Part III: General Summarizing Discussion of Problems Raised in Parts I and II

(*Editor's note:* In the interests of consolidating the papers treating a given general topic, we have rearranged the order of presentation of some of the papers in these proceedings from that in which they were actually presented. In terms of the major topics of discussion, this mid-symposium summary begins in these proceedings in its actual place in the Symposium. However, the topics surveyed in this summary were not all covered in one session but wound through several sessions interspersed among the general papers. Again in the interest of continuity of thought, we have collected (and at times abridged) the topics of this summary into one section. Chairmen for the several sessions were Batchelor, Kantrowitz, and Minnaert in sequence.)

G. K. BATCHELOR, *Trinity College, Cambridge, England:* Last night, Burgers, van de Hulst, Thomas, and myself, feeling a little dissatisfied with the way the sessions had gone up to that time, decided that it would be useful to have some stock-taking, in order to see what has been accomplished so far, and to try to crystallize some of the questions raised in the first two days of the Symposium. Many of the talks have covered a wide range of problems and of subject matter, and it was perhaps difficult, while listening, to focus on the more important aspects. Van de Hulst, therefore, has agreed to put them before us today, as a starting point for a general discussion.

H. C. VAN DE HULST, *Leiden Observatory, Leiden, Netherlands:* In the committee meeting of yesterday we talked over a few of the questions that had been brought forward originally, some of which have been answered, while others seem to be just forgotten.

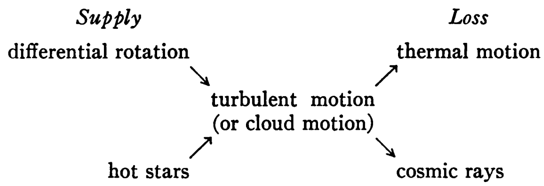
In summarizing them, I shall distinguish between "astronomical questions" and "physical questions." An astronomical question is a question about the situation in the galactic system, in the interstellar clouds, or anything else that we can actually observe and draw some empirical conclusion about. Such questions can be answered only by studying the observational material in the light of a theoretical interpretation. Physical questions are the more general questions which the astronomers try to formulate in order to make them digestible to the physicists, for instance: How will a gas cloud of such and such a size behave if it hits another gas cloud? We hope to get clear answers to such questions, even if it is not certain that such gas clouds actually exist. Furthermore, we hope that it will be possible to put a number of successively more difficult questions, each of which is clear and each of which should have a clear answer. Unfortunately, the clear solution is not the only acceptable answer. In Table I I have listed a few alternatives, and I hope to obtain an answer in one of the categories to the various questions that will be put forward.

TABLE I. Acceptable answers.

Astronomical questions	Physical questions
1. The answer is . . .	1. The answer is . . .
2. The observations are too poor.	2. Question makes no sense.
3. Observations are fine but interpretation is uncertain.	3. Question is too difficult.

1. *How precisely can we estimate the conversion from energy produced internally by hot stars into kinetic energy of turbulent motion (or cloud motion) in the interstellar gas?* (astronomical question).

The turbulent motion or cloud motion in the interstellar gas represents a certain amount of kinetic energy, which may derive from at least two sources: either from differential rotation of the galaxy, or from hot stars by the various processes that have been discussed. The kinetic energy may be converted into thermal motion by means of some dissipation mechanism, or used to accelerate cosmic rays. Thus we have the scheme:



Quantitative estimates of these processes have been on the blackboard during previous talks. The disturbing question raised by the estimates is: is there adequate energy supply for the various types of dissipation? The data in the Cambridge Symposium Report[1] for the supply by hot stars, when expressed as energies per unit volume per unit time, are:

Oort[2]	Schlüter and Biermann[3]
1×10^{-28} erg cm^{-3} sec^{-1}	3×10^{-25} erg cm^{-3} sec^{-1}.

These differ by a factor 10^3. Oort made it clear that he does not pretend to give more than a suggestion, and remarked that his source might be of the right order of magnitude to supply enough energy for the sink. The main differences between the two estimates is that Oort talked only about the huge cloud complexes, of which there are just 5 within the nearby visible region of the

[1] H. C. van de Hulst and J. M. Burgers, editors, *I. A. U. Symposium No. 2* (North-Holland Publishing Company, Amsterdam, The Netherlands, 1955).
[2] See reference 1, p. 154.
[3] See reference 1, p. 145.

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

galactic system, while Schlüter and Biermann have considered any region around B – O or O stars. There must be thousands of these in this nearby visible region—and even though they are smaller than the large complexes considered by Oort, their total contribution is large.

This is my answer to question 1—an apparent uncertainty of a factor of 1000; but perhaps the observations are too poor.

2. *Does a magnetic field effectively inhibit dissipation of energy from turbulent motion (or cloud motion) into thermal motion?* (physical question).

Explicit or implicit answers to this question were given by Parker and by Pickelner in different contexts, their answers generally agreeing, but meeting strong disagreement from other participants. Their position seems to be that any magnetic field will very effectively reduce the energy transformation, while others say no, it will only reduce it by a factor 2, and that is almost negligible. I somewhat suspect, however, that the answer should be that the question makes no sense, as the problem has not been sufficiently defined. After all, this "turbulent motion" that we talk about in the astronomical context is hardly well defined; when we talk about cloud motions, the velocities discussed imply that the motions are all supersonic. During the last Symposium the word supersonic turbulence was used, and the Symposium report contains a section of 3 pages[4] by Lighthill under the heading "Conjectures about the energy dissipation in turbulent motion with Mach numbers of the order of one or greater." This is about the extent of our information on the subject. We should like to know if today other answers are available besides my comment that the question is too difficult or that it makes no sense.

3. *A question about the differential galactic rotation* (physical question).

Given a distribution of mass in the galactic system that defines a gravitational potential, and given in this gravitational potential field a disk-like distribution of gas, in which the gas moves in laminar flow with circular orbits, is this motion unstable and will it develop turbulence? The answer originally suggested was that obviously it would develop turbulence. Lately, some people have told me that it will not.

4. *Is a galactic halo which is half ionized, half neutral, as proposed by Pickelner, compatible with the observations of the 21-cm line?* This is a purely observational question.

5. *Do the observations give convincing proof that the magnetic lines of force run along spiral arms?* (astronomical question).

Those of you who have listened carefully yesterday and who have not studied the literature before, may have received the impression that it was completely certain that the magnetic field is along the spiral arms, so that this is one of the data from which we can start in further discussions. I should like to divide this question into three parts: First, are the observations of the polarization reliable? Answer: 100% yes. Second, do these polarization measurements signify a magnetic field? Answer: 95% yes. Third, is this magnetic field generally parallel to a spiral arm? Answer: 50% yes, 50% no. This is my present notion; it may be open to discussion. Only when this question has been settled can there be further discussion about the dynamics of a spiral arm that has a magnetic field, which forms more of a physical question.

[4] See reference 1, pp. 127–129.

R. N. THOMAS, *National Bureau of Standards, Boulder, Colorado:* I would like to add to this list of questions by summarizing what I believe is the situation in connection with the galactic halo, let us say with Parker and Pickelner on one side, and Biermann and Spitzer on the other side. If I understand correctly Parker's and Pickelner's theses, they prefer a situation in which the lenticular main body of the galaxy is separated from the spherical halo. Parker prefers in the main part of the galaxy a situation where $B^2/4\pi$ is much greater than the local average kinetic energy $\rho v^2/2$, basing this requirement on the polarization data. He then argues that one has essentially no dissipation of kinetic energy because of the magnetic inhibition. On the other hand, Pickelner's main point, if I understand correctly, is that in the halo of the galaxy one prefers equality between $\rho v^2/2$ and $B^2/4\pi$, and then again one has no magnetic dissipation of energy, all material motions being essentially hydromagnetic waves in this part of the galaxy. The argument is that ultimately one must establish equipartition of energy between whatever the source of the motions is and the magnetic degrees of freedom. So it seems to me the sixth question is a physical question:

6. *Equipartition: does it establish itself, and how rapidly?*

I would point out that Parker argued strongly against any *a priori* argument for this in his original presentation, but if I understand the situation now, he agrees with Pickelner in arguing for it in the galactic corona. In all these arguments there arises, of course, the old question of kinetic temperature *vs* mass motions in interpreting observed velocities.

G. K. BATCHELOR: Another question is:

7. *How literally may we regard the gas clouds in the spiral arms as discrete?*

This has come up at every Symposium, and I think that many people take it for granted that the gas clouds are quite discrete, so that there is no hydrodynamical connection between them. The density outside a gas cloud is assumed to be so low that the clouds can be considered as separate moving bodies, making occasional collisions, rather than as representing regions in the fluid where the pressure is high. Perhaps this may be called an astronomical question, because we really wish

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

to know how big are the density variations from one point to another within a spiral arm.

Let us then regard this list of 7 questions as representing a fair summary of the outstanding problems raised thus far, and ask for clarifying remarks.

L. BIERMANN, *Max Planck Institut für Physik, Göttingen, Germany:* In connection with question 1, the figures which I put on the blackboard on Tuesday correspond to an energy dissipation of 10^{-26}–10^{-25} erg cm^{-3} sec^{-1}; hence I suggest a mean value of 3×10^{-26}, instead of the 3×10^{-25} as quoted by van de Hulst.

G. K. BATCHELOR: The two estimates then differ by a factor of 10^2.

[*Editors:* There followed a discussion between Biermann, Parker, and van de Hulst concerning the reasons for the disparity; the outcome may be summarized as follows]:

Oort treats only the large cloud complexes, which are apparently to be seen exploding; he thus gives a lower limit for the energy input. He states that there are many other O stars, which must also blow gas away, but are not included in his estimate.

Biermann and Schlüter assume 10^6 objects of bolometric magnitude -4 in the galaxy. Each of these is equivalent to an energy production of 10^{37} erg/sec from the central star. Assuming that 1% of this radiant energy goes into kinetic energy of the surrounding gas region, there is 10^{41} erg/sec fed into the galaxy. With a volume somewhat less than 10^{67} cm^3, this gives somewhat over 10^{-26} erg cm^{-3} sec^{-1} as energy input.

The questions then are: (1) how much radiant energy is produced per hot star? (2) how much radiant energy gets converted into kinetic energy? (3) how many stars of various types are there? Of these three, (2) appears to be the question with major uncertainty.

M. P. SAVEDOFF, *Department of Astronomy, University of Rochester, Rochester, New York:* We have considered energy from differential rotation, and energy from hot stars, going into cloud motions and turbulent motions. But both in the energy from the hot stars and in the dissipation of the energy, we should have a radiation term. The real question is how much of the hot stars' ultraviolet energy will be dissipated as radiation, and how much of the motion in the gas clouds will not only go into thermal motion, but will also go into radiation? For that reason, it is necessary to add to the astronomical part of the question concerning the energy input, the physical question of what is the real efficiency of the process. Further, since the time when the estimates by Oort and Schlüter were made, the observations on the 21-cm line have shown that there are certain regions of systematic motion. These regions of systematic motion must be short lived and they imply an energy input into the cloud motions or turbulent motions. Estimates of the energy input from observed regions of systematic motion might have higher weight than arguments based upon estimates of efficiency of energy input from the known existing stars. The observed 21-cm temperatures are sufficiently above equilibrium temperatures to permit estimates of the excess radiated.

M. J. SEATON, *University College, London, England:* At the moment we are concerned with the energy input, that is to say the first arrow in van de Hulst's diagram:

Differential rotation Hot stars	→	Turbulent motion (cloud motion)	→	Thermal motion	→	Cooling mechanisms

In order to arrive at a correct estimate, we must consider the energy balance at all stages of the degradation process. Several speakers have already referred to the rate of degradation from turbulent (cloud) motion to thermal motion, indicated by van de Hulst's second arrow. I would emphasize that it is necessary to extend the diagram and consider what eventually happens to this energy, and have therefore added a third arrow to the diagram. Consideration of the processes by which thermal kinetic energy is degraded to long-wavelength radiant energy indicates a rate of energy dissipation of at least 10^{-27} erg cm^{-3} sec^{-1}. A lower figure could be obtained by making the assumption that the temperature is constant at the 125°K obtained from 21-cm measurements; but this seems artificial, since it is difficult to see how cloud motion energy can be degraded to thermal energy without more or less violent impacts producing considerable temperature variations.

E. N. PARKER, *Enrico Fermi Institute for Nuclear Studies, University of Chicago, Chicago, Illinois:* I believe that the estimate by Schlüter and Biermann is too large, because it is larger than all the dissipation numbers we write down. It may be too large because the fragments of gas that are blasted away from stars have much smaller masses and momenta than the actual interstellar clouds which we observe. Using as analogy a bowling ball against which you are bumping, say, golf balls, I would point out that this is an extremely inefficient way of giving energy to the bowling ball. If one merely counts all early type stars, regardless of whether they have a massive exploding nebula around them, one is actually estimating the energy going into the golf balls and it remains to be shown how much the bowling ball receives.

R. W. STEWART, *Department of Physics University of British Columbia, Vancouver, B. C., Canada:* Is it not possible that the larger figure for the energy supply—in Parker's analogy the energy of the golf balls—is the source of the energy which dissipates at the rate 10^{-27} erg cm^{-3} sec^{-1}? That energy has to go somewhere. It may be inefficient in accelerating clouds, but that is not necessarily what our 21-cm people are observing. The smaller figure for the energy supply, the one obtained by considering only the very large complexes, seems enough for the mass movements of the clouds.

B. DONN, *Wayne University, Detroit, Michigan:* With regards to Parker's point, where he talks about collisions between small clouds and large ones, we should look at question 7. If the density fluctuations do not involve regions that are separated by a vacuum, then the high-velocity components will not be colliding so much as continuously losing energy to the surrounding medium. Hence, they could dissipate their energy much more efficiently than Parker suggested. The problem is not independent of the distribution of the gas in space.

A. SCHLÜTER, *Max Planck Institut für Physik, Göttingen, Germany:* (1) The efficiency factor of about 10^{-2} which we have assumed for the conversion of ultraviolet light of early type stars into kinetic energy of the interstellar gas, is supposed to take care of the possibility of direct conversion of light into heat. (2) While I don't have any figures, I recall that we have tried to check our estimates of the production of kinetic energy by considering the frequency and the volume occupied by the Hα regions. Since Hα emission can only be observed when the density in an HII region is fairly high, every observable Hα region should be an expanding HII region.

F. D. KAHN, *Manchester University, Manchester, England:* A large fraction of the ultraviolet energy from O or B stars may not be turned into kinetic energy of HI clouds. First, the ultraviolet energy output is used in part merely to separate the electrons from the protons. Second, a great deal of energy is lost in heating the HII region which is pushed outward, and this heat is taken away in cooling by O^+ ions. These two effects might reduce to 1% or less the efficiency of conversion of stellar energy into kinetic energy of the interstellar medium.

G. K. BATCHELOR: Let us now consider question 7, into which we seem to have been led.

G. C. MCVITTIE, *University of Illinois, Urbana, Illinois:* I am very much struck by the sharpness of the two classes of opinions on this question. On the one side, Oort speaks always of the interstellar gas clouds as if they are particles of a gas. It seems to me that he considers them as discrete entities, which sometimes collide and may even knock pieces off each other. Then there are the supporters of turbulence, who regard the medium, as far as I can make out, as continuous, with regions of high density here and there and with differences of velocity in various parts of a continuous medium. I would like to ask: are the two views essentially identical?

G. K. BATCHELOR: If I recall the discussions at the first symposium properly, some people made the remark that when the velocity fluctuations in the turbulence were large enough—and one meant by that comparable with the speed of sound—the pressure variations due to the turbulence would become an appreciable part of the absolute pressure. Thus the pattern of *pressure* presented by a turbulent fluid of such high intensity would show very large variations from point to point. I think that some of the supporters of turbulence theory jumped to the conclusion that the observed variations of *density* within a spiral arm of the galaxy, going from within a gas cloud to a point outside it, corresponded to the large density variations that would accompany the large pressure variations in the violent turbulent motion. However, many of us felt that perhaps it was going too far to allow the density variations due to turbulence to be so large that the clouds lost hydrodynamical connection with each other. For as soon as that happens, they cease to be part of a turbulent motion in the fluid and become separate particles.

I, for one, have never been clear about the explanation of the discreteness of the clouds. I do not see how any theory of turbulent motions of high intensity can lead to the view that there would be separate clouds of high density, proceeding more or less independently of each other.

A. SCHLÜTER: May I summarize a picture which, I think, is largely due to Spitzer and has come out already at the first symposium of this series? It is as shown in Table II.

TABLE II.

	Relative volume	T	n	p
Medium of HII	95%	10^4 K	10^{-1} cm^{-3}	10^{-13} dyne cm^{-2}
Clouds of HI	5%	10^2	10^{+1}	10^{-13}
Regions of HII	0.5%	10^4	10^{+1}	10^{-11}

The HI clouds fill only a minor part of the space and are surrounded by a gas of low density and high temperature—presumably HII—giving the same pressure as in the HI cloud and permitting them to move around without dissolving. Only a very small fraction of the total volume is occupied by dense HII regions of higher pressure, which therefore are expanding.

G. K. BATCHELOR: I take it that the idea of a turbulent motion would not by itself explain the appearance of these separate regions.

A. SCHLÜTER: It is assumed that all regions except the HI clouds are pervaded by ultraviolet light (direct or scattered). The radiation beyond the Lyman limit is effectively absorbed only where neutral hydrogen occurs. This makes the boundary between HI and HII regions fairly sharp. The total ultraviolet radiation of all stars is not sufficient to ionize all interstellar gas, but ionizes only a fraction of it. The un-ionized gas is then exposed to the pressure of the hot ionized gas and therefore adjusts its density so as to balance this pressure, thus forming dense clouds.

G. C. MCVITTIE: How does all this explain the point made so strongly by Oort in earlier Symposia, and by Münch here, that the interstellar absorption lines are always discrete? My impression is that, whenever these lines are seen, they are separated from one another, and I believe this was Oort's evidence for the discreteness of the clouds. What bearing does this discussion of ultraviolet radiation have on the phenomenon?

A. SCHLÜTER: The places of origin of interstellar absorption should largely be in the HI regions, because the density is so high there. And these HI regions, forming discrete entities, should show relatively sharp and discrete lines.

J. M. BURGERS, *University of Maryland, College Park, Maryland:* At the time of the second Symposium I think that Zanstra's mechanism (*Ed. note:* See Proceedings of Second Symposium, reference 1, p. 70) was considered to be a point of importance, and at one time we got the idea that the difference between the two regions is a kind of instability connected with radiation. Turbulence theory itself, considered as a hydrodynamic phenomenon, is not sufficient to produce two such regions; it must be combined with the influence of cooling mechanisms on one hand and radiation from the stars on the other hand. I understand that Zanstra's original mechanism is not so much to the point as it first seemed to some of us, but it may be that something of the kind imagined by him still is happening. Radiation from the stars into interstellar matter, and radiation away from matter into space by a cooling mechanism must certainly form important points in any theory attempting to explain separation into clouds.

M. J. SEATON: I do not think that anything new has arisen which invalidates the Zanstra mechanism insofar as it applies to ionized nebulae, but this has no particular relevance to the topics at present under discussion. Schatzman concluded [cf. Sec. IV] that the Zanstra mechanism would not operate in neutral gas clouds. I would go further and say that at rather high densities ($n \sim 10^4$ cm^{-3}) there may be an "anti-Zanstra mechanism." At low densities the main cooling agent may be C^+, but at sufficiently high densities the C will be mostly neutral. As Schatzman pointed out, neutral C also has low-lying levels, but, due to the absence of a Coulomb field, collisional excitation of these levels will be much less effective than collisional excitation of the C^+ levels. Thus, at sufficiently high densities the main cooling agent may be removed and, so far as this sort of mechanism is concerned, the gas may become stable against the formation of condensations.

G. MÜNCH, *Department of Astrophysics, California Institute of Technology, Pasadena, California:* The strongest argument for believing that the space distribution of the interstellar gas is better described by a set of disconnected independent gas masses, than by a continuous medium with some kind of density fluctuations, is provided by the appearance of multiple interstellar absorption lines. When looking at an interstellar line with, say, 4 or 6 discrete components, separated in velocity from each other by factors of two or three times their width (internal velocity spread), it is not difficult to visualize that a continuously varying density distribution, which might explain the observed facts at present, will in a period of the order of 10^7 years become quite nearly discontinuous. If we could find enough suitable stars nearby each other, we would be able to outline the contours of the gas masses. This possibility at present seems rather remote, as by optical means an attempt in this direction would be extremely laborious. The 21-cm techniques of today are not of much help, because of their low resolving power. The answer perhaps will be given by the first radio astronomer who overcomes the difficulty involved in doing interferometry at 1420 megacycles.

H. C. VAN DE HULST: Direct photographs, i.e., direct observations of the interstellar medium wherever it is visible, very strongly suggest that there are sharp boundaries. Many dark clouds, for instance, have rather sharp boundaries. We can hardly imagine that this happens in a normal field of turbulence. This has been one reason for talking about separate clouds. The other reason still is the separate components of the interstellar absorption lines; although I have to grant that perhaps a field of supersonic turbulence could produce such separate components without really requiring the interstellar medium to be a particle gas.

H. ZANSTRA, *Sterrenkundig Instituut, Amsterdam, Netherlands:* I would clarify one point. Originally, I had hoped to obtain two phases in the case of the HI regions of the interstellar gas—clouds and the surrounding medium. However, I did not succeed. It was Spitzer and Savedoff who made progress; they have got the cooling mechanism and they have got equality of pressure. But there are not two phases; it is not an equilibrium situation.

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

G. K. BATCHELOR: I suggest we look at No. 5, the question about the distribution of the magnetic field along the spiral arms of the galaxy. I understand that polarization data provide the evidence. Could somebody give us a short and simple account of those data?

MRS. ELSKE VAN P. SMITH, *Sacramento Peak Observatory, Sunspot, New Mexico:* Well-studied interstellar polarization of starlight gives the most general indication of a galactic field [see the diagram of polarization observed in the southern Milky Way given as Fig. 1 in Astrophys. J. **124**, 52 (1956)]. The electric vector lies approximately parallel to the galactic equator in that region where the line of sight is perpendicular to the Sagittarius arm, near longitude 280°. The vectors show no systematic orientation in Carina, longitude 255°, where we are looking along the spiral arm, i.e., down the tube defined by the magnetic field. In Sagittarius, on the other hand, toward the galactic center, the alignment is far from parallel, even though one should expect fairly good alignment on the basis of the present picture of spiral structure. Perhaps local clouds and local distortions in the magnetic field are responsible for the heterogeneity here. The stars used for the Southern survey nearly all lie in the Sagittarius arm, but possibly the dust clouds causing the polarization actually are still part of the Orion arm.

There is a similar picture of the polarization in the North, as determined by Hall and Mikesell[5] and by Hiltner.[6,7] In the direction of Perseus, the electric vector is particularly well aligned and the amount of polarization is exceptionally strong. In this region also, the line of sight is normal to the spiral arm axis. Most of the observations for the region near longitude 102° pertain to stars in the Perseus double cluster, which lies in the Perseus arm. Hence, these data are not strictly comparable to observations of the rest of the northern Milky Way, which refer chiefly to Orion arm stars.

In spite of local peculiarities, analysis of the polarization data does strongly suggest a magnetic field coincident with the spiral arms, but subject to distortion or variation in certain regions.

H. C. VAN DE HULST: The argument hinges strongly on the question whether we really know in which directions we are looking perpendicular to a spiral arm and in which directions we look along a spiral arm. If we look along a spiral arm, we should see no preferential polarization at all. In the northern sky, the argument was mainly based on the Perseus region, where there is a strong cluster and evidently a good alignment, but that is only one particular region. In the southern sky, I should expect that looking toward the galactic center we definitely look perpendicular to a spiral arm and yet there is almost no alignment. I do not precisely know where we look along it, but in all directions there is fairly good alignment. My personal impression is that the results of the southern sky are unfavorable to the argument that has been put forward.

MRS. E. VAN P. SMITH: Van de Hulst may be quite right, but I do not think we should overlook the possibility that the polarization of starlight originating in the Sagittarius arm may actually be produced by clouds nearer to us. The spiral structure so close to us is, of course, more difficult to disentangle.

S. B. PICKELNER, *Crimean Astrophysical Observatory, Simeis, Crimea, U.S.S.R.:* Shajn has investigated this question on the basis of the polarization data published by Hiltner, Hall, and Smith. His results support the existence of the arm in Sagittarius. It seems that the spiral arm in this region has another position than was thought several years ago. But as I remember, the 21-cm radio data also show in this direction the concentration of HI.

There is another point which I would bring forward: an anisotropy in the velocities of interstellar clouds, in which the effect of a magnetic field may appear. If a cloud gets its velocity by the mechanism pointed out by Oort, Spitzer, and others, a part of its kinetic energy will be transformed into magnetic energy. The velocity component transverse to the magnetic field, therefore, should be less than the longitudinal component.

We considered this question with the aid of Adams' catalog. The velocities derived from the components of the interstellar lines were corrected for the rotation of the galaxy in selected regions of the Milky Way and are shown in Fig. 1. The distances of the clouds were supposed equal to half the distances of the stars. Positive velocities are marked with points and negative velocities with crosses. The main components of the lines are without circles, the secondary components with circles. The reason to distinguish between them is connected with Donn's paper.

The upper part of the figure refers to stars lying in the direction of the normal to the spiral arm, and the two others are in directions along two spiral arms. These stars belong to the group near ζ Persei, which recede from the sun with a velocity of about 13–18 km/sec. The gas expands with a velocity of about 6 km/sec. The slow components are not very reliable. The fast components apparently are larger for the direction of the arm. If we omit the fastest components, the difference will be more pronounced.

The slow components in the two lower pictures permit us to check the hypothesis about the motion of the gas along the spiral arms (E. Edmondson and V. Rubin). The slow components show that the syste-

[5] J. S. Hall and A. H. Mikesell, Publ. U. S. Nav. Observ. **17**, Part I (1950).
[6] W. A. Hiltner, Astrophys. J. **114**, 241 (1954).
[7] W. A. Hiltner, Astrophys. J. Suppl. **II**, No. 24, 389 (1956).

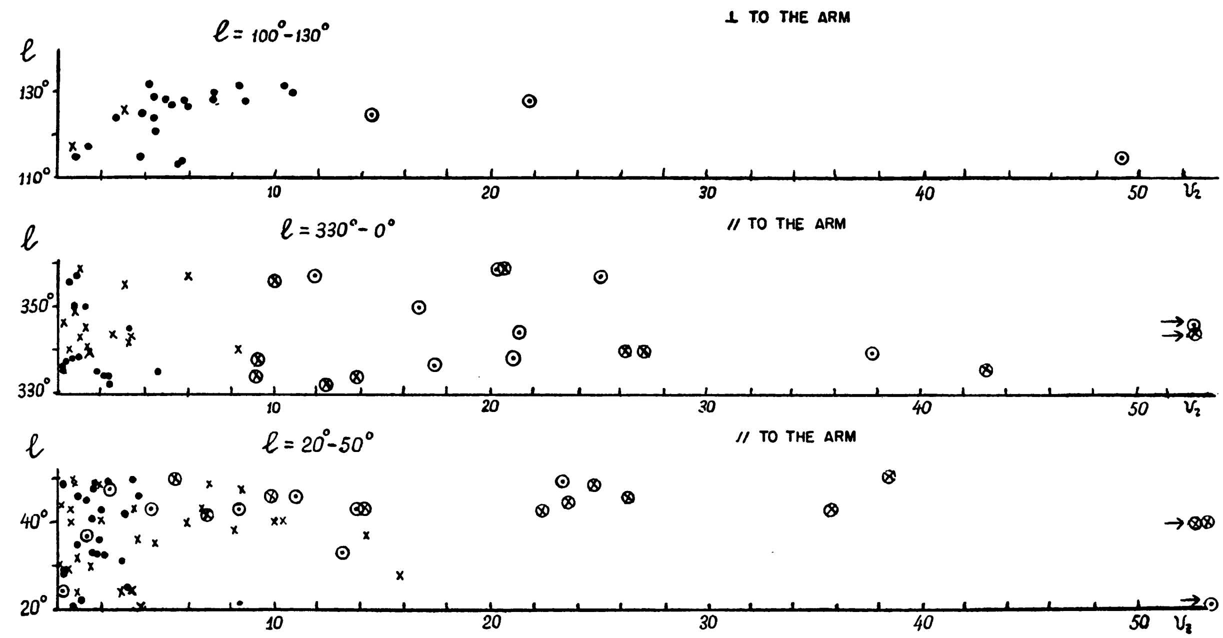


FIG. 1. Velocities from H and K lines.

I II
● ⊙ — $v_r>0$.
× ⊗ — $v_r<0$.

matic motion of the bulk of the gas along the spiral arms is less than 5 km/sec relative to the nearest stars.

A. SCHLÜTER: I doubt that there really should be a large effect of the kind assumed by Pickelner, i.e., that the motion should really be much slower perpendicular to the lines of force than parallel to the lines. You can have two extreme kinds of motion perpendicular to the lines of force: one is an oscillatory kind of motion where one essentially moves a point of the line of force up and down, and this is not hindered by the magnetic field. The other is where you move the whole line of force. There might be a preference for the motion to be along the lines of force, but I would not think that this preference would be a really large one.

Another point is the question of the interpretation of the velocities measured by the H and K lines. It was found years ago by Searle that if you take the large velocities observed in the H and K interstellar absorption lines (and these are the velocities Pickelner is dealing with) you find that the majority of these velocities lie towards the observer, essentially independent of which direction you are looking. This means that absorption occurs in a region which is intimately connected with the star in the light of which the absorption is observed, i.e., in the circumstellar region which is expanding around the star. This, of course, makes interpretation rather dubious.

S. B. PICKELNER: The energy of oscillation consists of the kinetic and magnetic energies. In the direction along the lines, the kinetic energy is the same as the total energy. If the total energy is similar in both cases, we should observe the effect mentioned. But fast motions are possible as the result of expansion from the stars. We compared the radial velocities of the stars which show the interstellar lines, and the radial velocity of these lines. The velocities of the stars are much less than the radial velocity of the clouds, because we compare only very fast clouds. If the shell does expand, its expansion perpendicular to the magnetic fields will be retarded, and its expansion along the magnetic lines will not be retarded so much. After some time the expansion perpendicular to the magnetic lines must be slower than that along the lines.

G. MÜNCH: In regard to Schlüter's point that he suspects that the clouds have something to do with the stars, I must observe that it can be shown that there can be absolutely no direct physical relation between the stars and the interstellar lines observed. In doing statistics with the observations of Adams, it should be kept in mind that the stars represented are strongly selected according to brightness. Many of them belong to large associations, around which there must be a general expanding tendency of the interstellar gas. It is thus to be expected that there will be a preponderance of approaching velocities of the interstellar lines observed in such stars. If Adam's catalog contained stars beyond the Orion arm, no asymmetry would appear in the distribution of high velocities. My observations of stars in the Perseus spiral arm verify

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

this point. A line formed in an expanding association, however, is truly interstellar and I would object strongly to the use of the word "circumstellar" in referring to them, because in stellar spectroscopy such designation has a special and precise meaning, quite different from which is implied in our context.

A. SCHLÜTER: That is exactly the mechanism which I meant.

F. D. KAHN: May I link up this question of expanding shells with question No. 1? If there are really shells which are expanding from O stars with speeds of 50 km/sec, we can be pretty sure that the energy fed into the interstellar material by such O stars must be quite high.

S. B. PICKELNER: Shajn's investigations, to which I referred before, bring out still another effect. In a diagram of galactic longitude of the stars *vs* direction of polarization, there was found a systematic effect which can be interpreted as a sine curve inclined at an angle to the galactic plane of about 18°. This means that the plane of the average magnetic field in the vicinity of the sun is about 1000 pc in diameter and is inclined to the galactic plane at this angle. This effect may be a fluctuation in the direction of the spiral arms. It is known that these arms are not in one plane, but sometimes form a complicated curve. Further, in this distribution there were some places where local fields with a scale of about 10 pc were observed. In such regions, there are several stars in whose spectra the direction of polarization lies inclined to the common direction at angles of 20, 30, and more degrees. These regions may be considered as the real local fluctuations of the field.

T. K. MENON, *Harvard College Observatory, Cambridge, Massachusetts:* Münch mentioned that radio-astronomy observations, mainly because of the larger beam width, might be a little blind as far as the motions are concerned. But there is the possibility that with the availability of dishes of high angular resolution, we may be able to select in the galaxy two regions, parallel and perpendicular to a spiral arm, and fairly homogeneous, and carry out a correlation analysis by taking the peak velocity of the hydrogen profiles at closely spaced intervals. In some of the regions I have been considering, these intervals are only $1\frac{1}{2}$ to 2 pc apart for the beam width that is available at Harvard. We get complete velocity field measurements, giving not only the peak velocity, but also the random motions for the hydrogen profile. Thus, we can find out whether there is any difference in the velocity correlations in the direction perpendicular to the spiral arm and parallel to it. Probably this may be a better way to judge whether there is a difference in motion in the two directions, because one does not have the optical selection effect in such a case.

B. DONN: If the cloud velocities are strongly affected by expansion, the sort of analysis Blaauw carried out (which I also tried) to determine from velocity distribution the random velocities of these clouds, does not mean too much. We are not getting random velocities, but rather mixed effects including systematic expansion. Something which seems to contradict this is the fact that the low velocity clouds appear to follow the galactic rotation quite closely. This suggests that their velocity is not affected so much by the expansion.

H. C. VAN DE HULST: It is not quite correct to say that the observations are too poor, unless we should say that although the polarizations are well observed, we do not know exactly where the spiral arms are. We know fairly well the density distribution, but we do not exactly know what to call a spiral arm, especially on the small scale in our immediate neighborhood about which we are now talking. Pickelner suggested that everything would be remedied if, from the sun, we look along a spiral arm in the direction $l \sim 340°$. I do not see any direct evidence for this suggestion in the observations, but within this rather small area it is difficult to interpret the 21-cm data. Oosterhoff's data on the Southern Cepheids distinctly give the impression that the Sagittarius arm would continue at the same distance from the sun and, that in the direction to the galactic center, we are looking perpendicular to it.

M. P. SAVEDOFF: The asymmetry in the velocities—that is, the excess of velocities toward us—shows up only for velocities exceeding something in the neighborhood of 20–30 km/sec. Above 30 km/sec there is a very small effect that might be a statistical fluctuation, and below 20 km/sec there is no strong effect at all.

G. K. BATCHELOR: There is one more observational question, No. 4, concerning the galactic halo, which we should consider.

G. FIELD: My impression from the curves which van de Hulst showed of the high-latitude 21-cm emission, was that in fact there is a high-velocity component which may be identified with the galactic halo that Pickelner has suggested. I am further supported in this idea by the fact that if you compute, simply using Pickelner's data, how much radiation one would expect from such a halo, you predict a temperature of about 3°K with a velocity distribution approximately as is observed. The crucial question seems to be

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

why the velocities are negative. Heeschen has pointed out, that if there really were a spherical distribution, we might expect it to be at rest in the galactic system; that is to say, we would have a component of motion due to the rotation of the flattened system with respect to it. I wonder if van de Hulst has any comments on what would happen if we took out that rotational velocity. Would the material then be distributed symmetrically about zero velocity?

H. C. van de Hulst: The observations really are quite preliminary. I agree with the computations: if we have neutral hydrogen density $=10^{-2}$ cm^{-3}, an extent of the halo $=12$ kpc, and a line width $=100$ km/sec, we can compute that the brightness temperature is 2°K. In the galactic system a brightness temperature of this order has been observed, but my impression is that the velocity dispersion is lower than would be needed by Pickelner. This gas would reach distances one or two kpc above the galactic plane and not extend throughout the halo. However, if we want to assume any similarity between the Andromeda nebula and the galactic system, we can look straight along a full diameter of the halo. We then have to see at least 3°K where we observe something like 0.3°K. So the effect should be 10 times larger than is observed.

S. B. Pickelner: The observation of the halo in 21 cm depends very strongly on the state of ionization of the halo gas. The ionization may change, as I stated in our report, from 10% to 80%. It depends very strongly on the physical conditions, on the density of the gas, on the magnetic field and on other effects. The ionization may be 2 or 3 times less and the halo cannot be observed. Finally, we note that the density of the halo decreases upward very slowly, only by a factor of 2 in 10 000 pc, and the 21-cm line is emitted in a layer about 10 kpc thick.

Heeschen: I think that there is observational evidence for a 21-cm halo from the observations of other galaxies. Four galaxies have been observed in 21-cm emission: M 31, M 33, M 81, and M 51. I think that in each case the observations show evidence for a very extensive distribution of HI, although the observations cannot really determine whether it is concentrated into a plane or whether it is in a halo. In the case of M 31 (observations at Harvard by Heeschen and Dieter), drift curves in right ascension about a degree from the center of the galaxy show a peak of emission at the major axis and a long wing extending about 5° from the major axis in the direction of smaller right ascension. The intensity observed on the major axis agrees with the Leiden observations as published, while the wing is 1°K or 2°K in intensity. In the case of M 33 and M 81, we are also finding a large extent of HI, although the observations are rather uncertain at the moment. In the case of M 51, however, there is very clear evidence for a very large extent of HI. A drift curve across M 51 shows a half-power width of 2° in the hydrogen line. The optical size of the galaxy is of the order of $\frac{1}{4}$°. The observations have probable errors of about 0.2°K, while the observed intensity is 4°K. The density of hydrogen, assuming that it is in a spherical halo, comes out to about 0.01 atom per cm^3. I have frequency scans made with the antenna tracking the center of the galaxy and from these the width of the line is of the order of 40 to 50 km/sec. So I think there is some evidence for an HI halo.*

A. R. Kantrowitz, *Avco Research Laboratories, Everett, Massachusetts:* In asking for discussion on question 2, does the magnetic field inhibit dissipation in the collision of two gas clouds, I would like to recall how it arose at this symposium. I think Parker asserted that one did not have enough energy input into the interstellar velocity fields to compensate for the dissipation that one would compute applying the Kolmogoroff spectrum—and corresponding turbulent dissipation—to the interstellar velocity fields. Therefore, he sought an inhibiting mechanism, offering the magnetic fields. Pickelner has supported this viewpoint. Others have opposed it. Remarks?

W. D. Hayes, *Department of Aeronatuics, Princeton University, Princeton, New Jersey:* It might be well to comment on some of the features of fluid-mechanic effects as opposed to electromagnetic effects. The fluid-mechanical effects are fundamental nonlinear and discontinuity-producing, which leads to the high dissipations that appear in ordinary hydrodynamics. The actual values of the dissipation coefficients usually have little or no effect on the over-all dissipation in a fluid mechanical process. Electromagnetic effects are fundamentally different in this respect. They are linear or almost always linear and, as far as I know, are not discontinuity-producing. A gradient is not steepened by electromagnetic effects; there is, however, the possibility that a hydrodynamic effect will cause an increase in a gradient which will cause an increase in an electromagnetic dissipation. For example, if fluid-mechanical effects cause a concentration of current, then the Joule dissipation will increase, but the original concentration of current cannot come from the electromagnetic effect itself.

W. V. R. Malkus: I believe this question must be somewhat rephrased to be satisfactorily answered—i.e., one must distinguish between magnetic fields produced by and intimately associated with the turbulent motion

* *Note added by editor in proof.*—Dieter's thesis, Harvard, 1958, presents evidence for a halo in M 33. Peak emission −4°K; velocity dispersion −50 km/sec; density ~0.1 M_H within radius 34 kpc; total halo mass ~3% mass M 33.

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

(whose half-life in the absence of renewal by the energy sources is estimated at 10^7 years or so) and quasi-steady magnetic fields such as those postulated to be associated with, and controlling the structure of, the spiral arms (whose half-life may be comparable to the spiral arm structure, say 10^9 years). The existence of these latter fields is somewhat in doubt experimentally, and Biermann tells me that plausible explanations for spiral arms may be advanced without postulating quasi-permanent magnetic fields. Even for infinite conductivity there appear to be a variety of ways in which magnetic energy could decay and be dissipated. Hence, semipermanent fields would either not exist or would be due to a highly organized process which continuously re-establishes them. If such fields do exist, the theoretical evidence is that they restrict the motion and reduce the rate of dissipation. But consider the other question: "Do the magnetic fields generated by and associated with the turbulent motions reduce the rate of dissipation of the turbulent energy?" The answer comes in three steps. (A) We first recognize that turbulence develops because the turbulent state of the medium is in some statistical sense "most stable." The statistically "most stable" turbulent motion releases more of the available potential energy than any alternate state or it wouldn't be the most stable. (B) We then ask why turbulent motion in a magnetic field generates the coexisting turbulent magnetic fields? Again, we must conclude that in some statistical sense the resulting mixture of magnetic fields and velocity fields is more stable than an alternate state and hence releases more of the available potential energy than any alternate state. (C) Therefore, I conclude that the magnetic disturbances associated with the turbulent field assists the rate of dissipation of potential energy into thermal motion, cosmic rays, and any other significant sink of macroscopic energy.

R. N. THOMAS: In other words you assert that the presence of the magnetic field increases dissipation rather than decreases it?

W. V. R. MALKUS: Yes, that part due to the turbulent motions themselves.

T. K. MENON: I would like to bring up an observational point which has been troubling me for some time. Three years ago, Lilley found from the 21-cm observations an almost perfect correlation between central intensities of hydrogen profiles and absorption determined from counts of external galaxies in the same region. But when he tried to correlate number of hydrogen atoms in the line of sight and absorption, the correlation was not as good. Since then we have in a number of cases determined in detail whether this correlation exists, and it seems that the first correlation does show up over large regions. If we investigate in detail over smaller regions, this correlation does not show up. But over all it seems to me that the correlation regarding the central intensities holds, which means essentially that there is a correlation between the random motions and the absorption. We would ask the cause of this decrease in the random motions in regions of high absorption—whether it is due to the macroscopic motions being inhibited by the magnetic field or arises from some other effect?

H. K. SEN, *GRD, AFCRC, Hanscom Field, Bedford, Massachusetts:* Has Malkus' comment any bearing on the results of Lenard, where the magnetic field sometimes has a stabilizing, at other times a destabilizing, influence?

H. W. LIEPMAN, *Daniel Guggenheim Aeronautical Laboratory, California Institute of Technology, Pasadena, California:* The last remark does not really apply because you have a magnetic field which is controlled from the outside, not a closed system. In Lenard's experiments you have to count in how much current he has to feed into his coil. And I think we have not specified very well the type of system in the collision of two clouds. Do we talk about dissipation between the two clouds only, or in the whole?

H. K. SEN: The crux of the discussion is that the magnetic field may not have a direct influence, but might alter the velocity distribution field, and in that way affect the problem. So we are not really concerned with whether the magnetic field is applied from outside or generated inside.

J. M. BURGERS: While the problem seems to be not quite clearly posed, it comes down to this question: suppose there is a general magnetic field in the galaxy about whose origin we do not worry. Inside this field, you have two highly conductive clouds which approach each other. Is it possible that the presence of the magnetic field will make the clouds collide virtually elastically, or is it more probable that even in this case the collision will be inelastic and all relative motion will be transferred into heat?

A. R. KANTROWITZ: There are a couple of experimental answers to the question just phrased by Burgers. One slide was presented on Tuesday by Petschek, which might be useful to repeat; Bostick has another.

W. BOSTICK, *Stevens Institute of Technology, Hoboken, New Jersey:* Two figures from my paper illustrate collisional phenomena between two plasmoids (*Editor's note:* Cf. diagrams accompanying Bostick's paper, page 1091, Figs. 1 and 2). The first figure refers to a situation in vacuum; the second, in the presence

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

of a conducting medium, a low pressure ionized gas. The magnetic field goes into the figure. In the first case, the two plasmoids, as they approach each other, avoid one another—it is a collision between two billiard balls except the balls don't quite touch and they go on their way. The speeds of the plasmoids are about 100 km/sec. In the second case, the plasmoids have a tendency to avoid each other—the black line down between them indicates that there is a magnetic field trapped in between. Finally, they coalesce together as the magnetic field and the plasma finally penetrate at the end of 10 μsec. My paper shows other examples.

A. R. KANTROWITZ: Refer back to Petschek's paper; Fig. 3 shows a collapsing ring of gas with a magnetic field in the center. I want to recall Petschek's point—the gas moves in toward the center and looks very bouncy. It looks as if it is possible to achieve situations where there is apparently very little dissipation. This is a gas at about 2×10^6°K so that it has a high conductivity.

H. E. PETSCHEK, *Avco Research Laboratory, Everett, Massachusetts:* In this case there was an appreciable magnetic field in the center of the ring, which increased as the gas came in. There are also experiments where the magnetic field in the center is small. In this case, the gas comes in closer to the center, and one does not get such an elastic collision. It was definitely true that, in this case, the magnetic field in the middle has made the collision more elastic.

H. W. LIEPMAN: I do not understand what this has to do with dissipation, because even if you bounce two clouds off, the dissipation can be in the internal motion. After all, two colliding diatomic molecules bounce off, but still may have transformed vibrational energy. One has to study the detailed motion to be sure the collisions are elastic.

A. R. KANTROWITZ: If the collisions are soft in the sense that they take place over a period of time long compared to the travel of a sound wave across the mass, then they cannot excite very great amounts of internal motion. This is the point. The collisions are gradual rather than sharp.

F. D. KAHN: In interstellar space, the mean free path of the neutral atoms for collisions with the ionized atoms is just about equal to the thickness of an interstellar cloud. That should make quite a difference between what happens in interstellar space and in the laboratory. With the usual data, the mean free path for collision of an atom with an ion is 10^{19} cm, or 3 pc.

L. SPITZER, JR.: What is the relevance of this figure? I would have supposed that the important quality was the mean free path for a positive ion, the scarce constituent, to collide with the abundant constituent?

F. D. KAHN: No. The ions are bound to a magnetic field, and only they can stop the motion of the neutral gas. Now the neutral atom moving through a cloud has to move 10^{19} cm before it has an even chance of hitting an ion. Therefore, if you have clouds of 10^{19} cm size, the neutral matter will tend to spill out.

L. SPITZER, JR.: I think that your picture is somewhat simplified. A hydrogen atom can collide with many other hydrogen atoms and, if it collides with atoms which have collided with carbon ions, it can then be slowed down. The correct procedure, I would suppose, is to use the actual equations for the drift velocity, which Biermann and others have developed.

A. R. KANTROWITZ: I think the point, however, is clearly well taken that one must not apply these results, which were gotten with almost completely ionized gases, to astronomical situations without due care.

L. BIERMANN: This figure of 10^{19} cm is in reality not a straight path but one composed of very small pieces by the collisions with the neutral atoms, which I think is much what you said. There is, therefore, not a straight path which could be compared with the diameter of the clouds.

R. LANDSHOFF, *Missile Systems Division, Lockheed Aircraft Corporation, Sunnyvale, California:* In the picture we have seen two ionized objects get together, and they completely repelled each other. Do the astrophysical objects actually repel each other, or do they go through each other?

H. C. VAN DE HULST: To summarize from the astronomer's standpoint: two clouds or local regions of high density approach each other. Do they interpenetrate, completely lose their identity, and lose most of their mass motion in the form of thermal motions? Or do they temporarily create a high density of magnetic fields and then get away again with their original identity and with most of their original motion? The main cause of wonder of the astronomers when these symposia started was that there are any clouds at all; for one can compute that in 10 million years they would have collided so often that the motion would be entirely smoothed out. If the clouds had magnetic fields which really help to inhibit this smoothing process, this may help our understanding them. The only alternative would be that all of them are quite young.

M. MINNAERT, *Sterrewacht Sonnenburg, Utrecht, Netherlands:* If indeed there would be elastic repulsion between two gas masses meeting in the presence of a general magnetic field, then that would mean that if two galaxies collide the stars would go through, while the gas masses would be repelling each other, and the result would be that the stars would interchange the gas masses!

L. Spitzer, Jr.: There may be some question as to what happens at the low relative collision velocities that we have within a galaxy, but I think there is no question as to what happens when they collide at 1000 km/sec. Those collisions cannot possibly be elastic, so I think we need not worry about that particular problem. As someone pointed out earlier, there is not enough magnetic field to give elastic collisions at 1000 km/sec.

A. R. Kantrowitz: You are taking a highly extraordinary astronomical situation—1000 km/sec—yet you insist on the ordinary values of the magnetic field strength, and then compute that the momentum is insufficient—it's too large for the magnetic field. Can one not under these circumstances have a larger magnetic field? It's only required to be one order of magnitude larger, as I remember. Two orders in the pressure, one in the magnetic field. Is that right?

M. P. Savedoff: No. There is no appreciable effect when the magnetic field pressure is 21 000 times the local pressure.

A. R. Kantrowitz: Is the local pressure a relevant item, or is ρv^2 the relevant pressure?

M. P. Savedoff: Well, you have a galaxy that has been moving around by itself so the relevant pressures are the pressures in the galaxy. You can then say that you have an unusual galaxy with a high magnetic field; and I will report on a case where the magnetic pressure in the galaxy itself—as compared to its own internal motions—is dominant by 21 000 times the internal motion pressure. Even in that case you do not get strong magnetic effects as long as the magnetic fields are confined to the medium. But if there are extensive fields between galaxies, of which at the moment we have no information, there is the possibility that intergalactic fields can balance things, but we would like to know a lot more about the intergalactic fields.

S. B. Pickelner: I agree with Spitzer that collisions between the two galaxies are not elastic, because the density of the magnetic field is much less than the kinetic energy. But if we have the collision of two distinct masses, the results of this collision will be the expansion of two gases with some velocity, less than the initial velocity, because the dissipation cannot be total, part of this energy will stay in the form of kinetic energy. Second, I cannot repeat my report, but I remember that the dissipation of perpendicular weak shock waves is decreased by the action of the magnetic field if the density of the magnetic field is the same as the density of the kinetic energy. This result holds also for the inclined wave if the angle is not very large. This calculation was done by O. Gollandsky. Third, between the clouds there is a magnetic field. This field is necessary to retain the cosmic rays in the galaxy. The energy of the field is comparable to the kinetic energy of the clouds. It follows also from the radio data that this space is not a vacuum. There may be a field in a vacuum if you have a magnet, there is no field in a vacuum in this hydromagnetic case. If the energy density between the clouds is the same as in the clouds, we cannot discuss the collisions of the clouds in vacuum—we must discuss the continuous medium.

M. Minnaert: We return now to the discussion put before you by van de Hulst. Suppose we have a diluted gas, in a space where there is a certain faint magnetic field. We stir that gas, we produce turbulence, and conjecture that the magnetic field will be increased. The question was: "Will there eventually be equilibrium and equality between the magnetic energy and the turbulent energy generated in this turbulent gas? And if this is the case, how much time will it take to establish that equilibrium?"

E. N. Parker: Let me repeat the view which I expressed on Monday. I suggested that a magnetic field whose energy density is weak compared to the density of the kinetic energy will be distorted, if the conductivity is very high, until some approximate equality (I mean: within a factor of 2 or 3) may exist between the two. On the other hand, if one starts off with a magnetic field which is strong compared to any motions which are present, then the two will forever remain independent. The magnetic field exists independently of the velocity field and in no way requires mass motions to perpetuate its existence: the electrical conductivity sees to that. I suggested that this was the case in the galactic arm.

G. K. Batchelor: The question of equipartition between magnetic energy and kinetic energy is strictly relevant only to a case of some kind of random motion. Equipartition is a statistical term, so that it is relevant only to cases of turbulent motion. When an ordered motion is present, there is no sense in speaking of equipartition or lack of equipartition between kinetic energy and magnetic energy. In any ordered motion, one can find places in the field where one is large compared with the other, places where the reverse is the case, and places where they are equal. The more relevant situation is one involving turbulent motion, where, as a result of statistical interaction between the two kinds of energy, some sort of balance will be built up.

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

In his opening talk to the Symposium, Parker supposed that the strength of the magnetic field along the galactic arm was such as to make the magnetic energy large compared with the kinetic energy. This does not conflict with any principle of equipartition; such a large magnetic field might well have been produced by the same ordered motion that led to the creation of the spiral arms. This is a problem which lies outside the present discussion.

With regard to those situations where the question of equipartition does apply, the problem of turbulent motion (and for simplicity we can take the turbulence to be homogeneous and isotropic, with a correspondingly statistically homogeneous and isotropic magnetic field), the answer is not yet known with confidence, as has been pointed out in the two previous Symposia. A number of people believe that there will be a rough equality between the two kinds of energy. My own view is that the magnetic energy increases as time goes on, to an ultimate level which is comparable with the kinetic energy of the smaller eddies responsible for the stretching of magnetic lines. There is also the view that the magnetic energy increases until it is comparable with the *total* turbulent energy. Biermann and Schlüter advocate this viewpoint.

M. MINNAERT: So the consensus seems to be—no equipartition in the spiral arms. But can there be equipartition in the galactic halo?

G. K. BATCHELOR: It will be proper to ask whether there is equipartition in the halo, if no definite ordered motion exists in the halo region. As to whether there is or not—that is the question to which the above two answers have been offered.

L. BIERMANN: Schlüter and myself argue that, since magnetic energy is assumed to be fed into the system at *all* wave numbers, the wave numbers where equality is reached are slowly decreasing with time, in the sense that, given infinite time, ultimately equality would be reached at the smallest wave numbers. At the present time, with an age of the galaxy of some billion years, a state would be reached with equality of the energy densities at a scale corresponding to, say, 100 pc or something of that order. This reasoning rested upon a general consideration about the exchange of energy between the spectral components of each of the two fields in question, and how the energy contents of both spectra are interlinked with each other (Biermann[8]). Similar arguments have been used in Chandrasekhar's recent work.

W. REID, *Yerkes Observatory, Williams Bay, Wisconsin:* Since Chandrasekhar's work has been mentioned, I would indicate what I think is a limitation of it. It is a stationary theory, hence deals, at most, with the small scale structure of the motion. It is shown that under these conditions the spectrum of the magnetic and the velocity fields are related by a factor of the order of 2. While this result suggests that equipartition may exist for small scale motions, it cannot throw any light on the question respecting large scale motions.

L. BIERMANN: In one of his last papers, Chandrasekhar clearly states that he reconsidered his position in this question, after originally having taken the view of equality of energy at high wave numbers.

L. SPITZER, JR.: I have had considerable discussion with Chandrasekhar on the significance of his results. I believe the situation is this: He has certain equations which he solved for the steady state. In this steady state, as I remember, there is a large amount of energy in the largest eddies (the very small wave numbers). If, however, you look at his equations, including the time-dependent terms, you will see that if there is no energy present initially at wavelengths above a certain limiting value, there will never be any energy at these wavelengths. There is no mechanism in his equations for building up the energy in the big eddies, if there is no energy initially.

E. C. BULLARD, *Cambridge University, Cambridge, England:* There is a matter which touches also on Bostick's earlier remarks; namely, if you want to produce a magnetic field over a very large volume of space, such as a spiral arm, you cannot do this by starting a current in a very large circuit by applying an electromotive force of any sort, because the times required are something greater than 10^{30} years to get the current running. I think the only way that you can get a magnetic field in a spiral arm is either to have the Lord put it there to start with, or alternatively, to draw it out in some way, as I think was suggested by Parker the other day.

W. BOSTICK: Against what Bullard has said, I would observe that although we may never get the current up to equilibrium (which would mean a fantastic storage of energy), this does not mean that there will not be some current flowing in the early part of the process. It may well be that the magnetic field within the arms is constantly increasing.

E. SCHATZMAN, *Institut d'Astrophysique, Paris, France:* In explaining the polarization of interstellar absorption lines, Spitzer mentioned that there are two possibilities. Can we decide between them?

L. SPITZER, JR.: One possibility is that the lines of force of the magnetic field are oriented in a preferential direction along the spiral arms; this situation is the one

[8] L. Biermann, *Kosmische Physik*, edited by W. Heisenberg (Springer-Verlag, Berlin, 1953), second edition, Chap. I. 4.

analyzed by Fermi and Chandrasekhar, and is usually assumed. The other possibility is that the field is a turbulent field, but that the distribution of field directions is ellipsoidal, with the major axis of the ellipse parallel to the spiral arm. In this situation there is no mean field, but there is a mean square field component along the spiral arm, which is stronger than the mean square field component perpendicular to it. A field of the second type might provide little interference with interstellar motions, since one could assume that such a root mean square field would be weaker than the steady field along the spiral arms required by the other alternatives.

There are various ways in which one might hope to distinguish between these two possibilities. One way is to do the sort of work that Serkowski has been doing—to look at the microstructure of the magnetic field and see if one finds smaller regions and more crinkly fields than one would expect from the picture of a uniform field along the spiral arms. Another way of analyzing the problem is to look at the energetics. It may be that to confine the cosmic rays, one needs a stronger field than one can twist by turbulent motions. At the present time the problem is really wide open, and there is no way of deciding conclusively whether the magnetic field is a uniform field along the arm, with slight deviations, or whether it is a random field with root mean square components which are preferentially parallel to the arms.

M. MINNAERT: I propose that we now pass to the last problem: given a field of stars which define a gravitational potential field, and given gas between these stars, moving in laminar and circular orbits, what will happen? Will turbulence originate, and in what time scale?

G. K. BATCHELOR: I think that a simple statement is possible. Supposing we have two-dimensional motion in circles around an axis of symmetry, the velocity at radius r being equal to v. The problem is: for what distribution $v(r)$ will centrifugal forces make the motion unstable? Of course, for any deviation from constant angular velocity, there is shearing motion, and usually at high Reynolds numbers, any shearing motion leads to instability. But there is the qualification that there must not be a strong centrifugal action which stabilizes the motion.

The action of centrifugal forces is easily found by imagining what happens when a material ring of fluid particles increases its radius. This was an argument first used by Rayleigh. In the course of the transition from one radius to another, the circulation vr is preserved; this is equivalent to conservation of angular momentum. Consequently one knows the velocity of the ring in the new position, and if the ring of particles has a higher velocity in its new position than those of the fluid particles in the environment, there will be an excess centrifugal force above that needed to balance the radial pressure gradient and the ring will tend to move further out. Conversely, if it has a smaller velocity in that new position than the particles in its neighborhood, then it will have lower centrifugal force and it will be returned to its initial position. Consequently, one has the general criterion that if $d(vr)/dr<0$ in the undisturbed fluid, there will be instability; if $d(vr)/dr>0$ there will be stability. This is based on the assumption that the Reynolds number is reasonably large, which is certainly the case in the galaxy.

If I remember the plot of the galactic rotation that was given by van de Hulst, *vr increases* over the whole range of the graph, and there is thus no region where the criterion for instability is satisfied. On that simple basis it would seem that there is no reason to expect galactic rotation to be a source of turbulent energy.

M. MINNAERT: Is it the same thing in the dimensions above or below the plane of the galaxy?

G. K. BATCHELOR: The simple criterion that I described is valid only for a velocity distribution that is independent of distance at right angles to the diagram. But we have no further knowledge about the way in which the distribution may change in that direction.

E. N. PARKER: Pickelner has suggested that we have an HI halo, or at least a lot of HI regions in the halo, where the thermal velocity presumably is of the order of 2 km/sec, and that on the other hand there are mass motions of 100 km/sec. He has also suggested that there is a magnetic field of comparable energy density, but what worries most of us is how to avoid the tremendous dissipation, because even in a strong magnetic field there will be motions and collisions along the magnetic field, which will set up shock waves and so forth. We must note, however, that there is not just one gas, HI, but two gases: HI and cosmic rays, in the same regions. If there are a lot of little weak shocks, so that there are small kinks in the magnetic field everywhere (this is merely a hypothesis for the moment), then the HI gas and the cosmic rays both are coupled to the magnetic field, and thus are coupled to each other, so that they will move as one gas, in a rough sort of way. If we want to compute the speed of sound in this two component fluid, I think that this should be $[(\frac{1}{2}\rho u^2+E)/\rho]^{\frac{1}{2}}=100$ km/sec where $\frac{1}{2}\rho u^2$ would be the ordinary thermal energy (u being the thermal velocity) and E is the kinetic energy density of the cosmic rays, for which I take Pickelner's figures. I am suggesting, therefore, that the mass motions are not supersonic in the true sense, so that the Mach number is not large and we may not have large dissipation.

Incidentally, the same trick works in HI regions in the spiral arm of the galaxy.

https://doi.org/10.1017/S0074180900226764 Published online by Cambridge University Press

S. B. Pickelner: We have in the HII region of the halo a temperature of about 15 000°K. The sound velocity is about 20 km/sec. In calculating the sound velocity, the cosmic-ray pressure is taken into account for the perpendicular wave (the magnetic field is parallel to the front and the movements are perpendicular to this front). The cosmic-ray particles spiral around the magnetic lines. The mass of the gas is essential, because the inertia of the gas must be introduced. The magnetic field gives a pressure and the cosmic rays also give a pressure; when the cosmic-ray pressure is the same as the magnetic pressure, the sound velocity is

$$[a^2+H^2/4\pi\rho+\tfrac{3}{2}p_{cr}/\rho]^{\frac{1}{2}}.$$

A. Schlüter: I should be somewhat reluctant to calculate the sound velocity in this way, because the gas of cosmic rays is a gas of very high viscosity. The mean free path of the cosmic rays being very high, we get, at least along the magnetic lines of force, an almost infinite conductivity as long as the magnetic lines of force are stretched.

S. B. Pickelner: The viscosity of the cosmic rays is not important in this case. The radius of curvature is not very large. The free path of the particles in the direction perpendicular to the field is this radius. The free path of the particles along the field is large, but I considered an infinite plane wave and neglected the motion of the particles.

A. Schlüter: If one considers a special motion across the lines of force, you are completely right. That is why I said that the viscosity is essentially infinite along the lines of force.