispersion as well as for the mean. We have written a system of Genstat procedures that implements quite a large variety of HGLM's. It's available now; it is not finished, but it seems to work. I think we know how to do it now. At the moment, I do all my programming on Genstat—I went back to FORTRAN a little while ago but found I had forgotten how to write in it.

Senn: You arrived at Rothamsted as head of statistics in 1968. How did this come about?

Nelder: When Yates retired, I obviously applied for the job because it was really the premier job of its kind in the country. I was interviewed by Fred Bawden, who was then director, and he offered me the job on the spot. I pressed for a grade higher than he was originally prepared to offer and I got it.

Senn: As an employee at Rothamsted and Wellesbourne, were you effectively a civil servant?

Nelder: No. Although we did have civil servants' salary scale and pensions, we were one stage removed from the civil service because we were all under the Agricultural Research Council. It was part of a setup, typical in Britain, to remove people one stage from government.

Senn: What was the state of the department when you arrived?

Nelder: I think it was, on the whole, flourishing. People were doing research and doing consulting work for Rothamsted. They had a large job dealing with the experiments from the National Agricultural Advisory Service (NAAS), they did surveys for NAAS and they had a good computing setup.

Senn: What were your relations with Yates? Did you get on?

Nelder: I think we did. He behaved impeccably. He had a room in the computing department and he got on with his survey program. He never attempted to interfere at all with anything I did, and I was happy to talk to him and get advice when I needed it.

Senn: Did he continue to come in the department during all the time you were head at Rothamsted?

Nelder: He continued to come into the computing department, which is where his room was, rather than the statistics department, and there certainly wasn't any problem.

Senn: Michael Healy wrote a rather nice memoir of Yates recently and suggested that Yates never used matrices (Healy, 1995). Clearly, your approach was rather different. Was there a revolution at Rothamsted in that respect?

Nelder: I don't know. When I wrote the two papers on general balance, the whole thing was done in matrix algebra because that's essentially what I knew. It's true that Yates did not use matrix algebra. I believe that when these two papers of mine were sent by the Royal Society to him to referee they were actually refereed by Michael Healy. I don't know whether this produced a revolution or not. I suspect not, probably.

Senn: How did you first get the idea of generalized linear models?

Nelder: People keep asking me this and I'm not sure that I really know. I wrote a paper which was published in 1968 in *Biometrics* (Nelder, 1968). I have only met one person who ever read it. I noticed that there was a certain similarity in the algorithm for probit analysis and for a gamma-error-type model that I had introduced for spacing experiments. I didn't get any further than to say that these things were rather similar, but I think that's where the original idea came from, that there was a set of models which had a single algorithm for their fitting. I think Wedderburn's knowledge about the exponential families then came in and resulted in our 1972 *JRSS A* paper (Nelder and Wedderburn, 1972).

Senn: I must confess to having some confusion when I was a young statistician between general linear models and generalized linear models. Do you regret the terminology?

Nelder: I think probably I do. I suspect we should have found some more fancy name for it that would have stuck and not been confused with the general linear model, although general and generalized are not quite the same. I can see why it might have been better to have thought of something else.

Senn: How did your collaboration with Wedderburn come about?

Nelder: He was appointed to the department and he was clearly very bright indeed; I think we then decided to work together.

Senn: Your paper would have seemed to be a natural candidate for a "read paper" for the Royal Statistical Society. Did you not consider putting it forward for reading?

Nelder: The paper was rather short and it contained almost no theorems about asymptotics or anything like that. It simply states the nature of the synthesis for this class of models. Perhaps we did not know enough in those days to consider putting it forward. It certainly wasn't suggested by the Royal Statistical Society for that purpose. I should add, perhaps, that a rather eminent statistical journal to which it was submitted first, turned it down flat without any opportunity to resubmit.

Senn: What were the reasons for turning it down? Can you remember?

Nelder: Not enough mathematics, I suspect.

Senn: Wedderburn died very young. Did you have plans for further collaboration?

Nelder: I'm sure we would have gone on. He was full of ideas, and I would have liked to develop many things. He died of anaphylactic shock from an insect bite on a canal holiday, aged 28. It was very sad.

Senn: You found another collaborator at Imperial College in Peter McCullagh. Can you remember how this came about?

Nelder: I suspect that it was David Cox's idea originally that we should collaborate on a monograph on GLM's in the series of which he was editor. That's where it started. We wrote the first edition largely at the University of British Columbia in Vancouver. I went there for about six weeks, wrote solidly in wonderful weather and that's how the kernel of that first edition came about.

Senn: Will there be a third edition?

Nelder: The answer is no, because neither of us really wants to try to integrate the enormous amount of literature that has appeared since the second edition into a new edition. Perhaps, also, we are aware that the second edition is still selling quite well.

Senn: To backtrack a bit. There is a paper that you read to the Royal Statistical Society which was extremely famous, and I think owes its origin to a manuscript that used to circulate at Rothamsted called the "great mixed model muddle." What exactly is the great mixed model muddle?

Nelder: There was an idea that random terms in a linear model were somehow quite different from fixed terms and the result of this was that the expected values for sums of squares for models which contained both fixed and random terms had a most peculiar pattern or nonpattern. Components of variance appeared and disappeared in the most incomprehensible way from the expectation terms in the analysis of variance, and I believed that this was quite wrong. I tracked it down eventually to the fact that they were imposing constraints on parameters for the fixed effects and not imposing them for the random ones. It's clearly wrong to impose constraints for the fixed-effect parameters, just because you have to have constraints on their estimates. If you abandon this approach, the fixed and the random effects produce exactly analogous formulae, with variance components in one case and sums of the squares of effects in the other. Also this constraints business produced faulty tests; they got the wrong denominators because of the appearance and disappearance of the variance components in the different sums of squares for the different effects. I tried to undo this. The Secretary [of the Royal Statistical Society] rang up and said there was going to be one very critical contribution to the discussion—which there was—in which I was accused of every scientific sin under the sun. As I said afterwards, fortunately this was greeted with more hilarity than concern. I'm still fighting the battle because the points are still not understood, with the result that something that ought to be relatively simple is actually made very complicated. That goes against all the things that I believe in.

RETIREMENT AND HIERARCHICAL GENERALIZED LINEAR MODELS

Senn: You retired in 1984. I suppose you must have been 60 then. Was that early or usual?

Nelder: It came at a time when the government did not like paying grants for research very much and they were determined that it was going to be cut back. Putting it very crudely, there was supposed to be too much food, so why did we want to spend more money on agricultural research. They started getting rid of people at 60 as a way of reducing numbers. This was the best thing that ever happened to me in the sense that, shortly afterwards, the squeeze was really on and I would have been telling people that they'd been made redundant and so on. Whole departments disappeared from Rothamsted. It was all really rather sad. I'm pleased, I must say, not to have had to be involved in that sort of activity.

Senn: One might have expected some sort of deceleration of research on your part when you retired but, in fact, that didn't happen. Tell us something about your working habits since Rothamsted.

Nelder: I had been a visiting professor at Imperial for some time while I was at Rothamsted. I used to give a course of lectures on statistical computing to the M.Sc. group. I continued in that and went to Imperial three or four days a week. Basically, I was still having ideas, so I wanted to go on working. I have published 20 papers in the last five years. Some of them have been quite substantial ones. They have been, as usual, a combination of computing applied to statistics together with the development of new models such as HGLM's.

Senn: You've been very critical of some modern statistical work in quality control. What have your criticisms been and what has your own work been in this field?

Nelder: First of all, one must distinguish between quality control and quality improvement. In the old system of quality control, you made things rather badly and then you sampled the output to see whether the number of defectives was sufficiently low for you to let it out to the unsuspecting customers. In quality improvement, you attempt to build in the quality at the production stage, so that effectively you barely have to look at the output because you know it's going to work. This was initiated in Japan by a Japanese engineer, Taguchi. Deming, who was their statistical guru, had an input as well. Taguchi reinvented fractional factorials without having any idea that statisticians had done it 30 years previously. He had very little notion of statistical ideas at all. It is not clear to me that he knew what expectation was. The designs had been done better by the statisticians previously and the analysis could be done much better, I thought, by using modern statistical techniques. So to that extent I have been critical. I think he inverted two distinct processes, one being model selection and fitting and the other being predictions from the model after you have done the fitting. Taguchi designed a quantity called the signal-to-noise ratio, which was, in effect, a prediction and then analyzed that. There are a whole lot of disadvantages to this, which I have talked about, but the basic one has been this inversion of the two processes of analysis and prediction.

Senn: How did you come to be interested in this topic?

Nelder: Oh dear! People always ask me! I am not sure again where that came from. I think George Box had a large part to play in it. He was very concerned to try to do in America what the Japanese had done, which was to make the industrial companies take this idea of continual experimentation and improvement seriously. I think I became interested in how the Japanese were using these ideas because they persuaded quite small companies to do many scores of factorial experiments in their own factories with this idea of constant piecemeal improvement as the aim. I became interested and thought it could be done better; I'm still interested in it and still writing on it.

Senn: I suppose we have to give credit to Taguchi and the Japanese for having done something even if one is critical of details.

Nelder: Absolutely. Taguchi's great achievement was to sell his ideas to the Japanese engineers and their companies and persuade them to do factorial experiments in their own factories as a means of improving what they had been doing; you would hear of people whose fault rate was 1 in a million. Perhaps they were working on ashtrays in cars. Then their idea was to reduce this to 1 in 10 million. Many American and British companies found themselves losing markets wholesale to the Japanese precisely because of this great improvement in quality, and it's all praise to the Japanese for actually having done this. Nevertheless, I think we can do the technical part of the analysis rather better.

Senn: Tell me about hierarchical generalized models.

Nelder: This is an integrated set of models which involves combining the original generalized linear models with their exponential family distributions and random-effect models which have become widely used in the normal-distribution context. We could include modeling of dispersion as well as of the mean because Lee discovered that this could be done by a second GLM drawing on the results from the mean analysis, and that by feeding this back into the GLM for the mean and so on we could fit arbitrary models for both mean and dispersion. In doing this, we generalized a quantity which had been known for a long time in the normal case; we called it an h-likelihood and it contains a distribution for the data given the unknown fixed and random components or also for the distribution of the random components. The second component is not a true likelihood, of course, because you don't observe those as data; however, it turned out that the fitting of these models required no prior probabilities and no integration; we developed this and read a paper to the Royal Statistical Society in 1996. The discussion was a disaster because everybody took the worst possible case of binary matched pairs and told us of the great difficulties. Nobody said it worked in other cases. We now know how to overcome these difficulties, but a lot of people still don't like the notion of h-likelihood, although our simulations suggest that it really works rather well. What we've also found recently is a way of modeling correlated errors with nonnormal Poisson and binomial data, by incorporating correlations via the terms of an HGLM, and we can now extend to the GLM class both spatial and temporal models relatively easily.

Senn: Youngjo Lee is a Korean who lives in Korea. How did a British and a Korean statistician come to collaborate?

Nelder: It was Lee's idea basically. He wanted to come to Imperial and I said yes. It's been a very satisfactory collaboration. He is very good on some of the asymptotics, which I know little about, and I have some

ideas in other directions. I think perhaps HGLM's typify what is—I hope—characteristic of my best work, namely, the notion of synthesizing apparently discrepant or distinct models into a single framework. You find this in general balance, you find it in generalized linear models and now you find it in HGLM's; interestingly enough, the correlated-observation extension to HGLM's allows us to generalize the analyses in Cochran and Cox (1957) to errors of the whole of the GLM family, not just Normal, so those are also included in this current synthesis.

Senn: You have already mentioned that HGLM's have been embraced with less enthusiasm by the statistical community than GLM's. Do you have any explanations as to why this is so?

Nelder: No. I am surprised at the resistance that there has been. We have had two alleged counterexamples to HGLM's from one committee, claiming that they that did not satisfy invariance properties. Both of them proved to be simply wrong. We had a third example, which was correct but pathological in a very well defined sense, so that we had only just to move the boundaries very slightly to maintain our theory. There appear to be some people who for some reason are suspicious and I don't know why.

Senn: In conversation to me you have made the analogy to the reception that the proportional hazard model got which also involves a likelihood that is not full in some sense. Do you think that there is any inconsistency on the part of the statistical community here?

Nelder: You're thinking about partial likelihood?

Senn: Yes.

Nelder: Yes, I think there is an inconsistency. I think this is in some ways a rather similar situation. Partial likelihood was a new kind of quantity for which Cox didn't give a full justification (Cox, 1972) but was later shown by other people to have the right sort of properties. I don't know why at the moment we have this resistance, but I hope to get over it before I die.

FIG. 7. *Seoul 1992. Youngjo Lee on the extreme right of picture.*

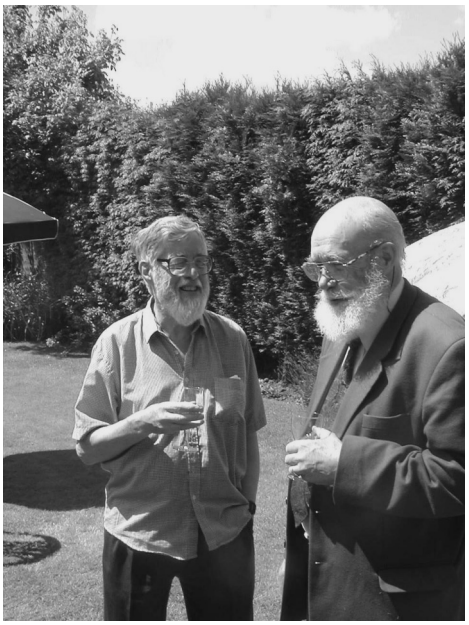

FIG. 8. *John Nelder (right) and Michael Healy at a party at Stephen and Victoria Senn's house in Harpenden July 2002.*

Senn: But retirement has allowed you time for other things apart from statistics, as I know you are still an excellent player of the piano. Do you play the piano regularly?

Nelder: Yes, pretty regularly.

Senn: What sort of things do you play?

Nelder: I have been rehearsing recently three late Mozart piano pieces because I am fascinated by the idea of what late Mozart would have sounded like, and I think that some of these late pieces give us some slight indication of the way he might have developed. At the moment, I have just started work again on the Chopin Fantasy in F Minor and the two Mozart piano quartets. I hope to play these pieces at a musical matinee in this house next summer.

Senn: What about ornithology? I believe that owls have been a disappointment to you.

Nelder: Yes. I have a beautiful snowy owl in this room where we are talking now, but a stuffed one. I wanted very much to see one in the wild. I went to arctic Canada to look for them, but the lemmings had swarmed the previous year and there were no owls. I walked with John Tukey and Lincoln Moses to look

for the great gray owl in Yosemite. Lincoln knew the very tree where it nested, but there were no owls so I haven't been terribly successful with owls. I tend at the moment mainly to birdwatch when I go to other countries. I've seen more than half of the land species in Australia now and it's getting quite difficult finding new ones there.

Senn: So that's a further project for your retirement in addition to getting HGLM's accepted. I wish you the best of luck with both projects.

Nelder: Thank you.

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