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A Conversation with Donald A. S. Fraser

Thomas J. DiCiccio and Mary E. Thompson

Abstract. Donald A. S. Fraser was born in Toronto in 1925 and spent his early years in Stratford, Ontario. His father and both grandfathers were doctors, and his mother was a nurse. He was a student at St. Andrew's College in Aurora, north of Toronto, before entering the Mathematics, Physics and Chemistry program at the University of Toronto as an undergraduate. He specialized in mathematics in the upper years and, in his final year, was a member of the winning team in the 1946 Putnam competition, standing among the top five competitors overall. For graduate studies he went to Princeton University to study mathematics, became interested in statistics and obtained a Ph.D. in 1949 under the supervision of Samuel Wilks.

He returned to the University of Toronto as an Assistant Professor in Mathematics in 1949, and stayed at Toronto for most of his career, becoming Professor in 1958 and first Chair of the Department of Statistics in 1977. He has held visiting appointments at Princeton, Stanford, Copenhagen, Wisconsin, Hawaii and Geneva, and is Adjunct Professor at the University of Waterloo. Following his formal retirement from the University of Toronto, he was Professor in the Department of Mathematics and Statistics at York University for several years. Currently at the University of Toronto, he is still teaching and supervising students. Among his more than 50 Ph.D. students are counted many university statisticians, and he has had a profound influence on the way statistics is thought about and taught, particularly in Canadian universities.

Professor Fraser is the author of several books, including *The Structure of Inference* (1968) and *Inference and Linear Models* (1979), and author and coauthor of more than 200 papers. He was elected a Fellow of the Institute of Mathematical Statistics in 1954, and Member of the International Statistical Institute and Fellow of the American Statistical Association in 1962. In 1967, he was the first statistician to be named a Fellow of the Royal Society of Canada. He was the first recipient of the Gold Medal of the Statistical Society of Canada, inaugurated in 1985. In 1990, he received the R. A. Fisher Award of the Committee of Presidents of Statistical Societies. His award lecture at the Joint Statistical Meetings in Anaheim that year was entitled "Likelihood and Tests of Significance: Linking the Fisher Concepts." In 1992, he accepted an honorary Doctor of Mathematics degree from the University of Waterloo. In 2002, the degree of Doctor of Science, *honoris causa*, was conferred to him by the University of Toronto.

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The following interview took place in Waterloo in June of 1999, with interviewers Thomas J. DiCiccio of Cornell University and Mary E. Thompson of the University of Waterloo.

DiCiccio: Don, you were an undergraduate at the University of Toronto. How did you come to attend Toronto?

Fraser: Well, I guess I was born in Toronto, though by accident! I came from Stratford, so Toronto was the obvious place unless one went to Western. The University of Waterloo didn't exist then, so it wasn't an option. I had been to high school north of Toronto, so I looked to the University of Toronto, along with some places that my family had had connections with—my father had gone to McGill and to Edinburgh—places where I might study medicine following in the family tradition. My parents were not reinforcing in that medical direction, and probably the fact that they were nonreinforcing had some reverse effect, making me feel that I should pursue medicine, but at a place where some other options were open. So I went to the University of Toronto, partly because I got scholarships in mathematics. Math was something you could think about, and intuit, and you could do it while you were doing other things, so it was always attractive.

DiCiccio: What program were you enrolled in?

Fraser: It was called Mathematics, Physics and Chemistry. They're now attempting to start an MPC program again, but the C is now, of course, computer science. The MPC program gave you contact with physics and chemistry. I think most people in that program who stayed on in math did not like the physics or the chemistry. I found those subjects kind of intriguing and they were a nice continuation from high school. I remember in the last year of high school having a rather extended dialogue–argument–discussion with the physics/chemistry teacher. He had expressed the view that you could pull down on a rope with a force greater than your own weight, and this did not make sense to me. Our argument actually went on for half a day and it culminated in the class taking a vote. The class was 100% in support of the teacher and 0% in support of my view, that you couldn't pull down with a force greater than your own weight without going off the floor. There was a bit of psychosocial message in that, how a majority could rule in science.

In a sense that experience allows one to understand some flow of thoughts in the discipline of statistics through the years, and maybe even in our present time, where a very dominant view can preclude or suppress a serious addressing of some alternative. As I have participated in addressing some nonstandard approaches, I do remain acutely aware of the force that goes with a dominant view in a discipline.

DiCiccio: Could you tell us something about your experiences in mathematics as an undergraduate at Toronto?

Fraser: The class that stayed on in math was not a big class, and had diverse interests, even to diamond cutting and polishing, and other idiosyncrasies. Some of them didn't pay much attention to the lectures and I was probably one of those. In one case, I recall I looked back over lecture notes before May exams and found that the poor professor had been explaining the same topic three different ways, and we had been so detached that we hadn't realized this. That professor was the notable Brauer who went on to Harvard. I met him years later and he was very cordial and I guess forgave me for being an inattentive student, but then algebra was always a stretch for me.

DiCiccio: Did you continue with chemistry or physics?

Fraser: Well, I think the chemistry never really attracted. What one was exposed to at that time seemed to be too much a succession of odd facts. Whereas in physics, there were the labs that a lot of people disliked and you had to go laboriously through something very detailed, but you came away with some feeling for spectroscopy and interference patterns and the like. I thought physics labs were great, both then at the time and later. The mechanics of doing the experiments often took a couple of hours, or the whole afternoon, and afterwards we could see that what we had done could have been done in a much shorter time. But it was sort of a necessary start-up.

DiCiccio: You were very successful in the Putnam exam. Tell us about that experience.

Fraser: I guess it was the first Putnam exam that had been held for quite a while. Thus, there was no information available from other people who had been in on it. It was something that was coming up and you were asked to do it. It sounded like fun but it wasn't something that you geared up for then. There had been something the day before, leading to a late night. But going in the next day it was a paper of intriguing questions and somehow they sort of clicked. And then there was the afternoon session. At lunch time, in returning a borrowed car, John Hilborn and I got lost. So after lunch I came in late and everybody else was started. Somehow with just the pressure of the moment and

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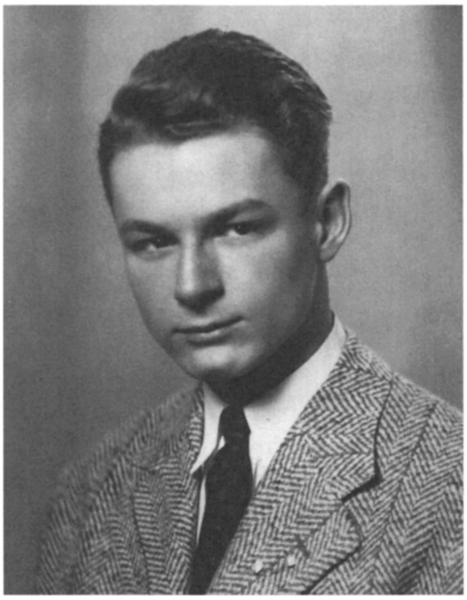

FIG. 1. *Fraser in 1946, the year of the Putnam success.*

adrenaline, I saw a question, bang! and started scrawling with a very scratchy pen and getting solutions. Here was this person, coming late into the room, scratching with a pen. Gilbert Robinson came over and offered me his pen, so that I could write more quietly! But it was a case of fun problems, and if you get a little bit of luck it reinforces things. What it probably means is you can come in at a certain estimated level and then things can trigger and you can be either much above that or much below that. Of course, there is luck in all things, but it was exciting. There were some good things that followed from it—the graduate work in the States.

DiCiccio: Did you do statistics as an undergraduate?

Fraser: There wasn't really statistics. There was a course in probability that used Uspenski's (1937) book and it created a kind of a mysticism about what statistics was. You didn't feel that you saw what the discipline was. It was sort of like you felt maybe if you put a lot of effort in it, you could see. The same was true with an applied statistics course at the graduate level—scratching my head, what's this all about? There must be some logic to it. It just didn't seem as though you were examining real things. The explanation of analysis of variance in books was in terms of a bunch of quadratic forms, but why did they decompose? At that time, nobody told you that you were projecting into subspaces, and differences in quadratic forms were quadratic forms for something in an orthogonal complement. I think a lot of books even now don't really tell you what's going on there, and if they did tell you, it would be a lot easier to see directions.

DiCiccio: What about your summer work experiences as an undergraduate?

Fraser: Well, I somewhere picked up the notion of working in insurance—perhaps from the program at the University of Toronto, where actuarial science traditionally had been a very big portion of the math department. (Now it's a small part of the statistics department!) I think the math and physics program attracted a lot of very mathematically able people, who flowed through actuarial science partly because there was a challenge to it. Maybe they did it because it actually had potential for applying some of the math they had, or maybe because downstream they could see that you could make a living and probably a very good one. So at the end of my first year I worked in a life insurance company downtown in the summer. The actuary in charge of one division was away on holidays, and the person who was looking after things gave me a lot of calculations of deferred annuities using commutation symbols and the like. It was neat to see how those calculations incorporated a bit of mathematics, making possible a lot of complicated computation based on fairly simple constructs. Of course, the amount of money I got was something like \$11 a week and living expenses cost me \$10.50 including streetcar fares, so it wasn't a profitable summer. Maybe that had something to do with why I didn't pursue some chances to go with some large life insurance companies in the States later on.

DiCiccio: And then subsequent summers?

Fraser: Then I worked for the actuarial faculty at the University of Toronto who did consulting on the side, evaluating pension funds using, in effect, large contingency tables. We were smoothing failure rates, mortality rates, withdrawal rates. There were some mechanics, such as recording age by age last birthday, age nearest birthday, or age something; these were necessary in order to lump things, put things into cells, but they seemed to require more mental effort than they deserved, for some reason. Probably because things couldn't be processed easily on a continuous scale and of course there wasn't a computer for the drudgery.

PRINCETON

DiCiccio: How did you get to Princeton for graduate work?

Fraser: Well, I had the chance to go to several places. Because of the results on the Putnam exam, an invitation came to go to Harvard. And I think Harvard had somewhat greater stature in the general view

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at that point. But there were people at Princeton that kept close contact with Coxeter and Gilbert Robinson at Toronto, so there was a bit more of a personal tie. I think probably there was direct contact with the Chair at Princeton at the time. Quite a bit later I did visit Harvard, probably in connection with an offer to go to MIT. The congested big city of Boston at that time didn't sit too well with me, and probably a little bit of that big city aversion had influenced my making the choice in favor of Princeton earlier. It turned out a close friend at the time was going to Princeton too, and that also influenced the decision to go there.

DiCiccio: What was Princeton like?

Fraser: Princeton was a great place. It was an exciting place and I guess I somehow migrated to the statistics group. There were grad courses in all areas of math that I took, but one was in statistics and it was very exploratory and loosely defined and very stimulating. I remember being in there, with a second or third year undergraduate who was taking a graduate course—Mel Peisakoff—and the two of us were competing to solve problems that Tukey had proposed in the course. As a point of interest, Mel was also refereeing at that time Wald's early papers on decision theory for *The Annals of Mathematical Statistics*. It was exciting. And then there was an interesting group of people at the graduate residence and you had a feeling of vibrancy and life and people concerned with something, going somewhere. The old Fine Hall was a very special place. The new Fine Hall, the big tower, it's a little bit segmented by floors. I have visited the new Fine Hall a few times because Andrea, my daughter, was doing her doctorate there in harmonic analysis under Eli Stein. She thought it was fine but it was not like the old Fine Hall—the chalk dust on the floors, the ivy you tripped over coming in and knowing everybody quite closely. You bumped into everybody at the old Fine Hall, whereas in the new building there must be people on staff who have been there for ages and haven't seen each other. For example, on one visit, I went to a totally different floor to say hello to Hale Trotter, whom I'd known from a visit to Princeton away back. I had been around there for quite a few days and hadn't seen him, although he was there in the building. If your times didn't click you wouldn't meet people. The architects were probably seeking to make some very special place, with these multiconnecting floors and elevators, but somehow it didn't fit in with the geometry that had driven the early Fine Hall. Maybe the whole future of Princeton might have been different with different architects, you don't know.

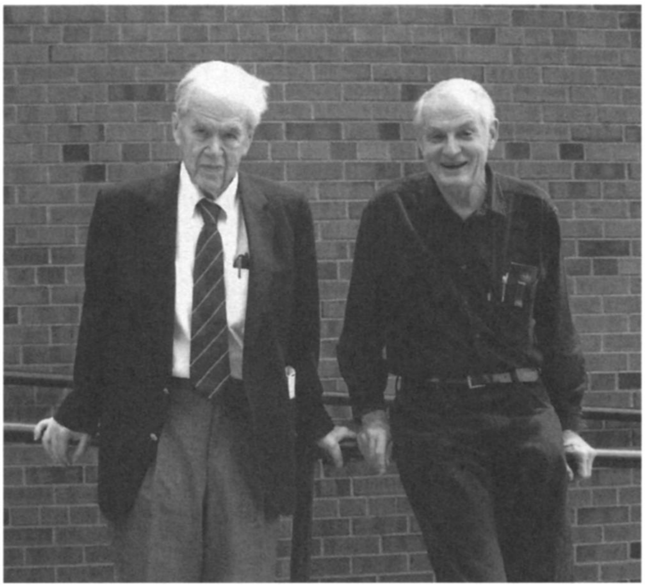

FIG. 2. *At University of Waterloo, 1999, with John Tukey.*

DiCiccio: Who was at Princeton when you went there?

Fraser: Well, Wilks was coordinating the statistics group. Tukey was there. A lot of the general instruction was given by graduate students, which was really kind of exciting. Now, it's very different because people pay big money for their kids to go to Princeton and they want them to get instruction by the big names at whatever cost. So they miss out on junior people who come in with all kinds of enthusiasm and may well at that stage be better at making first contact with ideas and students. In any case, I enjoyed it and got to know some of the people in the classes really well, and kept in touch with them later.

DiCiccio: What were your contacts with Tukey like?

Fraser: He was great, wonderful! Nominally my thesis supervisor was Wilks and he produced the problem, a fairly routine multivariate one, which I did work on. I guess I wasn't quite driven then the way I have been later; the discipline wasn't there. But Tukey always had suggestions or thoughts, and the thoughts were always very stimulating. They got one going in new directions which weren't always the direction to go. I think that's the way things are when you are exploring new directions. You have to try a lot of them and think about them, and not stop and lock up or insist that you must have an absolute reason for things. I think I tend to work that way now. I was very fortunate to have had close contact with John. And then meeting him here at Waterloo again was really great.

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DiCiccio: You said that later the discipline of statistics took hold of you, that you were driven in that direction. How did that happen?

Fraser: I don't know. I guess I tend not to be sure after the fact how these things evolve. I'd say I'm driven by curiosity, I've got to find out about things. To explore and the rest of it, and sometimes with some risk, like the example of disagreeing with your chemistry teacher to the point of obtaining a vote and then losing the vote completely. And yet, somehow that didn't suppress me. Those things can crush one, but fortunately that didn't happen to me.

DiCiccio: So part of the attraction of statistics was the element of risk?

Fraser: Yes, I think I was sometimes drawn to doing what you'd think of as risky things, taking chances. On a long canoe trip up in northern Ontario with John Hilborn, who later was quite high up in Chalk River and nuclear physics, we came across these rapids and there were logs, running logs that had jammed up in the chasm that the river flowed through. These logs would be 16–24 inches in diameter; and they weren't 16 foot logs: they must have been double that length, huge ones, 32 feet long and they were all in like matchsticks. Just looking at them, you sort of had the feeling that the balance was really very precarious. So I took a pole about three inches in diameter and started levering them. Well, I got my reward, they all went through. But one of them, one of these massive logs, swung on its pivot only feet over my head. You had this roar of air, of something going by, you came away feeling that but for randomness, or we'll call it randomness, you'd be a piece of hamburger. So there is risk in deviating from the norms of whatever it is. Twitching big logs in a rapids is "not a wise thing to do." But in any case, I'm here. I started as one of three children and I am the only survivor and, thinking about things like that, I think I'm perhaps very lucky.

BACK TO TORONTO

DiCiccio: When you graduated from Princeton, you went back to Toronto. How did that happen?

Fraser: Well, somehow I'd never really doubted that I would. Why did I want to come back to Canada? Well, I was born here, and I guess it's a bit of a national or cultural sort of thing. Certainly I did want to come back despite various opportunities to stay in the States.

There were job opportunities at MIT and then later at some other places. I did come back. I didn't regret it, particularly at the beginning time. It took a while before I really thought about other places seriously. I remember getting an invitation from Neyman, quite early on, to spend a visiting year at Berkeley. I was brazen enough to question the amount of salary offered and indicated that it wasn't really enough for such a big move and, boy, Neyman never spoke to me again. Well, that's not quite true, he did speak to me, but clearly I was, from then on, persona non grata with Neyman. And certainly my statistical directions had deviated from Neyman's.

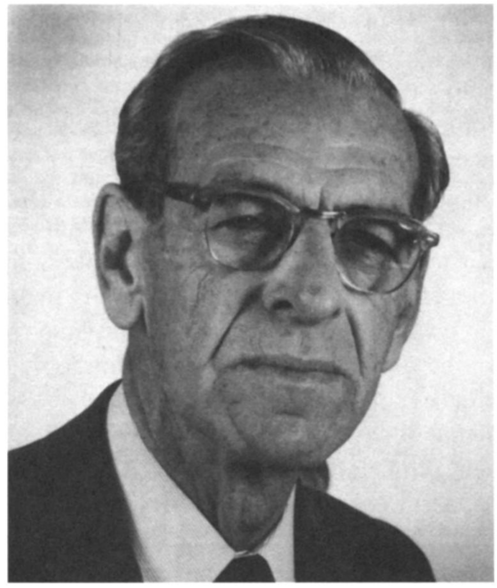

FIG. 3. *Dan DeLury, Chairman 1968–1975, Department of Mathematics, University of Toronto—a pioneer in statistics in Canada.*

DiCiccio: Had you by then moved away from the standard decision theoretic approach to statistics?

Fraser: I tended to do some nonstandard and nonconventional things, and certainly I wasn't a pure decision theoretic person. The work that Allan Birnbaum did, showing that conditionality and sufficiency implied likelihood, had a lot of influence. More recently, a group of us at Toronto have shown that conditionality alone implies likelihood. It's a matter of a set of formal relationships existing there in the center of statistics. A few people know about them and fortunately don't worry about them, because they would seem to say, "Boy, I can't do conditional inference without coming down to likelihood"—but Birnbaum's argument was very neat. The resolution is just that the whole of statistics doesn't depend on the assumptions that went into that argument, but it's really very elegant.

When Birnbaum spoke on that subject, it was at a meeting in New York City in 1960. There was real ex-

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citement at that time. It made people stop and think for once: here were these two things that got thrown around, likelihood and a little bit of conditionality and here they were more or less the same. At this meeting, Neyman got up and gave an introduction to decision theory, which was highly superfluous because everybody there was well aware of it. So, Neyman was really lecturing to Birnbaum himself for being so naive as to present these ideas seriously. Big conventions, as you know, are now kind of zoos. There are theme topics, but rarely do such meetings encroach on areas where there's controversy. When there is controversy, it creates an excitement, it gets lots of people involved.

THE NONPARAMETRICS BOOK

DiCiccio: Can we go to your book on nonparametrics (Fraser, 1957). How did that book come about?

Fraser: John Tukey had a paper, whose title I think had to do with nonparametric tolerance regions, and which followed a paper that Tukey wrote with Wilks. There was an intriguing bit of analysis involved with that topic which really caught my interest. You're talking about the probability content of a set. The set in turn is random, so you're dealing with the probability that the probability content is greater than something. The analysis was really beautiful and you could bring it all down to uniform distributions if you wanted to. In fact, that's how the basic proofs go.

DiCiccio: There's a lot of Neyman–Pearson theory in that book.

Fraser: Well, Neyman–Pearson theory was the background for the estimation and tests, and the treatment of that theory drew very closely on Lehmann's work. His printed notes were a big influence on many people. The Neyman–Pearson decision theoretic approach is just one portion of statistics now; perhaps a fairly big portion, a bigger-than-it-should-be portion in many places still.

DiCiccio: In the final chapters in that book, though, you were going in different directions. What were you thinking of when that happened?

Fraser: I was not consciously aiming at new targets as much as just thinking of how things build on themselves. There is a flavor in the book of some things that were developed subsequently, for example, the use of group structure in inference.

DiCiccio: So your interest in group structure developed while you were writing the nonparametrics book?

Fraser: The graduate students at Princeton were busy reading Fisher. In fact, Paul Meier was very strong on Fisher at the time and there had been a kind of study group about Fisher. I was led into thinking about fiducial. It seemed that there was something there, and yet we hadn't uncovered it, certainly not seen it clearly.

The irony is that what you get out of the fiducial argument is just a subset of what objective Bayesians get with objective priors. Fiducial in many people's eyes is clearly wrong, but yet objective Bayesian is acceptable. Probably, fiducial inference fell into disfavor because the claims made for it were too strong: If you make claims that are too strong, you evoke counterarguments, which then come out of the woodwork.

Thompson: What do you think it is that makes objective Bayesian seem more acceptable?

Fraser: The Bayesian method is loose and flexible in its structure. What it did was take statistics out of a stand-still in the mid-1950s. Then later MCMC (Markov chain Monte Carlo) meant you could calculate posterior probabilities, so suddenly you could do things and then you could worry about whether your prior was the right one or the wrong one afterwards, if it bothered you at all. Once you saw the numbers and got the printout, you had something, whereas before you had essentially nothing. So, there are some minimal claims there too, but they're fairly loose. If you make any kind of claims that sound like absolutes, you provoke all the conservatives with vested interests.

Thompson: Yet you found aspects of the fiducial argument compelling.

Fraser: The key to thinking about fiducial was using probability in a fixed space called an error space and doing transformations of that. There, the basic assumption is a probability space in the usual sense. I don't think many people realize that this is cogent and tight up to a certain point. Even now, if there is anything that looks like a claim that has a fiducial flavor, it brings out the heated arguments. Everybody knows that "fiducial's wrong," even to the point that the referees went wild on a recent paper when a colleague used a fiducial distribution to integrate out a nuisance parameter. Everybody knows "fiducial is wrong," in spite of the clear fact that the use of a fiducial method gave an extra order of accuracy to the resulting approximate distribution, and that's an objective asymptotic result. There is something kind of amusing about this, ironically amusing. It's as if the interplay of big things doesn't get examined because a red flag comes from somewhere. Maybe I see red flags even before they materialize, I don't know.

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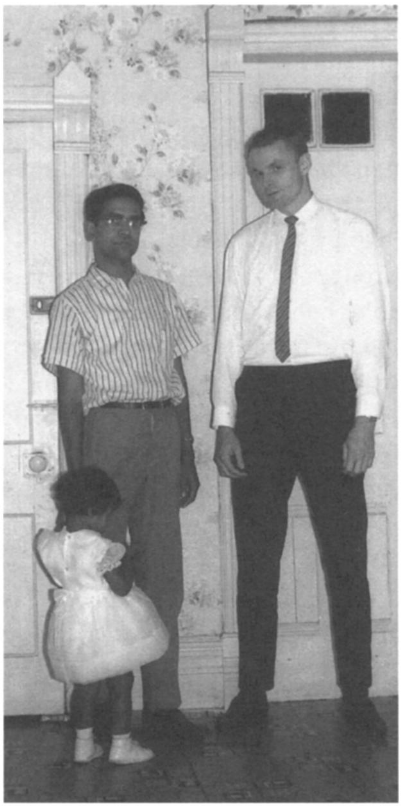

FIG. 4. *With Safiul Haq, 1965.*

STRUCTURAL INFERENCE

DiCiccio: How did fiducial lead into structural inference?

Fraser: Structural inference came from the same idea, namely that instead of having a class of probability distributions there should be a single probability distribution together with a class of transformations on that space. One of the intriguing questions then is, "When is such a class of transformations technically a function in itself?" The answer is, "If it is a group!" That became the so-called structural model, because there is structure, namely the parameter structure in the transformations on the space. It was intriguing and rewarding, and in terms of mathematical things, it dealt with continuous objects more than algebraic ones. My inclinations are more for analysis; a little of that must be something that's inherited, because my three daughters who have doctorate degrees in mathematics have all specialized in analysis, with minor variations like symplectic or contact geometry, or differential geometry or harmonic analysis.

The term "transformation parameter" comes from Mel Peisakoff, who was the undergraduate that was in the graduate class at Princeton way, way back. He'd examined those models from a decision theoretic point of view. Actually, his thesis was difficult to read at that time, compact spaces and so on, but it was very, very early on. I think it grew from Pitman's (1939) paper on location-scale models and estimation, which in turn came out of Fisher's work on inference for location and location-scale models, and had left people rather puzzled.

DiCiccio: How do you feel the structural inference book (Fraser, 1968) was received?

Fraser: There was a review by Lindley which was reasonably positive, but objected to the lack of Bayesian content. Lindley was "raked over the coals" by a lot of his colleagues for being complimentary. So I don't think it was received well. The book was probably judged as more fiducial type stuff which "everybody knows isn't right," so I don't think it got a fair evaluation. The real criticism is that it doesn't cover all of statistics. It's looking at a certain model context and saying what happens there. I'm quite aware that good friends, even very prominent ones, will say, "You shouldn't have statistics methodology that doesn't apply quite generally." I don't accept that view. I mean, things may be quite nice, where you measure parameters directly and everything is clean-cut, or the parameters may be built in and be very difficult to elicit, and to say that a method should apply to all cases is to lose sight of the fact that situations can be quite different. I think we should take account of such differences and structure (!), and give some acknowledgment to the rationality of flexibility.

DiCiccio: Did you design the cover of your structural inference book?

Fraser: The paper cover? Around that time I had close contact with a group of architects in Toronto, one of them being Ron Thom, who was quite well recognized across Canada, and a close friend and colleague of his, Brian Kilpatrick. Brian's artistic sense didn't quite agree with what John Wiley had suggested for the cover, so he made certain suggestions. I think the design person at Wiley went along with the changes. She was quite open to the suggestions. There was a way in which the writing on the cover wrapped around, and the only thing she didn't want was to see the letters stacked underneath each other. If they were going to be in line, they could be tilted up. And the day-glow color intensity, that provocative color? Maybe the book should have been a very low-key, deep, dark blue; maybe the red/orange was just too intense. I'm not consciously aware of provoking, but certainly that color choice has some such element and the profession is conservative.

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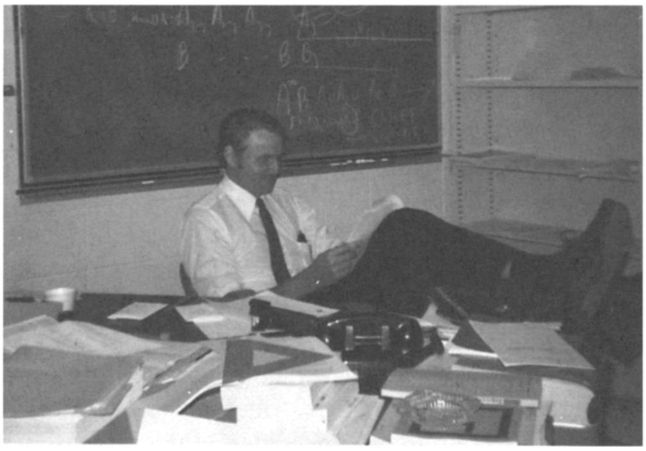

FIG. 5. *At the University of Toronto, 1973.*

DiCiccio: Were you happy with how the book went once you finished it?

Fraser: Well, the satisfaction and joy is in bringing something to some kind of culmination, both in coverage and logical coherence. And then you sort of wonder if people will take to it—that's a secondary thing.

DiCiccio: Your book on linear models (Fraser, 1979) that came out subsequently is closely related. How did that book develop?

Fraser: Fiducial inference with linear models involves an inversion of a pivotal quantity, from the pivotal space onto the parameter space, so a lot of what's in the book *Inference and Linear Models* is looking at the same things as in the structural book but without the inversion onto the parameter space—getting the response distribution itself using the group properties. Incidentally, it follows that taking account of the conditioning on an ancillary is a sort of automatic step. Then if you want to invert to the parameter space, you do, but you don't have to.

Again, the objective Bayesians are doing it all the time and doing it without the concern for fine philosophical or logical issues, and that has been good and opened things up a great deal.

Thompson: In what sense are you saying "opened things up?"

Fraser: I think the Bayesians have done a tremendous job for statistics. Statistics was locked in paralysis back when Jimmy Savage's book came out in the 1950s. There was more and more mathematics dealing with less and less. You studied all of this decision theoretic stuff, and you'd be faced with a problem and it didn't prepare you even to examine the problem, whereas the Bayesians could produce things, take a likelihood function, weight it and integrate it. Of course likelihood functions could have been weighted and integrated from the beginning of likelihood, but the Bayesian framework gave it some sort of sanction. Recent asymptotics and MCMC then give you ways of getting numerical results out. So, suddenly there's a wealth of ways of analyzing things, and they have had a huge effect. Times change.

There's Bayes rule, which we all know, which of course is just conditional probability in a particular context—but then on Jose Bernardo's T-shirt on the back in big broad letters it says "BAYES RULES." And it's delightful. It's the one large group where you go and people are concerned with foundations and with applications. Their interests might be very different from the interests and the directions that I am following, but it's very exciting to go to their meetings. There are a lot of people and they really care.

I was so brash as to go to one of their meetings and be suggesting priors that depend on the data. Of course, there's precedent for that: Box's and Cox's work on parametric transformations of data uses such priors. And when I mentioned to David Cox that I was going to talk about data dependent priors, he looked very sagely at me—"But of course, they must depend on the data." A delightful, low-key, very David-like response.

There was a particular example that came out of the meeting which has some implications in the Bayesian context. If the parameter is a location or scale parameter, then you probably shouldn't approach it any other way than with Jeffreys' prior—the arguments for that are pretty strong—but out of this playing with data dependent priors came a concern for ancillaries, and this ties in with the asymptotic analysis. This can be brought down to a simple case, one that actually the discussant, Tom Severini, mentioned independently, and it goes back to the old measuring instrument example. Suppose you select at random from two measuring instruments, one of which has one information function and the other a different such function. The overall information function is half of one plus half of the other, and you might think of directly using the Jeffreys prior, but it seems some Bayesians agree that the information function to be used should be the one corresponding to the instrument that was actually used for the measurement. Certainly that was my suggestion. If you adopt that, then you are not using Jeffreys' prior. The Bayesian, of course, says he just conditions on the data, the ultimate conditioning, but this is a case of choice of an objective prior—a nonsubjective prior—and so the question is whether that choice too should

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depend on whatever ancillary conditioning is appropriate to the context.

Once you acknowledge that issue, then you get a different way to approach some of those situations with location type reparameterizations that depend on the data. They are not quite as unusual as they sound. Essentially there's a certain conditional model that you are going to use and an approximate ancillary to support that, and your choice of data dependent prior depends only on the conditioning, and not where your data is given the condition.

CONDITIONING FOR INFERENCE

Thompson: Do you remember how it was that you first became aware that it would be important to think of conditioning in frequentist inference?

Fraser: There was a paper by David Cox (1958) in the *Annals*. I was visiting Stanford in 1961–1962 and somehow because of my past I got chosen to give a course in nonparametrics. I had gone through the seemingly needed material and had time left over, and so chose to talk about the inferential point of view as opposed to the decision theoretic. This had a bit of the flavor of Fisher and the discussion that David had in his paper somehow focussed on the measuring instrument example. The students in that course were very strong ones and some of them are quite prominent now. Conditioning was heresy to them because they'd been very decision theoretically imprinted. It was really kind of exciting getting into these long dialogues with them. They'd be passing a handout to me that had come out of the business school where there was only one way to do things, the decision theoretic, and that approach wouldn't tolerate the particular conditioning that David was presenting in the spirit of Fisher's work. So the primary awareness came out of that measuring instrument example plus my readings of Fisher.

Thompson: And it entered your own writing at about that time?

Fraser: I was at a meeting at Stanford, an IMS meeting in 1959, and I did a paper on fiducial (Fraser, 1961) and it outlined the group structure argument, the background for the two books we've just discussed. You can't come away from that approach without realizing that you have to condition, and in the process you acquire a sense of what conditioning really means in applications, particularly that what one conditions on would have physical meaning like the configuration of the data; such things that when taken from a standard statistical model become just coordinates of a technical ancillary.

DiCiccio: Did you correspond with Fisher?

Fraser: We had several exchanges. It started with a letter from him complimenting me on the support for fiducial in the *Annals* paper (Fraser, 1961) that came out of the 1959 Stanford IMS meeting, but with a big "But." The big "But" was: What about the correlation coefficient example? This was his original fiducial example, and yet it did not seemingly fit the group structure. So we had some discussion of that. My thinking wasn't really in the direction that his was, I don't think. When you get into the trivariate correlation problem, getting expressions for the distributions and the rest of it, he didn't seem to be proceeding in the same direction, seemed mainly concerned with the technicalities. Through those discussions came an invitation to Fisher to come to Toronto, and I think he even had the Qantas ticket in hand because that was a necessary part of having him come, you had to produce the ticket at the source. Tragically, he died before the scheduled departure.

DiCiccio: Did you meet him earlier on?

Fraser: Yes, he was at Toronto when I first came back from Princeton, visiting the genetics people, and then he passed through Stanford quite a few years later and gave a talk at the medical school. I had the impression that there were very few who attended from the Statistics Department. But I have been reminded that two of the graduate students were there, two who are in fact now very prominent in the discipline. For this I feel that I had some influence as both were in courses that I gave there during that sabbatical visit. Of course, the major statistical departments then, certainly those on the West Coast, were very firmly decision theoretic, so in a way it's not surprising that Fisher, representing some opposite view, would fail to draw a crowd from the local statistics milieu.

Also around that time a psychologist with major statistical publications gave a seminar in statistics in which he talked about inference from the likelihood function alone. This was certainly something rather Fisherian and quite at odds with the dominant decision theoretic view; he was told that you couldn't do that! There certainly were very definite dos and don'ts in statistics then, just as in some sense there are now. At the time you're not really aware of those forces. You couldn't use the likelihood function—a mathematician could have good grounds for that, in terms of counterexamples—and still the likelihood function, as we asymptotics people know, is telling you an awful lot. You may want to calibrate it as asymptotics also

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tells you to do, calibrate the parameterization in which you're looking at the likelihood function.

Just being at Stanford was exciting, because of these cross-currents, and I mean literal cross-currents, like Fisher going through, plus the instilled flavor of the decision theoretic—you can or you can't do this thing. If you came in from the outside not addicted to one side or the other, you'd see the intensity flowing, the fireworks flowing. You could view it as the ebb and flow of conflicting thought systems, and of course we may be immersed in its parallel now.

DiCiccio: What were Fisher's visits like?

Fraser: Well, he seemed very dour, very much involved, even for those days, and we're talking about 50 years ago. At the time when he came through Toronto, he gave a Saturday morning seminar and he was talking about observed information, expected information. He was talking with people that were in applied statistics. I did know what information was, which probably separated me just because at that time going through Princeton you did read about information because you read Fisher. When I asked him something about it, he was fairly blunt, sort of saying, well it's all on paper, but not in those words. You felt kind of stupid asking because, of course, you could have gone and read it in the original; his response seeming to be that you were asking the person in question, and he'd already said what it was, so why were you bothering him.

ASYMPTOTICS AND ANCILLARITY

DiCiccio: How did your interest in asymptotics develop?

Fraser: I guess Nancy (Nancy Reid, University Professor and past Chair of Statistics at Toronto) had been really impressed, when she had been at the University of British Columbia, with three papers that occurred next to each other in *Biometrika* (1980), by David Cox, by Ole Barndorff-Nielsen and by David Hinkley. There was a general feeling among those who were tuned in that there was something really very special going on, and I got interested in that. If you could just somehow see beyond the notation or get beyond some very slick things with the exponential model, you might find something. Part of getting into that then was talking with you at Stanford and getting a feeling for something that I should have had years ago. When you bring things from the exponent down, what happens in the power series? Great flash of light! Of course it led me into an area where there is a lot of algebra involved.

FIG. 6. *At University of Waterloo, 1985, with Sir David Cox, Nancy Reid and John Nelder.*

I find it kind of hard to keep my mind focused on collecting all the terms and so on. It's always dazzling to see how you can manage those things.

DiCiccio: What do you see are the goals of asymptotics and what are its triumphs? Of course there are the higher-order approximations, but what else is achieved?

Fraser: I think the approximations are secondary. The main point is that you are actually looking at the structure of the large sample model, how the parameter is paired with some critical variable and separated from the remaining variables. There are certain curves or more generally surfaces along which the parameter can be viewed as influencing the variable, and then from curve to curve or surface to surface you don't have information concerning the parameter. This can be viewed in terms of ancillarity, but it is more than just raw ancillarity; it is part of the structure derived from continuity in both parameter and variable, and becomes apparent from the asymptotics or, more practically speaking, from the data accretion process.

Being able to formalize, codify or somehow get on top of that structure is a major goal for me. People will say, "Well, it's all just MLE" (maximum likelihood estimator). Of course you are using MLEs, but the MLE is not there as an estimate: it's there as a function, a function on the data space that helps you for other things. Just because you use an MLE doesn't mean you're using an estimate that somebody has said is inconsistent or worse; you are using a function that's describing location or describing contours.

Thompson: So here again the location-scale model conditioning paradigm is basic.

Fraser: Very few people are really familiar with what the analysis is for location-scale models. People go through graduate programs, maybe here or in

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Toronto, and really haven't had much contact with conditional analysis.

When you look at inference this (conditional) way, it's really different from what most powerful theory or minimum length confidence intervals lead you to say. If you're focussing on a minimum average length and are constrained to 95% coverage as your ultimate goal, it's been shown you can get bizarre results; in fact, you can get 100% conditional confidence intervals for very precise cases and low or zero confidence intervals for other cases, which is counterintuitive for applications.

DiCiccio: I remember your making the point about the minimum average length confidence intervals rather colorfully at the Barnard Symposium here at Waterloo in 1981.

Fraser: Yes, I talked then about seeing the McDonald's sign on the way in, and how with 35 billion confidence intervals served, you could give the shortest intervals on average of all the fast interval outlets in North America. How do you become the outlet with the shortest on average 95% confidence intervals? You give the precise scientists oversized intervals, and the sloppy scientists undersized intervals, and your intervals are better on average; simple, but that is what's happening. But it's the conditional viewpoint that is of great importance: you'd never, and I mean NEVER, think of varying your conditional confidence level to achieve some optimization property, like minimum length or maximum power. That's obscene in an inference context: you can't call an interval a 95% interval when you can see from the context that it is categorically *more* or categorically *less* than the asserted 95%. That is fraudulence, pure scientific misconduct. What's at fault? The appeal to optimization and some overview that suggests you can average out clear mistakes in each direction.

DiCiccio: Optimality is seductive.

Fraser: Much creativity in statistics comes from people who are mathematically skilled. You have to have certain mathematical skills and abilities to work with the algebra and analysis needed to deal with these things. But then the optimality criterion takes over and becomes a raison d'être. So I think we need these examples, like Welch's (1939) uniform distribution example, even though it disturbs graduate students and others.

DiCiccio: What about ancillary statistics?

Fraser: Well, there are the Fisher ancillaries that go way back. Quite a lot of those are transformation group based and so there isn't really much argument that can be brought against them, because the group is somehow describing how the parameter moves or influences the data point.

Then you have Buehler's (1982) paper where he collects a lot of examples and the examples are abstracted statistical models where there is an ancillary, and you do get some contradictory ancillaries. If you use one ancillary, you get one result, and something different with another ancillary, so that makes you stop and think. I ask, "Where does the ancillary come from?" I think it should come from some relationship of the variable to the parameter, and I know this sounds heretical to most, but if you start to think of the development of the statistics structure, you have a density function and then a data point, and the frequency at that data point varies with the parameter. But in this, the continuity in the sample space plays very little role; it largely gets ignored. In the development of statistical theory you don't really think of where the data point is with respect to the parameter in some physical sense; but in any practical measuring situation, there's a very physical relationship between what the measurement shows and the thing being measured, and I think that should be a very important part of the modelling process. The transformation model we were just discussing provides a very clear, not approximate, example. And I think if you bring that consideration in, then you're going to get, subject to reasonable continuity, uniqueness for the ancillarity. Most of the nonuniqueness problems arise just because too much got left out at the beginning.

DiCiccio: You seem to accept in the discrete case that there's a possibility of nonunique ancillaries. Is this a problem in the continuous case?

Fraser: I don't see it as a problem. Part of it is off-loading the starting point. The development of the ancillary in the continuous case comes from a full dimensional pivotal quantity, so if you change your parameter, you can think of movement in the data that corresponds to that, as if the parameter is forcing the data. We've even gone so far as talking about the velocity of the data point under parameter change. That velocity defines a direction, and if you have reasonable asymptotic properties, then that velocity vector is going to be tangent to the ancillary.

Thompson: What about conditional inference in the sense of conditioning with sufficient statistics in an exponential model?

Fraser: When you see the structure of the asymptotic model in the big dimensional space, then you find that it approximates a transformation structure.

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The purer concepts of statistics, like sufficiency in that larger context, wind up being very singular cases peculiar to a special model form. If the model is of a certain very precise form, you may get sufficiency, but if it's not in that precise form, you're still going to be able to do the analysis conditionally. It just happens that with sufficiency, the conditional analyses are going to be the same, independent of the conditioning variable, so sufficiency is just a peculiar singular case. But sufficiency has captured a glamorous position in the profession and now we see it as an anomaly, and, in fact, it isn't there in most models that are really out there. A lot of people have put emphasis on that.

Dave Brillinger way back said he wanted a 10 page thesis disproving sufficiency. Well I think in asymptotics there are arguments that would disprove sufficiency as a useful concept. Disprove it in the sense that it isn't a viable concept in the bigger sphere of things where things can be done by conditional methods, and similarly when looking at conditional analysis for component parameters or subparameters. The typical nice example is an anomaly that arises with the canonical parameter of an exponential model. If you depart a little bit from pure exponential model form, then the analysis for a component parameter has to be marginal. It's only conditional if there are all kinds of symmetry in the models. Without the symmetry, you can't use conditional methods, but with the symmetry, the conditional methods become available, and they're marginally valid and agree with the marginally obtained results. It's as though the profession has been chasing for almost a century a marginal followed by conditional approach, when the available and intrinsic inference method is really conditional followed by marginal, the reverse order of reduction.

DiCiccio: Do you think higher-order inference is actually feasible? Unless you're prepared to specify an ancillary statistic, you might only be able to uniquely identify your inferences up to second order instead of third order.

Fraser: If the pivotal is somehow expressing the relationship between variable and parameter, and there's continuity, then it may well determine the ancillary, so the issue doesn't arise. More generally, if you change the ancillary, then your p-value for a test may be different, assuming the validity of the assumptions and the rest of it. The uniform distribution of the p-value is going to be OK to the third order. It's just that the p-value is maybe measuring how far the data are away from the parameter in a different way, and in the nature of things there may not be a natural or a best way for

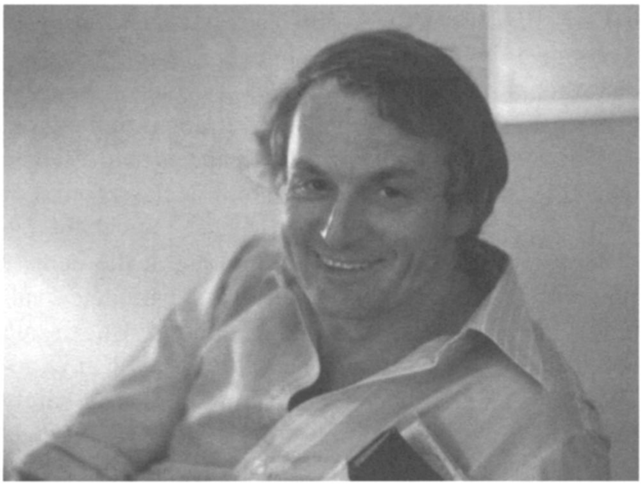

FIG. 7. *In the 1980s.*

this; and the inference would be qualified by the type of measuring relationship defined by the initial pivotal.

DiCiccio: What are your future directions in the area of asymptotics? Where do you see the area going?

Fraser: Well, there are things with respect to ancillarity that intrigue me. We've talked a little bit about some of these, such as examining what the asymptotics and continuity say about the geometric structure of the ancillary—whether it's built into the asymptotic properties of the density function itself or whether it has to come in from some coordinate-by-coordinate kind of pivotal, which in turn is trying to see how the parameter is moving the density structure. I mean, that's kind of a foundational issue. It's the kind of thing that wouldn't interest most people and it might be that most journals wouldn't see it as important, but I think it is. It would influence how you could develop and pursue a lot of statistics.

DiCiccio: Some statisticians might feel uncomfortable with the heavy reliance on models that's required for higher-order asymptotics. How do you see this? Do you see the ideas coming out of a very careful examination of parametric models ever being applied to nonparametric inference?

Fraser: I'm not sure. It might very well turn out that way. I mean, out of those ideas might come, hopefully, a very realistic way of measuring sensitivity if you allow certain kinds of modifications to your statistical model. I'm not sure if there has been very much done on that, but it certainly seems an important area.

TEACHING AND TEXTBOOKS

DiCiccio: I want to ask you about teaching. You've written a couple of textbooks. How did they come about?

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Fraser: They came out of a feeling of wanting to reorganize the material for students. There are certain restrictions on that. You can't be too radical because everything develops within a conventional framework, and there is a huge lag between the arrival of new ideas in statistics and seeing them in a classroom. For instance, if you're going to use a statistical model, necessarily you're going to be looking at the density function at a data point for various parameter values, which means you look at likelihood. But to talk about likelihood with students with the typical exposure, you're talking about something they have barely met or have met in a different way. Where's likelihood? Well, they've perhaps talked about the likelihood ratio test or the generalized likelihood ratio test, and of course that's a Neyman–Pearson type use. But that doesn't lead you to think of the likelihood function as something you can see on a monitor that is describing model possibilities for observed data. And then what happens if you change your data point slightly? How does that likelihood function move? What is the sensitivity of the likelihood function to movement in the data point? These are all crucial things, but you'd typically with many students be talking about something remote from their other courses, with the familiar "rejection if beyond the 5% point."

DiCiccio: Do you see it as a resistance to assuming models?

Fraser: Well, it's probably that models aren't really very deeply into most courses. Somehow they've been removed from there, so people don't think that way, except to say the superficial, "Let these be i.i.d. normal." And then it's probably a reaction to what statistics did to itself earlier on, where it assumed models to too great an extent and what was developed was meaningless, it didn't work. You couldn't think of applied problems.

DiCiccio: Why not?

Fraser: This bothered me from the beginning. I remember first going to graduate school in the States and asking "What's a random variable?" I mean there's what you read in various books, and the book that was around at the time was Cramér's (1946) book. He addressed it, but he addressed it in terms of sequences of repetitions, which is something like the formulation by von Mises, but then if you were taking new variables, you always had to recompound things and define a new base probability space. It left an insecure feeling. Then came the familiarity with Kolmogorov, where it's all sigma-finite measure and the rest of it, which is fine but it doesn't fit sensibly with applications. You don't get a feel for a problem that way. Then saying a random variable is a function on a probability space! That's fine; it solves the measure theoretic problems but in terms of how to think about real life problems out in the world, "a function on a probability space" is a rather ridiculous way of presenting a measurement process. Indeed, it's probably worse than ridiculous because it controls how people think of observable variables and the measurement process.

Certainly you can think of repetitions for a variable and a distribution for those repetitions; that's fine. But then to categorize that collection of repetitions as a function on some ultimate probability space down in the depths or up in the sky or elsewhere is getting carried away with the mathematics, and there's no way you could legitimize that in some global frequency sense. The "functions on a probability space" approach is a measure theoretic convenience for dealing with probability on an infinite product space. Examining a new variable means another function on that ultimate space and thus means a redefinition of that space in any realistic sense. In fact, the variable exists before the space and the ultimate space is backwardly defined. Thus the approach doesn't correspond to the realities.

If you were growing up with that as a background for how you thought about random variables, you'd have a lot of misperceptions, misfitting kinds of perceptions of the things you had to work with. I think Cramér's book was impressive at the time, but then viewpoints change. The current view of leaving probability out of statistics has gone to the other extreme. You can't think logically about things because you've left too much structure out.

Thompson: Do we challenge students enough these days?

Fraser: I'm rather vehement in my concern for what goes into a course. I think we need to give real attention to what goes into statistics courses, real attention for where the discipline is going. There was a bad name for theory fairly early on and then the right thing to do in departments was applied statistics. Well, of course, you should be doing applied statistics, but that doesn't mean you don't stop and think of what the overall methodology is and where you are going. I think a lot of statistics groups are really suffering from this disorientation that's come out of the past. You can't survive when everybody's doing the same *S* or SAS computer package analysis. There is a real need to address the appropriate methodologies.

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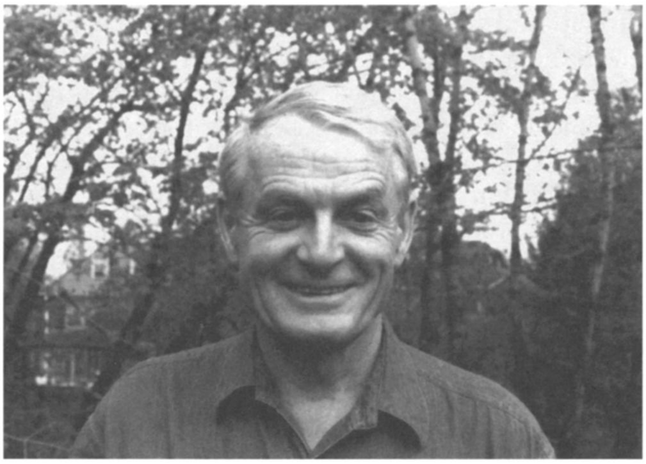

FIG. 8. *In Toronto, 1994.*

PERSONAL STYLE

DiCiccio: How do you work? When and where are you most creative?

Fraser: I'm not too good at working in the office, because I feel tied up and locked in. If you have to do something, you have to spread things out. If you have to pull things out of books, then you do have to stay in one place, but then you can get to a point that you're chewing your fingernails. At that point, it's time to quit, and go off and do something different, like swim a kilometer at lunch. Then you come back in and you find you know what the answer was. Somehow the pieces have fallen in place. A lot of formal work in an office is somehow forcing the mental processes to work in a way they're not supposed to work—or maybe that's idiosyncratic, I don't know. I guess I'm a bit of a maverick. I'm more concerned with how ideas and things fit together and not with taking a lot of material as the given; with saying something's not working here, so we'll throw all the pieces up and let them float around and then see which ones come down and fit together.

Thompson: I've always had the impression that your constructions are a matter of almost physically capturing pieces and fitting them together.

Fraser: A little bit of that happened with the evolution of fiducial through the transformation models and then the fact that you could use this kind of analytic differential geometric approach to do the asymptotic things. If your inclinations were more in the algebraic sense, then you'd be doing one kind of thing and manipulating symbolic objects; but if they were more in the geometric directions, you'd like to get in and feel and picture yourself there. The kind of geometry you get is almost a matter of focussing in.

Eventually, when you get in small enough, things are locally linear, and if you go a little bit bigger, then you have quadratic and other effects. Then if you keep going you're stuck with all the terms of a Taylor series approximation. Keeping track of them, for example, is the thing we've been talking about in asymptotics, how the terms will drop off as part of the accretion of data, which is kind of magic. Then if you want to see what's really happening, you have to go and put boundaries out there and put a lid on—a probabilistic lid—and control those in the same way you do for theory in going from the central limit theorem and the score function to the distribution of the MLE and the distributions of possible likelihood functions, looking at that not just in terms of the distribution of the likelihood function, but also in terms of what happens to the statistical model itself with accretion of data. And this leads to transects, such as one- and two-dimensional slices, through what's happening in n space. Even though n space is increasing in dimension, you're looking at slices, you can talk about angles in the big space. It's kind of fun, and part of it is that if you're talking about angles in a big space like that, people look at you as if you're a little bit off. But that's OK, it just means that sometimes you get a bit of a head start. Of course close competition is also fun.

DiCiccio: Three of your daughters are mathematicians. Do you think they were ....

Fraser: Coerced, forced or pressed into mathematics? I hope not any of those. I hope they got into it because they like it. But they also have to deal with the closure from being in mathematics, as opposed to this more open-ended feeling in statistics where things can be expansive and outwardly directed, something I think we all have to deal with.

DiCiccio: Were they aware of you being involved in the research process? I mean, if you were working at

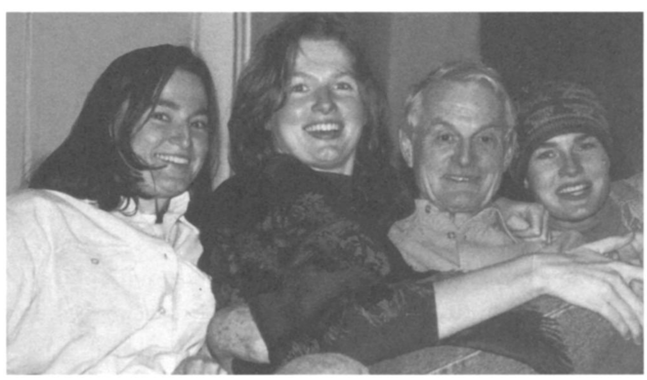

FIG. 9. *With three of his daughters: Ailana (Ph.D. Stanford); Andrea (Ph.D. Princeton); and Maia (Ph.D. Stanford).*

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FIG. 10. *June 2001, receiving the gold medal of the Islamic Statistical Society from Ejaz Ahmed.*

home, was family life such that they'd actually see you at the office?

Fraser: Why yes! Sure. They'd also see me doing maintenance things at a cottage, or plumbing, sweating copper pipes and so on. I was quite impressed—one of them was with me once when I put some plumbing together, and years later, when there was a problem and I wasn't there, she got down and, certainly with all kinds of precautions, took the pipes apart and resoldered the copper pipes and everything worked again!

There's also electrical wiring. I always tried to persuade them never to work with live wires. Of course, I wasn't always a good example, because it's easier to go into a circuit box when it's live—you can see what you're doing, but it's also dangerous. One electrician I knew was telling me about wiring in a particular house, and he was the only person there except for some very elderly person on the floor above. He was in a wet basement cutting a wire—if you get a jolt through a wire, it locks the muscles—so he couldn't release from the pair of pliers, and he was there calling to this person, getting 110 volts through him, and he could hear the person slowly move, till finally the switch got cut, and he fell on the floor and lay there for a while feeling blessed to be alive. I hope my daughters have learned not to do things like that!

It's easier to tell people what not to do when you've been a little bit wild and reckless and done them all the time yourself. In Princeton there was a third floor series of rooms in the graduate residence, and one time the caretaker for the building came in, and went into a receptacle that was live and changed it. Wow, you don't do that! And the result of that kind of dismay is that you end up doing it yourself years later. Sort of like with the Box and Cox paper. David reminds me that I was fairly vehement about that paper: Here's a Bayesian procedure and you're making the prior depend on the data; it's a violation of principle. Well of course it was a violation of principle, but they weren't doing it as a matter of Bayesian principle, they were doing it as a way of using Bayesian methods to explore something. The fact that you could grow up with quite a deep contact with the discipline and be afraid to do something because it violates some principle is confining—whether it's electrical or whether it's a matter of a data dependent prior.

DiCiccio: You've had very many Ph.D. students.

Fraser: They've been an extraordinary group, with different abilities, different drives, different contributions. That's the real joy of doing research in a place where there's teaching. A big part of it is coming in contact with students who get excited about new ideas and are persuaded to follow something. There's a bit of a risk in assigning the problems to match the abilities. You can run into difficulty if a person gives the impression that he's a greater master of an area than perhaps he is, and then he's going to be cross later if you get him into something that was beyond him. So there are those kinds of risks which are a real concern, but just the excitement of working with somebody and having him or her say, "Hey, this is neat!" and visibly doing some work and chasing it—that's really what it is all about. I think.

THE DEPARTMENT OF STATISTICS AT TORONTO

Thompson: I'm wondering if you would say a few things about what led up to the establishment of the Department of Statistics at Toronto.

Fraser: Statistics had flourished within mathematics. There was a general feeling among the mathematics people that they would support statistics and not try to second guess what the statisticians were doing. I thus assumed that most mathematics departments felt the same way. I've since learned that Toronto's was an extreme anomaly. That freedom to let statistics grow within a mathematics framework is some sort of ideal. It was just the opposite in some places where typically mathematicians would feel that they should do the evaluations of statistics people. Even the process of hiring and the recognition of merit would be from a foreign viewpoint. It would be very difficult for statistics to do creatively what it needs to do, instructionally and for research, in that kind of framework.

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FIG. 11. *In the late 1970s.*

But at Toronto the freedom was there and it worked, worked magnificently, and so at Toronto, statistics grew in the mathematics environment. There were more and more graduate students, very able students with an interest in statistics, and there was an administrative assistant who came to the math department to co-ordinate. Half her time was devoted to the graduate program in statistics and half her time to the *Canadian Journal of Statistics*. That gave us a strong momentum.

We formed a Department of Statistics, albeit only at the graduate level. That was at a time when administration was nothing like the kind of administration that you see or Nancy sees, which is masses of paper and going through detailed things and getting a report in for this and a budget for that. I couldn't have handled that! Then there was a sudden opportunity for the statisticians to develop things for themselves.

One of the things was a grant for a computer, a DEC computer. That's where the conflict arose with the mathematicians. There were certain mathematicians that were not going to have a computer in the department and were not going to have any consulting and felt very very strongly about this. Yet there were people within statistics that were very committed to those directions, like Dave Andrews, and in fact, the grant was through him to the department. So that led into statistics splitting off.

There was a big meeting voting on whether to split. Everybody agreed we should split and the Dean agreed, and each group produced a budget. The mathematicians asked for a little bit more everywhere and the Dean wasn't going to get into a hassle—he just gave it to them out of some pot of money he had. The statisticians also got a little bit more and the separation came about very amicably. But this meant there was a group of people in the new Statistics Department who'd been in one environment and were now in a totally free and different environment. So there was some unevenness, some tension, politically different views on what directions statistics should take.

There was a period of unrest, which is the kind of thing that has happened in other departments, and I think it's just in the nature of a change in the frame of reference. And coming from that, is a department that is new, and doing statistics, and I don't think any of those initial stresses are around now. The direction that each group wanted is the direction that it's gone—it's just that the picture has got broader.

THE DISCIPLINE

DiCiccio: And how are your feelings about the future of statistics? Optimistic?

Fraser: The discipline? Well, I get dismayed in terms of what the undergraduate/graduate curriculums do. They seem to have a very leadening effect on creative developments. I'm not sure what the answer is, but somehow you don't see a lot of people coming along with light in their eyes, that here there's something great they'd like to study. And I think it matters who teaches things. Teachers may well share their enthusiasm, but maybe it's separate from the area they have to give the course in. I'm not sure how to suggest change. Somehow courses have evolved to have fairly minimal directions in terms of content.

I think various schools differ a little bit in terms of how tightly they enforce curriculum or whether they let the instructor talk about just about anything. I remember when I was at Princeton, there was an introductory stat course where I somehow got involved in helping, and there were two sections. I don't remember who did one section, but the other section was given by John Tukey, and John was going off into some kind of algebra, really exciting things that I had no idea how to pursue. There's a place for that, but I think you'll find that really good schools, at least the smaller places, will have flexibility for various people to follow creative routes.

Beyond that, I think the big challenge now is growing statistics within our department and not having the new statistics being done in computer science or in economics. In economics they're doing much more elaborate statistical modelling than we see. Sometimes it seems as though we are the mother discipline and much is happening elsewhere. Are we going to address the challenges of statistics? It isn't as if we are locked into producing *Annals* type papers that no one will ever read.

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FIG. 12. *At the time of the interview, with Tom DiCiccio and Mary Thompson, University of Waterloo.*

Not to say that *Annals* papers can't address the challenges of tomorrow. A classic example is Henry Daniels' (1954) paper on the saddlepoint in statistics. Twenty-five years later came Barndorff-Nielsen's and Cox's (1979) paper on the saddlepoint: 25 years for a substantial idea to diffuse in the discipline from the *Annals* to *JRSS* (*Journal of the Royal Statistical Society*)! Probabilists talk about diffusion processes, but this is an absurd example of slow diffusion, 25 years for an idea to make that small progress. What was the idea? That you could do better approximations than with the central limit theorem. What could be of more importance to statistics or probability in certain directions than that?

It took 25 years for this idea to go from the *Annals* to *JRSS*. Then there was immediately all kinds of work on saddlepoint and cumulant generating functions. But then further development shows that you don't need the analyticity, that the likelihood function itself acts as an approximate cumulant generating function. The recent asymptotics is just based on the additivity for log densities; not additivity on the transform space but additivity on the log-density space. The likelihood function that's been neglected or forgotten in certain senses winds up being the central ingredient in asymptotic theory. The key methodology extends widely and then flowers: it brings all kinds of things to life.

APPRECIATION

Fraser: My deepest appreciation to you, Mary, and to you, Tom, for hosting this very enjoyable session. With your Toronto background connections you already had some feeling for the various issues we've discussed. And thank you, Tom, for much support in my earlier interests in the asymptotic area. I appreciate the chance to join you here today.

DiCiccio and Thompson: Thank you. It has been our pleasure.

REFERENCES

BARNDORFF-NIELSEN, O. E. (1980). Conditionality resolutions. *Biometrika* **67** 293–310.

BARNDORFF-NIELSEN, O. E. and COX, D. R. (1979). Edgeworth and saddlepoint approximations with statistical applications (with discussion). *J. Roy. Statist. Soc. Ser. B* **41** 279–312.

BUEHLER, R. J. (1982). Some ancillary statistics and their properties (with discussion). *J. Amer. Statist. Assoc.* **77** 581–594.

COX, D. R. (1958). Some problems connected with statistical inference. *Ann. Math. Statist.* **29** 357–372.

COX, D. R. (1980). Local ancillarity. *Biometrika* **67** 279–286.

CRAMÉR, H. (1946). *Mathematical Methods of Statistics*. Princeton Univ. Press, Princeton, NJ.

DANIELS, H. E. (1954). Saddlepoint approximations in statistics. *Ann. Math. Statist.* **25** 631–650.

FRASER, D. A. S. (1957). *Nonparametric Methods in Statistics*. Wiley, New York.

FRASER, D. A. S. (1961). On fiducial inference. *Ann. Math. Statist.* **32** 661–676.

FRASER, D. A. S. (1968). *The Structure of Inference*. Wiley, New York.

FRASER, D. A. S. (1979). *Inference and Linear Models*. McGraw–Hill, New York.

FRASER, D. A. S. and REID, N. (2001). Ancillary information for statistical inference. *Empirical Bayes and Likelihood Inference. Lecture Notes in Statist.* **148** 185–209. Springer, New York.

HINKLEY, D. V. (1980). Likelihood as approximate pivotal distribution. *Biometrika* **67** 287–292.

PITMAN, E. J. G. (1939). The estimation of the location and scale parameters of a continuous population of any given form. *Biometrika* **30** 391–421.

USPENSKI, J. V. (1937). *Introduction to Mathematical Probability*. McGraw–Hill, New York.

WELCH, B. L. (1939). On confidence limits and sufficiency, with particular reference to parameters of location. *Ann. Math. Statist.* **10** 58–69.

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