

March 28, 1979

Dr. Warren Kornberg, Editor
MOSAIC
National Science Foundation
Washington, D. C. 25050

Dear Dr. Kornberg:

Many thanks for sending me two copies of the issue of MOSAIC with the article on "astrochemistry". I was glad to see such an article included. By and large, it does a good job of bringing in many facets of the field. However, in places the article rewrites history in a way which may appear completely logical and as the textbooks would have it, but which is not correct nor the way real science frequently happens. Since I think this is important in understanding the problem of planning for science, let me explain.

On page 11 it is said, "Radio astronomers went hunting the molecules Townes had predicted [in a 1955 article*]. But telescopes weren't then up to finding them." The article then implies that the field began in 1969 only as the result of completion of the 11 meter NRAO telescope. It continues with, "Though Townes's original predictions attracted some observational efforts, because of their theoretical implications they had been met with some skepticism." The latter half of the second statement I believe is correct, but it is incorrect that astronomers went hunting for the molecules or that telescopes weren't up to finding them.

Firstly, I believe the only search was that of Weinreb, Barrett et al. who looked for OH and indeed found it. I am unaware of any other searches besides this successful one. If you know of anyone else who really searched and failed, please let me know.

Secondly, telescopes were in fact quite capable of finding molecules very much earlier than 1969, when the article indicates the millimeter antenna at Kitt Peak was completed. Obvious evidence of this is that the first complex ones, NH_3 and H_2O , were found in 1968 and not with this antenna, but with one at Hat Creek which Welch had recently completed. Still further, the next line detected, that of H_2CO at 6 cm, was detected with the somewhat older 43 m NRAO telescope and could have been detected with any one of a number of 85 ft. antennas which had been available for some years. Further than that, the H_2O line was so strong in some sources that available antennas and standard types of receivers could have detected it one or two decades earlier. To put the availability of antennas in perspective, I am enclosing a short table on the status of millimeter antennas in the 1960's [from Cogdell et al., Proc. IEEE

*Schlovskii in the USSR had made similar predictions at about the same time.

Dr. Warren Kornberg

-2-

March 28, 1979

18, 515 (1970)]. You will see that between 1961 and 1968 no less than 8 antennas for millimeter waves were available, including the 11 meter NRAO one which actually began operation in 1967.

Were receivers a bottleneck? No, though they were difficult at wavelengths as short as a few millimeters and use of a BTL design on the 11 meter telescope was an important event. Not many receivers with a suitable bank of filters were available for wavelengths near one centimeter, but some were. They were also not expensive nor difficult to build, and we built the ones which detected NH_3 and H_2O with completely standard designs and more-or-less commonplace performance.

Truth is indeed often stranger than fiction; you seem to have missed the really interesting story that so much science was waiting for someone to do a rather simple experiment. This is particularly notable since I had given the 1955 paper which you referred to at an IAU Meeting on Radio Astronomy at the invitation of Van de Hulst who asked me to suggest what might be found after the 21 cm hydrogen line. Yet so far as I know, for twelve years there was no serious search for these molecules except that carried out by Alan Barrett and associates who searched for and found OH. We had measured this transition in 1958-59 at Columbia University expressly for such a search, and Alan made two subsequent searches; the second, in 1962-63 with Weinreb et al., succeeded. There the search seems to have stopped until our efforts at Berkeley in 1968. Buhl and Snyder proposed looking for H_2O at NRAO at about the time we were looking for it at Hat Creek (1968) but were not given any immediate opportunity, I am told because referees felt the search was unpromising.

The real point and lesson is that in considering the national scientific effort we need to be alert to a multiplex of ideas and to the rigidities of conventional wisdom, in addition to needs for large pieces of equipment. The history of research does not necessarily follow the logic of plans by expert committees or central administration, and perhaps MOSAIC should reflect a little of this difficult but significant reality. In any case, it should not be covered up.

I want to add immediately that it is also true and significant that the 11 meter Kitt Peak antenna has been very important in enlarging the amount and range of work which could be done and in allowing some work which would not otherwise have been practical. Its importance need not be downplayed! I also add that oversights and surprises in science should not be looked at as anyone's fault, but simply as the nature of research.

There is at least one other historical misconception in the article, but the above comments make what seems to me the critical point. Please accept them as an effort to help our common enterprise of promoting the development of science.

Sincerely,

CHT:wk

Charles H. Townes

Enclosure

bcc: W. E. Howard, III; Alan Barrett; Arno Penzias; W. J. Welch (with enclosure)
David Buhl; L. E. Snyder (with enclosure) *