

HERZBERG INTERVIEW TRANSCRIPT IN TOTO

A: All right: this is for a special issue we're doing in Science Dimension, probably ^{coming up} at the first of the year... We're also trying a new technique on interviews: we'll put them on the inside cover to encourage people to rip out the centre section and just read the interview right across (H laughs) - Dr. Herzberg, I'm fairly conversant with your C.V., but what I'd like to talk about this morning is what you're doing now and what you hope to do in the future. We'll look ahead instead of looking back for a change, if that's all right with you.

H: You mean what I'm doing scientifically.

A: Yes. Well, we'll start with scientifically.

H: The trouble is you see, since I won the Prize, my time has been somewhat limited because of interviews such as this one --

A: Of course.

H: It comes almost every week, some interview or other - Then when I travel, more interviews - For example, I was recently in Bulgaria, Sofia, for a meeting - And I must have had at least five interviews during that week - (laughs) What they've done with it I don't know, because they had to translate it - Anyway, I'm a little handicapped for reasons like that; in pursuing my scientific work / but I'm trying to do my best, because I still want to do a few things before I completely retire - I'm not retired now, as I'm sure you know -

A: Do you expect you ever will retire?

H: Well ... I think there comes a time when one ought to retire... I mean, I won't retire from actually working as long as I can, and my health lasts, but I think the speed of operation of the brain decreases as one gets older, or so it seems to me...Anyway, I was if you like, fortunate in 1979, when I discovered, by accident, or if you like serendipity, a spectrum of triatomic hydrogen, which anybody could have seen fifty years earlier, but they didn't - And I was very familiar of course with the spectrum of hydrogen generally, but this occurred under a somewhat special condition. It was just served to me on a platter and I didn't know what it was at first, and it took me about two months to -- not work, actually, but just looking at it -- I had it here on my desk, the other desk there, on a viewer, and now and then I looked at it and said, "Well, is it an artifact, or -- what does it mean?" until, one nice morning, it suddenly occurred to me what it was, and it didn't take me long to verify that this idea was right.

A: How did that idea come to you? Did you wake up knowing?

H: No, I don't think I woke up knowing, but I - it's very hard to describe that sort of thing: it was a matter of a few minutes to realize what was going on...Of course, one had to know all the possibilities of what it could be; at one time, I thought it might just be ordinary hydrogen - and in a way, that seemed the most logical thing, because it was produced under conditions where the spectrum of ordinary hydrogen was produced - But then, it didn't fit with any of the known things about hydrogen, ordinary hydrogen,

- At this sudden moment, I recalled the predictions that had been made about H_3^+ , the ion of $H_3 - H_3^+$, I knew, had been predicted as being a very stable ion, and indeed had been observed 60 years ago by J.J. Thompson in the first work on mass spectrometry—

A: Oh, I see: so it had been detected?

H: H_3^+ had been detected: in every mass spectrum that has ever been taken of hydrogen, H_3^+ always appears, unless you work at very low pressures - That was well known: and indeed we already had going in this lab (my experiments had that aim, to find a spectrum of H_3^+ - Not H_3 - So I was familiar with the literature on H_3^+ , I was familiar with (what) had been predicted: it's one of the systems consisting of only two electrons and three protons, which can be treated by quantum mechanics with very high precision; and so it had been found that it was a very stable ion with a stability about the same as ordinary hydrogen, only it was an ion, and of course you can't keep an ion alive, unless you do something like a discharge...But I also knew what the size (pron. "Say-ze") of the molecule (ie. H_3^0) was, that is the so-called internuclear distances -- that it was a triangular thing -- there is an equilateral triangle. From the theory, one knew what the separation between (sic) the nuclei was, the side of this triangle - which was something like 0.87 Angstrom units - And from that, it was easy to calculate what a certain rotational constant is, which is related to the moment of inertia of the molecule -- anyway, that was all in the literature! And I suddenly remembered, that morning, that the value of the rotational constant, that I thought I could read off the spectrum just by taking a ruler and figuring out what the scale was and so --

so - I could figure out what this rotational value, the so-called B-value was; and then I remembered that for H_3^+ , the B-value was -- Well, actually, I was talking about D_3^+ ; this was done with a deuterium analogue, because it was (gives?) a better spectrum -- it was just exactly what it was predicted at that time for D_3^+ - Now I didn't jump to the conclusion that the spectrum I had was ", because I knew that couldn't be; because there's no room for an electron state in the right place and all that - I don't want to go into details - But then this sudden thought occurred to me: "Well, if the structure of the molecule is similar to D_3^+ " --

A: It would give the same B-value.

H: (Pause) -It would give the same B-value, even if there's an additional electron, provided that electron doesn't interfere with the stability - And therefore the idea occurred to me it is what we call a Rydberg electron as is (?) - in an outer orbit, orbital - then it doesn't disturb much the structure, that is the B-value, -

A: So for purposes of estimating the B-value it can be ignored.

H: Yeah - (Resuming) And so I came to the conclusion that the spectrum I was observing was a transition between Rydberg states of a neutral H_3 , or D_3 in this instance - And I also have the spectrum of H_3 , only it's much broader (Bah-rrrrrrhoader) - Everything after that fitted together, and we have observed many other features in the spectrum - they all fit together with this idea, that H_3 neutral is stable in Rydberg states, that is in

states where one electron doesn't disturb the structure of the molecule, you might say. That is, it (the electron) is somewhat removed from the core of the molecule. So-

A: Does this mean it's highly excited to put the electron in the-

H: It is excited. It is excited. Not all that highly -- Indeed, the transition that we observed, or the transitions that we observe, most of them I should say, correspond to a transition that is well-known in the hydrogen atom, where you have the Balmer (?) series corresponding to the transitions between Rydberg states in the...The so-called H-alpha line of hydrogen, the hydrogen atom, is the analogue of these transitions, or rather these transitions are the analogue of the H-alpha line, which is very familiar:

which is the first, sort of, spectrum that was interpreted by Niels Bohr back in 1912 - and that's really how atomic theory, and the understanding of spectra, started out. So ^{there's} a close relation between this beginning -- Well, we have (haff) followed this up, some of the people here in the lab, and some people outside NRC became interested in it -- particularly Dr. Jim Watson who at that time was just a visitor in our lab, and is now on the staff -- He made a very important contribution toward the interpretation of the spectrum, because some of the spectrum - the one that I first started with was very simple: I didn't need any help to analyze it; but there were some other spectra that were much more complicated, and Jim Watson was of tremendous help -

in analyzing these more complicated parts of the spectrum - and everything together extremely well - indeed, now, there's a group at the - in one of the Max Planck Institutes in Munich, the Institute on Quantum Optics, and they're dealing with H_3 by laser techniques and have obtained some very fine results just after quite recently; I was visiting there/the week I spent in Sofia- So it doesn't only exist in my imagination in Ottawa; it (H_3^2) also exists in other places throughout the world - But of course, you can never isolate H_3 , simply because the ground state of H_3 is completely unstable : that is, if you bring a hydrogen atom and a hydrogen molecule together, they don't like one another; they just repel one another - There's no attraction. But the moment you excite either the hydrogen atom or the hydrogen molecule, and then bring them together, then they attract one another - and then you have excited states of this species, which are - which have a potential minimum, that is they are stable - but they radiate within a fraction of a microsecond and go to the lower state, which is unstable -

A: So the bottom falls out of it when it emits the decay photon.

H: Yes! so indeed, the people just recently, very recently in Munich, have determined the lifetime of the excited states of this system, the upper state of this emission, and they find values that seem to agree rather well with what had been predicted. Well, this goes back a little further -- I mean, I don't know how long you want to make this story, but the first time I talked about this interpretation of the spectrum as due to H_3 was in Japan, at the new Institute of

Molecular Science I was visiting, and that was in 1979- And in the audience were two people who were doing calculations, so-called ab initio calculations, of molecules; and an American by the name of Harry Keene (?) of the University of Buffalo, and a Japanese by the name of Morokuma, who was located in this Institute, they started immediately to do some calculations on this system. And they published a very fine paper in I think it must have been September 1979, in the Journal of Chemical Physics - And they found that their calculations agreed very well with the available observations that we had obtained - We didn't have any lifetimes in our work, but they predicted lifetimes, and these lifetimes have now been (hesitates for word) checked by the people in Munich ...with a very elegant technique. It all fits together; I mean, there are some differences that one can account for, but it all fits together rather well.

A: Were they -- they Japanese and American people, aware of your experimental results in detail on the H_3 molecule, before they completed their ab initio calculations?

H: Ah - Well, all they knew was what I told people in this seminar talk I gave at the (Japanese) Institute, I think it was in January '79. I don't recall that I sent them any further material about our results; and in a way they didn't need it -- and it was just as well they didn't have it .

A: Exactly! I was going to say they didn't. For it to be a true ab initio calculation they -- it would have to be a true double-blind;

they would have to be in ignorance.

H: (laughs): Yes. They didn't have any details about our results;
now --

A: But that would be a spur to do these calculations, because the
instant they were done they could be checked experimentally --

H: Let me just check this out --

A: Oh, I can follow that up later, if I know the (references) --

H: Well, I won't take the time now, but -- (returns from searching
actually, issue of the
in old journals) At any rate, In the same/Journal of Chemical Physics
--this journal here -- there were three papers, three theoretical
papers, not one but three -- one by Keene and Morokuma was the full
paper. Then there were two letters, -they have a letter section at
the end of each issue, you see - and one letter was from a chap in
Switzerland, Basil, who had heard my second talk on the subject,
which was in April of 1979 --

A: Wherever you go you catalyze papers!

H: (laughs) In fact, I saw him the day after my lecture, in Basil.
He said, "I've done some calculations." That was just the day,
the morning, after (laughs) I gave this talk.

A: You mean he'd been doing them before, or he was galvanized into

doing-them--- action -

H: No! He was galvanized into action, and right during the night, or
then
I don't know when, (laughs) - And/there was another chap at Los
also
Alamos, who did/similar calculations: where he had the data from
I don't remember: well, yes, I had published, of course, by that
time, my first little note about the subject -- (rootles through
journals again) -

A: Had this chap in Los Alamos heard about your work?

G: He must have. Well, as it happened, I also paid a visit to
Los Alamos, because I was interested in finding somebody who would
look at the tritium analogue of this spectrum, that is the hydrogen
of mass 3 -- because I was hoping that the spectrum might be
simpler and sharper before everything (?) - and so I went -- I
have a colleague there whom I know very well, and I asked him
would he be interested, and he said "Yes" - the trouble with tritium
of course is that it is radioactive, and you have to be a little
careful when you handle it, so we couldn't do it here; and they
were going to do it (at Los Alamos), but they haven't done it yet
(laughs) - Because they have other things to do, and so on, and I
am again in correspondence intellectually--(?) on a similar problem -
Now whether this man, his name was Martin I believe, Richard Martin,
-- Well, let me not take the time.

A: OK.

G: You see, there are two volumes that appear each year of this Journal of Chemical Physics, and my first paper was in the first volume of it, that must have been around April of 1979, and these three theoretical papers were published six months later -- Now the people in Japan at that time, if you like, had advance notice, because they heard my talk in January

H (continuing): The chap at Los Alamos I don't think had any advance notice other than my first paper (it was a letter, also, in it must have been April) - (Gives a reprint) It was published in May: May 1979 - and it shows the kind of spectrum; but we have much better spectra now, and - but that was the first announcement which -- well, I felt rather pleased that at age 75 I was able to publish a note of a new, of something fairly new, with my own name attached to it .

A: (idiot): When were the fundamental predictions of triatomic hydrogen first made?

G: The Rydberg states were first considered by these two people, Keene and Morokuma, and these other two people, in 1979, after; on the other hand, there is a history of H_3 . It's rather amusing -

When I first came to this country back in 1935, I went to the University of Saskatchewan, as you may know - and at the University of Saskatchewan was a physical chemist who -- I think he was just about to retire -- I had contact with him; and the thing he was working about (sic), and he had one student, who later on became a dean in Saskatchewan, with whom he had done this work, in which they found some active form of hydrogen which they ascribed to triatomic hydrogen. (laughs at remembrance)

A: 'The active form.' Would that be H_3^+ , or -

G: Well, no, normally when you say 'active hydrogen' you mean hydrogen atoms. You send a discharge through hydrogen, and you stream (??) through the gas, and when you put some compounds in the way of this streaming gas that had gone through the discharge, you find there is a reaction, and this reaction is normally a reaction of hydrogen atoms with whatever you have there - But they observed a reaction even when they felt they were sure there were not hydrogen atoms there, and they said, "This must be H_3 " - But they really had no good argument for it.

A: Do you think it was?

G: (laughs) no. Because - it cannot be, because all the states that they could have observed in H_3 were (sic) the ground state, which is unstable - Excited states are so far up and they're so short-lived they couldn't possibly have seen them.

A: They wouldn't have had the technology.

H: So what I suspect, and what I in fact, I suggested to them at the time, if my memory serves me right, was that what they were observing was vibrating hydrogen. When you excite molecular hydrogen, of course it is also vibrating - Vibrations of molecular hydrogen are 'metastable,' so-called - they cannot radiate, or only very, very rarely - And so the hydrogen molecules, the ordinary hydrogen molecules, in excited vibrational levels of the ground state, the electronic ground state, could serve as a reagent, because they live long enough: just as long (as), or longer (than), as (sic) hydrogen atoms. So I feel fairly confident that/^{what}they (the Saskatoon observers) were really experimenting with were vibrating H_2 molecules, not H_3 - But they thought it was H_3 : I still have the(ir) reprint here! Yah, I remember, he died pretty soon afterwards, and they were trying to distribute the books that he had left - He was a very nice chap.

In my Ph.D. thesis - no, not in my Ph.D., my Master's thesis, if you read up, you'll find that I had mentioned the possibility (at any rate) that part of the spectrum observed in hydrogen is due to H_3 . But that again was quite off the beam (laughs)

A: In other words, you feel that was not a correct suggestion?

eventually

G: No. No. I didn't of course foresee that/I would find a spectrum of H_3 . What I had then was not it! (laughs)

A: So what you're saying, going back to something you mentioned very early on in the interview, that this discovery of triatomic hydrogen was serendipitous; it was perhaps in an immediate sense, but not in the sense that you had been dealing in this field for decades.

G: Yes. I have worked on hydrogen for, for decades.

A: So there's serendipity and serendipity.

G: Ay - I've lots of published papers to show it - but I never really thought of neutral H_3 . I thought a lot about H_3^+ , but for some reason we never thought of adding one electron, because we knew, "Well, then it's unstable!" (laughs)

A: When was the work done on the Rydberg states?

G: Rydberg, of course, was a Swedish physicist at the University of Lund, a contemporary of Bohr, a little older perhaps, - but when Bohr first interpreted the spectrum of the hydrogen atom, Rydberg extended this to other atoms and he was the first one to realize that you have the so-called 'Combination Principle' that every spectral line corresponds to a difference of two 'terms' (as they were called): this corresponds to the energy levels of the atom or molecule that you are considering (ie. whose spectrum you are observing) - but the series that he (Rydberg?) saw were called Rydberg series, and the higher lines in such series always involved (TAPE ENDS SIDE ONE)

--in such a Rydberg series, correspond to Rydberg states as we now know them: that is states in which one electron is excited to an orbital that is well above the ground state.

A: Among its last possible ways of being bound to the nucleus before ionization.

G: Yeah, you might put it that way. But there is really an infinite number of these states --

A: Ye-e-e-s. But in practise, the nucleus can't be here and the electron in Vancouver.

G: --because the attraction is rather far-reaching; the Coulomb attraction obeys the inverse-square law -- Rydberg series have recently been studied in great detail by laser techniques because, you know, Rydberg states up to 100 or 200 in the principal quantum (?) up to 200 have been observed by laser techniques.

A: I meant to ask you about that, Dr. Herzberg. Before coming here I spoke with Dr. Alf Tickner, who's the Council Archivist. I wanted his viewpoint on how the Council had reacted when you got the Nobel Prize, but as it turns out he was in chemistry in some studies that were contiguous with yours. He was talking about the work you were doing, and I asked him and will ask you now: If you could have been working with lasers say, in the 50's and 60's, would it have fundamentally altered your approach to experiment? That is, those packed quanta of energy --

G: I -- think the answer to that is no. Of course you are quite right in assuming that lasers could have been discovered 20 years earlier (I hadn't assumed that at all, as it turns out: it was just a junk question) than they actually were, if you like, 'invented,' because all the theoretical and experimental facts were there, but

nobody did the experiment - so if the development of lasers had come earlier, then certainly we could have made progress in a number of problems. But I don't think really the time was quite right for that. I'll give you another example: One of the points that is I think emphasized by the Nobel committee in the award is the work that we did here on free radicals, the structure of free radicals. And one of the free radicals that I felt was particularly important was the work on CH_2 , methylene. That spectrum I looked for for fourteen years: this was not serendipity at all. I worked, well, off and on, if you like, for fourteen years. I knew exactly what I was looking for: I wanted the spectrum of CH_2 . I thought I could predict roughly where it might be, but it took fourteen years to find it. When it was eventually found, we found out a certain number of facts about the structure of CH_2 : there were two kinds of CH_2 , singlet and triplet CH_2 that interconvert very slowly, relatively slowly I should say, and things of that sort -- we knew that CH_2 was strongly bent like H_2O in the singlet state; in the triplet state it was linear, or nearly linear. Well, it turned out after a few years that really we were a little optimistic in thinking that it would be linear: it was slightly bent, but then the situation rested there for quite a number of years until the laser methods were much more developed. In the recent three years, I would say, in our lab, the younger people have again looked at CH_2 with laser techniques and have, for example -- just -- I have the paper right on my desk, we are going to publish -- they have determined what is the energy difference between singlet and triplet(CH_2), which was a contentious question for a long, long time; and they

have determined the structure much more accurately by this so-called 'laser magnetic resonance' technique -- Techniques which would have taken me a long, long time to, ah, digest, and to actually use, -- but I was certainly very pleased when I found that these young people from their own initiative, without my pushing at all, felt that this was sufficiently important to devote time to it (sic), and they published a series of papers that have been very well received by the spectroscopic community and other people...So, laser techniques did prove useful in the study of methylene, but at the time when I discovered the first spectrum of methylene, we weren't ready for that sort of stuff.

A: What other work are you working on now, or don't you want to say?
(NOTE ELEGANT PHRASING OF THE PHRASED PHRASE)

G: Well, there's nothing secret about it...(laughs)

A: No, it's just that perhaps you might like to save any new discovery for the formal publications.

G: No...You see, by discovering H_3 , my whole program of work for the future was changed. I mean, at the time I found H_3 I was really looking for H_3^+ , which then was discovered, the spectrum was discovered, by Oka, in this laboratory, before he left...He used a different method, so in a way you might say we were in competition; but we were aware of the fact that there were two methods, the method by using the absorption spectrum, the method using the emission spectrum. I was trying for the emission spectrum, and nobody has yet seen the emission spectrum of H_3^+ , the ion. But Oka found it in absorption.

very
He worked/hard at it for four years, and again, he knew exactly what he was looking for: in fact, the spectrum had practically been predicted. But there were still things that had not been predicted: that the spectrum would be as complicated as it is, and so on... But it's a beautiful piece of work, of Natashai (?) Oka, and as one of the initiators of this lab I feel very proud that H_3^+ was discovered in this laboratory by Oka. I was also engaged, as I say, in that search; but it was only a part of my program, which consisted in ~~was~~ looking at the ~~moleccular~~ spectra of molecular ions, for example the acetylene ion -- I started out ~~looking~~ ^{with} at the N_2^+ ion, ^{many} where I found the absorption spectrum, years ago now... and I was going to go on with the search for structure of molecular ions on the basis of their absorption or emission spectra -- But then this happened with H_3 , and the question immediately arose: are there similar systems, similar to H_3 in character, molecules that you might call Rydberg molecules, stable in Rydberg states? And although our late friend Alex Douglas didn't like that name --

A: I think it's terrific!

G: (laughs) Anyway, some other people have already adopted that name (A. laughs); I use it --

A: Perhaps Dr. Douglas was whistling in the dark; it's been established anyway.

G: Well, he was a very good critic, and I may say that when I had

this bright idea one morning in January of 1979, the first thing I did was to go and see Dr. Douglas, because he was a good critic. If he passed it, with his criticism, then I felt sure I hadn't made a big boner. And he was duly impressed and he said, "That looks very good, I don't see anything wrong with it."

A: But he doesn't like the term 'Rydberg molecules.'

G: (laughs) That's right.

A: What a sh - I think that's a perfect description.

G: (laughs) Anyway, the question was, are there other Rydberg molecules. And it was very obvious to predict some, because H_3 is stable in Rydberg states because H_3^+ is stable in the ground state. (Over interviewer's attempts to think this through out loud) H_3^+ is stable in the ground state; you have an electron way out here, it doesn't change its stability. Now there are many ions of this sort. For example, one that immediately came to mind was the ammonia ion, NH_4^+ ; well, that has been postulated for almost a century, you might say: in chemistry, when you have ammonium sulphate, or ammonium chloride, or whatever, the ammonium ion, NH_4^+ , is part of the molecule. And of course, NH_4^+ in the mass spectrometer is a very stable ion, and indeed it has been found that if you start out with ammonia, that is NH_3 , and bring a proton along, the energy that is released is almost twice that that is released when you have NH_2 and bring a proton along. That is, the so-called

proton affinity of ammonia is very high, is $8 \text{ A.T.V.}^{e.v.} (?)$, eight and a half $\text{A.T.V.}^{e.v.}$, or something like that. So NH_4^+ is a very stable system. If you now take an electron and bring it along, you should get Rydberg states which are also stable N.A.B. (??) to turn out that way. and the amusing thing is that there were spectra in the literature connected with ammonia that nobody knew what they were (sic). And these spectra, one of them having been known for 110 years, turned out to be due to NH_4° ! The ammonium radical! The Rydberg radical, if you like. We have the spectra here. They have presented considerable difficulties in the analysis, and two excellent theoretical chemists (if you would like to call them that) have been working on it, that is the same Jim Watson

I mentioned earlier, and John Hawgen (?) at the Bureau of Standards, who was once a member of our lab for a number of years. They're both very able theoreticians, and ^{on} this 110-year-old spectrum, the so-called 'Schuster band' (?) of ammonia, we have had a very good beginning, due to John Hawgen. In the so-called 'Schütyler band' (??), that's a second spectrum that was discovered around 1956 or thereabouts by a German spectroscopist, Schütyler, that has just, a week or two ago, the first real sign of clear-cut analysis was ~~was~~ obtained by Jim Watson of this, in this lab. So that the details now have to be worked out for this analysis, --

A: You feel fairly certain that it's NH_4° , though?

G: Well, the reason why we feel certain that it's NH_4 were experiments with different isotopes. That is, first of all we take ammonia

, send it through a discharge at high pressure, and we get this spectrum. Then we take heavy ammonia, ND_3 , and do the same thing, and we get a shifted spectrum. But then we take a mixture of ND_3 and NH_3 , and we get three intermediate spectra. And you can only get three, if you think of it, if you have four hydrogen atoms there. You have NH_4 , NH_3D , NH_2D_2 , and NHD_3 , and then ND_4 of the two N's - NH_4 and ND_4 .

A: Yeah, all possible combinations.

G: And this isotope effect is I think a clear-cut indication that there must be four hydrogen atoms present in the molecule. And we've also done experiments with nitrogen isotopes to establish that only one nitrogen atom is present. So that part of the story I think we have well in hand. Now just, again, a few weeks ago, a group at the IBM lab in San José, in California, has worked on NH_4 and ND_4 , and they have obtained this spectrum by a very special laser technique which -- I don't know if I could ever get it together here; there's enough talent here to do that work if they wanted to, but this man, a Swiss chap by the name of Wimsigger (?), has obtained an absorption spectrum of this ND_4 which is exactly the spectrum that we have -- I have it right there on my desk. He called me up -- it was rather an interesting (sic) -- and he said, "I have here a number of lines. Can you check whether these are ND_4 ." And they were! And then a month later he came back and said, "I have now revised the experiment and we have now a large number of lines, and they check exactly with the lines that you gave me over the telephone." You see, we don't hide anything from

our competitors. (laughs) So I feel very pleased that somebody outside this lab has also tackled this spectrum, and has made a real, important progress.

A: Yeah, that must be gratifying when something that's dear to your heart also excites somebody on the outside.

G: Yes, yes. In many ways you might say that one of the motivations of a scientist is to be recognized by his peers, having done something worthwhile.

So, naturally, we're going on to complete this work on NH_4 . And there are again similar things. There's, for example, H_3O^+ -- 'aitch three oh plus.' That's a very well-known ion in the mass spectrometer. The moment you have water anywhere, it's just like if you have hydrogen, you get H_3^+ , if you have water, you get H_3O^+ . Then immediately the question arises: wouldn't H_3O^0 be another Rydberg molecule? And we haven't found it yet. I have a candidate there, but I, I can't be sure that it is H_3 , H_3O^0 . So that's one of the next things we want to see if we cannot get. Well, then, there's still some leftovers from my work on molecular ions. I have a spectrum over there which is possibly, possibly I only, say/ NH_3 -plus, ordinary ammonia, ion. But it's very hard to prove it: and I haven't been able to get a better spectrum. I hope that, before too long, this will be accomplished.

Then there's a general 'guiding light,' you might say. I had a considerable interest in astronomical questions, and indeed one might

say that these Rydberg molecules might occur in interstellar space. There's just a slight suggestion that one of these features that we have observed (on the ground) occurs in the spectrum of one of these planetary nebulae; But that is still very uncertain.

Nevertheless, molecular ions are important in interstellar space.

For example, H_3^+ , everybody believes, is responsible for a lot of the chemistry in the interstellar medium, although nobody has yet seen H_3^+ in -- in space, you might say. Oka himself has

tried. He's done some observations at Kitt Peak Observatory but so far has not been successful in seeing H_3^+ . But it was the observation of ions in the interstellar medium that induced me to study more molecular ions in the laboratory. Then, of course, I came across this H_3 and so deflected me from my original aim;

but I'm getting back to the original aim. And one of the aims that I've had for many years is a ^{spectrum -- a} number of absorption features observed in interstellar space which nobody knows what they are (sic).

There are about, I would say almost a hundred papers published on different interpretations of these so-called 'diffuse interstellar lines', and I don't think any single one is right. But I'm still trying to find the right one. (laughs) How this will turn out I don't know. Whether I'll live to see the correct interpretation of these diffuse interstellar lines --

A: Even if you do, there will be something else to involve you up to the end, I'm sure. You'll die with your boots on.

G: It is perhaps remarkable from the point of view of Science Dimension

or a(ny) Canadian publication that a lot of the work on interstellar molecules was either originated in Canada, or done in Canada.

A: You're thinking also of Norm Broten's work in detecting them over the years.

G: Yes! Yeah. But in going back much earlier, you -- I don't know if the name Karl ^{Beals} ~~Biers~~ (?) means anything to you -- he was the Dominion Astronomer until he retired in 1965 or thereabouts -- he died about three years ago -- and he was one of the first to see these diffuse interstellar lines and confirm that they are interstellar. And secondly, his colleague -- he was at the Victoria observatory at the time -- his colleague, another Canadian, Andy MacKellar (?), who incidentally is the father of one of our most prominent staff members, young staff members, Bob MacKellar, who did some of this work on CH₂, incidentally, ah -- anyway -- Andy MacKellar was one of the first, if not the first, to study interstellar molecules. This was back in the early 1940's.

A: That was at a time when a lot of people thought that there was no such thing as an interstellar molecule because the ionizing radiation would just tear one to shreds. You couldn't possibly --

G: Right. Very good. Yah. Exactly. Preceding his paper there was only one single paper, by two Belgian astronomers, who had one single line,

and they thought it was a molecule. They were correct, as it turned out -- (laughs) -- but it was a little risky to base something as important as that on one single line. Well, MacKellar was the one then who found half a dozen other lines. Well, ^{he didn't --} he found them in ^{taken} the spectrum/by somebody at Mt. Wilson Observatory. They didn't know what it was. So. And then this work, as you point out, by Broten and his people (SHIT! BROTEN'S AN OLD MAN, READY TO RETIRE, CAME TO NRC 30 YEARS AGO AND SPENT HIS WHOLE CAREER THERE, AND HERZBERG HAD BEEN WORKING MORE THAN TWO DECADES IN SPECTROSCOPY WHEN THE 'NEW GUY' BROTEN BEGAN..!), and Oka was also intimately connected with that. They found these long-chain molecules in interstellar space. A very beautiful piece of work and generally recognized as such. So Canada has a long history in contributions to the problem of the interstellar medium. Well, it was actually Alec Douglas, who was a Master's student with me in Saskatchewan, who helped identify another set of interstellar lines which were due to CH⁺. And that work of course/^{has} stood the test of time... that was also back in about 1940. So those are the Canadian contributions, I think (?)

A: What about that part of Dr. Herzberg that is not directly involved in science? What do you hope to do in the future?

G: Well, do mean questions of science policy, or this kind of thing? Or -

A: Anything you want to talk about, whether it's to go on singing lieder, or whether you want to go and be involved with science policy.

G: Well, with respect to that, of course, I'm not getting any younger, obviously, and a time will come...I have...Or I'm trying to avoid speaking about problems of science policy. Sometimes I can't help but speaking up (sic), but normally I avoid it because I've really nothing new to say.

A: They know how you feel.

G: They know how I feel. And with Senator Lamontagne gone, I have nobody to fight...

A: Alf was telling me about it this morning.

G: He (Lamontagne?) is a very nice person, I met him a couple of times, and in fact at one time we were even on the same podium (laughs) But the politician's approach to science is in my opinion not a very good one. Because they only look at the usefulness of science; and when science is produced, in the production of science, you cannot always use -- ah, look at the usefulness. Because you cannot tell. If you think of the laser, when the laser was first discussed and invented, who would have thought that it would be, if you like, a billion-dollar industry? Nobody. Everybody would have thought you were a fool if you had thought so. And surely X-rays were not discovered with that (X-rays?) in mind. They turned out to be extremely important for medicine, but that was not in the mind of Röntgen when he discovered X-rays. In fact, that was a sheer case of serendipity also. It happened to be there: and what was it? And

the question what is it? Who are we? How is the universe made up?
That is the motivation of many scientists, not primarily How can I help suffering humanity? That comes also in, and should come in; but it's not the exclusive motivation. And it cannot be foreseen: that is the main point. If you want, if the government wants to support science, it should support basic science, and not tell the scientist, 'You must invent something to cure cancer.' It's not that easy.

Well, I've given enough speeches about that, and that's now a few years back. Once in a while, in interviews of this sort, including the one I had in Bulgaria, (laughs) I speak up for basic science as a pursuit of the human spirit in the same way as we pursue music or literature. And if you like, that brings me to my other hobby, that is lieder and singing: music in general. I'm still doing the best I can, but I'm not of course publicly (?) performing or anything of this sort. But I'm enjoying myself, and I'm singing.

A: There was a beautiful program on CBC FM English last night -- I don't know if you heard it. They'd read some letters of the great composers and then music that they had composed around that time.

G: I had to give a lecture last night. (laughs)

A: I should have taped it for you.

G: I would have liked to hear that.

A: I hadn't realized that Monteverdi's son was jailed for months by the Inquisition at one point, and he (Monteverdi) was petitioning the responsible duke to see if he couldn't talk the Grand Inquisitor into getting his son sprung. He'd been reading some books that he shouldn't have been.

G: God.

A: Yeah. Who says there isn't any progress?

G: That reminds me. I am also involved in -- not very actively, but I'm a member of the Committee of Concerned Scientists, both the Canadian committee and there's also an American committee. About what happens to our scientific friends in South America, Uruguay, and the Soviet Union --

A: Sakharov and the like.

G: Sakharov, and all that. But progress there is really very slow.

A: A kind of Amnesty International that's of scientists, for scientists.

G: Yes, yes. Well, I'm sort of supporting Amnesty International.

A: Yes, it's one of the more unsordid activities of people these days.

(END OF FINAL SIDE OF TAPE)

the question what is it? Who are we? How is the universe made up?
 That is the motivation of many scientists, not primarily How can I help suffering humanity? That comes also in, and should come in; but it's not the exclusive motivation. And it cannot be foreseen: that is the main point. If you want, if the government wants to support science, it should support basic science, and not tell the scientist, 'You must invent something to cure cancer.' It's not that easy.

107 Centre issues - Science cannot be confined to specific moulds.

spoken with personal conviction as well as the authority which comes with international recognition

Well, I've given enough speeches about that, and that's now a few years back. Once in a while, in interviews of this sort, including the one I had in Bulgaria, (laughs) I speak up for basic science as a pursuit of the human spirit in the same way as we pursue music or literature. And if you like, that brings me to my other hobby, that is lieder and singing: music in general. I'm still doing the best I can, but I'm not of course publicly (?) performing or anything of this sort. But I'm enjoying myself, and I'm singing.

A: There was a beautiful program on CBC FM English last night -- I don't know if you heard it. They'd read some letters of the great composers and then music that they had composed around that time.

G: I had to give a lecture last night. (laughs)

A: I should have taped it for you.

Not his most eloquent

Control/Pure/APP Herzberg ①

G: Well, with respect to that, of course, I'm not getting any younger, obviously, and a time will come...I have...Or I'm trying to avoid speaking about problems of science policy. Sometimes I can't help but speaking up (sic), but normally I avoid it because I've really nothing new to say.

A: They know how you feel.

G: They know how I feel. And with Senator Lamontagne gone, I have nobody to fight...

also discovery semi-conductors ('chip') was most unintentionally useful bit of ASU - study defects in crystal structures!

A: Alf was telling me about it this morning.

G: He (Lamontagne?) is a very nice person, I met him a couple of times, and in fact at one time we were even on the same podium (laughs) But the politician's approach to science is in my opinion not a very good one. Because they only look at the usefulness of science; and when science is produced, in the production of science, you cannot always (use---ah) look at the usefulness. Because you cannot tell. If you think of the laser, when the laser was first discussed and invented, who would have thought that it would be, if you like, a billion-dollar industry? Nobody. Everybody would have thought you were a fool if you had thought so. And surely X-rays were not discovered with that (X-rays?) in mind. They turned out to extremely important for medicine, but that was not in the mind of Röntgen when he discovered X-rays. In fact, that was a sheer case of serendipity also. It happened to be there: and what was it? And

Charles Townes =>