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A Conversation with Richard M. Cormack

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Abstract. Richard Melville Cormack is one of the giants who developed the theory of mark-recapture. Referring to his key paper in 1964, and the papers published back-to-back in 1965 by George Jolly and George Seber, the "Cormack–Jolly–Seber model" is central to the development of mark-recapture methods for estimating survival.

Richard was born on 12 March 1935. His father was Principal of Stow College of Engineering in Glasgow. From the age of 7, Richard attended Glasgow Academy, and later entered directly into the second year at King's College, Cambridge, intending at the time to be a theoretical astronomer. He secured first class honours in Special Mathematics from London as an external student in 1954, and second class honours in Mathematics from Cambridge in 1955. After changing direction, he left Cambridge in 1956 with a Distinction in the Diploma in Mathematical Statistics.

Richard's PhD, undertaken while a lecturer at Aberdeen, was completed in 1961. Richard's period at Aberdeen (1956–1966) coincided with a golden era for statistics there, and his colleagues included D. J. Finney, Bill Brass, Peter Fisk, David M. G. Wishart, Michael Sampford, Robert Curnow, George Jolly and Andrew Rutherford (the last four being members of the ARC Unit of Statistics). In common with a number of these colleagues, he moved to Edinburgh in 1966, holding a Senior Lectureship there until 1972, when he became the first Professor of Statistics at St Andrews.

Richard's groundbreaking contributions to mark-recapture in the early 1960s continued when he addressed the issue of heterogeneity in capture probabilities, publishing a test for heterogeneity in Biometrics in 1966. Then in 1972, in another Biometrics paper, he showed the logic behind capture-recapture estimates, making the methods more accessible and understandable to the user community. In 1981, jointly with Philip North, Richard published important insights into mark-recovery models. His work on log-linear models for mark-recapture led to papers in Biometrika in 1984 (with Ron Sandland) and 1991 (with Peter Jupp), and in Biometrics in 1989, and, additionally, to four book chapters. There was also a sequence of Biometrics capture-recapture papers in the 1990s: on modelling covariates (1990), on interval estimation (1992) and on variance estimation (1993). After retirement in 1994, his publications in mark-recapture were mostly as co-author in epidemiology studies.

Richard also published on other diverse topics, often with scientists from other disciplines. His 1971 review of classification, read to the Research

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Committee of RSS and later appearing in JRSS A, is a classic, and while his 1988 exposition on statistical challenges in the environmental sciences (also in JRSS A) has had substantially less impact, it too showed his characteristic incisiveness. His contributions to a wide range of committees, working groups, visiting groups and scientific organisations (including council member for NERC and the Freshwater Biological Association) were substantial. He was elected a member of the ISI in 1962 and a Fellow of the Royal Society of Edinburgh in 1974. He held various offices within the Biometric Society, as Secretary of the British Region 1970–1977, Regional President 1990–1992 and President of the International Society 1980–1981. He served on the Council and various committees of the Royal Statistical Society.

Richard married Edith Whittaker on 1st September 1960, at King's College Chapel, Aberdeen. Edith is a plant ecologist, and a past chairperson of the Fife and Kinross Branch of the Scottish Wildlife Trust and of the Friends of St Andrews Botanic Garden; she was also a founding member of the Garden's Education Trust. Their son Andrew is a European Chartered Engineer working for the JANET network, while their daughter Anne is a Marketing Manager.

Photography has been a passion of Richard's for many decades. He was a lecturer and judge for 40 years for the Scottish Photographic Federation, and was placed on their roll of honour. He has held exhibitions in Dundee (Land of the Berbers), St Andrews (Growth and Form) and Aberdeen (Walking in the North), and has given many talks.

Richard firmly established the University of St Andrews as a centre for statistical ecology, a strength that continues today.

Key words and phrases: Capture–recapture, Cormack–Jolly–Seber model, Jolly–Seber model, open populations, survival models.

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1. EDUCATION

Buckland: How did you start off in statistics?

Cormack: I was always very interested in numbers as a small boy. The first thing I remember about numbers must have caused my mother some trouble. She was foolish enough to show me a book published early in the war to encourage a healthy diet. This gave the recommended daily dietary intake for every mineral and vitamin and also gave a breakdown of contents of many available foods. I demanded that my mother weigh out every bit of food and I would go to her and complain that I had not had enough magnesium or whatever that day.

Buckland: How did you then move into the discipline of statistics?

Cormack: I was way ahead of my age group at school. I started my Scottish highers on my 14th birthday, whereas the average was 17. That was OK. I then used up one year preparing for Cambridge scholarship exams, but when I went to Cambridge with a major scholarship in maths, they put me direct into second year, age 17. So I essentially finished my degree in two years, which I think in retrospect was bad. I had to spend a third year for a residence qualification to get the degree, and my interest then was very much in becoming a theoretical astronomer. I had been enthused by a set of evening lectures in Glasgow by Dr Tannahill on astronomy. So I set off to do part III in astronomy and I hated it. Some of the lectures were very difficult, however eminent the lecturers. Hermann Bondi was great fun but gave you dreadful lecture notes. Abdus Salam and Nevill Mott, both of whom got Nobel prizes shortly after, were rather dull teachers, but the worst lectures were from Fred Hoyle, on the structure of stars, which were really appalling. Partly as a consequence, I didn't do much work, and I failed part III astronomy.

I had been fortunate earlier that my two tutors at Kings in Cambridge were Dr. Ingham and Philip Hall, who many regarded as one of the greatest algebraists

Britain had produced up until then. Both of them were very shy men. This was before Philip Hall got the Sadleirian Chair in Mathematics. They felt I didn't deserve to leave Cambridge as a failure. So, with my parents' support, they arranged that I be accepted to do the Diploma in Statistics.

I thoroughly enjoyed this fourth year and got a distinction (and a half-blue). In the history of the Statistical Laboratory that Peter Whittle has written, my year was described as the first year of transition before disaster, with all staff leaving. David Cox had not been reappointed, so they were understaffed. I had Henry Daniels as my main supervisor—an excellent teacher and guide. You had to wait for five minutes at the beginning of supervision while he put the watch that he had to bits on the desk back together again before you could get down to work!

The main lectures were given by Henry, Frank Anscombe and Dennis Lindley, all excellent. As I remember, Dennis did not use the term "Bayesian." One course, on combinatorics, was given by John Wishart, then Director of the Lab. He went on sabbatical at Christmas, and drowned off the coast of Mexico.

On the Diploma, you were farmed out to departments to do a book of work of applications. I was unlucky in that I got a very bad dose of flu when the allocation was handed out in the first week of term. I was given the minor tasks remaining. Part was good—one client was Dr. Ingram in food sciences, and I became interested in dilution series. What I remember most about my book of work was totting up data on schoolboy participation in sport in Cambridge schools.

2. ABERDEEN

Buckland: When you left Cambridge, what came next?

Cormack: I thought I might spend five years in university and five years in industry, and then decide which I wanted to do; you could do that in those days. I applied for a job with Shell Research in the Wirral and didn't get it. The second job I applied for was with Torry Research in Aberdeen. When I went there for interview, David Finney, head of the department in Aberdeen and then a Reader in Statistics and running the ARC Unit of Statistics, was asked, as the senior statistician in Aberdeen, to conduct the interviews. He told me that he had lost his assistant lecturer in statistics, as she had just gone to Shell Research(!). "Would you consider yourself a candidate for the University post? I feel I have to fill it before the Torry post." So I said "yes." I was very happy to be coming back to Scotland. Cambridge was very nice, but flat and no sea or any of the things I had grown up to love in Scotland.

The first course of lectures I was given—bear in mind I was then just 21—was a service course of ten lectures to botanists, zoologists and soil scientists. With a First in maths and going into academia, I had avoided National Service, but many of the students had not, so the average age of the class was considerably older than I was. I adapted notes from Ian McDonald, a statistician at the Rowett who had previously given the course. I must have made a reasonable job of it because the students came with honours projects asking for help, and they must have reported to their staff that this guy was doing all right because the staff in turn came along: Charles Gimingham in botany and George Dunnett in zoology—the two leading field ecologists in Scotland at that time, I would say. I can remember Charles coming into my office and dumping on my desk a newly published paper and saying "Richard, is this a load of rubbish?," and this was the first paper by Williams and Lambert on the classification of vegetation communities. I worked with him on field trips to Gotland, among others, with their students. This led to my interest in that area, and an invitation from RSS to write a review paper on classification.

3. MARK-RECAPTURE

Buckland: How did your interest in mark-recapture arise?

Cormack: George Dunnet had been away that first year on leave in Australia. When he came back, he came along and said, "I have been doing work on mark-recapture of quokkas on this little island of Rottnest off the west coast of Australia; an Australian genetic statistician has looked at the data for me and I can't understand a word that he has said. Can you help?" George then asked if I could help out with their long-term study on fulmars on Eynhallow in the Orkneys. This brought me into mark-recapture in this very special case; they were not interested in how many birds nested on the island—they knew that as they could count virtually all the birds—but what they were interested in was lifespan, because some years earlier James Fisher had stated categorically that fulmars did not start breeding until seven years of age. No one knew where Fisher had got this information. This is a bird that doesn't breed until age seven, lays one egg a year, and doesn't replace the egg if it's lost. It is a population that was largely absent until the 1890s, then spread rapidly around the

coasts of Britain. Given the disadvantages of its breeding strategy, it must live for a very long time. Could we estimate the survival rates of the adults? They marked the birds as adult breeders on the nests with a pattern of colour rings, and fieldwork involved sitting on the tops of cliffs with binoculars (in the sunshine if you were lucky) and trying to identify birds as they flew past. The recovery mechanism was totally different from the original marking, so there was no way, it seemed to us, that you could get estimates of population size. So I developed this over the years.

Buckland: George Jolly was a colleague of yours in the 1960s. Could you describe your interactions with him?

Cormack: George was in the ARC Unit of Statistics in the same corridor as I was. His main job was designing and conducting agricultural surveys in Scotland. There wasn't a practice of giving seminars in the department to talk to colleagues about what one was doing, and David Finney's appointees had been chosen to cover all the varied areas of statistics rather than build a research group in a particular area. So despite the fact that I met George every day at coffee, and, indeed, we caused David a lot of angst as, on many mornings, we played kriegspiel (a version of chess where you don't see the other person's board and a referee judges—very good for developing inference), we never mentioned work and mark-recapture. I don't remember George noticing my Biometrika paper in 1964 (Cormack, 1964), or indeed the practical paper in British Birds in 1963 (Dunnet, Anderson and Cormack, 1963). It was completely unknown to the two of us that we were working in the same area.

When I submitted the paper to Biometrika, the referee was John Darroch who had developed deterministic methods. It was strange to me that so many top mathematical statisticians and biometricians had a go at mark-recapture and not come up with the estimates for it: RA Fisher, Pat Moran, John Hammersley. Egon Pearson, the editor, told me who the referee was, as it would be obvious anyway. So I corresponded with John Darroch, and his report included the statement: "Although this theory was designed for an experiment in which capture is a different process from recapture, your estimation theory provides a solution to the problem in paragraph 4 of my 1959 paper (Darroch, 1959) where the two processes are the same, for the density for which you derive your likelihood is… it follows that the particular formula provides a sensible estimate of the size of the current population." So it's all there. I wrote back, adding to the paper, "It may be profitable to carry the comparison with Darroch's work a stage further: if the assumptions were the same, then you could get the estimates in that way." So although my paper was concerned only with survival, the estimates of population size were sitting there.

In the same way, George Jolly in the last section of his paper (Jolly, 1965) gives ideas for generalizing his methods to all sorts of other cases, which a lot of people then spent a lot of time redoing from scratch later on. George knew all of this in 1965.

Buckland: How did your mark-recapture research develop after those initial papers?

Cormack: I never believed the assumptions of CJS: that real animals were equally catchable and independent. As it happened, after eight years in Aberdeen, David Finney suggested I take a sabbatical. Blair Bennet, who was a medical statistician from the University of Washington, came to visit David Finney. He said that they were about to set up a graduate programme in biometrics. There had been nothing until then; statistics was in the mathematics department. "Would you like to come as a teaching fellow?" I thought "I know some names associated with the University of Washington," so I looked it up and of course Doug Chapman was there and so I went for a year. The bulk of the year was devoted to teaching, itself an eye-opener, as Doug had done his PhD with Neyman while David Finney was entirely Fisherian. Their approaches to statistical inference were totally different, though both were motivated by immediate biological problems. Doug encouraged me to develop ideas I had for testing the assumptions of CJS (Cormack, 1966).

Washington wanted me to stay. We seriously considered it, but decided against it for family reasons. Later we returned to Seattle for virtually all my sabbaticals. The University of Washington set up a Center of Quantitative Science for Fisheries, Forestry and Wildlife, of which Doug Chapman became head. When Doug retired, David Ford from Edinburgh applied, and I was very happy to be his referee, both knowing his work and knowing what went on at the University of Washington. So I was encouraged to revisit.

One year after that first visit, David Finney moved to Edinburgh and George Jolly and I, among others, went with him. Two research students continued my concerns with the assumptions of mark-recapture. One was Andrew Carothers. The test of equal catchability, developed in Seattle, had very low power with a small number of sampling occasions. Andrew extended work by PH Leslie to develop better tests (Carothers, 1971, 1972, 1973). His practical work was nearly as good as

sitting on a cliff top looking at fulmars. He sat in the coffee room in Jenners in Princes Street, looking down and observing the numbers of all registered taxi cabs—a population whose size was known!

The second student was Neil Arnason. I had got involved with the Freshwater Biological Association, particularly with Malcolm Elliott who was trying to estimate the number of trout in a stream. The fish moved up and down the river. Neil developed CJS models to include movement into and out of neighbouring areas, so bringing in spatial aspects as well (Arnason, 1972, 1973).

George Jolly and I again failed to communicate in Edinburgh, despite playing squash and piano duets together (our party piece was "The Arrival of the Queen of Sheba"). I had just started to develop the log-linear models and wanted to find a way of making population size a regular parameter on the same footing as capture and survival probabilities. One of the books my parents had loved when I was young was Maurice Maeterlinck's play "The Bluebird." In that play, all the humans in the world are sitting in another space. They all exist and are waiting to be born. So I got this idea that the population size was really a sample from this superpopulation which could be treated as a Poisson random variable. George Jolly in one of his papers in the early 1970s independently proposed the same idea. Once I came to St Andrews, we had no more contact.

Buckland: What interactions did you have with George Seber?

Cormack: Before the 1965 papers (Jolly, 1965; Seber, 1965), George Seber and I had no contact whatsoever. After the papers, yes, we did. We got into deep communication after the first papers, and he was all for sending me drafts of everything he did. He produced stuff at a colossal rate and his encyclopaedic knowledge was unbelievable. I'm not sure he ever actually worked closely with biologists, but, when he was writing his book, he asked if I would comment on the draft chapters on the bits I knew about. But you have to realise that communication between opposite corners of the world took time. At one point, I received a plaintive handwritten letter saying "The University has cut down on postage and I'm not allowed to post the draft chapter airmail and you will have to wait for it to come by surface mail from New Zealand." By the time it arrived, I already had another airmail letter from him saying, "I'm sorry you haven't been able to comment on the chapter—I've had to submit it!" To some extent, the opposite is true now: response is too quick.

Buckland: Yes, life is dominated by email these days! You mentioned that what you did was a very special case dealing with survival. But most of the interest by the 1990s was in that special case of open-population mark-recapture.

Cormack: That may be true for wildlife estimation, and surely started earlier than the 1990s, Burnham and Anderson and so on. As you know, I turned to closed populations, and I'm very strongly of the belief that mark-recapture in the ordinary way cannot provide decent estimates of population size. I am happy about survival estimates—what you're estimating is the survival rate of marked animals, and this is quite explicit. If anyone wants to say that applies to the whole population or the species in general, then what they have to do is to say that there is no reason why the survival of marked animals should be any different from the survival of unmarked animals. However, the size of the population is unique to the particular study and cannot be generalized to a wider family of populations. You have got to bring in other information, and I think some of the new work, your work and work here and by others within the last 10–20 years since I've stopped taking a detailed interest, has shown ways of doing this.

Buckland: Some of your views about estimating abundance come from your work with mark-recapture in epidemiology.

Cormack: When I left Aberdeen, I lost contact with the fulmar study and so I didn't have the motivation of working with the biologists, which has always been what has driven me most of all: trying to provide an answer and help for a biologist sitting across the table from me, discussing his problems with his data. The next big jump as far as I was concerned was reading Steve Fienberg's paper on representing mark-recapture of a closed population as a multidimensional contingency table. I liked that. It put it into a framework of statistical inference which was well understood, whereas mark-recapture had always been out on its own, separate from other areas of inference.

Let me make two comments on the impact of computers. The first was that, when I was developing CJS, computers were just being introduced into universities. You had to go through this horrible algebra, pages and pages of it. When I came here, John Howie, Professor of Pure Mathematics, said to me that this is the kind of algebra that should only be done by consenting adults in private. The variance formulae were really appalling, and I was never sure of them when I had these long formulae and didn't see how they would simplify. When we got a computer and I analysed some data just typing

in the formulae term by term, lo and behold the computer said that a lot of the covariances were zero. So I went back to the formulae and saw how they could be simplified.

Secondly, when it came to the contingency table approach, we were setting up an honours course in data analysis, and had decided to base it on GLIM, which I thought was a beautiful package, simple and direct, though you had to be specific in the instructions you gave. In preparing the course, I thought "You can do contingency tables in GLIM. Let me just feed in numbers generated as the expected values from a four-sample open population recapture study and fit the saturated model." As hoped, the fitted values gave the CJS estimates. The surprise was that estimates of some interaction parameters were zero. I worked through the algebra to discover the exact correspondence between each biological parameter (capture probability, survival, birth) and the GLIM log-linear parameters. Recapture data on open populations could be analysed as a contingency table (Cormack, 1981, 1989). I wrote to Steve Fienberg; his level of disbelief was such that, on his next visit to Europe, he diverted to St Andrews for me to try to show him that it actually worked. The reason why it proved very difficult to get it across to many people was the particular way in which GLIM parameterized the contingency table. The GLIM mean is not an overall average, but the expected number in the first cell of the 2^k contingency table, chosen to be the cell for capture in every sample. Main effects are not the main effects averaged over all data; they are the ratios of expected numbers seen in all samples but one to those seen in all. This asymmetry is wholly appropriate for mark-recapture, where the two categories of seen and not seen represent known and unknown; an average over the two classes makes little sense.

That work then attracted attention from various people. I'll start with epidemiologist and social scientist Martin Frischer in the University of Glasgow who came to me with data on injecting drug users in Glasgow who had been recorded on four different lists, and could I help analyse data of this kind. I got estimates out—I was dealing with what they thought was a closed population—and then began to doubt whether population size estimates would be at all reliable. For that particular example, perhaps it was. Three of the lists were medical lists, while the fourth list was created by the police when they arrested some of these people. When I did the analysis, the medical lists had interactions between them, while the police list was independent of the others. This to me is the crux: you must have at least one list that is independent of the others. It's probably still not a sound estimate because, as I was told later, the police classification of what constituted an injecting drug user was not the same as the other three, so they weren't estimating the size of the same population.

Then lots of other people came wanting to use this methodology. The comments I was making about not getting justifiable estimates of population sizes meant that other people from around the world came up with examples where they didn't believe estimates that other scientists or epidemiologists were producing using mark-recapture; would I look and try to decipher what was going on? I found GLIM ideal for this, because each biological concept—survival, trap-dependence, heterogeneity—is associated in a comparable way with one, or a subset, of effects and interactions in the GLIM analysis. They are not averages over classes, and so you're able to examine each parameter successively, together with residuals for each capture history and a range of standard tests of goodness of fit of each considered model (Cormack, 1985). Human populations tend to show unequal catchability. Population size estimation is based essentially on the inverse of the catchability—the harmonic mean of capture probabilities—whereas the data depend on their arithmetic mean. If some such probabilities are small, the two can be very different. Even if we can justify other assumptions, this makes population size estimates from sets of existing lists unreliable. Emphasising this set me on a collision course with various epidemiologists around the world, as is evident from the published discussion of Cormack (1999).

Buckland: Do you have thoughts on where the discipline is going in the future?

Cormack: You have to get information from another source to back up the mark-recapture. I am not convinced that individual covariates help, though replacing log-linear by logistic analyses allows estimates to be found. How do we know the distribution of the covariates over the unobserved individuals? Some element of random sampling is required. But how, when the population is unlisted, of unknown size? Careful study design is required whether in ecology or epidemiology. Distance methods from line transects or point samples, such as you and your colleagues have developed, are one approach. They also extrapolate from continuous measurements to the zero cell, whereas for mark-recapture you're extrapolating from counts to the zero cell, and the scope for getting that wrong is bigger than when you have a continuous curve.

4. SPATIAL METHODS

Buckland: One source of information that hasn't been fully exploited until the last decade is the spatial information.

Cormack: That was another area of statistical ecology that I got involved in quite early on. My early work on classifying vegetation communities gave me a continuing interest in spatial pattern of plants. In my Edinburgh days, David Ford, then with ITE (later in Seattle) came to me with questions of competition. There was great interest in whether competition between individuals was more for light from above or for nutrients from the soil. He had a monitoring scheme in a wood outside Edinburgh measuring the light intensity high in the canopy. To mimic this, he set up a model experiment where he grew French marigolds from seed set in a hexagonal array in trays inside with lights above and continuous video recording. They were planted very close together so they competed from the moment they had proper leaves. Competition was intense. You could see this from the astonishing film: the leaves folded up at night, and when the light was switched on, they were fighting to get on top of their neighbours' leaves. Questions arose from this about how to model competition.

At that stage, spatial processes were just being developed, and I have a hazy memory of an extremely intense evening in an underground bar somewhere in Bath after an RSS meeting with, I think, Julian Besag, Brian Ripley, Bernard Silverman and Peter Green, all on this new stuff of spatial processes. Statistical interest was more in positive spatial correlation. Instead, I looked into ways biologists were using to test for randomness and to describe pattern. The most elegant paper I ever wrote was a one-page note in Biometrika on that. Trevor Cox and Toby Lewis had produced a description for pattern, based on the pairs of distances of a random point to the nearest individual, and from that individual to its nearest neighbour. From a large number of special cases (several pages of the thesis as I remember—I was Trevor's external examiner), they conjectured that there was no information about the pattern from those pairs where the second distance is more than twice the first. I just turned the question around and produced a general proof. That was one bit of nice mathematics that I have done. I suppose some of the mark-recapture log-linear modelling was quite elegant too.

5. INTERACTIONS WITH ECOLOGISTS

Buckland: Would it be fair to say that your contacts from ecology motivated your work more than your colleagues in statistics?

Cormack: David Finney was much involved with the Biometric Society, and he pushed me into becoming involved as well. At first there were my contacts with Gimingham and Dunnet in Aberdeen. Then I became Secretary of the British Region of the Biometric Society. With my work on classification, I had met and talked with Greig-Smith, Mark Williamson and other plant biologists, and I was sure that the Biometric Society in Britain had to get far more biologists involved in the society, and so set out to do this. Somehow—I don't remember how—Roger Mead from Reading and I were invited to join the annual summer meeting that the ecologists from Imperial College and York held jointly in alternate places. So I met the two groups of top theoretical ecologists in Britain. I didn't get on with Bob May, who was always invited to these meetings, because at that stage, he would have nothing to do with statistics whatsoever: everything was deterministic. Roy Anderson was of the same mind, though he later converted.

I got these ecologists to join and talk to the British Region of the Biometric Society. Together with Michael Usher from York, at the suggestion of Roy Taylor, who was then Secretary of the British Ecological Society, we formed the Mathematical Ecology Group and that met for a few years. The first meeting was on classification of communities. So I was talking to biologists more than I was talking to other statisticians.

Buckland: I guess that was true in your personal life as well?

Cormack: Oh yes! This is why I object to the view that a service course to biologists is a second-rate activity. Out of that ten-hour course in Aberdeen, I got the two main thrusts of all my career and a wife as well! She was then an honours student in botany, but went on to do research with Charles Gimingham on heather moors and the management thereof—she was the first person to measure the temperature under a heath fire.

6. ST ANDREWS

Buckland: You haven't said much about your time at St Andrews.

Cormack: When I came to St Andrews (Figure 1), I followed the model I knew for a Statistics Department. A key duty of the department should be to serve the rest of the University. Nowadays, I don't think I would get the job with this attitude. We did not try to build up a major research unit in a specific area. In St Andrews I had another research student,

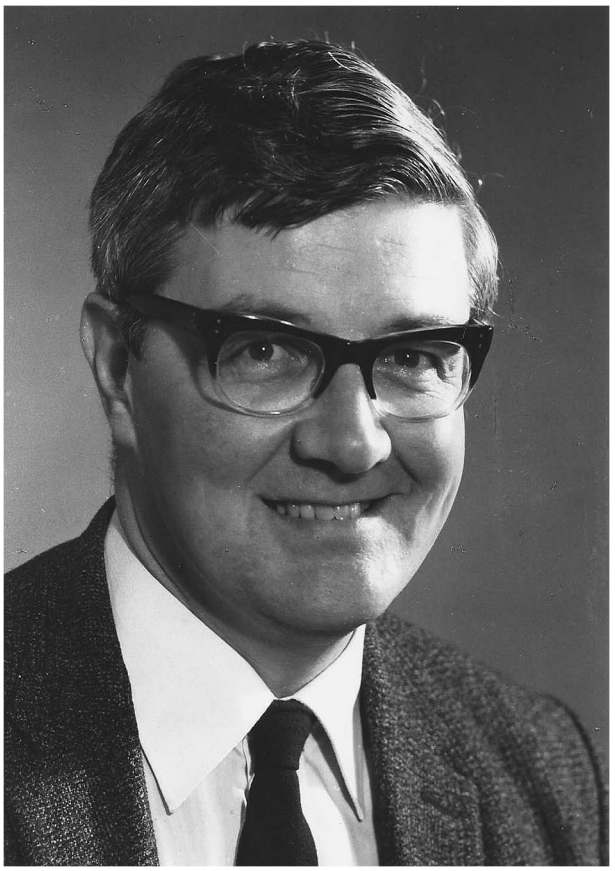

FIG. 1. *Richard at about the time that he became the first Professor of Statistics at St Andrews.*

Robert Fryer, working with Malcolm Elliott, modelling growth curves of fish and developing new approaches to the tricky problems of inference involved. Andrew Lawson worked on spatial modelling, more in epidemiology than ecology. You didn't need to feed him any ideas—it was restraining him from following up impractical ones!

Buckland: What was impressive about him was the fact that he had such a high teaching load while he was doing his PhD.

Cormack: Yes—huge! Not at St Andrews. And he had just got married and had a young child while he was doing it. He has gone on to great things in spatial epidemiology. My other really successful PhD student was Ian Diamond on a totally different topic, but arising initially again from my involvement with biologists. Because of my involvement, I was invited onto various committees of NERC, and onto visiting groups to research stations, to comment on their research. One aim I had was to press the statisticians to do some publication of their own—when they do their consultancy and produce something new for one of their colleagues, to write a methodological paper to establish themselves as scientists in their own right. In due course, I was invited onto the Natural Environment Research Council. I served three years. My first chairman was Hermann Bondi who I had as a lecturer at Cambridge in mathematics—vectors were arrows with much arm movement. I was then reappointed for a further three years and became chairman of the Terrestrial and Freshwater Sciences Committee. NERC also got me involved in how one could fairly assess the status of different scientific areas from numerical data on publications, citations, etc. This chimed with a problem we had in St Andrews as to how to compare the relative value of GCE (English) and SCE (Scottish) school qualifications for predicting a student's university performance. We had roughly equal numbers from each background and had designed tests on basic mathematics for all entrant students on arrival for a number of years. With these test marks, school grades and results at the end of First Year, I applied for a research grant to develop methods to assess the merits of the simple system then used by admissions officers at all Scottish universities. Ian Diamond developed a wide range of methods for multivariate analyses of such data, not requiring any feeding of ideas, a fair bit of holding back. The one thing he says now that I taught him was how to write good English because I tore his writing to pieces just as David Finney, and my father before David, had torn mine.

7. PHD

Buckland: You haven't told us about your own experience as a research student.

Cormack: My own PhD was on a topic in population genetics. I had come from my book of work at Cambridge wanting to work on design and inference for dilution series. It didn't take me long to discover that Fisher had developed everything I had thought of and a lot more. I felt somewhat aggrieved that David Finney hadn't guided me earlier. So he felt obliged to give me a topic: this was in population genetics and involved much more of the kind of algebra that came up later in mark-recapture—I became the world expert I would say in obtaining solutions to six simultaneous cubics in six unknowns. David put forward a conjecture on isoplethy, that is, that in self-incompatible systems without selection, at equilibrium there are equal numbers in the different phenotypes. It didn't take me long to prove that this wasn't the case, and I've always regarded the thesis as a dead end. I didn't have direct

input from a geneticist and was already getting inspiration from the ecologists. But the thesis provided me with one story. I had met RA Fisher when he visited Aberdeen. To go to the ISI in Paris, I offered a paper on my thesis. It was scheduled in a session on populations, mainly demographers. The Italian statistician Corrado Gini was chairman. Although I was third in the list, I went to the start of the session, since I hadn't been at a conference before. On the dot of the scheduled start, Gini said, "First speaker," who wasn't there. "Right, second speaker," and he wasn't there either. So "third speaker"—me. Just as I was sitting down at the end of my talk, David Finney brought Fisher in to hear me; I knew his reputation for sitting in the front row and tearing the speaker to pieces. Oh, the relief!

I also met Tukey when I was a student. Anscombe was leaving to go to America at the end of that year, and he had a farewell party for diploma students and grad students in his garden outside Cambridge, and Tukey was visiting him. I think Tukey was his brother-in-law. Tukey came over and we students were terrified of the great man. In order to say something, one student commented on the pattern of the clouds in the sky, and how did it change, and how do you estimate this. We had a lecture for 25 minutes in the garden: Tukey, off the cuff, designing possible sample schemes for how to estimate the clouds in the sky. Another thing I remember about him: when he came to give a lecture, he sat on the bench, put a pile of coins on one knee and, while he was talking, would move them to build a pyramid on the other knee until it was finished, then move them all back again.

My favourite quote comes from Tukey, the date 1986: "The data may not contain the answer. The combination of some data and an aching desire for an answer does not ensure that a reasonable answer can be extracted from a given body of data." That is something modern statisticians should bear in mind. In consultancy work, you have to produce an answer—you're paid to produce an answer. I had the flexibility that, if I thought an answer wasn't there, I could just say so, which was a freedom that is not so available now.

Buckland: Thank you very much.

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