

Chapter 2

Quasars in the Life of Astronomers

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In this chapter, we collect reminiscences and experiences about the early days of quasar astronomy. The recollections of two of the major pioneers who have recently passed away, Geoffrey Burbidge and Allan Sandage, can be found in two Annual Review papers [38, 197]. Others have given their experiences at earlier times [34, 201]. Our contributors involve mostly astronomers who entered the field around or in the decade after the time of the discovery, living both in different Western countries and in the Soviet Union. These authors provide insight into the state of experimental and theoretical astrophysics largely in the period from 1965 to 1980. Questions addressed include early developments in detectors (the pre-CCD times). Specific goals were to discover when it was realized that Seyfert galaxies and quasars were part of the same phenomenon as well as when the black hole—accretion disk paradigm became widely accepted. We also try to catch the flavor of the debate about the nature of the redshift which was provoked by the discovery of the quasars. The first sections consider the discovery of quasars. They are intended to reflect the state of extragalactic astrophysics in the early 1960s with a focus on the hottest issues and the instrumental and computing capabilities of the time. Questions will deal with the early development of radio astronomy, the unexpected discovery of hyper-luminous radio galaxies, and subsequently, extragalactic X-ray emission. There are some questions about early assessments of the physical problems entailed with the discovery of quasars that eventually led the standard paradigm and unification.

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2.1 Quasars seen from Europe

Dear Suzy (Collin), what attracted you to this research field? When you entered the field, did you find a paradigm (e.g., supermassive black hole, accretion disk, obscuring torus) already fixed in place? If not, what ideas about, and interpretations of, the quasar phenomenon were in vogue?

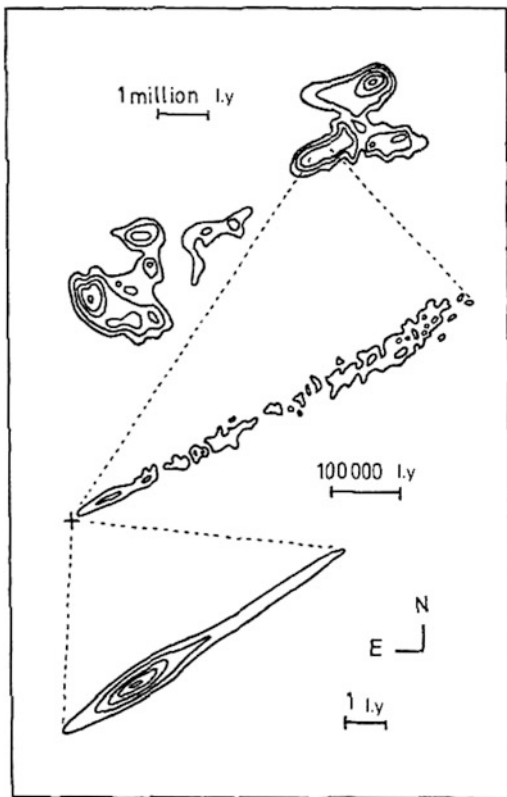
I began to work on Seyfert galaxies in 1965, 2 years after the discovery of the first quasars. I was previously preparing a thesis on solar flares—thesis could take up to 8 or 10 years in France in the 1960s—when I discovered the paper by Greenstein and Schmidt [101] where they concluded that the only possible explanation for the redshifts of the recently discovered quasars 3C 273 and 3C 48 was the Hubble law, which led to attribute them the power of hundreds of galaxies. And almost at the same time, came the discovery that 3C 273 was variable in timescales of weeks! I decided to change the subject of my thesis (my adviser, Evry Schatzman, often incited his students to follow their own preferences). I chose quasars because, having worked a little on solar flares, I was attracted by energetic phenomena. I was also aware of the papers on radio galaxies by Burbidge [37] who noticed, on the basis of simple considerations of energy equipartition, that the intensity of synchrotron radiation implied an enormous amount of energy in magnetic field and relativistic particles, corresponding to the transformation of 10^6 – $10^8 M_\odot$ into radiation for instance in Cygnus A (the uncertainty is due to the fact that one does not know how much energy in protons is associated with relativistic electrons).¹ I have the impression that very few people realized the importance of this result.

Finally, I preferred to work on the galaxies discovered by Carl Seyfert 20 years before [209] which were much less “fashionable” than quasars in 1965. Few weeks before the discovery of quasars, Burbidge et al. [40] have indeed published a long paper entitled “Evidence for the Occurrence of Violent Events in the Nuclei of Galaxies” which was a prefiguration of quasars from the energetic point of view. Thus, after the quasar’s discovery, Seyfert galactic nuclei were immediately suspected to have some relation with them because of their bright blue nucleus and intense broad spectral lines. It was quite premonitory since after all, quasars were proved to be active galactic nuclei (AGNs as we call them now) only when the first “host galaxies” were unambiguously identified with large telescopes in the 1980s. For radio galaxies, one can consider that their relation with nuclei of galaxies was demonstrated only by the first observation of the jet linking the compact radio nucleus to the big radio lobes in NGC 6251 [187] (cf. Fig. 2.1).

When I began to work on the subject, there were almost no studies of Seyfert nuclei since that of Seyfert himself, except two by Woltjer [237] and by Burbidge et al. [36] published in the same ApJ issue, whose conclusions were in total conflict. Burbidge et al. studied the rotation curve of the Seyfert galaxy NGC 1068 and

¹Actually, he underestimated the energy by an order of magnitude because he used an old value of the Hubble constant.

Fig. 2.1 This radio map of NGC 6251, as published by [187], shows that a small jet of 5 light-years long located in the galactic nucleus is aligned with a larger jet of 600,000 light-years, itself aligned with the direction of the radio lobes, separated by 9 million light-years



deduced that the gas giving rise to the emission lines should be ejected from the nucleus. While Woltjer, discussing the properties of the line emission in six Seyfert galaxies, concluded that it should be gravitationally confined by a massive body. Woltjer was right, but with wrong arguments, because he overestimated the mass of the ejected gas (which he assumed to be homogeneous) and concluded that it could not be ejected unless the whole galaxy would have been emptied rapidly. Burbidge et al. were right because NGC 1068 is a peculiar Seyfert (we call it now a Seyfert-2), whose emission lines are formed at a much larger distance than in the other Seyfert galaxies and are partly outflowing, while the others were mainly what we call now Seyfert-1 galaxies. From the rotation curve, Burbidge et al. deduced a central mass of $3 \times 10^{10} M_{\odot}$ which was basically correct, corresponding to an escape velocity of 450 km s^{-1} , and since the lines indicated a velocity spread of $1,450 \text{ km s}^{-1}$, the gas was clearly not gravitationally bound. I think it is unfortunate that Burbidge et al. were studying just this galaxy, which led them to conclude that what occurs in Seyfert nuclei is related with the phenomenon taking place in radio galaxies, that is, ejection of plasma and relativistic particles, and to generalize this result. It was the first aspect of the controversy on “anomalous redshifts” which lasted for 20 years (and is even lasting now).

I observed the most brilliant Seyfert nuclei at the Haute-Provence Observatory, with the help of Dr. Yvette Andrillat who was specialized in the observations of novae and planetary nebulae. We used an old 120-cm Cassegrain telescope, to which were attached several low-dispersion spectrographs from the near UV (3800 Å) to the infrared (9000 Å), necessary for our study. We used photographic plates because at this time, there was only one electronic device in France, the so-called “electronic camera” which could be used only for relatively bright objects like the M31 nucleus [133]. We had to go through a complex reduction process, implying in particular the measure of a comparison star located on the sky close to the galaxy, whose absolute flux as a function of wavelength was known. Moreover, since the sensibility of the detectors was so low, the exposure times were very long in order to get not too noisy spectra, and it was a real challenge to maintain by eye the slit exactly on the nucleus. All reductions were made with pencil and paper, and the equivalent widths of the lines were measured with a planimeter.

In 1968, when I published my thesis (in French, as it was common at this time [216] and [217]), a paper appeared also on Seyfert nuclei by Dibai and Pronik [70]. We came to the same conclusion that there were two different emission regions, one made of dense and small clouds emitting broad lines (the “broad-line region” or BLR as it was called), and one made of dilute and large clouds emitting narrow lines (the “narrow-line region” or NLR). The “narrow lines” were actually mainly forbidden lines, like the [OIII] lines at 4959–5007 Å.² Their presence implies that the medium is very dilute and therefore extended. The second type of spectral lines (like the Balmer or the Lyman series of hydrogen) are “permitted”, because their upper level has a short lifetime, and line photons are very easily emitted. As already mentioned, the line widths in Seyfert nuclei were (and are still now) attributed to Doppler motions. But the permitted and the forbidden lines are not broadened similarly, except in NGC 1068. The widths of the forbidden lines correspond to velocities of a few hundreds km s⁻¹, while those of the permitted lines correspond to velocities of a few thousands km s⁻¹. It means that they come from two different regions, which must have different densities: in the region emitting the forbidden lines, the density must be smaller than 10⁶ cm⁻³, and in the region emitting the permitted lines, the density must be larger than 10⁶ particles cm⁻³ to explain why no forbidden lines are observed (from the study of the line intensities, one deduces that it is in fact at least 10¹⁰ cm⁻³). In the 1970s, one begun to make a distinction between two types of Seyfert nuclei: type-1 Seyfert, with both broad and narrow lines, and type-2 Seyfert, with only narrow lines.

From the temperature found in the dilute clouds, I deduced that they were photoionized by an intense UV continuum, contrary to the widely accepted opinion

²When they were discovered in planetary nebulae at the beginning of the twentieth century, these lines were attributed to an unknown element called “nebulium”—not expected by Mendeleiev table!—and their nature was understood only 30 years later. The same story happened for a line observed during solar eclipses and attributed to an unknown element called “coronium”; it is actually also a forbidden line, this time of highly ionized iron, also present in the spectrum of Seyfert galaxies.

that they were heated and ionized by suprathermal particles (as proposed by [168]). Besides, Andrillat and myself [3] found evidences for variations of the profile of the broad $H\beta$ line in the nucleus of NGC 3615 since Seyfert's 1943 observations. Owing to the difficulty of the measurements (in particular we feared to have moved the slit during long observations and to have therefore observed circumnuclear regions), we went on observing carefully this nucleus during several years, and we confirmed the variation in a timescale of a few years, but I think that the result was not accepted before other people confirmed our finding (though it was quite predictable, owing to the small size of the emitting region). I must add that I wrote to the referee of our paper that "if we were not women and French, he would have accepted our paper without discussion!" One knows that such variations have been extensively monitored since 1985 within the "AGN Watch", and have led to the determination of the masses of about forty black holes in active nuclei. It is why I am always paying attention to ideas which are not in the main stream.

During the 1960s, there was no single paradigm concerning the origin of the energy radiated by quasars. On the contrary, many explanations were proposed, and they almost all involved stars: front collisions of stars with high velocity, chain explosions of supernovae, "flares" at the galactic scale, etc. The most popular was a "supermassive star" energized by nuclear reactions or by pulsations leading to gravitational release. The reason was certainly because stellar studies had been very successful during the previous decade, and some of their eminent promoters (the Burbidges, Hoyle, Fowler) were active in the quasar and AGN field. This was illustrated during the meeting held in 1971 in Cambridge for the 60th birthday of Hoyle, "supermassive objects," which was devoted mainly to quasars. After the discovery of the first pulsars in 1968, "supermassive rotators" were also privileged, because massive stars are highly unstable and can be stabilized by rotation. However some people have guessed immediately the correct explanation: Salpeter [194] and Zel'dovich [240] suggested independently that a massive black hole (MBH) was present in these objects, and Salpeter proposed that the matter and angular momentum was transported, thanks to a turbulent viscosity (as one knows, this is the presently most accepted view). But nobody took this idea seriously, except general relativity specialists. Lynden-Bell was the first to reiterate their suggestion [138], and again, the year after at the famous Vatican conference on "Nuclei of Galaxies."³ Apparently, almost none of the 25 well-known astronomers that were attending it paid due attention to this model. Instead, most were attracted by "non-cosmological redshifts", which were still largely discussed at this time.

You attended the first focussed meeting on Seyfert galaxies held at the Steward Observatory in 1968. What kinds of observations and ideas were presented at that conference? Was it already established that Seyfert galaxies and quasars were different luminosity manifestations of the same phenomenon?

³These meetings in the Vatican take place from time to time, and it was the first devoted to astrophysics. They gather a few of the best scientists in a given science, to discuss some fundamental issue on which experts have contradictory opinions.

I attended the meeting on “Seyfert nuclei and related objects” in Tucson (1968), which was indeed the first one on Seyfert nuclei. “Related objects” were actually mainly quasars and radio galaxies (at this time, often called “N-galaxies”). There were already several books published on quasars and radio galaxies, but none on Seyfert nuclei. When reading the introduction (by Minkowski) and the conclusions (by Woltjer and Osterbrock), I am amazed of how many things were not understood in spite of the available observations, but of course, these were limited to radio and to the optical and near-infrared bands. The meeting was mainly observational, with only a few theoretical papers. In particular, the idea that the ionization and heating of the gas could be due to UV radiation was stressed by myself and by Williams and Weymann. The following topics were among those debated in Tucson:

- It was not clear at all that quasars were galactic nuclei like the Seyfert ones, owing to the enormous gap in luminosity between the two classes (Barnothy’s suggestion that quasars could be gravitationally lensed, Seyfert nuclei was statistically not tenable). This is strange since Schmidt [200] had already proved that quasars undergo a big cosmological evolution in luminosity and/or in number, but this result was not extended to the present time. We think now that there is a continuous transition between quasars and Seyfert galaxies along the cosmic time, Seyfert galaxies being very low luminosity quasars, the only ones which can be activated presently.
- Concerning Seyfert nuclei, the difference between Seyfert-1 and 2 was not pointed out (and a fortiori not understood). It was thus difficult to understand the diversity of the spectra and the line broadening (which was attributed also to electron scattering).
- Synchrotron radiation was assumed to be responsible for the blue continuum of Seyfert galaxies and radio-quiet quasars, presumably by analogy with radio galaxies. This idea prevailed during almost 20 years, and it certainly did much damage to the black hole model. It was dismissed by Shields [212] who proposed instead that the radiation was of thermal origin, due to the accretion onto the black hole.
- Coronal lines were attributed to a very hot gas, by analogy with the solar corona. It is mostly believed now that they are formed in a warm gas photoionized by X-rays, but at this time, one could certainly not imagine that the bolometric luminosity could be dominated by a “big blue bump.”
- A density larger than 10^8 cm^{-3} for the BLR was not accepted, and this is why a long discussion (without any solution) was devoted to the “Balmer decrement” problem. One knows now that the large Balmer decrement, as well as the small $L\alpha/H\beta$ ratio put into evidence by Baldwin [20], are due to the combined effects of a large optical thickness and a high density (larger than 10^{10} cm^{-3} cf. Netzer [160] and subsequent papers).
- The problem of the affiliation of several classes of objects to Seyfert nuclei was not clear at all (and one understands now why, in particular thanks to the Unified Scheme).
- The bulk of the luminosity was assumed to be in the near infrared.
- etc, etc...

Clearly, at the time of the Tucson meeting, there was a lack of key observations, in particular in the UV and X-ray range, and also a great need for a theoretical model. Then, during the 10 following years, progress was relatively slow, as is always the case when a theoretical background is not ready at the time of a discovery (contrary for instance to what happened for pulsars or for the cosmic radiation), and when the observations are sparse and not statistically significant. The breakthrough came only at the end of the 1970s, when Rees proposed an astrophysical context for the MBH model with his “flow chart” [185]. Then immediately after, “accretion onto a massive black hole” was widely accepted and became a paradigm.

Thank you Suzy for your recollection of the debate that took place soon after quasar discovery. We now would like to hear the point of view of a radio astronomer about the early developments that were pivotal for that discovery.

2.2 Quasars in the Early Radio Sky

Dear Giancarlo (Setti), please share with us your impressions about the first years after the discovery of quasars.

The 1960s will be remembered as the mirabilis decade of astronomy for the string of major discoveries that took place at an impressive rate (extrasolar X-ray astronomy, quasars, the CMB, and pulsars), a revolution in cosmology, and the foundation of high-energy astrophysics. Second, in the string of discoveries, the quasars immediately attracted much attention not only among astronomers but also in the physics community and in the media, to the extent that the name became part of the common language. On this last point, I recall the headlines on newspapers reporting the suggestion of some Russian radio astronomers that the strong variability on short timescales of the compact radio source CTA 102, then identified with a quasar ($z \sim 1$), could be evidence of the broadcasting station of a very advanced extraterrestrial civilization. As a young researcher attending the 2nd Texas Symposium on Relativistic Astrophysics (Austin, December 1964), I was impressed and almost scared by the extremely qualified list of participants including, among a large number of well-known physicists and astronomers, Paul Dirac, a Nobel laureate in physics. A large part of the symposium was dedicated to the radio galaxies and quasars of which only a handful were known, and so everybody was eagerly waiting for new observations which could possibly constrain the field of theoretical speculations. Obviously, the nature of the redshifts, was central to the discussion. I was particularly impressed by the lucid exposition of Maarten Schmidt, based on a paper [101] just published in *The Astrophysical Journal* in collaboration with Jesse Greenstein of Caltech. From the analysis of the optical spectra of the quasars 3C 48 and 3C 273, he concluded that the gravitational origin of the redshifts was not viable because the differential redshifts would have washed out the lines and proposed, in the framework of the cosmological hypothesis, a simple quasar model consistent with all available data as an HII region of parsec

Fifty Years of Quasars

From Early Observations and Ideas to Future Research

D'Onofrio, M.; Marziani, P.; Sulentic, J.W. (Eds.)

2012, XXXII, 584 p., Hardcover

ISBN: 978-3-642-27563-0