0957+561: THE UNPUBLISHED STORY

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Happy birthday, Bernie, whenever it was. I also bring greetings from the Astronomer Royal to "young Bernie." I think the last time I saw you, Bernie, was at the center of the universe. Remember that? And for those who don't know the center of the universe, it's in Beijing in a park at the spot that was defined by an emperor as the middle of the middle kingdom and hence the center of the universe.

About five years ago, there was a symposium in Liège, and on the evening of the closing day, I went out with Bernie and other people for a gourmet meal, and Bernie gave me a pretty rigorous examination on how we came to find 0957+561, which was of course the first example of multiple imaging by a gravitational lens (Walsh, Carswell, and Weymann 1979); so it occurred to me that this was a good occasion to tell the story to a wider audience.

The story actually started about 1970 or 1971 when the 76-meter Mark I telescope at Jodrell Bank was upgraded and Sir Bernard Lovell asked the members of the staff to propose new programs. I proposed to do a survey to measure accurate radio source positions for identification purposes. A student of mine, Ian Browne, had been doing this kind of work for a year or two using the interferometer at Defford, which was built by J. S. Hey and his colleagues, and which gave 1-arcsecond positions on *unresolved* sources, which permitted unambiguous identifications. This was the first time that really unambiguous identifications had been possible. In 1970, source identifications were still a very important objective of radio astronomy. There were few identified sources, and there were only a very few incomplete samples; 3C had only partly been worked through. That was the time before the Cambridge 5-km and Westerbork, and long before the VLA came into operation. Routine measurements of a second of arc were not common. They practically didn't even exist. The work done with the Defford interferometer showed that you could do this, but by then it was running out of things to do. There were two 25-meter dishes and they had observed almost everything possible within their capabilities. At Jodrell, we had the Mark IA telescope of 76-meter diameter, with the smaller Mark II, of

25-meter diameter; together these would make an interferometer with better sensitivity and better primary resolving power. I thought these could be used to find accurate positions.

We actually started in November 1972. I was working with Ted Daintree and Ian Browne. In addition, I had a new student, Richard Porcas, who lived with this project for a couple of months and did the donkey work for us. The idea was that with the Mark IA at 966 MHz we would do a finding survey consisting of raster scans for declinations 40° to 70°. As I go along, I'll point along several things that were improbable, lucky, whatever you want; you might think at the end of it that we really had no right to find the object 0957+561. But maybe that's the way science works. We had an allocation of about a month on the Mark IA telescope, and there was very high pressure, very high demand for the telescope. It was estimated that we could do the raster scan survey in three weeks. It took twice as long as we expected because we had interference from satellites and other things. At the end of our allocation, which came at the end of December 1972, we had reached declination 55°. However, Sir Bernard was very interested in what we were doing, looked at it, liked what he saw, and said, "Right, you can have another month." We carried on for another three weeks into 1973. Without this extension, our survey would have stopped just short of declination 56°.1. Lucky break number one. (As a footnote, another professor at Jodrell Bank, who was anxious to use the Mark IA to pursue his investigations into pulsars, was quite annoyed about this delay in his plans. Fortunately, he doesn't bear a grudge, and we remain friends; he is now Astronomer Royal and director of Jodrell Bank, and I owe my attendance at this meeting, in part, to him.)

The first observation that started me down this long trail was the survey scans. Figure 1 shows the first detection of the object on several adjacent declination scans. Now this was not a particularly exciting event; 0957+561 was just one of many sources, and not a very distinguished one for any obvious reason. This was on 4 January 1973, just after we got our extension. So that was the first observation, six years before it came to public notice as a gravitationally lensed object. The rate at which sources in the survey increased led Richard and Ted to devise an automatic, routine searching method. This produced a finding list of about 800 sources with typical positional accuracy of about 2 arcminutes.

We went through them all, with the Mark IA–Mark II interferometer to measure the accurate positions. We started that in June 1973. Another student, Anne Treverton (soon to become Anne Cohen), started to work on the program soon afterwards. We made the first identifications around the end of 1973. Richard did them on the Palomar Sky Survey before we

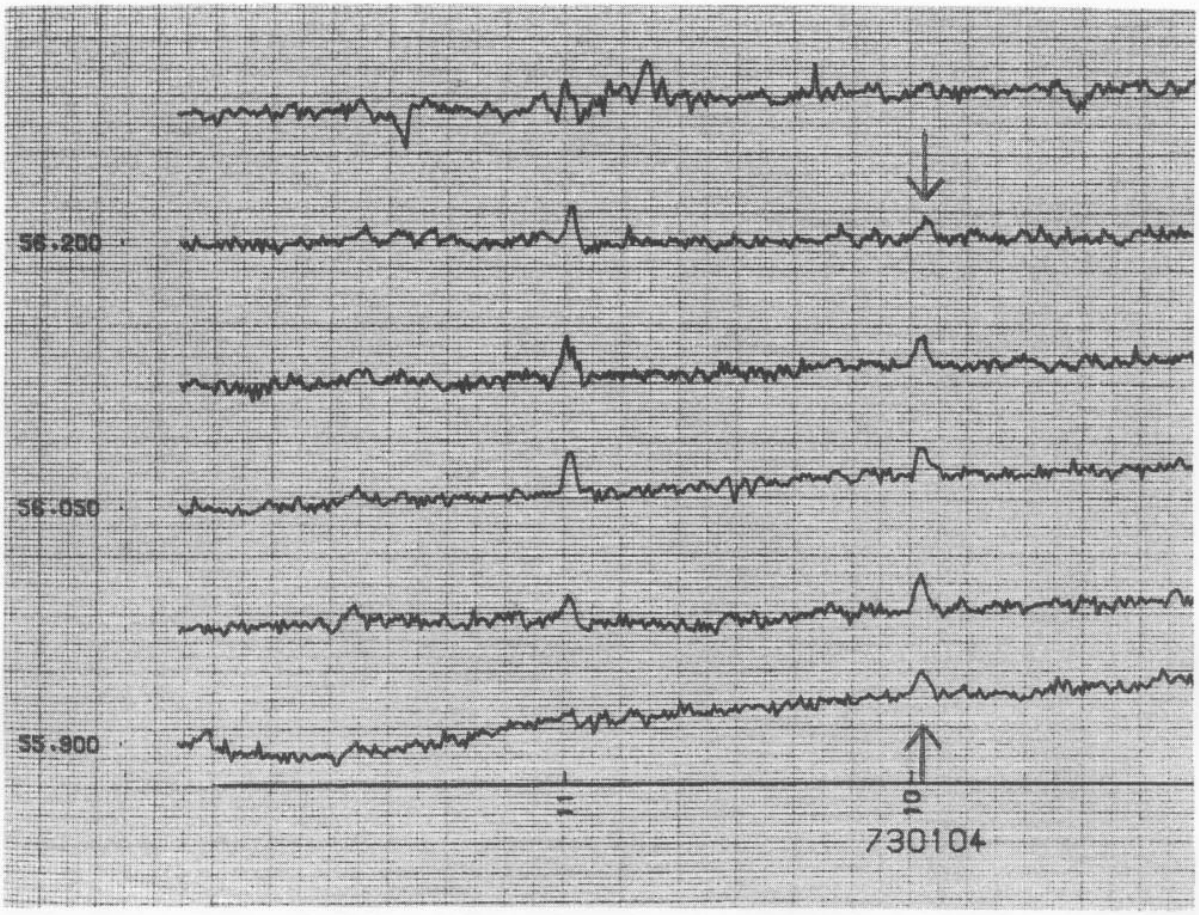

Fig. 1. Survey scans taken at the Mark IA telescope, right ascension range 09^h–12^h, declinations indicated, 0958+56 indicated by arrows.

had an accurate way of measuring the survey prints. Anne carried on later. Richard submitted his thesis in February 1975 and went off to NRAO. Anne Cohen carried out the remaining identifications, by which time we had an accurate measuring engine and could measure optical positions to half an arcsecond (Cohen *et al.* 1977). We got a final radio position accuracy of better than 2 arcseconds for about 70 percent of the sources, and we could do unambiguous identifications. The reason we could only do 70 percent instead of all of them was because some sources were resolved on that baseline or were confused by other sources within the primary beam. So it left about 30 percent for which we couldn't measure positions very accurately and so we couldn't get unambiguous identifications. The source 0958+56 (as it was originally listed in Richard's thesis) was one of these.

The program that Richard went to NRAO to do was to measure the positions of these sources using the 300-foot dish at wavelengths 6 and 11 cm. The primary beamwidth at six centimeter wavelength was 1.8 arcmin, and this was good enough to get positions to 5–10

arcseconds. That isn't really good enough for unambiguous identifications, but it's pretty useful. I want to show you some results that came essentially from Richard's work. Figure 2(a) is reproduced from the Palomar Sky Survey, and the identification of 0957+561 is indicated. It doesn't look like a double object on the scale of Figure 2(a). The dominant object in the field shown is an 11th magnitude galaxy, NGC3079. The separation of 0957+561 and NGC3079 is 14 arcminutes, and our Mark IA survey with an 18 arcmin beam listed a single source 0958+56 midway between 0957+561 and NGC3079. Figure 2(b) is reproduced from the recent all-sky survey by Condon and Broderick (1986) made with the NRAO 300-foot telescope at 1400 MHz. Their beamwidth was $\sim$ 10 arcmin, and the point-source response has circular contours. With their greater resolution, it is clear that the "source" listed in our original survey is really two unresolved sources, the stronger being NGC3079 and the weaker being 0957+561. The radio position of 0958+56 listed in Richard's thesis is close to the centroid and is really quite reasonable. It is within 2 arcmin of the 4CP position of the source 4C55.19. Using this position, Caswell and Wills (1967) had identified 4C55.19 with NGC3079. The latter had been detected as a radio source even earlier by Heeschen and Wade (1964). So this area had already been picked over.

The interesting thing is the search procedure that Richard followed with the 300-foot dish. He started with the 4CP position and searched immediately around it but didn't find anything. Then he proceeded to move north. He could have moved south, but he moved north and found a source. He *could* have found NGC3079 then stopped, but he moved north and found 0957+561. The source 0958+56 in our Mark IA survey had a 966-MHz flux density of 1.7 Jy, which puts it well above the survey threshold of 0.7 Jy. If 0957+561 had been a little further away from NGC3079, the two would have been resolved in our survey and we would have identified NGC3079 but not 0957+561. We know now that NGC3079 is much stronger than 0.7 Jy; it should be in our survey and it isn't. 0957+561 is weaker than 0.7 Jy; it shouldn't be in our survey, but it is.

The identification still had to be done. In the summer of 1976, Richard supplied us at Jodrell Bank with a lot of positions. Anne Cohen was doing the identifications using this information. Simultaneously and independently at NRAO, Richard had a summer student, Meg Urry, who also was trying to make identifications using the positions. Anne Cohen had drawn up a preliminary list of identifications in November 1976, which contained the source 0958+56. It was classified as "no identification," but she mentioned "a double-stellar object, magnitude 17.4, 24 arcsec north, not the galaxy of Caswell and Wills." Twenty-four arcsec was a little bit

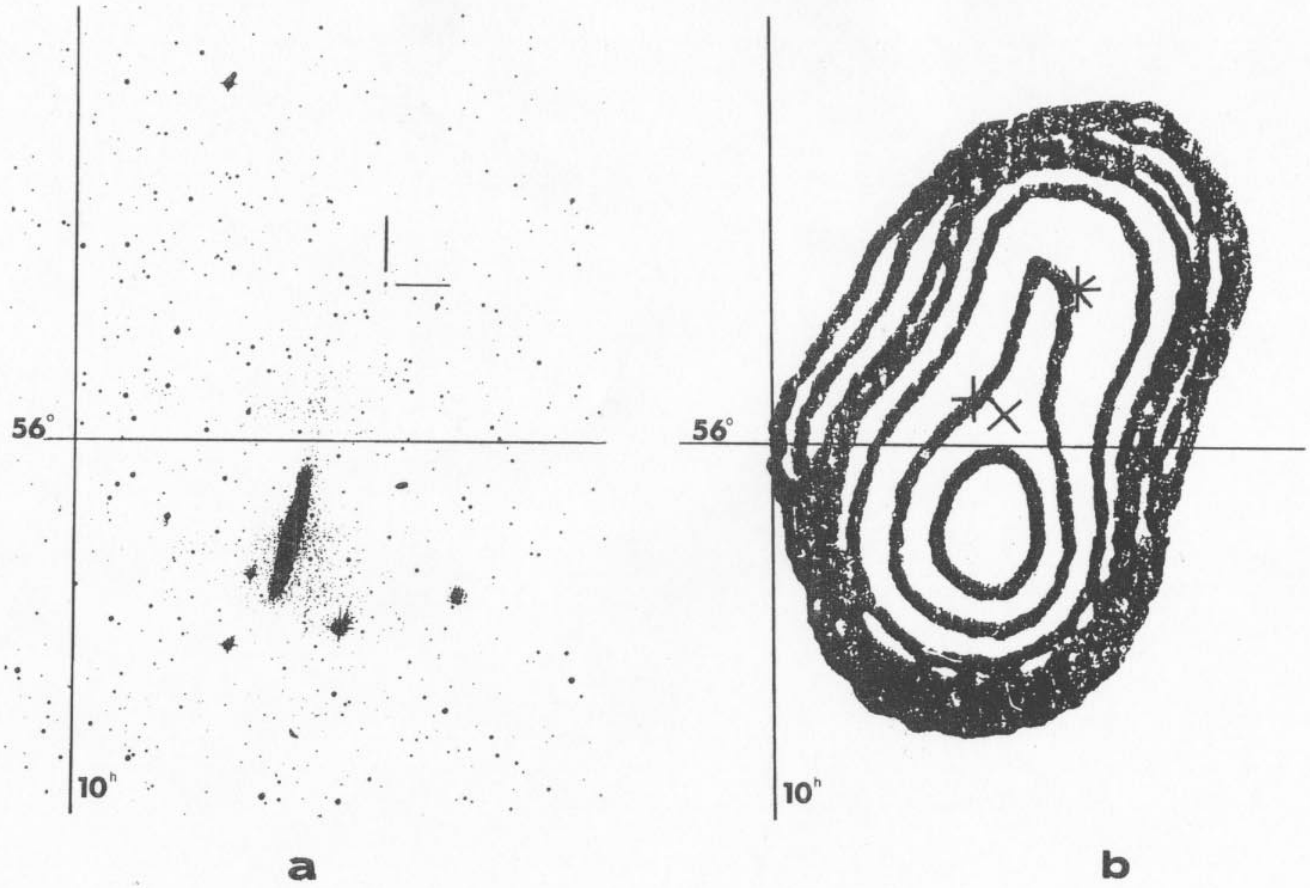

Fig. 2. (a) Field of 0957+561 reproduced from POSS. The "double BSO" is indicated. The prominent galaxy is NGC3079; its separation from 0957+561 is 14 arcmin. Right ascension 10^h and declination 56° indicated. (b) Portion of 1400 MHz Sky Atlas (Condon and Broderick 1986) to the same scale as Fig. 2(a). The peak contour is centered on NGC3079. Indicated are: position of 0957+561 (✳), position of 0958+56 from 966 MHz survey (+), 4CP position of 4C55.19 (×).

far away to make an identification. If you identified everything within 24 arcsec of our positions, you'd end up with a lot of false identifications. However, these were very blue, which made them interesting. Quite separately, Figure 3 shows an extract from Meg Urry's log from the summer of 1976. She draws attention to the pair of objects, one BSO of magnitude 17, five arcseconds north, the second BSO of magnitude 17, ten arcseconds north. Those positions were fortuitously good because the final radio position determined by Richard was 17 arcsec from the nearer BSO,

Source	ID	Description (possible objects and comments)	
0847+49	E	nearest object is 30" away	
0849+54	U	1. BSO, m~15; 2. rG, m~17	
0850+58	Q	1. BSO, m~17.5, on radio position	
0856+51	E	some faint objects (grains?) on red plate	
0857+56	G	(1. RSO, m~19, 15"-20" N); 2. bG, m~20.5, 10"-15" NW	
0903+48	U	1. RSO, m~17.5, 5" W; 2. rG, m~19.5, ~10" ESE	
0917+45	G	1. rG, m~18:; 2. RSO, m~17.5	X
0924+60	U	1. rG, m~19.5, 20" NW; 2. rG, m~20, ~25" W; 3. rG, m~20, ~20" NE.	
0928+48	U	1. BO, m~19, <5" N (faint fuzziness: galaxy on BSO?)	
0935+42	G	1. rG, m~17, (very red)	
0958+56	Q	1. BSO, m~17, 5" N; 2. BSO, m~17, 10" N	
1008+42	E		
1017+48	G	Two galaxies: 1. rG, m~15, ~10" N; 2. rG, m~15, ~10" E	

Fig. 3. Extract from M. Urry's log made at NRAO, summer 1976.

still too far for a confident identification. Richard had several starting positions and we needed him to tell us which one to work with. Anne and Meg were the first to draw attention to the pair of BSO's. However, it's still a long way from drawing attention to an object to having a firm identification of a radio source.

Anne measured the accurate optical position, as published (Porcas *et al.* 1980), on the X–Y machine on 9 June 1977. I personally became aware of it in August 1977. For several years, I'd been trying to obtain optical spectra of the stellar objects in the survey. If we had a blue stellar object, it was almost 100 percent certain that it was a QSO. We had a few red stellar objects too, but the great majority were blue. I'd been working with a number of people on this. Ian Browne worked with me; Alec Boksenberg and Maarten Schmidt took some interest in it, and I got some 200-inch time with them in 1974; Derek and Bev Wills in Texas became interested in taking spectra at McDonald, and I observed with them there in 1976. In 1977 I was preparing an application to Kitt Peak to do more of this work. Figure 4 shows the first record of 0957+561 that I can find in my notes. It is a list of queries to Anne Cohen. I had asked her to give me a list of all the stellar objects in the identification list, and one can see the note adjacent to 0957+561 that reads, "Where did this come from? Magnitude? X,Y?" Note, it's called a double BSO with 6 arcsec separation. I guess this realization came into my consciousness on August 23 in that year. This source was included in an application to Kitt Peak. Inevitably, the application arrived after the September 30 deadline, so we didn't get time at Kitt Peak that year. With a much improved version of the application the next year, we got time.

I was quite excited about this whole thing because, although 17 arcsec away from the radio position wasn't in terribly good agreement (in fact, it was poorer than the criterion for inclusion, which was a search radius of 10–15 arcsec), the two stellar objects were very, very blue. The A object is the bluest object in our whole survey, which is a fortuitous thing, I suppose. My proposal to Kitt Peak was joint with Bob Carswell, with whom I had been working since the earliest days of the spectral work. On our first clear night, we quickly got onto this object. Figure 5(a) shows the first spectrum ever obtained for 0957+561A. It was taken on 29 March 1979 with the IIDS. Anyone familiar with the instrument will know that this was taken at the telescope: we've still got the instrumental profile, which gives the overall shape. After 20 minutes, there are two strong broad emission lines, which were clearly carbon IV and carbon III at redshift 1.4. We couldn't reduce the data fully at the telescope, but the redshift was clearly approximately 1.4. Fine, we had a QSO. Then we moved on to object B. Figure 5(b) is a spectrum of object B obtained a few minutes later. There are two strong emission lines, the

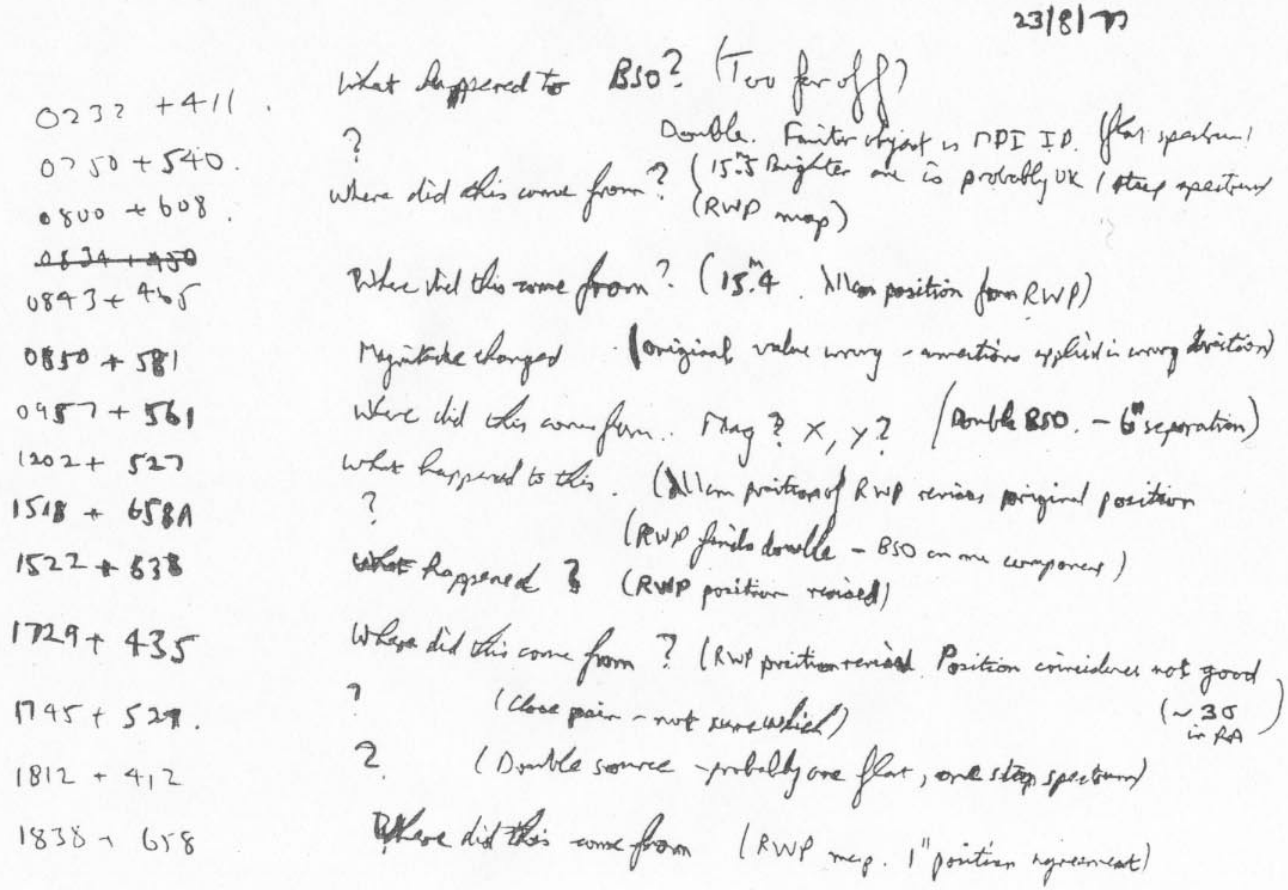

Fig. 4. List of queries from Dennis Walsh to Anne Cohen, 23 August 1977. (Partial answers in parentheses.)

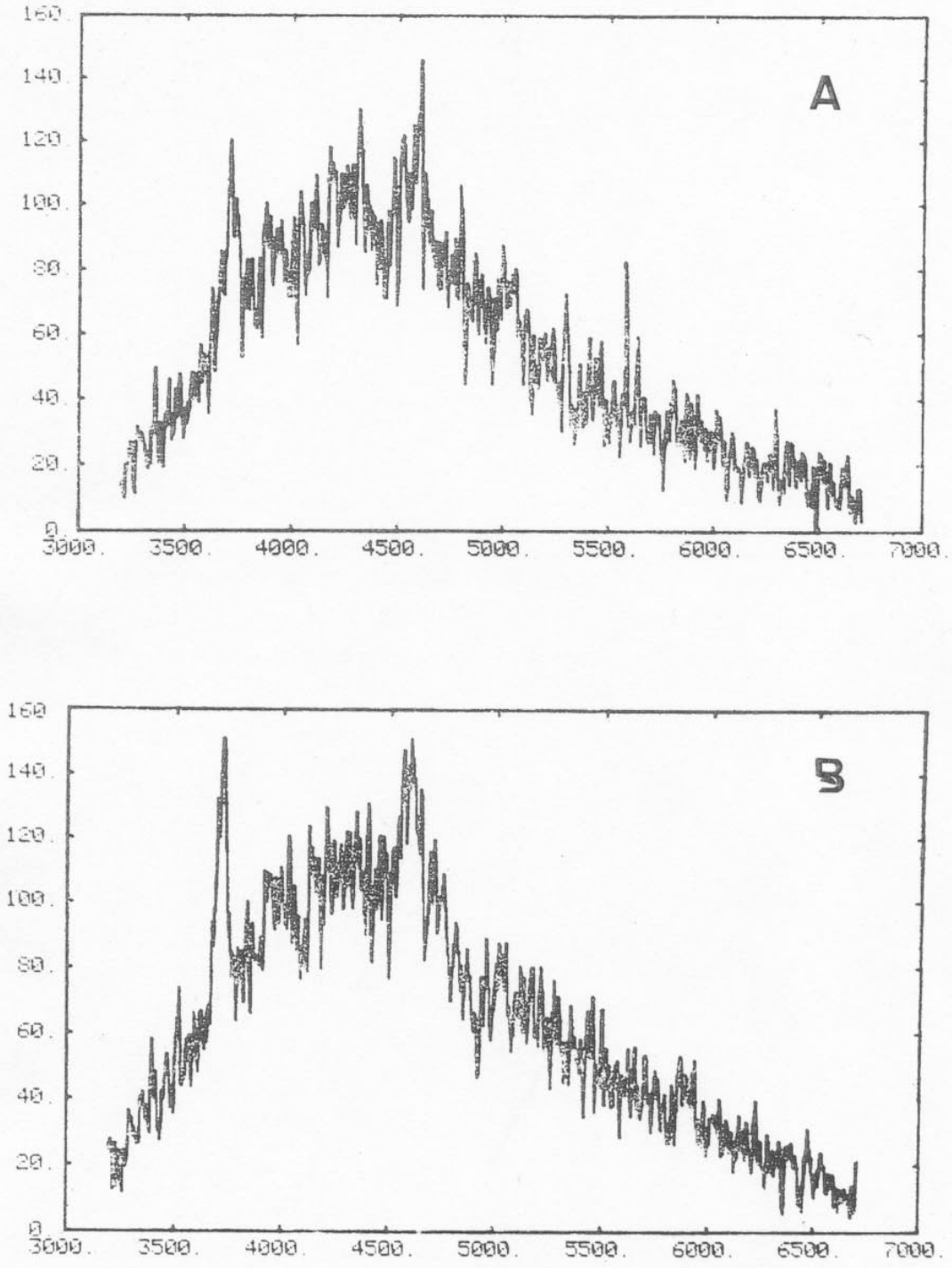

Fig. 5. Upper: First spectrum obtained for 0957+561A at the 2.1-meter telescope, 29 March 1979. Lower: First spectrum obtained for 0957+561B, a few minutes later.

same two emission lines. Same redshift. Clearly, we'd made a mistake and had set on the same object twice. So we went back and made sure. We went back and forth a few times before we were quite certain that these two objects had the same redshift. We did all kinds of tests for light scattering and seeing. There was no contamination of one spectrum by the other. We had two QSO's with the same redshift. We got very excited and thought about this. We did not think right away that it was a gravitational lens. Anybody now finding two objects at similar redshift calls it a gravitational lens, but you didn't do it so readily in those days.

What other observations could we make? Meanwhile, we were steaming along, with the long list of QSO candidates to go through: that's what we'd come to do. While we were exposing these, we were discussing other desirable observations. Clearly, one important observation was a deep image with good resolution. How would we do that? Well, if you go backwards a year to 1978, Ian Browne and I had used the Soviet 6-meter telescope to take deep images of empty fields (empty on the Palomar Sky Survey prints). We had quite a lot of time on the 6-meter but not much success because we had terrible weather and one or two problems with the emulsions. I was keen to do more of it, but with the emulsions available, we were very limited. And so I'd arranged (after a lot of bureaucratic negotiations) for a McMullen camera, an electronographic camera from RGO, to go out to the 6-meter telescope. McMullen himself went there earlier in 1979 to check the interfacing arrangements and so forth, and indeed the camera went out there in the spring. The allocation time at the 6-meter had coincided with the allocation of time at Kitt Peak. I went to Kitt Peak, and Ian Browne had gone out to the 6-meter. We were observing simultaneously, in the same dark run. We wanted a deep exposure, of high resolution; it was perfect. So at 2 a.m. in the morning of 30 March 1979, on our first night at Kitt Peak, I phoned Sir Bernard at Jodrell Bank and said, "Could you get a message through to Ian Browne at the 6-meter to tell them to look at this field?" Sir Bernard sent the telegram off, but they had not received the telegram at the 6-meter when Ian left 10 days later. It never worked, those are the breaks.

Then, the next day, two interesting things happened. First of all, I phoned Derek Wills and told him about it. I'll come back to this later. Second, we talked with Ray Weymann at Kitt Peak. Bob Carswell and Ray Weymann had been long-time collaborators. He was very interested, for two reasons. He had just come up to Kitt Peak because a single night had become available at short notice on the Steward 2.3-meter telescope. I guess that was another lucky break. Ray was doing a study of intermediate dispersion of carbon IV absorption lines in intermediate redshift QSO's, which is exactly the redshift range we were talking about, namely,

1.4. So he was very keen to observe this, and he observed it that night, his one night on the 2.3-meter telescope. It was a superb night, at 1 arcsec seeing, everything was perfect. We got better spectra at the 2.1-meter at low resolution than we got on the previous night. Ray was using an intensified photographic technique, and some time in the early hours of the morning, he phoned to say that he had similar absorption features in both objects. This was the first time that a close pair of QSO's had redshifts so close to each other. Now, we had the first case of two QSO's with common absorption features. Bob went down to the 2.3-meter, while I kept the show running at the 2.1-meter, and came back and said that the absorption features were there. Ray drove back to Tucson as soon as he finished observing.

Meanwhile, Bob and I were still measuring redshifts for a lot more QSO's. The next evening while we were having dinner, a telephone call came from Ray in Tucson that Bob took: he came back and said that Ray had measured the spectra accurately, and the redshifts of the QSO's in the absorption features were as identical as they could be. It looked like we had a gravitational lens. That was the first time that I recall those words being used. I think that it was probably Ray Weymann, who was the first to see the absorption spectra, who used them first. At this point, I had not yet seen the absorption spectra. However, I think that if we had not obtained the absorption spectra, we would have been very hesitant about suggesting we had found the first example of lensing.

Let me move on to other things. In the field, we've got two objects with the same redshifts; maybe there were others. We weren't thinking that maybe there were more than two gravitationally lensed images, though it would be nice to rationalize it in retrospect and say that. We were going to search around. So we took mostly bright stellar objects and took five minute exposures, or something like that. Sure enough, they were all stars. So there were only two objects with this redshift.

That summarizes what we knew when we left Kitt Peak. We quickly drafted our first paper, and the rest of the story is published. I remember well the mixed reaction it received, from immediate conviction to a great wall of disbelief.

One remaining part of the story has not appeared fully in print, although Malcolm Smith (1981) did tell part of it. I said earlier that I'd been doing collaborative work with Derek and Bev Wills; on my way to Kitt Peak in 1979, I passed through Austin to finish off a paper with them, and I talked about what I was going to be doing, and I showed Derek this finding chart of

0957+561 and said, "There are two stellar objects here. What do you think these are?" Derek said, "They're stars." Derek, as some of you may know, had done a very systematic search for pairs of QSO's, he'd looked at about 100 bright QSO's, expecting to find some pairs and found none. So it seemed that there were no pairs, no close pairs. So I made a bet with Derek. I said, "Look, these are very blue, I don't think they're stars," and I wrote on his blackboard, "No QSO's, I pay Derek 25 cents. One QSO, he pays me 25 cents. Two QSO's, he pays me a dollar." It was on the tip of my tongue to say, but I didn't say it, because it sounded facetious, "Two QSO's, same redshift, he pays me $100." When I phoned Derek the next morning and told him what we had found, we laughed and I said, "You owe me a dollar. Suppose I had said to you, 'Two QSO's, same redshift, $100,' would you have taken it?" He said, "Of course." So I lost $99, and I kept a friend. Well, he didn't pay up immediately. Derek and Bev went and looked at them on the 107-inch. A couple of months later, he sent copies of much better spectra than we'd taken, with a silver dollar fastened to them with scotch tape. I kept the silver dollar for many years; unfortunately, I somehow managed to lose it. But it served me very well: I had four teenage sons, none of them particularly interested in science. So when they asked me, "What good is this gravitational lens?" I was able to say, "Well, I made money out of it."

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