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July 17, 1962

NOTES ON FUTURE PROGRAM AT GREEN BANK

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1. INTRODUCTION

The purpose of this report is to set down the general ideas concerning prospective activities at Green Bank which I have been able to formulate during my short visit to Green Bank and my time in hospital. They should form the starting point for a general statement on policy which I should like to prepare in co-operation with the Green Bank staff and the A.U.I. executive as soon as I am able.

I should like to start by trying to define what my general objectives should be in coming to Green Bank. As I see it, there are three major objectives, though these are highly inter-dependent. They are as follows:

(a) The development of a first-class scientific team at Green Bank.

(b) The provision of extremely powerful radio astronomy equipment for the use of American radio astronomers. These may be members of the Green Bank organization, of universities or of other research institutions. The use of the equipment should not be restricted to American radio astronomers, but it is essential, since America is paying the bill, that Americans with appropriate abilities should have access to the equipment. The essential qualification is that the person concerned be likely to be able to produce results of high scientific value. I think the allocation of facilities should be the responsibility of the director at Green Bank.

These two objectives illustrate the interdependence I mentioned above. Equipment can only be really effective if someone at Green Bank knows thoroughly well how to operate it, and the only sure way of attaining this is for the person concerned to have used the equipment in obtaining important physical results.

(c) The stimulation of research in radio astronomy generally in the U.S.A.

This, again, is inter-dependent with objectives (a) and (b) because while there are so few good and experienced radio astronomers in the U.S.A., it is extremely difficult for Green Bank to acquire the staff necessary to realize the first two objectives.

In the remainder of the report I shall first discuss what I consider the two major projects which I wish to see completed at Green Bank, then the development of some current lines of research already under way at Green Bank. Finally I mention a number of aspects of radio astronomy which I consider to be of particular interest, but which, mainly because of the limited staff, it may not be expedient to attempt to tackle at Green Bank. However, I wish to maintain a general interest in these aspects, and it may be possible to stimulate some other research organization in the U.S.A. to undertake research in these fields.

2. <u>MAJOR PROJECTS</u>

The two projects which I consider to be the major projects are (a) the 140 ft. telescope and (b) a project which is not yet clearly defined but which I shall describe as "the high resolution project." I think the latter should have first priority on the Green Bank program.

2.1. The 140 ft. telescope

Plans for the completion of this instrument are already well under way and I would propose to continue existing arrangements and to maintain close liaison with the engineers in the hope of anticipating snags before they become serious or of permitting the possibility of changes in design which might facilitate the construction without appreciably impairing the performance of the instrument.

I think it is important that Green Bank should have a quite large fully steerable paraboloid like the 140 ft. telescope. Such an instrument is extremely versatile and permits the undertaking at short notice of numerous types of observations which current results may suggest as being important. e.g. observations of linear polarization

When this radio telescope is completed, it will probably not be unique in performance. There should by then be at least two steerable paraboloids of approximately equivalent performance - the recently completed Australian 210 ft. instrument which, however, is located in the southern hemisphere and so is complementary, and the proposed Canadian 150 ft. instrument. Relative to the Australian one, the Green Bank instrument will have a lower resolution - approximately two-thirds - at the important wave length of 21 cm., but will have approximately the same resolution at the shortest possible wave length of each (3 to 5 cm. for Green Bank, 10 cm. for the Australian one). It may be found that atmospheric conditions frequently prevent precise observations at 3 cm. I have therefore suggested 5 cm. as a possibly more realistic minimum working wave length. The specifications relating to size and surface accuracy of the Canadian instrument are very similar to those of the Green Bank one, but I hope the

Green Bank one will have considerably greater precision of setting. The Green Bank dish should be far more useful than the much larger 250 ft. Jodrell Bank one because of the much lower minimum working wavelength.

N.A.S.A. also proposes to construct several 250 ft. steerable paraboloids, but we do not know the extent to which these will be used in radio astronomy.

It will be desirable to provide special receivers for the 140 ft. radio telescope to be ready immediately the erection is complete. Because the competion date is fairly distant, I think the only specific receiver we should have under way now is an extremely sensitive receiver at the lowest practical working wavelength. This has already been provided for through the ordering from A.I.L. of a travelling wave maser to work on the wavelength of 5 or 6 cm.

2.2. <u>High Resolution Project</u>

One of the greatest challenges to instrumental radio astronomy has been the design of radio telescopes with sufficient angular resolution to show the ~~superficial~~ significant physical features of objects of interest in the sky. The difficulty is associated with the diffraction limitation in which the resolving power is relat~~ive~~ed to the number of wave lengths in the aperture of the instrument and the long wave lengths of radio waves calls for inconveniently large apertures. There are three basic approaches to the provision of greatly improved angular resolution (a) making the structure very large (b) drastically reducing the wave length (c) using one or other of the interference techniques which have been developed in radio astronomy for this purpose. With respect to (a) at Green Bank I should be reluctant to embark on a program of building an extremely large paraboloid. I think that the 300-ft. and the 140 ft. when completed will supply quite outstanding facilities of this type.

With respect to (b) we are planning in the millimetre wave project to make just such a step but it should be pointed out that if successful the gross change of wave length may well mean a considerable modification in the relative emission from different parts of radio sources and indeed if the spectral trends in the known wave length range can be extrapolated to the millimetre wave region, the signals from most of the known radio galaxies will be too weak to be observable. We expect to observe only a very small sample of the most intense. This project is pioneering work in a new spectral range rather than an improvement of resolution in the radio frequency range. The third approach, the application of interference principles, involves the combination of signals from a limited number of moderately large antennae and in my opinion is much the most promising for the provision of greatly improved angular resolution. As an illustration of the power of the method one may quote (a) the original Mills Cross which had a single circular beam of diameter 48 minutes at a wave length of 3 1/2 metres (b) the Christiansen Cross (used to produce radio pictures of the sun) having a beam of 3 minutes of arc at 21 centimetres but with multiple responses. These crosses were each constructed at a cost of a few tens of thousands of dollars (c) the California Institute of Technology interferometer, which, using ~~operative~~ aperture synthesis methods, has produced radio isophotes with a resolution of a few minutes of arc over a few dozen of the most intense of the discrete sources in the sky. This experiment employed two movable 90 ft. fully steerable paraboloids.

I consider The specific objectives of the Green Bank high resolution project should be the provision of equipment furthering the study of radio galaxies. The discovery of radio galaxies in the sky has provided some of the most interesting problems of present day astronomy. It is found that certain galaxies, and we cannot tell from optical observations which ones, show a radio emission vastly in excess of most other galaxies. We refer to these high radio emission galaxies as radio galaxies. An extraordinary feature of these galaxies is that the radio emission appears to come from areas in the sky which are adjacent to the optical nebulae but which frequently do not even overlap them. Thus for example the Cygnus source appears optically as a galaxy of diameter about minutes of arc and radio wise as two blobs which have not yet been carefully delineated each about a minute of arc in diameter and disposed symmetrically one on either side of the optical nebula, each a bout 1 minute of arc from the nebula. The radio emission is currently believed to originate in regions in space in which high energy electrons associated with cosmic rays move in magnetic fields but the detailed nature of these regions and the reason for their occurence at places apparently outside the optical galaxy are still merely fields for speculation. It would seem that one of the most powerful methods of attempting to solve this mystery would be the study of the actual forms, i.e. radio brightness distributions over a reasonable sample of radio galaxies.

There is further reason for interest in the radio galaxies, in that, irrespective of their origins and mechanisms, the radio emission from some is so enormous that they can be observed at distances far beyond the limit of observation of the largest optical telescope. Observations of radio galaxies thus provide the possibility of finding out something of the nature of the universe beyond our present horizon and so contributing information on cosmology. In fact, from statistical investigations of the number-intensity distribution of radio sources the Cambridge group claim to have shown that the "steady state" theory in cosmology is incompatible with observation. This conclusion is highly controversial. Other observers obtain results which are discordant with the Cambridge ones and if correct leave the cosmology question quite open. The differences in results are probably associated with the differant observing techniques, the Cambridge group using an interference technique and the Sydney group, for example, a "pencil beam" technique. The different methods would give different results if many of the sources were not simple blobs but showed complex brightness distributions e.g. if they were broken up into a number of distinct bright areas.

At the present time I know of several major projects for the study of radio galaxies. Two important ones, each in the construction stage are:

(a) The Benelux Mills' Cross (1 minute of arc beam at 1420 mc/s - OOrt).

(b) The Sydney University Mills Cross (3 minutes of arc at 400 mc/s plus about 10 minutes at about 130 mc/s - Mills). Each of these is capable of surveying the whole of the sky visible to it. I think of these as analagous to the 40 inch Schmidt at Mt. Palomar and I should like at Green Bank to aim at the design of an instrument somewhat equivalent to the 200 inch in that it would have considerably higher resolution so as to be able to show the physically significant detail of an adeqquate sample of radio galaxies. I should be willing to permit much slower operation than is feasible with a whole sky instrument, i.e. to trade observing time for resolution.

I do not at this stage have a clear idea of the best method of realizing this objective, but wish to approach it from the point of view of a design study. There are at the present time three or four methods which seem theoretically possible. These include (1) 2-antenna aperture synthesis, probably using a number of antennae instead of simply two moving ones. (2) a big

Mills Cross (3) the "ring" system of Paul Wild. I think that the existence of a multiplicity of methods means that the project will prove to be technically feasible but I do not like any of the methods in their existing form. However, I think that there is a high probability of some simplifying ideas arising in the course of the design study which could make all the difference to the practicability of the project. I have not until now mentioned the resolution and sensitivity required in this project because I am not fully conversant with existing work, but it is clear that a resolution of a fraction of a minute of arc will be required. I think the first step in this project should be a careful evaluation of the existing observations to help determine the specification to be aimed at. Observations highly relevant to this objective have been made in Jodrell Bank (Palmer) and Sydney (Scheuer) and at the California Institute of Technology.

I think the general tactics to be pursued should be as follows: 1. We should attempt to appoint a leader for the project. W.C. Erickson is my choice and I have already approached him to find out if he is interested (no reply yet). 2. When the leader is appointed we should attempt to appoint several new members of staff who are skilled in these high resolution fields.

With regard to actual studies I think they may be divided into several phases. Phase 1. A careful study of existing observations. I should like to see Scheuer and Palmer come to Green Bank to make the best possible statistical estimate of the distribution of Fourrier components ~~over~~ for the radio sources. I take it they could enlist the cooperation of the California Institute of Technology folk. I have written to Scheuer asking if he would be interested (no reply yet). Heeschan has spoken with Palmer who is reasonably interested.

Phase 2. We should write down as many methods as we can think of and make a careful study of the basic limitations of each, in particular the sensitivity-time aspect and also the technical difficulties. At this stage we should be able to make some generalizations e.g. Is the aperture synthesis method fundamentally more sensitive than other methods?

Phase 3. The above two studies should give us a knowledge of what is to be observed and some appreciation of the technical difficulties so that it would appear feasible to select a preferred method. I should then like to see this put into operation on a relatively small scale in order to have practical experience before embarking on any large scale venture. We should then be in a position to assess the whole project and make reasonable compromises. It is probable at this stage or indeed earlier that a number of subsidiary experiments may be required. With the results of these at hand, the final decision whether to proceed or not should be taken.

DEVELOPMENT OF CURRENT RESEARCH AT GREEN BANK

3.1. The 85-foot and 300 foot radio telescopes

Work on these was commenced by the Greenbank staff and I should like to leave the initiative with them. The 300-foot one is of special interest as it is currently the largest steerable parabaloid radio telescope on the world. It is a meridian-mounted instrument, steerable in elevation only which is the reason for the remarkably economical construction (gross cost $900,000). In consequence it can observe a particular region in the sky only once per day - at transit. Hence it is a time-wasting instrument, in comparison with a fully steerable one, for studies of a single object in the sky. But it is planned to evade this limitation by programming observations so as to have a considerable number of objects, suitably spread in time of transit, under observation at a particular period. There is now good reason to suppose the dish will be adequate for observations at wavelengths as low as 21 centimetres where the beam width should be about 10 minutes of arc. This should give most interesting results both on the hydrogen line and the continuum.

3.2. Millimetre Wave Project

It is good policy in a research laboragory to have several projects which are distinct from the main line of research but which are sufficiently close to them in some respects so that ideas from one field can fertilize the other. The millimetre wave project is a good example of such. It is an attempt to open up a new region of the electromagnetic spectrum for astronomy. The phenomena observed may well be appreciably different from those observed in the conventional radio frequency range but there is the underlying unity of objective of attempting to find out about the universe outside the earth. This project began with the invention by Frank Low of Texas Instruments Inc. of a new form of bolometer employing germanium at liquid helium temperatures which was an order of magnitude more sensitive than previously known bolometers. A description of this bolometer is published in the Journal of the Optical Society of America (51, 1300, 1961) and it was noted by Frank Drake that with this phenomenal sensitivity it would be possible to make an instrument for the reception of millimetre wave length radio waves with a sensitivity far exceeding that attainable by conventional means. The complete instrument would consist of a parabolic antenna, suitable filtering arrangements to exclude wave lengths other than those wanted, and the bolometer. Drake has determined that a suitable antenna can be purchased at a reasonable price, the filtering out of wave lengths longer than those required should be easy using wave guide techniques and the remaining technical problem is to filter out the residual short wave length radiation which in many cases will be many orders of magnitude greater than that being studied. However, the problem looks a reasonable one to undertake.

At the time of my visit to Green Bank, Drake and Heeschen discussed the project with me and I gave it my warm approval. Since that time, Frank Low has been offered and has accepted a position at Green Bank which immensely strengthens the scientific talent. My suggestion for the future development of the project would be to form a small group, including Drake and Low and one or two technicians plus a new bright young physicist and I think the prospects for success are bright. In this particular case, the bolometer has applications outside the particular project described above, e.g. it is an ideal instrument for infra-red spectroscopy. Low is anxious to develop these other applications and I am sure this should be encouraged. Such development is likely to involve collaborative research with other institutions.

SOM E PROMISING FIELDS OF RADIO ASTRONOMY WHICH MIGHT AT SOME TIME BE DEVELOPED IN THE U.S.

4.1. Studies of low-frequency cosmic radio waves

An interesting new field in radio astronomy has been opened up by the work in Sydney of the late Alex shane and his colleagues. Working at the relatively low frequency of 20 mc/s they have shown that it is possible to observe ionized interstellar hydrogen (H II regions) in absorption against the relatively much brighter non-thermal background. Their work shows a series of dark regions along the plane of the Milky Way due to the concentration of H II regions in this vicinity. An analysis of the results by Komesaroff has shown that it would be possible to determine the relative placing of H II regions and non-thermal emitting regions along any line of sight if it were possible to measure the variation of brightness with frequency over an adequately wide frequency range. However, the important range appears to be well below 20 mc/s and I think that it is not desirable to become involved in such research utilizing observations taken from the ground in a country like the U.S.A. where radio interference is particularly severe. It can best be carried out in remote areas like Tasmania. However, it is possible that some worthwhile results might be obtained from satellite observations. I was recently approached by Dr. Henry Whale, a New Zealander who has been working with NASA who wished to get my perspective on the utility of such observations. I should like to encourage men like Whale who think they can make useful observations but do not think this is a field in which Green Bank should take any active part.

4.2. Studies of Magnetic fields in Inter-Stellar Space

Recent thinking in astrophysics is permeated by concepts involving the existence of magnetic fields in interstellar space. Direct evidence for the existence of such fields, however, is ver y hard to obtain. Optically, the most significant evidence for extensive magnetic fields is the observation that the light from a large number of stars is slightly plane polarized. These stars are ones showing interstellar absorption effects and the polarization is attributed to absorption by elongated dust particles having a preferred orientation in space. This orientation is attributed to weak magnetic fields. More direct evidence exists in a few exceptional cases such as that of the Crab Nebula which is the remains of the supernova of 1054 A.D. The light from this ~~which~~ shows a marked degree of linear polarization which appears to arise at the source and not through absorption effects as in the case of the stars noted above. In this case the light emission is believed due to the synchrotron mechanism which involv es the presence of magnetic fields. Definite radio evidence at present is restricted to the observation of plane polarization in the radio waves from the Crab Nebula and one or two other discrete sources. In these cases the mechanism of emission of radio waves is also believed to be the synchrotron mechanism and the observation of linear polarization supplies strong confirmatory evidence further ~~and~~ Accepting this hypothesis the polarizaton gives evidence on the existence and direction of magnetic fields in that part of space. It is claimed, further, by Leiden observers that in extensive areas in the sky, the background continuum also shows plane polarization. This is interpreted again as providing strong evidence for the emission mechanism being the synchrotron mechnaism and if this interpretation is accepted, such observations could provide very important evidence on the general distribution of magnetic fields in interstellar space. This is a field in which the observations are excessively tricky and it is most desirable that independent confirmation of the Leiden claims should be provided. This subject is so important that I think it desirable that some observations should be attempted at Green Bank though the staff and the facilities available at present would not permit a really thorough-going investigation. I should like in this particular field to enlist the cooperation of C. Mayer of the Naval Research Laboratory, who was the pioneer in the observation of radio polarization from the Crab Nebula and who is a quite outstandingly careful experimentalist, and to work with him to such an extent that we at least become familiar with the technical difficulties of the problem, ~~so that~~ We could thus put ourselves in a position where we could undertake new observations in this field at relatively short notice.

In principle there is another method of obtaining radio evidence of the existence of magnetic fields in interstellar space. This is through observations of Zeeman splitting of the 21 cemtimetre hydrogen line. Various attempts at such observations have been made including one by Sandow Weinreb, a Massachusetts Institute of Technology Ph.D. student who worked for some time at Green Bank. The results in all cases so far have been negative. I should like to see Weinreb extend his observations under Green Bank auspices if he thinks that there are reasonable possibilities of success.

4.3. <u>Counting discrete sources</u>

In the earlier section on the high resolution project it was mentioned that there was violent disagreement among observers on the question of the statistical distribution of the intensities of discrete sources. Two radio telescopes in the United States have been built which should be capable of giving information on this statistical distribution. These are the radio telescopes at the University of Illinois and the Ohio State University. The Green Bank 300-foot instrument if fitted with a very sensitive continuum receiver, could also be quite a powerful instrument in this field. I should like to see the Illinois and the Ohio State University groups combine on the problem and if they so desire borrow the Green Bank 300-foot instrument for checking purposes. My own reaction to the problem, however, is that the disagreement among observers so far has been so fundamental that the all-important next step is to take high resolution observations of a fairly large sample of discrete sources as is ~~considered~~ planned in the high resolution project, ~~so that~~ [illegible] We ~~can~~ understand more clearly ~~what is~~ the meaning of counts, made with particular radio telescopes of the apparent numbers of sources at various intensity levels.

4.4. <u>High resolution observations of solar flare phenomena</u> Paragraph 1 of the Paul Wild proposal preface.

It appears to me that the next major advance, whether it be related to these hypotheses or to some totally unexpected discovery, is likely to be made through the development of tehhniques capable of yielding pictures of the Sun in the "light" of radio waves either in the wavelength range around several metres, which would give information on the disturbances in the corona, or around 10 centimetres, which would give information on ~~distribution~~ disturbances in the upper chromosphere. I believe that we are in the best phase of the sunspot cycle to begin the development of such a project because it will take several years to become operational, and by that time the sunspot minimum will have passed but only a few disturbances will normally be present on the sun at one time, thus offering the best conditions for getting the equipment operating and reaching a general understanding of the phenomena observed. This will provide a better chance of interpreting the complex situations appearing at sunspot maximum.

I do not think that we have the staff and facilities to undertake such a project at Green Bank, but I should very much like to see a University or research institute with adequate technical facilities and personnel U.S. undertake it. From the Green Bank points of view it would have the merit that it would train a number of Americans in the art of high resolution radio work.

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~~PREFACE~~

In the last few years the wonderfully detailed observations of the Sun obtained by optical astronomers have begun to be supplemented by information from completely new sources: by radio observations, by observation from rockets far above the terrestrial atmosphere, and by a better understanding of solar-terrestrial relations. This new information, in conjunction with the optical, has begun to yield a physical understanding of what is going on in the solar atmosphere which promises to transcend all that was previously known. Among these new data, observations of the radio waves emitted by the Sun have played an outstanding part, and studies, pioneered in the Radiophysics laboratory, of the spectra of short duration bursts of emission at times of solar disturbance have been particularly significant. These, and associated studies, have led to hypotheses of giant explosions on the Sun which require for their verification and further investigation more powerful observations than any yet available.

July 17, 1962

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