Partial transcript of interview with Maarten Schmidt, on his discovery of the first quasar. (Full transcript here)

Schmidt:

I suspected that the lines were variable but I wasn't sure, and I gave a possible explanation of some lines but it was all very unclear, very unsatisfying. Now that was in December 1962, probably a few days later I went to Palomar and I did some work then on number five, 3C273, of these stellar objects of Matthews 3C48, 286, 196, 147 and 273. It must have been after Christmas 1962 then that I took a number of spectra. This was a very bright object with a jet. The jet was very faint and the star was very bright and when Tom showed me this object which he had in fact gotten from Cyril Hazard in Australia, from the occultation I immediately thought that the Jet had to be the interesting object. But it was exceedingly faint so during that run I thought I might as well take a spectrum of the star. I had no idea that it would be of any major interest but just to get the star out of the way. The first exposure was very much over exposed, a 13th magnitude star. I usually worked on 18th magnitude galaxies and I over exposed it, really badly. It looked already a bit odd. I thought I saw one or two structures in the spectrum that looked odd but I didn't give it much attention and the next night, or the night after that, I took a number of exposures. Those exposures showed, I think, four emission lines. And I measured them and I told my colleagues about them. I think Bev Oke must have come up to Palomar to work with the multichannel spectrophotometer, probably in early January. It sticks in my memory that not very long after that time that we knew about the existence of yet another line in the infrared near 7600 Angstrom. And in hindsight, I sort of treated this material almost with abandon because I freely showed everybody where the wavelengths were and told everybody would they please try and identify it. [laugh] Instead of locking the door and trying to do it and discover it myself. So a number of people in fact were cooperative and gave me suggestions of what it could be but things never fitted exactly.

There were always one or two important lines missing and things were never satisfactory. It was under those circumstances that I got a letter from Bolton in Australia who said that Hazard and the others who had been involved in the occultation were writing an article for Nature, would I perhaps write a brief companion article to describe the spectrum of this thing because I'd kept Bolton informed about what I found. So it was on the 5th of February in 1963, it was a Tuesday afternoon and I sat down in my office over there where Jim Gunn now is, and I sat down to write it down for Nature and I had a spectra next to me and I was looking at the spectra once in a while to make sure that I was writing down everything I should. I realized at a certain moment that out of the six lines that were known, if I left out one in the red, one in the very far blue at the end, then the remaining four lines seemed to be fairly regular. In order to investigate this regularity, what I did was entirely foolish. I tried to construct an energy level diagram, which is something I'd never done before. In hindsight it is not reasonable to do that. So I tried to do that, determined frequencies and differences of frequencies for the four lines that seemed to be regular. When I did this things didn't work out.

So I got a slight bit frustrated, look here, it is regular isn't it, I said to myself as it were. And in order to prove that, I decided I would take the ratio of the wavelength of the lines to the nearby balmer line because they are spaced regularly, one, two, three, four, five. So I did that and that's when the discovery came because the first ratio was 1.16, between the first line and the nearest balmer line. And then the next line and the nearest balmer line gave 1.16 again and the third was 1.16 and the last one was 1.16. Then I suddenly realized that if I took the balmer spectrum and increased it by 16% wavelength that I could explain four of my lines. That included Oke's line in the red and three of the lines I had here. Then I immediately checked the other two lines. If those were also 16% shifted up, where did they originally come from? The one came from 2800 Angstroms and I was vaguely familiar with Magnesium II and that's what it turned out to be. The other one gave me about 5,007 which is the strongest line of the well known OIII doublet which is often seen.

The only thing was that it was somewhat faint in this case but except for that it was quite reasonable. So I, in about an half an hour or so, suddenly had all the lines identified with a redshift of 16%. A redshift of 16% is not so large because a redshift of 46% was known for a galaxy. But this was a bright star and not an excessively giant galaxy and that was the astonishing thing, that a bright star could have a big redshift. Well, I opened my door, I think I saw Jesse walk by or I went to his office and said could he come in my office for a moment, I wanted to tell him something. So he came to my office, then he sat down and he got pale when he heard it. He said perhaps we should look at 3C48. Now a funny thing had happened, again in hindsight it was funny a week before we went through this discovery process, Jesse had finished a thick paper about the spectrum 3C48 with what afterwards appeared to be a no good explanation of a spectrum. He himself was not necessarily very convinced, yet he put a lot of work into it because he found the things should be explained. And I remember that the week before this all happened he came to my office, he'd thrown the thing down on my desk, somewhat frustrated, and said well, if you don't have any remarks within a week, I'm going to send it off. So in hindsight, this whole thing was in order to... Alright, we looked at the spectrum of 3C48 for awhile and we soon found, suddenly our eyes were opened, we were willing to try big redshifts, that is it had a redshift of 36 or 37% and the interesting thing is we got again Magnesium II that way. Magnesium II from 2800 so we now saw in two different quasars that they both had Magnesium II emission. That was of course a mutual confirmation. So it seems quite promising at that moment.

So we got a redshift of 36 or 37%, we then spent quite a bit of time on the derivation of whether this could be perhaps due not to a redshift at all, whether we could think of highly ionized light atoms, iron, with only one electron remaining in that outer shell that would give a Balmer type spectrum but shifted. So we spent on the order of half an hour on my blackboard to try and work this out. And our initial impression was that it could not be done. Because of all the activity in my office, Bev Oke was attracted and we discussed the thing for another while and I think it must have been 5:30 when we adjourned to Jesse's house where we had a couple of drinks. Then Naomi his wife was just flabbergasted because we never do that kind of a thing. We just entered there and we discussed things for another while and then I went home and I told my wife that, I

think I said something like, something terrible happened at the office today. And I proceeded to tell her. And I remember that night, in the evening, in the living room I paced up and down like a caged tiger for probably hours. Because it all became clear, already at that moment, although it was yet to come what it meant, what the future held in this case. Because if you see very bright objects with such large redshifts then somewhat fainter ones must have much bigger redshifts. I think I also spent that evening on finishing the work we'd done on my blackboard to see if there could be hydrogen-like ions. Although we never published it, I think we could convince ourselves easily on the basis of that proof that it couldn't be. As soon, of course, as you have two or three very different redshifts you'd have to invent a different ion for each of them. So that was fruitless anyhow. There was in the beginning, certainly with me, a deep worry that we were missing something completely, that we were being fooled somehow. And that, and I remember that was the main worry with me certainly for two or three weeks.

One felt that there might be a big trap. That you might feel after you had published it and somebody came up with a simple way out that showed that things were not so extraordinary at all. As if you'd been sort of taken, as if you'd been taken in, that you proved that you're so gullible that you really thought the star of 13th magnitude could be at a distance of two billion light years, ha ha ha. Who could ever believe that. And yet, we thought as hard as we could and we told our colleagues and we simply couldn't think of anything that was really a way out. Now we thought also very briefly about gravitational redshifts, in other words not motions. But basically we thought that these were redshifts just like the Hubble Expansion. Therefore, if you saw a 16% redshift for these things, it had to be just as far away as galaxies at 16% redshift. In other words, the Hubble constant applies to them. Although we did briefly think about gravitational redshifts, we didn't initially give much, as much idea that they could be the cause of these redshifts. So anyhow, it wasn't too long after that I finished writing in Nature article which took a rather different tone. Jesse and Matthews had a 3C48 discussion right next to it and Bev Oke came with a description of the red spectrum of the 3C273. So together with the Australian paper we got four papers in a row in Nature which must have happened in March or April, I think, 1963. So that was how it happened, I think.

Wright:

I think it was quite interesting you giving some of your insights as to the agonizing experience it was for you.

Schmidt:

Yeah. At that time it was real agonizing about science. Later on there came agonizing about publicity, you know, just publicity to such a degree that it became enormous pressure. At that time it was simply a matter of knowing that Nature forced you to say something. You couldn't keep quiet and you had to say something and it better be good because it was clear it was an occasion. So it was to be an occasion and whatever the explanation was, it was to be a rather remarkable event in astronomy. That was really what was, of course, going through my head when I was pacing up and down that night.

Boy, I'll have to say something, what do I say, do I believe it, is there a way out, what does it mean for astronomy? It is sort of interesting because, I would say that indeed it was, in a sense, the birth of the present era of exotic phenomena, exotic and explosive phenomena in astronomy, with the quasars, the pulsars, the x-ray binaries, the black hole, the 3° background radiation. I mean all these things were yet to come. The quasars suddenly started it and since then just about every two years there has been a major development of another discovery. Astronomy in an accelerated development that is just unbelievable. I mean before 1963 things were so unlike after 1963, there was no way to compare it. So in a sense the agony and the pressure of making a good on-the-spot scientific judgment just in one day essentially, the fifth of February, was a very interesting one. Because we had not been subjected to this yet. Later on it was much easier for people to accept extraordinary things in astronomy because we've seen it as I said every two years we've seen them. This has come on with about five to six, even with seven different types of phenomena including the gamma ray bursts that you may have heard about. Fantastic things. You never heard things like it in astronomy! And if they came, it was one a lifetime... So it was the beginning of an era that, of course we didn't know at that time, we couldn't help but realize that the quasars would play a very important role from then on, it was clear enough.