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# A Conversation with Joe Gani

**Chris Heyde**

*Abstract.* Joe Gani was born in Cairo, Egypt, on 15 December 1924 and attended schools in Cairo and in Kobe, Japan. He commenced tertiary studies during World War II at the British Institute, Cairo, and continued in October 1945 at Imperial College, London, from which he graduated with a B.Sc. in 1947 and D.I.C. in 1948. He obtained his Ph.D. at the Australian National University in 1955.

His employment experience has been varied and international. He began as a science teacher in Cairo, 1942–1945. Next he was Demonstrator in Applied Mathematics, Imperial College, 1947–1948; Lecturer in Applied Mathematics, University of Melbourne, 1948–1950; and Lecturer in Mathematics, Birkbeck College, London, 1951.

He then returned to Australia and was a schoolteacher in the Victorian Education Department, 1952–1953; and Lecturer, 1953–1957, Senior Lecturer, 1957–1959, and Reader 1959–1960 in Mathematical Statistics, University of Western Australia and Senior Fellow in Statistics, Australian National University, 1961–1964. From there he went as Professor to the Department of Statistics, Michigan State University, 1964–1965, and thence to the United Kingdom where be became the first Professor of Probability and Statistics, University of Sheffield, 1965–1974, and later Director of the Manchester–Sheffield School of Probability and Statistics, 1967–1974. He returned again to Australia in 1974 and was Chief of the CSIRO Division of Mathematics and Statistics from 1974–1981. Subsequently, he was Professor and Chairman, Department of Statistics, University of Kentucky, 1981–1985, and Professor and Chairman, Statistics and Applied Probability Program (subsequently Department of Statistics and Applied Probability), University of California, Santa Barbara, 1985–1994. Currently he is a Visiting Fellow in the Stochastic Analysis Program, School of Mathematical Sciences, Australian National University.

He founded the *Journal of Applied Probability*, *Advances in Applied Probability*, *Mathematical Spectrum* and *The Mathematical Scientist*, and he created the Applied Probability Trust to publish these.

His distinguished career has been recognized by his election to Fellowship of the Institute of Mathematical Statistics in 1966, the International Statistical Institute in 1968 and the Australian Academy of Science in 1976. He was made an Honorary Life Fellow of the Royal Statistical Society in 1982 and an Honorary Life Member of the Statistical Society of Australia in 1983, and he was awarded the Pitman Medal of the Statistical Society of Australia in 1994.

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The following conversation took place in Canberra.

## BEGINNINGS

**Heyde:** Joe, would you begin by saying something about your family background and how you came to be born in Cairo?

**Gani:** I was born in Cairo, Egypt, in 1924, into a middle-class Jewish family. There was at that time a vigorous Jewish community in Egypt, much of it of southern European origin. Both my father and mother were born in Cairo, but my grandparents had emigrated to Egypt from different regions of what is now Greece. My father's family came from Janina in Epirus in the mid-1880's, and my mother's from Corfu, one of the Ionian Islands, following the antisemitic riots of 1891.

My parents were married in 1923 in Cairo and travelled together to Germany, where my father carried on his business ventures in Hamburg and Berlin. They returned to Cairo just before my birth. My father did not have the temperament for a businessman; he lost his assets in Cairo during the Great Depression and became the manager of the Japanese branch of an import–export firm. The family joined him in Kobe from 1932 to 1937, when we all returned to Cairo. I learned English in Japan at the Canadian Academy, Kobe, and continued my studies in English at the English School, Cairo.

**Heyde:** You have been a person with a very international career. How did you come to London to study and what got you interested in mathematics in the first instance?

**Gani:** After finishing school in 1941, I very much wanted to get a university education. It would have been possible, like Michael Hasofer, to go to the Fuad I University of Cairo, where the teaching was done mostly in Arabic. But my own Arabic was not up to it, so I decided to take evening courses run in English by the British Council. The British Council then had a very large office in Cairo, and its mission, as it is today, was the dissemination of British culture. There were courses in English language and literature, but also courses in science for people who wished to sit for external degrees from London University. I started off by doing the first year of an engineering degree. I was awarded my Intermediate certificate from London, and at the end of this first year realized that the equipment required for serious engineering studies was not available at the British Council. I therefore looked for something else to do, and decided to study for the Intermediate in science, with an emphasis on mathematics.

I was working between 1942 and 1945 as a teacher at the English School, Cairo, where I had previously been a pupil. This was a school based on the British grammar school model, mainly for the children of British diplomats and civil servants, businessmen and clerical workers in the east-

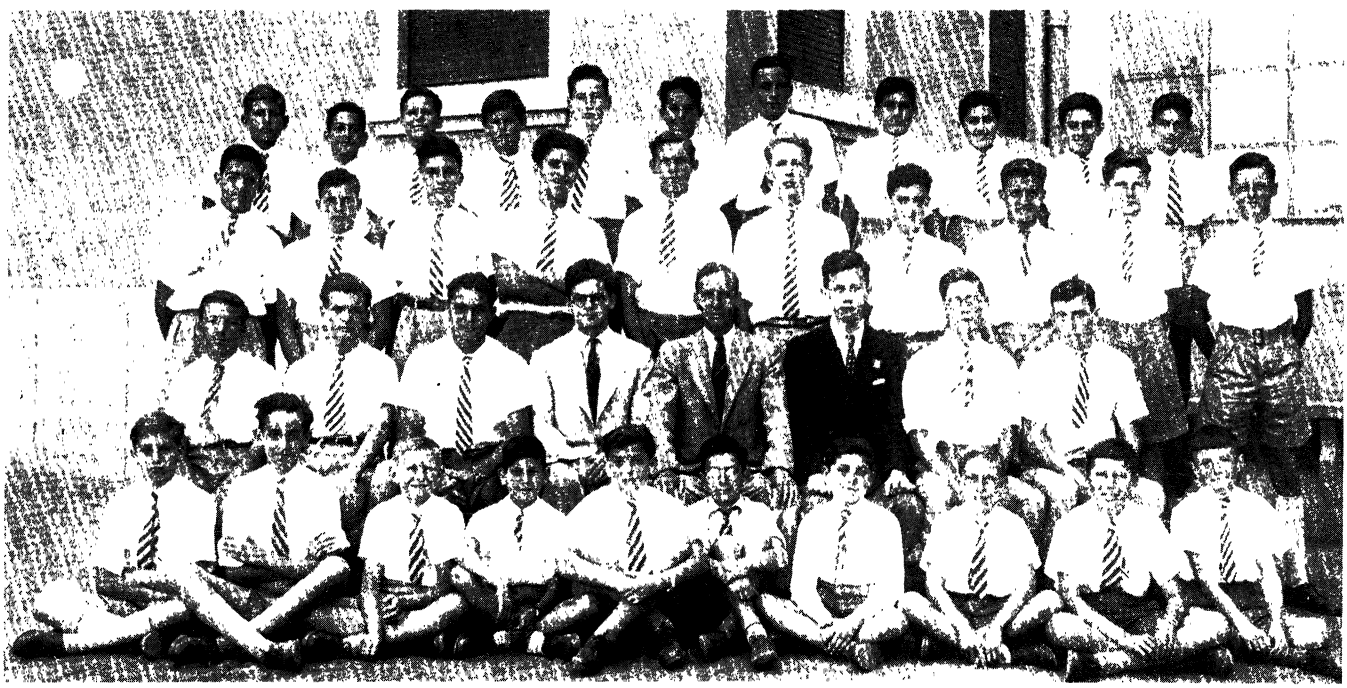

FIG. 1. *Drake House, English School, Cairo, June* 1945: *second row, fourth from left, J. Gani (Assistant House Master); fifth from left, H. Bell (House Master).*

ern Mediterranean region. The Headmaster was Douglas Whiting, and some of the teachers were members of the British Council. What Whiting did for me at the end of my stay in 1945 was to look for a university place for me in Britain. He put my name down for Cambridge, London and one or two other universities, and since I had already passed the London Intermediate examinations in both engineering and science, I was accepted at the Imperial College of Science and Technology, London University. I went there immediately after the war finished.

## LONDON AND MELBOURNE

**Heyde:** What prompted you to stay with science rather than with engineering?

**Gani:** During my evening studies, I became interested in mathematics and applied mathematics. I thought to myself, why not pursue this subject? So I went to Imperial College to complete a degree in mathematics. I should add that the Imperial College degree was really a degree in applied mathematics. For example, I never heard about measure theory until after I left Imperial College; but they had excellent courses on all sorts of topics in applied mathematics: hydrodynamics, electromagnetic theory, the kinetic theory of gases, the theory of relativity, quantum mechanics and other classical subjects.

**Heyde:** And some statistics?

**Gani:** Yes, they had courses in statistics, and these fired my interest in the subject. One of the professors teaching the courses was George Barnard; I owe him and Emlyn Lloyd, who is now retired from Lancaster, my interest in statistics. K. D. Tocher was another of my lecturers: Tocher at that time was moving out of statistics into computing...

FIG. 2. *J. Gani, student at Imperial College*, 1946.

**Heyde:** And operational research?

**Gani:** Yes, that is right. But he was at that time involved in teaching some statistics. These were the people who got me interested in the subject.

**Heyde:** But at that stage you were basically calling yourself an applied mathematician?

**Gani:** Nothing quite so specific; I had obtained a degree in mathematics, and, as I said, at Imperial College that meant a degree in applied mathematics.

**Heyde:** But then eventually you ended up in Melbourne, with a lectureship in mathematics.

**Gani:** That is true. When I had completed my bachelor's degree in 1947, I worked for a D.I.C. (Diploma of Imperial College), roughly equivalent to a master's degree, on some statistical topics under Barnard.

**Heyde:** So that was really your start in statistics?

**Gani:** Yes. The first Arab–Israeli war broke out in 1948, and my mother wrote to ask me to get her and my two brothers out of Cairo. My father had died in 1947. My younger brother Maurice had apparently been arrested as a Zionist; in those days it was easy to get arrested if you were under the slightest suspicion. I mobilized my friends and wrote around for visas; we had friends in Australia, among them Grace Drummond, who had also been a teacher at the English School, Cairo, so we got our Australian visas first. In September of 1948 I emigrated to Australia.

**Heyde:** This was without a position lined up?

**Gani:** No; I was lucky to have a job offered to me at Melbourne University. Hyman Levy, one of the senior professors at Imperial College, had written to Tom Cherry, Head of the Department of Mathematics at the University of Melbourne, who agreed to have me as a lecturer. So I joined the department in September 1948 and lectured mostly in applied mathematics. I taught elementary mathematics, mechanics and dynamics, as well as hydrodynamics. I spent three years in Melbourne as a lecturer in mathematics and tried to get interested in the research that Cherry was doing. But I must confess that it defeated me, partly I think, because I was not tremendously interested in the kinds of hydrodynamic problems that he was studying.

**Heyde:** Was statistics still part of the Mathematics Department in Melbourne at the time?

**Gani:** It broke away in 1949, if I remember correctly, during my first year there. At the beginning, Maurice Belz was teaching statistics at Melbourne from within the Mathematics Department, of which he was an Associate Professor. My memory, which may not be totally accurate, is that in 1949 a separate department was created. In any case, it was shortly after I arrived in Melbourne, and Belz asked me to give a few informal lectures in statistics, which I did, although I was a member of the Mathematics Department. It was at that time that I met Evan Williams, as well as Geof Watson.

**Heyde:** How did you come to make the transition to statistics? How did you end up in Canberra at ANU to do your Ph.D.?

**Gani:** It became increasingly obvious to me that I did not have the motivation to become a hydrodynamicist in the Cherry mould; research interests are a very personal matter. One must not only have an interest in the subject, but also a certain degree of empathy with the people working with one. As you may have heard from other sources, Cherry was an eminent mathematician who was a little stiff in his personal relations.

**Heyde:** A difficult man?

**Gani:** I would not say that; rather a man with whom it was not easy to relate comfortably. My good friend Harry Levey, whom I met at that time, was working with him on hydrodynamic problems and found the going rather hard. I shied off hydrodynamics and got more interested in statistics; I tried to get back to Britain in 1951 to complete a Ph.D. For a short while I got a job at Birkbeck College, London, under Cyril Offord and tried to get started on a Ph.D. in statistics. But I had a misadventure there: when I tried to renew my Egyptian passport, the Egyptian Consulate insisted that I go back to Cairo, something I was not going to do under any circumstances. As a result, I was not able to renew my passport, and the Egyptian Consulate reported me to the U.K. Home Office; I was threatened with deportation as a stateless alien. So I told the Home Office I would deport myself; I had taught for one term at Birkbeck College and then had to leave in December 1951. I came back to Australia, where my immigrant visa was still valid, and decided to stay put for a while. I was naturalized as an Australian citizen in 1954 in Canberra.

When I returned to Australia, I spent my first 18 months looking for an academic job. In these 18 months I worked as a labourer, a clerk, a country school teacher: I had nine different jobs. To be quite honest, I think I grew up during this period. Previously, I had been a privileged member of the middle class who had obtained an education and worked in academe. When I went to the Commonwealth Employment Bureau to look for jobs, I suddenly found out what the real world was like; it made quite a difference to my attitudes. At last, in 1953 I got offers from a couple of universities in Australia; one was the University of Western Australia, where Larry Blakers offered me a job as a lecturer in mathematics. Simultaneously I was offered a research scholarship at the Australian National University (ANU) by Pat Moran. My memory of the sequence of events is a bit vague, but I think what happened was that Larry Blakers gave me leave to work for my Ph.D. in Canberra after I had taken up the job in Western Australia. I am not absolutely certain of the dates.

## WESTERN AUSTRALIA AND AN ANU STUDENTSHIP

**Heyde:** Did you start to teach in Western Australia before you finished your Ph.D., or did you have an arrangement to do the Ph.D. first?

**Gani:** I cannot remember the exact details, but believe I taught during 1953 in Western Australia before going to ANU. Without the help of Larry Blakers I would have had a great deal of difficulty. He was very understanding, and assisted me in getting a doctorate.

**Heyde:** You were employed by the University of Western Australia from 1953 to 1960, which overlapped the time you were doing your Ph.D. in Canberra.

**Gani:** Yes; I was given two years' leave of absence in 1954–1955 to complete my Ph.D. at the ANU.

**Heyde:** So you came to ANU fairly soon after the Department of Statistics began, and Ted Hannan came at approximately the same time?

**Gani:** Ted Hannan actually preceded me. My memory is that Pat Moran came to Canberra in 1952. Ted was there from early 1953, shortly after. I believe he was not originally in the Department of Statistics but Pat discovered him reading mathematics in the library and soon transferred him to his department. I arrived in Canberra shortly afterwards, at the end of 1953, if I remember correctly.

**Heyde:** Pat Moran was remarkably fortunate to start with two such distinguished Ph.D. students.

**Gani:** We were equally lucky to have Pat Moran starting his new department in Canberra, and willing to undertake our training. He was not a systematic supervisor, but he was very generous with ideas and expected you to follow them up. He generated a lot of research problems and would suggest you to look at them, with a view to your working things out independently, if possible.

**Heyde:** Without watching you very closely?

**Gani:** Without watching at all closely. After the initial suggestion of the problem, you were essentially on your own; I think that in Britain this is called the Cambridge style of Ph.D. training. Students are supposed to be mature enough to proceed under their own steam. It works quite well in many cases, but not so well in others where the students may need more guidance. I must confess, that when I started, it took me something like nine months before I latched on to what research was about. I had the mistaken idea that to do research you needed to read very widely, then sit and think very carefully: great thoughts would eventually be generated, which you would write down.

But, in fact, research does not proceed like that at all; I kept trying to think great thoughts and they would elude me. Eventually, Pat suggested I read a paper of H. R. Pitt, who wrote about optimal methods for provisioning stores during World War II. The topic had something to do with his wartime work, and when I read it I realised there was a connection with Pat's ideas on the theory of dams. The two topics fitted together, and I suddenly understood that what could be done for provisioning could also be done for dams and vice-versa. That was basically the subject of my first paper in 1955. I had suddenly grasped that what research amounted to was asking questions, working out their results in detail and then putting pen to paper; ideas would develop as one went along.

**Heyde:** The same probabilistic model fitted to a number of different contexts?

**Gani:** Exactly. In my case, this first piece of research was something of a minor revelation. I suddenly realised one did not have to be a genius to do research; one could do very useful work by starting from ideas or models already in existence, modifying and elaborating these to fit particular conditions. One could also improve the conditions under which certain theorems held. It dawned on me that research was not such a complicated art. Obviously one had to be reasonably bright, but one could do something useful without having the originality of a genius. In any case, great thoughts are for the very few among us.

**Heyde:** So your early work in storage was stimulated by Moran at the time you started on your Ph.D. He had been doing his initial work on dam theory just a little earlier than that or even overlapping that time?

**Gani:** From memory, I believe he started in 1953–1954. I think his first paper in the *Australian Journal of Applied Science* was dated 1954.

**Heyde:** After you finished your Ph.D., you went back to the University of Western Australia and you were concerned, amongst other things, with the state of mathematics in Australia. I just wonder if Larry Blakers was a strong influence in terms of his educational and general concern about mathematics?

**Gani:** Since you mention Larry Blakers, I should say that he did indeed have a strong influence on me. He was extremely generous in the conditions he set me after my doctorate. When I finished my Ph.D. at the ANU in 1955, he allowed me a further year's leave to go to Manchester, where I worked with Maurice Bartlett.

## BROADENING INTERESTS: MANCHESTER AND COLUMBIA

**Heyde:** That was on a Nuffield Fellowship for that period?

**Gani:** Yes, I was lucky to be awarded a Nuffield Fellowship for 1956–7. I had been fortunate to work under Pat Moran, to whom I owe a great deal. We did not always see eye to eye, but I have always regarded him as a scientist of considerable stature. Maurice Bartlett was also a very eminent statistician, but with a completely different personality; he is a thoughtful person, very concerned with the depths of a problem, not so much with the mathematical technicalities as with the structures that lie underneath. I learned a great deal from him, and I am very grateful to him for the insights he gave me. I spent a year in Manchester and then came back to Western Australia, rising gradually through the ranks to Senior Lecturer and Reader.

I was very kindly allowed another year of leave in 1959 to go to Columbia University, New York; there I acquired more experience of graduate courses. My view is that Blakers, having been trained in America, was used to the more liberal conditions and regulations prevalent in American universities. American universities will usually let you take reasonable leave provided they do not have to pay your salary during the period. Australian universities at the time were somewhat more rigid in their regulations. But Blakers adopted the American attitude; he also held the view that anything you learnt while you were away, any experience that you gained, would eventually prove useful to the department in Western Australia. So he ran his department in a much more enlightened manner than many other professors in Australian universities. He was genuinely interested in the development of mathematics, and I persuaded him to join me in a survey of the state of mathematics in Australia; we wrote a paper together in the *Universities Quarterly* in 1959 which we both enjoyed.

**Heyde:** Later on, in 1963, you wrote a book about *The Condition of Science in Australian Universities*.

**Gani:** Yes, I got carried away with the development of science in Australian universities; I do not think I will ever be fully forgiven for taking the book to work on at home in the evenings! It is the only book, so far, that I have written on my own; I have edited books and written sections of books, but I have not completed another book (yet). I don't think it is entirely fair to one's family to work in the office during the day, and then at home again in the evening; one misses out on a lot, but I only realised that later.

**Heyde:** Were your interests in stochastic processes and modelling broadening out quickly at that stage, or were you still mostly interested in storage and related topics? When did you get into epidemics, phages and literary text modelling, as well as other sorts of stochastic modelling?

**Gani:** What happened to me in Manchester was precisely a broadening of my interests. I continued to work on storage and queueing for several years, and have maintained my interest in these areas. I have recently written a short paper on this topic. I started with it in 1954 and have continued to return to it from time to time. For example, I worked with Uma Prabhu in 1956–7 and later; we considered time-dependent dam and queueing models in continuous time, rather than the stationary models which Pat Moran was initially interested in. But in Manchester I also became interested in the epidemic work that Bartlett was doing. Later in the 1960's I became more concerned with biological and medical models, and rather less with queueing and storage models.

**Heyde:** Was that partly because of the influence of your wife, Ruth, who was a biologist by training?

**Gani:** Yes, it was she who sparked off my interest in genetics. Pat Moran had written a book on mathematical genetics in 1962, *Statistical Processes of Evolutionary Theory*, which I found illuminating. I became interested in biology, realising that it had reached the stage where mathematical models of biological phenomena could be very useful. So in the 1960's, my interests became focussed on bacteriophages and epidemics. I was fascinated by general biological models and wrote a paper as late as 1975 on a model for lymphocytes in the DNA cycle. Ruth helped me quite a bit with this and should have been a joint author; however, she decided that she was too busy to get totally involved, although she gave me a great deal of excellent advice. The paper was really a joint effort; I regret that it does not carry her name.

## SOME RESEARCH CONTRIBUTIONS

**Heyde:** With the benefit of hindsight, what do you think were your most significant research contributions? Would you include your exact solution

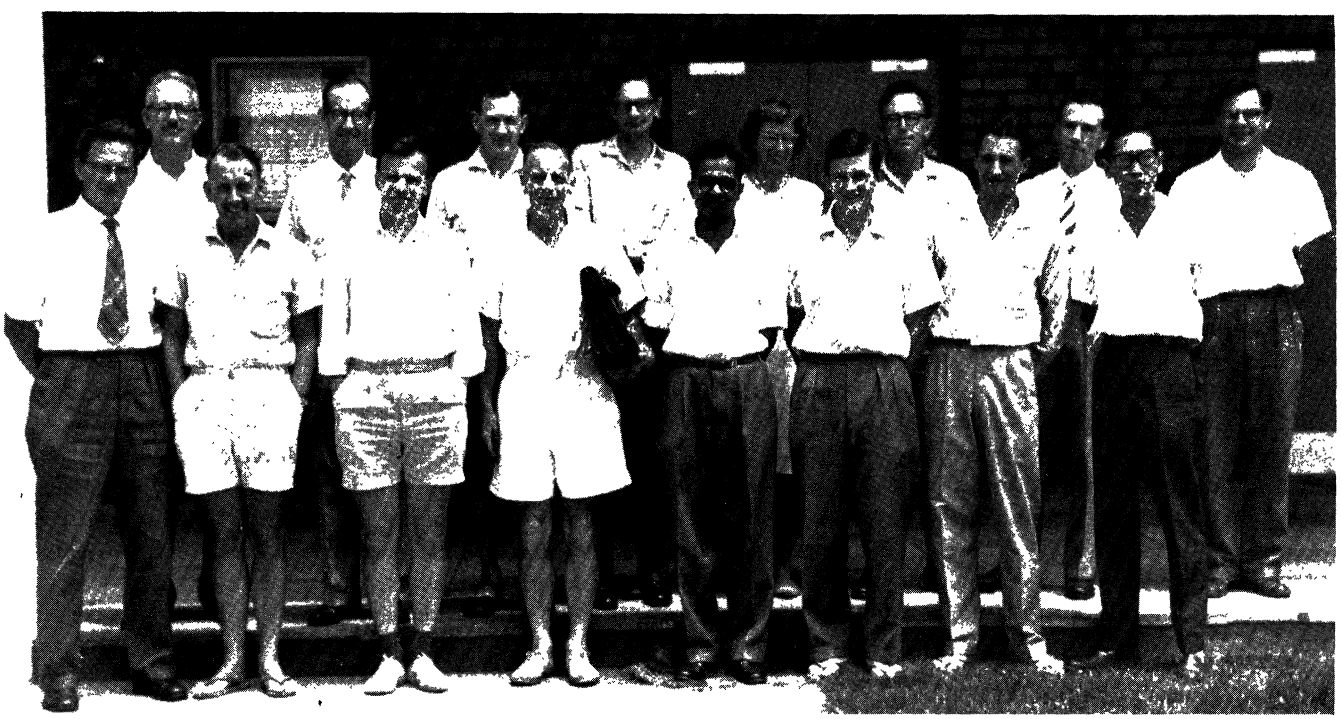

FIG. 3. *Australian Mathematical Society First Summer Research Institute, January* 1961: *first row, E. J. Hannan (second from left), T. M. Cherry (fourth from left), S. R. Adke (fifth), G. F. Yeo (sixth); second row, E. J. Williams (second from left), J. Mahony (third), J. Gani (eighth).*

to the general epidemic equation and your work on projections of hierarchical populations?

**Gani:** It is difficult to answer such a question without serious misgivings: perhaps the best I can do is present my views as factually as possible. I began my research in 1954 on two distinct topics, Moran's theory of dams [3] and inference on Markov chains [4]; both have remained lifelong interests. I worked for several years on various problems in the theory of dams [5] and queues, including first emptiness problems in both [9]. Perhaps the hardest challenge consisted of finding the transient distribution of a dam content subject to a gamma-type infinitely divisible input. Moran had previously found the stationary distribution, and in 1963 Prabhu and I [16] were delighted to work out the transient version. Another interesting problem was the development of a theory of dams in which the inputs were no longer independent and identically distributed, but formed a Markov chain. Ali Khan and I discussed this [1] in 1968. A more practical study in 1987, modelling both dam content and sedimentation, was the result of a joint effort with Todorovic and Yakowitz [18].

The motif of inference on Markov chains recurs in my work on statistics in literature, of which I gave a brief account [12] in 1982. I became interested in the statistical study of literary texts in 1975, when it emerged that the Markov chain models used for type-token relationships [10] required validation. A fairly complete account of such methods applied to the works of several French authors, as well as Shakespeare, was given in [2] written jointly with Daley and Ratkowsky.

In 1963, in response to a demographic problem, I developed a hierarchical population model which is used very often [6]; this is regularly employed to predict the number of university students in an educational system. Bartholomew has studied it in greater detail in connection with promotion rates sustainable in a stationary system.

Perhaps my most useful work is in biological modelling; my research in this area dates back to 1962 when I became fascinated with bacteriophages. In 1965 I wrote a long review [7] on stochastic models for bacteriophages, before I began to concentrate on epidemic theory. In 1967, I presented a solution of the general stochastic epidemic at the Fifth Berkeley Symposium [8]. This seemed rather theoretical at the time, but computers have made its use practicable in calculating the final size of an epidemic, as Purdue and I [17] indicated in 1984.

Other work which I think may be of value is my paper with Jerwood [15] in 1971, showing that chain binomials were Markov chains. Also, my 1978 *Journal of the Royal Statistical Society* review of epidemic processes in discrete time [11], which outlined some threshold theorems derived with de Hoog and Gates [14]. Of my most recent research, I believe the paper [13] on carrier-borne epidemics with two stages of infection has been the most complex, and the recent Gani–Yakowitz model [19] for the spread of HIV among intravenous drug users the most practically useful. And, of course, my current joint book project on epidemic modelling with Daryl Daley gives me a lot of pleasure.

## THE APPLIED PROBABILITY TRUST

**Heyde:** At about that time you must have been beginning to get an idea of starting the applied probability journals? Or was that a little bit later?

**Gani:** I think it was late in 1960 that I joined Pat's department at the ANU as a Senior Fellow. I left the University of Western Australia reluctantly, but the temptation of full-time research was too great. It was becoming obvious from my own and my colleagues' experience in the ANU department that papers on stochastic models which were sent to *Biometrika* tended to come back with a note saying "Sorry, but this paper is too mathematical; you should submit it to the *Proceedings of the Cambridge Philosophical Society* or some other mathematical journal." If you then submitted to *Proceedings* you might easily get the response "Sorry, but this paper contains too much statistics; you should submit elsewhere."

It was clear that the field of applied probability, namely, the art of building probabilistic models of all kinds of real-life situations (e.g., dams, queues, biological models, epidemics), was falling between two stools. The topic had initially been defined by a symposium of the American Mathematical Society held in the later 1950's; the term "applied probability" had then become popular because Maurice Bartlett had used it in the Methuen monograph series on *Applied Probability and Statistics*, of which he was Editor. But applied probability did not, as yet, have established publishing outlets.

It seems that one had to have a clearly statistical paper to be published in *Biometrika*, or a clearly mathematical paper to get into the *Proceedings of the Cambridge Philosophical Society*. If a paper consisted of a mixture of probability and statistics, then it might be rejected by both. So I started to canvass the idea of starting a *Journal of Applied Probability*; it would have been about 1962 that I went on a short sabbatical and talked to colleagues in Britain and America, extracting promises of editorial help from them. I came back to Australia hoping that I would get financial support for what

would be a great Australian venture. But I was disapppointed; I could not get any support from Pat Moran, probably because he believed that this was a rather foolhardy venture. His attitude was that journals should be started only by people of the stature of Karl Pearson, and he felt, quite correctly, that I did not have that sort of stature. I think that he may have been discouraged by the complexity of the job.

I then went on to the Australian Mathematical Society and they turned me down flat. I also approached the Australian Academy of Science and they turned me down too. I decided that this was not a very sensible way to attack the problem. At that time David Kendall, who had heard of my efforts, wrote to me saying that I should approach the London Mathematical Society. So I flew to Britain and spoke to the Council of the London Mathematical Society (LMS). At that time the LMS treasurer was Sir Edward Collingwood, of the famous naval family; he was a mathematician and a person of broad interests who thought the journal was a good idea. Between him and David Kendall, they persuaded the London Mathematical Society Council to give us half the money required to start it. Four people became trustees of the Applied Probability Trust: one was the representative of the LMS and that was Sir Edward Collingwood; the other three were Ted Hannan, Norma McArthur and myself.

The Australian trustees provided half the money, while the other half came from the London Mathematical Society. My memory is that the total was £4,500; our funds were supposed to see us through the first year, even if we did not sell a single issue. As it happened the first issue appeared in June 1964; by that time I had left ANU to go to Michigan State University. We have not had any serious financial problems in the past 30 years. It is true we have had some losses in eight of our financial years, but from about 1968 on, the Trust has been reasonably sound financially. The journals obviously fill a niche in the area of probability and its applications.

**Heyde:** The time was obviously right for such an enterprise.

**Gani:** The time was indeed ripe. Let me add that while the Trust requires good management, I think it also needed good luck. We had a good deal of luck in starting the Trust at the right time.

**Heyde:** So you were starting the *Journal of Applied Probability* (*JAP*) just before you went from Michigan State to the Manchester–Sheffield School?

**Gani:** Yes, most of the initial work was done in Canberra in 1962–1963; Cynthia Bartlett helped me with the early organization of *JAP*. I found the lack of support in Australia very discouraging, and being somewhat hot-tempered I decided that I was not going to put up with it. So I accepted a job at Michigan State University (MSU) and left Canberra in 1964; I recall your joining me there, and both of us were at MSU for about 18 months before going to Sheffield. I took the journal with me from Australia to MSU, where I was assisted by Alice Spier, and then from MSU to Sheffield in 1965.

## NINE YEARS IN SHEFFIELD

**Heyde:** What, particularly, prompted you to take a job in the U.K. rather than staying in the United States?

**Gani:** You may recall the problems we had at MSU when we were trying to establish courses in stochastic processes. We were met with a lot of resistance, and it seemed to me wasteful to spend our energies overcoming the many hurdles in our way, whereas Sheffield offered an open field. There was hardly any staff there in 1965, so anything that could be built up could be done with far less difficulty. In fact, I spent nine years (1965–1974) in Sheffield and look back on that time as one of the most constructive and happy periods of my life. The vice-chancellor of the time, Jack Whittaker, made me a certain number of promises all of which he honoured. I often tell the story about his being the only person who ever interviewed me and made promises, not in writing but by jotting them down on what seemed to be the back of an envelope, later delivering every single one of them to the letter. That kind of scrupulously honourable behaviour is, sadly, pretty much a thing of the past.

**Heyde:** Very, very rare.

**Gani:** That's right; plenty of promises are made on paper today which are not honoured. I was absolutely delighted with Whittaker; one of the things which he did for us was to provide some minor funds to help the *JAP*. That was the only applied probability journal published at the time; the next journal, *Advances in Applied Probability*, was started in 1969, five years after the first. The funds allowed me to employ one research assistant, and that was Mavis Hitchcock, who retired in 1992. Later she became a full-time employee of the Applied Probability Trust, paid with Trust funds, but initially her research assistantship was provided by Sheffield University; the university was very good to us.

Although there were the usual difficulties in starting courses and developing the Department of Probability and Statistics, the atmosphere was favourable and the time was right; in fact, as you probably recall from your stay in Sheffield, we es-

tablished ourselves very rapidly. In 1967, after I had been in Sheffield for two years, the University of Manchester lost Peter Whittle to Cambridge and made some tentative approaches to me. I thought I should not leave Sheffield after only two years, so I talked to the Vice-Chancellor of the University of Manchester, and made the suggestion that a joint Manchester–Sheffield School of Probability and Statistics be created. This would initially allow Sheffield to help Manchester out, but Manchester would gradually become autonomous while continuing to collaborate with Sheffield. The idea was that we could save on faculty by having joint master's examinations, so that we need produce only one set of papers for both Sheffield and Manchester and use subject specialists to teach advanced courses in both places. Somewhat to my surprise the University of Manchester agreed to this, and the joint school worked out as a very interesting and useful experiment.

**Heyde:** It was a surprise for such a conservative organisation.

FIG. 4. *J. Gani in office at the Department of Probability and Statistics, University of Sheffield,* 1968–1969.

FIG. 5. *Department of Probability and Statistics, University of Sheffield,* 1968–1969: *front row, Mavis Hitchcock (last on right); second row, C. C. Heyde (fifth from left); third row, K. R. Parthasarathy (second from left), E. J. Hannan (fifth from left), H. E. Daniels (sixth from left), J. Gani (last on right).*

**Gani:** I was surprised, but also delighted at the outcome. I think the motivating force was Manchester's recognition that it would have been left without any statisticians at all. The only statistician left in Manchester's Statistical Laboratory was Richard Morton, who was himself preparing to leave Manchester. I persuaded him to stay on, and you will recall that you went to Manchester in the first year of the school, to take charge of the Statistical Laboratory. After you, K. R. Parthasarathy went from Sheffield as full professor to Manchester, and thereafter things went from strength to strength: we rarely had any trouble there. Sheffield helped Manchester out in the later 1960's and the relationship still persists; there is now full autonomy of individual departments, but the Manchester–Sheffield School has become enlarged and other institutions have joined it, including Salford and Keele.

**Heyde:** But Sheffield became what may have been the largest Department of Probability and Statistics in the U.K.; and if you look at the Manchester–Sheffield School in the whole, it was a very large and influential group and clearly so successful that you must have been really rather reluctant to leave it eventually and follow up on possibilities elsewhere.

**Gani:** Well, to be quite honest, it is easy for people to take credit for what, in effect, are accidents. It is true that I had put in a lot of work into Sheffield and Manchester; you and Parthasarathy, Bob Loynes and others also contributed tremendously. But the time was right: it was still the period of university expansion in Britain, so it was relatively easy to get support for departments which performed well. Grants were readily available for graduate students; provided they achieved a certain standard, and we benefitted from these advantages. So when I left Sheffield and Manchester at the end of 1974, we had probably gone through nine of the best years in the history of universities in Great Britain. Of course, many colleagues contributed to the development of the school; obviously, there was a lot of work to do, but we must not underrate the favourable conditions of the time. If the same thing were to be done now, I think we would have a great deal more difficulty because of the shortage of finance, the increased accountability of universities and the problems of recruiting graduate students; conditions are very different today.

**Heyde:** This is not an expansionary period.

**Gani:** This is a very different period. But we had a great deal of fun at the time, and we showed what could be done with a little imagination in two redbrick universities, away from the big centres like Cambridge and London. With a bit of foresight and enterprise, we were able to create the Manchester–Sheffield School, which I think was a great success.

**Heyde:** And even still is.

**Gani:** Yes, relatively speaking: I believe Sheffield is still one of the larger departments of probability and statistics in Britain. I am not quite sure which is the largest, but it is certainly one of the larger ones.

## THE CHALLENGE OF CSIRO, AUSTRALIA

**Heyde:** So you came back to Australia almost by accident, I believe. You were asked to give advice by CSIRO and that eventually led to a job offer. You might like to say a little about this.

**Gani:** CSIRO is the government research organisation in Australia. My memory is that the after the death of A. E. Cornish in 1973, Victor

FIG. 6. *European Meeting of Statisticians, Budapest*, 1972: *I. Vince, K. Sarkadi, J. Gani, A. N. Kolmogorov and H. Chernoff.*

Burgmann, who was a member of the CSIRO Executive, travelled around the world soliciting advice on who should be appointed as the next Chief of the CSIRO Division of Mathematical Statistics, as it then was. My experience of the Australian academic scene, particularly in the years just before 1964, had been rather unhappy. I sensed that there had been resistance to what I considered to be useful, innovative ideas, and I was very wary of becoming involved again in the Australian scene. Victor Burgmann tried to get me to apply for the job of Chief of the Division but I declined. After the first round, CSIRO appointed Dr. J. A. Barker, who was working for IBM in San Jose in California. I believe he accepted the job, came out to Australia and shortly after returned to IBM. I am not sure exactly what happened, but certainly something seemed to have gone awry, and Victor Burgmann went on a second round of international travel and consultation. He talked to me as well as various other people, but he could not stimulate the level of interest in the position that he had hoped for.

I had always felt rather badly that I had an Australian passport and had spent many years in Australia, which I considered to be my country, but had never been able to do very much for it. Every time I tried to do something imaginative, people felt I was getting uppity and kept trying to put me down. I still wanted to help and eventually agreed to come out to Australia and survey the Division, with a view to giving some advice on how it might develop. So in early 1974, I think it was February 1974, I flew out to Australia to visit CSIRO and review the Division of Mathematical Statistics. After visiting its various outposts in six states, I made a lengthy submission to the CSIRO Executive, explaining that Australia was changing from a predominantly agriculture-oriented country to a much more industrialized one. It was not enough to encourage agricultural and biometric statistics, which was the great strength of the CSIRO Division at the time; to deal with Australia's problems, it was necessary to move into industrial statistics and, more generally, into mathematics, both applied and computational.

To my surprise, with some minor modifications, the CSIRO Executive not only approved the plans that I put forward, but wrote back to me to say that they thought I was the only person who could carry them out. This was flattering, and it was very difficult to say no to their offer. I could see that I could do a great deal of good if I was allowed to carry out the reforms and follow up the directions that I suggested. I felt I could at last be of some use to Australia, so I came back to Canberra in August or September 1974 to head the CSIRO Division, which was then renamed the Division of Mathematics and Statistics (DMS). For seven years until 1981 I spent my time implementing this plan. We developed computational mathematics; we formed a group in applicable mathematics; in statistics, we moved towards statistical modelling and statistics in industry. We were able to launch SIROMATH, an autonomous organisation linked with DMS to help with mathematical and statistical consulting in business and industry; this started off under Richard Cowan and was later headed by Richard Tweedie, who has now moved to Colorado State University.

**Heyde:** Richard Cowan is currently Professor in Hong Kong.

**Gani:** Yes; he had many excellent ideas, but after an initial year when things became stabilized, the job of manager of SIROMATH was advertised and Richard Tweedie was appointed.

All in all, DMS flourished, and I found the job of Chief very challenging; it seems to me that we had many successes. We collaborated with the universities; there was a joint CSIRO–Universities Visitors' Scheme, funded by both collaboratively. We helped in a variety of other ways with the development of the universities' statistics and mathematics programs; we ran conferences; we seconded DMS staff to departments and offered university faculty visiting positions. In fact it was a very exciting time.

**Heyde:** Towards the end of this period, circumstances were changing somewhat in CSIRO; the organisation was becoming more inward looking and there was obviously less enthusiasm for the sorts of programs that you had been implementing.

**Gani:** My memory is not exact, but I believe that a new CSIRO Act was passed by Parliament in 1979. The government began to scrutinize more seriously what it considered to be the accountability of CSIRO; directives were obviously being received from government that CSIRO must fulfil this or that condition, or must pursue this direction but not another. The head of CSIRO at the time, Paul Wild, a first-rate radiophysicist, found it difficult to cope effectively with the new political climate. Very soon the whole of CSIRO was to be altered quite radically; we went through a review of the CSIRO Division of Mathematics and Statistics in 1981. You were there at the time yourself, and you will remember how unsettling it was. All of us in DMS thought that we had done extremely well and were disappointed with the review recommendations.

**Heyde:** We did do well.

**Gani:** The Review Committee acknowledged that we had done extremely well between 1974 and 1981, but told us that that was not what was now

required of us. We would need to change our direction, narrow our field of enquiry and return the Division from the research and consulting unit recommended by the Executive in 1974 to a much more project-oriented and consultative role. The committee wished to persuade me to continue as head of such a division. Rightly or wrongly, I felt rather upset by what had happened, and pointed out to the committee that it was very difficult for me, after seven years of encouraging members of DMS to become researchers, to go back to them and report that we were now going back to our role as consultants, with research placed on the back burner. I felt it would make a mockery of my leadership: I decided that the correct thing for me to do was to leave CSIRO. I went for a short visit to the United States, and got a couple of offers...

## BACK TO ACADEME: KENTUCKY AND CALIFORNIA

**Heyde:** And you went to Kentucky?

**Gani:** Yes, I went to the University of Kentucky, in Lexington. The Department of Statistics there was well established; in fact they just celebrated their 25th anniversary in April 1993. It was set up by Dick Anderson, who had created a department which was essentially statistical, with some strong links to the College of Agriculture; what I tried to do in my four and a half years there was to consolidate it and introduce some courses on stochastic processes and probability, as well as create a statistical laboratory. It seemed to go quite well, and I was reasonably happy with it.

**Heyde:** Tell me, how much support did you have from management within the university with the sorts of things you wanted to do there?

**Gani:** The administration started off being very helpful but when it came to the crunch and finance became difficult, as it did towards 1982–83, it reneged on the promises which it had made in writing. One of the things I was unable to build up to full strength was the Statistical Laboratory.

**Heyde:** A consulting laboratory?

**Gani:** Yes, a consulting laboratory; Dick Kryscio of the University of Northern Illinois helped us to do that. He is now chairman at Kentucky; my memory is that we were promised $50,000 in financial help to start the Statistical Laboratory, but when we needed it, it was no longer available. I was not very happy about that and did not know exactly what to do at that stage. I was becoming a little cynical about the promises made to me.

**Heyde:** Did you think of moving at that stage?

**Gani:** I was pretty angry at the lack of support, but I did not act immediately. I was 60 years old and wondering whether perhaps the time had not come for me to think of early retirement. What in fact happened was that I received an offer, just out of the blue, from the University of California at Santa Barbara (UCSB). They must have heard

FIG. 7. *At the Division of Mathematics and Statistics, CSIRO, Canberra,* 1979: *J. Gani making a point to Paul Wild (Chairman) and Tom Ferguson (Executive Member).*

about my work at Kentucky, and they offered me the chairmanship of the Statistics Program within the Mathematics Department. The intention was to convert this into a department if the Program was able to gather enough strength. So I went there in 1985 with this objective in mind, and I am very glad to report that after four years the Statistics Program became the Department of Statistics and Applied Probability at UCSB.

**Heyde:** So it would have more than doubled in size when you went there.

**Gani:** Yes, when I went to UCSB, there were basically two and a half professors on the faculty: Sreenivas Jammalamadaka, James Robertson (joint appointment with Mathematics) and Milton Sobel. We began to build up the department. I think we now have about eight or nine tenure-track or tenured positions and three or four visitors, so the group has become quite large. In southern California, I understand it is considered one of the strongest statistical groups, while within the University of California it is among the three main statistical centres.

## LOOKING BACK

**Heyde:** So what do you think have been the most personally rewarding things in your career? Would it be the applied probability journals or the Manchester–Sheffield School? What has given you the most pleasure?

**Gani:** I will try to be honest. The thing that has probably given me the most pleasure throughout my career is the collaboration of many of my colleagues, yourself included. I enjoy working with people: I think that if you have certain goals and explain them, so that your colleagues are on your side, you can achieve much more than through the uncoordinated efforts of the individuals involved. I have been very fortunate in that respect, both in Manchester–Sheffield, in the CSIRO Division of Mathematics and Statistics in its golden years and at Kentucky and UCSB. Obviously no job is without its difficulties, and you always get the occasional uncooperative or difficult person to deal with; but I really feel I have been very lucky in enjoying the collaboration of so many of my colleagues. That is the first point I should like to make.

The second is that I enjoy both teaching and research; I seem to be one of these people who is never happy just doing a single thing at a time. So I have always aimed to spread by contributions among three main areas: (a) research; (b) teaching undergraduates and training graduate students; and (c) establishing scholarly enterprises which are going to be of lasting value. Among these enterprises are the applied probability journals; the Applied Probability Trust has proved surprisingly successful. It has been the one thread that has run through my working life consistently from 1962–1963, when I started planning it, till now. At times when I became depressed or upset, as I did when things didn't work out as I had hoped in DMS or elsewhere, the fact that the journals were doing well provided me with something to lean on.

I felt that if I was the person who could successfully direct the journals when their relevant operational criteria were essentially objective, my judgment could not be too defective. For example, did I judge the market correctly? Was I providing the applied probabilists with papers that were worthwhile reading? Was I running the Applied Probability Trust office in Sheffield on reasonable lines? On the whole, these aspects of the Trust activities were going well. If I was indeed the person responsible for these results, I could not be the same person who was being criticized by the members of the CSIRO Review Committee of 1981 for failures of judgment about the CSIRO Division of Mathematics and Statistics. I thought that, after seven years of experience, my understanding of conditions in DMS and Australia in 1981 was more likely to be correct than the Review Committee's after their brief survey. I had an extremely bad time after I left DMS, which had meant a great deal to me. It took me about three or four years, almost all my period in Kentucky, to get over the unpleasantness and trauma of the 1981 review.

I felt as if I had been guilty of not understanding Australia's needs, of being too stiff-necked to compromise about future directions and so on; the history of DMS since then has, if anything, proved my initial estimate of the situation to be right. You may recall that I was followed as chief of DMS by Terry Speed, who left for Berkeley after two or three years; he was then followed by Peter Diggle, who also left for Lancaster after a year. By then, DMS and CSIRO had changed so much that the Division required somebody quite different to lead it; I am delighted to see that Ron Sandland is the right person for the present job. He has an excellent managerial style, is close to the problems of the new CSIRO and has successfully retrieved what could be retrieved of the Division of Mathematics and Statistics.

**Heyde:** But now it has a strong project-orientation with an industrial flavour.

**Gani:** That's right; it must now get 30% of its funds from external sources, and the biometric (consulting) sections have been hived off. It is a completely different division; in our time DMS had a scientific orientation and what mattered was the

quality of its research. Now it has a much more mixed objective; this is perfectly legitimate, but it may not be the sort of objective that would appeal to the earlier generation of research workers.

**Heyde:** It is not the sort of thing that you would have wished to head up in exchange for a job with the Manchester–Sheffield School?

**Gani:** No, my view is that the CSIRO that we had until 1979 was very much a scientific organisation, proud of its research and its contribution to Australian agriculture and industry. We now have something very different. I do not think one could say it was better or worse, it is just different and much more project-oriented, much more commercial. Many scientists find it difficult to work under the constraint of projects, except perhaps in time of war.

## STATISTICIANS AND SCIENCE

**Heyde:** Do you see yourself first and foremost as a scientist with general qualitative interests?

**Gani:** Yes, I see myself as a general scientist with some talent for organisation, and with ideas which are somewhat beyond the strictly scientific. My feeling is that if you are a scientist of any kind, whether an applied probabilist, a statistician, a mathematician, a geneticist, a chemist or whatever, one of your purposes is to contribute to your society. How can you contribute to your society? If you are a genius, obviously you must contribute through your deep gift, as Mozart, Proust or Einstein did. Unfortunately not many of us are geniuses; some of us are talented, and what we have to do is try to understand the direction of our talents and follow where they lead us. If our talents are mixed, then we should rationally divide our time between them. For example, there are people who are gifted scientifically and also as organisers, like Robert Oppenheimer. He was an excellent physicist but also an excellent organiser; he was able to put both his talents to good use in the Manhattan Project.

Of course, many people only value a single talent and they may prefer to concentrate on it; but I have always felt that one should try to be a rounded human being and do more than one thing in life. I have been fortunate because I have been reasonably productive in my scientific research and reasonably successful with my organisational work; I have also tried very hard to harness my energies to the service of the society in which I am working. For example, I attempted to do something constructive in Manchester–Sheffield, and I think I made a contribution there. I tried to be useful in the Australian context and I think I achieved something worthwhile in the CSIRO Division of Mathematics and Statistics. More broadly, I endeavoured to serve the international community of probabilists by creating the Applied Probability Trust and launching the applied probability journals.

**Heyde:** And it has been very successful.

**Gani:** I suppose it has been reasonably successful, and I would be foolish to complain about my lot. But of course, being human, I do complain; my wife, Ruth, pulls me up on occasion: "What are you complaining about? You have had a very fulfilling life," she says. And I must confess that I have; it has not been without stress and difficulties, but I think it has been very rewarding.

**Heyde:** You see yourself very much as a general scientist as well as an applied probabilist; you have obviously taken a broad view of these things. Would you like to comment on your general stance?

**Gani:** I think I would be classified as a general scientist. Of course, I have been trained as a mathematician, and I use mathematics as a tool to categorize, describe or analyze certain problems. For example, at the moment I am working on epidemic models, particularly those connected with AIDS; but as I indicated earlier, I have also been interested in queues and dams, statistical linguistics, various types of biological and genetic models, viruses, inference on Markov chains, the development of mathematics in Australia, climatology, sedimentation and soil erosion. One of the problems which we encounter with some of our graduate students today is that their interests tend to be rather narrow. They learn a certain set of techniques and apply them within limited boundaries; they sometimes forget that there is a whole wide world of expanding science out there, and if one loses touch with it, one loses touch with some very original and fascinating ideas. Look at our colleagues Warren Ewens and Motoo Kimura and their contributions to mathematical genetics: they have applied probabilistic methods to a rapidly expanding field. Likewise, in the area of epidemics, Roy Anderson and Robert May have applied their mathematical and biological knowledge to the modelling of epidemics.

I think that, today, we need to remind ourselves that our work in mathematics or statistics is set within the framework of general science; if we forget that, we are likely to miss some of the most penetrating ideas of our time. You are well aware of that, because you have yourself worked on problems in biochemistry and chemistry; losing contact with scientific developments means that one loses sight of a large number of interesting and deep ideas. May I say here that I owe something of that viewpoint to Pat Moran; he was also very interested in general science, and I should like to give

him credit for the ideas which are kept alive through his students.

**Heyde:** Yes, his experience as a Scientific Liaison Officer in the Second World War brought him into contact with a very broad range of scientific activities; he took an interest across this spectrum, having to write reports, and I guess he carried that with him subsequently. Such attitudes tend to rub off on one's students.

**Gani:** I think breadth is very important. I haven't had a very large number of graduate students since I went to Santa Barbara in 1985; in fact, only four. One of them worked on search methods, the next studied epidemic models, the third worked with me on epidemics and AIDS modelling and the fourth is in the process of completing his Ph.D. I hope they have felt encouraged to maintain broad contact with general science, in which so many new ideas are cropping up.

FIG. 8. *Formal photo of Ruth and Joe Gani at conferring of Sheffield D.Sc. degree, July* 1989.

## APPLIED PROBABILITY: A LOOK FORWARD

**Heyde:** After something like 40 years in applied probability, starting as one of the founding fathers, how do you see the evolution of the discipline? We would, of course, note that many of the practitioners now are not belonging to mathematics and statistics groups but come from areas such as operations research or engineering schools or business schools.

**Gani:** I think that when a subject matures, it tends to have a variety of interest groups; it seems likely that, as so often happens with mathematical or statistical societies, these groups will separate out. For example, I can imagine this happening in the Operations Research Society of America. They have a very active group in applied probability, and it is likely that before very long they may ask to have either a section of the OR journal or a separate journal which is devoted to applied probability in operations research.

In epidemic modelling, for example, a large number of articles are currently published in the *IMA Journal of Mathematics Applied to Medicine and Biology* or in the *Journal of Mathematical Biology*. It seems to me that if the interest in epidemics continues to grow, it is not unlikely that we may end up with a specific journal of epidemic modelling which will contain some applied probability papers on epidemics. The number of research scientists in the world seems to be increasing; there is now a great deal more material written, as you will know from your editorship of the applied probability journals. These research scientists will want their special interests represented, and we can expect some breaking away of certain subdisciplines. To some extent I regret that; it would be nice to see papers in applied probability all gathered under only a few umbrellas.

**Heyde:** But it is very difficult to provide unifying threads to such diversity. Then, of course, people cannot keep abreast of what is happening in another slightly different subbranch of a slightly different discipline.

**Gani:** I think that what may happen with journals like the applied probability journals or the *Annals of Applied Probability* is that they will tend to concentrate on the central themes, the important directions of applied probability, and allow the more specialist papers to go wherever they are most appropriately housed. I have no doubt at all that there will always be plenty of material on the important processes of applied probability.

**Heyde:** The paradigms and the basic models?

**Gani:** Yes, one may have to apply some selectivity as to what these are. For a paper focussed on an

aspect of AIDS modelling, one might select a specialized journal for which the topic is appropriate, but if one writes about an epidemic model which may occur in a variety of situations and which stresses some basic principles, then the applied probability journals might be more appropriate. I think most research workers already make some attempt at selecting which journal is most appropriate for a particular paper. Perhaps we should both rejoice that there are so many journals publishing material in applied probability, because what it indicates is that applied probability has come of age.

**Heyde:** It is healthy.

**Gani:** It is extremely healthy. The subject has developed rapidly and it is broadening; research workers in the field very naturally want special publications for their particular interests. That is entirely understandable and I could not be more pleased.

**Heyde:** And you have plans for the future yourself? You are writing this book on epidemics.

**Gani:** What I find is that I am still very interested in research, but I do not feel quite as sharp as I used to. Errors escape me more easily, and my concentration is perhaps less focussed. Nevertheless I remain interested in research and I continue to work. I now have 249 publications, both technical and general, behind me, and several reports awaiting publication.

**Heyde:** So 250 is just around the corner?

**Gani:** Yes, and a little celebration is called for when that happens. But I might add, rather self-critically, that I regard as much as 90–95% of my work to be perishable. However, even if only 5% survives and is useful, that will be a contribution. The important thing is that I retain my interest in research: I enjoy it at a fundamental level; I like it; it's fun. What I enjoy most now is writing papers jointly with colleagues because, for one thing, they add so much to whatever I know.

The book on epidemic modelling which I mentioned earlier is a joint monograph which I am writing with Daryl Daley. He is, of course, very interested in epidemics himself and has contributed much to the field; he complements me in many ways, and turns up references I did not know of, as well as presenting alternative points of view. I find this very rewarding. I have read many of the source works in epidemic theory: I have got notes, al-

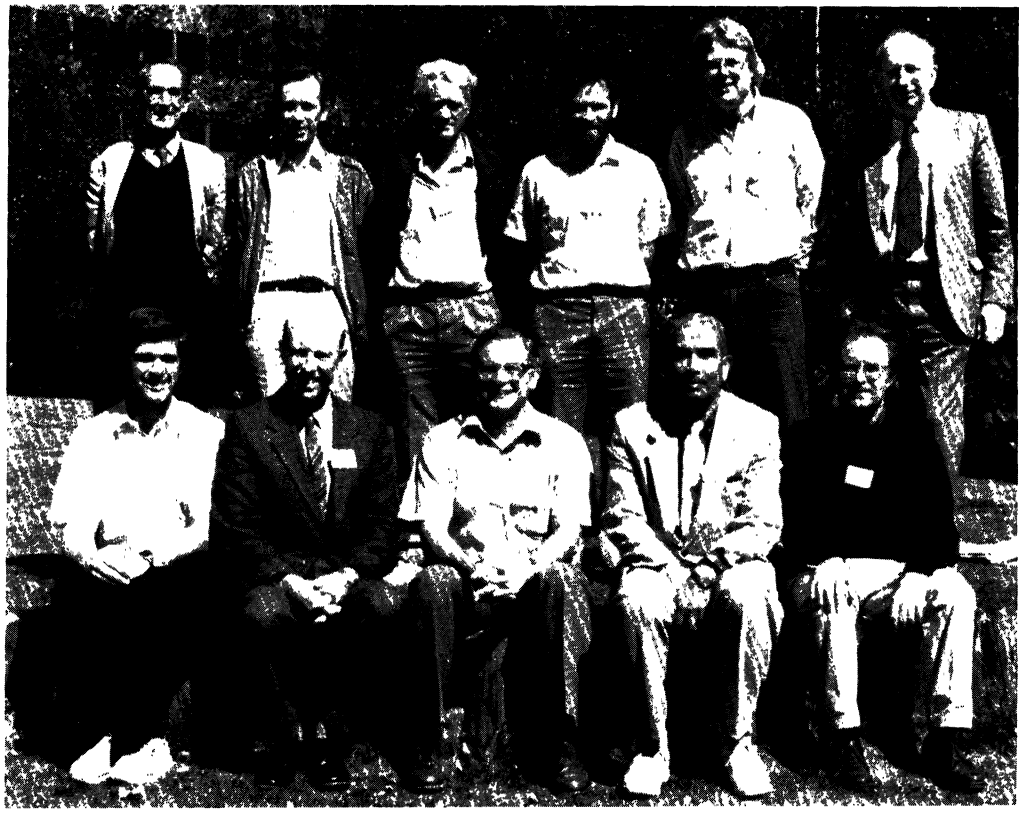

FIG. 9. *Sheffield Symposium on Applied Probability, August* 1989, *plenary speakers and organising committee: first row (left to right), R. L. Taylor, C. C. Heyde, J. Gani, N. U. Prabhu, C. Anderson; second row (left to right), D. G. Kendall, D. Gray, C. Cannings, J. Biggins, R. Durrett, R. M. Loynes.*

FIG. 10. *Applied Probability Office lunch party on* 23 *June* 1992 *for Mavis Hitchcock's retirement*: (*left to right*) *C. C. Heyde, M. Smith, L. Nash, P. Pritchett, M. Graham, K. Lyle, J. Gani, M. Hitchcock.*

though they are not very orderly, and what I should like to do is put the material down on paper before it begins to slip away. So I look forward to a very enjoyable "retirement," in which I shall remain as busy as ever, doing more of the things I enjoy and less of the things that I don't.

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