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Dr. A.E. Bell
Little Eversham House
Wellington Road

CHELTENHAM, Gloucestershire
England

Ons no./Our ref.: WTS/jn

GRONINGEN, 11 July 1978
Nettelbosje 2 (Paddepoel)

Onderwerp/Re:

Dear Dr. Bell,

I am a historian of science and radio astronomer currently involved in a long-term project on the history of radio astronomy. I have recently learned that you were an acquaintance of J.W. Phillips, who mainly worked (and presumably still does work) on coal research. Immediately following World War II, however, Mr. Phillips was a member of a radar group led by J.S. Hey which made fundamental discoveries concerning extraterrestrial radio emission. Since little other than the published record is known about this group, it would be of great value if I could locate Mr. Phillips for further information. Do you know of his present or past whereabouts?

Thank you very much for any information you can give. Please reply to the above address in Holland, where I will be until 25 September.

Sincerely yours,

Woodruff T. Sullivan, III
Asst. Prof. of Astronomy
permanent address: Dept. of Astronomy FM-20
University of Washington
Seattle, WA 98195
USA

ST TEWKESBURY GLOUCESTERSHIRE GL20 6EB
Telephone: Tewkesbury 294003

15. 8. 78

...man,

...hillips is a very old friend. He left the ...stablishment, which was then directed by ... and spent a year in Australia. He then ...ching and recently retired after some ten years ...hops, Cheltenham. He lives at Hillside Cottage, ..., Cleeve Hill, Cheltenham (Bishops Cleeve) 2063

...s very interested in this observational ...omers, done I understand with modified radar ... glad to see J. W. Phillips gets into indexes, ... all you want from him. I think it will ...rection if I send him your letter to me ... elusive these days & cannot imagine ... his time. He is in some ways a distinct ... delightful man.

... hoping to get over to Leyden later this year ... a youthful book on Christian Huygens!

Yours sincerely. Arthur Beer

Cleeve Hill, Cheltenham Glos
23rd August 78

Dear Professor Sullivan,

Arthur Bell passed on your letter to me. It came as a pleasant surprise — I have long resigned myself to being submerged in "et al", so that to find that someone is still aware of my name is very gratifying.

I will be happy to give you any information I can, and give here a few notes which may help to guide your enquiries

I am retired now. Since I left radio-astronomy (and the army) at about the time the cold war started, I have been in fuel research and, latterly, in coating. I am quite out of touch with astronomy, apart from what I learn from seeing Scientific American each month. I am also out of touch with other members of the team. Hey continued in research at Malvern, but must be retired by now, and perhaps no longer alive — you may know.

The work was done in a mixed civilian and army research group (Army Operations Research Group) of which the radar section was led by Hey, and which I joined in 1944. We had been tracking V.2's on 5 metres and in the course of this and other work had noticed interference coming apparently from the sun. On Hey's initiative, the team directed their efforts this way at the cessation of hostilities. I was working in the field in northern Europe up to May 45, and by the time I returned the work on radio astronomy had already started. ~~(I was the only army member of the team)~~

way he directly involved?

The team consisted of Hey, Gordon Stewart (in his mid-twenties) Parsons (technical & admin. mainly) and myself. In the first place Hey exercised general supervision (and inspiration of course) Stewart looked at-

notes

turned out to be galactic radiation. Radiation from the quiet sun was not detectable on our equipment (an array of 4 horizontal Yagi's rotatable in azimuth with an effective elevation of the beam of (I speak from memory) about 12°.

I set out to survey ~~the~~ as much of the galactic radiation as we could see with our limited manoeuvrability, & I had to invent what later came to be ~~the~~ known as a relaxation method to compute the results. This resulted in the work written up in the Proc. Roy. Soc. paper. This was essentially my work, with the contributions from Hey as noted above.

There were two spin-offs. I happened to be sitting ready and waiting when the sun blew up.

when?

It wrecked my survey, but I was able to log radiation from the active sun ~~in~~ a way that had not been done previously. I alerted various other groups that I thought might be interested, and the whole thing became much bigger, and was taken over by Appleton, who appropriated my results with the barest acknowledgement.

what?

More interestingly, amongst the troublesome and increasing sources of interference in the post-war world, we – no, I – detected one with a period of 23 hrs. 56 min., and this turned out to be the first of the "radio-stars". I did what I could to fix it with the very poor resolution of our equipment (another source of interference was the first television transmission from Wimbledon, which I blew off the air during the "run-up", and which I (magnanimously) made way for – they had no idea where the trouble was coming from until I told them!)

cal. oscillator in balloon

This was partly
because we lacked the scientific background (Hey was the only physicist amongst us – I was a mathematician, with a veneer of wartime radio) partly because we had to move our trials ground out of London (Richmond Park), where we had been during the war and then, before we got going again on the new site, the cold war started and the army directed us to more serious things. I pulled out too at this point, went back into my ~~previous~~ pre-war occupation of teaching, for reasons I find it hard to explain or understand now, left that after two years because I found I could not rear a family on the pay, did twelve years in fuel research here and in Australia, and finished up in teacher training, for reasons I will not weary you with.

I am now retired, active, living in a lovely part of England (the Cotswolds) in a cottage with a panoramic view, and if it would please you or assist your researches to visit here we would be delighted to have you. (train to Cheltenham or, possibly, fly to Staverton nearby where I would meet you).

Let me know if I can be of any help.

Sincerely

James Phillips

(after interview)

Hillside Cottage, Stockwell Lane,
Cleeve Hill, Cheltenham Glos
16th Oct. 78

©

Dear Professor Sullivan
(or do I now call you Woodruff, or what?)

Thank you for your note, and the extracts from Hey's book. I thought it best to read the book further before I replied, and since it is now out of print, and not available in the local library, it took a little time to get hold of a copy.

In answer to your question – no, I do not take issue with Hey's account. I would have given different emphases, much less to meteors which, although the work was fun (I had virtually nothing to do with it) had much less important implications than that on the Sun, and even more so that on galactic and extra-galactic radiation. Also, I feel that the ~~discovery~~ description of the discovery of the first discrete source as "yet another startling discovery" (Pp23 & 43) was rather inappropriate, superficial even. Shklovsky's description (P.82 in the Harvard translation) was more suitable. ~~But~~ Perhaps this gives weight to my (personal) assessment, that Hey was more interested in work on meteors and solar activity (in which he was initially involved) and that the work on galactic radiation, and the recognition of the existence of a discrete source, was essentially my contribution, although Hey initiated the survey. Obviously we both of us failed to pursue the work in the face of obstacles. I think that perhaps the only one around who really appreciated the significance of what we had found was an assistant of mine, Bernard Spencer, who made up in dogged determination what he lacked in academic background and who, if he had been leader, might have broken his way through all obstacles. I don't know what happened to him. He was of the stuff of which researchers are made.

I thought that Hey's comments on the liaison (Treason?) with Appleton (Pp 18/19) were well phrased and accurate. Certainly the only time I saw Appleton on the site was when he was there for the press visit. Since it was my work that opened up the programme of observations on the 1946 sunspot, I might have expected a closer contact. As I have already indicated, I thought the acknowledgement of my contribution was very slight (Phil. Mag. Feb 46 P76 footnote)

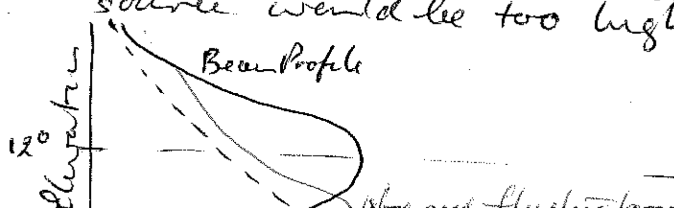
Perhaps we all do it. I suspect that Hey tries to make too much of his contribution to the beginnings of Jodrell Bank.

superimposed in the passage through the upper atmosphere. I was well out of the field by the time these findings were made known but I certainly welcomed the finding. The suggestion in the 1946 paper, that the source must be at most a few light-seconds in size, was very difficult to maintain with conviction. As we hinted there, the magnitudes don't fit. Of course, we kept all our options open, but our last comment (P445) turned out to be the most relevant. (Proc Roy Soc, 1948)

For my content, the realisation where and how the variations were superimposed cleared up what seemed to be gross errors in our original attempts at fixing the location. I have not discussed this with anybody previously (I was out of touch by then) and no one apart from myself has the necessary information to reexamine the results. Of course it is all ancient history now, and probably of no interest, but since you seem to be interested in details I pass it to you for use as you like. All this, like deducing the existence of a discrete source, was very much my work. If I had not done it, nobody else would have done

Some fuller discussion then;

Our attempt at locating the source was based on the observed amplitudes of the fluctuations, and these were a product of the intensity of the Cygnus signal, and the intensity of the disturbance in the atmosphere, integrated over the aperture of the radio beam. The figure so obtained was attributed to the direction of ~~a source located on~~ the axis of the beam, and the direction of greatest fluctuation was taken to be the direction of the source. If any of the factors involved departed much from circular symmetry, the assigned direction could be in serious error. The effect of the disturbed atmosphere is certainly such a factor, especially at elevations of 12° and less, increasing rapidly as the elevation decreases. The effect would be to give greater fluctuations when the source was below the axis of the beam than when it was on it, and the elevation assigned to the source would be too high.



The effect would vary with time of day and time of year. Our observations were made at two times of the

the day, as Cygnus set in the N.W. (10 hrs GMT in June & 02 hrs in Dec) and as it ~~rose~~ rose in the N.E (16 hrs & 08 hrs respectively). The fluctuations observed in summer (and daylight) were, on the whole, much more intense than those in winter. (night or early morning – December sunrise in these latitudes is about 8 am) (See Proc. Roy. Soc. paper Pp 436 & 444.)

I enclose original records of some sets of observations which were prepared direct from the log book (which I do not have). The slightly curved lines of ~~scan~~ represent scans in azimuth each made within a short period of time, adjacent lines representing successive scans. Scans thus are for Cygnus setting, thus for C. rising.

If my contention is correct, the true location of the source was different from the assigned location in directions thus [or, lower δ's] and

Averaging results would lead to cancellation of errors in R.A., but not in Declination.

Our estimates were:

	α (hrs)	δ (°)
June 46	20.00	+43
Dec 46	20.30	+38
c.f. Shklovsky P.124 (1960)	19.58	40.36

The June results (which were much the more difficult) were well within estimated accuracy for α, and, allowing for the errors discussed, reasonable for δ.

I could go further, but not all the information is in convenient form so it is hardly worth it. [more info. available] It is interesting that for the winter observations the assigned bearings of the source setting and rising were notably asymmetric about N (318° & 18° or 26°), and the fluctuations observed for Cygnus setting (N.W. 02 hrs GMT) were notably stronger than for Cygnus rising (N.E. 08 hrs GMT). It was nice to realise why these discrepancies occurred, as one could once the ~~cau~~ cause was identified.

I do ~~not~~ have any photos, and not much in the way of anecdotes. (I think) The incident with Wimbledon T.V. occurred as we were ~~fol~~ about to fold up the trials ground (which was monopolizing part of Richmond Park, to the exclusion of the public.)

of operations research and tried to set up a parallel all-military organisation. I left in '48. We would never have had the money to build another "mat" (artificial ground), which owed its existence to the demands of war-time gunnery, and which was by then obsolete. The mat was about 120m across, suspended, with a cat-walk to the cabin. Some of the early ones were built with a ground-clearance of as little as a foot in places. When it was realised that grass growing up through the netting affected the calibration, the army seized on (with glee?) an entirely new "fatigue" — belly-crawling under the mat to cut the grass with shears! Ours was more manageable, and the worst we ever had to do was to crawl under to free a deer tangled in the supporting wires.

The night-through vigil (walking out across the cat-walk to make a scan, returning, perhaps to brew coffee, or set the alarm for what seemed a few minutes ahead) was quite an experience. It helped to have some beer, but at that time beer was in short supply (something that never happened in war-time!) One evening I went to the King's Head in Roehampton village for supplies (in uniform, riding an army motor-bike) and found it closed — out of beer. Time was short before I was due to start the night's work so I rode at furious pace to the Green Man at the top of Putney Hill (of literary fame), to find that it too was closed. As I coasted to a standstill, dejected, feet flat down, one each side, I became aware of ~~[illegible]~~ two figures doing likewise, one each side: — speed cops! They asked was there a hurry? When I explained, they quite understood, and wheeled away to leave me to face a lonely night without beer.

The (Feb) 1946 sunspot arrived, fortuitously or not according to one's point of view, just as we were setassistants to start the survey early (probably about 8am) and myself arrived soon after to make sure everything was all right (having been up until 3am. the night before leading the life of a young un-attached soldier). 'All was dismay' — we had been blasted off the air by a newly-risen Sun. We abandoned the survey and watched the sun (having alerted everyone I could think of) The Sun remained active for months, and it was not until mid-year we resumed the survey and discovered the Cygnus source.

I was interested to find when you asked that I did not know Parsons' fore-name (or Christian name as we called them then). It might have been Stanley. I was not sure of Hey's even, ~~[illegible]~~ It sounds unbelievable in these days, but it is quite likely that we did not use them. At school, at work, and in the army, it was usual to use surnames except for close friends

actually Sydney John

I don't think there is much here you can make use of, but let me know if anything else occurs to you, and let me have the charts back some time

returned 3/79

All the best

James Phillips

CSIRO

10/3/78.

WITH THE COMPLIMENTS OF

MINERALS RESEARCH LABORATORIES

I hope the attached will be of some assistance

Ken Donaldson

DELHI ROAD
NORTH RYDE, N.S.W.

P.O. BOX 136, NORTH RYDE
N.S.W., 2113, AUSTRALIA

J.W. Phillips

Date unknown

"probably 1955 ish"

March 28, 1979

Mr. J. W. Phillips
Hillside Cottage
Stockwell Lane
Clewe Hill, Cheltenham
Gloucestershire, ENGLAND

Dear Mr. Phillips:

A much belated, but nevertheless sincere, thanks for your long, informative letter of 16 October. I was delighted with your comments on Hey's book, anecdotes about observing, and explanation of the first analysis of the Cygnus fluctuations. At one point, you wrote, "I could go further, but not all the information is in convenient form". Does this mean that you have other original data or calculations? If so and you are willing, I would be very interested in hearing about it or seeing it. I enclose your original charts on the fluctuations - thank you very much for the loan. I will be in touch with you again when I send you the transcript of my telephone interview for checking.

Sincerely yours,

W. T. Sullivan, III
Associate Professor of Astronomy

WTS:cv

Enclosure

Department of Astronomy FM-20
University of Washington
Seattle, WA 98195
U.S.A.

11 February 1985

Mr. James W. Phillips
Hillside Cottage, Stockwell Lane
Cleeve Hill
Cheltenham GL52 3PU
~~Gloucestershire~~
England

Dear Mr. Phillips,

It has now been over six years since I interviewed you over the telephone for my project on the history of radio astronomy, but I have not forgotten your helpfulness then and in your letter of October 1978. Under separate cover I am sending you the transcript of that interview for your review and for signing a release form.

I have now completed drafts for two of the chapters in my book _A History of Radio Astronomy_ (Vol. I). Under separate cover I am sending the portions (a) based on information which you have given me, (b) relevant to your participation in the development of radio astronomy, or (c) relevant to your interest in the history of the subject. I am extremely interested in your comments on this draft, in particular with regard to the following points:

(1) historical accuracy
(2) fair emphasis of the events and issues
(3) any omissions of important ideas or developments
(4) the "flavor" of the times -- has it been captured?
(5) style of writing and presentation

Besides the above general areas, there are the following points:

(1) I now formally ask for your permission to use the Fig. E.3, which you supplied, in my book and in any other articles on this subject which I may write. Please send me your written permission.

(2) The enclosed sheet of questions covers many points on which I need your help. Please answer those which you can.

If you wish, return the draft copies to me with your comments; otherwise, a separate sheet (referring to page numbers in the draft) will be fine. If at all possible, I urge you to cite publications or to send me photocopies of supporting documentation (reports, memos, letters, notebook entries, etc.) to buttress any arguments which you may wish to make. All comments will be seriously weighed; based on these I will then develop the final version of the chapter.

Finally, if you have any photographs (of which I may not be aware) relevant to the material in this chapter, please send me prints (with full captions) and indicate whether I may keep them or should make copies and return the originals to you.

It will greatly aid the schedule for the book if your comments could reach me by 15 March, but if this is not possible I will still profit from comments received on a later date. I thank you in advance for your time in reviewing this draft and I trust that you will find the end product worth your effort.

Sincerely yours,

Woodruff T. Sullivan, III
Associate Professor of Astronomy

HILLSIDE COTTAGE
STOCKWELL LANE
CLEEVE HILL
CHELTENHAM, GLOS.
GL52 3PU
Bishops Cleeve 2063

10th March 1985

Dear Mr Sullivan,

Your letter (originally 12 Feb, redated 22 Feb) took 11 days to reach me, (5th March) so that it was rather unrealistic to expect a reply to reach you in a further 10 days (15th March), even if the reply had been simple and straightforward, which it isn't.

pp 11-12 of MC1

I am very concerned about your comments on the method of analysis ~~of~~ in my Proc. Roy. Soc. paper, ~~both~~ on account of both their ~~its~~ contents and style. The style I find dogmatic and patronising. The statements contain misrepresentations (arising perhaps from ignorance or carelessness, and not necessarily deliberately) and errors of fact, and these comments, which amount to a total demolition of my analysis, are put forward without a shred of evidence or analysis, and also without a reference. I find this very remarkable in a supposed scientific paper.

I shall send you a detailed refutation in due course. Meanwhile I ask you to write to everybody who has received these pages (Chapter E, pp. 6&7) withdrawing these remarks until you are in a position to substantiate them. I see you have been in touch with Hey & Parsons, and I should be glad to have their addresses so that I may ~~alert~~ alert (and perhaps reassure) them.

In due course I shall write to Lovell, to Ryle's successor at Cambridge, and to CSIRO, to present my refutation of your statements. !

Finally, I think I am entitled to some explanation why it has taken over 6 years for you to put these most dismissive criticisms to me, after what you describe as my helpfulness, and why your comments finally reached me with no time for me to make an adequate reply.

Yours sincerely,

J W Phillips

see Bracewell, pp 172+174-5 of [illegible]

Department of Astronomy FM-20
University of Washington
Seattle, WA 98195
U. S. A.

30 July 1985

Mr. James W. Phillips
Hillside Cottage, Stockwell Lane
Cleeve Hill
Cheltenham GL52 3PU
England

Dear Mr. Phillips,

Thank you for your letter of 10 March, to which I must apologize for not replying sooner. Initially, I was waiting for comments on my draft chapter from Parsons (from whom I have still not heard) and further from you (you mentioned that you would send me "a detailed refutation in due course"), but then most of the delay is simply that I got off on other projects and other aspects of the book I am writing. In your letter, you asked why it had taken me over six years to write the draft, after I had interviewed you. The simple answer is that my project on the history of radio astronomy is now 14 years old, and has all along strictly been a one-man, part-time effort (most of my time is spent doing astronomical research and teaching, although I have recently cut back on the former in order to concentrate on writing this book). I have found that it takes a great deal of time to gather the material, digest it, and try to put it together into a coherent picture.

But of course the substantive point of your message was that you were upset with my account of your method of analysis in the 1948 Proc. Roy. Soc. paper. I apologize for any offense I may have caused you, for that was surely not at all my intention. I am simply trying to present the historical development as accurately as I can, and in fact this is precisely the main purpose of sending out these draft chapters to participants such as yourself. Since you gave no specific details, I cannot know exactly which parts of the draft you find objectionable and/or wrong, but nevertheless I have reconsidered my discussion and I see several desirable changes. In particular I have studied the papers of Bracewell, who from the 1950s on has been considered the expert on these matters, and I believe I should modify my remarks, e.g. the method was not "very soon" challenged by other workers (Bracewell's key paper on this was not published until 1954) and the analysis is not "improper". I enclose a copy of a 1958 Bracewell paper which, if you have not seen it, I think you will find interesting. He considers in particular the effects on any restoration of the inevitable noise in any real measurements, but in your case the measurements, it seems to me, were of reasonably high signal-to-noise ratio, and so noise did not obviate the method.

I suggest that you send me your comments on the draft, and I will then send you the revised version, which I trust will be acceptable to you. But please also send me your comments on the entire chapter, return the transcript of your interview, sign permission forms, etc. Finally, you asked for the addresses of Hey (who has sent me detailed comments, but did not raise any issue with my treatment of the method of restoration) and Parsons (address dates from 1978, and no reply received this year): Dr. J.S. Hey, 4 Shortlands Close, Eastbourne BN22 OJE, East Sussex; Mr. S.J. Parsons, Arbury House, Lapworth, Warwickshire.

Thank you for your efforts spent on all of this.

Sincerely yours,

Woodruff T. Sullivan, III
Associate Professor of Astronomy

WTS3/vax

(Sent me a carbon!)

27th September, 1985

Dear Professor Sullivan,

I was glad to have your letter. Certainly I was angry when I wrote, I think with reason, and your letter helps. I have put a lot of thought and work into this reply.

Let me put before you what you wrote. (1) 'Phillips . . felt that he had sufficiently 'sharpened up ' detail to allow one to discriminate features fully ten times smaller than the antenna beam area . . (2) The ultimate test of the method's validity was taken to be the consistency check of convolving the finally derived model distribution with the antenna pattern and seeing how closely it matched the original observations. (3) In its actualization Phillips felt that the resolution . . was 4°, a vast improvement over his antenna's 14° main beam. (4) As an example, a measured width of the galactic plane emission of 31° was transformed into a map value of 15° . . (5) it should be noted that (this method) was very soon challenged by other workers and (6) shown to be an improper analysis, a case of trying to pull too much out of incomplete data. (7) Radio astronomers learned that they had to live with the poor angular resolution of their antennas, and (8) that a <u>consistent</u> result was not necessarily a <u>correct</u> one. (9) For the map derived from Phillips's method was unfortunately only one of many which could be similarly derived, with no way of discriminating between the various possible 'restorations'.'

I am unable to read this as anything other than a direct attack on my competence and scientific judgement. in this instance, only! The whole paragraph, with its sweeping criticism of the work, is put forward with no supporting evidence, no analysis, and no reference to authority or other work. It must be left to your judgement whether this 'faction' style is appropriate to a history of a science, but at least you should get your facts right; nearly every statement and inference in this paragraph is wrong, not least the 'feelings' you ascribe to me.

Let me take the points individually. In (1), (2), and (3) you attribute to me claims of your own invention, that in <u>(2) being nonsense</u>. (4) is correct, the only statement in the paragraph that is on present evidence, but in it you you hold up to what, in the context of <u>(6)</u> I can only take to be ridicule, a result which turned out to be a very good one. In (5) you have now withdrawn 'very soon', but 'challenge' remains. You have sent me Bracewell's paper (Proc. I.R.E. 1958, 46), presumably in support of (5) - (9) - the only evidence you have produced so far. It does not support them, which is just as well, because (8) and (9) are certainly incorrect and, on present evidence, the others also.

Taking the points in detail:-

(1) & (3) Angular resolution. In the course of three pages of mathematical analysis we wrote (P.434) 'The section of least solid angle . . represents the limit of resolution <u>for this method of analysis</u> (my underlining). The size of the axial section was chosen to be 0.0044 steradian (or 14 square degrees).' Presumably it is this exact statement which you have transcribed into popular form, and in doing so have misrepresented it as a claim to having achieved a resolution of 4° 'in actualization'. A mathematical limit is a theoretical limit which, in a practical situation, perhaps may not be even

approached because of the intervention of some other factor - in this case the imperfection of the data. Nowhere do we claim or imply a resolution (to half-power) better than about 12° plane angle - roughly the angle of the beam. (The statement (P.435) that 'The angular subtensions shown for these features . . may exceed the true dimensions by an amount not greater than 2°' is based on a synthesis of a complex situation; it may be thought over-ambitious, but it is not directly translatable into an estimate of angular resolution) The stated limit refers specifically to the method of analysis, not to the investigation as a whole.

This was part of my reasoning here

(2) 'The ultimate test of the method . .'. The 'ultimate test' of a mathematical process is to examine the logic; a less-than-ultimate test is to examine results against some independent criterion. In a scientific method there is no 'ultimate', a word more appropriate to journalese than science. However, in this case, the statement is wrong in both ~~xxxxxx~~ senses. The process of convolving assumes the validity of the method, which cannot be used to test itself. What is being tested here is the derived hypothetical distribution.

OED + W's 3rd both say "refining" + final results

Poor wording on my part

(4) Bracewell says (P106) 'It is therefore worth emphasising that the merits of the procedure (of successive approximations) rest on practical verification.'. I would expect that before you cite a result as an example of 'improper analysis, a case of trying ~~of trying~~ to pull too much out of incomplete data' you would check it against later findings. If you had done so you would have ~~done so you would have~~ found that the result was good, and I would expect you to say so, even if you regarded the result as a fluke, rather than leave the reader to assume that it was rubbish.. You might even have asked ~~xxxxx~~ yourself 'Why'.

(see p. 4 of letter)

(5) The challenge came how many years later - six, eight ? Hardly 'very soon' (which you have withdrawn), Is Bracewell's paper part of the challenge? His only direct comment on our paper is (P106) ' the results were very good in that one or two stages of adjustment yielded a corrected distribution compatible with the observations, to their order of accuracy.'. A very muted challenge ! It is unfortunate that he invokes the order of accuracy, which we did not mention in this context, and which seems to me inappropriate. The criterion for stopping a sequence of successive approximations is that changes in the derived pattern become irregular or acceptably small, in this case the first.

(6) You have withdrawn 'improper', but the rest still stands (see below).

(7) Presumably based on Bolton + Westfold's (1950) Bracewell's paper. What he wrote was (P.106) 'A mathematical argument (not ours) . . was later shown to be erroneous and has contributed to a reaction against the method itself.'. He notes (P.107) that recent publications have omitted a correction for antenna smoothing, and goes on to say (P.111) that 'when the level of error is moderate, a considerable degree of restoration is warranted . . Two stages by the method of successive substitutions . . when the error level is as much as 20 per cent . . In recent published work where correction has been omitted there is no indication that the relative errors are this great.'. Not much support there for your 'Radio astronomers learned that they had to live with . . ' !

(8) Bracewell again? First I note that he discusses only one aspect of the problem, and that by his own admission (Conclusion P.111) he stops short of direct practical application. He tests his findings in a very simple

theoretical ~~model~~ one-dimensional model. He makes no mention of the importance of stable and rapid convergence. On the subject of multiple solutions he says (P.106) ' . . a distribution compatible with observation was not unique. . . the self-checking feature of the method of successive substitutions was shown to engender false confidence, for an infinite number of distributions would possess the self-checking property.' He goes on to qualify this heavily (which you do not) by reference to ' . one, the principal solution, which is distinguished by absence of those spatial Fourier components which would be rejected totally by the antenna . . is best fitted to represent the conclusion.' He goes on to say 'However, it is very interesting to note that, under certain ideal conditions, the method of successive substitutions itself leads, in the limit, to the principal solution. One of these conditions is freedom of the observed data from errors.' The rest of the paper explores what degree of error is tolerable, and concludes (P.111) 'Some special cases show that when the level of error is moderate, a considerable degree of restoration is warranted. In particular, two stages of correction by the method of successive substitutions are warranted when the error level is as much as 20 per cent . . In recent published work where correction has been omitted there is no indication that the relative errors are this great.'. We estimated our errors at 5 to 15 per cent (the lower figure applies to regions of high intensity, which are the ones of main interest). So what is your justification for such a sweeping and conclusive dismissal of all attempts at restoration, and of mine in particular ?

You do not say whether your unqualified strictures apply because the principal is wrong, or because the data are imperfect. If the first, then as I see it there is a very simple argument against. The mapping (in the mathematical sense) from the celestial distribution through the characteristics of the antenna beam onto the observed distribution is single-valued and linear (in the sense that distributions are additive in superposition). So also must the reverse mapping be. Else if two different celestial distributions can map yes to the same observed distribution, then by differencing we can derive a non-zero celestial distribution which maps to an identically zero observed distribution. This argument would not apply if the mapping were non-linear, as would happen for instance if the celestial distribution were variably polarised.

Of course if the data are imperfect and inconsistent there will be no unique solution, but except in the trivial sense that there is room for an infinity of solutions within finite limits of error, there will not be different solutions. If the data are grossly in error, the defect will show up as poor convergence. Bracewell identifies some probable causes, variable ground conditions and different orientations of the beam, for instance, and our system survives inspection fairly well. Poor convergency would be more likely ✓ if the distribution were rather featureless, which it certainly is not. As I remember, I was uncertain whether to show the feature at (18.20 hr.,-25°) as a peak or as a buttress, but the difference was small and did not, for instance, affect the estimate of the half-power width of the galactic band.

It must be remembered that using spot values to plot contours involves a large element of judgement (as in the case mentioned above for instance) and a map must be viewed accordingly. We are looking for a pattern, which is something the human brain is very good at. For instance, the numeral 5 can be formed in Roman script, italics, broad pen, shadows, curly bits, cherubs, monkeys' tails, coloured dots amongst coloured dots, and so on. It would be very difficult to programme a computer to recognise the numeral in all of these forms; a child of five can do it with ease. We explored a pattern by traversing it with a lattice of two intersecting rasters; uncertainties in restoring any one traverse in isolation were much reduced when observations on neighbouring and intersecting traverses had to be reconciled. Fortunately the observations were close-packed in the regions of most interest. I enclose a map which shows the locations of observations at intervals of 1 hr.S.T. and 10° azimuth; we observed at half these intervals in the high-intensity regions.

I have examined our work in the light of later reports, and it has stood up well. I do not have ready access to a scientific library, and I have used as my source Shklovsky's Cosmic Radio Waves in the Harvard translation (which is updated to 1958). Page numbers refer to that. Rather sadly, Shlovsky seems not to be aware of our 1948 paper, and refers only to the brief preliminary note in Nature 1946, and so does not give us full credit. I do not have the original records of our work, and must use our 1948 paper.

Our estimate of Shklovsky the southward bias of the main galactic centre (-1° galactic latitude) accords well with Shklovsky's preferred values of -1 to -1.4° (P.51), our peak at (18.22,-12½) with Blythe's (18.24,-12½) (P.69), and the Cygnus peak (galactic 44,+1) with Hanbury Brown and Hazard's double peak at (44,+4), (46,+1) (P.47). The intensities accord in Piddington's survey (P.54) and elsewhere. There must be a doubt against our second peak at (18.20,-25), which nobody else shows. I remember that I debated whether to show this as a minor peak or a buttress (it was a matter of 'judgement'). This area is not covered in most of the surveys at comparable wavelengths. Blythe (P.69) just fails to include it, but significantly his contours very definitely indicate a buttress or rise 'off-stage'. Baldwin (P.48), Kraus and Ko (P.46), and Allen and Gum (P.37) do not show it, but neither do they show peak at (18.22,-12½) which is independently confirmed. So I think that we can claim the case 'not proven'. Interestingly, Kraus and Ko are cited by Bracewell as a case where correction for beam-width was not made, but could have been with advantage. Perhaps they would have found both peaks !

but λ matters very much!

The example of (improper?) restoration from 31° to 15° at λ 4.7 m that you cite is on the traverse at 19hr.S.T. Maps at metre wavelengths give the following results. All figures are estimated, sometimes with difficulty (19hr.S.T. corresponds roughly to galactic longitude 330° in this region)

P.37	1.5m	Allen & Gum	1950	8-9°	beamwidth and correction if any not stated. (in Shklovsky)
P.46	1.2m	Kraus and Ko	1955	11-12°	b.w. 1°R.A.x8°d. no correction
P.48	3.7m	Baldwin	1955	18°	b.w.1°x7°
P.61	3.5m	Mills	1956	5°	b.w. 50'. The map shows the 2° bright band which dominates the profile, and which could not be detected by our antenna, of course.

Other maps can be used to obtain widths of other sections (e.g. P.69 l=353°, P.67, d=-29.5°, P.69 21hr.S.T.), which yield widths of 12 - 15° (in the first two by ignoring the bright band). I have also compared the widths across the peak at $l = 350^{\circ}$ (Pp.46,65,67), with good agreement at about 12°, and at $l = 45^{\circ}$ (P.47), again with reasonable agreement (our 15° against 10°). Our results nestle quite nicely amongst the others. Presumably you attribute all this to coincidence; for myself it illustrates the well-known fact that the person who actually does something gains experience which is not available to the person who uses a theoretical approabh, which is almost certain to be incomplete.

I am at a loss to understand why you should have written in this manner. I take it seriously; after all these years, and at my age, I am vulnerable. It is only too easy for youth to take the line 'the old folk, they did their best of course, but they did not really know what they were doing', and your comments imply exactly that. our ~~my~~ contemporaries, such of them as survive, are no longer active in the field, and your comments will go unchallenged except by us. They are wrong in substance, and they misrepresent and denigrate our work in a quite irresponsible way.

The paragraph is a shambles. I ask you to withdraw it, to inform whoever has seen it that you have done so, and why, and to inform me by early return. If you wish to, you will be able to write an acceptable substitute on the basis of this letter. When this matter is mended, I shall be happy to resume cooperation with you.

I shall send a copy of this letter to Hey and Parsons of course - thank you for the addresses - and perhaps to one or two others who may be interested.

Finally, the footnote (12) to page 6 is out of context. The 'young women' may have worked for others in this way, but not for me, not in this capacity. My assistant was a young woman, a civilian. The story (of her, I presume) is personal, and to me ~~more~~ interesting, but not to others perhaps.

Yours sincerely,

J. W. Phillips

10/14/87 – I call him from Cleeve + he has no good excuse for the delay + is very apologetic
– he is not upset w/ me any longer, + w/in 2-3 wks will send me all the stuff, along w/ some corr. he had w/ F.G. Smith + Lovell re this controversy (1982?)
– he only recently saw Hey's SRA + was very upset w/ its "I, I, I" approach

Department of Astronomy FM-20
University of Washington
Seattle, WA 98195
U.S.A.

(024267)
Bishop's ~~Cleeve~~ Cleeve 2063

27 January 1986

Mr. James W. Phillips
Hillside Cottage, Stockwell Lane
Cleeve Hill ← 10/29/87 – we went right through this town on our "Stratford trip", but I decided not to try to make contact
Cheltenham GL52 3PU
England

Dear Mr. Phillips,

Once again I am extremely tardy in replying to your letter (of 27 September) and I apologize.

I have carefully considered all of your arguments and am very impressed with their strength and essential accuracy. I am grateful to you for spending such a great deal of time and effort on this, for my original ideas were clearly wrong. I therefore have dramatically modified the paragraph in question and I trust you will find the enclosed new version acceptable. As you requested, I am also sending this corrected version to the other persons to whom I sent my original draft.

I trust that you will find this now satisfactory and will now be able to respond to the various items that I sent you last February: comments on the draft chapter, responses to the sheet of specific questions, checking of your interview transcript, vita, and various permission forms (of which I enclose a new one).

Thank you for your cooperation and I look forward to hearing from you.

Sincerely yours,

W. T. Sullivan III

Woodruff T. Sullivan, III
Associate Professor of Astronomy

WTS3/vax

cc A.C.B. Lovell, G.S. Stewart, S.J. Parsons, and J.S. Hey.

27 January 1986

New version of last paragraph of Ch. E, Sec. I

-- Woodruff T. Sullivan, III

One further feature of the maps published by Hey's group deserves discussion: they do not represent contours of intensity as observed, but have undergone considerable further analysis. Phillips developed a mathematical method of successive approximations designed to alleviate their coarse angular resolution. By the time of the second survey the method had grown quite complex, but in essence it worked in the following manner. Phillips first put the observed intensities on a grid drawn on a large sheet of plastic. He then moved around a transparent overlay having the contours of the measured antenna pattern and estimated the contribution which arose at each measured point of the sky from the side of the primary lobe and from sidelobes. In this way the measured intensity value could be iteratively corrected for the responses of the outer portions of the beam in a trial-and-error fashion (Phillips 1978:9T). Results were checked by convolving the finally derived model distribution with the antenna pattern and seeing how closely it matched the original obervations. As an example, a measured width of the galactic plane emission of 31° was transformed into a map value of 15°.

① 2/86 – Smith → Phillips

Previous letters G.S. to JWP 4/2/86
JWP to G.S. Feb. 86
with notes. (enclosed here)

Copy of letter from G.S. 24.2.86.

I had no idea that a rather interesting controvery was raging! - I do suggest that a formal analysis could settle the matter. My non-rigorous approach, using Fourier components, tells me:

(1) The removal of sidelobe responses is entirely legitimate, and your process should be acknowledged as proper and appropriate.

(ii) An antenna cannot give a response to Fourier components smaller than an angular scale determined by the size of the antenna. For large radio telescopes this 'size' is close to the physical width of the aperture; for arrays such as yours it can extend somewhat beyond the actual width.

(iii) Fourier components which cannot be detected by the antenna can be added to the map without making it incompatible with the observations.

(iv) Fourier components, i.e. angular structure, near the limit may be recorded with low sensitivity, and may therefore be legitimately amplified in constructing the map.

(v) Even if the sensitivity to Fourier components actually cuts off at a certain angular structure, there will be some continuity which allows an extrapolation from measured components into the area not measured. The allowable extent depends on reasonable assumptions about the structure in the sky: for example, it must have no negative values of brightness.

(vi) Restorations using (iv) and (v) require an accuracy in measured components which increases rapidly as the restoration proceeds to greater angular resolution.

The above outline is the basis for my suggestion that you were correct in attempting a restoration to somewhere near the extent you achieved. You would certainly have gone wrong if you had attempted to go much further; your success at the attempted level is hard to assess, as it depends on the accuracy of your observations, and in particular the reliability of the finest structure which your records show. You did not ~~set out~~ explicitly set out the reasons for limiting your restoration process, and I think it is fair to say that these limitations were analysed later by others.

followed by my reply ~~24/2/86~~ March 86 (copy enclosed)

(2) 3/86 – Phillips → Smith

March 86. Reply to G.S's letter of 24/2/86

Thank you for your interesting and helpful letter. I am glad to accept your invitation to write again, but in doing so I am conscious that the subject cannot now have any other than historical interest, and if after this you feel that there is nothing more worth saying I shall be content.

I find that I have not moved appreciably from my position, but my reasons become clearer to me. Of the two lines of thought that are running, the widely accepted one based on consideration of Fourier components and the other, mostly mine, based on considerations of convergency, it appears at first sight that the former is more fundamental, but I believe that this is not so and that, in the context of the investigation under discussion, the first is a derived method and the second is more direct and fundamental. The special feature that makes this so is the aerial we used. Modern systems consist essentially of simple and well-understood components, perhaps assembled in large arrays such that each component can be regarded as being on its own. In contrast, the aerial we used consisted of complex components, which at that time were little understood, used in close proximity to each other and to the ground. The only way of characterising the performance of the aerial was to measure the polar diagram, and all analysis, mine or, typically, Bracewell's, must start from there.

Both presentations, in terms of Fourier components or a polar diagram, contain the same information. Presumably it is not practical to measure the polar diagram for a large array, and in any case the Fourier method offers the information in more suitable form for that application, but if the information comes in the form of a polar diagram, one must ask whether there is any point in converting it. Of course, an awareness of Fourier concepts is still helpful, in preventing one from attempting the impossible for instance.

Starting with the polar diagram, the essential steps in the Fourier method are, whether taken consciously or not,

A. Set up a hypothetical antenna of equivalent performance, e.g. an aperture having a suitable current distribution. (Your (ii))

B. This can detect large features, but not small – it is the gray area in between that is uncertain. To explore this one must make assumptions about the accuracy of observations. (Your (iv),(v),(vi))

C. A rigorous but limited analysis in terms of Fourier concepts can now determine the angular resolution that can be achieved.

In practice, A is only approximate. Possibly only one of the parameters of the polar diagram is taken into account, not the side lobes or the shape of the main beam. In B it is the random errors that matter, and these are mostly guess-work, so that in C the apparent rigour ~~is an~~

is an illusion. All three steps are open-ended and not self-checking.

In the alternative method we take the measured polar diagram and the observed distribution without prior assumptions and use them in a process of successive approximations which is robust and, fortunately, but not surprisingly, convergent. As in all such processes the individual steps do not affect the final result, which is reached when the corrections become irregular and of random sign. This point is determined by the errors in the data, the truncation errors, and the shapes of the polar diagram and the distribution. It is possible to continue to paint in detail, but only by ignoring the internal checks. The mathematical processes are not subject to arbitrary limits, and require no more than what I have called mathematical common sense. When I read Bracewell's paper (Proc. [illegible] 19[illegible]) I was dismayed by the string of unverifiable assumptions having unexplored consequences which he has to make to deal with even the simplest cases, and at the end the trumpet sounds with a very uncertain note.

Interesting

You suggest that I was just about [illegible] it, but that to go further would be pushing it [illegible] (true translation). I think that it was not just timidness to stop when I did; this is how I would expect the internal checks to operate. The fact that they allowed me to approach the limit in the bed of the Fourier concepts shows that the initial data were good; if they had been less good the convergence would have stopped sooner. One could argue that the resolution achieved was a measure of the accuracy of the observations, rather than the other way round, as Bracewell and others have done. Bracewell credits me with achieving a resolution compatible with the observations 'to their order of accuracy', but neither he nor anybody else indicates how this is to be attained in a steeply varying distribution. The phrase seems to me to be irrelevant in this context.

For your other points - firstly, as I see it - the success of the restoration must be assessed by reference to later results, but the process was robust, and I would not expect serious errors; I could have made it clear that approximations [illegible] convergent values, and perhaps the phrase 'devising values of [illegible]' was unfortunate, but I did say 'the trial values converge rapidly', which in the context of a process of successive approximation would be well understood.* The subsequent analysis of the limitations by others was, in my opinion and to the best of my knowledge, inconclusive and misleading, and as you will have gathered, I see no way to offer a formal analysis.

26/10/87. — by those experienced in the art.

(21)

Sent to G.R.S. as an addendum to my reply to his invitation (4/2/86) (not sent to WTS?) to send my thoughts on the limitations of the deconvolution process — Feb/~~March~~ 86

Notes supplementary to 'An investigation of galactic radiation in the radio spectrum' (Hey, Parsons and Phillips, Proc. Roy. Soc. A, 1948,192,425) and a letter J.S.H. to Prof. Sullivan 27.9.85. References are to Bracewell, Proc. I.R.E., 1958,46,106. and Shklovsky, Cosmic Radio Waves (Harvard transln.)

The validity of the method of correction for the width of the beam has been questioned. The description in the paper is rather laborious, and I was perhaps over- concerned to give a rigorous treatment. Some more detailed comment 'in clear' might have helped. I will try to give some here.

The first stage, of correction for side lobes, was very straight-forward. At this stage the thinking was in terms of regions of the sky of about the size contained within the main beam or a side lobe (which were of roughly equal angular dimensions). A source intensity was calculated for such a region by noting the power received when the main beam was pointing in that direction and treating it as though it came from a uniformly illuminated sky. Because of the highly directional pattern of aerial sensitivity the level of illumination so calculated was a useful first approximation to that in the main beam, and in this way illuminationlevels were assigned to all parts of the sky which could be viewed in the main beam. These levels were then used to calculate contributions from side lobes, and the power in the main beam was obtained by differencing, and was used to calculate an improved estimate of the illumination in that part of the sky. These improved approximations were used as they became available, and the cycling was continued until the corrections became irregular.

The second stage, of correction for the profile of the main beam, was rather different. Only the solid angle contained in the main beam had to be considered, and this was divided into segments of equal receptivity (to coin a word) in which the higher sensitivity at the axis was compensated for by a smaller solid angle. I see that I did not state the number of segments; it must have been about seven, one central and six in a ring, say. This way the calculation of a total power required the straight addition of ~~six~~ seven figures, one viewed in each segment. The sum was subtracted from the figure for the whole beam, which was known and which did not change, to give the required correction. This correction had to be distributed over the segments, and judgement was used in the manner of doing this which took account of the need for continuity in the distibution of the source, and of its emerging pattern. Improved values were used as they became available. The final result did not depend on the pattern in which the corrections were applied within the beam, but convergence was speeded up by using arrays which anticipated interaction with later corrections. The process was continued until corrections became irregular and, fortunately, small. The features of the method whereby adjustments were made first in the regions where the greatest corrections were required, and in arrays designed to speed convergence, had affinities with were treated formally in Southwell's development of 'reaxation' methods, but I did not know of his work at the time. And really, in this application, it was just common sense.

The transparent overlays carrying the information of the antenna pattern - one for the subsidiary lobes and one for the segments of the main beam - rotated like the cursor of a planisphere, and simulated the rotation of the earth. It was not possible to use an analogue for the rotation ~~of~~ in azimuth of the antenna because of the distortion in the projection, and the pattern had to be re-drawn for different orientations in azimuth. This was not very difficult - the lobes required only a few points to be marked, and the main beam only the boundaries of the few elements.

The main limitation, which applies to any wide-beam system, was that there were parts of the sky that could not be reached by the axis of the main beam, but only by extrapolation. In our case these were the circumpolar regions, and regions near the southern horizon, and no value of flux intensity could be assigned directly. The first appeared to be low-intensity regions, but the second included the galactic centre, the strongest source of all, and there must be serious errors at about -25° declination in our derived distribution. The attached maps show the traverses of the axis of the main beam at hourly intervals.

And now for the main question - why was there such a loss of confidence in the possibility of correcting ~~for beam width~~. Bracewell reports (p.107) that investigators who might with advantage have attempted it did not do so, and Shklovsky dismisses the possibility in very superficial manner (p.16). I can only think that the nature and potential of the process of successive approximation was not appreciated.

I suppose it started with Bracewell and Roberts who 'showed that a solution, i.e. a distribution compatible with observation, was not unique.' I do not have the original paper, and I quote from Bracewell (p.108). In fact, Bracewell does not make the sweeping rejections made by others who appear to have taken his ideas uncritically. I have quoted from him extensively, in my letter to Sullivan (~~the letter~~), where I note that he allows ~~substitutions~~ 'the method of successive substitutions' subject to conditions which are met without difficulty by our investigation, and that he says that later investigators who did not attempt to correct for beam width might with advantage have done so.

But my case does not rest on this. Really, the statement quoted above is nonsense. The only situation in which being 'compatible with observations' could be used to identify a solution is in the exact solution of an exact equation. Our work is certainly not of that kind, and of course nor is any that takes its information from 'observation'

Let me take a familiar example. When you evaluate the root of an equation, even an exact equation, by a method of successive approximations, it is useless to attempt to judge the quality of the evaluation by substituting back in the equation to see if you have a sufficiently good fit; the merit of the result is judged by examining the sequence of approximations. I say merit, because more than the accuracy is involved; there is also the question of robustness. Typically the accuracy is taken to be comparable to the last correction made, ~~but~~ and the robustness is indicated by the strength of the convergence. If, for instance, the equation is ill-conditioned, the value of the root is very sensitive to errors in the accuracy of the parameters of the equation, and in a practical situation the evaluation may be useless. In our case, the convergence was good because the source was so concentrated.

Bracewell refers frequently to the idea of compatibility with observations, which he associates with the concept of experimental error. For instance, in his Conclusion he says 'the process (of successive substitutions) is halted when smoothing the trial distribution gives a result agreeing with I_a within the experimental error'. Nowhere does he mention the importance of strong and stable convergency, in fact there is nothing in the paper to suggest that he gave the question of convergency any thought. Speaking of our paper he says 'It was found that the results were very good, in that one or two stages of adjustment yielded a corrected distribution compatible with the observations, to their order of accuracy'. Now this is a very curious statement. First, I did not say that one or two stages of adjustment were required; I said that often one, and not more than three, cycles of approximation were required in the first stage. For the second and more complicated stage I gave no figure for the number of cycles. Second, I did not mention accuracy of observations in this context, where it is irrelevant. Third, I did not invoke the idea of compatibility. One way and another, I have been credited with so many things I did not say that I begin to wonder whether anybody has actually read the paper, or whether they have just been told about it!

I have read the paper carefully, to see whether there is anything which could have led to a misunderstanding. I wrote 'Trial values of $\bar{P}_x$ were substituted in the equation $\bar{P}_n$ (the mean power in a segment, see above) were substituted in the equation and the calculated values of $\bar{P}_n$ (the mean power over the whole beam) compared with those already obtained[4]'. I can see that this might have been misconstrued, particularly by someone with preconceived ideas of compatibility. In fact it referred to a step in the algorithm (see the account above) like that in Newton's method, when the current approximation is substituted back in the function in the course of estimating the next correction. I also offered 'before and after' diagrams, which I thought might be of more interest to the non-mathematical. As I say, this might have been misunderstood, but what cannot have been misunderstood by a mathematical reader was that this was a method of successive approximation. I said so explicitly (P.431) and implicitly (Pp431, 432). I gave the source of the initial trials values, and described how improved approximations were calculated and used, and twice noted that the approximations converged rapidly. I did not spell out the features common to methods of successive approximation, but these should have been well known to a mathematical reader. After all, Newton's method has been around for quite a while! How it is possible for anyone to discuss a process of successive approximation (or successive substitutions, as Bracewell prefers to call it) without mentioning convergence I do not understand.

If a solution is plucked out of the blue, or obtained by a method which does not reveal convergence, and is tested only by compatibility, then it is true that, as Bracewell says, an infinite number of distributions would possess the self-checking property, or as Sullivan says (and has now withdrawn) 'a consistent result was not necessarily the correct one. For the map derived from Phillip's method was unfortunately only one of many which could be similarly derived, with no way of discriminating between the various possible 'restorations'[5]'.

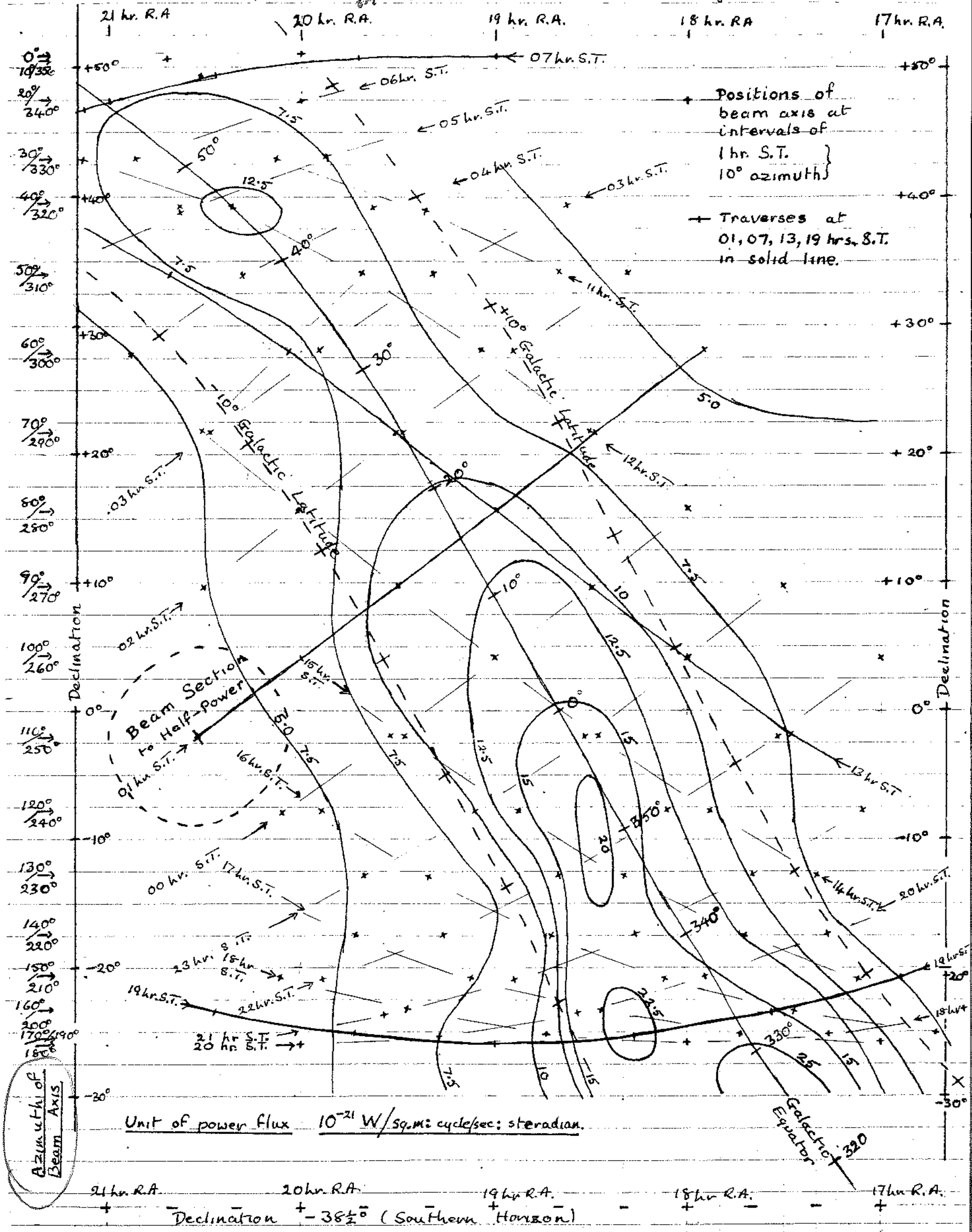
Bracewell and Sullivan do not make it clear whether these ambiguities arise only because the information is imperfect, and would not do so if the situation were exact, and Sullivan has not commented on this point which I made in my letter, so perhaps it would be as well to restate here the argument that I put there. <u>The mapping</u> (in a mathematical sense) from the celestial distribution through the characteristics of the antenna beam onto the observed distribution <u>is single valued and linear</u> (in the sense that distributions are additive in superposition). So also must the reverse mapping be. Else if two different celestial distributions can map to the same observed distribution, then by differencing we can derive a non-zero celestial distribution which maps to an identically zero observed distribution. This argument would not apply if the mapping were non-linear, as would happen for instance if the celestial distribution were variably polarised. How to reconcile this with Bracewell's 'infinite number of solutions' which contain 'spatial Fourier components which would be rejected totally by the antenna' I do not pretend to understand, but maybe we are not talking about the same thing.

As I say, Bracewell did not dismiss our work. Hey has sent me the following extract from Pawsey and Bracewell's book 'Radio Astronomy (Oxford, 1955), p.256. 'Surveys of the distribution of radio brightness over the sky have been carried out at frequencies between 18 and 1200 Mc/s. Of these, only two, those of Hey, Parsons and Phillips at 64 Mc/s and Bolton and Westfield at 100 Mc/s are reasonably complete'. They reproduce our map.

[Portion of final map]

Cosmic Radiation at 64 Mc/s.

(from Hey, Parsons and Phillips, 1948, Proc. Roy. Soc. A, 192, 425)



(from Phillips 10/87)

Copy of letter J.W.P to J.S.H

11th March 86

Dear Hey,

Thank you for the copy of the book. I had heard that you had written one and was interested to see what it was like. I found it very readable and informative, and generally speaking comprehensive.

However, I found the way you described the work of the group quite extraordinary, as remarkable for what you put in as for what you left out. The balance of emphasis must have puzzled an informed reader. The numbers of lines devoted to various topics are, approximately:

110 Meteors
65 Details of your successful career
30 The Sun
30 How you guided Lovell's early steps.
20 Discrete source
13 Cosmic noise

No doubt these figures reflected your own interest and involvement, and would have been appropriate in an autobiography, but they scarcely reflect the relative importance of the topics in a history of radio-astronomy. A count of mentions of the members of the group tells the same story.

The small space given to the last two items might have been justified if there had been nothing more to say. The twice you refer to the mapping of cosmic noise you have no comment beyond that the results resembled Reber's. Lovell, Bracewell and Schklovsky all said more than that, and reproduced the map. One would not have guessed from your account that ours was one of the only two that were 'reasonably complete'. But more remarkably you omit all mention of what was new - the attempt to allow for the width of the beam, the description of which takes up more than a quarter of the paper. Others thought it important - Bracewell, Lovell, Schklovsky, Graham Smith - but not the 'leader of the team'. I can now understand why Sullivan wrote as he did; he must have thought that we no longer believed the method to be sound.

Pawsey & Bracewell 'Radio Astronomy' P256 quoted by Hey

(not the [illegible] at all)

Schklovsky described the discovery of a discrete source as opening a new epoch in radio-astronomy . . an outstanding development, not only for radio-astronomy, but for astronomy in general'. The score-sheet above hardly gives that impression. Your account of the discovery is very bare, and gives no idea of how it was made. Compare it for instance with your long and colourful account of how you were first at the winning post in the Giacobinid show, or of Lovell's 'keenly pursuing the work I had initiated at AORG.'.

When you wrote in October you said 'I think the work of our group has never been given the credit it deserves'. Having read your book, I am not surprised. When you as 'leader of the team' lay down its most important achievements, not only is the work of the group devalued, but also your scientific judgement.

I enclose extracts from my second letter to Graham Smith.

Yours sincerely,

(J.W. Phillips)

HILLSIDE COTTAGE
STOCKWELL LANE
CLEEVE HILL
CHELTENHAM, GLOS.
GL52 3PU
Bishops Cleeve 2063

24th. October 87.

Dear Sullivan,

- please do not take this form of address as unfriendly. It is the one I was brought up with, and am most at ease with.

I have tried to get back into the picture as it was when we were last in touch:

I sent you my protest (27.9,85) and received your handsome apology (27.1.86) for which I thank you. I thought that one sentence in your r revised version was still off target (Results were checked . .) but the point was no longer of interest in the contemporary world of radio-astronomy and I thought I would let it go. Then, at about the same time, I heard from Graham Smith, who had heard through Lovell that you and I were in touch, but not that we differed, and who asked for my thoughts on the limitations of the deconvolution process I had used.

I enclose a copy of notes which comprised the substance of my reply, and of his reply in return; together they cover most of what I have to say. I think that at this point we realised that our separate approaches could be bridged only by using more time and skill than one or the other of us could command, and we let it rest.

I would add in postscript that any analysis of data cannot yield more information than there was to start with, and that a direct method is likely to lose the least. Sophisticated methods enable a better awareness of possibilities and limitations which are necessary in a general approach, but in a particular case the simple and direct method of an iterative process may have the advantage.

Also I think it is possible to be blinded by mathematical techniques. I gained my experience in pre-computer days, when the need for economy of effort was paramount, and methods yielding rapid convergence were essential. One acquired a very direct impression of the power of a convergent sequence, and I cannot see that a result obtained in this way can be 'wrong', provided of course that the sequence is not continued beyond the limits set by the convergence.

Also at this time I was in touch with Hey (I had sent him a copy of my original protest to you, and he had replied supporting me). I sent him a copy of my letter to G.Smith, and received a copy of his (Hey's) book, which I had not seen previously apart from the extracts you had sent me.

Che did [illegible] 1980 — see his letter to me of [illegible]

I was dismayed by what I saw. I had not realised that the brief comments in the extracts were all he had to say about my work (or, as he preferred to describe it, our work). My feelings are best conveyed by my letter to him, a copy of which I enclose.

I suppose that this was the last straw. I had long ago realised that by an alphabetical accident I was not likely to be remembered as the discoverer of the first 'radio star', and Hey's minimising my contribution did nothing to redress the balance. I hd a lot else to do, and so I let it all run into the sand.

but [illegible] E takes care of this

Now to return to your ~~xxxx~~ revised paragraph. I think you will see now why I think that the sentence is off target. I am not very worried - the point cannot now be of any interest to anybody but myself - but if you can without difficulty make a substitution the following is of about the same length:

'The iteration was contiued until irregularities in the convergence indicated that no further improvement could be made. In this way a measured width of galactic plane emission of 31° was reduced to a map value of 15°, which agreed well with later results.'

It remains only to wish your project well. If you feel like taking time off there is a bed and a meal here. We live in an eighteenth century cttage in the Cotswolds, one of England's beauty spots. If you come, I suggest soon, or delaying until the spring.

(the family)
10/29/88 – we went right through his town, but I decided not to stop.

Yours sincerely,

James Phillips

P.S. I see on re-reading your letters that you asked for my general comments on the extracts. I found your book very pleasant and easy to read, informative, and successful in recapturing the atmosphere of the time.

I note that you have a reference

(fn 28) "Some characteristics of solar radio emission" by H. P & P. AORG Rep. 5784 [5/48], which you say

correct is the only form it took. I have a reprint of the same title and authorship MNRAS 108, 352, '48.

P.P.S. It is gratifying to see my contributions so firmly recognised.

J.

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4 November 1987

Dear Mr. Phillips,

Thank you for your letter of 24 October and for its multitudinous enclosures, including copies of your correspondence with Smith and with Hey. I also appreciate the time you spent annotating the transcript of your interview. It is good at last to get all these matters cleared up. One item remains: I do not understand why you say (on the enclosed form) that you do not have the rights to Fig. E.3, which is your original worksheet that you sent to me many years ago. I am certain that you do have the rights and that my publisher will want me to have official permission (and of course I will give you a credit). Please do sign and return it. Thanks.

[never rec'd]

Finally, thank you for your kind invitation to visit your home in the Cotswolds. It does sound tempting and perhaps I shall be able to visit in the spring. Good luck with your house-building!

Sincerely yours

Woody Sullivan

W. T. Sullivan, III