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THE 'GREAT DEBATE': WHAT REALLY HAPPENED

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The meeting of the National Academy of Sciences in Washington on 26 April 1920, at which Harlow Shapley of Mount Wilson and Heber D. Curtis of Lick Observatory both gave talks under the title "The Scale of the Universe", has passed into the literature as "The Great Debate".[1] It is true that the two resulting papers[2] published in the May 1921 *Bulletin of the National Research Council* contain the best presentations of the opposing arguments in the current controversy over the dimensions of our Galaxy and the status of the 'spiral' nebulae. But these papers, even if read without comment or discussion, would have taken well over two hours to deliver and therefore cannot possibly represent the proceedings at 'The Great Debate', which took place at 8·15 p.m. with a *Conversazione* timed to follow at 9·30.[3] Nevertheless, most historians persist in treating these published papers as the *verbatim* record of a dramatic trial of strength, and so have created an historical romance. In what follows we draw on surviving archives to compile a more accurate account of what actually took place.

The encounter grew out of a remark which George Ellery Hale, founder and Director of Mount Wilson Observatory, made at a Council Meeting[4] of the National Academy of Sciences late in 1919. Hale suggested that an evening of the Academy meeting planned for the following April should be devoted to one of the annual lectures paid from the fund set up in memory of Hale's father, William Ellery Hale.[5] On 3 January C. G. Abbot, the Home Secretary of the Academy, wrote to Hale:

> You mentioned the possibility of a sort of debate, either on the subject of the island universe or of relativity. From the way the English are rushing relativity in Nature and elsewhere it looks as if the subject would be done to death long before the meeting of the Academy, and perhaps your first proposal to try to get Campbell and Shapley to discuss the island universe would be more interesting. I have a sort of fear, however, that the people care so little about island universes, notwithstanding their vast extent, that unless the speakers took pains to make the subject very engaging the thing would fall flat. . . . Are there not other subjects—the cause of glacial periods, or some zoological or biological subject—which might make an interesting debate?[6]

It is a little surprising that the island universe theory of spiral nebulae—the claim that the spiral nebulae are galaxies in their own right and independent of our Milky Way star system—was to be defended, not by Curtis but by his Director at Lick Observatory, W. W. Campbell. For Curtis had been engaged for nearly a decade on the photography of nebulae with the Crossley reflector, and for much of that time had been an enthusiastic convert to the island universe theory; only that March he had dined with Hale in Washington within a week

of lecturing on "Modern Theories of the Spiral Nebulae" to the Washington Academy of Sciences.[7] And on 8 October, when organizing the observing programmes for the 60in. and the new 100in. reflectors at Mount Wilson, Hale had written to Campbell to say "We are planning an extensive attack on spirals, with special reference to internal motion, proper motion, spectra of various regions, novae, etc., and here again *I should be glad to know what Curtis has in hand*, so that our work may fit in with it to advantage" (emphasis supplied).[8] Whatever the reason for the initial selection of Campbell as speaker,[9] by the time the question comes up again in correspondence Hale had received from Campbell a copy of the volume on nebulae published by Lick Observatory, in which "three splendid contributions"[10] were the work of Curtis, and thereafter Curtis and not Campbell is the projected speaker.

Meanwhile, however, Hale in fact favoured relativity, but on this Abbot had many misgivings:

> As to relativity, I must confess that I would rather have a subject in which there would be a half dozen members of the Academy competent enough to understand at least a few words of what the speakers were saying if we had a symposium upon it. I pray to God that the progress of science will send relativity to some region of space beyond the fourth dimension, from whence it may never return to plague us.[11]

Evidently Abbot's views prevailed, for he cabled Hale on 18 February: "Am wiring Heber Curtis suggesting Debate him and Shapley on subject scale of universe for Academy meeting forty five minutes each suggest communicate Shapley and Curtis and wire if favorably arranged."[12] Curtis accepted, at first with marked reluctance, then with increasing relish at the prospect of battle. Shapley likewise accepted—Hale was his 'boss' and the invitation a compliment—but with deep misgivings, for his career was now at a crossroads. In February 1919 the death of Edward C. Pickering had at last brought to a close his fortytwo-year reign as Director of the Harvard College Observatory. Pickering had been an outstanding administrator. The obvious choice as successor would have been Henry Norris Russell, Shapley's sometime teacher and mentor and the only American astronomer with influence comparable to that of Hale, except that he lacked Pickering's administrative abilities. Kapteyn, writing to Hale from Gröningen, thought Shapley perhaps the right candidate;[13] but Shapley, though a brilliant and original astronomer, was as yet only in his mid-thirties.

This handicap did not deter Shapley. In later life he vividly recalled the day he heard of Pickering's death, and decided to "take a shot" at succeeding him.[14] He promptly wrote to both Russell and Hale to state his claims.[15] Russell was equally frank in reply: "To tell the naked truth, I would be very glad to see you in a good position at Harvard, free from executive cares. . . . But I would *not* recommend you for Pickering's place, and I believe that you would make the mistake of your life if you tried to fill it."[16] To Hale, Russell remarked that Shapley "would not suffer if he pondered the old fairy tale about the man who got all sorts of good things from a magic fish whose life he had saved—until his wife wanted to be Pope!"[17] Hale warned Shapley: "My advice to any candidate for a position would be never to attempt to take an active part in

securing it, as this is the surest way to defeat one's end."[18] Shapley, chastened but secretly unconvinced, wrote to both men to declare himself no longer a candidate.[19]

On 20 December, about the time that Abbot and Hale were considering Shapley for the Washington meeting, A. Lawrence Lowell, the President of Harvard, telegraphed to Mount Wilson: "Is Shapley coming East Xmas time for some scientific meeting? If so could he visit Cambridge? If not when could he come here?"[20] The secretary's reply that Shapley had no such plans led to a mysterious visit to Shapley from a Regent of Harvard. "He evidently sailed under sealed and secret and telegraphic orders," Shapley told Russell on 6 January with some excitement, "for he knew nothing of astronomy or physics or science, or me or anyone here. He asked about the scientific meetings here last June—that A.A.A.S. convention that I managed. . . . I might say that I am naturally very confident that Harvard is not too big for me and that the things I could and would do there would be a credit to American astronomy."[21] The visitor's interest in Shapley's ability as an organizer rather than as an astronomer was no doubt because he was being considered, not for the Directorship, but for a post in support of the new Director. Certainly in June Russell was to be offered the Directorship with "a second astronomer, younger, and with modern ideas, to be called, to act as the Director's right hand man" (Shapley was to Russell the obvious choice), and a *third* person to act as administrator;[22] and even when Russell eventually declined, Shapley was merely offered the post of Assistant Professor and Astronomer.[23]

Evidently believing he was nominated for the Directorship itself, and eager for the appointment, Shapley viewed the proposed encounter with Curtis with dismay. As ill-luck would have it, Curtis was an experienced and accomplished public speaker who might well put Shapley to rout, whatever the scientific merits of their respective cases, and this—taking place within easy reach of Harvard—could cost Shapley the Directorship. In the ensuing flurry of correspondence between Shapley, Curtis, Hale and Abbot, Shapley tried—half-heartedly—to get an Easterner substituted for Curtis,[24] and—tenaciously—to undermine the seriousness and length of the proposed encounter. Four distinguished and busy men repeatedly discussed whether it should be a 'debate' as originally proposed, or a 'discussion'—"two talks on the same subject from our different standpoints", as Shapley wished.[25] No sooner had Hale been won round by Shapley to a 'discussion' than the latter received from Curtis a letter which reawakened all his anxieties:

> I agree with you that it should not be made a formal "debate", but I am sure that we could be just as good friends if we did go at each other "hammer and tongs". . . . A good friendly "scrap" is an excellent thing once in a while; sort of clears up the atmosphere. It might be far more interesting both for us and our jury, to shake hands, metaphorically speaking, at the beginning and conclusion of our talks, but use our shillelahs in the interim to the best of our ability.[26]

Curtis sent a copy of the letter to Hale. It was 3 March before Hale could talk with Shapley and formulate his reply: "I do not think that the discussion should

be called a 'debate', or that Shapley, who is perfectly willing to speak first, should have time allotted him for 'rebuttal'. If you or he wish to answer points made by the other, you can do so in the general discussion." Each should be manifestly a seeker after truth, "willing to point out the weak places in his argument and the need for more results".[27]

Not only had Shapley persuaded Hale away from the original concept of a debate, but he had convinced Hale that the proposed 45 minutes for each speaker was too long (on the grounds that this would tax the patience of the audience), and that 35 would be better. Curtis was aghast. The Lick Observatory Journal Club had recently devoted several meetings to the size of the Galaxy and the problem of external galaxies,[28] and Curtis had prepared a paper on the subject which he was circulating to friends for comment.[29] He knew how long he needed to make a serious scientific case. "We could scarcely get warmed up in 35 minutes", he protested to Hale.[30] Again the letters passed back and forth, and eventually a compromise of 40 minutes was imposed.[31]

The next problem concerned the subject matter. In Shapley's view, "The Scale of the Universe" made Curtis's main concern, the island universe theory, "an incident to a general discussion of the present guesses as to galactic dimensions and arrangement".[32] On the other hand, both men recognized that if, as Shapley maintained, the Galaxy was much larger than had previously been thought, it would be more difficult for Curtis to sustain the claim that the spiral nebulae were independent island universes; and it was clear from the pictorial slides Curtis proposed to use that he would indeed concentrate on the island universe theory.[33] Shapley welcomed their different but interrelated approaches as offering scope for a partnership instead of the dreaded confrontation: "I shall not be able to get as far as details of nebulae in my half of the talk, but I shall get some of the explanatory, introductory, illustratory requisites out of the way so that you can probably go farther into the details."[34] But he knew that Curtis planned to present a serious scientific argument, summarized on typewritten slides which he would show to the audience.[35] Shapley decided to appeal to Russell, his powerful ally, for vocal support, though putting the suggestion as usual into the mouth of a senior colleague:

> I lead off (with pictures), then Curtis presents his views, and then follows general discussion. Mr. Hale is anxious that you lead that discussion in whatever way you see fit, and I believe he plans to ask the presiding officer to call upon you as a starter. . . .
>
> Curtis swears by Newcomb and other patriarchs, and will show(?) that my distances are some ten times too big. Now that ten times, as Mr. Hale realizes, is as bad on your hypotheses as on mine; it is a violation of nearly all recent astrophysical theory. So unless Curtis actually bowls us over with the only true truth in these celestial matters, you will be interested in this general assault from the self-styled conservatives.
>
> Professor Brown is here at the observatory; also Professor Frost. They, as well as the people at Lick and at Mount Wilson, seem to regard that coming discussion as a crisis for the newer astrophysical theories. . . . But, crisis or not, I am requested to talk to the general public of non-scientists that may

> happen to drop in. Consequently whatever answer must be made to Curtis and his school must be made in the discussion.
>
> I write you this because you may be interested in knowing what the situation is, and so that you may be ready to defend your own views if they are imposed upon by either of us. To make matters worse for me, Mr. Agassiz of the Harvard Obs. Visiting Committee is coming down to the lecture and to eat a lunch with me; and A.L.L. himself has written for an appointment in Washington.[36]

In fact Russell made so substantial a contribution in support of Shapley that the question arose of whether he should be a third author of the published version, for in July Shapley told Curtis: "Russell is probably not coming in the published discussion, according to Hale, so either I should have the come-back or I should know what you are going to do and rebut in advance."[37]

There remained the crucial question of the content and level of Shapley's presentation in Washington. His decision was to treat the National Academy of Sciences to an address so elementary that much of it was necessarily uncontroversial. The typescript he used—covered with pencilled emendations, some in shorthand—runs to some 19 pages.[38] Of these, the last three pages are devoted to the intensifier he had developed to permit the photography of very faint stars —irrelevant to the theoretical argument, but perhaps directed in part to those members of the audience responsible for the future development of Harvard College Observatory. Of the first 16, it takes him more than six to reach the definition of a light year! The remaining ten pages are published below; this, and not the technical paper which appeared over a year later, was what Shapley actually said in Washington.

Although Curtis intended to present his case through a series of typewritten slides, he also had a script of sorts, no longer extant. It was probably by way of introduction to the more technical material on the slides, for he wrote to Shapley the following August: "I am sending with this a copy of my talk at Washington. This will recall to you the general lines of the arguments used. . . . Unfortunately, most of my actual argument was shown in the form of typewritten slides; I have no copy of these to send on to you at present. . . ."[39] These slides (or some of them) have survived and are reprinted below. They relate closely to the published account, and at Washington must have formed an odd contrast to the elementary talk by Shapley which had preceded. The contrast is echoed in Shapley's letter to Curtis on 9 June, telling him that Hale thought that "in a slightly different form the papers would go to the Proceedings—he favors that, in fact, even if the papers are long, providing the material is suitable in being not too popular (like mine?) or too tabular or technical (like yours?)". Curtis modestly accepted the criticism: "Yes, I guess mine was too technical. I thought yours would be along the same line, but you surprised me by making it far more general in character than I had expected. Had some thoughts of changing entire character of my presentation about five minutes before close of your part, but decided at last minute to go ahead with program as planned."[40]

A referee might have declared 'no contest', but insofar as there was a contest, Curtis was the winner. Shapley in old age recalled: "Now I would know how to dodge things a little better. . . . As I remember it, I read my paper and Curtis

presented his paper, probably not reading much because he was an articulate person and was not scared."[41] Curtis, writing to his family on 15 May, reported "Debate went off fine in Washington, and I have been assured that I came out considerably in front".[42] Russell, writing to Hale in June about his invitation to become Director at Harvard, declared: "Shapley couldn't swing the thing alone. I am convinced of this after trying to measure myself with the job, and observing Shapley at Washington"—but if Shapley joined Russell there as his 'second' he ought to offer a lecture course for this "cultivates the gift of the gab, which he needs".[43]

In spite of the disparate performances, the occasion lived in the memory of those directly or peripherally involved.[44] For Curtis it was the climax of his decade of research on the nebulae; by July he was at Allegheny Observatory as Director and his creative years as an observer were over. For Shapley it was the occasion when he (and Mrs Shapley!) were vetted for the Harvard appointment. For the staff of the two great Californian observatories, it was something of a duel between champions.[45] Above all, the time was ripe for an appraisal of conflicting evidence and opposed interpretations on the fundamental question of the nature of the universe in the large; as R. G. Aitken of Lick remarked,

> I would like to hear the debate between Curtis and Shapley. I have read Curtis' paper—a very good one—and have had long talks with Shapley also, and each one has many very good arguments to present. For my own part, I am still "on the fence" on the question. I very greatly doubt the visibility of half-a-million or more 'island universes' on the one hand, and, on the other, I am not ready to accept Shapley's conclusions *on the basis of his measuring-rod*. It seems to me that its value is not yet sufficiently demonstrated. I am open to conviction.[46]

Curtis went prepared for the fur to fly;[47] his contribution was by common consent well presented and, as the slides show, at a high technical level. Russell, a talker of legendary capacity[48] and an outstanding astronomer, made a substantial reply from the floor, and we may be sure the rest of the session was hard fought. No wonder it was a memorable occasion. But the scientific argument and counterargument between Shapley and Curtis enshrined in the *Bulletin* papers belong, not to the verbal fisticuffs of Washington, but to their ensuing and protracted correspondence; and that is another story.

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SHAPLEY'S WASHINGTON ADDRESS[49]

. . . Now that we have a satisfactory unit of sidereal distance [the light-year], let us go rambling about the universe. We see at one edge of this Milky Way field a cluster of stars—a typical phenomenon in the galactic system. To the unaided eye we see the Pleiades as such a cluster; stars of the constellation Orion also form a real physical system of stars moving together and probably all of common origin. We know from special investigations that even the big dipper is a stellar organization. We may, indeed, trace by continuous steps the denseness and richness of the clustering motive from the richest of globular clusters to the poorly organized nearby systems. Messier 11 is a society of a few hundred stars, forming an open cluster in a rich part of the galactic clouds. In the same region is Messier 22, a transition type from open to globular clusters. It is one of the nearest systems of its class—only 25,000 light-years away—nearer and brighter than the great cluster in Hercules, Messier 13, but not so well known because far south and less condensed. This eleven hour exposure of Messier 13, made by Ritchey with the 60-inch reflector, probably shows the faintest stars ever photographed with that telescope. Since all globular clusters are very much alike except in distance, this picture is suitable for an illustration of the dimensions and physical properties of a typical system.

We do not know how many stars are in Messier 13, probably not less than 50,000; about 30,000 have been counted, and the bottom has not yet been reached. The distance of the cluster, I find, is some 35,000 light-years; its linear diameter is therefore more than 350 light-years, and its total radiation is 300,000 times that of our sun.

The Hercules cluster has been extensively studied. We know for example the positions, magnitudes, and colors of all its brightest stars, and many relations between color, magnitude, distance from the center and star density. We now have the spectra of many of the individual stars, and their radial velocities; and the velocity and spectral type of the cluster as a whole. We know the types and periods of light variation of its variable stars, the colors and spectral types of these variables, and something also of the absolute luminosity of the brightest stars of the cluster from the appearance of their spectral lines. With knowledge of all this structural and historical detail, is it surprising that we venture to determine the distance of Messier 13 and similar systems with more confidence than was possible ten years ago when not a one of these facts was known or seriously thought about?

I shall not impose upon you the dreary technicalities of the methods of determining the distance of globular clusters. That would involve discussion of parallactic motion, probable errors, Cepheid variation, giant stars, and such matters. I think it will suffice to sketch briefly the principles involved.

For nearby stellar objects we can make direct trigonometric measures of distance, using the earth's orbit or the path of the sun through space as a base line. For many of the more distant stars the spectroscopic method is available, using the appearance of the stellar spectra and the readily measurable brightness of the stars; for certain types of stars, too distant for spectroscopic data, there is still a chance of obtaining the distance by means of the photometric method. This simple device, which is one particularly suited to studies of globular clusters, consists in determining, by some indirect means, the real light power of a star, that is, its so-called *absolute* magnitude, and then measuring its apparent magnitude. Obviously, if a star of known real brightness is moved away to greater and greater distances, its apparent brightness decreases; hence, for such stars of known absolute magnitude, the apparent magnitude gives, by a simple formula, the distance from the observer.

As I have suggested before, it is because within recent years we have advanced our knowledge so greatly that we can use these powerful spectroscopic and photometric methods of measuring distance. The advance is in two directions; first, in the study of the nearby stars we have learned of the uniformity in absolute brightness and spectroscopic characteristics of various types; and second, we have shown that in the clusters we have exactly the same kinds of stars as those around the sun—the same except that the cluster stars *appear* to be very faint. This apparent faintness of the cluster stars is due to *distance*, and is a *measure* of it. For instance, the very extensive studies by Plummer, Kapteyn, and Charlier, have shown that stars of spectral type B in the solar neighbourhood (the blue stars) are closely restricted around an average absolute luminosity about 200 times that of the sun. In Messier 13 we have the same types of stars. This we know from direct observations of spectrum with the 60-inch and 100-inch telescopes, supplemented by thousands of measures of color. But these blue cluster stars, which are actually about 200 times as bright as the sun, appear to be only one five-thousandth as bright as the sun would be if it were 33 light-years away; the distance of the blue cluster stars is therefore readily found to be some 35,000 light-years, and *their* distance is of course that of the whole cluster.

You may ask, however, is it not likely that these stars in the cluster, just because they are in a globular cluster, are of a different magnitude from our standards, even though com-

parable in color and spectrum? May they not be dwarfs in luminosity, and therefore not so far away? All the evidence, theoretical and observational, argues to the contrary. There should be little doubt in this matter of comparability for several good reasons. For instance, one reason—an all important one—*is that these nearby standard stars are themselves cluster stars*. I have already pointed out the continuous sequence from the densest globular cluster to the constellation Orion; and the stars in such open nearby clusters as the Hyades, Orion, Scorpio, *are* the standards.

It is thus because of this comparability of local B stars with those of the distant open and globular clusters, and because the absolute luminosities of the local stars are based upon thousands of good measures of proper motion and radial velocity, that the blue stars give us the strongest evidence for the great distances of globular clusters.

Similarly the giant red stars of our local clusters are found to be comparable with the many red stars in globular clusters, and qualitatively at least, they give through the photometric method the same distances for remote systems as the blue stars give.

Another class of stars, the Cepheid variables, have been used extensively, and in much the same way, in exploring not only the system of globular clusters but the star clouds of the Milky Way. By determining the light-curve of such a star in any cluster, the distance may be known with remarkable accuracy. The particular luminosity that corresponds to a given period is found, as for the B stars and red giants, from studies of nearby examples of the class. Professor Curtis may tell you more of the photometric method of getting at the distances of the nearby Cepheid variables—he may question the sufficiency of the data or the accuracy of the methods of using it. But this fact remains: we could discard the Cepheids altogether, use instead the thousands of B-type stars upon which the most capable stellar astronomers have worked for years, and derive just the same distance for the Hercules cluster, and for the other clusters, and obtain consequently the same dimensions for the galactic system.

To conclude, in the face of these results, that the Hercules cluster is not approximately at the distance derived for it photometrically, is to avoid the most direct, and simple, and conservative interpretation of the data. To suggest, as I believe Prof. Curtis may do, that the clusters are only one-tenth as remote as I place them, is equivalent to subscribing to views so radical in several departments of astronomy and physics that we instinctively hesitate.

If the distances I have assigned must be decreased to one-tenth, then the light-emitting power of distant cluster stars must be only *one-hundredth* that of local cluster stars *of exactly the same types*. As a consequence I believe Russell's illuminative theory of spectral evolution must be largely abandoned, and Eddington's brilliant theory of gaseous giant stars must be greatly modified or given up entirely. Now both of these modern theories have their justification, first in the fundamental nature of their physical concepts, and second, in their great success in fitting observational fact. Similarly, the period-luminosity law of Cepheid variation would be meaningless; Kapteyn's classic researches on the structure of the local cluster would need new interpretation, because his luminosity laws could be applied locally but not generally; and a very serious loss to astronomy would be that of the generality of the spectroscopic method of determining star distances, in fact, the whole application of that method as an independent tool, for it would mean that identical spectral characteristics may indicate stars differing by five magnitudes, depending merely upon whether the star is in the solar neighborhood or in a distant cluster.

With so many satisfactory methods and theories at stake—the very foundations of modern astrophysics—is it any wonder that we hesitate and argue against revolutionary interpretations? I believe I cannot follow Professor Curtis in calling those older, long-established interpretations conservative.

Suppose, therefore, we admit the obvious comparability of local cluster stars with those of distant clusters; is there not, however, a possibility that the distant stars appear faint through the loss of light in travelling through space? The Mount Wilson photometric studies show, however, no effect on star colors of such loss—a result checked by Hertzsprung and others. Two Swedish students find no suggestion of it in their studies of the very distant spiral nebulae. In the plane of the Milky Way, to be sure, we have dark nebulosity that *may* obscure more distant objects without affecting the color. But outside the galactic regions, and Messier 13 and most of the other globular clusters are far outside, we have in the distinct correlation of the angular size of a cluster with the brightness of its stars a fairly good proof of the absence of general light absorption. That is, if a cluster were removed to twice the present distance, its stars would be fainter, and also its area would be one-fourth as large. That is just what we observe—the faint clusters small, the small clusters faint—an obvious indication that the small faint clusters owe both of these characters to distance. Whereas, if the faintness of the cluster were due to obstruction of light, we should expect the angular diameter to be little affected; moreover, we should also expect to find, in such obstructed clusters, wholesale irregular variations, lop-sidedness, and other curious effects that are not

observed, unless, miraculously, the hypothetical obstructing matter were exactly at rest with respect to the cluster beyond, or exactly uniform.

When we accept that the distance of the Hercules cluster is such that its stellar phenomena are all harmonious with local stellar phenomena, then it follows that fainter, smaller clusters are still more distant. Thirty of the 86 known are more distant than 100,000 light-years; the most distant is more than 200,000 light-years away, and the diameter of the whole system of globular clusters is about 300,000 light-years. Since the affiliation of the globular clusters with the Galaxy is shown by their concentration to the plane of the Milky Way and their symmetrical arrangement with respect to it, it also follows that the galactic system of stars is as large as this subordinate part. During the past year we have found Cepheid variables and other stars of high luminosity among the fifteenth magnitude stars of the galactic clouds; this can only mean that some parts of the clouds are more distant than the Hercules cluster. There seems to be good reason, therefore, to believe that the star-populated regions of the galactic system extend at least as far as the globular clusters.

One consequence of the cluster theory of the galactic system is that the sun is found to be very distant from the center of the Galaxy. It appears that we are near the center of a large local cluster or cloud of stars, but that cloud is at least 60,000 light years from the galactic center. Twenty years ago Newcomb remarked that the sun *appeared* to be in the galactic plane because the Milky Way is a great circle—an encircling band of light—and that the sun also *appears* near the center of the universe because the star density falls off with distance in all directions. But he concluded as follows:

> "Ptolemy showed by evidence, which, from his standpoint, looked as sound as that which we have cited, that the earth was fixed in the center of the universe. May we not be the victim of some fallacy, as he was?"

The answer to Newcomb's question is: Yes, we have been victimized by the chance position of the sun near the center of a subordinate system, and misled by the consequent phenomena, to think that we are God's own appointed, right in the thick of things. In much the same way ancient man was misled, by the rotation of the earth, and by the consequent apparent daily motion of all heavenly bodies around the earth, to believe that even his little planet was the center of the universe, and that his earthly gods created and judged the whole.

If man had reached his present intellectual position in a later geological era, he might not have been led to these vain conceits concerning his position in the physical universe, for the solar system is going rapidly away from the center of the local cluster. If that motion remains unaltered in direction and amount, in a hundred million years or so the Milky Way will be quite different from an encircling band of star clouds, the local cluster will be a distant object, and the star density will no longer decrease with distance from the sun in all directions.

Remembering these delusions, relative to his physical status in the universe, may we not appropriately ask if man is also biologically blindfolded? Does he, perhaps, hold his self-assumed and self-defined position at the peak of animal development as a victim of psychological fallacy?

Another consequence of the conclusion that the galactic system is 300,000 light-years or more in greatest diameter, is its bearing on the problem of the spiral nebulae. I shall leave the description and discussion of this debatable question to Professor Curtis. We agree, I believe, that if the galactic system is as large as I maintain, the spiral nebulae can hardly be comparable galactic systems; if it is but one-tenth as large, there *might* be a good opportunity for the hypothesis that our galactic system is a spiral nebula, comparable in size with the other spiral nebulae, all of which would then be "island" universes of stars. On one other point I think we also agree, or at least we should agree, and that is that we know relatively so little concerning the spiral nebulae and we are soon going to know relatively much because of the increasing activity in the nebular field, that it is professionally and scientifically unwise to take any very positive view in the matter just now.

But to summarize my view, which I hope is not positive and which is certainly subject to change if future data justify, the spiral nebulae are distant objects, not members of our galactic system, except that the nearer brighter ones have some sort of a relation to the Galaxy, not only in distribution, but also in motion. This relation to the Galaxy may be transitory for any given nebula, for with their enormous speeds of recession, they may eventually pass out of our domain of space. I prefer to believe that they are not composed of stars at all, but are truly nebulous objects. For instance, these two photographs of the typical spiral, Messier 51, by Mr. Seares indicates that the nebulosity is not composed of stars—as required by the island universe theory. The picture on the left is taken with a plate sensitive to yellow light, the one on the right records the blue light. The exposures are adjusted so that the superposed stars are of the same intensity. If the arms of the spiral were composed of stars the two pictures should be comparable in intensity throughout. If, however, the spiral is truly nebulous, it would appear faint on the photograph in yellow light. That, you

see, is the actual condition in this system. No type of star is known with anything like as large a negative color index as is shown by the nebulosity in this spiral. But even if the spirals are stellar, they are not comparable in size with our stellar system, and our system is not comparable in constitution with the spiral nebulae. Professor Curtis, I hope, will have time to go farther into this interesting question. . . .

SLIDES SHOWN BY CURTIS IN WASHINGTON[50]

[A] The Size of our Galaxy; Older View

Studies of the distribution of the stars and the ratios between the numbers of stars of successive magnitude have led a number of investigators to fairly accordant dimensions for the galaxy.

Wolf; about 14,000 light-years in diameter.

Eddington; about 15,000 light-years.

Shapley (1915); 20,000 light-years.

> "That the maximum radius of the Milky Way is probably not greater than ten thousand light-years and may be somewhat less has been deduced from many lines of evidence, the most important of which is the color of the faint stars." (Mt. Wilson Contr., No. 116, 1915.)

Newcomb; not less than 7,000 light-years; later—perhaps 30,000 light-years in diameter and 5,000 light-years in thickness.

A maximum galactic diameter of 30,000 light-years will be assumed as representing sufficiently well the older view; it is perhaps too large.

[*cf.* ref. 2, 195–6]

[B] The Shape of our Galaxy of Stars

Studies of the distribution of the stars over the entire sky, with investigations based on the ratios between the numbers of stars of successive magnitudes, have given the following results:

1. The stars are not infinite in number, nor uniform in distribution.
2. Our Galaxy, delimited for us by the projected contours of the Milky Way, contains possibly a billion suns.
3. Our Galaxy is shaped much like a lens, or a thin watch, the thickness being perhaps one-sixth of the diameter.
4. Our sun is located fairly close to the center of figure of the Galaxy.
5. The stars are not distributed uniformly through this galaxy. A large proportion may be actually in the ring structure suggested by the appearance of the Milky Way. There is some slight evidence for a spiral structure. Our position near the center of figure of the Galaxy is not a favorable one for a determination of the actual galactic structure.

[*cf.* ref. 2, 196]

[C] The Size of our Galaxy; Shapley's View

From evidence to be referred to more fully later, Dr. Shapley has derived very great distances for the globular star clusters, 220,000 light-years for the most remote.

The apparent distribution of these globular clusters shows incontrovertibly that they are an integral feature of our galactic system.

This evidence has formed the main reason for Dr. Shapley's adoption of a diameter of 300,000 light-years for our galactic system, fully ten times greater than that accepted hitherto.

[*cf.* ref. 2, 197–8]

[D] THE STARS OF THE MILKY WAY

The smaller postulated dimensions for the Galaxy require stars whose absolute magnitudes are in fair accord with those of known distance. The larger dimensions require a very large proportion of giant stars.

Apparent Magnitudes	Corresponding absolute magnitudes for distances of	
	10,000 l.y.	100,000 l.y.
8	−4·4	−9·4
10	−2·4	−7·4
12	−0·4	−5·4
14	+1·6	−3·4
16	+3·6	−1·4
18	+5·6	+0·6
20	+7·6	+2·6

[*cf.* ref. 2, 200]

[E]

The conditions of star concentration obtaining in the Magellanic Clouds and in the globular clusters appear to render these regions of space unique as regards variable stars.

The Magellanic Clouds contain .. .. 1800 variable stars

Total of all variables in the rest of the sky, excluding those in globular clusters 1686

The globular clusters contain numbers of variable stars ranging from 137 in N.G.C. 5272 to 0 for N.G.C. 3293 and 4755. Practically all are shorter than one day in period. Total .. 509

Short period cluster-type variables discovered to date in the rest of the sky 45

[?Replaced in ref. 2 by discussion of Cepheid variables and "giant" stars]

[F] THE SPECTRUM OF THE SPIRAL NEBULAE

As island universes

The spectrum of the average spiral nebula is indistinguishable from that given by a star cluster.

It is such a spectrum as would be expected from a vast congeries of stars.

In general type it resembles the integrated spectrum of our Milky Way.

The spectrum of the spiral nebulae offers no difficulties in the island universe theory of the spirals.

As galactic phenomena

If the spiral nebulae are an integral part of our Galaxy, we must assume that they are some sort of finely divided matter, or of gaseous constitution.

If galactic, we have no adequate and actually existing evidence by which we may explain their spectrum.

The diffuse nebulosities of our galaxy give a bright-line gaseous spectrum. A few, associated with bright stars, agree with their involved stars in spectrum, and are well explained as a reflection or resonance effect.

Such an explanation is untenable in the case of a large proportion of the spirals.

[*cf.* ref. 2, 212]

[G] THE DISTRIBUTION OF THE SPIRAL NEBULAE

The spiral nebulae are found in greatest numbers just where the stars are fewest (at the poles of our Galaxy), and not at all where the stars are most numerous (in our galactic plane). No spiral has as yet been found actually within the structure of the Milky Way.

As island universes

It is most improbable that our galaxy should, by mere chance, be placed about half-way between two great groups of island universes.

So many of the edgewise spirals show peripheral rings of occulting matter that this dark ring may be the rule, rather than the exception.

If our Galaxy, itself a spiral on the island universe theory, possesses such a peripheral ring of occulting matter, this would obliterate the distant spirals in our galactic plane, and explain their peculiar distribution.

There is some evidence of such occulting matter in our galaxy.

Additional observations on the spirals south of the galactic plane may remove this recession excess. Part of this may also be due to the motion of our Galaxy in space.

As galactic phenomena

If the spirals are galactic objects, they must be a class apart from all other known types.

Their abhorrence of the regions of greatest star density can only be explained on the hypothesis that they are, in some manner, repelled by our Galaxy.

We know of no force adequate to produce such a repulsion, except perhaps light pressure.

Why should this repulsion invariably have acted at right angles to our galactic plane?

Why have not some been repelled in the direction of our galactic plane?

The repulsion theory is given some support by the fact that most of the spirals observed to date are receding from us.

[*cf.* ref. 2, 213]

[H] NEW STARS IN THE SPIRAL NEBULAE (1)

Within the past few years some twenty-five novae have been discovered in spiral nebulae, sixteen of these in the Nebula of Andromeda, as against about thirty in historical times within our own galaxy.

Apparent Magnitudes

	Thirty galactic Novae	Seventeen Novae in Neb. Andromeda
At maximum	+ 5	about +17
At minimum	+15	perhaps +27 ?

Absolute Magnitudes

	Novae in Nebula of Andromeda, if at distance of		Four galactic Novae of known distance
	20,000 l.y.	500,000 l.y.	
At maximum	+ 3·1	−3·9	−3·4
At minimum	+13·1	+6·1	+7·2

[*cf.* ref. 2, 214–15]

[J] THE SPIRAL NEBULAE AS ISLAND UNIVERSES

Summary

1. On this theory we avoid the almost insuperable difficulties involved in the attempt to place the spirals in any coherent scheme of stellar evolution, either as a point of origin, or as a final evolutionary product.

2. On this theory, it is unnecessary to attempt to coordinate the tremendous space-velocities of the spirals with average star velocities.

3. The spectrum of the spirals is like that given by a star cluster.

4. A spiral structure for our own Galaxy has been suggested, and is not improbable.

5. If island universes, the new stars observed in the spirals seem a natural consequence of their nature as galaxies. Correlations between the new stars in spirals and those in our Galaxy indicate a distance ranging from perhaps 500,000 light-years in the case of the Nebula of Andromeda, to 10,000,000, or more light-years for the more remote spirals.

6. At such distances, these island universes would be of the order of size of our own Galaxy of stars.

7. Very many spirals show evidence of peripheral rings of occulting matter in their equatorial planes. Such a phenomenon in our own Galaxy, regarded as a spiral, would serve to obliterate the spirals near our galactic plane, and would furnish an adequate explanation of the peculiar distribution of the spiral nebulae.

[*cf.* ref. 2, 216–17]

REFERENCES

1. Discussions include: Otto Struve, "A Historic Debate about the Universe", *Sky and telescope*, xix (1959–60), 398–401; Norriss S. Hetherington, "The Shapley–Curtis Debate", Astronomical Society of the Pacific Leaflet no. 490 (April 1970); Otto Struve and Velta Zebergs, *Astronomy of the twentieth century* (New York, 1962), chaps 19 and 20, *passim*; and "The Great Debate", chap. 6 of Harlow Shapley, *Through rugged ways to the stars* (New York, 1969). A new treatment will shortly be published in Richard E. Berendzen *et al.*, *Man discovers the galaxies* (New York, 1976).
2. H. Shapley and H. D. Curtis, "The Scale of the Universe", *Bulletin of the National Research Council*, ii, Part 3 (May 1921).
3. As shown by the official programme of the Academy meeting.
4. The meeting took place on 19 December. There is no reference in the minutes to Hale's suggestion.
5. W. E. Hale had used his wealth to support his son's projects, notably by providing the disc for the 60in. telescope eventually erected at Mount Wilson.
6. Archives of the National Academy of Sciences.
7. As shown by the letters from Curtis to his children, 8 February and 9 March 1919 (Michigan Historical Collections, University of Michigan). The script of Curtis's lecture is in the archives of Lick Observatory.
8. Hale microfilm.
9. Not surprisingly, in 1968 Dr Abbot did not recall the reason for the choice of Campbell, but remarked that Campbell was of course "a more important astronomer than Curtis" (personal communication). Mr Robert Smith points out that Campbell had supported the island universe theory in "The Nebulae", *Science*, xlv (1917), 513–48.
10. Letter of Hale to Curtis, 24 February 1920 (Hale microfilm).
11. Letter of Abbot to Hale, 20 January 1920 (Hale microfilm).
12. Hale microfilm. Hale cabled at once to Shapley and Curtis, offering each an honorarium of $150.
13. Letter of 7 February 1919 (Hale microfilm):
    In America you have Russell and Shapley. Shapley is a brilliant man and personally I, who know him mainly only through his scientific work, would think him the best fitted for the position. Meanwhile I do not know him sufficiently to know how he would do as an organiser at the head of such a large and complicated Institution as the Harvard Observatory.
14. Shapley, *Through rugged ways to the stars*, 82.
15. Shapley to Russell, 12 February 1919; to Hale, 13 February 1919 (Shapley Archives, Harvard University).
16. Russell to Shapley, 19 February 1919 (Shapley Archives, Harvard University).
17. Russell to Hale, 19 February 1919 (Russell Archives, Princeton University).
18. Hale to Shapley, 27 February 1919 (Hale microfilm).
19. Shapley to Russell, 27 February 1919; to Hale, 7 March 1919 (Shapley Archives, Harvard University).
20. Hale microfilm.
21. Shapley to Russell, 6 January 1920 (Russell Archives, Princeton University).
22. Russell to Hale, 13 June 1920 (Hale microfilm).
23. Invitation dated 10 November 1920 (Shapley Archives, Harvard University).
24. Shapley to Hale, "Sunday", *i.e.* 22 February 1920 (Hale microfilm).
25. Shapley to Hale, 19 February 1920 (Hale microfilm). On the 24th Hale wrote to Shapley, Curtis and Abbot approving the concept of a 'discussion'.
26. Curtis to Shapley, 26 February 1920 (Shapley Archives, Harvard University).
27. Hale to Curtis, 3 March 1920 (Shapley Archives, Harvard University).

28. Curtis to E. E. Barnard, 28 January 1920 (Archives of Yerkes Observatory). Curtis reports that "most of us here find it impossible to subscribe to some of the recent theories on these points".
29. *Ibid.* On 23 February Curtis requested from Barnard the return of this paper as a matter of urgency.
30. Curtis to Hale, 9 March 1920 (Hale microfilm).
31. Abbot to Hale, 18 March 1920 (Hale microfilm).
32. Shapley to Abbot, 12 March 1920 (Shapley Archives, Harvard University).
33. Curtis to Shapley, 14 March 1920. Since Shapley maintained to the end of his life that Curtis did not address himself to the subject of the title (Shapley, *Through rugged ways to the stars*, 79), it is worth recording the synopsis Curtis proposed to Hale (Curtis to Hale, 20 February 1920, Shapley Archives, Harvard University) and which Hale welcomed: "Dr Shapley will discuss recently secured evidence pointing to dimensions of our galaxy about ten times greater than held in the older theories of the Milky Way, i.e., a diameter of about 300,000 light-years, with the spiral nebulae regarded as a galactic phenomenon. Dr Curtis will defend the older view that our Milky Way is approximately of the dimensions suggested by Newcomb, i.e., about 30,000 light-years in diameter, with the spiral nebulae regarded as very probably individual galaxies, or 'island universes'."
34. Shapley to Curtis, 18 March 1920 (Shapley Archives, Harvard University).
35. Curtis to Shapley, 14 March 1920 (Shapley Archives, Harvard University).
36. Shapley to Russell, 31 March 1920 (Shapley Archives, Harvard University).
37. Shapley to Curtis, 27 July 1920 (Shapley Archives, Harvard University).
38. Shapley Archives, Harvard University.
39. Curtis to Shapley, 2 August 1920 (Shapley Archives, Harvard University).
40. Shapley to Curtis, 9 June 1920; Curtis to Shapley, 13 June 1920 (Shapley Archives, Harvard University).
41. Shapley, *Through rugged ways to the stars*, 79–80. In private conversation Shapley was much more emphatic as to his disappointing performance.
42. Michigan Historical Collections, University of Michigan.
43. Russell to Hale, 13 June 1920 (Hale microfilm).
44. Writing to Shapley on 10 July 1922, Curtis spoke of "our memorable set-to" (Archives of Allegheny Observatory). C. D. Shane of the University of California (Berkeley) wrote to Curtis on 3 December 1923 about "the famous debate", and Curtis in reply on the 10th again referred to "our memorable set-to" (Michigan University Archives). Campbell, writing on "Do we live in a spiral nebula?" in *Popular astronomy* for 1926, speaks of the "memorable discussion" of 1920 (p. 175).
45. Robert G. Aitken later wrote of Curtis that "For a time only his colleagues at Mount Hamilton, and a few other astronomers agreed with him in his views" (*National Academy of Sciences Biographical Memoirs*, xxii (1943), 280), and this may be true, although Aitken had forgotten that he himself had been "on the fence" (*cf.* ref. 46 below).
46. Aitken to Barnard, 27 April 1920 (Archives of Yerkes Observatory); emphasis in original.
47. "... some fur ought to fly, on both sides", Curtis to W. J. Hussey, formerly of Lick Observatory, 15 April 1920 (Michigan Historical Collections, University of Michigan).
48. "He sure is a talker.... I never saw any man better qualified to teach the unwashed astronomy then he", "Benny" writing to Shapley of a talk by Russell to the general public at Mount Wilson, 30 June 1921 (Shapley Archives, Harvard University).
49. This rough typescript, now in Box 1 of the Shapley Archives at Harvard University, contains pencil amendments in longhand, and occasionally in shorthand. Those in longhand have been incorporated in this printed text. Of the typescript, the first one-third is too elementary to justify reprinting; likewise, the final three pages dealt with Shapley's intensifier, and, however significant this might have been as an instrumental advance in the study of faint stars, it was not directly relevant to the theoretical discussion and is omitted here as it was in the printed version of the proceedings (ref. 2).
50. The slides survive at Allegheny Observatory. Of the nine, eight are represented in modified form in the printed version of the proceedings, as indicated, and the slides have accordingly been arranged in a probable order. The title of slide H suggests that Curtis may have used other slides no longer extant, but surely very few additional slides could have been fitted into a 40-minute talk.