27. FINAL DISCUSSION

(Thursday, September 18, 1969)

Chairman: R. N. THOMAS

Editor's remarks: In the present form the Final Discussion is divided into three sections: 1. Discussion on the Overall Energy Flow in the Interstellar Medium; 2. Summaries and Suggestions for Future Research (more or less prepared talks); 3. Summaries and Suggestions for Future Research (more or less free discussion). The discussion in Section 1 actually took place on Saturday, September 13, but it belongs in the Final Discussion. The (invited) summary papers, presented in Section 2, were given by Parker, van Woerden, and Kaplan. Section 3 starts with the remarks by Busemann on the interrelation between interstellar gas dynamics and aerodynamics, as viewed by an aerodynamicist; these remarks were made on Tuesday, September 16. The rest of Section 3 is taken up by the actual discussion held on the final day of the Symposium.

In view of the total length of this Final Discussion I have felt it necessary to shorten several of the longer contributions. I apologize for this, although I am convinced that the text presented here is closer to what was actually said than the texts submitted to me by some of the authors.

1. Discussion on the Overall Energy Flow in the Interstellar Medium

[This section of the discussion actually took place on Saturday, September 13.]

Van de Hulst: In my introduction the first day I mentioned that the energy balance was a major point in earlier Symposia. I have waited until now to see if I could picture the total energy flow from one reservoir into another. Consider the very schematic diagram of Figure 1. The top circle *SN* represents supernovae but is meant to include also other stellar sources of energy such as OB stars. Below it are the cosmic rays *CR* (right) and the magnetic fields *MF* (left). At the bottom I have drawn the energy reservoirs *GK* containing gravitational (potential) energy and kinetic energy, conveniently combined because there may be a lot of give and take between these two reservoirs by the ordinary laws of motion. The unit rate of energy exchange is 10^{-26} erg cm^{-3} sec^{-1} [$=0.2$ eV cm^{-3} $(10^6\ \mathrm{yr})^{-1}$] and attention is confined to the solar neighborhood, about which we know most. In earlier symposia the consensus was that energy was supplied from *SN* directly to *GK* and then transferred to *MF* by induction and to *CR* by a Fermi-type acceleration. It was difficult, however, to match the numbers. During the Symposium I have heard a firm estimate of 5 units as the total loss from *CR* and a loss of 1 unit from *GK*, whereas the direct supply from *SN* to *GK* would be only 0.1 unit. If these numbers are at all correct, the conclusion must be drawn that a rate of 5 units is supplied by *SN* directly to *CR*, of which 4 units are lost by radiation and

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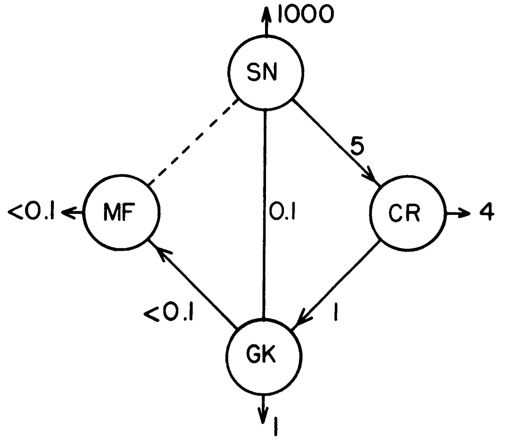

Fig. 1. (See the remark by van de Hulst.) Overall flow of energy in the interstellar medium in the region near the Sun.

escape and 1 unit goes to *GK*. This requires an inverse Fermi mechanism, for which Parker has given us a plausible description. The magnetic fields have estimated losses < 0.1 unit, but whatever the number is, the energy replacement can be supplied to them either from *GK* or directly from *SN*, if the ideas of Piddington and Kardashev are correct. I estimate direct losses of stellar radiation into space at 1000 units. I should like to ask the other speakers if they agree that this is a reasonable synthesis of the numbers they presented.

Pottasch: I am confused about the supply of energy to cosmic rays by supernovae. I thought Woltjer said the other morning (p. 234) that the Crab Nebula supplies less than 10^{48} erg of cosmic rays. Later on people talked about much higher supplies; 10^{51} to 10^{52} erg were mentioned. Does Woltjer agree that supernovae other than the Crab Nebula will supply much higher amounts of energy to cosmic rays?

Woltjer: I agree that there is enough energy present in supernovae to supply all the cosmic rays one needs, possibly in the initial explosion, and certainly in the pulsar that remains. It is possible to convert with very high efficiency the energy of the rotating pulsar into cosmic rays. So there is no energy problem in principle. However, in the Crab Nebula you do not see any evidence that such cosmic rays are being produced. But still I think it is almost certain that supernovae do produce the cosmic rays in one way or another.

Another point with respect to Figure 1 is that probably the energy flow from *SN* (the supernovae plus O and B stars) to *GK* should be multiplied by a factor of five or so.

Shklovskii: In the case of the Crab Nebula, it is quite simple to show that the main part of the cosmic-ray particles is in the form of relativistic electrons and there is some indication that similar situations may exist in other supernova remnants as well. In that situation, it is not possible to connect the present cosmic rays with the relativistic particles produced at the time of the supernova outburst.

Zel'dovich: Should not the nucleus of the Galaxy be included in Figure 1? There is,

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as we know, an outflow of hydrogen from the nucleus. There is also a large infrared flux. What would happen if we tried to make the energy balance for the nucleus of the Galaxy? Perhaps stellar encounters are important there, although they do not occur in the rest of the Galaxy.

Van de Hulst: Initially I had drawn the galactic nucleus also in the circle containing the supernovae. But I later omitted it because some of the other estimates we know only for the solar neighborhood and it is unfair to mix in one diagram estimates referring to quite different regions of space. But I fully agree there is a lot of energy produced in the galactic nucleus. It may be of the same order or bigger.

Mestel: The question of the galactic non-uniform rotation was raised the other day. A long while ago people wanted to tap this reserve of energy by appealing to shear-flow turbulence. This is no longer popular, largely because, I think, the rotation law satisfied the Rayleigh stability criterion. About ten years ago Hoyle and Ireland (1960) suggested magnetic coupling. Their picture involved twisting of the galactic magnetic field by the non-uniform rotation, buckling of the field-lines into the halo, sliding of gas down the field-lines into the 'galactic flare', etc. Again, one is ultimately drawing on the energy in the gravitational field of the stellar disk population. Is all this now ruled out for some reason, qualitative or quantitative? (Hoyle, F. and Ireland, J. G.: 1960, *Monthly Notices Roy. Astron. Soc.* **120**, 173.)

Field: In answer to the question by Mestel, Hoyle and Ireland proposed that the main energy source of the interstellar gas clouds was the winding up of the interstellar magnetic field. In order for this source to work, the galactic magnetic field must be systematic and have a radial component connecting one spiral arm to another. From the discussion earlier, this does not seem to be the case. Another remark is that van de Hulst has also omitted the infall of gas from intergalactic space, the Oort model of accretion. From Oort's data I estimate that the energy flow is 10^{40} erg sec^{-1}, one solar mass per year coming in with 100 km sec^{-1}.

Woltjer: But again you have to apply an efficiency factor. This gas comes in at 100 km sec^{-1} and if you want to couple that to cloud motions at 10 km sec^{-1}, much of the kinetic energy will be radiated away.

Field: I agree.

Mestel: Van de Hulst glided rather quickly over the problem of the interchange between cloud kinetic energy and their energy in the galactic gravitational field. On the first day he was concerned about the Parker mechanism, and how a gas cloud got back into regions of high potential.

Van de Hulst: Again I tried to summarize what I have understood from the discussions. I understand now that stellar sources, including O and B stars, supernovae, and even the galactic nucleus, will give motions to the gas and push them away. Perhaps there will then be one-sided flow back to the galactic plane. This would differ from the many oscillations back and forth which we envisage for stars but would agree with the raining down and settling in the low pockets of Parker's picture. I'm fairly convinced now that this may be the better picture, compared to the old picture of frequent oscillations.

2. Summaries and Suggestions for Future Research

A. E. N. PARKER, *Department of Physics, University of Chicago, Chicago, Illinois, U.S.A.*

I have jotted down a few ideas that I think this Conference has pointed up. Let me begin with a brief review which will serve as a background for suggesting where future theoretical problems lie.

In the theoretical discussions that you have heard, such things as the density, temperature, scale height, and turbulent velocities of the gas have come in and, as you are aware, have been debated at some length. I hope that in the next few years observations will give more detailed pictures and values for these quantities. The magnetic field, which seems to be a few μG, is being observed in more than one way: by Zeeman broadening, by Faraday rotation, and by polarization. I rather suspect that in the next four or five years our knowledge of the galactic magnetic field may be enormously increased. The cosmic rays are a subject in themselves. There are several things about the cosmic rays that are striking, including the fact that they are very steady, apparently rather stagnant, and not streaming rapidly through the Galaxy. One of the biggest gaps in our knowledge of cosmic rays at the present time is the modulation effect of the solar wind. What really is the cosmic-ray density in interstellar space? There is little uncertainty about the density of the relativistic particles which seem to comprise the major energy in cosmic rays; but in regard to the low-energy cosmic rays, which play other roles besides brute pressure, one really has little more than guesses as to what their intensity is outside the solar system. If space programs continue as presently foreseen, there is a good prospect that within six or eight years suitable instruments will be sent to distances of 10 AU. There is every reason to believe, but no guarantee, that at that distance from the Sun, one will see mainly the interstellar cosmic-ray spectrum without much modulation by the solar wind. That is not to say that the solar wind stops at 10 AU but that most of the modulation of the cosmic-ray intensity takes place between 10 AU and the Sun. Therefore I hope, that, at the next Cosmical Gas Dynamics Symposium, there will be much more definite information on the low-energy cosmic rays. This information will have relevance for the origin of cosmic rays, too. At the present time the explanation for the origin of cosmic rays merely involves objects (supernovae and pulsars) which are very energetic and have suitably anomalous abundances. I will have more to say on that a little later.

Finally, there is the dust in the interstellar space, about which we have had some discussion. I want to emphasize that the gas, field, cosmic rays, and dust all go together to form a coherent system. The common binding agent is the magnetic field. The dust is very tightly bound to the magnetic field. The cyclotron radius of a 10^{-5} cm dust grain, with a velocity of 1 km sec^{-1} in a field of 3 μG, is 2×10^{15} cm. That is a microscopic distance on the galactic scale; the dust is therefore very closely tied to the field. In many cases the dust has a sufficiently small density that its inertial effects are very small and it can be ignored, but in some cases this is not true. It has become increasingly apparent over the past few years that one can consider idealized cases of

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magnetic field alone, gas alone, cosmic rays alone, or dust alone, or any combination of these. But, in fact, they are all tied together; and the entire system is a composite fluid of field and gas. All the constituents must be treated together if we are to see the overall picture.

Now the overall picture of the interstellar medium can be studied in several parts. One part is the equilibrium of a disk of gas. The question of equilibrium is not simple because one immediately faces the question of the thermal properties of the gas, that is the temperature. Field, van de Hulst, Pikel'ner, and others have elaborated the rather gory details of this thermal equilibrium of the two phases with high and low temperature and low and high density. Then there is the question of mass inventory. If I understand the numbers properly, it is estimated very roughly that 1 $M_{\odot}$ yr^{-1} is being converted from interstellar gas into stars, give or take a factor of two or three. In the reverse direction, there is ejection from stars of the order of 0.4 $M_{\odot}$ yr^{-1} again give or take a factor of two or three. The net flow, therefore, is from gas to stars; when you begin to worry about abundances, the flow from stars back into the interstellar gas is of course extremely important. The return of gas from stars back into the interstellar gas can be carried out explosively (as in supernovae) or in less violent but transient phenomena, or in steady winds. While we are on the subject of inventories, I would remind you that the cosmic rays are subject to similar considerations, the present evidence being that cosmic rays in the disk of the Galaxy are replenished about every million years. They are being replaced fairly quickly; in fact, many of the dynamical properties of the disk have characteristic times of 10^7 yr, and the cosmic rays are replaced many times during the growth of, say, a cloud structure. When you consider the dynamical properties of the gas (since, in fact, any equilibrium of the disk is a conspicuous fiction), then the problem becomes rather complicated.

The disk of gas, field, and cosmic rays is unstable, as various speakers including myself have elaborated. It is unstable because it has cosmic rays in it, which give a Rayleigh-Taylor instability; it is unstable because there are magnetic fields in it, which enhance the Rayleigh-Taylor instability; and last, but not least, it is unstable because of thermal instability. All three of these effects combine to give a very complex situation, even under the idealized cases that one considers. The instability tends to clump the gas together, and I think people quite naturally refer to this phenomenon as *clouds*. But the theory also goes on to say that there is no such thing as an equilibrium cloud, and it reminds us once again that a cloud is only a transient shape, something you catch in a snapshot. If one could take snapshots at intervals of 10^5 yr, instead of at intervals of five years, one would in fact see these phantoms, these clouds, changing rather rapidly and probably in a very chaotic manner. Then there is the question of the dynamical balance between the cosmic rays and the magnetic field and, perhaps, *via* plasma turbulence, with the interstellar gas. The magnetic field seems to act as a safety valve. Unknown sources, perhaps supernovae and pulsars, are busy pumping up the magnetic field in the disk of the Galaxy; and the magnetic field is continually inflating and leaking at the surface. We have a pressure balance here. That seems to be as far as we have got at the present time in understanding the interstellar gas.

And now, I would like to say a few words about interesting research problems for the future that have occurred to me and probably to many of you, too, during this discussion. I was most intrigued in the observational discussions by the large negative velocities which seem to occur both at high and at very low galactic latitudes. You have heard the hypotheses for the inflow of gas at high latitudes; I did not hear any explanations for the tendency for negative radial velocities in the disk. Also, I am very interested, and very puzzled, by the present definition of clouds in 21-cm observations. A cloud is defined as any gas in the line-of-sight with a common velocity. It is certainly a very practical way of going about defining clouds, and yet I wonder what it means. The word *cloud* actually means something slightly different; it means 'neighbors in space'. And sometimes, when I see two different lines with different Doppler shifts, I wonder whether or not they might not come from regions of gas which are very close together in space, all lumped together in one cloud; whereas, of course, in this Doppler definition of clouds, they would be treated separately. I do not know the answer to that; I do not know any way of getting around that problem.

So far as theoretical problems are concerned, a lot of work still remains to be done on a variety of effects studied and reported here, such as the dynamics of H II regions and ionization fronts. But there is also the question of dust formation, which, in connection with its formation in lanes along the inside of the spiral arms, is certainly a curious question. And of course there is the classical problem, in which work is going ahead steadily, on star formation: namely, given the chaos that we call the interstellar medium, how do stars form? There again theory and observations are necessarily progressing hand in hand. In a slightly different direction, the structure of shock waves in tenuous gases is one that is still with us; I am speaking of the collisionless shock. I think that great progress in understanding the structure of the collisionless shock has come in the past few years. The collisionless shocks are beginning to be observed in considerable detail in the solar system (the bow shock upstream from the Earth, the shocks associated with blast waves from the Sun, and the collision of streams in the solar wind). Tidman, and a number of other people, have gone a long way in classifying the various kinds of shocks and in exploring the detailed plasma physics which are involved in those shocks. I think that we, as astronomers interested in the Galaxy, should not forget that wealth of information, which may be very useful when applied to the shock waves that we see in the interstellar space.

I can see a number of problems that remain yet to be solved in regard to sources of cosmic rays. A lot of work has been done on supernova dynamics, motivated, at least in part, by a search for the origin of cosmic rays. Recently pulsars have come onto the scene; and, if the neutron-star magnetic rotating model is correct, they may be responsible for much of the cosmic rays, too. The subject is very new, and a great deal of work remains yet to be done.

I commented to you earlier that it seems as though all magnetic fields in nature, and the galactic field in particular, have the stochastic property in which the magnetic lines of force random walk relative to their neighbors. Certainly there is a wealth of observational information that is available on this subject. It is interesting that the dynamical properties of such fields have not really been explored in any great detail, and I think there are quite a number of rather conceptually elementary problems that can and should be pursued in studying the propagation of waves, and the overall dynamical properties, of these stochastic fields.

This brings me to the subject of the general motion of cosmic rays in the disk of the Galaxy, followed by their ultimate escape. This and related problems in plasma turbulence are just beginning to be worked on. Lerche, Kulsrud, Pearce, and Wenzel in the United States and a number of people in the Soviet Union, including Kadomtsev and Tsytovich, are thinking about these problems and have made a number of calculations. Tsytovich has been exploring the effects of plasma turbulence on fast particles, which is so important for understanding the dynamics of the disk of the Galaxy. But he, wisely, has been exploring them in the Sun, the motivation being, if I understand it correctly, that there one can really begin to check one's theories. One knows the temperature and the density of the gas in which the turbulence is taking place. One knows the magnetic field and something about the characteristic scales of the phenomenon. Once one has learned the 'plasma turbulence, fast particle' trade in the solar system in this way, one may then be in a much better position to apply it to some far-off, and obscure, region of the Galaxy.

Finally, I should not fail to mention the question which has gone somewhat unanswered in this Symposium: What is the origin of the galactic magnetic field? It is too soon to answer this question, of course. My own inclination, and this is only an inclination, is that in some way the galactic field is not primordial; it seems too active. We must figure out somehow the motions which produce the field, or whatever process is at work. I think a good deal of very profitable theoretical work can be done in that direction.

I would close with some moralizing, which, of course, is always the temptation of the speaker with the captive audience. I think that, in theoretical work, and to some extent in the direct interpretation of observations, we must have a suitable mix of idealized examples – ridiculously oversimplified examples, if you like – where one effect at a time is studied until it is understood and then the separate effects are put together to form a more composite whole. We may well ask how far one will get with a composite. The interstellar weather we are talking about is even more complicated than the terrestrial weather; and it is well-known that twenty-four hour weather predictions are quantitatively unreliable. So how far can we expect to go in quantitative understanding of interstellar weather? Clearly there are limitations. This brings me to my last point. I hear that some people at Berkeley are beginning to think about the enormous problem of combining the thermal effects in the gas with the magnetic and cosmic-ray effects, to produce a gigantic numerical synthesis of all these things into one or more idealized examples to illustrate all the effects in operation at once. It seems to me that, if we do not have another cosmic-aerodynamical conference for five years, the first tentative results may be reported at that next meeting.

Thank you.

B. H. VAN WOERDEN, *Sterrekundig Laboratorium Kapteyn, Groningen, The Netherlands*

In these concluding remarks, I shall summarize what I consider the most important recent progress reported at this Symposium and outline a number of problems on which new observations are needed. My viewpoint is that of an observational astronomer, primarily specialized in 21-cm research.

1. *Highlights of Recent Progress*

a. *Observational Information*

As indicated by van de Hulst in his opening review, the amount of *21-cm line* observations has grown enormously in recent years. However, this mass of data has not yet given us a clear understanding of the structure and dynamics of the interstellar medium. The large-scale structure as described by Weaver (p. 22) was a matter of some controversy at the Basel Symposium (Becker and Contopoulos, 1970); and from our discussion here about hydrogen clouds it appeared that a detailed picture cannot be drawn, mainly because the data remain largely undigested.

For the *ionized hydrogen*, too, there is much new information. There is the optical work on Hα by Courtès, and in the radio range many new observations (especially of recombination lines) have been made. Mezger's work on compact H II regions (p. 336) underlined the relationships of the ionized hydrogen to the problem of star formation.

Great progress has been made in the observation of *interstellar molecules*, discussed by Stecher (p. 316). The subject of interstellar molecules was discussed at length during a conference in Cambridge, England, last July (for a summary see Feldman *et al.*, 1969); this conference also devoted much attention to the infrared radiation of the interstellar medium, a subject which was mentioned only in passing during our present Symposium – as were X-rays, gamma rays, and cosmic rays.

Verschuur (p. 150) summarized the impressive amount of data on interstellar *magnetic fields*. I believe this is the subject in which the most striking observational advances have been made since the Noordwijk Symposium three years ago (van Woerden, 1967a), with molecules and recombination lines competing closely.

b. *Theoretical Developments*

It appears that *thermal instability* and *Rayleigh-Taylor instability* play leading roles in the formation of interstellar clouds and of stars. (See the Reports by Field, p. 51, and by Parker, p. 168.) Further development of these theories, together with Lin's density-wave theory of spiral structure, will allow interesting observational tests. A *magnetic-field* strength of 3 to 5 μG in the general solar neighborhood now seems observationally well-established and does no longer present severe theoretical problems. And, while the observational evidence for a *galactic halo* was severely questioned at the Noordwijk Symposium, the theoretical need for it now also appears to have vanished. The *supernovae* may, *via* cosmic rays, supply the major energy source to the interstellar medium, but the details of the energy balance remain poorly understood (see Woltjer's Report, p. 229; Discussion, p. 236).

2. *Problems for Future Research*

Both the reviews and the discussions during this Symposium have indicated a number of problems which call for new observations or for further analysis of existing material. In outlining some of these* I shall discuss 21-cm line research in more detail than other subjects.

a. *Neutral Hydrogen*

The overall distribution of (neutral and ionized) hydrogen in the Galaxy remains an unsolved problem; there is no good agreement on the pattern of *spiral arms and branches*. Kerr, Weaver, and others have followed different methods of defining and placing the structural features; a thorough comparison of assumptions, details of application, and results of these methods would seem valuable. The need to obtain a reliable picture of our Galaxy's spiral structure has become more pressing now that, on the basis of the Lin theory, it appears possible to predict variations of interstellar cloud properties as a function of location in the spiral pattern.

In addition to the structure as projected on the plane of the Galaxy, the *distribution perpendicular to the plane* is of great interest (see the Report by Parker, p. 168). The recent surveys of Kerr and Weaver should soon yield results superseding those from Westerhout's (1957) cross-sections.

The need for more quantitative information about *interstellar clouds* has become obvious from our special discussion about this subject (see Discussion, p. 98). A first, basic requirement is the need of a quantitative way of *defining* clouds. The recognition of clouds in contour diagrams has not been brought on a quantitative basis yet although this could, and should, be done. Least-squares procedures for fitting components into 21-cm profiles represent a first step towards quantitative treatment of the recognition of clouds. Schwarz has overcome some fundamental difficulties associated with existing component-fitting procedures and he succeeded in developing an automatic and fully-quantitative method for the definition of hydrogen clouds. He has made a first application to a region in Camelopardalis (Schwarz, 1970). This method, although laborious, is quite feasible on big computers and deserves further application; in my opinion it represents a major advance in this field, provided that the results of this fully-mathematical process are critically checked for being physically meaningful.

What quantities should we determine for a cloud? One of our aerodynamicists, Willis, listed: mass, size, density, scale of density variations, density contrast across the cloud 'boundary', velocity of the cloud, internal motions, composition. The work in Camelopardalis (Schwarz, 1970), and similar studies in Orion (van Woerden, 1967b, pp. 14–17) and Scorpius (Schwarz and van Woerden, unpublished), do indeed

* Field and Habing have kindly allowed me to incorporate here some suggestions which they made during the ensuing general discussion.

supply these quantities, although the distance appears as an unknown parameter affecting the derived masses and densities.

Field has suggested (p. 51) that cloud properties should *vary with location of the clouds* in the spiral arms. Also, he expects systematic motions towards the plane, depending on height; downward motions have indeed been observed (references may be found in Blaauw and Tolbert, 1966), but these have not yet been differentiated with respect to distance.

Now let us discuss the structures smaller than 'clouds'. Heiles (1966, 1967) has, from a high-resolution study, announced the existence of '*cloudlets*', a new and distinct category of small interstellar clouds. Rots *et al.* (1970) after a critical analysis conclude that many of Heiles' 'cloudlets' must be interpreted as small concentrations or irregularities in larger structures; the number of small, isolated clouds may be only a minor fraction of the number of 'cloudlets'. Obviously, an independent check of these conclusions should be of great value for our knowledge of the spectrum of cloud masses.

The theorists expect *shock fronts* in neutral hydrogen clouds (Field, p. 51) and thin (10^{18} cm) *shells* of neutral hydrogen around H II regions (Discussion, p. 93). The angular resolution of existing 21-cm line equipment is inadequate for the observation of such features in emission. However, aperture-synthesis systems will change the situation. We hope to equip the Synthesis Telescope at Westerbork (The Netherlands) with a line receiver by 1972; its resolution at 21 cm is 22″, or 0.03 pc $= 10^{17}$ cm at a distance of 300 pc. This is only two orders of magnitude above the mean free path (10^{15} cm) of hydrogen atoms at a density of 10 atom cm^{-3}.

The limit for *velocity resolution* is close at hand. For hydrogen atoms a passband of 2 kHz = 0.4 km sec^{-1} width will resolve the velocity distribution at $T_g = 20$ K. Heiles' survey was done with a 5 kHz bandwidth, and observations with narrower passbands are quite feasible with present low-noise, multi-channel receivers.

Neutral hydrogen shells around H II regions may be observed in *absorption at 21 cm* against the continuum background emitted by the H II region. Clark's (1965) investigation of the absorption spectrum of Orion A may contain some information of this nature, but further similar work is badly needed. Such work will be particularly fruitful if combined with studies of the interstellar sodium and calcium lines.

Combination of emission and absorption measurements is required for a determination of the spin *temperature* of neutral hydrogen. This subject has too long been neglected. Comparison of the internal motions within a cloud with its temperature will show whether these motions are subsonic or supersonic – an item of great importance to the physics and dynamics of the clouds.

b. *Interstellar Sodium and Calcium*

In Adams' (1949) interstellar calcium absorption spectra, the best velocity resolution was about 10 km sec^{-1}. Since then, hardly any data of higher resolution have become available, but just two months ago, Hobbs (1969a) published highly accurate sodium absorption profiles for 77 stars, with a velocity resolution of 0.5 km sec^{-1}. This clearly represents a tremendous advance. One of the important results of this work (Hobbs 1969b – which was mentioned only in passing during the Symposium – is that the velocity dispersions σ of well-resolved individual sodium-line components range between 0.6 and 1.5 km sec^{-1}, while the passband distribution corresponds to $\sigma = 0.2$ km sec^{-1}. It appears quite feasible (Hobbs, 1969a) to extend such high-resolution measurements to large numbers of stars, including stars of spectral types A and F. This opens great possibilities for detailed studies of the interstellar medium at high velocity *and* angular resolutions.

Of particular interest would be the determination of sodium and calcium absorption in front of early-type stars in H II regions, for comparison with possible neutral-hydrogen shells or with motions in the H II regions. [See, e.g., Herbig (1968, *Z. Astrophys.* **68**, 243) on ζ Oph (Ed.).]

c. *Interstellar Molecules*

Hobbs' Fabry-Pérot interferometer would also be quite powerful for the detection of interstellar molecules; the optical information on these (see Stecher's Report, p. 316) is quite meagre so far. Both the velocity resolution and the high accuracy of Hobbs' spectrometer are important in this respect. In addition to measuring molecular lines in the spectra of more stars, observers might also try for molecules so far undetected, for instance OH (see Goss and Spinrad, 1966).

We may, of course, expect much further radio work on the newly discovered polyatomic molecules and on OH. Also, we look forward to success of the long search for CH. Of far higher priority I would rate the detection of H_2 molecules. Although the anomalous gas/dust ratios in dark clouds are still open for discussion (see p. 331) there is, in my opinion, little doubt that considerable quantities of H_2 must be present.

d. *Supernovae and Supernova Remnants*

In the supernova remnants, further attempts to measure the radio recombination lines should be of interest. The last stage of remnants discussed by Woltjer (p. 229) is that in which they become neutral and merge with the interstellar medium. The 21-cm line observers might try to look for these old remnants, presumably invisible both optically and in the radio continuum. But where? If supernovae of type II (that is, Population I) do occur in OB associations, these old remnants may occur in the surroundings of older associations or – after their dissipation – of galactic clusters. Katgert (1969) and van Woerden and Hack (1970) may have discovered a few such old remnants. Detection of supernova remnants in external galaxies would be valuable. Synthesis telescopes such as those at Cambridge and at Westerbork will here again open a fruitful field of research.

e. *X-Rays*

Field has noted (p. 51) that the hot halo suggested by Spitzer (1956) should be observable through its X-ray radiation. However, I presume it will be difficult to distinguish such a halo from the extragalactic background. Nevertheless, this is a subject that should qualify for future observational attempts. An easier observation may be the X-ray radiation from (stellar and/or interstellar) supernova remnants.

f. *Inter-Arm and Inter-Cloud Conditions*

During this meeting there has been considerable discussion of the properties of regions inside and outside interstellar clouds, or inside and outside spiral arms. I believe we know very little about conditions in inter-arm and inter-cloud regions; in fact, I think they were often confused here, and the difference may not even be physically significant.

Measurements of the non-thermal radio continuum should provide relativistic-electron densities and magnetic-field strengths. The thermal radio continuum, and possibly spectral lines (particularly Hα with Courtès' interferometry), may furnish thermal-electron densities. Further, the 21-cm line can be used to determine atomic-hydrogen densities and temperatures. The work is obviously difficult in our Galaxy, because of the unfavorable location of the Sun. Observation of external galaxies may give more secure answers, although the requirements on sensitivity are quite severe – particularly for synthesis systems.

g. *Extragalactic observations*

I conclude by listing a few problems mentioned above, for which studies of external galaxies appear particularly helpful:

structure and kinematics of spiral galaxies;
scale height of the gas layer in a galaxy;
relative distribution and location of H I complexes;
H II regions and other features;
occurrence and properties of supernovae and their remnants;
haloes of galaxies;
inter-arm and inter-cloud conditions.

These then are the problems I suggest for future research. Successful pursuit of a majority of these would certainly justify another, later symposium in the present series.

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Let me begin with some general remarks. First, to many of us the way in which this Symposium worked was completely new, i.e., Introductory Reports to begin and then a long and unprepared discussion to follow. I think that in general this scheme was very satisfying. Many of the U.S.S.R. participants found the Introductory Reports very informative. Second, this Symposium and the previous ones serve the purpose of arranging discussions between astrophysicists and aerodynamicists. It appears to me that such discussions are very valuable. The astrophysicists need help for the interpretation of the observations and for clarification of the physical picture. The aerodynamicists can help in solving the gas-dynamical equations and, in this process, learn something new about their own thoroughly studied equations. I wish that in future symposia in this series the organizing committee will stimulate this discussion more strongly, for example by having an Introductory Report on recent developments in gas dynamics. Third, a new feature of this Symposium was the large role given to plasma physicists (although there were still too few plasma physicists present, I think). This seems a very important development, from which both astrophysicists and plasma physicists can profit.

Now I want to come to direct scientific matters. I will touch only on those problems which are close to my present scientific interests. First of all, we need methods to detect and observe plasma turbulence. Direct measurements can be made only in interplanetary space. Such measurements have been made (see the Report by Lüst, p. 249) but their interpretation is still difficult. Indirectly one can measure plasma turbulence only through the electromagnetic radiation emitted or influenced by the turbulent plasma. One existing mechanism converts plasma wave energy into radiation. In another mechanism plasma waves serve as a catalyst for the conversion of nearly all the energy present in fast and relativistic particles into electromagnetic radiation. Plasma turbulence accelerates charged particles which then generate radiation, including synchrotron radiation. Progress in understanding these processes is at present being made both in theoretical calculations and in the interpretation of the observations. Many radio bursts on the Sun are definitely connected with plasma turbulence phenomena. Plasma physics is able to provide basic mechanisms for cosmic masers. I have mentioned (p. 133) the possibility of the scattering of electromagnetic waves off plasma waves.

The second set of problems I would like to consider concerns changes in the usual hydrodynamic and magnetohydrodynamic equations. These changes are brought about by plasma physics. One of the changes, for example, is the thickness of shock fronts; we have discussed rather intensively the collisionless shock front. Other questions concern the conductivity in turbulent plasmas, which is considerably lower than in non-turbulent plasma. This may have consequences for the dissipation of magnetic fields, for the heating of the plasmas and for an increasing tendency toward instabilities.

Third, in studying plasma turbulence, its excitation in astrophysical objects deserves much attention because the interpretation of the observations will not always be straightforward and unique, and a clear picture is required of all the possible instabilities in plasmas. We require not only theoretical work but also laboratory experiments with cosmical models. Good examples are the latest experiments simulating the solar wind flow around the Earth's magnetic field, which have been discussed by Podgornii (p. 144) and by Karpman (p. 147). I want to stress the importance of the interaction of plasma turbulence and cosmic-ray particles. The latter give high-frequency electromagnetic radiation, are accelerated by the turbulence, and form the main source of excitation of plasma turbulence.

These are some examples of areas where plasma physics and astrophysics interact. It is certain that all the problems of what I would like to call 'plasma astrophysics' may be resolved ultimately by the combined effort of astrophysicists and plasma physicists. A discussion between these two groups is therefore very important. Perhaps we should ask not only the IAU and the IUTAM, but also the International Agency of Atomic Energy to sponsor our next Symposium.

3. Summaries and Suggestions for Future Research

[The first part of the following discussion took place on Tuesday, September 16. At the request of Thomas a short discourse was given by Busemann on the interrelation of interstellar gas dynamics and aerodynamics.]

Busemann: I was asked to speak only a few minutes ago; thus I am completely unprepared, a fact on which I cannot cast doubt by 'accidentally' having a slide with me. The questions which I would like to discuss are: Is there anything the invited aerodynamicists can contribute to help the astrophysicists, or is there something in the more sophisticated problems presented, that can open the eyes of the aerodynamicists, so that they may help the astrophysicists?

I still remember the days when incompressible flow was just established as a young but very exact science of almost mathematical precision while its sister science, the compressible flow or gasdynamics, was rather in its infancy especially at supersonic and transonic speeds. Since gasdynamics was always being compared with its perfect sister, I felt that it was very easy to make disastrous mistakes and I therefore decided to find graphical representations or physical models which could help in the interpretation of the difficult gasdynamical equations. Many such pictures remain still in my mind as valid representations of the facts. When I hear the remarks of some speakers at this Symposium, the pictures instantly come back to me either casting doubt on or reconfirming their statements. However, while working in the field of magneto-hydrodynamics I learned that there exist many domains of very different behavior, not just distinguished by the Reynolds number and the Mach number as in aerodynamics, but also by magnetic field strengths, conductivities, free path lengths compared to Debije lengths, degrees of ionization, etc. Considering those differences, a reasonable criticism needs some thought and time.

Again as an early gasdynamicist, I start with those helpful models to which I already referred. One of these is the shallow water theory. It is a two-dimensional analogy to compressible sub- and supersonic flow (though it corresponds to a ratio of specific heats in perfect gases above the actual values for gases). If I take a pitcher with water and pour the water jet upon a flat bottom of a bowl, the water will rush towards the walls of the bowl and it is stopped there, forming a water jump which is representative of the shock wave in a gas. The rushing flow may be regarded as the supersonic solar wind which is terminated by a shock when joining the interstellar gas. The height of the shallow water indicates pressure, the surface waves simulate Mach waves, and the somewhat wiggling water jump resembles the shock wave of finite amplitude. If I use two pitchers over the same flat bottom, the symmetry line between their points of impact is surrounded by water jumps simulating two solar winds hitting each other head-on. If I let one pitcher pour faster than the other one, the symmetry is spoiled, but the principle effects are still similar. Thus I can instantly demonstrate a large variety of (two-dimensional!) solutions. The water jump near the wall of the bowl produced by using a single pitcher, can be regarded – for a straight wall – as one half of the symmetric arrangement produced by two pitchers. After some time the water accumulation near the wall moves the water jump away from the wall and it simulates, sooner or later, the shock of the solar wind joining the infinite interstellar gas in a coordinate system which contains the interstellar gas at rest.

Such simple and cheap models of our compressible flow problems give a very good introduction for becoming acquainted with combinations of hyperbolic and elliptic non-linear differential equations. However, like this shallow water analogy, the models are only available in two-dimensions while we need solutions in three dimensions. There may even be misleading models. Consider a thin sheet of an electrically conducting material and let the electric flux lines correspond to the potential lines of incompressible flow and *vice versa*. Compressibility of the flow is introduced as soon as the conductivity of the material increases with temperature, while the temperature is controlled predominantly by the radiation of heat to the environment. In such an electrical analogy the subsonic or elliptical flow behavior is modelled correctly. But as soon as the electrical potentials are increased to include supersonic flow regimes, the expected hyperbolical flow does not know about upstream and downstream directions. It acts more like lightning, which persists on its original random paths by heating them up more and more.

I come now to my current interest: finding a cure for the 'sonic boom'. To indicate at least the main lines of the sonic boom research, I want to give you some of the results. The problem would completely disappear were we to return to the lighter than air vehicles, the airships which do not transfer any weight to the surrounding air. This, however, seems rather remote as a realistic approach. Removing the airplane weight by centrifugal forces on the great circles around the earth comes automatically at a Mach number of 25, but again this does not appear to be a realistic solution. (On the other hand space vehicles use this speed as earth satellites already.) A more realistic, though not easy solution, is to slow down the pressure rise in the shock wave to a time of about 0.01 sec. Then the noise may be very different for our ears and hopefully not alarming at all. Under this, not quite generally accepted, assumption the appropriate fuselage and wing configurations can be found and the airplane shapes, differing vastly with altitude, can be studied. The concept of finite rise times does not prevent an asymmetrical reception by our ears. The asymmetry is caused by the natural deformation of sound waves along large propagation paths and it corresponds to the deformation of water waves at the beaches. To prevent steepening of the waves there seem to be two approaches possible: by making the airplane nose extremely slender (in which case the noses are probably not stiff enough to carry the plane at supersonic speeds) or to change the surrounding atmosphere in such a way that shock waves do not result. Only a gas, in which the velocity of sound decreases (by van der Waals forces) sufficiently when the isentropical pressure increases never creates shock waves. Such a gas is called 'Chaplygin' or 'Karman-Tsien' gas and has a straight isentropic line in pressure versus volume plots. The mathematical simplifications are obvious, but the existence of the gas is not yet proved and the change of our atmosphere toward such behavior is even less likely. Since it might be difficult to modify the air, changing the nose shape seems a more accessible 'road'. The nose stiffness cannot be increased by deviating from the axially symmetrical shape unless we make the sting hollow. Visualizing the extreme difficulties, it is not surprising that substitutions are proposed for the concrete nose: 'ghost' noses created by electrical charges, laser beams, etc., thrown ahead of the actual nose. But to produce such 'ghost' noses the energy required appears to be prohibitive either in weight or fuel consumption.

When the human brain seems to see no way out, perhaps nature itself can give us a hint. Is there any place in the interstellar gas where the expected shock does not occur? Why don't we hear a boom-boom when a celestial body passes along? If the astrophysicists were fully acquainted with out problems, they might perhaps find examples of electrical effects, radiation effects, special gas properties, etc., which would give us aerodynamicists new hopes and perhaps answers.

In summary I may say that the astrophysicists and the aerodynamicists use the same laws of nature. A striking difference is, however, that the aerodynamicist can control his experiments, but has to find solutions for a given purpose with difficult and sometimes impossible constraints. The astrophysicist observes events far away that have their own natural time scale. Sometimes he may wish he could also put his hands on the phenomena, but all he can hope for are new methods or places of observations. His purpose is to explain the observed phenomena as exactly and precisely as freedom from controversies allows. Apparent contradictions force him to make more and more sophisticated assumptions without always being able to check the results in the laboratory. There is no doubt that looking over each other's shoulders, as in this Symposium, can be very useful for both parties.

Kaplan: Busemann spoke about gas without shockwaves. I would like to mention that in plasmas with magnetic fields, supersonic motions are possible without formation of a shock wave. Such motions are called 'solitons' and their existence has been verified experimentally.

Goldsworthy: I would like to make a comment which I hope will be helpful in trying to bring aerodynamics and astrophysics together. First of all, in reading the proceedings of past symposia and listening to the present one, one cannot help but note that the particular emphasis is placed upon global properties, such as total energy, etc. These are interesting quantities from the astrophysical point of view; but for the aerodynamicist, as he looks at the associated gas dynamics, these quantities act only as parameters in the problem. They do not, in general, say anything precise about the dynamical model. For instance, if we take the problem of an explosion, we may know its energy; but this tells us very little about the nature of the blast damage that results. I therefore appeal that, first, when such global properties are given, the variation about the average be included; and second, where possible, greater emphasis should be placed on velocity and density distribution in the separate objects. It is perhaps the latter that the aerodynamicist has most difficulty in obtaining from the astrophysical literature. [The major part of Goldsworthy's remarks at this point have been transferred to the Discussion following the Reports by Weaver and Field, p. 94 (Ed.).]

Verschuur: Dr. Goldsworthy, is it more useful to study specific clouds in great detail, or to discuss a standard or average cloud? Would you prefer as detailed as possible information on a specific H I region; or should we look at hundreds of these and give you an average?

Goldsworthy: An 'average' really does not mean anything to us. We would prefer more detailed observations on one particular structure, so that we might then be able to analyze associated motions.

Lüst: I would like to make one remark to the aerodynamicists who say that it is difficult to devise experiments in this field. One area where we can do experiments is in the magnetosphere. There we have a magnetic field; we can inject an artificial plasma and observe it from the ground. At higher altitudes the mean free path is quite long, while the gyroradius of the ions and electrons is rather small. We at Munich have carried out several such experiments in recent years. In particular we carried out an experiment at about 70 000 km altitude in the magnetosphere. There we injected about 100 g of barium ions. The density of the surrounding plasma was of the order of about 0.1 proton cm^{-3}, and the magnetic field strength of the order of 5×10^{-4} G. The kinetic energy density of the expanding ions was larger than the magnetic-energy density for about 20 sec, and the barium cloud formed a magnetic cavity. After 20 sec, the initial expansion stopped, and the cloud as a whole moved through the ambient plasma like a comet tail, until the cloud was accelerated to the velocity of the ambient plasma. I feel that experiments of this kind could be of interest to aerodynamicists and plasma physicists, if they want to understand astrophysical phenomena. [A general description of such experiments may be found in Härendel and Lüst, *Scientific American*, November 1968, p. 81 (Ed.).]

[The remaining part of this Final Discussion took place on the last day of the Conference, Thursday, September 18 (Ed.).]

Weymann: As one who dabbled in thermal instability some time ago, the apparent occurrence of clouds at these intermediate temperatures seems most disturbing to me. If I understand correctly, it is not just that the theory predicts that the clouds will be at such-and-such a temperature which is fairly well determined by the excitation potential of the cooling lines; but it also predicts that the clouds cannot be at the other temperature. I think that it would be worth a lot of work to find out observationally whether or not clouds at these intermediate temperatures really exist. If they do, it means at best that one has to have a fairly complicated dynamical theory, and at worst that there is something fundamentally wrong with this picture of heating by cosmic rays or X-rays.

Field: Pikel'ner and I have a running debate on the question of whether or not the intermediate temperatures should occur. The direct formation of a typical cloud requires the gathering in of material by means of instabilities of very long wavelengths. The time scale τ for this is about λ/c, where c is the speed of sound. If $\lambda = 300$ pc and $c = 10$ km sec^{-1}, $\tau = 3 \times 10^7$yr. Thus some gas would be in the intermediate phase for observable lengths of time. However, while these instabilities of the order of 300 pc are developing, shorter wavelength instabilities should occur as well, as they have a growth time of only 10^6 yr; they will tend to wipe out the intermediate phase as soon as it occurs. It is therefore a dynamical problem which should be worked out into the non-linear regime in order to give final results.

Pikel'ner: Field has pointed out correctly that before the gas forms a large dense cloud it should form little droplets of denser phase in the rarefied background. Therefore it is very probable that a cloud with an intermediate *average* density is in fact a system with gas in both of the stable phases. The time of formation of such a system should be about the same as the time of temperature relaxation. Perhaps we can check this theory by observations of different parts of clouds.

Habing: I want to suggest observations that may yield the value of ζ, the ionization rate per hydrogen atom. Actually the method I would like to propose measures ζ in dense, cool, neutral clouds. The method works as follows: Consider a non-thermal radio source. First, determine at a low frequency (say, 20 MHz) the optical depth τ_{ff} for free-free absorption; τ_{ff} is proportional to $\int n_e^2 T_e^{-3/2}\, dl$. Note the heavy weighting of cool electrons. This means that, unless we happen to see an H II region projected on the radio source (a situation that occurs for both Cyg A and Cas A), most of the absorbing electrons are associated with H I regions. Second, determine the 21-cm absorption profile and integrate the optical depth τ over the velocity range. The integral τ_{21} is proportional to $\int n_H T_e^{-1}\, dl$. If ζ is constant along the line of sight $\zeta n_H = \alpha n_e^2$, where α is the recombination coefficient. For $T_e < 1000\,K$ we have $\alpha = \alpha_0 (T/T_0)^{-0.65}$, so that $\zeta \propto n_e^2 n_H^{-1} \langle T_e^{-0.30} \rangle$. Therefore $\zeta \propto (\tau_{ff}/\tau_{21}) \langle T_e^{-0.30} \rangle^{-1}$. If we can guess T_e within a factor of 5, we can get very accurate values of ζ. This method should give much smaller upper limits than have been known so far.

Pikel'ner: I should like to stress that for many theoretical calculations it is of fundamental importance to know the electron density n_e in the interstellar space; the figure given by Mills ($n_e \approx 0.06\ cm^{-3}$, see p. 92) is too high from a theoretical point of view (perhaps also from the observational point of view). It is necessary to be very careful, because there are perhaps some H II regions near the Sun which contribute to this number. Also, it is necessary to be careful in comparing high-frequency radio emission and low-frequency radio absorption, because the low-frequency absorption is dependent very strongly upon temperature and will occur mainly in the cold clouds. High frequency radio emission is not so strongly dependent upon temperature, and may originate mainly in the rarefied gas.

Mestel: As I mentioned before (see p. 364) Hoyle and Ireland (1960) suggested that the centrifugal energy in the galactic shear could be tapped by means of the galactic magnetic field. If the field has a large-scale poloidal component, then energy must be fed steadily into the toroidal component until the field becomes too strong for stability, and it buckles into the halo. Some of the energy is thus converted into gravitational potential energy, which in time becomes kinetic energy as the gas streams down into the disk. I am not clear whether or not this model can still be defended as an important source of galactic turbulence. What I want to emphasize is that, if we retain the picture of a large-scale galactic field, then we cannot avoid considering this problem of the winding of the field. The Lindblad-Lin gravitational wave model automatically resolves the classical problem of the winding up of the spiral arms by having them rotate at a fixed angular velocity; but the gas plus magnetic field and the stars still move through the wave pattern with the local angular velocity. As remarked before (p. 364), the shear will produce a dominant toroidal component for the field, which will therefore appear to be nearly parallel to a tightly wound spiral; but as long as there remains a poloidal component, the shear will steadily feed energy into the toroidal component, unless something like the Hoyle-Ireland instability relieves the toroidal field of its excess energy.

Field: In regard to the question raised earlier of the nature of the turbulence and its cause, it seems to me that the outstanding result of 21-cm radio astronomy is that the observed turbulence is highly supersonic. Therefore the energy input must be compressive in nature; $\nabla \cdot \mathbf{V} \neq 0$. The relevant energy sources are those which will result in condensation, collapse, and motion along the field lines at a supersonic rate. The three very definite possibilities discussed at this conference are: (i) Rayleigh-Taylor instability; (ii) thermal instability; (iii) expansion of certain objects, notably H II regions and supernova remnants. It seems to me that we have given very little attention here to the essentially different type of motions associated

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with shear turbulence ($\nabla \times \mathbf{V} \neq 0$). In particular, we have not considered the excitation of Alfvén waves in the interstellar medium. On various occasions it was pointed out that large amplitude Alfvén waves can be responsible for certain of the observations. I hope that high resolution work, which can identify scales of 1 pc or less, may yield information on the presence or absence of such motions. The rotation of the Galaxy should be considered as a source of such motions, particularly in connection with the question of the winding of the magnetic field. Another source is the instability caused by streaming of cosmic rays, discussed by Tsytovich (see his Report at p. 108). Although the emphasis here has been on the compressive motions, we should not overlook the fact that the rotational motions are still important.

Lynds: It seems that there are problems in identifying neutral hydrogen clouds. There are no problems whatsoever in identifying dust clouds – at least the densest ones, so that for such clouds direct observational information is available regarding the existence of sharp edges, characteristic patterns, filamentary structures, and so on. Furthermore, in larger dark clouds, there are subcondensations. Semiquantitative estimates of extinction can be made through the clouds, but no information on velocity is available. You therefore have a problem with the dust clouds which is more or less opposite to what you have with the 21-cm clouds. Although there is apparently no correlation of dust clouds with the 21-cm data, studying the characteristics of the dust clouds will perhaps suggest an interpretation for the 21-cm analysis.

Van Woerden: I think that Beverly Lynds' suggestion that boundaries for dust clouds are more easily defined than for hydrogen clouds is due to observational selection; you see the darkest clouds with the sharp edges most easily, and the others you do not see so well. But there are also hydrogen clouds which stick out very easily, and others which are difficult.

Spiegel: We have these 21-cm contour diagrams which were shown at various times. These things are rather similar to spectroheliograms. Leighton made rather sweeping discoveries by treating the spectroheliograms in a statistical way; for example, he would take a negative of one and print it against a positive of another. The two plates he took were only a little bit different in velocity (or wavelength) so that certain small-scale fluctuations of density were suppressed and large-scale structures of kinematic nature emerged (Leighton *et al.*, 1962). In a similar way, he did that with the Zeeman splitting, and saw direct photographs of large-scale magnetic fields. I've been wondering how much of that kind of thing would be useful in 21-cm line work. Of course, the possible range of distance makes the problem rather complicated. But then in the Sun, too, depth variation is very subtle and crucial. How much of this could be done; how easy would it be; and are there such things already? (Leighton, R. B., Noyes, R. W., and Simon, G. W.: 1962, *Astrophys. J.* **135**, 474.)

Field: I think I disagree with everybody on what an interstellar cloud is. In my definition, an interstellar cloud is a region of high density in the three-dimensional space defined by the galactic coordinates, l and b, and velocity. It is a closed contour in this space. These objects exist. Along the lines of Spiegel's remarks, the possibility exists for computer programming which will do the following. We have a three-dimensional array of data. Ordinarily this is projected on two of the canonical coordinates (say l and v at fixed b) as a contour diagram. But with programs now available one can rotate the coordinate system and view the results on a cathode-ray tube in real time. This added perspective would be helpful to the theorist trying to understand the cause of the flow.

Another point that I want to bring up is the possible existence of what one might call the dust cycle. This cycle is connected with gas flow through a spiral arm and has to do with the freezing out of metal atoms (say, carbon and silicon) on the dust grains. This freezing out affects the chemical composition and the temperature of the (atomic and ionic) gas. If this freezing out occurs during the passage of the dust through the spiral arm (with evaporation of metals after leaving the spiral arm), one would predict that the temperature of H I clouds increases the more a cloud progresses through the spiral arm. In principle this behavior can be checked with 21-cm absorption measurements.

Verschuur: I would like to say something about the possible progress over the next few years in magnetic-field determinations. The efforts, I think, will be spent mainly in looking at the data that is now available, because there is a lot of data available and I do not see that great progress is likely in the collection of data in the near future. To illustrate this in particular, let me say something about the future of the Zeeman effect measurements. It took a long time to measure the first magnetic field, due to the difficulty of the experiments. And now, I already have met with instrumental limitations in measuring the weaker fields. I think that the greatest progress will be made when larger radio telescopes become available, because we need to observe very narrow emission lines at higher latitudes and these narrow emission lines have velocity structure, which broadens the lines when examined with an insufficiently small beamwidth. I have already looked at all the absorption sources available in the Northern Hemisphere. Therefore only the biggest radio telescopes can be useful; we need a fully-steerable dish, greater than the 300 foot.

Van Woerden: We have heard many suggestions for radio observations with large angular resolution. In The Netherlands we are getting a synthesis array which will give such a resolution. I would like to say that we in The Netherlands hope to have frequent visits from foreign colleagues to work with our equipment. Our policy for the coming years is to have two or three positions available on a regular basis for foreign workers.

[At this point the Chairman asked several participants to give their overall impressions (Ed.).]

Ozernoi: I have had very fruitful conversations with Field, Colgate, Mestel, Spiegel, Weymann, Woltjer, and others; and it seems to me that informal conversations are often more useful than the formal sessions. The discussions of the relation between cosmology and the modern state of the Galaxy have been very useful for me. The distinguishing feature of this Symposium was the repeated necessity to speak about phenomena outside the limits of our Galaxy or to look at epochs earlier than the present one. I should like to recall some examples or suggest new ones.

First, concerning the dynamics of gas, the influence of the galactic center on the interstellar gas by ejection of relativistic particles or by outflow of plasma cannot be passed over. These processes, as well as the explosions in the nucleus, are miniature copies of phenomena which are observed in Seyfert galaxies, N-galaxies, and quasars. Thus, the origin of some fraction of stars of the Galaxy is possibly due to explosions in the nucleus, which, as in quasars and radiogalaxies, eject large amounts of gas. In what way could this gas, after cooling and fragmentation, be transformed into stars? An analysis shows that besides star formation by gravitational condensation we have formation of massive rotating magnetoplasma configurations, which eject gas by explosions, transforming some of it into stars. There exist other similarities which connect some unusual phenomena in our Galaxy with the phenomena so distinctly observed in quasars. Second, the fact that QSS's and other strong radiogalaxies probably belong to spherical rather than to flat systems (to which Seyfert galaxies belong) reminds one of the necessity to explore more carefully the properties of stars and gas in the halo of the Galaxy, as well as in elliptical galaxies. This is an important channel of information about the origin and evolution of spherical systems. The comparison of the helium abundance in old halo stars with that in the galactic nucleus and near supernova remnants may help in answering the question of whether the anisotropic character of cosmological expansion or some local nonthermal processes determine the helium abundance. The third and last point refers to the traces of the gas-dynamic past exhibited by the Galaxy in its present state. The whirl model of galaxy formation, which has been discussed earlier (p. 216) shows that dynamical properties of the Galaxy as a whole are the consequences of the turbulent past of the Metagalaxy. Apparently the intriguing problem of the origin of the galactic magnetic field would be resolved in a whirl-turbulent cosmogony, starting from the old idea of Biermann and Schlüter of generating the magnetic field by a velocity field. All the questions mentioned are too difficult to describe briefly. Possibly, a future Symposium could be devoted especially to the gas-dynamic phenomena from the aspect of the origin and evolution of galaxies and quasars.

Toomre: In response to Thomas' inquiry about my personal reactions to this conference, I wish to thank the observers for giving me a good summary of the recent experimental evidence. One of the dynamical aspects that I will be keen on examining involves the two-phase systems emphasized by Field. The dynamical consequences of considering even Burger's one-dimensional model turbulence, while using an equation of state with two phases, may be quite illuminating for this subject. Kadomtsev has further pointed out to many of us that plasma turbulence may provide us with some useful energetic transport mechanisms between quite different length scales. This is an area which we have probably slighted and which deserves some careful study.

Thomas: If you do this, may I just add the suggestion that beyond this simple, two-phase picture, you should look in the Proceedings of the last (Vth) Symposium in this series (Thomas, 1967) – at the introductory-summary-survey paper by Goulard, who discussed the problems of radiative control of aerodynamic flows. There one has this question of the energy equation put in a broader form. If you put it in aerodynamic jargon, the question is one of frozen-in degrees of freedom, where the motions don't have much to do with the equation of state, but where the internal energy is fixed by the radiation field. (Thomas, R. N. (ed.): 1967, *IAU Symposium No. 28, Aerodynamic Phenomena in Stellar Atmospheres*, 5th Symposium on Cosmical Gas Dynamics, Academic Press, New York.)

Now I would ask you to join with me in thanking our hosts, who have done far more than any host for a previous Symposium in this series: they have adopted a language that is not their own. They have made every effort to speak English and to act as a buffer between us and the affairs of the outside world, so that we could concentrate here in this beautiful seaside resort only on science, swimming, and tennis. I cannot say, in English or any other laguage, how grateful we are. I ask you to stand and to thank all of our hosts at this Symposium. (Applause.)

Shklovskii: On behalf of my Soviet colleagues may I express our deep gratitude to Dr. Thomas for his invaluable contribution in the organization of this Symposium. It hardly would be successful without his energy and experience. We are very obliged also to Dr. Gebbie, Mrs. Low, and Miss Thomas for their extremely hard work in transcribing the discussions. We are very obliged to all foreign participants and especially to the invited speakers whose contributions made the Symposium fruitful and enjoyable.