

QUASARS AND THE CALTECH-CARNEGIE CONNECTION

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Abstract: A collaborative relationship existed between the California Institute of Technology (Caltech) and the Carnegie Institution of Washington (Carnegie) beginning in 1946, when a formal agreement was signed between the two groups of trustees. This agreement was designed to integrate Mount Wilson Observatory and the new unfinished Palomar Observatory into a single scientific entity. During the period from 1946 to 1979, much astronomical research was done at both institutions as a direct result of this collaboration. Part of this research included the first identification of a radio source with an apparently stellar object by Allan Sandage of Carnegie and Thomas Matthews of Caltech in 1960, and the first identification of spectral lines at large redshift from a radio source associated with such an object by Maarten Schmidt of Caltech in 1963. This paper examines how the discovery of these objects—which came to be known as quasars—and subsequent research on them, indirectly had an impact on the relationship between Caltech and Carnegie by leading to an environment of increased competitiveness that eventually resulted in the formal dissolution of the relationship in 1980. In this paper, the controversy surrounding the discovery and the interpretation of quasars is examined to provide further understanding about the working relationship when the two institutions were formally collaborating. Some of the data used in this paper were drawn from personal correspondence and interviews with the researchers themselves, and this research forms part of a dissertation for a Ph.D. degree in the Centre for Astronomy at James Cook University, Townsville, Australia.

Keywords: quasars, Carnegie Institution of Washington, California Institute of Technology, 200-inch Palomar Telescope, Owens Valley Radio Observatory, J. Greenstein, T. Matthews, A. Sandage, M. Schmidt

1 THE CARNEGIE INSTITUTION OF WASHINGTON AND ITS OBSERVATORIES

1.1 The Beginning

The Carnegie Institution of Washington was founded on 4 January 1902 when its Articles of Incorporation were signed. The institution was reincorporated by an act of the Congress of the United States, approved 28 April 1904, under the title of the *Carnegie Institution of Washington* (Carnegie Year Book No. 47, 1948: xi). Andrew Carnegie, a multi-millionaire steel baron and philanthropist, financed the institution with an endowment of registered bonds with a par value of ten million dollars, in order "... to encourage, in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the improvement of mankind." (ibid.). Mr Carnegie made an additional contribution of two million dollars to this fund on 10 December 1907, and he contributed a further ten million dollars on 19 January 1911 (ibid.).

Carnegie gave the Board of Trustees "... full power to decide how the institution would meet its mandate, and even to amend his mandate ..." (Sandage, 2004: 30). Accordingly, the Board selected a seven-man Executive Committee to formulate research methods in a variety of fields, and these were presented to the Board from time to time. The first move of the Executive Committee was "... to canvass the state of knowledge in seventeen different fields of human endeavor ..." (ibid.), and to select leaders in each field to form Advisory Committees, which would write position papers outlining where major advances were likely to be made in their respective disciplines. Edward C. Pickering, Director of the Harvard College Observatory, was appointed Chairman of the Advisory Committee for Astronomy.

In 1904, George Ellery Hale (Figure 1), seeking clearer skies than existed near Chicago, obtained support from the Carnegie Institution to found the Mount Wilson Solar Observatory in the mountains near Pasadena, California. Hale, who had invented the spectroheliograph and discovered solar magnetism, wanted to understand the physics of the Sun and stars.

In pursuit of this goal, the initial complement of solar telescopes at Mount Wilson was followed by the 60-inch Reflector and then the 100-inch Hooker Telescope, which was the largest in the world at the time of its construction (Carnegie Observatories, 2006). Hale's motivation came from an enduring goal "... to solve the problem of stellar evolution." (Sandage, 2006a).

The Observatories of the Carnegie Institution at Mount Wilson transformed astronomy and astrophysics with a succession of major breakthroughs, including Harlow Shapley's mapping of the globular clusters of our Galaxy, Edwin Hubble's extragalactic studies and his redshift-distance relation, and Walter Baade's recognition of stellar populations (ibid.).



Figure 1: George Ellery Hale shown here in his office at Mt Wilson Observatory. This photograph dates to about 1905 (from <http://www.mwoa.org/hale.html>).

From the success of the Mt Wilson telescopes, Hale was determined to build a 200-inch or even larger telescope that would enable astronomers to see farther into space and to attack problems ranging from the structure of the Universe to the evolution of stars and the composition of stellar matter (Goodstein, 1991). In February 1928 Hale asked the editor of *Harper's* to send an advance copy of "The Possibilities of Large Telescopes", which he had written, to Wickliffe Rose,

the General Education Board President at the Rockefeller Foundation. When Hale called on Rose on 14 March, Rose asked him, "Do you want a 200-inch or a 300-inch?" Hale replied "A 200-inch telescope." (ibid.). Rose wanted to put the proposed telescope into the hands of a school, not the Carnegie Institution or the National Academy of Sciences, as Hale had initially proposed. Rose's suggestion that Caltech would make better use of the new telescope if it belonged to them infuriated John Merriam, Carnegie Institution President. This hostility meant that no real progress on a joint Caltech-Carnegie astrophysics program was likely while Merriam remained in office. Nevertheless, Merriam changed his mind, and in the fall of 1928 the International Education Board of the Rockefeller Foundation gave the green light to Hale's \$6 million proposal. This pledge, for which responsibility was later assumed by the General Education Board and which was supplemented by funds from the Rockefeller Foundation, was made to Caltech, of which Hale was a trustee.



Figure 2: Ira Sprague Bowen was Director of the Mount Wilson and Palomar Observatories from 1948 to 1964, and oversaw the completion of the 200-inch Hale Telescope and the 48-inch Schmidt Telescope (from <http://www.oss.org/bios/fellows-bowen.html>).



Figure 3: Robert Bacher joined Caltech in 1949 and remained there for the rest of his career, serving as Chair of Physics, Mathematics, and Astronomy from 1949 to 1962, and as Caltech Provost from 1962 to 1969, and Vice-President and Provost from 1969 to 1970 (from http://en.wikipedia.org/Robert_Bacher).

1.2 Administration

In the fall of 1928, the Observatory Council, with Hale as Chairman, was formed to direct the planning, construction and operation of the 200-inch Telescope. Hale assembled the team of scientists and engineers to build the 200-inch Telescope, choosing John Anderson, a Mount Wilson astronomer, as the Executive Director (Goodstein, 1991: 221). The site was to be on Palomar Mountain, southeast of Los Angeles. This site was chosen to enable very long exposures at the limit of the telescope's reach, which Hale acknowledged might not be possible at Mount Wilson because of the illumination of the night sky from the sprawling development of Los Angeles (Florence, 1995). Title to the Palomar Telescope was given to Caltech, which joined with Carnegie to form the Mount Wilson and Palomar Observatories (Caltech, 1951).

The man picked to head the Mount Wilson and Palomar Observatories was Caltech Professor of Physics, Ira Sprague Bowen (Figure 2), who held the position from 1946 to 1964. Bacher (1981) has credited Bowen with making the 200-inch the best telescope in the world at the time. Equally important was the fact that the joint operation of the two staffs worked well under Bowen's tenure. This success was attributed to a mix of subtlety and power in his personality, coupled with good scientific judgment and wise decision-making in administration (Sandage, 2004). Because of Bowen's outstanding credentials, the Carnegie Institution was willing to allocate up to three million dollars of endowment for the Telescope, and this was to be given as either a single grant or as a series of endowments (see Florence, 1995).

The administration of the two Observatories was affected through an Observatory Committee which comprised Bowen (as Director), Robert Bacher (the Chairman of the Division of Physics, Mathematics and Astronomy at Caltech), plus two additional members from the Observatory and two from Caltech. When Bowen became Director of the Observatories, he also became an employee of the Carnegie Institution, and perhaps this was a contributing factor to "... the observatory problems that developed between Caltech and the Carnegie Institution." (Bacher, 1981).

Robert Bacher (Figure 3) was Caltech's first Provost, from 1962 to 1969, and when asked if there were any problems in administering Palomar he responded as follows:

You know, the two Observatories have now separated. I have a certain sadness over this, because there were forces in this direction even during the period in which I was Provost, and I tried very hard to put the thing together in a way that would work better. But the forces toward separation became very large. When I came out here, one of the ways the operation was carried out was that there was an Observatory Committee and two *ex-officio* members—Bowen as Director of the Observatories, and myself as Chairman of the Division. At that time, I think, there were two additional members from the Observatory and two from Caltech. I used to talk to Bowen a great deal about the fact that we should talk about the research planning in the Observatory Committee, but Bowen never liked to do it that way. He was glad to talk to me about it, but he didn't really like to get into it in a meeting of that sort. And the Observatory Committee became a committee that sort of put the rubber stamp on things to be done and particularly

supervised the allocation of Observatory time. (Bacher, 1981).

Bacher's comment that the Observatory Committee was never used to plan the research programs indicates that at this senior administrative level there was a basic lack of communication between Caltech and Carnegie. Consequently, the concerns of both institutions were never properly addressed, and "... the problems between Caltech and the Carnegie Institution ... became worse as the years went by." (ibid.).

Another interesting comment from Bacher was that he and Bowen got along very well together, except when it came to staffing appointments:

The only problem I ever had with Bowen was that he hated to act on any appointments at Caltech in astronomy. He was responsible, not I, for the research carried on at Caltech in astronomy. Things having to do with teaching reported through the Division, and things that had to do with research reported through the Observatory. But if somebody had to be appointed, connected with research and so on, he'd always say, "Well you do it, you do it." [But] Overall we got along just fine. (ibid.).

Bowen's reluctance to make appointments was somewhat disconcerting to Bacher, and the problem manifested itself later in conflicts which were to have serious repercussions (as reported below).

1.3 Conflict

Despite the agreement between Caltech and Carnegie regarding the equal right of access to all the equipment on either mountain, a letter written in 1969 by Jesse Greenstein (Figure 4) to Allan Sandage (Figure 5) indicated that there were problems:

Your letter brought up anxieties about the relations between the two Institutions. I might feel them also, but I believe it is important to act as if there were no important problems which we could not solve by mutual agreement. Most certainly there are real problems, and they are not all one-sided. We are doing our best to keep our cool, and to work out a rational arrangement with mutual respect. I have completely disinvolved myself in any CARSO [Carnegie Southern Observatory] activities from the beginning; I have been involved in attempts to foster better planning for all of Caltech astronomy, and for the future of Palomar, the possibility of a search for a new location ... (Greenstein, 1969).

However, these relational problems already existed in 1965 when Jesse Greenstein wrote John Bolton that the use of the Caltech and Carnegie telescopes was a delicate issue that impacted on the relations between the radio astronomers and their optical counterparts:

I should point out to you that the question of the use of our telescopes for identification of radio sources and accurate optical positions has been one of the most delicate ones between relations of the radio observers, the optical observers and guest investigators. At the present time a precarious working arrangement exists in which John Wyndham is identifying the sources for which the Caltech Radio Observatory finds positions, quite on his own. Subsequent to the preparation of his manuscript these positions are made available to Sandage and Schmidt. Thus I should warn you that you will be coming into a fairly complicated situation. Sandage is taking direct photographs for accurate optical positions and doing the photoelectric photometry and Schmidt the redshifts. Consequently, where your new data might overlap any from Owens valley or

Green Bank you are going to have direct competition with Sandage. (Greenstein, 1965).



Figure 4: Jesse Greenstein, who collaborated with Maarten Schmidt on the interpretation of quasars in 1963, and helped to instigate the founding of Caltech's OVRO. He became foundation Head of the graduate astronomy program at Caltech at the time of the inauguration of the 200-inch Telescope and the joint operation of the Mt Wilson and Palomar Observatories (from <http://pr.caltech.edu/periodicals/336/articles/Volume%202/10-31-02/greenstein.html>).



Figure 5: Allan Sandage has been a staff member of the Carnegie Observatories since 1952, and has been involved in researching the evolution of stars, galaxies and the Universe (from <http://www.ociw.edu/research/sandage.html>).

By 1969, there were indications of a possible rift in the relationship between the two institutions, as suggested in the following letter from Olin C. Wilson to Horace Babcock.

... no one here, I feel sure, has the slightest desire to break up the arrangement for joint operation of the Observatories which began in 1948. If there is any interest in such a move it certainly does not come from the C.I.W. staff, but must have originated elsewhere.

But I find another aspect of the matter even more unsettling, namely, what do we mean by the partnership of C.I.W. and C.I.T. in the astronomy business? My understanding is that it consists of an agreement for joint operation and joint use of certain expensive equipment, for the mutual benefit of both partners, but

that in no way implies dominance by one nor the loss of identity and self-determination of either.

If this view is basically correct, then I interpret your statement to mean that one of the partners does not subscribe to it. It seems to me that one partner is attempting to use threats and coercion against the other in order to force the latter to spend a large sum of its own money in a manner it deems deleterious to its own interests. Personally, I feel that such methods have no place in the partnership in question, and are entirely unworthy of either of the members. (Wilson, 1969).

What Wilson appears to suggest is that Caltech was coercing Carnegie into spending money in a manner that was not in its best interests. At the time, Wilson was the person who allocated observing time on the Mt. Wilson and Palomar Telescopes while Babcock (Figure 6), the recipient of his letter, was the Director.



Figure 6: Horace Babcock invented and built many astronomical instruments, including a ruling engine which produced excellent diffraction gratings, the solar magnetograph and microphotometers, automatic guiders, and exposure meters for the 100-inch and 200-inch Telescopes. By combining his polarizing analyzer with the spectrograph he discovered magnetic fields in other stars. He developed important models of sunspots and their magnetism, and in 1953 he was the first to propose adaptive optics. He was the Director of the Mt. Wilson and Palomar Observatories from 1964 to 1978. During this time he founded the Las Campanas Observatory in Chile (from <http://www.phys-astro.sonoma.edu/BruceMedalists/BabcockHW/>).

In a recent interview, George Preston, the Director Emeritus at Carnegie, explained why there was conflict between the Caltech and Carnegie astronomers:

There was a profound asymmetry in the relationship between Carnegie and Caltech by 1980, because we had been contributing, since the end of World War II, to the aging outmoded telescopes on Mt. Wilson—the 60-inch and 100-inch reflectors—in a light-polluted site. Because the telescopes were old and because the site was polluted, nobody was much interested in investing money in them, and they were growing more antiquated and inadequate with every passing year. Caltech was interested in supporting Palomar at that time, and I think that Caltech astronomers felt that we were not pulling our own weight in the joint operation. We were

contributing aged telescopes in a light-polluted site that nobody wanted to use, and we were making demands for the much-coveted telescopes at Palomar (which were) bigger telescopes, more modern and in a darker sky. This led to a kind of estrangement and a feeling on the part of the Caltech astronomers that they were not getting their money's worth. They were giving telescope time and they were not getting anything back. (Preston, 2006).

It seems clear that even in the early 1960s Caltech and Carnegie had a somewhat precarious relationship, despite the contractual arrangement between the two institutions. This is similar to the way in which Maarten Schmidt saw the situation when he became Director more than a decade later.

When Schmidt (Figure 7) assumed the Directorship of the Hale Observatories in 1977, the 'Observatories' consisted of Palomar and the Big Bear Solar Observatory, and on the Carnegie side Mt. Wilson and Las Campanas in Chile. In an interview conducted in 1999, Schmidt commented that

... the relationship between Caltech and Carnegie concerning the observatories had not been overly good. And curiously enough, that didn't apply so much to the astronomers but more to the administrative levels. Jesse Greenstein certainly had his conflicts with the Carnegie administration. (Schmidt, 1999).

Schmidt acknowledged that part of the conflict stemmed from the fact that while the two halves of the Hale Observatories were financially and organizationally independent and the facilities were utilized jointly.

An additional operational difficulty was that the Caltech astronomers had undue influence over the appointment of Carnegie staff, and *vice versa*:

If the Caltech group proposed that a potential faculty appointee become a staff member of the Hale Observatories, that then had to be approved by the Observatory Committee, which consisted half of Carnegie and half of Caltech astronomers. So that meant that the Carnegie side was able to influence, or bias, or perhaps even veto, or make difficult, Caltech's academic appointments. (*ibid.*).

In October 1979 an appointment by the Carnegie side was rejected by the Caltech astronomers, and because of the bitterness that resulted Schmidt felt that the system was not working, so in his capacity as the Director he wrote a letter to the Carnegie and Caltech Presidents proposing that the operational agreement between the two institutions—which had existed since 1948 and been amended several times—should be terminated (*ibid.*). At the same time he tendered his resignation, effective from 1 July 1980 (i.e. in nine months time).

According to Schmidt (*ibid.*), telescope accessibility was not the issue. The problem seemed to be the organizational structure that created awkward relationships that could have devastating decision-making implications. Apparently, Carnegie President, James Ebert, and Caltech President, Marvin L. Goldberger, were very surprised by Schmidt's letter. As it turned out, the Carnegie side opposed the separation, while the Caltech side supported it. In Schmidt's opinion, Carnegie felt that part of their strength was in a solid union with Caltech in astronomy, while access to the 200-inch Palomar Telescope might be jeopardized by separation. However, physicists at Caltech involved in

astronomy could have their appointments influenced by Carnegie, which did not seem right in Schmidt's view because "... if anybody influences our appointments, it ought to be the physicists and the mathematicians, with whom we are joined." (ibid).

Schmidt elaborated on the reasons for terminating the agreement between the two institutions in the 1979 *Year Book* of the Carnegie Institution:

Problems manifested themselves in particular on the occasion of staff appointments. When one of the institutions would consider the appointment of an optical astronomer, the Observatory Committee would evaluate the person in parallel for appointment as a staff member of the Hale Observatories. This procedure was a potential source of conflict, since a President would get recommendations for a given appointment from both his institution's faculty and from the Observatory Committee. In practice, this resulted in administrative interference by one institution into the affairs of the other institution (Schmidt, 1980).

Schmidt elaborated on this during an interview conducted on 28 July 2006:

The Hale Observatories had a staff. One was a member of the staff of the Hale Observatories, so appointments were made to it and they would be very naturally accepted by both sides so long as it was about people of the professorial faculty right here in astronomy, and on the Carnegie side appointments of the permanent staff over there. It was indeed true that when that letter was written [to the two Presidents] we had been through a period of disagreement about a particular proposal. This staff membership was a curious one and most of the difficulties would arise if somebody from elsewhere in Caltech or at positions that were not entirely full-blown observing astronomers came up. It was sort of in that nature. Now if it had been only that, I don't think that the situation would have developed the way that I proposed. The reason that we had this arrangement with Carnegie was that the Rockefeller Boards in the early thirties, in deciding to give money for the 200-inch Telescope, upon George Ellery Hale's proposal awarded it to Caltech rather than to Carnegie. So what happened was that the Telescope was essentially given to Caltech ... and the understanding was that the two would go together, Carnegie and Caltech, in managing the place. The arrangement with Carnegie was accepted by the Caltech administration (and) was made without input from its astronomy staff, since there was none. (Schmidt, 2006).

Clearly, there were distinctive reasons for the way the Caltech-Carnegie collaboration was established, but one can imagine the reaction from the Carnegie staff when the 200-inch Telescope was effectively given to Caltech, a university that at the time had no astronomy staff members whatsoever. Later, this organizational structure was part of the reason for the rising dissonance mentioned in the foregoing quotes, and Schmidt's reference to "interference" (in 1980), says a great deal about why problems developed at an administrative level.

In spite of the organizational tension, important scientific work was carried by the Caltech and Carnegie astronomers during the 1960s. A particularly impressive collaboration involved the discovery of quasars, and this is discussed in the following Section of this paper. Yet even this research was not without its share of controversy and conflict, which may have contributed—at least in part—to the dissolution of the Caltech-Carnegie relationship.

2 QUASI-STELLAR OBJECTS (1959-1979)

Quasi-stellar objects (QSOs) are objects with a star-like appearance whose spectra show large redshifts. Another characteristic of QSOs is an excess of ultra-violet radiation. A more definitive feature of QSOs is the presence of broad redshifted emission lines (Burbidge and Burbidge, 1967). QSOs also exhibit variability in the emission of their radiation. All of these characteristics, when taken collectively, help to define a QSO.



Figure 7: Maarten Schmidt joined the staff of the Mt. Wilson and Palomar Observatories (now Hale Observatories) in 1959, and from 1978 to 1980 served as the Director. His discovery and interpretation of quasars challenged many previously-accepted theories on the origin and age of the Universe (from <http://www.phys-astro.sonoma.edu/BruceMedalists/Schmidt/index.html>).

In this paper, the terms 'quasi-stellar object' and 'quasar' will be used synonymously. 'Radio quasars' are those that have been detected as radio sources, and sometimes these are also referred to as 'quasi-stellar radio sources' (Schmidt, 1975). For the purposes of this paper, all of these objects will be collectively designated 'QSOs'. The term 'quasi-stellar' was first used by Maarten Schmidt, while the word 'quasar' was apparently coined by Hong-Yee Chiu from the Goddard Institute for Space Studies in the 1960s, when he was talking to a newspaper reporter (Kellermann, 2006).¹

Back in 1953, very few discrete radio sources were identified with conspicuous optical astronomical objects. However, F. Graham Smith from the Cavendish Laboratory had reduced the uncertainties in the positions of Cassiopeia A and Cygnus A to $\pm 1''$ in right ascension and $\pm 40''$ in declination (Baade and Minkowski, 1954a). The new positions were accurate enough for an unambiguous identification of both radio sources on plates taken in September 1951 by Baade and Minkowski with the 200-inch Palomar Telescope (ibid.). These two astronomers showed that one of the most intense radio sources, Cygnus A, was associated with an 18th magnitude galaxy with a redshift of $z = 0.056$, or $16,830 \text{ km sec}^{-1}$ (cf. Greenstein, 1984).

As soon as the two 90-foot (27-m) radio telescopes at Caltech's Owens Valley Radio Observatory (henceforth OVRO) began working successfully as an interferometer, Thomas A. Matthews began a program to determine the precise positions of large numbers of discrete radio sources (see Matthews and Sandage, 1963). The first accurate positions were obtained in 1960 and these were published three years later (see Read, 1963).

When Matthews began his OVRO research, he suggested a collaboration with optical astronomers who had access to the 200-inch Palomar Telescope and therefore could search for optical identifications (Sandage, 1999). It was at this point that Allan Sandage became involved in the optical identification program, which was to last far beyond the discovery of quasars, until most of the radio sources in the Cambridge 3C Catalogue had been identified.

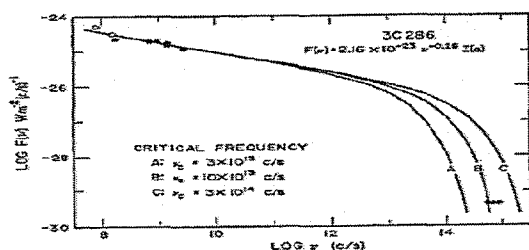


Figure 8: A comparison of the radio and optical power for 3C 286. Photometry over thirteen months shows variability by at least $\Delta V = 0.4^m$ (Matthews and Sandage, 1963: 43).

Sandage was one of the first Ph.D. students in astrophysics at Caltech, and he used the 200-inch Telescope to show that the most distant 'stars' that Edwin Hubble had observed were actually ionized hydrogen clouds. Later he would discover a new class of stellar object that came to be known as 'quasi stellar radio sources'.

The first identification of a radio source with a star-like object was made by Matthews and Sandage in 1960. Sandage began by taking plates of about 20 unresolved radio-loud quasars. From this work came optical identifications for 3C 48, 3C 196, and 3C 286 (Matthews and Sandage, 1963).

A plate of the 3C 48 field was exposed on 26 September 1960, and the only object lying within the error box of the radio position was a 16th magnitude stellar object with some faint associated nebulosity (ibid.; Matthews et al., 1961). Sandage found that the spectrum showed broad emission features that did not correspond to those seen in the spectra of any known Galactic stars. Meanwhile, optical photometry in 1960-1961 showed this object to possess an ultraviolet excess compared with Main Sequence stars; furthermore, it varied by 0.4 magnitude in the course of a thirteen month period (ibid.). It was this seminal investigation during 1960-1961 that initiated the study of quasars (see Burbidge and Burbidge, 1967).

Matthews and Sandage (1963) subsequently identified the radio sources 3C 196 and 3C 286 with faint star-like objects, whose colors were similar to those of 3C 48, and from photometry carried out during 1961 they noticed there was a good fit between the optical and radio data for 3C 286 (see Figure 8).

What Matthews, Sandage or Schmidt could not explain at that time was the anomalous emission lines associated with these objects. It was the identification of the strong radio source 3C 273 that eventually led to the solution of this particular problem. Cyril Hazard (1961) pioneered the use of lunar occultations to determine radio source positions with high accuracy, and on 8 December 1960 he used the 250-foot Radio Telescope at Jodrell Bank to observe an occultation of 3C 212 (ibid.). Hazard, Mackey and Shimmins (1963) subsequently applied this same method to 3C 273, using the 210-foot Parkes Radio Telescope in Australia. It was established that 3C 273 was a double source, where the ratio of the flux densities of the two components changed with frequency (ibid.). The positions of these two components, A and B, were determined with greater accuracy than any other sources known at that time, and were calculated from the observed times of disappearance and re-appearance, which were estimated from the calculated flux density at the edge of the geometrical shadow and from the positions of the diffraction lobes (ibid.). At a frequency of 410 MHz, Component B had a diameter of $\sim 3''$ and a flat radio spectrum, and it coincided with a 13th magnitude star (ibid.). At 400 MHz, Component A had a diameter of $4''$ and a spectral index of 0.9, and it was located at the end of a jet-like optical feature $20''$ from the star (Greenstein and Schmidt, 1964).

Schmidt (1963) used the prime focus spectrograph on the 200-inch Telescope to photograph the spectrum of the 13th magnitude star seen near 3C 273 at dispersions of 400 and 190 Å per mm (see Figure 9). In February 1963 he realized that the spectrum could be explained if the four emission bands were actually hydrogen Balmer lines exhibiting the very considerable redshift of $z = 0.158$ (Schmidt, ibid.). The remaining lines could then be satisfactorily interpreted as [O III] at 5007 Å and Mg II at 2798 Å.

Two possible explanations of this stellar object were suggested (ibid.):

- (1) That it was a star with a large gravitational redshift, and a radius estimated to be ~ 10 km.
- (2) That it was the nuclear region of a galaxy with a cosmological redshift of 0.158, corresponding to an apparent velocity of 47,400 km/sec. The distance would be ~ 500 megaparsecs and the diameter of the nuclear region < 1 kiloparsec.

Schmidt (ibid.) concluded that 3C 273 was an extragalactic object because the derived diameter would be unrealistic if it were located within our Galaxy. In a recent interview, he elaborated on this reasoning (Schmidt, 2006):

Jesse and I [wrote up this work] soon thereafter and it was published in 1964 [i.e. Greenstein and Schmidt, 1964] ... which was the one thing we ever did together, in which we tried to interpret 3C 273 and 3C 48. Our interpretation included an extensive discussion of gravitational redshift, and we found that it was essentially impossible because if you assume it was a gravitational redshift and you see an emission line spectrum like we did, including forbidden lines, you get into a spectroscopic squeeze through which you can show that the object has to be excessively faint intrinsically. So it had to be very nearby to be seen at thirteenth magnitude. In effect I could prove, and I had already done so before the article with Jesse, that if 3C 48 was a compact object of one solar mass, and it showed a gravitational redshift, which means it had to be very very small, I

could show that the distance had to be ten kilometers. Now that's not very agreeable—to have a solar mass at ten kilometers. And then I increased the assumed mass and I found you kept getting in trouble every time again. So I think especially with the publication of the article with Jesse, that we made the most compelling argument that it was not a gravitational redshift ...

Oke (1963) used the 100-inch telescope at Mount Wilson to determine the absolute distribution of energy in the optical region of the spectrum of 3C 273. Accepting Schmidt's redshift of 0.158, Oke confirmed that H α should appear at 7599 Å, because "... this is in satisfactory agreement with observation, when it is recalled that the atmospheric A band absorbs strongly beyond 7594 Å." (ibid.). Oke's research also showed that the absolute energy distribution of the apparent spectral continuum for 3C 273 can be represented by $F_\nu = \nu^{0.28}$, where F_ν is the flux density per unit frequency interval and ν is the frequency (ibid.).

In 1962, Greenstein and Matthews (1963) used the Palomar Telescope to investigate the redshift of the optical correlate of 3C 48, and obtained a value of $z = 0.3675 \pm 0.0003$. They interpreted the radio source as the central core of an explosion in an abnormal galaxy, and from their research concluded that 3C 48 was at an estimated distance of 1.10×10^9 parsecs and had an absolute visual magnitude of -24.0 , or -24.5 when corrected for interstellar absorption. By comparison, 3C 48 radiated about 50 times more powerfully in the optical region than intense radio galaxies, and Oke (ibid.) concluded that this unusually strong optical emission was associated with synchrotron radiation. Matthews and Sandage (1963) arrived at a similar conclusion concerning the optical and radio flux densities for 3C 48 and for 3C 196 by showing that the radiant flux in the optical region can be computed from the radio flux data if one invokes synchrotron radiation.

Schmidt and Matthews (1964) used the Owens Valley interferometer to confirm the identification of 3C 47 and 3C 147 as QSOs with large redshifts. It was found that the position of 3C 47 practically coincided with a stellar object of visual magnitude 18. With a Hubble constant of $100 \text{ km sec}^{-1} \text{ Mpc}^{-1}$, the nominal distance of 3C 47 was 1,275 Mpc, and its absolute visual magnitude was about -23 . Schmidt and Matthews (ibid.) concluded that 3C 47 clearly belonged to the class of QSOs like 3C 273 and 3C 48, which exhibited optical luminosities much larger than those of the brightest galaxies.

Sandage (1966) used the 200-inch Telescope in October 1965 and January 1966 to show that the colors of forty-three QSOs were correlated statistically with redshift, and he concluded that because the redshift of quasars varies across the U-B and B-V diagrams in a regular fashion, statistical predictions of the redshift are enabled using the U-B and B-V values alone. Observations during the outburst of 3C 446 revealed that the equivalent widths of the emission lines and the slope of the continuum both changed (Wallerstein and Oke, 2000). Similar results were found by Oke (1967) when he observed 3C 446 and 3C 279. Continuum changes of 20% were seen in 3C 279 on time scales of one day (ibid.). Yet despite these changes, Sandage (1966) still concluded that quasars were sufficiently similar in their continuum distributions

and in the strengths of their emission lines relative to this continuum for the statistical correlations to be valid.

Wampler and Oke (1967) carried out spectrophotometric observations of QSOs from Palomar (between 5,100 and 6,000 Å, with a resolution of 25 Å) and from Lick Observatory (between 5,412 and 7,056 Å, with a resolution of 30 Å), and their investigations revealed the existence of several emission features previously unknown or only suspected. The evidence indicated that most of these were associated with Fe II, from which an electron density of the gas producing the line can be calculated. The results obtained by Wampler and Oke (ibid.) reinforced the conclusions of Greenstein and Schmidt (1964): that 3C 48 and 3C 273 were associated with distant superluminous objects in galaxies, or were intergalactic objects (if one accepted a cosmological interpretation of their redshifts). Assuming a Hubble constant of $100 \text{ km sec}^{-1} \text{ Mpc}^{-1}$ the distances for 3C 48 and 3C 273 were 1,100 and 474 Mpc respectively. The absolute visual magnitudes then became about -25 and -26 respectively, making these objects among the most luminous in the Universe (ibid.).

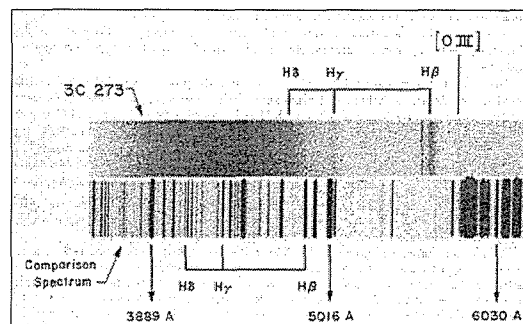


Figure 9: The spectrum of 3C 273 showing the hydrogen Balmer lines with a redshift of $z = 0.158$. Part of the emission indicated as [O III] is due to [Fe II] according to Wampler and Oke (1967). At the bottom is a comparison spectrum of hydrogen, helium, and neon with various wavelengths indicated (after Schmidt, 1975: 284).

Using the 100-inch Telescope and the 84-inch Kitt Peak telescope respectively, Sandage (1965) and Lynds et al. (1965) found that there were sometimes ultraviolet objects on their plates that did not lie close to the positions of any known discrete radio sources. When Sandage began taking photographs in ultraviolet and in blue light with the 48-inch Palomar Schmidt Telescope, he found such objects turning up with a frequency of ~ 3 per square degree down to a limiting magnitude of $B \approx 18.5^m$. Sandage (1965) detailed his results in a controversial paper, announcing that he had discovered in the quasi-stellar galaxies a "... major new constituent of the universe." but Burbidge and Burbidge (1967) demonstrated that there was considerable uncertainty associated with the claim that these objects were as common as Sandage indicated, and other researchers (e.g. Lynds et al., 1965) concluded that some of these objects at high Galactic latitudes might be Galactic. Burbidge and Burbidge (op.cit.) felt that it was ambiguous to describe such objects as galaxies unless indisputable evidence of the presence of stars could be produced.

3 CONTROVERSIES

3.1 The Non-Cosmological Interpretation of QSOs

A small minority of astronomers adopted an interpretation of quasars that was very different to that proposed by Schmidt and Greenstein, and these are presented here to illustrate the nature of the controversy. The commonly-accepted rebuttals supplied by present-day astronomers to these ideas is also included in order to provide the reader with a better understanding of historical developments associated with the discovery and interpretation of quasars.

The local-Doppler hypothesis was first proposed by James Terrell (1964) to avoid some of the problems that were believed to exist if quasars were indeed at very large distances. Terrell identified the nucleus of our Galaxy as the nearest possible explosion center. He then estimated the minimum distance of 3C 273 to be 200 kpc on the basis of the absence of a detectable proper motion and the minimum explosion age of 5×10^6 years (ibid.). He also showed that there were relativistic limits to the fluctuations in brightness which may be observed for a large spherical surface, and also for more general sources. He inferred that quasars were probably no more than light-days in diameter, and there was also a possibility that they may be close to our Galaxy. Terrell's conclusions were based on the relatively rapid fluctuations in the light intensity of known quasars.

Schmidt (1975), however, determined that 10^6 quasars will carry a total kinetic energy of about $10^{60} M$ ergs, where M is the average quasar mass in solar masses. This is about 10^{64} ergs, or the rest-mass energy of 10^{10} solar masses, which is approximately 10% of the mass of our Galaxy. This would make the local-Doppler hypothesis an unlikely one to account for the redshift of QSOs. It should be noted, in parentheses, that Schmidt's quoted numbers are 1975 values, and the typical quasar mass today is understood to be between 10^8 and 10^9 solar masses (e.g. see Vestergaard, 2002; Yu and Tremaine, 2002).

Other hypotheses included that by Halton Arp (1967), who claimed a correlation between peculiar galaxies and radio sources, including QSOs. Arp said that quasi-stellar radio sources (QSS) were associated with galaxies at 'intermediate' distances of 10-100 Mpc (ibid.). He added that one of the problems with the cosmological hypothesis included the difficulty of using conventional physics to understand the origin of the energy required for the high luminosities found in QSOs. Another argument against the cosmological hypothesis was that the diameters implied by the time-scale of radio variations were so small that they indicated the QSS to be much closer than cosmological redshifts would allow. A third difficulty was that the scatter in the redshift apparent-magnitude relation for QSS indicated "... that it is dubious whether there could exist a redshift relation." (ibid.). Finally, Arp claimed that in some QSS such as 3C 9 the expected absorption from intergalactic material was not present "... as it should be, if the light traverses such great distances." (ibid.). A graphical summary of Arp's observations is shown in Figure 10.

Arp (1987: 178) later contended that it

... is ironic but appropriate that in this Hubble diagram we are able to see at the same time the refutation of the conventional viewpoint of quasars and redshifts, the reconciliation of intrinsic redshifts with expanding Universe concepts, and the clear continuity of how the intrinsic redshifts evolve from high redshift quasars into low redshift companion galaxies.

Arp's argument (1987: 38) against the cosmological hypothesis also relates to Figure 11, and is as follows:

The analysis of this photograph seems very simple to me. There are only two possibilities. Either the quasar placed at the head of the filament is an accident, or the two objects are physically connected. Since the configuration has negligible probability of arising by chance, I conclude that this demonstrates the physical association of quasar and galaxy. There goes the whole cosmological quasar hypothesis!

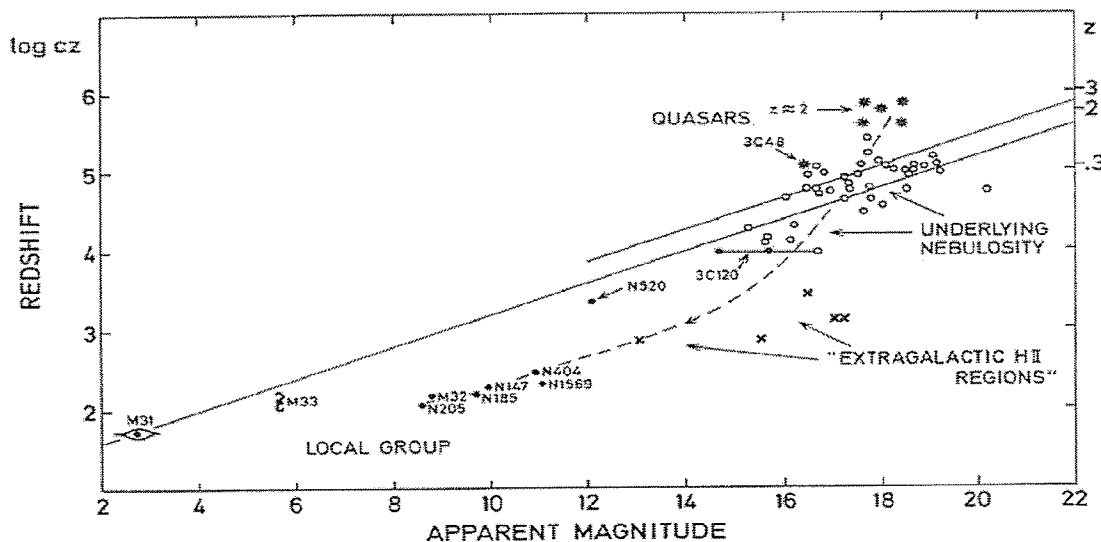


Figure 10: The Hubble Diagram for Local Group objects. The open circles represent measures of underlying nebulosity for a selection of quasar images (after Arp, 1987: 175).

... Another interpretation, since the quasar is quite bright in apparent magnitude, is that, along the lines of Chapter 5, they could both be close to us in space and have been expelled by a nearby galaxy. They might also simply represent a rare accidental collision of a galaxy and quasar in the same locality of space. One thing that is inescapable, however, is that the high redshift quasar is at the same distance as the low redshift galaxy. (Arp, 1987: 38).

I do not find Arp's analysis sufficiently compelling to draw the conclusions that are cited above, because the analysis makes *a priori* assumptions. Most astronomers in the 1970s assumed quasar redshifts were cosmological, in spite of the superluminal motions. Marshall Cohen (pers. comm., 2006) relates that his research involved making measurements of objects such as quasars, blazars and BL Lac objects using Very Long Baseline Interferometry (VLBI):

With VLBI you measure a proper motion, that is, the angular motion on the sky, and then if you have the distance you get the linear motion. We interpreted it in terms of velocity; and the numbers came out at speeds that were faster than light.

This is explained by the fact that there is a relativistic beam aimed nearly at the observer, from which it can be shown that the apparent velocity sideways on the sky looked faster than the speed of light (ibid.).

There were astronomers, however, who supported Arp's views and established hypotheses of a controversial nature. These astronomers used the idea of relativistic motion as an argument against the common cosmological interpretation of the redshifts. As Cohen (2006) indicated in an interview:

There is nothing wrong with something being relativistic at the Galactic Center. There is an enormous amount of evidence showing that there are extraordinarily energetic things going on in the centers of galaxies. So I think that Arp is wrong. You cannot use (the) superluminal motion picture as evidence against the redshift interpretation ...

Geoffrey and Margaret Burbidge are two astronomers who did not accept the cosmological interpretation of quasars (see Burbidge and Burbidge, 1967), and they suggest that the discovery of quasars had an impact on subsequent studies in cosmology, which continues up to the present day (G.F. Burbidge, 2006). This is reflected in a recent paper by Cohen et al. (2006) which contains strong evidence that relativistic beams emanate from quasars. Cohen et al. plotted the apparent transverse velocity or superluminal velocity against the apparent luminosity for 119 discrete radio sources, and found a correlation for the jets in quasars: high apparent velocities were only noted for radio jets with high luminosities. This implied a similar correlation between the Lorentz factor and peak intrinsic luminosity, namely that high Lorentz factors must preferentially exist in jets with high intrinsic luminosities.

3.2 The Role of J. Beverly Oke

In a letter to the author, Allan Sandage (2006b) claims that Schmidt was not alone the day that the 3C 273 spectrum was examined, and that J. Beverly Oke was present. Furthermore, Sandage (2006c) stated that Oke told him that Schmidt could not have made the identification of 3C 273 without a spectrum that Oke obtained on one of his Mt. Wilson observing runs. It should be noted, however, that in Schmidt's 1963

paper, Oke's contribution is specifically acknowledged:

It thus appears that six emission bands with widths around 50 Å can be explained with a redshift of 0.158 ... *The present explanation is supported by observations of the infra-red spectrum communicated by Oke in a following article ...* (Schmidt, 1963; our italics).

The paper that Schmidt refers to is Oke (1963), which states, *inter alia*:

During the course of the infra-red observations a strong emission feature was found near 7600 Å with a possible error of about 10 Å ... Using this line and others in the visible spectrum Schmidt has shown that the most prominent emission features are Balmer lines and that the line at 7590 Å is *H α* .

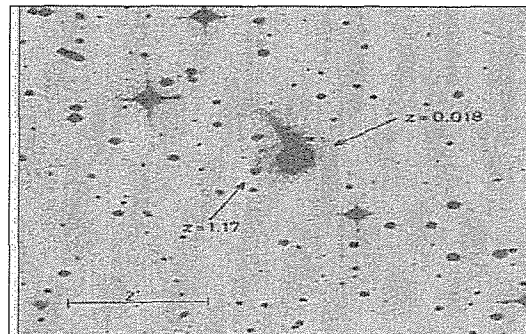


Figure 11: The quasar, Parkes 1327-2006 connected by a luminous filament to a galaxy with a jet (Arp, 1987, 38).

The aforementioned quotations indicate that Oke's contribution was not ignored by Schmidt, and in a presentation at the NRAO in 1983, Schmidt made the following pertinent remarks:

J.B. Oke observed 3C 273 spectrophotometrically at the 100-inch telescope on Mt. Wilson and detected a strong emission line in the infrared, at 7600 Å. A total of seven emission lines were [*sic*] now known in 3C 273 and in hindsight it seems strange that with so much information no larger effort was undertaken to identify the lines ...

It was on February 5, 1963 that the puzzle was suddenly resolved. Cyril Hazard had written up the occultation results for publication in *Nature* and suggested that the identification results be published in an adjacent article. It was in the process of writing the article that ... I noticed that four of the six lines exhibited increasing spacing and strength toward the red. I attempted ... to construct an energy-level diagram based on these lines, then made an error which seemed to deny the regular pattern ... to check on that, I started taking the ratio of the wavelength of each line to that of the nearest Balmer line. The first ratio was 1.16, the second 1.16, the third ... 1.16!

Realizing that this was a redshift, I divided the wavelengths of the other two lines by 1.16 and found that they landed near those of the [Mg II] doublet at 2800 Å and forbidden [O III] line at 5007 Å. Oke's line observed at 7600 Å came close to the wavelength of H- α . Clearly, a redshift of 0.16 explained all the observed emission lines! (Schmidt, 1983).

This long yet invaluable quotation illustrates the crucial insight that Schmidt had when he interpreted the spectrum of 3C 273, and Oke's contribution is fully acknowledged when understood in its proper context.

However, the comments by Sandage show the depth of feeling relating to this discovery that still exists more than forty years after the event.

3.3 The Breakdown of the Caltech-Carnegie Collaboration

Allan Sandage (2006c) offers his opinion as to why the Caltech-Carnegie collaboration ended:

... at the higher levels of the Caltech administration the Caltech physicists had always been dissatisfied with the joint operation of Carnegie at Palomar, and after Bowen's time began to work for the separation of Caltech from Carnegie so that Caltech could pursue an independent path in fund-raising and development of new astronomical facilities. The Caltech physicists were also unhappy with Babcock's push for a Southern Observatory in Chile that would be part of the Hale Observatory organization. This became an increasingly severe problem with the Directorship of Horace Babcock, and when Schmidt succeeded him as Director of the joint Hale Observatories, Schmidt recommended a 'divorce' of the two institutions. He was pressured into this recommendation by the three Caltech physicists, Robert Christy (Provost of Caltech), Robbie Vogt (Head of the Division of Physics, Mathematics, and Astronomy), and Robert Leighton (a senior physicist).² The Trustees of each institution agreed to the divorce. It was not a pleasant event, and has led to the severe estrangement on both sides that yet exists. I suspect it will continue until each of the astronomers working at that time will be dead, perhaps in 20 years.

The rivalry that spawned the remarks made by Allan Sandage can perhaps be explained in light of the following comments by George Preston (2006), Director Emeritus at Carnegie:

... several individuals and groups of individuals within the Hale Observatories staff in the 1960s and 1970s were pursuing the great issues of extragalactic astronomy in those times—the nature of quasars and the expansion of the Universe. These people were all in pursuit of goals that could only be conducted effectively at the 200-inch Telescope, and they brought their rivalries to the meetings of the Time Allocation Committee and the Observatory Committee. Such stuff just went on and on. For these people this research was extraordinarily important. Fame and sense of accomplishment hinged on being able to do it. We had the biggest telescope in the world, the one that was best able to pursue these issues, and a bunch of people on the staff all wanted to do more or less the same things. And with regard to quasars in particular there was Halton Arp, who had his own interpretation of the quasars at odds with well-established laws of physics, and who felt that he wasn't getting a sufficient share of the Telescope time to support his heretical conclusions. I sat in the Time Allocation Committee and Observatory Committee meetings and listened to the endless wrangling. It was unbelievable. I should add that some views were much more moderate and reasoned than others. To say the least, those were exciting times in Pasadena.

Clearly there is some difference of opinion about what actually happened when quasars were discovered. There are also some possible reasons for the relational problems that began to be manifest between Caltech and Carnegie. These differences and reasons are discussed in the following section of this paper on the basis of oral history interviews and what has been preserved in correspondence and in the published literature.

3.4 Quasars and the Caltech-Carnegie Nexus

The discovery of quasars impacted on the relationship between Caltech and Carnegie because of the competitive nature of the astronomers involved, and because quasars were a major area for research interest in the 1960s. The Caltech-Carnegie split, however, appears to be a direct result of administrative problems rather than scientific differences. It is certainly true that the creation of Caltech's radio observatory opened up many opportunities for collaboration between the astronomers at the OVRO, Mount Wilson, and Palomar, but equally important were the bonds in nuclear astrophysics that those in Caltech's Astronomy and Physics Departments forged with their Carnegie colleagues at the same time. It should also be noted that under the agreement between Carnegie and Caltech, all staff members—including graduate students—had equal rights of access to all of the instrumentation at Mount Wilson and at Palomar (Greenstein, 1982).

Yet the two institutions were very different, for research was emphasized at Carnegie while at Caltech the professors have many other duties, including teaching. But while there were obviously cultural differences between Caltech and Carnegie, these do not appear as a pragmatic reason for the breakup.

It is apparent that the discovery of quasars played a role in the conflict between Caltech and Carnegie, as suggested by Allan Sandage. In a recent letter to me (Sandage, 2006a), he says that although he and Tom Matthews were very much involved in the optical identification of quasars, Matthews was never given enough credit for the discovery. After all, it was his precise radio sources positions that allowed the optical identifications to be made. It would be interesting to obtain Matthews' perspective on these views, but to date all of my attempts to contact him have proved unsuccessful.³

The competitive nature of the Caltech-Carnegie astronomical environment in the 1960s has already been referred to by George Preston, and this is also mentioned by Sandage (1999: 477):

Beginning in 1963 the quasar program became quite frenzied with the 3C 273 redshift discovery, not only at Palomar, but also at Kitt Peak, Lick, and Hawaii, with rivalry between all groups and within each group often leading to severe tension.

In a previously-cited interview, Maarten Schmidt (2006) commented that the use of telescope time was not an issue in the breakup of the Caltech and Carnegie nexus, but the previously-mentioned letter to Allan Sandage, Jesse Greenstein (1969) states that the use of the Caltech and Carnegie telescopes was a delicate issue between the radio astronomers and their optical counterparts. Perhaps the conflicts that arose in the 1960s were at the operational level and proved to be more surmountable. As time went on, however, the conflicts appear to have risen to an administrative level, where the decision-making affected the careers of several people. It was at this point, in 1979, that Maarten Schmidt took the action that he did which led to the dissolution of the Carnegie and Caltech relationship.

The controversies surrounding the interpretation of quasars by Arp, Burbidge and Terrell would seem to have had little if any effect on the Caltech-Carnegie

nexus, but the controversy surrounding the *discovery* of quasars does deserve closer examination. The views expressed by Sandage (2006b) and Schmidt (1999) in the aforementioned correspondence probably filtered through to the administration of these two institutions and helped precipitate the breakdown of the Carnegie-Caltech relationship. This is a logical conclusion because even though the two institutions were financially independent, their facilities were utilized jointly.

When scientific organizations compete for facilities, it is difficult to imagine that scientific differences of opinion do not affect how these entities operate, and the interviews cited above with Sandage and Schmidt provide evidence that this was indeed the case with the Caltech-Carnegie nexus. Quasars were a major area of astronomical research in the 1960s and 1970s, and any scientific group that could claim a discovery as its own would want to be protective of its position. The discovery and subsequent interpretation of quasars was not without controversy, which led indirectly to a deterioration of the relationship between Caltech and Carnegie staff.

In interviews conducted with some of the current staff at Carnegie who were present at the time of the breakup, I found some memories of a rather bitter nature. When asked about the reaction to the decision to formally separate the two institutions, Eric Persson, a staff astronomer at Carnegie, responded:

Well I can tell you that there was a very bad feeling on an October morning in '79 when—and anybody who was here then will tell you the same thing—we came in and it was there in our mail boxes—this short paragraph from the Director, Maarten Schmidt, saying, well I hereby dissolve the Observatories and there is no longer any Hale Observatories. It came as a real shock. My colleague, Steve Schectman, downstairs, and I, remember it like it was yesterday ... it was just a bad feeling. (Persson, 2006).

Persson's comments are not atypical of how many of the Carnegie staff felt as a result of the breakup. In retrospect, however, this was not a bad thing because both institutions have subsequently acquired astronomical instruments that are unique and have established their own independent world-class research programs. Today, Caltech operates two 10-meter telescopes and Carnegie two 6.5-meter telescopes, yet this may never have happened if the two institutions had not separated.

The actual separation was executed by both Presidents on 1 July 1980, when Maarten Schmidt stepped down from the Directorship. The joint operation of the observatories was replaced by joint utilization, and it meant that the Time-assignment Committee still consisted of Carnegie and Caltech astronomers. According to Schmidt (1999), this arrangement worked "... very well, until the late eighties; it ran smoothly and was appreciated by both sides ..." As soon as the separation took place, each of the institutions became aware that they were responsible for their own astronomical facilities and destinies.

This acknowledgement carried over into the 1990s when discussions began about the next generation of very large telescopes. These were attended by representatives from both institutions, but by this time the Carnegie already had a major investment in Chile—which they wished to develop—while the Caltech

astronomers favored an Hawaiian-based project (Cohen, 2006).

4 CONCLUDING REMARKS⁴

The results of this research may be summarized as follows:

1. The administrative organization of Caltech-Carnegie never provided a unified sense of identity to each institution. There was always an 'us-them' syndrome which was competitive rather than cooperative, and this led ultimately to a breakdown of the collaboration and the dissolution of the Caltech-Carnegie nexus.
2. In a 5 July 2007 email to one of the Editors (W.O.) Maarten Schmidt disputes this: "I can only say that I was never pressured into recommending a separation by Christy, Vogt and/or Leighton."
3. The discovery of quasars in the 1960s augmented the competitiveness within the Caltech-Carnegie nexus, because these objects fundamentally altered our understanding of cosmology. In effect, astronomers realized that astronomical history was being made, and many astronomers wanted to be part of this process.
4. The controversies surrounding the interpretation of quasars by Arp, Burbidge and Terrell were short-term distractions that did not contribute significantly to the breakup of the Caltech-Carnegie nexus.
5. The availability of telescope time was not an issue in the Caltech-Carnegie breakup, even though some friction was felt by both sides at various times.
6. The inherent difference in the duties and responsibilities of staff members at the two institutions did not seem to be a factor in the breakup.
7. Any conflicts that developed regarding the actual discovery of quasars did not materially affect the Caltech-Carnegie nexus. The majority of astronomers acknowledged Schmidt's interpretation of quasars, and the contributions by Oke and Matthews were properly credited in the associated literature.
8. In the long term, the integrity and prestige of both Caltech and Carnegie has not been diminished by the breakup. In fact, the acquisition of new 6.5m and 10m telescopes by the two institutions was a result of the breakup of the nexus.

5 NOTES

1. Chiu's term 'quasar' was first used in his paper on "Gravitational Collapse" presented at the First Texas Symposium in Relativistic Astrophysics, held on 16-18 December 1963 in Dallas, Texas (Chiu, 1965), but it took some time for it to be generally accepted by astronomers.
2. Maarten Schmidt (pers. comm., July 2007) disputes this claim: "I can only say that I was never pressured into recommending a separation by Christy, Vogt and/or Leighton."
3. If any reader can supply me with the current address of Thomas A. Matthews, please email me at: Edward.Waluska@jcu.edu.au
4. This research is part of a continuing doctoral project, and as additional information comes to light hopefully it will lend further credence, or otherwise, to some of the controversial statements contained in this paper.

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The following abbreviation is used:

CA = California Institute of Technology Archives

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