

Takeshi Oka at NRC

In 1964 my PhD supervisor at Sheffield University, Richard Dixon, arranged for me to go to do a postdoc with Don Ramsay at NRC in Ottawa, Canada. I assumed I would be in the group headed by Gerhard Herzberg and Alec Douglas who had created the world famous Molecular Spectroscopy Group. When I arrived in the late Summer of 64 Richard was actually there for ca 3 month period and before he left to return to the UK he suggested that while I was at NRC I should explore some of the other techniques being pioneered at NRC such as microwave and Raman spectroscopy etc. Almost as soon as I arrived Ramsay and many of the NRC spectroscopists went off to a Gordon Conference. I did not go as Ramsay declined my request to be allowed to go. This conference became somewhat legendary as I think it was the last appearance of Edward Teller at a conference of this kind. When I arrived early in August 1964 I had no idea there had just been a split between the Spectroscopy group headed by Gerhard Herzberg and Alec Douglas and Don Ramsey's group. Ramsey for various reasons – probably mainly because he was rather difficult when it came to sharing equipment - had split off into his own group known as "Larger Molecules". As I looked around the lab almost everything had an elliptical red "Larger Molecules" label and I wondered why there were no labels in the centres of the lenses! Anyway Ramsay's group was supposed to work on molecules with six or more atoms which as far as I could tell had electronic spectra far too complicated to be well resolved and analyzable with the resolution available in those pre-tunable laser days. One of the first people I met was Takeshi who was also in Don's group and almost immediately we and our wives Margaret and Keiko became firm family friends, indeed a friendship which continues to this day. Werner (Vern) Goetz, Don's technician, was the third member of a small, friendly close-knit troica. Although many call him Oka, as demanded by Japanese etiquette, I have always called him Takeshi and have always had as much difficulty calling him Oka as he probably has hearing someone who is not his immediate family calling him Takeshi. Maybe it was the unique atmosphere that existed at that time in the NRC spectroscopy group which engendered a closer family-type relationship among almost all the researchers that was able to undermine traditional customs.

At one period while I was exploring the electronic spectrum of CH_3NO , created by flash photolysis of t-butyl nitrite, some of the precursor vapour occasionally wafted through the lab causing heart palpitations. Also about this time all three of our wives (Margaret, Keiko and Lynn) gave birth and this coincidence led to some humorous comments around the lab that this triple coincidence might be due to the fact that t-butyl nitrite, a close analogue of the well-known heart stimulant amyl nitrite, might be an aphrodisiac! I must admit to being much in awe of Takeshi's scientific ability as he was already an accomplished scientist before arriving in Canada, having written some important papers with the eminent Japanese scientist Yonezo Morino. Takeshi was not only experimentally outstanding but highly capable theoretically, to me a somewhat awesome combination. Morino came to visit around the time of his 60th birthday and Takeshi taught Jon Hougen and me the famous Japanese bath song "Kusatsu" which Jon and I performed as though bathing in a mock bath set up for a cabaret at the party for Morino. I shall be forever grateful for being taught this song as it has earned me a lot of points since at social gatherings on numerous visits to Japan. I wondered whether I would ever be able to make it as a researcher but I made up a little of the leeway when Jon Hougen gave a fantastic series of lectures for two MSc students at Carlton University. Almost all the NRC spectroscopy postdocs such as: Anthony Merer, Neil Travis, Reg Colin, Fokke Creutzberg went along, as did some of the senior staff such as John Johns. Jon was the best teacher I ever had and after the highly intensive and arduous course I ended up with a fairly strong grounding in the fundamental aspects of Quantum Mechanics necessary for a good understanding of theoretical aspects of molecular spectroscopy. I also learned a lot about how to give a lecture from a master at presentation. Without this course I should not have been able to produce the book on rotational spectroscopy I later wrote.

I remember one day Takeshi mentioned that he was impressed by a paper written by Kelvin Tyler on the spectrum of HCP. I had never heard of this molecule before and looked into it in detail. The molecule had been produced in 1961 by Gier by passage of phosphine between carbon electrodes – a curiously déjà vue aspect of this experiment is the fact that C_{60} is produced in essentially the same sort of apparatus and even more curiously CP containing molecules can form closed cages when they polymerise rather than the expected flat aromatic nitrogen analogues such as triazine! At that moment, my chemical intuition told me that if one could make HCP one could surely make a whole family of RCP molecules, basically phosphorus analogues of the nitriles. This idea lingered perennially in my mind and finally bore fruit in 1974 when I at last got my own microwave spectrometer at Sussex and found a general way to produce phosphalkynes. This work was carried out in collaboration with my Sussex colleague John Nixon. The technique I developed involved the thermolysis of various precursors such as EtPCl_2 which produced MeCP and CCl_3PH_2 which produced ClCP etc.

Don had, I think met Takeshi on a trip to Japan and recognized his genius and invited him to NRC to carry out a microwave based analogue of flash photolysis, the technique developed by my Sheffield professor, George Porter with Ronald Norrish. Don's idea was to pump the rotational levels of a molecule, whose ground electronic state rotation-vibration spectrum was known, with a powerful single frequency microwave pulse in an attempt to modify the rotational population of the pumped level sufficiently strongly that associated electronic lines could be identified in the spectrum produced by the second analytical "spec" flash pulse. I read Takeshi's treatise on how he intended to do the experiment with awe and wondered whether there was any point my continuing with research as a career. It seemed such an erudite and meticulously researched project which was far beyond anything that I could even contemplate undertaking. Finally the day of the initial test arrived and after the experiment I asked Takeshi how things went. In my memory his answer was roughly: "Good for me but not good for Don". The first experiment was carried out on formaldehyde and although I do not remember the details I think the microwave pulse affected whole sets of lines, not just those whose ground rotation-vibration state was pumped. Takeshi found that the microwave energy was efficiently transferred by collisions selectively to other specific levels. Thus I think that the energy transfer phase of Takeshi's research life was born.

While Ramsay was away at the conference in that late summer of 1964 I had really nothing to do and heeding Richard's advice I asked whether I could sit in on some interesting microwave spectroscopy experiments being carried out by Jim Parkin in Cec Costain's laboratory. I was immediately fascinated by the technique and also got on really well with Cec who with his wife Cyn rapidly became firm lifetime family friends. The research atmosphere at NRC was fantastic and to this day several of the postdocs are some of our closest friends. I was able to talk to people like Jon Hougen and Jim Watson and Alec who all also became (a)

good friends. At the start I was under the impression (wrongly as it turned out) that I could work on Ramsay's project most of the day but also avail myself of the expertise of others. I had been advised almost at the moment I arrived not to agree to work on glyoxal, should this project be offered, so when it was indeed suggested I turned it down and chose instead to work on the electronic spectrum of pyridine. Pyridine had a peculiar spectrum which had puzzled scientists since the 30's when Hertha Sponer and the legendary (and contentious) Edward Teller had looked at it. The reason for its peculiarity turned out to be the fact that it had a non-planar excited state. This sort of change of structure for such a flat aromatic molecule on excitation was unexpected in those days. At some point in the first month or two after I started at NRC Cec took me quietly aside and told me, without any explanation, that I could no longer work in his lab. I guessed that Ramsay had decided that he did not want me to fraternise too much with others or carry out experiments outside his group. So I focused on pyridine and carried out a few experiments on my own such as work of CH_3NO and NCN as well as the carbon chain species C_3 . Only the work on NCN led to a publication and as it was the first on my own it was an important personal milestone. We spent quite a lot of time together as Takeshi was hard at work building his apparatus and on the other side of the lab I spent the days carrying out low temperature studies of pyridine or flash photolysis experiments and popped into-and-out-of the dark room developing the spectroscopic plates. Takeshi and his wife Keiko and I and my wife Margaret got on well. Takeshi amazed me one day when we talked about Shakespeare and told me he had read all the plays twice in English! He voraciously absorbed intellectual material of all kinds as well as books on every aspect of science. I felt truly out of my depth especially as there was also Jon Hougen and the amazing James Kay Graham Watson just down the hall together with a panoply of smart young postdocs from all over the world. Then also upstairs was the legendary Gerhard Herzberg.

It think it is fair to say that Don Ramsay and I lived in different worlds, there was little or no empathy between us and so when one day, out-of-the-blue, Don said that if I still wanted to do microwave spectroscopy I would have to do it full time, I could hardly believe my ears. As soon as my year as a Larger Molecule postdoc was over I was up-and-away into Cec's microwave laboratory where I was to spend some of the happiest and fruitful times as a researcher learning the microwave trade. Takeshi also occasionally spent time in Cec's laboratory as the energy transfer experiments clearly demanded the application of specialized microwave technology. One day after I had been in the lab much of the day I realized that Takeshi must have been under the equipment for several hours from well before I arrived in the morning, without my knowledge, fixing some technical problem or other. At some point Takeshi popped into the lab and asked me how I had managed to move from Ramsay's lab to Cec's. I did not really know the background details but I told him that I thought that Cec had engineered my escape and that he should chat to him. Cec was a very big guy with a most generous and kindly easy-going disposition, indeed a true Canadian. I do not know but I guess it was Cec who pulled the strings with Herzberg and Douglas that finally enabled Takeshi to set up his own lab. I had left NRC by then but always kept in touch over the years. Takeshi would occasionally send me reprints of his work and one in particular interested me greatly. It was a study carried out with the 46 metre NRC radiotelescope in Algonquin Park on the H_2CS a molecule. I had just been working on this molecule at Sussex where we had obtained the photoelectron spectrum. This was about 1974 when with my colleague David Walton, who had developed ingenious techniques to generate long polyyne chains, I set up a research project for an undergraduate student, Anthony Alexander. "Alex" turned out to be outstanding experimentally and truly meticulous in his work. He synthesized cyanopropadiyne (or what I still call - incorrectly - cyanodiacetylene HC_3N). HC_3N had been identified in space somewhat earlier and so one does not need to be Hubble's grandmother to contemplate the possibility that the slightly longer 5-carbon atom chain might also be in space. I wondered to whom I should suggest a joint search project. There was only Guisbert Winnewisser in Europe whom I knew only slightly and there was a rumour that a UK group at Kent was setting up a programme but it was not yet ready. Takeshi was a close friend so I wrote to him. His response is shown below.

National Research Council Canada
Division of Physics

Conseil national de recherches Canada
Division de physique

2.7-3.5 GHz
and 22-23.5 GHz
File Reference

March 11, 1975

Dear Harry,

Thank you for your letter. Yes!!

I am very very very very very much interested in your molecule

$\text{HC}\equiv\text{C}-\text{C}\equiv\text{C}-\text{C}\equiv\text{N}$. If you do not mind it, please give me B_0 for the molecule. If it happens to be in our observable ranges of frequencies I would like to try it.

Sincerely yours,
Takeshi

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One notices that Takeshi indicated in this letter that he is "quite" enthusiastic about the project and has written the same number of "very"s as there are C atoms in the HC_5N . Thus started our highly successful venture into interstellar space. With Lorne Avery Norm Broten and John Macleod we detected HC_5N in SgrB2 and we found that it was significantly stronger than we expected indicating that it was in some way a favoured species. Then David devised a synthesis of HC_7N and one of my super research students, Colin Kirby, set about making it. I came out from the UK to Algonquin for the search and arrived at the telescope before we had observed the spectrum. Colin however managed to get the stubborn beast into the waveguide cell and measure the B value a couple of days after I left and on a Friday called my wife Margaret who passed on the magic numbers to Fokke Creutzberg, another former NRC postdoc and good friend, by then on the staff at NRC and he phoned us at the telescope. (And did you but know it.) Takeshi with his usual perfect timing arrived on the Saturday morning just as the B value arrived. With Lorne Avery who was the astronomer who knew where the sky was we rushed to the telescope to attach a different receiver and started observing TMC-1 which by then had been identified as a good source of HC_5N . The computing power available was not enough to run the telescope and process the data at the same time so during the observing session all we had was an oscilloscope screen which displayed each set of very noisy 10 min slabs of data one at a time. We watched as the dots moved up and down in the channels for hour-after-hour. We knew that the line must be in one or two of the central three channels and so we focused on these central channels assiduously and were elated when after each 10 minute period the central ones seemed to be, on average, higher than nearby channels and tended to groan when they appeared lower. My memory is that they were high about 5 times more often than they were low and as time progressed we got more and more excited. As TMC-1 gradually went down towards the horizon and the signal from the warm atmosphere started to swamp everything we got restless and Lorne said that he could no longer wait and had to process the data. So the observing session was curtailed and Lorne went round the back of the computer racks to process the data. I do not remember the time of day; it could have been very late at night. Then suddenly when the line appeared on the screen Takeshi, Lorne and I were totally ecstatic. This was the most exciting specific experience I have ever had as a scientist. I remember that during the nights I had tried listening to the radio and heard Jethro Tull's most recent record "Songs from the Wood"; the signal was terrible but the signal from TMC-1 was perfect. I bought the record as a memento of the great experience which was all the more cathartic because we had no real-time data-processing capability. Today the line would gradually rise up from the baseline and the excitement of having to wait for the sudden revelatory experience that we had indeed detected the molecule would not have been anything like so exciting.

Back in Sussex I wondered about what to do about HC_9N and did have a thought that there might be a straightforward relationship between the B values and the number of carbon atoms but rather stupidly discarded the thought without trying it out and decided to concentrate on the synthesis with David. Takeshi had the same thought and smartly explored this idea and found a fairly smooth and accurate extrapolation. Thus we detected HC_9N as well. It was of course this series of experiments which led to the detection of carbon chains emanating quite copiously from the atmosphere of the fascinating red giant carbon star IRC+10216 which Eric Becklin and Gerry Neugebauer discovered. Ultimately it was this set of observations which led to the discovery of C_{60} in an experiment designed to simulate the chemical conditions in IRC+10216 which produced the carbon chains.

Of course no article about Takeshi can go without his brilliant detection of H_3^+ which happened well after I left NRC. Two comments seem appropriate as far as I am concerned. The first is that Takeshi spent several years of his life working on this problem by himself and one year of Takeshi's life as a researcher is equivalent to five or more of any other major scientist. The second is a comment with regard to a comment in Takeshi's elegant review article on H_3^+ . This refers to the fact that the day he finally obtained the spectrum he gave it to Jim Watson who analysed it overnight. In the article Takeshi writes that Jim should have been a co-author of the first paper. I know these two great and supremely ethical scientists better than most and I feel sure that Takeshi would have asked Jim to be a co-author of the original paper and I am equally sure that there was no way that Jim would have accepted.