A Conversation with I. J. Good

David L. Banks

Abstract. Irving John Good was born in London on December 9, 1916. He attended the Haberdashers' "secondary" School, distinguishing himself as a mathematical prodigy, and then entered Jesus College at Cambridge University in 1935. He studied under G. H. Hardy and A. S. Besicovitch, obtaining his Ph.D. in 1941, and was the Cambridgeshire chess champion in 1939. Then he was called into World War II service as a cryptanalyst at Bletchley Park, working partly as the main statistician in teams led by Alan Turing and, later, by the British chess champion C. H. O'D. Alexander and by M. H. A. Newman. The work employed early electromagnetic and electronic computers and applied Bayesian statistics relevant to reading the two main secret ciphers used by the German Army and Navy, providing crucial intelligence to the Allies. After the war, Good taught briefly at Manchester University and made a few suggestions for the electronic computer project. He was then drawn back into classified work for the British government. During that time he obtained an Sc.D. from Cambridge and a D.Sc. from Oxford. In 1967 he came to the United States, becoming a University Distinguished Professor at Virginia Polytechnic Institute. Officially he retired in 1994, but in practice he can be found at work late in the day when the snow isn't deep.

Jack Good has made fundamental contributions to mathematics, physics, computer science, philosophy and especially statistics. In his free moments, he amuses himself with chess, Go, grammar, kudology, botryology and whimsical acts of creative intelligence. He's written on the order of 800 papers (counting is difficult because publications vary in dignity, from a note to a seminal paper) and four books (one joint), and he conceived and was the general editor of *The Scientist Speculates: An Anthology of Partly-Baked Ideas*, in which famous researchers outlined pet ideas on the border of current scientific thought. To statisticians, Good is chiefly famous as a pioneer of Bayesianism, especially hierarchical, and of the Bayes/non-Bayes compromise. He is an innovator with contingency tables, the co-discoverer of the fast Fourier transform, the rediscoverer and developer of penalized likelihood procedures, the developer of an empirical Bayes idea of A. M. Turing and a fundamental contributor to theories of explanation, the dendroidal classification of kinds of probability, and the philosophy of statistics.

When I visited him on December 23, 1993, his office was large, dark and very crowded. Overhead, an enormous suspended three-dimensional reticulum with peculiar periodic cavities canopied the room (it had come to him in a dream). A different contribution to mathematical art, Dioximoirékinesis, on permanent exhibition at the Exploratorium in San Francisco, was built to his specification by Martine Vite, an artist whom he met in a café (because there was only one seat available). Below the reticulum, scrawled calculations contested for space among heaps of books, journals, notebooks and letters; even the vertical surfaces were awash with mathematical notes and cartoons. All chairs (including his own) had been conscripted to serve as auxiliary desks, and the computer and typewriter were layered with papers. One got the impression of a teeming jungle ecology, with each organism competing for the constraining resource of I. J. Good's attention.

In the center of Jack's desk was a tiny clearing, about the size of a regular folio sheet. Every piece of research that Jack had done in the last 15 years

David L. Banks is Associate Professor, Department of Statistics, Carnegie Mellon University, Pittsburgh, Pennsylvania.

was written out within its confines. I put the tape recorder there.

BEGINNINGS

Banks: Let's begin with your childhood and go through the basic biographical details. I understand that your father was a watchmaker who then went into antique jewelry.

Good: He came from Poland, which at that time was owned by the tsar of Russia. He learned how to mend watches largely by observing a watchmaker through a window. They often sit right at the front of the shop where there is plenty of light. And, later on, in London, he got into antique jewelry, and became a prominent dealer near the British Museum.

Banks: How did he happen to move into antique jewelry?

Good: He enjoyed cameos and traded in them. That, and a romantic love of the past, led him to antique jewelry. So the original name of his shop was "Cameo Corner," and later "Good's Cameo Corner" because a sign writer was too drunk to spell Goodack.

Banks: How did your father decide to come to England?

Good: He didn't like the country he was living in. He didn't see why he should fight for Russia where pogroms were going on. So at the age of about 17, having 35 rubles to his name, he and a friend managed to escape, without even a train ticket. They took a large round cheese and slept underneath the seats. His friend used the cheese as a pillow and as a potential bribe for when the ticket collector came around. They eventually reached England. He did odd jobs and saved until he could start on his own. He was getting along adequately until his shop was burglarized and he had to start again by borrowing from my mother. Later he wrote an autobiography called *Visions and Jewels*. There is a 1952 edition published by Faber and Faber of London, under the authorship Mosheh Oved. This was the name he adopted after his marriage with my mother broke up.

At the age of 8 my mother also came from Russia. She came with her parents and later met my father, in London I suppose.

I was born in Queen Charlotte's Hospital in London on the same day as Kirk Douglas, whose parents also emigrated from Russia. He too changed his name from Isidore, which he didn't like, but I had an extra reason. There were posters all over London advertising a play called *The Virtuous Isidore*. That, together with my surname, was too much of a good thing.

Banks: You're fairly famous for having been a child prodigy. When were you first recognized as such?

Good: Well, I don't think anyone called me a prodigy. When I was about four I stood up in bed and asked my mother what a thousand times a thousand was. She didn't know and I told her it was a million. That sort of thing suggested I had some mathematical ability. In elementary school I was good at mental arithmetic and discovered, at the age of 9 or 10, how to guess, in 10 questions, what number up to 1000 someone was thinking of. But my spelling was terrible and I didn't read a book right through until I was nine or more. It was entitled *Smiler Bunn, Crook*.

When I was nearly 10, I was in bed with diphtheria (that's a disease of course, not a girl—unfortunately). I worked on trying to extract the square root of 2. My sister had shown me how to extract square roots by the method resembling long division, bringing down two digits at a time. And when I'd reached about 10 or 12 places, I squared the result and of course I got something like one

FIG. 1. *Young I. J. Good and his mother.*

point a large number of nines. And I began to suspect that perhaps it would never end. Then it dawned on me that it couldn't possibly end, since the last digit when squared couldn't be a zero, so the answer couldn't be exactly 2. So then I guessed, incorrectly, that perhaps it was not a terminating decimal, but recurring, like 1 over 3, and had the form of one whole number divided by another whole number.

When I began to look for such numbers, I always missed by 1. I found a way of getting as many solutions as I wanted, but they were always off by 1. For example, 7^2 was twice 5^2 minus 1. It was always plus or minus 1. Of course, I didn't know I had solved a Pell equation. [Note: Pell was a seventeenth century mathematician; Euler named the equation $x^2 - my^2 = \pm 1$ for him. Jack was addressing the case when $m = 2$.]

So I then suspected that the square root of 2 wasn't even what we would now call a rational number, and it was at that time unknown to me that there could be such things as irrational numbers. Once I started to think about the problem in that way, it was fairly straightforward to rediscover the Pythagoreans' *reductio ad absurdum* proof of the irrationality of $\sqrt{2}$, based on a parity argument.

And of course I'm proud of that, even now, because if there was any single instance in my life that shows that I had a little bit of mathematical genius, I think that was it. At the age of 9, or nearly 10, it wasn't bad to make a discovery that Hardy described as one of the greatest achievments of the ancient Greek mathematicians. Being anticipated by great men is now familiar to me, but it is not usually by 2.5 millenia.

Banks: By all means, that's an astonishing accomplishment. Do you think the diphtheria helped?

Good: Well, it gave me a lot of time. Some of the best work by scientists occurred because they were away from the madding crowd. Newton is a prime example of that. The plague was responsible for the beginnings of physics. [Note: Newton developed much of his theory of calculus and gravitation while in enforced isolation at Woolsthorpe, avoiding an outbreak of the bubonic plague of 1664–1665.] Newton was kicked in the stomach at school, which also helped.

Banks: You mentioned that you instructed your mother on the product of a thousand times a thousand. What sort of family background did you have that would make this arise naturally in conversation?

Good: Actually, I asked the question out of the blue. But, to answer your question, my father was an intellectual, though he was self-educated. He was

FIG. 2. *I. J. Good and his dad.*

temperamentally a philosopher, but not a philosopher of science. My mother was not well educated, but she was keen on education for her children, and I was encouraged a lot by my parents. I owe a big debt to them. There is a Jewish tradition of supporting intellectual activities. Perhaps it's because of the study of the *Talmud*.

Banks: J. B. S. Haldane speculated that Jews excel in scholarship because throughout the Middle Ages, virtually every Christian who could read had to take a vow of chastity, while the Jewish community supported their rabbis, who had large families and who would marry their daughters to star students.

Good: I'd not read that. Compensation for antisemitism is another theory. Haldane was a master of partly-baked ideas and he submitted an article to *The Scientist Speculates: An Anthology of Partly-Baked Ideas*. [Note: Later Good edited a column of partly-baked ideas, from 1968 to 1980, for the *Mensa Bulletin*. There were about 800 partly-baked ideas which are collected together in a report (number 2166). An example was the proposal of DNA "fingerprinting."] But Haldane was annoyed when

the publisher, which was Heinemann's, in the early stages gave more prominence to Arthur Koestler's name as a "scientist." So Haldane withdrew his contribution and published it elsewhere.

Banks: It invites anticlimax to ask, but were there other childhood incidents of similar mathematical insight?

Good: Well, when I was 13 I discovered mathematical induction for myself from a problem in H. E. Dudeney's book *Amusements in Mathematics* (a book that taught me a lot about solving problems). It concerned the number of balls needed to construct a pyramid based on an n-sided square. I looked up the solution and wanted to prove it. After thinking about this for two days, I proved it by what I now know to be induction. It came in a flash of inspiration.

Another instance, at about the same age, which was important in my mathematical education, was when a schoolmaster, Mr. Smart, wrote out about nine exercise questions on the blackboard and as soon as he finished writing the ninth, I said obstreperously "I've finished." He said "You mean the first question?" and I answered "All of them." One of the exercises was how high would a gallon bottle be if a one-pint bottle of the same shape were nine inches high. That took me about three seconds; he asked me what was my thinking. I said I'd imagined the bottle to consist of a very large number of small cubes, and then applied a magnifying glass to this which would double the length of each cube. And since I knew there are eight pints in a gallon, the magnifying glass would convert the pint into a gallon, so to speak.

After that I didn't have to listen to his "lectures." He, and my later teachers Oliver, Edge and S. L. Baxter, just gave me books and notes to read. I was in the classroom, but working on these books from then on in high school and I never again had to listen to mathematics schoolmasters' lecturing. But of course they did help me. They suggested what I should read and solved problems that I failed to solve.

Banks: It sounds as though your school was admirably flexible. Could you give us a bit more information about it and the exposure it gave you to mathematics?

Good: I went to the Haberdashers' Aske's school in Hampstead from 1928 to 1935. It was a secondary day school, as opposed to a boarding school, for boys. People did not always go into the top class, called the sixth form. Only about 1 person out of 10, even in the sixth form, went on to the university. So the level of education was, for the average person, much lower than it is now.

There was no experimenting with the new math, which had not yet been "invented." It was straight trigonometry and algebra and geometry. I went considerably beyond what the other boys were doing, reading right through Joseph Edwards' larger book on differential calculus. After that I did the same with G. H. Hardy's *Pure Mathematics*. In the early days my main supervisor was H. C. Oliver, who had real mathematical ability. So, of course, when I went to Cambridge, my first courses were easy. In the sixth form I read a history of mathematics. It mentioned the marvelous theorem that every prime of the form $4n + 1$ is uniquely the sum of two squares, with an indication that it could be proved by using Gaussian integers (of the form $a + ib$). I managed to prove it in two days, using 13 lemmata.

Banks: This school apparently did a great deal to encourage your mathematical bent. What were your other courses like?

Good: I rather enjoyed physics, which of course is somewhat like mathematics. But I didn't like heat experiments, because when I did them the steam would escape and my results would be wrong. But once, in an experiment on sound, my observations plotted so precisely on a straight line that I thought I'd be suspected of cheating, so I cheated by moving the points slightly off the line.

History I did not enjoy at school. My attitude has changed because I now know some history, partly from films and partly by living through a lot of it. I would always fall asleep during history lessons, so they had some value for my health. When you have very little historical background, it can be a dull subject. Usually the attention was on whether a particular ruler was a good king or a bad one, as in *1066 and All That*. I usually went to sleep as soon as the teacher, Mr. Meadows, started to talk and I wouldn't even remember which king he was talking about. Mr. Meadows was himself rather dull until he got married.

The French schoolmaster liked talking about his travels and we always encouraged that because we didn't want to learn vocabulary. Some of his travel stories were repeated many times.

Another character, nicknamed Chaucer, was the deputy head of the school. When he retired he wrote a book called *Schoolmasters All, or Thirty Years Hard*, for which he might well have been sued. It was bitingly funny.

Banks: Let me ask a question that loops back to our previous discussion. As a child you discovered Pell's equation, proved the irrationality of the square root of 2, found the principle of induction and laid several other independent cornerstones. How do

you think you were in a position to do so much fundamental work so young?

Good: As for the square root of 2 that's an area for which I had time. There was nothing else particularly to do. I was in bed for five or six weeks. And as I said, mathematics or arithmetic interested me, I suppose as an art form or a game.

Banks: I was wondering whether the progress of mathematics has made certain of these questions easier to ask when you were a child than they would have been for the ancient Greeks. For example, you had been taught about repeating decimals.

Good: I did have that advantage over the Greeks. All they had at first was integers and fractions. As you probably know, the Pythagoreans suppressed their discovery of irrational numbers, which made progress more difficult for their contemporaries. It was one of the earliest cases of a ban on new technology. There is a legend that they assassinated a man who revealed the secret; I suppose they were fundamentalists.

The Pythagoreans wouldn't have used decimals, but they might also have arrived at the proof by first "missing by 1" again and again before suspecting the irksome truth that $\sqrt{2}$ was not kosher modulo their religion. Perhaps they too solved the same Pell equation. So for me the decimal system was a distraction as well as an aid.

I was helped in the induction case because the formula in terms of n was given at the end of the book. Without the answer being given, I don't know whether I would have discovered it—I might have done so in the context of *scientific* induction, but I doubt it. It would have been a much better achievement if I'd both discovered the formula and discovered how to prove it.

Banks: What do you think was the wellspring of your creativity in mathematics?

Good: Interest—being interested is almost the entire answer. I thought, wrongly, that I had a bad memory, so I preferred logical thinking as a compensation. I liked mathematics partly because it was the only thing I could do well. At an early age I wasn't physically strong compared with my classmates, who were older. At first I was rather scared to play cricket with older boys, but I once scored 37 not out when I was about 17. At that age I reached the final round in the 220 yard race. The way I got training was by running to catch buses because I always got up late. I'd see the bus about 200 yards down the road coming along and nearly every morning I would get to the bus just in time. As Churchill would have said, I gave the bus a sporting chance to get away.

CAMBRIDGE YEARS

Banks: Could you tell me what it was like when you went on to Cambridge? How did the university seem to you?

Good: Well, I was at Jesus College, in part because it was something of a tradition to go there from my school. While I was in school we had two or three people going to Cambridge who went to Jesus College. I would have been better off, I suppose, if I had chosen Trinity College, which of course was a well-established mathematical college.

Banks: Do you remember any details of your mathematical lecturers at Cambridge?

Good: One was A. E. Ingham, who wrote a well-known tract on prime number theory. He was a very accurate lecturer. If he went into parentheses or even into brackets within parentheses when he was talking, he always emerged, closing everything perfectly. You could write down everything he said if you were a fast writer and you would generate a little textbook. He wasn't so much an inspired lecturer, but he was always interesting and always accurate. Another excellent lecturer was J. C. Burkill. Years later he told me my homework answers were the best of any student he ever had. G. H. Hardy was a rather inspiring speaker, much harder to follow than Ingham or Burkill, but his course was more advanced.

I also recall F. P. White, who taught projective geometry. He reminded me of a cartoon character, "Professor Strabismus, whom God preserve, of Utrecht." I used to come in a quarter of an hour late to his classes; there was a door at the back of the room so I could slink in unobtrusively; but not unobtrusively enough, for on one occasion he said people who come a quarter of an hour late shouldn't come at all. But I was pretty good at geometry.

L. A. Pars was my tutor at Jesus College when I was an undergraduate. He was a splendid mathematician and an excellent tutor. He later became the master of the college and he wrote a very well-received book on classical mechanics published after my time. He always had slick proofs, but they were too slick. He taught us quite a bit about complex variables. He seemed to delight in giving the solution to homework at the next class so quickly that you could hardly follow it.

Banks: Did you enjoy Cambridge?

Good: Most of the time I enjoyed it. I was extremely shy at that time so I didn't enjoy life as much as I might have done at that age. I think I cured my shyness much later by autosuggestion.

Banks: That's an unusual approach. I can't say I put as much stock in autosuggestion as you seem to.

Good: I certainly do, because of the success I had with it. It was popular in the 1920s. I first read about it seriously in a book called *Suggestions and Autosuggestions*, by Charles Baudouin, who was a follower of Emile Coué. The techniques were popular then with psychologists, but not now, and I think they are making a mistake. I think it's the most important idea in psychology from a purely practical point of view and I conjecture that it's not regarded as entirely respectable in the trade because it doesn't help psychologists to make a living, and perhaps because it is not a good topic for doctoral theses.

Banks: Apparently the basis for your support of autosuggestion is that it worked well in your case. Doesn't this seem to be a sample of size one? And isn't the one involved an atypical member of the population?

Good: Well, yes, but consider the combination of my experience plus Baudouin's book plus the uncontroversial existence of suggestion that isn't auto and which is effective enough to be a cause of wars and religions. There's a very simple experiment showing that autosuggestion does work to some degree, at least. Hang a weight on a string and imagine the weight going round clockwise, counterclockwise, back and forth or left and right. Without conscious muscular effort on your part, you find that after about half a minute, it goes the way you imagined. I think most people can do this experiment successfully.

Banks: Interesting; it still seems like a slender thread on which to hang a claim for the "most important idea in psychology."

Good: Yes, it works best with a slender thread. The point of the experiment is to break down initial resistance. Relaxation is essential when practicing autosuggestion, as in hypnotism. If you ask someone to try autosuggestion, you must first make little tests; for example, you hold them by the wrist and say I'm going to let go of your arm suddenly and your arm should drop as if by gravity. If it drops differently, then they are not yet relaxed. Some people find it difficult to relax to that degree, but usually I can teach anyone who can relax how to do autosuggestion in ten minutes.

Clinical trials take for granted the importance of autosuggestion; otherwise placebos wouldn't be used. I don't know what trials have been done to test the value of placebos themselves! Such experiments would be easy to do. I think we continually use autosuggestion unconsciously to maintain a stable self-image. Perhaps men who feel tough walk about doing isometrics, thinking "Clap that man in irons."

Banks: Were there classmates of yours who stand out in your memory?

Good: There were several, but let me mention my friend John Francis O'Donovan with whom I used to play a lot of chess. I think he played board one for Ireland in an international chess tournament at the beginning of World War II. It was held in Argentina and he stayed there—he's still there, teaching English at the university. He never fought in World War II; since he was Irish he didn't particularly feel that he should, though I think he regarded himself as more British than Irish. Similarly, my parents were from Russia, but I always thought of myself as British. My license plate is double-oh 7 IJG.

Banks: You've got an inimitable style. Would you like to comment on your graduate study at Cambridge?

Good: It was fairly natural for me to go on to graduate study because I knew I was pretty good at mathematics. I discovered that I wasn't quite as good as I thought, compared with the very best of the undergraduates. (Perhaps I played too much chess, which can easily become an obsession.) It was quite different from my school where my name was regarded as a synonym for mathematics, but at Cambridge I discovered there were other people just as good as I was.

My graduate advisors were Besicovitch and Hardy. I think what I most liked then was amazing formulas such as those of Ramanujan. I think the closest I've got to Ramanujan was in my paper on characteristic functions of functions. [Note: For citations to this paper and many others that are mentioned in passing during this interview, see Jack's publication list in Good (1983).]

In my doctoral program, my first research was on functions of a real variable. I probably chose that topic because I'd just taken a course on it. At one point I said to Besicovitch, "You can sometimes prove that a set is measurable by proving that its measure is zero, but now consider a nonmeasurable set of real numbers expressed in base 10. By leaving the digits as they are, but imagining the base to be 11, the new set becomes of zero measure and is therefore measurable. But the set doesn't deserve to be called measurable because it is not constructible, so it's a swindle." (I was unaware of the very similar construction of the Cantor set, moving from base 2 to base 3, at that time.)

Besicovitch then pointed out that the swindle could be avoided by means of the concept of Hausdorff instead of Lebesgue measure, in other words, by means of fractional-dimensional measure. Besicovitch had written several papers on the topic. He suggested that I might investigate the fractional

dimensions of sets of simple continued fractions defined in a simple manner. My research in this area was awarded a Smith Prize.

I found, for example, that the set for which the partial quotients a_n tend to infinity has fractional-dimensional number, or fractal number, $\frac{1}{2}$ (the fractal number of a set can be regarded as a measure of its texture, especially in two or three dimensions). When $\sqrt[n]{a_n}$ is unbounded, the fractal number is again $\frac{1}{2}$, and if $a_n = 1$ or 2 for all n, the fractal number is about 0.53. The set can be generated chaotically by the transformation

$$x_{n+1} = 1/(x_n + \delta_n),$$

where $\delta_n = 1$ or 2 at random. To get a planar picture, which would be visually more interesting, one could take $\delta = w$ or z at random, where w and z are complex numbers.

BLETCHLEY PARK: SECRET CODES AND EARLY COMPUTERS

Banks: When you left Cambridge, you began your World War II involvement with Alan Turing and the Enigma project. How did you happen to get recruited to Bletchley in the first place?

Good: I was on what was called the reserve list, so I didn't have to join the Army. They were deliberately reserving some of the mathematicians, physicists and so on, in case they were required elsewhere. That was different from World War I, which had no reserve list. Henry Moseley, the physicist who used x-rays to determine atomic numbers of elements, was killed in battle, and so was the poet Rupert Brooke.

Banks: I doubt that the World War II reserve list conserved poets.

Good: No, that's probably true, though I met one poet, Henry Reed, at Bletchley. He had been extracted from the Army, where he had written a famous poem about the naming of parts of a rifle. He again proved he was a poet by saying he was a chronic pneumonic. I was in the same billet as he and David Rees.

Banks: I don't recognize the name, Rees.

Good: I didn't know Rees at Cambridge. He was an algebraist, who worked on semigroups (in which elements are not assumed to have inverses). He was in on the ground floor because he judged early on that the topic was important. He first convinced me that the topic might be of value by pointing out the example of mappings of finite sets *into* themselves rather than *onto*. That struck me as a natural enough idea to be worthy of study but I didn't expect the topic to have much structure. It had enough

to get David Rees an FRS (Fellow of the Royal Society); there's also E. Hille's book on functional analysis and semigroups. I mentioned David Rees, as a colleague, in my chapter in *Codebreakers: The Inside Story of Bletchley Park* (Good, 1994).

Banks: Well, let me go back. How did you happen to land at Bletchley rather than in any of the several other areas of war research?

Good: I was offered two war jobs simultaneously. If I had taken the other one, I would have been in radar, although I didn't know that at the time. So then I might still have gotten into computers, via cathode ray tubes, but working with a firmer engineering background, rather like Tom Kilburn, who later came to Manchester to organize the construction of the Manchester University Computer.

In any case, as I said, I was on the reserve list. At one time Bletchley was hiring several people. No doubt the organization would have preferred to recruit cryptanalysts, but there weren't many of those, and mathematicians were thought to be rather good at that sort of thing. (They hired one cryptogamist by mistake!) So I was up for that job and also this other job involving radar.

Not knowing about radar, I thought it would be more romantic to work on German cipher systems. Although we weren't told precisely what kind of work was involved, another chess friend of mine, Bernard Scott, guessed it was about secret ciphers. We were both interviewed on the same occasion for this job. He showed his friendship by saying "Wear your scarf inside your coat, otherwise you'll look too much like an undergraduate," even though we might have both been applying for precisely the same job. In fact he did work on the Enigma cipher machine for a short time, but not on the Naval Enigma, and I lost touch with him during the war. Later, he headed the mathematics department at Sussex University.

Banks: Can you give me some better sense of the arrangement of things at Bletchley?

Good: Bletchley is a rather small town halfway between Oxford and Cambridge, and about 50 miles from London. Bletchley Park (BP) was where we worked. There was one large Victorian central building; two modern buildings and a number of improvised huts were constructed. Most of the huts still exist, although perhaps in disrepair. The government has agreed to establish BP as a historic area. There is now a pub in Bletchley called The Enigma. I had an invitation to visit BP not long ago, but I declined because I no longer like to travel much. I would have felt like a ghost. Through not attending, I missed meeting Prince Phillip.

Banks: What kinds of people were gathered together at Bletchley?

Good: Intellectuals of various kinds. Let me tell you how I met Hugh Alexander. He had been British chess champion twice, so he was well known in British chess circles. I used to play rapid chess with him before the war, in the comfortable chess club in the John Lewis store in London, which was later destroyed by a bomb. He was one of the people who interviewed me, and became my boss and friend for many years. At that time I could hold my own against him and against Golombek and Vera Menchik in rapid chess, but they would have won easily in serious chess. Vera Menchik was the strongest woman player ever, up to that time.

Bletchley Park liked chess players—they believed that chess players and mathematicians had an aptitude for cryptanalysis. Hugh's closest friend was probably Stuart Milner-Barry, another chess master. I met him at a chess match a week or two before going to Bletchley, and I asked "Are you working on German ciphers?" and he said "No, my address is Room 47, Foreign Office," but when I arrived at BP I found, sure enough, that he was working on German ciphers. Milner-Barry wrote a fine memoir for Hugh in *The Best Games of C. H. O'D. Alexander* (Golombek and Hartston, 1976).

I arrived at BP on the day the *Bismarck* sank, on May 27, 1941. Alexander met me at the train station and he immediately told me we were beginning to break the German Naval cipher, one of the German uses of the Enigma, an automatic encryption machine. Alexander was the deputy head of Hut 8, whereas Milner-Barry was the head of the adjoining Hut 6, which dealt with non-naval uses of the Enigma. When I arrived, the head of Hut 8 was Alan Turing, but after about a year, Turing was moved to work on developing methods for the encipherment of speech, and Alexander became the head of Hut 8. He was first class both technically and as an administrator. When the work on the Enigma needed more resources, Alexander, Milner-Barry, Turing and Welchman wrote a successful appeal to Winston Churchill, over the head of the director of BP.

Another person, who arrived somewhat after I did, was Max Newman. He was a Fellow of the Royal Society, a mathematician who'd written a well-known book on plane topology. He was also interested in logic. After the war he became president of the great London Mathematical Society and was highly influential in appointments in the U.K. He did not encourage people to work on logic. Turing had been his student in Cambridge before the war, but now Turing had become more famous, being regarded as a genius. Turing's reputation still grows; the play *Breaking the Code* was about his life and

death, as was the book *Alan Turing: the Enigma* by Andrew Hodges.

In 1943 I moved from Hut 8 to the "Newmanry," working on the use of machine methods for decrypting the German teleprinter cipher system that we called Fish. Donald Michie and I were Newman's first two cryptanalytic assistants; eventually there were at least 16, including the famous topologist, J. H. C. Whitehead. There were also about six engineers and 273 Wrens (members of the Women's Royal Naval Service). Newman had previously worked in a section, under Major Tester, where hand methods were used.

Being in on the ground floor was a big advantage (though the initial breakthrough was made by W. T. Tutte, who deduced the structure of the machine; based on his work, our job was to infer the current day's parameter values, or the key, and then decrypt messages). I did much of the cryptanalytic and statistical research of the section.

Banks: What are hand methods?

Good: The non-machine approaches. Peter Hilton (now a prominent mathematician), for example, could think of two teleprinter letters (pentabits) in his mind's eye and add them together modulo 2 almost instantaneously. I think Newman felt inferior in the "Testery" and especially when he compared himself with Peter. So he thought "Well, this is really mostly mechanical work. We need a machine for this purpose." I once said this was unconsciously suggested to him by his familiarity with Boolean logic, but Newman denied that (rightly or wrongly). He said he had simply felt this was the way to do things properly. Newman probably knew of the so-called Bombe, which was the electromagnetic cryptanalytic computer used for the routine attacks on the Enigma cipher. So you've got a sort of square—Enigma, Bombe, Fish and, ultimately, the Colossus computer, used as an aid to deciphering Fish messages.

So as part of the Fish project, a machine was built, mainly to Newman's and perhaps Turing's specifications. The first cryptanalytic machine for attacking Fish was called "Heath Robinson," which was the name of an cartoonist like Rube Goldberg. It tended to go wrong frequently. Some of its faults could be diagnosed by the sound or even by the smell, when it was trying to catch fire. Some statistical work by Donald Michie and myself, and my insistence that what is not checked is wrong (Good's law), led to occasional success by Heath Robinson, and this was enough to justify the building of a better machine.

That machine was the Colossus, which has some claim to being the first working large-scale electronic computer. The main engineer was Tom Flow-

ers, who deserved a knighthood. It wasn't a general-purpose computer, though it did become more general purpose than was originally intended because it was based on Boolean logic, which gave it a flexibility that came in very useful. It was about 12 feet wide, 7 feet high and 8 feet thick. It had about 2500 bottles or what we called electronic valves, which were like vacuum tubes, though they were gas-filled thyratrons.

Most engineers thought a machine with so many tubes couldn't possibly work, and I doubted it too. Just on probability grounds, you'd expect some of the tubes to break down each time the machine was used. But Flowers happened to know something that most people didn't know, namely, that if you leave the tubes on all the time, then, if they don't fail early on, they are much more reliable than if you turn them on and off. Sure enough, after they had some running time, there was little trouble; if a tube went wrong, it was replaced by a new one. Gradually the machine, and its successors, became highly reliable. I once calculated that the machine could sometimes run for 10^{11} binary operations, within the whole machine (which was partly parallel), before it went wrong. I should say before it went *noticeably* wrong, since it was possible for the machine to have errors that wouldn't make any essential difference.

The building of the Colossus required half the staff of the Dollis Hill Research Station (which was normally responsible for telephone research). Most of the staff weren't told what their work was for because of the "need-to-know" principle.

This principle was pervasive at BP. When I was working on the Enigma, it was some time before I asked Turing "How on earth did we discover the wirings of the wheels?" I didn't have the right to ask the question—I didn't need to know—but my curiosity eventually overcame my scruples. And Turing was vague, he said "Well, I suppose the Poles," and I said "And I suppose a pinch," a pinch being a capture. (At that time I didn't know of the contribution of the Poles; three Polish mathematicians, who, using information obtained by the French secret service, had applied group theory and guesswork to solve an early form of the Enigma machine.) Later, we captured a U-boat (submarine) which contained an Enigma machine and some keys. The captain of the U-boat was supposed to have immersed the keys in water, which would have removed all the print, and he realized he'd forgotten to do that. After he emerged from the submarine he tried to go back and was shot. So he didn't manage to destroy the keys, which were very useful for the next two or three months.

They were going to tow the U-boat into port, but fortunately it sank. I say *fortunately*, because if it hadn't sunk, the Germans might well have discovered we had captured a U-boat, and they would have assumed we had captured the keys. If they didn't change from the Enigma itself, which would have been very expensive for them and unlikely, at least they would have printed new keys.

Banks: What kinds of mathematical work did you do at Bletchley Park?

Good: I did several kinds of mathematical work there, some of it was fairly technical and some was fairly elementary. For example, I devised decision trees for the Colossus computer. Also, instead of summing squares, I suggested summing absolute values because it was faster. There was a simple way of converting summed absolute values into a "sigmage," rhyming with "porridge." Speed was of the essence.

Banks: I imagine there were lots of applied tricks that to a mathematician were sort of trivial, but in practice were important and useful.

Good: Sure, I can mention another one. It was extremely trivial. When I first arrived on the Enigma project they were working on a process called Banburismus, which was a sequential Bayesian cryptanalytic process. They were using decibans (weights of evidence), with one decimal point. So I thought, why don't we drop the decimal point and call the unit a centiban, thus saving a lot of writing. And then I noticed that if we used a half deciban (hdb) we would save much more time in both writing and arithmetic because most of the individual scores would then be single digits. I also worked out (and this is where it wasn't entirely trivial) how much information (expected weights of evidence) we would lose by this additional rounding off, and it wasn't much. The formula looked a little like something that arises in Planck's original paper on quantum theory.

This must have saved half the time of the work on Banburismus. Of course, every numerical analyst knows that you shouldn't carry more decimal places than you need, in hand calculations, and it was essentially in that spirit that I made this suggestion, but here were these highly intelligent people, who for some weeks had been using the deciban with a decimal point.

Banks: What was the intellectual and social life like at Bletchley Park?

Good: Speaking for myself, my main social life was with a few friends, and going to dances at Woburn Abbey, where the Wrens were billeted. In the bus returning from Woburn Abbey, Peter Hilton would lead the singing of bawdy songs.

Also I played chess and Go and had intellectual and mathematical discussions with Turing and Rees. For example, Turing and I discussed the possibility of machine intelligence and automatic chess. Other people were involved in drama and tennis and, presumably, sex. There were many young ladies around, but I was too shy and too busy to profit much by that.

Banks: How were things administered at Bletchley? Some of the books on the subject make it sound like you were all an ungovernable pack of eccentric boffins.

Good: None of my three bosses was bossy. There was no point in being bossy because everybody wanted to help the war effort. Turing was one of the eccentrics. He was head of Hut 8 when I arrived, but I don't think he liked administration and he wasn't too good at it. It is probably just as well that Alexander eventually took over.

After Turing left Hut 8, he continued to live at his billet, The Crown Inn, three miles from BP. I kept in touch with him because he had taught me the game of Go and I used to visit him to play the game. (I didn't know he was homosexual.) Eventually I was able to give him a handicap of six stones. He thought deeply rather than quickly and said his I.Q. was only about the average for Cambridge undergraduates. Also I think I had the advantage of having played a lot of chess in which all players think, "If I go there, then he might go there, then I could go there, and if instead he goes there, then ...", in short, a tree analysis with evaluations at the end nodes. I think I transferred an ability from one field into another one.

Soon after the war, my main Go opponent was Roger Penrose. He was a little better at the game than I was, but that wasn't why he was knighted. It is a fascinating game, but I've played very little in the last 40 years. I think it is more complicated than chess, and chess than checkers, because $3^{361} >> 13^{64} >> 5^{32}$. Sometimes I discussed consciousness, mathematics and physics with Roger and his brother Oliver. The earliest discussions occurred before Roger had become a physicist, when I was puzzled that $\sqrt{-1}$ seemed to have a real physical meaning. Later discussions convinced me that I didn't understand quantum mechanics. I was relieved to learn in my later reading that if you think you understand it, then you don't. (I've modified the way that Niels Bohr expressed it.)

Banks: Perhaps you could comment on Turing's influence in your development in statistics and probability.

Good: He invented a Bayesian approach to sequential data analysis, using weights of evidence (though not under that name). A weight of evidence is the logarithm of a Bayes factor; for a Bayesian, this is the only possible definition, and the concept has been an obsession of mine ever since.

Once I asked him for the "real reason" that when the Fourier transform of $f(x)$ is $g(t)$, then the Fourier transform of $g(-t)$ is proportional to $f(x)$. Turing drew my attention to the discrete Fourier transform (DFT), which I have used in about 20 publications, including (1) my form of the fast Fourier transform, (2) the exact distribution of Pearson's X^2 statistic for the "equiprobable" multinomial, (3) for speeding up the penalized likelihood method for probability density estimation in one or two dimensions, (4) for the enumeration of rectangular "arrays" (a famous combinatorial problem), (5) for a generalization of complex function theory, (6) for number theory, (7) for a geological or evolutionary application (with Norman Gilinsky), (8) for a new finite series for Legendre polynomials (related to an analog of Poisson's summation formula), (9) for a discrete multidimensional analog of Poisson's summation formula and (10) for polynomial products. Brian Conolly and I published a table of DFT pairs.

Another influence was that Turing had an empirical Bayes approach to code-word frequencies. This led to a substantial paper of mine, and later to one written jointly with George H. Toulmin, dealing with the frequencies of words and species. For example, I deduced a simple formula for the probability that the next word sampled will be one that has not previously been observed. Makers of dictionaries and teachers of languages ought to know about this work, because it tells you the minimum size of vocabulary required to cover, say, 98% of running text.

One of Turing's ideas was developed by me in a paper on regenerative Markov chains. He also pointed out some properties of weight of evidence, which I generalized in a paper on false-alarm probabilities. Also, jointly with Toulmin, I found that expected weight of evidence, was a natural tool for proving a coding theorem in Shannon's theory of communication. Weights of evidence are closely related to amounts of information.

On one occasion I happened to meet George Barnard during the war, in London, and I confidentially described the use of Bayes factors and their logarithms for distinguishing between two hypotheses sequentially. Barnard said that, curiously enough, in his work for the Ministry of Supply, he was using essentially the same method for choosing between two lots of manufactured goods. Thus Turing and Barnard invented sequential analysis

independently of Wald. Barnard has forgotten this discussion. I remember it with great clarity; he and I were standing next to a table in his home and a third person had gone down one level to the restroom while we were talking. If that person had been present, I wouldn't have mentioned the topic, though it was obviously not a secret concept. I thought that perhaps it would be useful to tell Barnard about this, because he might be able to use it in his war work, but he was already using it!

Turing developed the sequential probability ratio test, except that he gave it a Bayesian interpretation in terms of the odds form of Bayes' theorem. He wanted to be able to estimate the probability of a hypothesis, allowing for the prior probability, when information arrives piecemeal. When the odds form of Bayes's theorem is used, it is unnecessary to mention the Neyman–Pearson lemma. One morning I asked Turing "Isn't this really Bayes' theorem?" and he said "I suppose so." He hadn't mentioned Bayes previously. Now, Harold Jeffreys with Dorothy Wrinch had previously published the odds form of Bayes' theorem (without the odds terminology and without the sequential aspect), and Turing might have seen their work, but probably he thought of it independently.

In his book on probability, Jeffreys wanted to sell his methods, so nearly always he assumed that the prior probability of a hypothesis was 1/2 (as did C. S. Peirce in 1878, in relation to his throw-away line on weight of evidence). So the Bayes factor simply turned out in nearly every application made in that book to be simply the final odds, and I think that was, so to speak, for political reasons. He didn't want to appear subjective in the *first* edition of *Theory of Probability*. He aimed to use logical probability (called *credibility* by Bertrand Russell and others).

Banks: Did you become a Bayesian in Hut 8 or were you already inclined to that way of thinking?

Good: I'd already read J. M. Keynes on probability and had been reading some of Harold Jeffreys, which, by the way, Maurice Bartlett thought ought not to be taught at Cambridge at the same time as classical statistics. In other words, he wanted everybody to be brainwashed according to the "orthodox" methods and not to be confused by a conflicting philosophy. (Incidentally, the first English use of the word *Bayesian* occurred in my review of de Finetti's 1956 paper "La notion de 'horizon bayesien'," as a translation from the French, and the first use of *Bayes factor* occurred in my 1958 paper "Significance Tests in Parallel and in Series.") Jeffreys' book on probability is rather hard going for a beginner,

and it starts philosophically with a chapter on scientific inference and how one discovers natural laws from experience. As a philosopher, I was interested in the Bayesian approach to the philosophy of science, which I think covers what is correct in the Popperian approach. My taste has mostly been toward applicable philosophy. As you said over lunch, when a topic becomes sufficiently worked out, it tends to leave the Philosophy Department, but I don't agree with a remark you made once in print that an idea is not respectable until that happens. Philosophical respectability is possible, as well as statistical respectability.

Banks: You left Bletchley around 1945 and went to Manchester, arriving there before Turing. What did you do at Manchester?

Good: I was recruited to Manchester by Max Newman, who took a professorship there after the war. My official position was as a lecturer (which corresponds roughly to an American associate professor) in pure mathematics, and I had some responsibility for thinking about the electronic computer. F. C. Williams—of the Williams tube device—soon arrived. He was the first engineer hired. He brought in Tom Kilburn and handed over the project to him a little later. D. R. Hartree, the physicist who had built a differential analyzer [Note: an analog machine; the first differential analyzer was built by Vannevar Bush, in Cambridge, MA] out of Meccano (a British toy similar to an Erector Set), had visited America and learned about the American work on electronic computing. Newman, with support from Hartree and P. M. S. Blackett, had obtained a grant (approved by the Royal Society) for building an electronic computer. Newman's aim was to do pure mathematics on the computer, but it turned out to be mainly a number cruncher and data processor. The tool creates the demand (supply-side economics?) and the number of multiplications in a random algorithm has a thick-tailed distribution like a log-normal. I made a mistake that many other people made, thinking that one computer was enough for all the calculations to be done in the British Isles. Newman more correctly judged that computers would be a million-dollar industry. Of course, it's now well over a billion dollars.

By the way, I have updated a biographical note on Newman, based on one by Shaun Wylie, for the forthcoming *New Dictionary of National Biography* (Oxford University Press).

I was at Manchester for a couple of years only, and, on the side, I tried to understand quantum mechanics without much success. A philosopher of science ought to know something about how the uni-

verse works, so I was learning and kept a notebook of barely-baked ideas.

I didn't do much on the electronic computer project, but I made about 90 pages of notes, some of which were distributed to Newman, Rees, Williams and Kilburn. I have given copies of most of the notes to 12 historians of computing. These notes are number 975 in my bound papers, copies of which are in the VPI Statistics Department and university library. I've promised to bequeath copies of those bound papers to the university libraries at Oxford and Cambridge. Up to 1990, there are 8932 pages. The Oxford librarian says he hopes he'll have to wait a long time!

Independently of M. V. Wilkes (and perhaps earlier) I had the (undeveloped) idea of microprogramming, but for the *user* of the computer; that is, the user could reconstruct the elementary instructions of the machine for speeding up specific programs. I think I thought of this by analogy with Colossus. I called the idea "machine building" in quotes. The idea was ignored.

After I left Manchester, Newman hired Turing to develop ideas for the electronic computer, for he had previously designed the logic of the National Physical Laboratory's electronic computer, the ACE (automatic computing engine), but Turing wanted to do all the work himself; he wanted to be the engineer and the programmer. He didn't want the baby to be bisected, but it was, both at the National Physical Laboratory and at Manchester.

Banks: That certainly seems of a piece. He is said to have been very much into hobbies and doing things his own way.

Good: Oh, absolutely. He liked doing things from scratch. For example, he knew how to distinguish poisonous from regular mushrooms and he would collect, cook and eat them. Also, I understand that he rewired his house without calling in a professional.

Banks: You were at Manchester until they replaced you with both Turing and A. H. Stone.

Good: When I want to boast, I say they needed two mathematicians to replace me, though of course the department was growing at the time. Newman built up the mathematics department at Manchester; he had lots of initiative.

ON HER MAJESTY'S SECRET SERVICE

Banks: After a few years at Manchester, you left for the Government Communications Headquarters (GCHQ). Was there any particular thing that led you to leave Manchester or was there something that drew you to GCHQ?

Good: I didn't much like lecturing at that time. Here at VPI, I was appointed as a research professor, and specifically exempted from teaching duties, but I've been happy lecturing some of the time to statistics students. But last year, I was asked to give *two* courses of lectures per year and I decided to retire. Before my letter of retirement had been seen, the administration raised the ante to four courses, which would have effectively ended my research activities. Thus I'm the guy who retired just before he was fired, though I'm allowed to keep my office for as long as I wish. Of course the attempted firing had originated from the government of Virginia Commonwealth. To help to balance the budget, they thought it would be kind of cute to encourage well-paid professors to retire if they did not still have outside contracts. I had previously brought in more than a million dollars from N.I.H. (National Institutes of Health), but I prefer the freedom of not having a grant.

In Manchester I was very conscientious about the teaching. Max Newman once said it's possible to be too conscientious. You can easily use up all your time preparing lectures and marking examinations and I think some people do that. For some, it is the best contribution they can make, but had I done that, I think it would have been a waste of my research ability, although I know that teaching is a good way to learn. I later discovered that one of the students at Manchester thought I was the worst lecturer in the department, perhaps because of my shyness, but another student thought I was the best. One of my students was G. B. Whitham. His mathematical ability was clear because, already in his first year, he wrote mathematics in grammatical sentences, and therefore eventually he became an FRS. Another student used to ask absurd but imaginative questions. He became a full professor. Don't knock wild speculation.

A second reason for moving from Manchester was that my book *Probability and the Weighing of Evidence* (Good, 1950), which was completed in 1948, had been declined by the Cambridge University Press. Who the referee was, I don't know, but it could have been Jeffreys, since Frank Smithies told me he saw the manuscript on Jeffreys' desk. I was discouraged and did nothing about the manuscript for several months at least. Then Donald Michie, a good friend of mine right from the Newmanry days, said "Why don't you send it in somewhere and let someone else do the work if you're not working on it?" I said "Look, the manuscript's in Manchester and I'm in London." He replied "Take a train to Manchester and come back the same day on another train." I didn't have enough faith in myself

without his moral support, and I did what he advised and submitted the manuscript to Charles Griffin, who published it in 1950. (Most publication was slow at the time.) I think it wasn't such a bad little book. Somebody phoned me recently and called it a classic. (Perhaps judgments should be made by oracles. Marc Kac's naughty philosophy of refereeing was to accept everything and "let history decide.") Anyway, referring back, the Cold War was heating up and I thought I could do more good in government service.

Banks: Without teaching, you perhaps had more research opportunities at GCHQ?

Good: It's hard to say. My original work on probabilistic causality was done in the evenings and weekends, when I was at the Admiralty Research Laboratory (ARL), outside office hours. I worked on it for a whole year. One referee rejected it in a cavalier manner, but a second one was enthusiastic about it. It seemed to me that causality ought to be described in terms of probability; Jimmy Savage once made that remark to me and I think that's what started me in that direction.

Banks: When did you meet Savage?

Good: The first time I met him, he came to see me when I was in London. It was in 1951 or 1952 while he was working on his book. He'd been working in France and was visiting England briefly. He knew I'd written the 1950 book, so, perhaps on his way back to the U.S., he visited my home. Jimmy and I began corresponding after that. He pointed out an error in my very first paper on causality, when I sent him a draft. Later I saw him in Chicago. He was remarkably well read for a person with such bad eyesight. I once complimented him on the modesty of his delivery of a lecture. He replied that it is important to appear modest. He was a frequentist who defected, like Lindley.

Banks: Speaking of famous statisticians, did you have any interactions with Fisher during this time?

Good: I knew Fisher, but not well, and generally was on pretty good terms with him. He once told me that the best thing he could do for genetics was to teach it to mathematicians. He also said that he found my 1950 book interesting.

There was a small difficulty once when Fisher and I were both invited to be discussants at a lecture by R. B. Braithwaite (November 22, 1954), who'd been appointed as Professor of Moral Philosophy at Cambridge. In the discussion, after considering a hierarchy of probabilities, I said that the problem with minimax procedures (which Braithwaite had been proposing for ethical decisions) and with any objectivistic procedure was that they threw away information (such as by shutting one's eyes to the specific randomization, for example), and I said this criticism applied even to the work of R. A. Fisher. I meant it largely as a compliment, implying that Fisher could be regarded as the father of statistics, but he rose with a white face and said "Kindly direct your remarks to the lecturer's words" or something to that effect. That evening, he told Henry Daniels I was an upstart. I wrote to Fisher immediately afterward, explaining that I hadn't intended to cause a falling out, and he wrote back that he'd gotten the impression that the organizers had deliberately selected the two of us as discussants in order to get us into a dog fight.

He knew he had a bad temper—he once told George Barnard that it was the bane of his existence. It made me feel much better about Fisher when I heard that he had confessed.

It is not always realized that Fisher was somewhat of a Bayesian, paradoxically enough. His fiducial argument was a failed attempt to arrive at a posterior degree of belief without mentioning a prior. (Harold Jeffrey pointed out what priors would patch up the argument.) Another example of his Bayesian proclivities was his 1957 paper "The Underworld of Probability," which was concerned with a hierarchy of probabilities reminiscent of part of my 1952 *JRSS* paper "Rational Decisions" (Good, 1952).

Banks: Much of your work at GCHQ was classified, but can you give me any sense as to what types of mathematics or what types of things you were thinking about?

Good: I think it's better that I don't say anything about GCHQ, except that I resigned because I had accepted a full professorship in Chicago, but I changed my mind for personal reasons. One thing I did later, at the Admiralty Research Laboratory, which wasn't classified, was a paper on how to estimate the direction of a Gaussian signal.

Banks: You left the Government Communications Headquarters in 1959, there was a brief interlude because of some odd administrative procedure and then you were appointed to the Admiralty Research Laboratory.

Good: Yes, the RNSS (Royal Naval Scientific Service) paid scientific employees at GCHQ, so, after resigning from GCHQ and having been replaced, I was still an employee of the RNSS! Meanwhile, I did a few weeks of consulting with IBM in America, and later they offered me a job, which I declined after much deliberation. I did the first evaluation of Frank Rosenblatt's Perceptron at the request of IBM. While I was visiting the IBM Mohansic Research Laboratory in Yorktown Heights, I also wrote a paper on the kinds of mathematics that might come in useful in information retrieval.

The Perceptron, and a 1949 book by the psychologist Donald Hebb, provoked me to write an article called "Speculations Concerning the First Ultraintelligent Machine," based on the concept of artificial neural networks and what I called a subassembly theory of the mind. I thought neural networks, with their ultraparallel working, were as likely as programming to lead to an intelligent machine, but brains use *both* methods; they have parallel architecture and also use language and reasoning. So we can learn *from* our brains as well as *with* them. When discussing complex systems, like brains and other societies, it is easy to oversimplify: I call this Occam's lobotomy. Evolution is opportunist; it doesn't *have* to choose when a compromise works better.

One of the suggestions in my article was that the communication between artificial neurons might employ microminiature radio transmitters and receivers. If this is possible it would save an enormous amount of wiring. The network would be capable of learning if the amplitudes could be increased or decreased according to the amount of use of the transmitters and receivers.

Banks: You came to the U.S. in 1962 to work at the Institute for Defense Analysis (IDA). Can you talk about any of your work there?

Good: A lot of it was unclassified, but I still think they'd prefer me not to discuss any of it apart from my monograph *The Estimation of Probabilities: An Essay on Modern Bayesian Methods* (Good, 1965).

My boss there was Dick Leibler, of the Kullback–Leibler or Kullback divergence (which had previously been called expected weight of evidence; Kullback told me his work was sparked by an unpublished paper of mine). Dick's administrative philosophy was that he judged you by your productivity and didn't care what hours you worked. He was another non-bossy boss. The Princeton branch of IDA was organized to save your time—the less technical employees would do almost anything to free up your time for work. It was very different from an ordinary Civil Service environment. People would shout down the corridor "I'm now going to give a colloquium," without prior announcement.

THE ACADEMIC LIFE

Banks: After your stint at the IDA, you were a research fellow of Trinity College at Oxford. How did that come about?

Good: John Hammersley, who was a Fellow of the college, informed me that they were seeking to fill the position and he invited me to apply. There were 123 applicants for this three-year appoint-
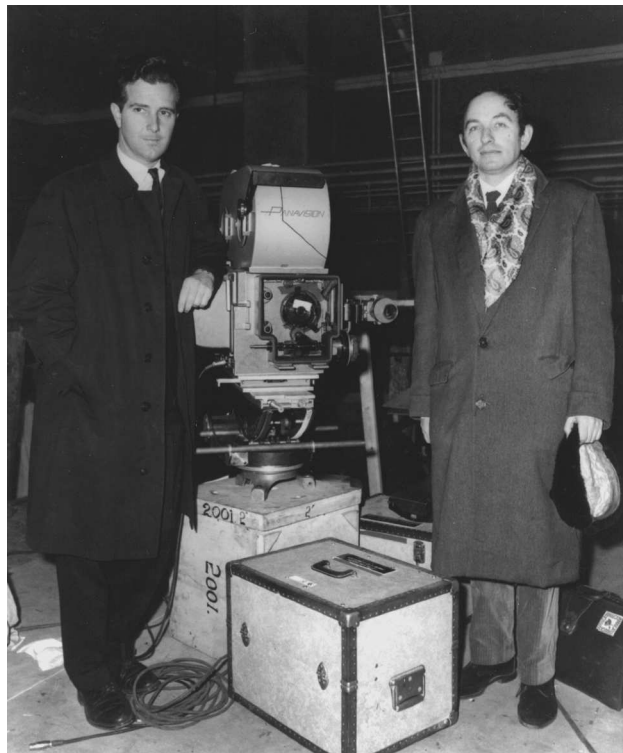

FIG. 3. *Fred Ordway, III and I. J. Good at Borehamwood Studios, Elstree, Hertfordshire, England when the movie "2001 Space Odyssey" was being produced.*

ment, which was joint with the Atlas Computer Laboratory, where the head and deputy were my friends Jack Howlett and Bob Churchhouse. For a few months, the Atlas was the fastest computer in the world, but IBM overtook it. The laboratory was 16 miles away from Oxford, along a road dangerous for an inexperienced driver, so I didn't visit the lab as often as I would have liked. I did most of my work at the college, some of which was on the "underware" for a "five-year plan" for chess programming. One day I came into the laboratory and found my office taken over, as in the beginning of *The Loved One* by Evelyn Waugh, but, as you've presumably noticed, I didn't commit suicide. Jack and Bob are still my very good friends.

Banks: How did you happen to come to VPI?

Good: I was invited at a time when my three-year appointment in Oxford was coming to an end, and I accepted provided that the pay was doubled and that I wouldn't have to teach. (The increase was 67%.) I arrived in Blacksburg in the seventh hour of the seventh day of the seventh month of year seven of the seventh decade, and I was put in apartment 7 of block 7 of Terrace View Apartments, all by chance. I seem to have had more than my fair share of coincidences. I have a quarter-baked idea

that God provides more coincidences the more one doubts Her existence, thereby providing one with evidence without forcing one to believe. When I believe that theory, the coincidences will presumably stop.

Banks: More numerology—surely it is of the bad kind, in your categorization. Perhaps we can discuss the origins of some of your major research interests. How did your work on contingency tables begin?

Good: One of the related problems close to philosophy is the estimation of the probability of one category of a multinomial when the order of the cells is irrelevant. The philosopher W. E. Johnson assumed that the estimate ought not to depend on the ratios of the observed frequencies of the other categories. Given that assumption, together with a permutability assumption, he proved (1932) that the correct estimate could be obtained by adding a flattening constant k to each cell frequency, and then normalizing to make the total 1. (The proof breaks down for the binomial case; there is no evidence that Johnson knew this.) I don't think Johnson realized that his estimate was *equivalent* to (not merely deducible from) that found by assuming a Dirichlet prior for the multinomial probabilities (this follows from a generalization of a theorem due to de Finetti), but since I think the roughness of the other frequencies *is* relevant, that led me on to the development of a hyperprior for the hyperparameter k, which I discussed at some length in my book *The Estimation of Probabilities* (Good, 1965) and in several later works.

The next problem along these lines was testing "independence" in contingency tables, where similar methods, combined with a neat trick, could be applied. Some of the work was joint with J. F. Crook. (Before he received his doctorate, we would introduce ourselves as Dr. Good and Mr. Crook.) That was an early example of hierarchical Bayesian analysis, which I had already suggested, in a philosophical sort of way, in my 1951 paper on "Rational Decisions." In that paper (Good, 1951) the example I gave of a hierarchical Bayesian idea was that of a "Type II" minimax procedure, one step up in a Bayesian hierarchy.

The econometrician, L. Hurwicz, turned out to have published an abstract a few months before my 1951 paper, suggesting the minimax example, and there may have been some even earlier mention of hierarchical Bayes—it is difficult to trace the origins and anticipations of such simple ideas. The work also led to an interesting Bayes/non-Bayes compromise statistic that generalizes likelihood ratio procedures to apply at higher levels in a Bayesian hierarchy.

Another piece of work on contingency tables (1956; previously rejected in 1953) anticipated the EM algorithm in a special case. This work made early use of a log-linear model before that expression was used. [Note: In 1963, Good showed that log-linear models are implied by maximizing entropy.] Most things are not (entirely) new under the sun, as pointed out by Stephen Stigler, who was anticipated by *Ecclesiastes*. I also anticipated the generalized linear model in a small way. I was acknowledged for both of these minor precursors by those who developed them.

I rediscovered some elegant algebraic work on "prime words," largely anticipated by the algebraist Marshall Hall, who was anticipated in his turn by Philip Hall, both of whom were "Halls of fame." However, my work originated from a statistical problem, and I conjectured a prime-word theorem analogous to the prime number theorem.

Banks: When did you get involved with the fast Fourier transform?

Good: I knew about the adding and subtracting algorithm of Frank Yates for interactions of all orders in 2^n factorial experiments, and I realized that this could be expressed as a multivariate discrete Fourier transform (DFT), modulo 2. (See my 1953 review of a book by Quenouille.) Frank Yates had not realized this; I asked him in 1966.

The assumption that high-order interactions vanish is equivalent to filtering out high frequencies and, hence, by using the inverse transform, to smoothing and probably "improving" the original data as in image reconstruction. I saw in 1956 that Yate's algorithm could be extended to a general fast Fourier transform by making use of a relationship between multivariate discrete Fourier transforms and univariate ones based on the Chinese remainder theorem. This relationship applies also to other transforms, such as Walsh's and Hartley's. Even the continuous multivariate Fourier transform (and other integral transforms) can be expressed as a succession of ordinary univariate transforms. Basic too is the surprising fact that the Kronecker (or direct) product of n matrices is equal to the ordinary product of n large but "sparse" matrices (elements nearly all zero). [*Note*: See papers 20, 146, 209 and 708 in Good (1983).]

The extremely valuable FFT has an extensive history and a "literature" comparable to that of communication theory. I can add something personal to that history.

John Tukey (December 1956) and Richard L. Garwin (September 1957) visited Cheltenham and I had them round to steaks and fries on separate occasions. I told Tukey briefly about my FFT (with very

little detail) and, in Cooley and Tukey's well known paper of 1965, my 1958 paper is the only citation. At first we thought the methods were the same, but they use a different relationship between univariate and multivariate DFT's. Both methods are valuable.

Garwin told me with great enthusiasm about his experimental work related to "parity violation" in physics. Unfortunately, not wanting to change the subject too much, I did not mention my FFT work at that dinner, although I had intended to do so. He wrote to me on February 9, 1976 and said, "Had we talked about it in 1957, I could have stolen [publicized] it from you instead of from John Tukey in 1963 or thereabouts." That would have completely changed the history of the FFT. In 1963, Garwin became highly enthusiastic about the FFT and was an "entrepreneur and missionary" for the paper by Cooley and Tukey (1965). He said that a scientist's work is not finished until it is published and often not even then.

The Cooley–Tukey method was anticipated, at least in part, by a few writers, going back to Gauss, but Gauss' contribution was published only posthumously (Heideman, Johnson and Burrus, 1985). Most methods can be regarded as based on the factorization of the multivariate DFT matrix, and specialized circuits are sometimes used. C. V. Loan said that I was the first to publish factorizations of the [multivariate] DFT matrix which are central to his book *Computational Frameworks for the Fast Fourier Transform* (Loan, 1992, page xii) and he adds that "Life as we know it would be very different without the FFT." A "hard science," by definition, is one that makes use of the FFT.

It often happens that a field is ready for a tool and several people independently reach similar ideas. In 1951, I proposed the idea of paying people in such a way as to encourage honest probability estimates. This idea was anticipated a year earlier by Brier, in a meteorological journal, who suggested a quadratic payoff, whereas my payoff was logarithmic and related to entropy. There is now an extensive literature on this topic, which Jacob Marschak described as a new branch of economics.

Questions of anticipation are often difficult. Sometimes the early statements of an idea are not clear or are not published in the best place or not much publicized, and they can arise in such specific contexts that one cannot tell whether the first writers realized their wide applicability. For example, this was true of my early use of the EM method. Often an idea is overlooked for decades and then rediscovered, the influence of the originator having died

out. This creates problems for kudologists, who have enough problems anyway.

Banks: Your work on bump hunting came much later, but it led to the method of penalized likelihood, which is now a widely used concept. How did this begin?

Good: I got interested in bump hunting because at the Waterloo Conference in 1971 a couple of physicists were asked what was the most important statistical problem in their work, and they said it was the problem of finding significant bumps in histograms. Now, in my 1950 book I had vaguely discussed bump hunting, though not under that name, and when I heard this was of interest to the physicists, I decided to think about it more carefully. That led me to the method of penalized likelihood, which I developed in conjunction with R. A. Gaskins and M. L. Deaton. I have since found that E. T. Whittaker anticipated the idea (for graduation or the smoothing of serially observed data, rather than for bump hunting), but people had been overlooking or ignoring it.

Banks: Your account of the practical impetus for your entry into serious bump hunting also suggests how your psychology rescued you from being a probabilist, into which you might very well have been drawn.

Good: It would be more complete to say I'm a Jack of all trades (period!). I am sure you mean I'm not a full-time card-carrying mathematical probabilist, but I have by no means neglected the topic in its theoretical and practical aspects. It has mostly been combinatorial rather than "measury-weasury," to use an expression of Jimmy Savage's. An example of that work is the use of a generalized Lagrange expansion for multivariate branching processes, applicable in polymer research and for the enumeration of colored planar trees. Other examples were the geological application mentioned earlier and a neat proof of a conjecture by Freeman Dyson.

A wonderful thing about probability is that sometimes what seems intuitively impossible can seem intuitively reasonable by thinking in a different way. This was mentioned at my 70th birthday celebration (a Festschrift published three years later in a special issue of *Journal of Statistical Planning and Inference*). This doesn't mean that you have to do the detailed mathematics or hardly any of the mathematics. I was thinking then especially of a gambler's problem, based on a sequence of coin tosses. Naive people, like me at one time, assume each player is ahead about half the time, and are surprised to find that the ultimate winner is probably ahead most of the time. (I have in mind the arcsine law; see Feller's *An Introduction to Probability*

Theory and Its Applications, Vol. 1.) Here the distribution of the time the ultimate winner is ahead is not easy to derive, but some qualitative thinking switches the intuition. Likewise the "birthday problem" becomes intuitive when you notice that among 23 people there are 253 pairs, and the probability of no matches is approximately $(1 - 1/365)^{253} \approx \exp(-253/365) = 0.50000$. (This extreme accuracy, which is not the point, but adds to the fun, occurs because 253/365 happens to be a convergent for the continued fraction for $\log_e 2$.) Similarly, but in geometry, Pythagoras' theorem becomes obvious if you drop a perpendicular on the hypotenuse and get three similar triangles. Euclid also knew this method [Book VI, Proposition 31], and I rediscovered it somewhat later. I like "real reasons" or intuitive explanations.

I like to resolve paradoxes. I recently wrote a paper on the kinematics of special relativity for that purpose. Herbert Dingle had claimed that there was a contradiction in the special theory and my paper answered briefly, and I hope clearly, all four of his arguments. (I also defended the kinematics of the special theory against an ingenious argument by Ian McCausland and two arguments by George Galeczki.) Dingle was misled by a remark of Einstein's that was not literally correct. I am convinced that the kinematics of the special theory is self-consistent and can be disproved only experimentally, but there are still flat-earthers, who think the theory is self-contradictory.

A lot of people feel that paradoxes are just little games, but some are more than that. Take, for example, Bertrand Russell's paradox about the set of all sets that are not members of themselves. It's not just a trivial little party game; it's that too, but it leads to an important logical concept.

Banks: Well, I suppose there are paradoxes and there are paradoxes, but there must be some sense of taste as to which ones are important and which ones are frivolous.

Good: That's true, and of course there are also two definitions of paradoxes. Definition one is an essential contradiction. Definition two is something that just looks like a contradiction at first, but which can be resolved in an interesting way. My work on a paradox in information theory, joint with Sir K. Caj Doog, is of the second category; it is outlined, along with other relevant literature, in a Springer monograph by Dave Osteyee, with me as very much a junior author.

Russell's paradox is an example of the first category. The only way he could resolve it was by changing the definition of sets or classes. So, as language and mathematics were being used, it was a para-

dox of the first kind. Again, Gödel's earth-shattering work arose from a paradox and finished up as one—from dust to diamonds.

Banks: Given that there was an apparent conflict between Bartlett's classical statistics and Jeffreys' Bayesian statistics and that you were buzzing around Cambridge at the time that things were coming to loggerheads, how did you reach some resolve in your own mind?

Good: In Cambridge I didn't attend any lectures on classical statistics (although I solved one of the statistics problems in the mathematical tripos) and only a few by Jeffreys. He was an appalling lecturer in his regular course, so I soon gave up. Once I counted 72 "ers" in five minutes.

But I've always thought that "orthodox" statistics was important (I'm a Bayes/non-Bayes compromiser) and I tend to classify it, for the most part, as a collection of techniques rather than a philosophy, but of course it has its concepts. I think in all scientific subjects, perhaps also in the humanities, there's a technique and a philosophy, and this leads, incidentally, to a strain in many university departments, between theoreticians and practitioners, especially during the selection of a department head. I think it may be true in every department, even in English or foreign languages. In dictionary-making there are descriptivists and prescriptivists. Descriptivists are what I should call practitioners, studying the way vocabulary is used, while prescriptivists look for logical and philosophical reasons to slow down the rate of change of the meanings of standard vocabulary. There are also neologists, who like to invent logical new words such as *kudology*, *ifif* (instead of *iff*), *hopably*, *likelily*, *explicativity* and *antineologisticism*.

I like to link philosophical and practical ideas, in statistics and elsewhere, but I don't have time for everything, because, apart from statistics, as you know I'm interested in physics and mathematics, on the probability that God exists and how to define Him/Her/It and cabbages and kings. So I don't feel up-to-date in statistics. At my age and with my multifarious interests, I cannot be up-to-date, not that I ever have been.

Specifically, there are two things I'd like to work on. One is something you don't believe in. That's my physical numerology about the masses of subatomic particles. I want to learn more, in the hope of explaining the mathematical regularities, but I'm probably too old. But if the numerology turns out to be right, then whoever explains it will get a Nobel Prize, with probability 0.999. I would just be the modern Balmer, who was the guy who discovered the formula for hydrogen's spectral frequen-
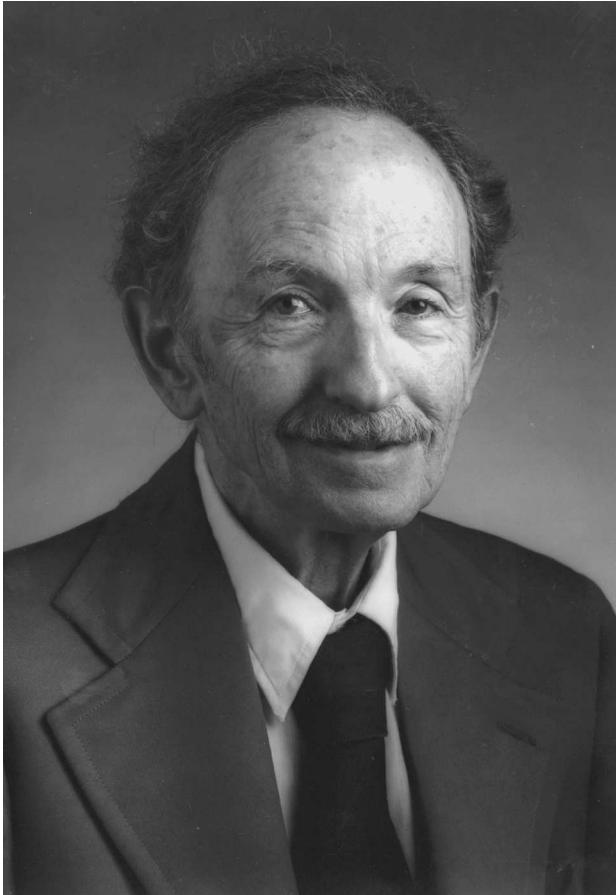
F<small>IG</small>. 4. *Jack Good, 1994.*

cies as a piece of numerology—that is, with no explanation.

After a simple formula has been found numerologically, it sometimes (though rarely) suggests the physical mechanism behind the phenomenon. When Balmer's expression, which was the difference between two simple expressions, was shown to Niels Bohr, he immediately thought of the idea of an electron jumping from one orbit to another, so the numerology was the spark that ignited a new approach to the study of the atom, the "old quantum theory." Most physics books get the history wrong and give the impression that Balmer's formula *confirmed* the theory rather than *suggesting* it to a prepared mind. I think people are prejudiced against numerology because of its main dictionary definitions. [In the latest unabridged Oxford English Dictionary, one use (*The Times*, February 23, 1962) is by physicists as a term of "near-abuse."] But for some decades (and I believe before 1962), the term has been used neutrally and semihumorously by physicists at their own expense.

One of my interests is how to distinguish between good and bad numerology. Many people, apart from the "tyranny of words," simply have a blanket rejection of anything that smacks of numerology because they have no idea how to judge what's good and what's bad—or at least they think they don't have an idea—but they do have some because when it is good enough they don't call it numerology. Some numerology (in physics, chemistry and genetics) has changed the world. So I prefer to think of numerology as a kind of exploratory data analysis.

Banks: I'll agree that the payoff is enormous if you find something as numinous as numerology. What is the other area to which you are devoting your time?

Good: Your alliteration is cute, but *physical* numerology isn't numinous. My other current main interest deals with necessitivity, sufficientivity and legal allocation of responsibility. I'm going to give a colloquium, entitled simply "Legal Responsibility," at the Center for the Study of Science in Society. People won't know what I'm going to talk about and they'll complain that I'm not even a lawyer (I've dedicated my life to creating reasonable truth, not reasonable doubt), but ignorance of the law is no excuse for not talking about it nor for not serving on a jury. Another, more topical, colloquium on legal matters has been presented with the title "Bayes Factors, Batterers, Murderers, and Barristers." It exemplifies Bayes factors in the law, a concept that should be taught to all potential jurors. Quantitative thinking teaches us the structure of qualitative thinking.

In my 1961 work on probabilistic causality, I overlooked the need for "necessitive" and "sufficientive" discussions. When I dipped into the interesting book (written for lawyers, rather than philosophers) *Causation in the Law*, by Hart and Honoré, I found many such discussions though they are not explicitly probabilistic. In the last few years I have found simple and I think convincing explications of quantitative measures of *necessitivity* and *sufficientivity* in terms of probability, either physical or subjective. These are measures of the tendency of an event to be necessary or to be sufficient for a later event. (Most things are a matter of degree; that's why I have a "graded" philosophy.) The best philosophical write-up so far is my contribution to the *Festschrift for Patrick Suppes*, edited by Paul Humphreys (1993). For possible bridges to statistics see my review in *Mathematical Reviews* (1995, June) of an article by Richard Stone.

Banks: We've worn out three tapes and need to close this conversation. Thanks very much for having done so many interesting things in so many important areas, and thanks also for your time, good humor and determination to look at everything from a uniquely fresh perspective.

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