ESN INTERVIEWS:

The Editors talk to that most active European

Professor Sir Harold Thompson

Master of St. John's College, Oxford

ESN: Your interest in Europe we believe, goes back to your immediate postgraduate years. You have a Doctorate in Philosophy from Berlin?

HT: Yes, I have been a European since I was exactly 21 years old when I went to Berlin to do some research after I had finished at Oxford. I worked at Fritz Haber's laboratory in the Kaiser Wilhelm Institute in Dahlem and, by the grace of God and good luck, a few weeks after going to Berlin not knowing a soul I was in the house of Max Planck as a paying guest, and lived there for a year.

The degree was accidental in a sense. I was working with Hinshelwood on gas chemical kinetics for my Part 2 at Oxford and I wanted to go and work with Fritz Paneth who had just heated zinc dimethyl and produced methyl radicals. It was the time when any radicals suddenly became fashionable in chemical kinetics, and I wrote to Paneth, who was in Berlin, and asked if I could come and work for him. St. John's College, Oxford had asked me if I would be a Fellow of St. John's, and I had the audacity to say to them, "No, I don't want to be a Fellow of the College unless you let me go away first for a year at least because I don't want to stick my life here in Oxford and go nowhere else". Well they gave me a year's Research Fellowship and off I went to Berlin; and I didn't know a soul. I had some terrible lodgings in Dahlem and so I went to the house of a chap who was working in the Kaiser Wilhelm Institut für Silikatforschung.

I was in Physikalische Chemie, I had written to Paneth who answered that I could go with him if I liked but he had just been given a Professorship in Koenigsberg (which is now Kaliningrad) and it would be much better for me to settle for somebody in Berlin. So I wrote to Polanyi in Dahlem who was doing highly diluted flame reactions and Polanyi said that he had taken on six chaps and could not take any more but he said that Fritz Haber would have me in his laboratory downstairs. This was in 1929 and Haber unfortunately was in his declining years. However, it was a great experience, a truly wonderful experience. When I had been there for about a month he said, "Why don't you do a doctorate while you're here?" I said that there wasn't time. I was only there for a year or a bit more. Well, he said that he could let me off a term or two on my back record and so on. I did it, I was examined for my Doctorate by Haber, Nernst, Bodenstein and in philosophy by Wolfgang Köhler, the behaviourist philosopher.

I shall never forget one colloquium on the Reichstagsufer in Berlin in the Nernst Laboratory. There was a discussion on the Nernst heat theorem, the Third Law of Thermodynamics: was it valid or not? It was a philosophical argument between Otto Stern and Nernst. At the end of a long colloquium, I stood up in the back row, the little new boy scared stiff — could hardly speak German — and I made some comment. Well, as we walked out down the steps, a little man came up to me and patted me on the shoulder and said "Herr Kollege ... Das hat mich sehr interessiert was Sie gesagt haben über meinen Wärmesatz". This was Nernst. I was made into a European that year and it was terrific.

ESN: You said that you lived in the house of Max Planck.

HT: Well, as I said, originally I had rotten lodgings and decided to look for some more. I found some in Dahlem and the landlady was Frau Danz. I booked to go on 1 November and the very next day I was asked to a cocktail or sherry party or whatever they were called in those days. Schrödinger's wife — Anne-Marie Schrödinger — whom I had just come to know during the time I had been there, asked where I was living. I said that I had to get some new lodgings as mine were so awful. She said, "Just come over here", and she introduced me to Frau Planck. Then she said, "Where are you living at the moment?" and asked why didn't I go and live with them in their home as their children had all grown up and left. So I went to live there at number 24 Wangenheim Strasse in the Grunewald, and the education I got there in that house was far greater than any I could get in any laboratory in 12 months. I was able to meet people like Richard Strauss, and his wife, the widow of Hugo von Hoffmansthal, Marlene Dietrich, Dietrich Bonhöffer and so many others.

One Saturday night Einstein had come, as he often did on Saturday nights, to play his violin, and Schrödinger and his wife came in and asked me to go to a fancy dress ball with them. I said that I couldn't as there were 20 people in the evening salon and I couldn't just walk out like that. She said, "Just go up to Frau Planck and say that you are coming with us to a masked ball". So I went up to Frau Planck and, in my best German, I wanted to say that I had been taken away. I thought "to lead away" is "führen" (to lead) plus "ver" (in the opposite direction) making "verführen", so I said, "Gnädige Frau, ich bin verführt worden". In fact of course it meant "I have been seduced". Frau Planck asked "by whom" and I innocently said "Frau Schrödinger!"

ESN: What was Berlin like in the late 1920s?

HT: 1929 and 1930 was the "Höhepunkt".

One day I went in for a *viva-voce* with Haber. He said, "Bitte setzen Sie sich; nehmen Sie eine Zigarre" So I took a cigar; I was a bit nervous. He told me, "Give me a few minutes I can't do anything for a few minutes: I have just had a chap here and I couldn't do a thing with him, so eventually after half an hour I asked him —

"Wie macht man Jod" (how do you make iodine)?
He said, "Ach, Herr Geheimrat, it comes from the iodine tree!"
"So! — and what colour are the leaves of the iodine tree."
"Green — green, Herr Professor."
"And what colour are the flowers?"
"Violet"
"Where does it grow?"
"I think in Central Africa."
"So, hmm! — tell me when is iodine blossom time?"
"I believe, Herr Geheimrat, it blooms in November and in March."
"Well then, come and see me again when the iodine tree is blooming!"

They were great days.

Naturally, I became a romanticist and a European.

ESN: Where did practical infrared spectroscopy get started?

HT: The beginning of it all I still declare was H. S. Taylor at Princeton. I have told this story before. The atomic weight of hydrogen found by mass spectrometry was not quite equal to that found by the old method (burning hydrogen and oxygen together). There was a slight difference (it was too big to be error) and the only explanation which could be given was that an isotope was present in the hydrogen. That is how heavy hydrogen first came to be isolated. When they had isolated deuterium in 1932 or thereabouts, H. S. Taylor said that he was going to do some reactions between deuterium and methane. He started some kinetics of reactions between deuterium and other hydrocarbons and at the end of it he got some ungodly mixture, which could not be analysed until he used vibrational spectroscopy in 1937 to do it, although that passed unnoticed.

ESN: Presumably by this time your own group in Oxford was well under way.

HT: Don't forget the fact that when I started in infrared work I was supported by grants from just two sources. One was the Royal Society, who bought me a spectrometer. This was in about 1933 or 1934. The only other interest or help I had at all was from Imperial Chemical Industries, who bought me what was at that time the zenith of galvanometers, a Zernica moving coil galvanometer, which was said to be the best thing ever. Well in fact in the early days we still used Broca moving magnet galvanometers, a Paschen type galvanometer. The point I want to make is that the only people who showed any interest in me were the Royal Society and ICI. I don't know why ICI, but probably because they knew me anyway. I knew the people at Billingham well and I knew the Alkali Division and I should like to say that for those days ICI was a very far-seeing organization. It was in the time when the physical chemistry end of it all at Billingham was being built up in the 1930s, and they were a far-seeing group.

ESN: You mean that in those years there was little interest in your work?

HT: What I want to tell you is that until the war came along no-one thought the work some others and I were doing at Oxford and Cambridge was worth a damn.

ESN: So, things changed when the war came?

HT: As far as the United Kingdom was concerned, yes. The Ministry of Aircraft Production sent for Sutherland, myself and the then Chief Government Chemist, J. J. Fox. His work, incidentally, was his hobby also. He built a grating spectrometer which was in a glass case, dried out, and he did that rather fine work in the early days, with Martin, on hydrogen bonding in alcohols and acids. We went and explained things to the Ministry and they said: "how much money do you want, you can have what you like"! Anyhow, the problem we were given was to find out whether the Germans were making iso-octane, synthetic octane, and there was no good way of doing it. The work was split up, and Sutherland at Cambridge and our group measured the spectra of I don't know how many hydrocarbons, many of which of course were not pure, but I should think 100 to 120 hydrocarbons in the boiling range between room temperature and 200 degrees. We mapped that out in the course of six months or thereabouts, and we worked in conjunction with Imperial Chemical Industries (ICI) at Billingham, who did the fractionation. The long and short of it was that, by using ultraviolet work too for the aromatics, and the infrared for the aliphatics, we really learnt a great deal. The Germans were not making synthetic octane, they were using a lot of Fischer Tropschs and they were hydrogenating creosote or something, and this really all came out comparatively easily. After about a year or a year and a half working on petrol from crashed German bombers, and retrieved from their tanks, we changed over to the control of our own fuel production. At that time we linked up with what was going on in the U.S.A. We went to the States in 1943 and went round the oil refineries. At that time Van Zandt Williams had been working at American Cyanamid.

Then, of course, spectroscopy spread to other things. Problems included finding in polyethylene traces of carbonyl groups which determined the dielectric loss factor, and synthetic copolymers like GRS, which is butadiene/styrene, and whether it had gone 1,2 or 1,4 addition. Then of course the uses of spectroscopy widened still further into general organic chemicals, including the structure of penicillin and a host of other things. I remember the famous day when we received a telephone call to say that the nylon manufacture plant in Huddersfield had broken down and they did not know why. They were making nylon for parachutes. That was a wonderful sleuth case. They sent us two samples of adiponitrile, one pure and the one used. They said something had gone wrong, and in the sample there were three bands (little pips): one was in the OH region and another was a C=C or something. By a little bit of thinking it turned out that someone had slopped some water in so it had cyclized and then hydrolysed and formed a ring ketone which had then enolized a quarter of a per cent or a tenth of a per cent or something. They got rid of the water and the process went forward. There were some things like that which were very fascinating.

Then there was 666, the insecticide — benzene hexachloride. When they chlorinated benzene in an ultraviolet light they made stereoisomers. I think they made five — there are eight. There was one that was effective as an insecticide, the gamma form. These were white solids and they were said to have rows of girls crystallizing these in conical flasks for weeks to do an analysis. Well of course you can take milligrams now and do the thing in 10 min. It was all done like that in those days.


GUNTER HEYDEN

ESN: How about the American scene in those days?

HT: Van Zandt Williams was at Princeton doing some of the work with Bowling Barnes. After this, Williams went into industry with Cyanamid. That's how he kept his interest in infrared, and they did a lot of the pioneer work. Their first spectrometer was the Beckman little box affair for spot point analysis of butane/isobutane, used in the isomerization process to make isobutylene and iso-octane. Van Zandt Williams and Dick Perkin (Perkin-Elmer was nearly on the doorstep of Stamford, Connecticut) decided to make some instruments. Dick Perkin at that time had a little optical firm which didn't account for much, he did it as a hobby; he was an amateur astronomer, and he got into spectroscopy this way. Later on he hired Van Zandt Williams of Cyanamid to come in fully. That led to Perkin Elmer building the Model 12-C, which was a splendid little machine — I still have one which is capable of working, though it is a museum specimen. Later came the Model 21, but all this was in the early part of the war. I went first in 1943 to Glenbrook where Dick Perkin had his little factory at the start, and that is how I first got to know him. We became very close friends, also Van Williams. Van Williams, as you will probably remember, died during a visit to London for the American Institute of Physics in 1964. Dick Perkin suffered a heart attack in an aeroplane and died a day or two later in hospital in Limerick, Southern Ireland, about five years ago.

Dick Perkin was a very unusual man. He was never trained by any university or anything, he was not trained as a scientist, he was a bank operator. He used to call himself a "bond salesman", but he was a chap with a remarkable capacity for seeing something that was going to be important and good, and also for picking good men. That was his great strength, although he was no fool either when it came to learning the higher technicalities. He was a very remarkable man, Dick Perkin, I wish we had many more like him. I have heard very leading figures in the U.K. over the past 10 or 15 years saying, "by God, I wish we had some Dick Perkin in British industry".

Dick had just about all you need in a job of that kind, scientific knowledge, insight, but he had learned for himself and developed. He had charm; he won everyone's confidence. He also had certain contacts, from his banking days, with people in big business, and he could advertise his firm pretty well. He said to me, "I am going to set up a firm in England, and want it to be quite clear that I am not coming to England to make money. I think really we owe a debt to England from the war and if I can pay a little back this way that's what I am going to do." That's what he did when he set up Beaconsfield.

ESN: Presumably your links with America were easier at the end of the war.

HT: Well even then it was funny. In about 1947 I went to America at the invitation of Albert Noyes, who was then Professor in Rochester and I think President of the American Chemical Society. When I went I was asked to buy two Western Electric thermistors, those little thermistor/bolometers which were nickel and cobalt oxide or something, and I got all the paper work, import licence and other documents. They wanted them at Malvern so I went and brought them back. It was at the time when we had no food at all in the U.K. so the last night in New York I went off to Sixth Avenue to Christy's Delicatessen, and I got them to pack me some food and cans and one thing and another in two or three big packing crates. I came back on the *Queen Mary* or *Elizabeth* to Southampton Docks and the customs fellow said

"What have you got?" "Well, I've got these packing crates but before you go any further I have got two bolometers in my pocket." I took them out of my waistcoat pocket and there they were wrapped in little cellophane bags. He said, "What's that?" I said, "A bolometer". He said, "What's a bolometer?" Well I didn't tell the one I tell in my lectures:

A gentleman called Langley invented the bolometer,
Which as you all know is the kind of thermometer
Which will measure the heat
Of a polar bear's seat
At a distance of a kilometre.

Anyway I said that I had two of them and he wanted to have a look. He started opening them and I said, "Don't open that cellophane bag, there's a special window of rock salt in there and if you do it will get wet, you mustn't do that at all . . . here's all the paperwork, you see". He said, "You can't take these in." "Well, have you got any superior officers?", I asked and he said "yes", and I went upstairs to see these superior officers. They said, "You can't take these in, you'll have to pay the duty". I said, "I'm not paying any duty, these are for use in a government department, here are the papers, the licences, etc." So I went to the top man and he said, "You will have to sign a cheque for the duty and if what you say is true you will get your money back". I signed the cheque on my own account. It was £200 or £300. When I went downstairs, the fellows said, "What have they done with you?" so I told him that I had paid the duty. "What have you got now?" he asked, and I told him, "All those crates". So he said, "I'm not going to waste any more time with you," and chalked the lot as cleared! I received the money back from the Ministry. Anyway, while we are on bolometers, yes, they were very scarce. I remember that we were going round well after the war, a few of us in the United States bringing back selective photomultipliers. Dobson at Oxford, he's dead now, "robbed" me of my two best photomultipliers to do measurements on ozone in the upper atmosphere, from his lab at Shotover Hill! That must have been in 1948 or 1950.

ESN: Do you think spectroscopy is just going to disperse into being a subsidiary to other things or do you think it will preserve itself as an entity? This in a sense is connected with the aims of *European Spectroscopy News.*

HT: That is a very difficult question to answer. To the best of my knowledge there is no part of the known electromagnetic spectrum which is not investigated. Going from the extreme cosmic rays to the extreme radio waves there is no gap anywhere. There is no chance that new techniques will open some other region. With respect to improvements in existing methods, however, obviously better resolving power will lead to a more precise knowledge of energy levels, and these will lead to greater understanding of chemical and physical theory. It could be that you can apply further tricks and, say, vary conditions of measurement like temperature: take it from high (plasma) to very low (liquid helium) temperatures and maybe learn fresh things. That is academic more than applied. I should not be surprised if for some years to come we shall just see the use of existing techniques consolidated into routine applied work more and more, rather than a completely new breakthrough. All branches of science really grew like this. Chemistry at the moment is in a trough. Physics is living in a faded honeymoon of the post-war boom. It is coming down because people realize that nuclear energy isn't the dream they thought it was five or more years ago. They all go up and down like this. Spectroscopy firms will go on making "bread and butter" instruments for the next 50 years, and there are bound to be a few fancy machines too.

ESN: What would you say is a fancy machine, Sir Harold?

HT: Well, how about the photoelectron spectrometer? Of course, they have made reasonable instruments but one knew from the word "go" that photoelectron spectroscopy with ultraviolet sources has a limited life. I mean the number of problems it can solve and the things you can do with it are fairly restricted, and even now we are beginning to see the tail-off, except for a very few organic chemical high "recherché" type problems. I have felt for years, and still think so, even though the impression may be going against me at the moment, that X-ray photoelectron spectroscopy has always had far more potential application than the ultraviolet photoelectron, and it amazes me that it isn't working out that way. The truth of the matter probably is that some of the firms who are using X-ray photoelectron spectroscopy today are using it more than they are telling because they are using it for catalyst studies, and they don't want to disclose how much they are learning from it. I am amazed that this hasn't taken on more. The high cost of equipment is a starting problem. When you have someone who builds an instrument for something like $100,000 the price is immediately going to be an obstacle, but if it tells you something which is worth $20,000,000 you want the instrument.

ESN: Is there perhaps a point here, in that the war-time developments we were talking about earlier took place largely within a university environment, whereas these developments in ESCA may be taking place more in a closed industrial environment in which people are very much less anxious to say what they are up to?

HT: Of course this is true. There are two things about ESCA. I don't like to use the word ESCA, as you know. I think it was a bad name for it: Electron Spectroscopy for Chemical Analysis, although you certainly can do analysis with photoelectron spectroscopy. Siegbahn knows I don't agree with this name but he got it coined. Then you put the technique in the hands of industrial chemists and you don't give it the chance to develop in the way that other things like infrared and Raman did, including X-ray crystallography. Those techniques developed in an atmosphere where you were just doing it for sheer advancement of knowledge, in the abstract in the university department or similar.

ESN: Do you also class computers as fancy machines?

HT: Coupling a computer is simply consolidating what is now possible and what is now known. But you should be careful. Do not choose a complicated method if a simpler one will do. I have never thought that computers were the answer to all things. You don't need a 50,000 card index served by a sorting mechanism if a few spectra on the table will tell you what you want to know. I am just using this as an extreme example.

ESN: Probably where computers have come to the fore is in carbon-13 spectroscopy, and other types where signals are weak.

HT: Carbon-13 spectroscopy is a case in point. In polymer chemistry for example, certainly carbon-13 spectroscopy looks like being something really remarkable. But I think that it is no more than an embellishment of an existing theme and if you are looking for new things, well they are difficult to find. That's why I feel considerable sympathy with the young lads today because it was so much easier in my day for a young man to make some sort of a corner for himself. It's the shortage of empty corners that I am concerned with. I suppose this may be only just a symptom of age. I never forget giving a lecture in Duke University in Carolina — about 1960 I suppose it was — and in the front row was Hertha Sponer who had been in Göttingen, Germany, originally. Sitting alongside her was her husband, you know who that was? Her husband was James Franck and he was 88 then. After I gave my lecture, during which I was pretty scared because Franck was sitting about two yards in front of me, he came and had a word with me. I said, "Well I am afraid that was nothing like the quality I used to read of you in the 1930s," but he said, "Ah, but it was much easier for us in those days because there were so many gaps, today you have to look for them much harder". So maybe this just goes on.

ESN: We are sure it does go on because in a sense this is something ESN wants to reach and tap. The vast number of people who one feels are about the place all doing their bit and who aren't really getting very much back for it. What ESN is hoping to do is to reach those people who are working in spectroscopy in a lot of different areas, all over Europe, who are going to come along and tell us their thing, and to give them the opportunity to tell others what they are doing.

HT: Good, as I say I have worked hard for many, many years voluntarily and sometimes at great pain and energy for European scientific unity and to some extent we have had success.

I remember how after the war, three of us used to go to Germany several times every year between 1946 and 1951. Lord Todd, Harry Emeleus and I, in order to help resurrect the University of Science after the war. I will never forget the first Colloquium we had in 1946 or 1947 in Göttingen, when Blount was Head of the British Control Commission and he said we want you to come and give three or four lectures. We each gave two I think, Todd, Emeleus and I. I gave a talk about the emergence and application of infrared, they were all there: Eucken, von Laue, Ziegler who was a famous organic chemist, all the gang in fact were packed into the lecture theatre. I told Ziegler how instead of using ozonolysis to analyse different types of olefin we used infrared. He didn't believe it. I never forget Eucken getting up at the end and saying, "Well, what can I say — you have informed us about so much and I just don't know where to start. None of us have heard anything like it for years: it is tremendous!"

Later J. Lecomte, Paris, and R. Mecke, Freiburg, started a couple of little meetings in Konstanz around 1948. We then developed these into regular European Molecular Spectroscopy Meetings. The first was held in Basle in 1951. In 1953 Lecomte held it in Paris, in 1955 I held it in Oxford, in 1957 Mecke organized it in Freiburg, in 1959 Mangini in Bologna, and in 1961 it was held in Amsterdam by Ketelaar. Then in 1963 it was held in Budapest by Kovacs, in 1965 Bak organized it in Copenhagen, in 1967 Hidalgo did it in Madrid, and in 1969 Duyckaerts held it in Liège. There wasn't one in 1971. In 1973 it was held in Tallinn in the Estonian S.S.R., and this year the 12th Meeting was held in Strasbourg at the Institut de Physique.

Recently I was in Rome for a couple of days at the EUCHEM annual meeting of the committee. This is getting bigger and bigger, with more excitement and expansion, and this is very cheering.

ESN: There is plenty of young talent coming along in spectroscopy and we aim to connect with this talent through ESN. Have you a message for young people in general?

HT: Sure, but I am a bit less interested in youth than I used to be, as my opinions have become changed. But one always has to be careful that one isn't just becoming a "silly old man". I don't think I am. I believe that values have changed and I happen to have been brought up with certain fundamental values, but a lot of youth today has not been brought up with the same values. So they don't quite understand. In the end I am a firm believer that the fundamental laws of nature will win. I mean the First Law of Thermodynamics will win because that tells you that you can't get something for nothing but too many people nowadays think that you can.

ESN: In conclusion, may we please have your thoughts on European spectroscopy communications.

HT: I would only say what I think I said at the very beginning. I believe it is highly desirable, in these days more than ever before — and I have never had any doubts about this — that Europe should be much more a unity. This applies to science as well as industry or anything else. I am not very much attracted by the idea of building fresh, formal organizations which require money to set up a bureaucratic secretariat and all the rest of it. In my view this is not the answer. I believe that in my lifetime the things which have come my way which have been the most successful have ultimately been based on the individual enthusiasm of a few people, and I believe that it will always be so. A lesson I have learned in most of my scientific work is that if the initiative and the drive are taken on by a few, the effects will be far greater than by first setting up a mammoth organization to achieve something you know not what. You are much better advised to let the thing expand: drive it, of course, put pressure in behind, but let it expand almost in the way in which it wants to develop. ■