In this occasional series we record the views and personal experience of people who have especially contributed to the evolution of ideas in the journal's field of interest. Charles Fletcher, the first person to inject penicillin into a patient, edited the first Royal College of Physician's report on smoking and health in 1962, and was instrumental with others, in setting up the campaigning body Action on Smoking and Health in 1971.

BJA: Could you start by briefly taking us through your medical education.

CF: I went to Cambridge in 1930. My father, by the way, was an eminent medical scientist, the first secretary of the Medical Research Council, and when I was 17 he asked me what I wanted to do. I said that I might want to be a doctor, so I took my first MB before I went to Trinity College, Cambridge. My father gave me two bits of advice. He said if you think you are going to do medicine, then while you are at Cambridge don't spend your time with medical students, because you will spend the rest of your life with doctors. Widen your education by talking to other students. The other excellent advice was to enjoy Cambridge—leave your evenings free during the terms and do the hard work during the vacations. I failed to get an entrance scholarship to Trinity College, but I managed to get a first class in my first year exams, and in my second year finals I managed to get a first again in Physiology, Zoology and Chemistry. I was then offered a senior scholarship at Trinity.

BJA: So you completed a first class honours degree in your first two years at Cambridge?

CF: That's right. In my third year I was spending a lot of time rowing because I was in the Cambridge crew. I caught up on my anatomy and nothing else so that at the end of that year I was in the position to attempt my second MB which I also passed. But I was then rather fascinated by the idea of doing physiological research, so I did the second part of my degree course in physiology and got a first in that in 1934.

BJA: You delayed your clinical training to do some more research?

CF: Yes, I settled down to do some research in physiology having got a research studentship at Trinity. I worked on the action potential of the unstriated muscle of the edible mussel, which had various properties which were rather interesting. I worked next door to Alan Hodgkin, who was in his fourth year, two years behind me. I put in a fellowship thesis on this work, but Alan Hodgkin did something nobody had ever done before and put in a fellowship thesis in his fourth year. Professor A. V. Hill was the referee for both our theses, and he told me many years later that mine would have been awarded a fellowship if it hadn't been that Alan's was so superb. So I was beaten to a fellowship by a future Nobel Prize winner. I was offered a fellowship at Peterhouse, to supervise their medical students. But by that time I had decided that I wanted to practise medicine, so I left Cambridge and went to Barts, where I was given a free education in memory of my father, who had qualified in medicine there.

BJA: You went to Barts when?

CF: My period at Cambridge was 1930–1936. Then I went to Barts and managed to get the final year clinical prize, the Brackenbury scholarship. Nobody had previously managed to get it in medicine and surgery. It was rather a curious thing but one had to choose which to take, and I took the medical one of course, so there's no record of this double.
When did you finally qualify in medicine?

In 1939 when I was 28, which was very late.

So you had a distinguished educational career in medicine and then you became a house physician at St Bartholomew’s Hospital. And then?

My first job coincided with the outbreak of the war. Barts was evacuated to mental hospitals in St Albans and New Southgate where I mostly worked. Acute admissions to Barts were transferred there and patients from the waiting list, so I had little training in acute medicine. I wanted more experience in academic medicine, so I wrote to Leslie Witts, formerly Professor of Medicine at Barts and then Nuffield Professor in Oxford, and he offered me a job as his house physician at the Radcliffe Infirmary. After that he took me on as a Nuffield Research Fellow (equivalent to a junior lecturer). While there I developed acute diabetes, and also passed the exam for membership of the Royal College of Physicians. My uncle, who was a senior fellow of the college, told me I got easily the best marks of any candidate in that exam. My success in exams was not because I was a great scholar, which I have never been, but was due to good technique. For example, I discovered in vivas that if I couldn’t answer a question, and just waited patiently and looked at the examiner, he would give you a useful hint.

So very early on you learned about communication.

I was also very good at guessing likely questions in written exams. My exam performance was always above the level of my professional performance.

You were the first person to give an injection of penicillin to a patient?

Yes. Professor Florey and his team, having shown that penicillin was very effective in mice, managed to produce enough for a clinical trial. Florey came over to Leslie Witts, and asked if he had someone who could do the clinical trials. I happened to walk into Witts’ room at that moment and he asked me if I would take this on. Florey first asked me to find a patient who had a fatal disease and was going to die soon, so that if for any reason penicillin was fatal, this would not matter. There were no ethical committees in those days. I found a woman who had disseminated breast cancer and she agreed to have this injection. I told her that it wouldn’t do her any good but that it could one day help a lot of other people. So with Florey and Witts standing by I gave an intravenous injection of penicillin to the patient. It gave her a brief fever but nothing more. I next explored if you could give it by rectum, by mouth, or whatever. I then treated seven patients by intravenous drip and showed that it could cure gross staphylococcal infections for which there was then no effective treatment. The first patient we tried it out on therapeutically was a 43 year old policeman who had combined staphylococcal and streptococcal septicaemia. He had terrible abscesses on his face and orbits, discharging sinuses, abscesses in his lungs. He was desperately ill and we knew he had everything to gain and nothing to lose from treatment. Five days after treatment began the improvement was incredible, but in the end the penicillin ran out and he deteriorated again and died a month later. I returned to Barts in November 1942, soon after my wedding, as a young registrar, but since most consultants were away at the war, my job was effectively that of a consultant, only three years after I had qualified.

Would you, in the normal course of events, have hoped to have got an appointment at Barts?

Do you mean a staff appointment? I had to make a decision in 1945. I had rather fed up with the routine clinical work at Barts and I wanted to get into more academic research oriented work. So I went to see a friend of my father’s, Professor Francis Fraser, who had moved from the chair at Barts to that at the Postgraduate Medical School at Hammersmith. During the war he had been seconded to the Department of Health to run the wartime emergency medical service. He suggested I should go to see John McMichael, who was acting professor at Hammersmith. I found McMichael pioneering cardiac catheterisation. He was short of a lecturer, who had just developed tuberculosis. He took me on and I worked under him for nearly a year. With his help I published my only cardiac paper. I used to drive to work with him because we lived close together just outside London. One day in early 1945 he told me he had a letter from Sir Edward Mellanby, secretary of the Medical Research Council, saying that they were looking for someone to run a pneumoconiosis research unit in south Wales. He didn’t suggest me, but Leslie Witts and Ronald Christie did. I then had to face a terrible problem. I wanted to go on in clinical medicine but most of my contemporaries had done military service, which I had avoided because of my diabetes. I didn’t want to go into pneumoconiosis because I was interested in gastroenterology, but I went to
Cardiff to do this job because I thought that this was a way that I could serve the country, so to speak.

BJA: You went from Barts to Hammersmith to do research?
CF: Yes, I did some rather elementary research there but it was cut short by this invitation to go and run the Pneumoconiosis Research Unit in Wales in 1945. But I did my MD thesis in gastroenterology, on diagnosis by gastroscope. With the help of some insurance money on the family silver, which had been stolen, I bought a gastroscope and I used to go regularly from North London to Avery Jones at the Central Middlesex Hospital to learn how to use it.

BJA: So in spite of your obvious interest in research you really wanted to be a clinician?
CF: I did want to be a clinician. But I had a sort of vain hope that I might be a professor of medicine. I wanted to be an academic physician. Because I strongly disliked the idea of private practice. I didn't only want to treat patients who could afford it. I wanted to do medicine unfettered, so I joined the Socialist Medical Association and vigorously supported the NHS.

BJA: At that time was that a radical thing for a doctor to do?
CF: Yes, I was in a minority at Barts. On one day I was called a communist by a medical student and a bloody Conservative by a member of the communist party. I was extremely keen on the Health Service and delighted when it came about. Medical students in those days were a tremendously conservative lot. They were mostly sons or daughters of doctors who were mostly Conservatives. Whilst at Barts some of us thought that it would be nice if we had some say in the way that we were educated. So we held a meeting and agreed that we ought to approach the staff to ask for some influence on the way they taught us. At the meeting one of the students got up and said that when he was at school he didn't criticise his masters and so why should he start now?

BJA: You've already explained that your father was the secretary of the Medical Research Council, that you went to Eton, Cambridge and then to London. You had a very traditional education. Where did you develop these rather radical ideas?
CF: My family were Liberals. The tragedy in my life was that my father died when I was only 21, during my third year at Cambridge, so I no longer had his influence. But I continued to feel it within me very much. In one way this made life rather difficult. He was a great scientist and a Blue, a Fellow of Trinity, and an FRS. He was very disappointed when I didn't get a scholarship to Trinity, Cambridge and when I eventually did get a scholarship I had a feeling that I had joined his class. I think I was keen on research but had no real originality. I think that because of my father's influence I thought I ought to do it.

BJA: So when you were invited to become director of the first pneumoconiosis research unit in South Wales was that a difficult decision?
CF: Very difficult. I hesitated a long time and eventually consulted my brother-in-law who was a parson. He made me see that this was a question of perhaps doing what I ought to do rather than what I wanted to do.

BJA: How would you summarize the contribution the research unit made, and your own contribution?
CF: Well, by the time I left the unit in 1952 we had shown how pneumoconiosis could be prevented. The National Coal Board acted on our proposals and coal miners pneumoconiosis has now disappeared from British mines. My own contribution was to devise a quantitative method of X-ray diagnosis. When we started, the classification of chest X-rays of miners was purely descriptive. There were two obviously two types of disease: a simple one where dust in the lung produced a speckling effect, and a complicated one in which you got huge fibrosing masses growing and causing severe breathlessness. This could be quantified by the size of the masses.

To quantify the simple form I got 100 films of the condition, ranging from normal to very marked, and asked 10 different doctors to classify them all into 5 grades. To my great surprise there were enormous differences between them. There were 30 films in this series which one doctor on one occasion said were quite normal, and another doctor on another occasion said were certifiable pneumoconiosis. I went up to London and asked Peter Kerly, who was then the top radiologist in the country, why there were these differences in diagnosis. He said it was because none of the doctors were radiologists. I got him and another radiologist at the Brompton Hospital to read all these films twice and the same thing happened: there were 30 films which one of them said were normal and the other said was
certifiable pneumoconiosis. He was absolutely shattered and thought this was impossible.

This was the beginning of my lasting interest in medical observer error. What we did was to develop standard films, based on many doctors' readings, which the reader would refer to and compare to each x-ray. We achieved reproducible readings and ultimately found, from the films of men who died, that the readings agreed closely with the amount of dust in their lungs. This method is now used internationally.

**BJA:** So you really developed a scientific approach to diagnosis.

**CF:** There had been few studies of that kind then, but after that it became a major interest, especially in the unit at Cardiff. Subsequently, observer error has been found to affect virtually every diagnostic test, and steps have been taken to reduce it. The other thing I did at Cardiff was recruit an extremely competent team. This was mostly good fortune. Many scientists had been doing operational research in the armed forces and were seeking the sort of work which our unit was doing. Sir Henry Dale, a great friend of my father's, was then President of the Royal Society. He knew what I was doing and he asked me if I would like to come to London to a symposium they were holding on war time research. I went around at the symposium telling the bright young people there about the pneumoconiosis problem and four of them said that they would like to join in. Philip Hugh Jones joined us, then John Gilson, who said that this was just the sort of research he wanted to do. Later, when I was looking for an epidemiologist, it happened that Archie Cochrane had just returned from the USA and Jerry Morris told me he was in London. He wanted to research into tuberculosis and since we then thought the complicated stage of pneumoconiosis was a form of tuberculosis, we persuaded him to join us.

**BJA:** Then in 1952, when you were 41, you were invited back to the Royal Postgraduate Medical School at Hammersmith in London by John McMichael. How did that come about?

**CF:** John Crofton, who was lecturer on chest diseases at the Hammersmith, left to become Professor of Tuberculosis in Edinburgh. John McMichael asked me to take his place. I had always had a hankering to get back to academic clinical medicine and I continued to have a low opinion of myself as a pure scientist, and wanted to help patients—so I went back to Hammersmith. It was an appalling shock at first because having been out of clinical medicine for seven years I found it hard to catch up with new clinical knowledge. I was precipitated into a severe depression and I was referred first to a Jungian psychiatrist. This didn't help me at all. He was always asking me about my dreams and one effect of the depression was that I had none. Eventually I told him that I had decided to commit suicide and he referred me to William Sargant's department at St Thomas's Hospital where I had two ECTs. The effects were dramatic. Till then I had been able to think of nothing other than how I had wrecked my life, but after the shocks I began to think positively.

**BJA:** Why did you think you had wrecked your life?

**CF:** Because I had been a world expert on pneumoconiosis but never trained as a chest physician. I was a gastroenterologist by inclination. I had never been to the Brompton Hospital and learned all the tricks of the trade. I came back to find that my registrars knew much more about chest medicine than I did.

**BJA:** Why then did you accept the invitation to go back to the Hammersmith?

**CF:** Because I thought that it would be a delightful chance to get back to clinical medicine.

**BJA:** What was the post at Hammersmith?

**CF:** Clinical lecturer in respiratory diseases.

**BJA:** So you went back to Hammersmith. What happened next?

**CF:** I also developed tuberculosis. I had an idea of investigating ciliary action in the bronchi by getting people to inhale fine barium dust and then watching by x-ray how it was cleared from the lungs. My colleague at the Pneumoconiosis Research Unit, Martin Wright, who developed the Wright Peak Flow metre, had invented a machine for getting animals to breathe in dust. So I breathed in some fine barium dust from his machine and had an x-ray taken. There was an obvious tuberculosis lesion visible on the x-ray. So I had 6 months off which was really valuable. I was asked to write a section on chest diseases for a medical text book which meant that during my six months off work for tuberculosis I read all about chest diseases. When I came back to my clinical work at least I knew something about them.

**BJA:** When was that?

**CF:** The second half of 1953.
BJA: You recovered from depression. Was that when you became interested in bronchitis?

CF: Before the war chest disease had been almost synonymous with tuberculosis. When this came under anti-biotic control chest physicians started looking around, and it was obvious that there was an enormous amount of bronchitis and emphysema around.

BJA: Could you summarize how your work at Hammersmith and your interest in bronchitis and emphysema developed?

CF: As the biggest problem in chest diseases it was the obvious thing to work on. I started a bronchitis clinic and was handicapped by not being expert competent at pulmonary physiology. I knew from my experience at Cardiff that you could only unravel the early stages of a disease by surveys outside the hospital. Encouraged by Professor Donald Reid at the London School of Hygiene, who had done epidemiological studies among post-men, I designed a study of diagnostic methods among post office workers. It involved using a questionnaire on respiratory symptoms which we had developed at Cardiff. My first task was to see how observer error could be reduced. We had three doctors and three nurses doing duplicate interviews on each subject, all recorded on tape. We found that if interviewers stuck to the questions strictly you got reproducible results, but if they added words of their own there were serious disagreements. For example, one of the nurses would ask about phlegm by starting ‘it is not a very nice subject but I produce some so don’t be shy’. She got about twice the incidence of phlegm that other interviewers got. From this work came the MRC standard questionnaire on respiratory symptoms, used all over the world.

Another important event was the great London smog of December 1952, which arrived in London just after I had come back from Cardiff. Our beds were flooded with bronchitic men dying from suffocation. The government set up various anti-smog committees which led to smoke control legislation. The MRC set up a Bronchitis Research Committee with Professor Ronald Christie as chairman. I was asked to be secretary of that committee. So I suppose it was the impact of the fog and the air pollution problems that led me to work on bronchitis.

BJA: What did this committee do?

CF: Well it sponsored my research in the Post Office. After that research we had formed a hypothesis on the natural history of bronchitis—that the irritants of tobacco and smog caused hyper-secretion of mucous, causing expectoration. This increased liability to infection which in turn caused emphysema. To test the idea I asked the MRC to finance a prospective trial over five years of men working on London Transport buses and trains, and at the Post Office savings bank—a large population with a small turnover.

BJA: Your interest in a scientific approach to diagnosis had a very practical effect in the bronchitis field didn’t it?

CF: That’s right. At that time there was chaos in the terminology of bronchitis and related disorders. The words bronchitis, asthma and emphysema were used without any agreed definition of their meaning. On a lecture tour in the USA in 1957 I found that these words were used in different ways in different centres and usually differently from their use in the UK. When I got home I suggested to Kenneth Donald and John Gilson that we should arrange an international conference to seek agreement. They suggested that we ought to get agreement within Britain first, so I asked the CIBA foundation to arrange a guest symposium. For three days a meeting of clinicians, physiologists, pathologists, and epidemiologists, which I chaired, defined these terms. The 400 copies of the report were used within a few months and it led to a general international acceptance of the meaning of these words which greatly reduced confusion.

BJA: If I could go back historically. During the time you were in Cardiff were you aware of the work Doll was doing on smoking and lung cancer in the 1940s?

CF: I was only aware of it when their first paper was published. I knew Richard Doll well and asked him to come as epidemiologist to Cardiff. Although he was keen for a variety of reasons he didn’t come. When their paper was published of course I asked myself why aren’t we looking at the effects of smoking?

BJA: Doll and Hill’s paper was published in 1950, when you were still in Cardiff. Did that change your work?

CF: We started asking about smoking habits and found that smoking and bronchitic symptoms were closely related.

BJA: Am I right in thinking that at the beginning of the 1950s when you went back to Hammersmith, Doll
did not think there was a connection between smoking and bronchitis?

CF: I went back in 1952. Their paper showed a close relationship between smoking and lung cancer, and also with bronchitis as a subsidiary finding. The history of this is interesting because when they decided to study lung cancer they thought of air pollution, traffic and various things, but only as an afterthought did somebody say 'what about smoking?' To their astonishment they found that it was closely related.

BJA: Can you summarize the development of your interest in the relation between smoking and bronchitis.

CF: It became apparent, and my education by Archie Cochrane and my experience in Cardiff showed, that you could not study the natural history of any disease if you started at the end of it, you must start earlier on. The Post Office study showed a very close relationship between bronchitic symptoms and smoking.

BJA: This was the prospective one?

CF: No the previous cross-sectional one at the Post Office. This was published in 1959. It was from that study that the hypothesis was developed that smoking caused hyper-secretion which made the lung susceptible to infection, which could cause emphysema. There was also a lot of evidence from pathologists suggesting that infection might be causing emphysema. I realized that one couldn't really test this hypothesis by cross-sectional research. A prospective study was essential to see how the disease developed.

BJA: When did you become convinced that smoking caused chronic bronchitis and emphysema?

CF: When we did our cross-sectional survey. The association was very close.

BJA: When did you start the prospective study?

CF: In 1961 intending it to run for five years, but we saw that we needed a bit longer and carried on until 1968. That was by far the most important study I ever did. But it was muddled a bit at first by the fact that Richard Doll had asked Bradford Hill's son, David Hill, to do the statistics. He was interested in mathematics but uninterested in the problem. This became frustrating so I told Richard that this arrangement wasn't working very well. He agreed and suggested that we might try a young mathematical scholar who had just joined his unit from Cambridge—Richard Peto. He transformed our study which disproved the hypothesis we had started with. It showed that infection played no part causing emphysema which was a direct effect of smoking. We completed the study in 1968. Richard Peto had a lot of other things on his plate, including the 20 year study of doctors's smoking, so our study wasn't published until 1976, as a monograph with a summary in the British Medical Journal the same year.

BJA: So what you had established in that work was the true or major cause of bronchitis?

CF: Yes. It was the direct effect of smoking. This also caused hyper-secretion and infection but there was no causal relationship between the infection and the obstruction. At the beginning of that time I was appointed a reader in epidemiology but I didn't get the chair until later on. I think that was because John McMichael didn't really think epidemiology was a scientific discipline.

BJA: You became a reader when you were 51 and got a chair when you were 62. That is late isn't it?

CF: It was very late. I know that some of my friends had been writing to John McMichael suggesting that I ought to have a chair. But when I put to him the idea of doing a prospective epidemiological study, he asked why I couldn't do it clinically in the ward. He instanced a man with emphysema on his ward who three years previously had developed pneumonia—why didn't I study this sort of case in the wards? I asked him if there was any measurement of the man's lung function (FEV) before his pneumonia. He thought for a moment and said "alright do your study". The absolute blindness of clinicians to the fact that in the wards they see only the late stages of diseases—so that you had to study early onset in the general 'healthy' public—always surprised me.

BJA: Would it be fair to say that this was one of your major contributions—to develop epidemiology?

CF: I think so. Of course at that time English medicine was remarkable at developing a plethora of people interested in epidemiology: Jerry Morris, Donald Reid, Richard Doll.

BJA: You're talking about the 1940s/50s?

CF: Yes. We led the world, but epidemiology was really not regarded as part of medicine.

BJA: So what you're saying is that until then, it was
thought that you could learn about and understand the development of disease just from clinical observation?
CF: That's right.

BJA: You and some of the colleagues you have just mentioned, began to understand that this was not so.
CF: Yes, that you had to look at the early stages of disease. Richard Doll and Austin Bradford Hill's first study was done in hospital patients, but their subsequent one was an epidemiological study of doctors. That was a revolution in which I participated. I wouldn't say I was responsible for it, but I became aware of it and orientated my research in that direction. When I gave a paper on our prospective study at a meeting of clinicians and physiologists just outside Washington in 1963 it caused a sensation. People had no idea that emphysema developed in this way. Eminent chest physicians and chest physiologists thought that emphysema started acutely after an infection.

BJA: Between 1961 and 1972 you were medical advisor to BBC television. How did you get into that?
CF: Shortly after I came back to London a BBC producer, Andrew Miller-Jones, had been given the job of producing a series of five programmes on medical subjects, called 'Medical Matters'. That was in 1952. He was assisted in designing these programmes by Dr John Agate, who had done industrial medicine at the London Hospital and eventually became one of the first leading geriatricians. He had helped Miller-Jones with the medical aspects of these programmes. They wanted an anchor man and John Agate suggested me. We met the producer, who seemed to think I was alright and so that was the first series which I presented on television.

BJA: How long did that series last?
CF: It was only five programmes at fortnightly intervals I think. It was broadcast early in 1953.

BJA: Were you one of the first television doctors?
CF: The BBC had broadcast one or two isolated programmes before, but I was the first person to run a series. They did one on pain and one on dust. Andrew Miller-Jones was impressed with the way I coped. The shows were all done live then. When we were doing the one on dust, I left out something, they prompted me, and I was able to take the prompt without breaking my flow at all.

BJA: Why were you so good at it?
CF: I don't really know. I've always enjoyed acting and even flirted with the idea of going on the stage. I did a lot of amateur dramatics at Cambridge and at Barts.

BJA: Is that something else that runs in your family?
CF: It does on my mother's side of the family.

BJA: Because you are a musician as well aren't you?
CF: Yes. My mother was a very good musician too. I am very fond of music, had a good baritone voice and sang in a Bach Choir.

BJA: Did you play an instrument?
CF: Clarinet, but only for a short time, because I found it very hard to find the time to practice. I think another reason I find it easy to talk to a camera is that I have a habit, when puzzling over a question, of talking to myself about it. I think that when I was talking to the camera I was really talking to myself. There was a gap after the Miller-Jones series and then a series called 'The Hurt Mind', for which Christopher Mayhew stayed two nights in a mental hospital and described his experience. This was a series of five programmes whose purpose was to get the public to realize that mental disease was a disease like any other and not a 'madness'. They did a study of public opinion before and after the series and found a considerable shift in attitudes. This was about 1955.

BJA: You presented this?
CF: I shared it with Mayhew. He presented the first two and I presented the last three. Then in 1958, Mary Adams, who was in charge of scientific programmes at the BBC, asked me if I would introduce a series on medicine and surgery in hospitals called 'Your life in their hands'. The BBC had done a programme at St Mary's Hospital which had received a tremendous public response, and one of the regional offices of the BBC asked why they concentrated on London. So they allocated two programmes to each of the five regions of the BBC, ten programmes altogether. They were done live and I introduced them. These were such a success, attracting large audiences, that they did three more series of programmes over the next few years.

BJA: How did the medical profession react to these programmes?
CF: Many of them were bitterly opposed.
BJA: Why?
CF: I think it was a feeling, dating from Victorian days, in middle classes that the inside of the body should be treated with disgust.

BJA: They just didn't think that the body should be talked about?
CF: In the polite classes it wasn't. My mother told me that in the early part of the century, a guest at her home mentioned the word appendicitis at lunch. There was a ghastly hush and the subject was quickly changed by my grandmother.

BJA: Surely that wasn't the only reason?
CF: Another was that when medicine was ineffective the only weapon doctors had was magic. Here was somebody breaching the secret code which doctors thought was their own. The BMJ published five leading articles in successive weeks called 'Disease education by the BBC, or something like that, attacking the television series.' There was even a debate in the House of Commons on the impropriety of talking about this sort of thing in public.

BJA: Did this reaction both you?
CF: It bothered me a lot. My chief, John McMichael, asked me to see him after the second programme which showed a heart operation. He told me I must withdraw from the series and assured me that if I did not I would not be promoted in the distinction awards system, and that I would lose the confidence of my colleagues. That was fairly tough. But I felt I couldn't leave it in mid-stream. I thought its benefits were going to be greater than any harm it might do.

BJA: Did it damage your career?
CF: Well it didn't because that year I was elected to the Council of the Royal College of Physicians and I was promoted from a C to a B merit award. So the warnings he gave me were disproved within a year.

BJA: Things have changed haven't they?
CF: Well, I have never really thought I was a revolutionary but shortly before he died Theodore Fox, who was the editor of the Lancet, said to me that it must be wonderful to have brought about a revolution in medicine. It happened so gradually I never thought of it as a revolution.

BJA: The radical idea was doctors talking openly and publicly about medicine, destroying the magic?
CF: Yes, although they didn't say that of course. They said it was improper, it would cause hypochondria and frighten people.

BJA: So the prevailing attitude seemed to be that medicine and health care were the province of the professionals, not of the people and that people needed protection?
CF: Yes, but preventive medicine was alright—it was alright to tell people how to keep well, but not to tell them how to get well once they'd become ill.

BJA: It's very relevant to what we are going to discuss about smoking isn't it, because once the causes of smoking-related diseases had been discovered, there was not all that much to do except prevent them.
CF: Yes. You see I had been nurtured in preventive medicine in Cardiff. Pneumoconiosis was untreatable so our role was to discover the cause and see how it could be avoided. The critical thing was that dust could be suppressed in the mines but to what level? We showed that by a combination of mass x-rays of the miners and dust measurement you could determine a safe level.

BJA: So you went on from your successful television work to write on the importance of communication.
CF: Well that was much later. The Nuffield Provincial Hospital's Trust awarded me a Rock Carling Fellowship in 1972. This meant that you had to study a subject for a year, write a monograph and deliver a lecture summarizing the monograph. I was given the subject 'communication in medicine.' In preparing that, I asked a recently retired medical librarian to look out some references for me. Practically all these references were about the way that patients didn't understand their doctors, work of which I had not been aware. So I wrote the first chapter on communicating with patients and left the section on communicating with the public to the second half of the book. I was genuinely unaware that my patients had any difficulty in understanding
what I said to them. I was probably better than most doctors, but even so I didn’t take deliberate steps to check that they had understood and accepted what I had said. The idea of patient non-compliance had only recently been recognised by doctors, when they found that tuberculosis patients weren’t taking antibiotics as instructed. This was thought to be wilful disobedience by the patients and no blame in those early papers was placed on the doctor.

BJA: Can we go back for a moment? What was the reaction, professional or public, to the 1950 Doll and Hill paper?

CF: Disbelief. It can’t really be true.

BJA: Did you have close connections with the MRC in the 1960s?

CF: Yes, they were supporting the research I was doing.

BJA: Did they accept the Doll and Hill conclusion?

CF: First of all the Ministry of Health published a report saying that they accepted the findings. But it was hardly referred to in the press. The MRC said they agreed that this study had shown that smoking was the main cause of lung cancer, and they published an official report in 1954, also with little response.

BJA: I seem to recall from Sir Richard Doll that the Ministry kept asking the MRC to do more research to confirm the findings, and the MRC refused because they said that they were satisfied with the conclusion and no more research was needed. What made you start thinking that you should do something like getting the Royal College of Physicians to publish a report?

CF: My research was concerned with bronchitis. It became very obvious that smoking was the cause of bronchitis and also, of course, I was seeing patients with lung cancer in increasing numbers. At the same time we were concerned about air pollution after the 1952 London smog. Patrick Lawther was appointed director of an air pollution research unit in Barts. We were collaborating with him on studies on masks to protect patients with bronchitis and also to protect them in the ward. A doctor from the Ministry of Health called Max Wilson came to see what we were doing. As he was leaving I asked him ‘when is your Ministry going to justify its title and do something about smoking?’ He reported this to George Godber, Deputy Chief Medical Officer, who invited me to lunch. He said that he wanted to talk about what we could do about smoking. He made it clear to me that his chief, Sir John Charles (the Chief Medical Officer), was dead against the Ministry being involved in any action on smoking because he thought it would cause trouble. I asked him if he thought it would help if we asked the Royal College of Physicians to produce a report on smoking, sort of side-stepping John Charles.

BJA: At this stage what was your position in the College?

CF: I was just a fairly junior fellow. Robert Platt had just become president, and he said that if any fellow at the college had a problem which they thought he should know about he would be glad to hear from them. Based on that encouragement I rang him, although I didn’t know him particularly well, and asked to see him. “What for?” he asked. I told him that I thought the College should produce a report on smoking. He immediately said “Of course we should. Who should we have on the committee?” Later Avery Jones told me that he had made the same suggestion to his predecessor, Russell Brain, who had replied in a letter that smoking was no concern of the College.

BJA: Perhaps you could explain some of the background here. The Royal College of Physicians did not have a tradition as a campaigning body did it. Why was Robert Platt interested in taking on such a controversial subject?

CF: I think, possibly, because he had been a chain smoker and had managed to stop shortly before he became president. He realized that this was important and he just had a wider vision for the college. He didn’t only do this. He started post-graduate lecturing in the college—previously there had just been about five formal lectures a year and otherwise the college was only an examining body. The last time the college had acted on a public health problem was in the 18th century when it petitioned the government to take action against cheap gin. Robert Platt was a remarkable man to change the college as he did.

BJA: So he formed a committee?

CF: I suggested that he should invite Avery Jones, Guy Scadding, and Bodley Scott, who had written an article on the effects of smoking in the BMJ some years previously (in which he hadn’t mentioned lung cancer). I had suggested him because he was an eminent physician interested in smoking. They suggested other members and the college approved,
but asking the committee to include air pollution in its remit. When we met we decided that air pollution was so obviously unimportant compared with smoking we took no notice of this.

BJA: Why did the College take that position?
CF: I think because about half of them were smokers and hoped that air pollution was more important than smoking.

BJA: Do you mean at that stage they still didn’t believe Doll and Hill?
CF: Yes, even though Doll and Hill had found no effect of air pollution.

BJA: You were largely responsible for the drafting of that first report weren’t you? How was the work of writing the report handled?
CF: I think that I have got a slight talent at writing simple English. I really had in mind the average Member of Parliament as the audience for this report. Each member of the committee wrote a chapter which I then re-drafted into a consistent style.

BJA: Intending the report to be for Members of Parliament was a very unusual way for a doctor to think?
CF: Well, Professor Crew, who had been Professor of Public Health in Edinburgh, described the House of Commons as the ‘pharmacy of preventive medicine’. That phrase had stuck in my mind.

BJA: So from the beginning you realized that the voice of doctors, expressed through the report, must influence the politicians?
CF: That’s right. We were really quite sure about that. They were the ones who had to act.

BJA: So you had a press conference to advertise the report and there was tremendous coverage of it.
CF: This was because of two fortunate things. Firstly, there was no other important headline in the newspapers that day. Secondly, we were a group of practising doctors saying smoking was dangerous. I think that was the thing that the media recognized as being new, compared with official statements by the Medical Research Council and the Ministry of Health.

BJA: Do you remember how many copies Pitmans published?
CF: I remember we bargained with them. They said they thought that it wouldn’t be worthwhile printing more than 5000 and we asked for 10000. They agreed but said that we would have to pay for any losses. The 10000 went in the first two days and they did a second printing within a week. The American Cancer Society also ordered 5000 copies to distribute to their members.

BJA: What was the response of the politicians?
CF: Neglect. Enoch Powell was Minister of Health and he agreed that the Ministry of Information should produce some posters, but he said that any action on this must be taken by local authorities through their health education money.

BJA: He accepted the conclusions of the report?
CF: Yes.

BJA: What was the response of the tobacco industry?
CF: That brings in Geoffrey Todd, who had been dismissed from his post as public relations officer with the tobacco manufacturers when he told them he accepted that smoking caused lung cancer. But they missed his administrative skills so much that they re-appointed him. He wrote to me before the report was published and said that he would like 20 copies to distribute to the main manufacturers because he wanted to insist that they took the issue seriously. Later the manufacturers set up a research division directed by Todd, which financed some of my work at Hammersmith. John Partridge, chairman of Imperial Tobacco, accepted the evidence but was sure their scientists would be able to find a way of removing the cancer producing substance from the smoke. There’s an interesting side issue here. His son was appointed a chaplain at St Martins in the Fields, which alerted me to the fact that he was a keen Anglican. So I wrote and asked how he reconciled his Christian beliefs with killing people through smoking? He asked me to meet him and some of his staff. At this meeting the head of the Wills Division in Bristol asked me ‘Do you think you are justified in trying to destroy our industry?’ John Partridge said he was sure they could overcome this temporary problem.

BJA: So the tobacco industry’s initial response was that they accepted the evidence but hoped they could change the product—take out the toxins?
CF: That’s right.

BJA: What was behind the American Cancer Society’s great interest in the report?
CF: They were quite different from any cancer organization in this country, in that they were involved in health education and persuading people that cancer could be prevented or cured. Here was a report indicating the cause of cancer and so they circulated it to their members. President Kennedy got to know about it and told Luther Terry, his Surgeon General, that he had to produce a better report and so they produced the first in the marvellous series of reports which have gone on ever since.

BJA: In Britain you were disappointed with the lack of response to the report. What happened then?  
CF: Well about 1970 it seemed obvious it hadn't had the effect we hoped so we thought another report might rectify this. Sir Max Rosenheim was then president of the Royal College of Physicians. He agreed so we set up a new committee and produced the second (1971) report.

BJA: Why did you think that if the first report had not produced real change that another report might?  
CF: Basically because if you fail once, you try again.

BJA: Had there been any change in public or political opinion during the 60s after the first report?  
CF: Temporarily but it wore off pretty quickly, as was shown by the recovery of cigarette sales. We were rather more forceful in our introduction to the second report; I put in more colourful words. I said that the word holocaust could describe it and that it was like the worst of the epidemics of the past. Keith Joseph said that that was the sort of lobby he couldn't refuse.

BJA: So the Royal College of Physicians published a second report in 1971, which you also drafted?  
CF: Yes. At the same time we approached the college about setting up an anti-smoking organization.

BJA: What exactly did you propose?  
CF: That we should have a council which should incorporate all the main medical societies and should act as a channel for communication about the effects of smoking.

BJA: What was Max Rosenheim's reaction?  
CF: He agreed and called a committee to see how it should be set up. The name we first proposed was the Council for Action on Smoking and Health, until we realised that it would be abbreviated to CASH. So we took off the C and called it Action on Smoking and Health. The American ASH was fairly different from ours and was set up by a lawyer.

BJA: When you first suggested that the Royal College of Physicians should set up an advocacy body were you surprised that they agreed?  
CF: No. Max Rosenheim had been chairman of the committee for the second report and was keen for action to follow.

BJA: Was this an implicit acknowledgement that the information which you had successfully communicated through the report wasn't enough?  
CF: That's right. Dr John Dunwoody was the first (part-time) director of ASH, but he soon wanted to go back into general practice.

BJA: So you advertised for a new director of ASH. Did you get many applications?  
CF: Just three—an elderly man, an elderly woman, and Mike Daube. He was so eager and confident we were clear that he was our man. He turned us from rather an inactive information facility into a real campaigning body.

BJA: In its first year or so ASH's function was to disseminate information and to try and change public opinion?  
CF: Well not so much the latter. We wanted to, but at first it simply acted as an information centre. We were given £125,000 to help get started by Keith Joseph, the Health Minister, who said that we weren't going to get any more. Mike Daube changed ASH fundamentally. For example, he took all the tobacco trade papers, got to know of new cigarette brands that were coming along and succeeded in stopping some. He insisted that we must, if we were going to have any real effect, be a campaigning organization. He set up an inter-party group on smoking and health in the House of Commons, and he got us into the position where the Department of Health found us helpful and has funded us ever since.

BJA: So the council of ASH was happy with the direction Mike Daube wanted to take?  
CF: Yes. He made the tobacco companies really angry with us. They said that we couldn't be a charity and campaign politically. Lord Goodman's committee on charities found that many other charities were doing political work, such as Shelter, and felt that this was perfectly legitimate. Our
original terms of reference were to encourage research and the dissemination of information. After that we were able to change to take an operative role.

BJA: *Can you remember whose idea it was to create ASH?*

CF: I think it was myself and Keith Ball. He said that he and Avery Jones had already been to see Rosenheim before I did. I hadn’t known that. I was impressed by the much greater activity in the USA by the American Cancer Society and other medical societies.

BJA: *What are your hopes for the development of tobacco control advocacy during the next decade?*

CF: I think that the most effective weapon is price, but that this is difficult for politicians because of its effect on the retail price index. I think banning advertising is also very important, and that cigarettes should be sold in plain brown packets with large health warnings covering most of the surface.

BJA: *Is there a danger at all that if you have a campaigning body like ASH, the professionals might feel that they don’t have to go on speaking out?*

CF: I don’t think so. The BMA are actively committed now and we get tremendous backing from them. Nicotine is a very addictive substance and we won’t ever achieve complete non-smoking. Even a small number of doctors still smoke.

BJA: *You mentioned getting diabetes when you were at Oxford. Has this been a handicap during your career?*

CF: It hasn’t because I’ve managed it without getting any of the serious complications from which many diabetics suffer. It is a nuisance, mainly because of the inevitability of recurrent hypoglycemia caused by my careful avoidance of hyperglycemia. Fortunately my wife and many colleagues, especially secretaries, housemen and registrars, have tactfully reminded me to take glucose when they notice me acting strangely, to avoid awkward consequences.

BJA: *Several decades after you first started to emphasise the importance of communication for doctors, we are in a position for doctors to speak out and play a very active role. Would you agree with that?*

CF: Yes. But I think it’s absolutely crucial that they continue to do so. They really do have a powerful voice, and it’s a voice that politicians cannot lightly ignore.

**References**

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