# 6. DISCUSSION FOLLOWING THE INFORMAL MEETING ON INTERSTELLAR HI CLOUDS

*(Friday, September 12, 1969)*

Chairman: S. B. PIKEL'NER

**Editor's remarks:** On Friday afternoon, September 12, an informal meeting was held to discuss the distribution of neutral interstellar matter. Chairman of the meeting was Thomas. Summaries were presented at the formal discussion meeting, later the same afternoon. No change of importance has been made in the original discussion. The text has been inserted where it seemed most functional, namely closely following the Discussion on the Reports by Weaver and Field.

**Pikel'ner:** Dr. van Woerden, will you now present to the Symposium your summary of the special discussion meeting held this afternoon on the HI clouds?

**Van Woerden:** To my surprise, in this afternoon's discussion we have not had any trouble about what a cloud really is. It appeared immediately that all the 21-cm observers define a cloud, or a feature, by its velocity. I have myself added that one should also look at the velocity dispersion of the feature, since that might be another important characteristic. In one line profile there may be two features, with the same velocity but different velocity dispersions, on top of each other, which are due to two separate clouds in two separate parts of space.

We next drew a picture of the distribution of neutral hydrogen in space, in the solar neighborhood, which started out with the map of the Galaxy presented by Weaver earlier in this Symposium (see p. 22). Between the Sagittarius Arm and the Perseus Arm, there is an 'Orion Branch', with an estimated width or thickness of some 500 pc. Weaver thinks within the Orion Branch there is a cucumber-shaped structure around us, with a length of about 300 pc, and an average neutral-hydrogen density of 0.3 atom $\mathrm{cm}^{-3}$. We are not fully agreed on this point; although I am convinced that there is a large structure, as evidenced by the continuity of hydrogen profiles over large areas of sky, I am sure that there are directions where we do not see it, so that the Sun might be at the edge of this cucumber. Then, going down in size, we believe that there is a whole spectrum of sizes reaching down from 100 pc to perhaps 3 to 5 pc and probably even lower. (The uncertainty here is mainly determined by limitations of angular resolution, higher resolution being obtained only in 21-cm line absorption spectra of radio sources.) Within this range of sizes we considered two possibilities. (1) There may be a progression of sizes (Figure 1a): big clouds, smaller clouds, still smaller clouds, which are all separate in space; or (2) there also may be what we call a hierarchy (Figure 1b): a big cloud, a smaller cloud within it, and again a smaller cloud within that one. It seems that both cases occur; I believe we agree on that point.

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So much for the sizes. The next thing is the spectrum of densities within clouds. There is no good agreement on that. The density within the cucumber shape's large-scale structure was 0.2 atom $cm^{-3}$; next there are higher densities within the clouds, varying from 1 or 2 to 10 or possibly 100 atom $cm^{-3}$. The disagreement involves the density contrast between big cloud, small cloud and no cloud. If I understand Weaver

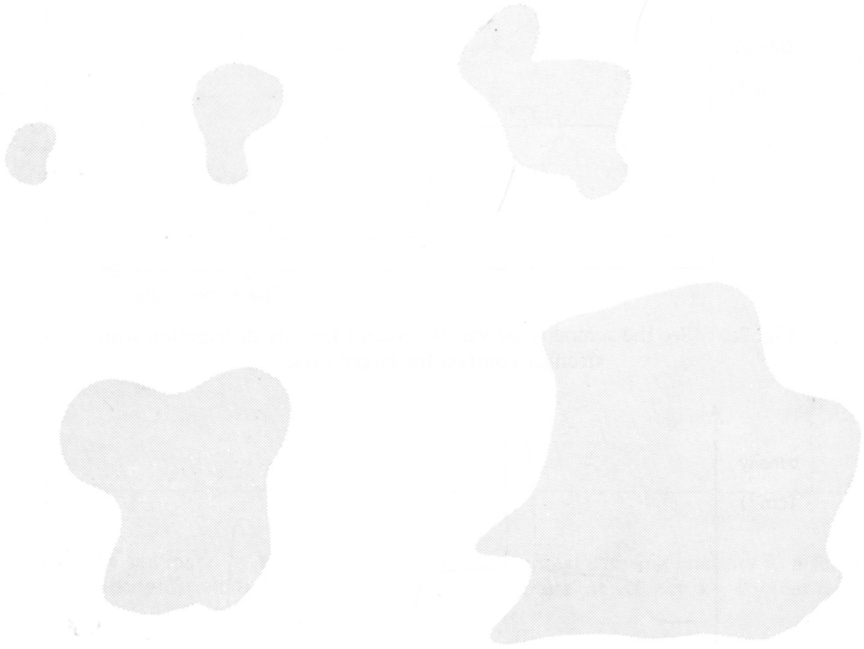

Fig. 1a. (See the summary by van Woerden.) A progression of sizes of interstellar clouds.

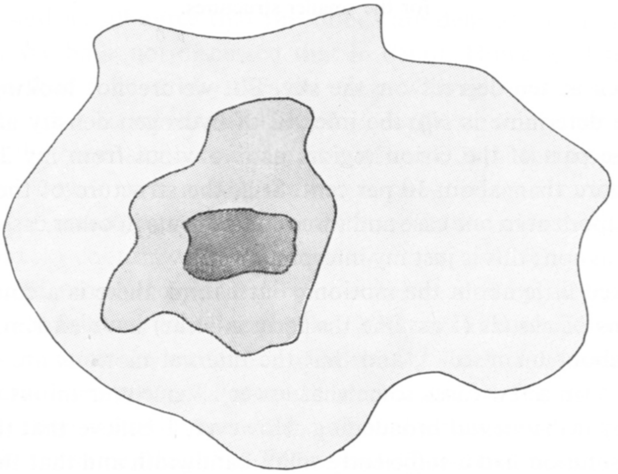

Fig. 1b. A hierarchy of cloud sizes; the smaller clouds are supposed to be inside the larger ones.

correctly, he considers that the density distributions are as shown in Figure 2a: stronger density contrasts for larger scale sizes. I know several cases where the distribution is as shown in Figure 2b: smaller structures having higher density contrast.

In connection with this problem of densities, Stecher has drawn attention to the Ly-$\alpha$ measurements in Orion, which show very slow variation, say 10 per cent over perhaps as much as ten degrees on the sky. But we are not looking at one cloud there; what we determine is $N_H$, the integral of hydrogen density along the line of sight. In a large part of the Orion region, as is obvious from my 21-cm work, $N_H$ varies by no more than about 10 per cent. Still, the structure of the profile varies: there is one component in one case and three components in other cases. (This was not part of our discussion; this is just my interpretation.)

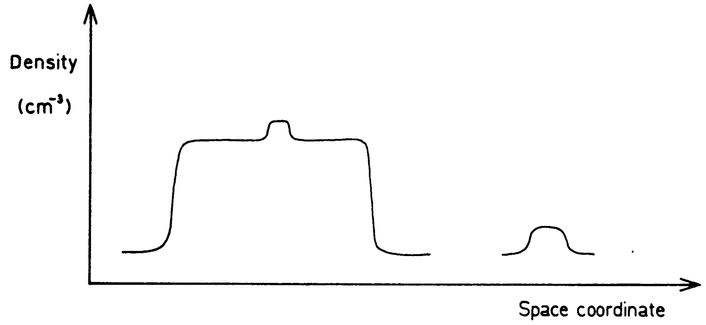

Fig. 2a. (See the summary by van Woerden.) Density distribution with stronger contrast for larger sizes.

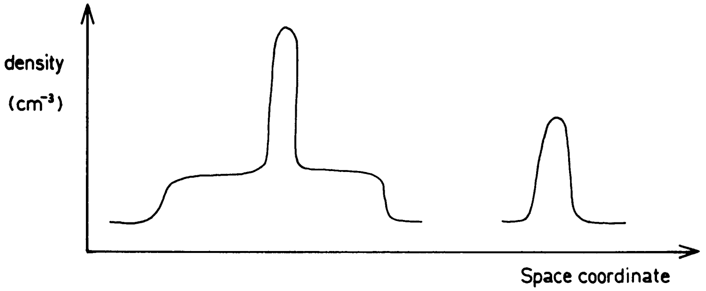

Fig. 2b. (See the summary by van Woerden.) Density distribution with higher contrast for the smaller structures.

We have talked little about the motions, but I think there is a consensus that the external motions of clouds (I assume the term is clear) have an r.m.s. value in one coordinate of about 6 km sec$^{-1}$, and that the internal motions are of the order of 1 or 2 km sec$^{-1}$ (in a few cases somewhat lower). Verschuur thinks the latter value is determined by instrumental broadening. However, I believe that the observations with highest resolution had a sufficiently small bandwidth and that the values for the internal motions are reasonably reliable.

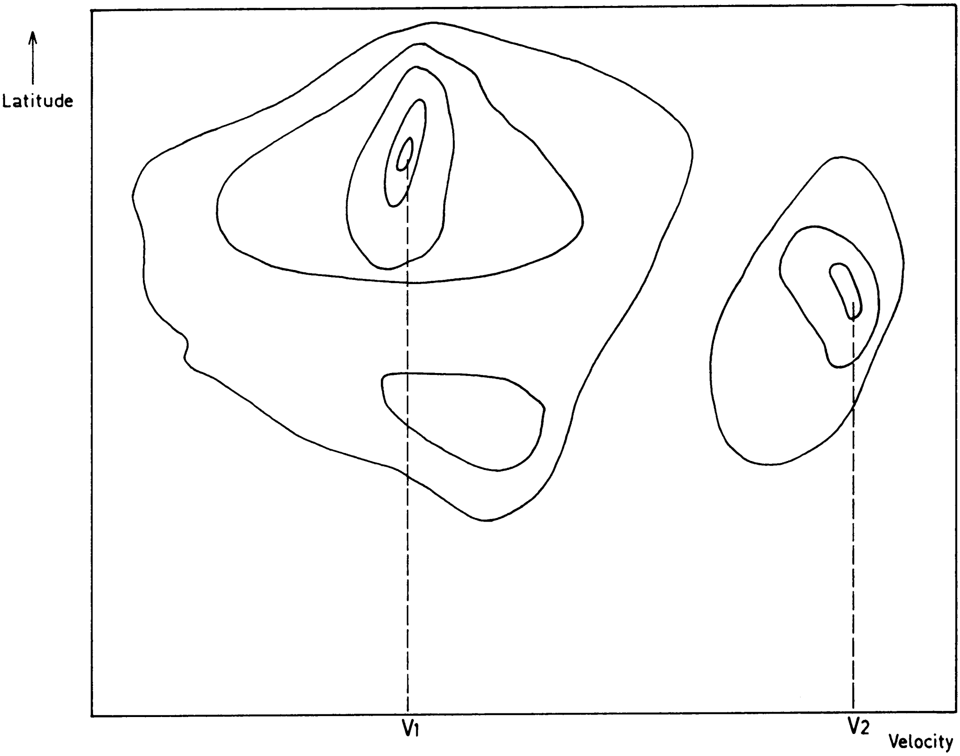

Fig. 3. (See the summary by van Woerden.) Contour diagram showing intensity as a function of latitude and velocity for contrast longitude. There is a 'feature' at velocity $V_1$, another one at $V_2$.

That was essentially all of the discussion about interstellar clouds from the observational side.

**Thomas:** You said we all agree that the clouds are defined by the velocity. How?

**Van Woerden:** We have not discussed that in detail. However, I think there are two ways. The method followed by Weaver works with contour diagrams: intensity contours in a diagram with velocity and one sky coordinate (longitude $l$ or latitude $b$) as variables, and the other sky coordinate fixed (Figure 3). If the intensities are high at a particular velocity $V$, over a range in $l$ or $b$, one distinguishes a feature (say, a cloud or some big structure) at velocity $V$ and tries to follow it on the sky by considering also the second sky coordinate.

The method used mostly by us at Groningen, but also by other workers (van Woerden, 1967) is as follows: in a line profile $T_b(V)$ obtained at one position ($l$ and $b$ fixed), a few components are recognized (Figure 4), which must mean that in this direction we observe a few groups of atoms, each characterized by an average velocity, by a velocity dispersion around the average, and by the number of atoms in the group. (This is essentially the same thing that Adams and others have done in the analysis of cloud spectra.) We next examine whether similar components (that is, components

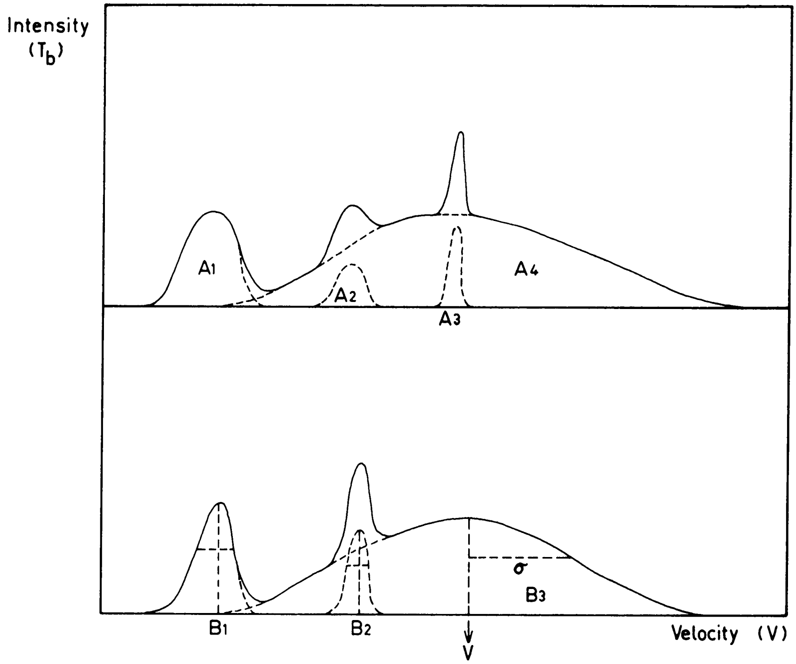

Fig. 4. (See the summary by van Woerden.) Profiles $T_b(V)$ at two neighboring positions on the sky. The top profile has four components: $A1$, $A2$, $A3$, $A4$; the bottom profile three: $B1$, $B2$, $B3$. For the latter three, velocity $V$ and dispersion $\sigma$ are indicated by the position and half the width of a cross. Components $A1$ and $B1$ are considered to belong to one cloud, $A2$ and $B2$ to another, $A4$ and $B3$ to another; $A3$ belongs to a cloud appearing only at position $A$.

having similar velocity $V$ and dispersion $\sigma$) are present in profiles at neighboring positions; if so, we consider these components as belonging to the same 'cloud', and can then determine the density variations within the cloud and various other cloud properties. Obviously, our procedure requires the variations of $V$ and $\sigma$ within a cloud to be slow. The two methods described are, I think, not very different in principle. [Van Woerden, H.: 1967, *IAU Symposium No. 31, Radio Astronomy and the Galactic System* (ed. by H. van Woerden), Academic Press, London, p. 11.]

**Verschuur:** I disagree with about half of what van Woerden has said, but I am not sure yet which half!

**Pikel'ner:** I now call upon Dr. Field to present a summary of this afternoon's discussions on the theoretical aspects of H I clouds.

**Field:** The discussion of the theory centered on the presentation by Pikel'ner of a complete, but not yet worked-out, picture of the formation of, first, spiral arms and, second, interstellar clouds. In this picture, the basic mechanism involves a gravitational instability in the stellar disk, which leads to a condensation of stars into spiral arms producing a larger gravitational field in the arm than in the interarm region. This mechanism has not been discussed in detail at this meeting, but we have heard about it several times. It was presented at the IAU Symposium No. 38 in Basel. For our purposes, however, the important point is that the interstellar gas is swept over by this spiral pattern; it experiences an increased gravitational field and consequently a compression in the vertical direction ($z$-direction). This compression raises the pressure everywhere through the medium and leads to thermal effects which then become important in discussing the formation of clouds. In the ($P$, $\varrho$) diagram (see Figure 3 at p. 55) a critical pressure exists, and if within a spiral arm the pressure anywhere exceeds this critical value, a transition must occur to the high-density phase; the results of this transition are identified as clouds. The time-scale for such instability is essentially the cooling time for the material, $10^6$ yr. In addition, because of the existence of the magnetic field, this instability leads naturally to a second instability, namely that discussed by Parker in his Report (see p. 168). Basically it is a Rayleigh-Taylor instability. In this second instability the material moving along the lines of force causes a downward depression in these lines and the gas slides further down along them. Cosmic-ray pressure outside the clouds accelerates the growth of the instability. We expect the Rayleigh-Taylor instability to occur behind the leading edge of the spiral arm, along with the development of clouds. The associated time scale is $10^7$ yr. As increasing amounts of material stream into massive clouds, their density increases as well, and there may come a point where self-gravitation of the gas becomes important. This can lead to the collapse of clouds and possibly to the formation of stars (including unstable stars such as supernovae), which may accelerate the gas out again and account for the motion of clouds. Moreover, the mere fact that the gas is flowing into the low region in the magnetic field gives it a velocity, the energy source being the cosmic rays which are inflating the magnetic field and tending to drive the gas up to levels of higher gravitational potential.

Pikel'ner mentioned the fact that, the magnetic field being strong, transverse Alfvén waves will be a very important mode of motion. It seems to me that up until this time we have not properly considered Alfvén waves in discussing 21-cm observations. Such motions could be as large as several km $\sec^{-1}$, and therefore could be important in the discussion of both internal and external velocities of clouds. In particular, large-amplitude Alfvén waves may explain the large (probably supersonic) velocities within individual clouds inferred both from 21-cm line and from $Ca^+$-line studies.

It seems to me that we have a splendid opportunity in the next few years to merge theory and observation. On the one hand, the observational situation has greatly improved as a result of the possibilities of accumulating large numbers of profiles and making maps in a limited amount of time. On the other hand, we have developed a fairly clear picture of clouds, and while there is, of course, much work to be done in elucidating the non-linear problems here, still one can already make some predictions. For example, one might expect that clouds would tend to develop toward the rear of the spiral arm, rather than in the front, because of the time delay involved. Among the questions of interest to theoreticians are the ones mentioned earlier: the density contrast, the sharpness of distinction between dense regions and not-so-dense regions, and the velocities within certain structures (are they supersonic or are they subsonic?). Also, can structures be found which stretch along the magnetic field (e.g., along

Mathewson's helix), or are they, as some theories would suggest, compressed into pancakes along the magnetic fields? These questions, I think, are directly relevant for the theoretical developments in the next several years.

Finally I would like to express some thoughts about how we are to use movies that we have seen and those that are still to come. I think the history of solar physics suggests that it is possible to make movies without doing science. Movies can be helpful in giving qualitative ideas about the phenomenon involved; but for comparison with theory we, of course, need numbers. It is absolutely essential that as much of the data as possible be reduced to quantitative numbers.

**Thomas:** I think it is interesting that we have the same situation in the interstellar medium as we have in stellar atmospheres; viz., turbulence is still just a word of ignorance, associated with very little clear understanding of the physical implications. I got that feeling, after listening to the discussions in the preceding two-hour session on the H I clouds. This is a beautiful field to work in. The concepts are undefined, the interpretation can only be called optimistic. Observers can disagree violently on what they see, then summarize by saying that all agree on definitions. They can argue whether the velocities are subsonic or supersonic, then make a linearized theory for their origin. Also, as an occasional solar astronomer, I have seen many solar movies, and I would take a position 180° from Field's. In a situation such as the interstellar medium or the inhomogeneous solar atmosphere, I think that progress comes first by looking at the most graphic presentation of the greatest possible array of data. I was extremely impressed by Weaver's presentation and his cautious attempt to give what might be a definition of a cloud or concentration. I have the feeling that at present we do not have any more physical feeling for what we mean by 'turbulence', 'cells', 'clouds', or 'concentrations' here than we do when we discuss 'turbulence' and 'inhomogeneities' in the stellar atmosphere. I regard these as optimistic remarks, because there is so much to be done. I hope we can systematize the data and our conceptual thinking about them.

**Weymann:** How long does it take for a spiral density wave to travel across a given gas element?

**Field:** Between $(3 \text{ and } 10) \times 10^7$ yr.

**Weymann:** Do you think it is a trivial or a non-trivial point that only clouds exist in a fairly narrow range of pressure and density on your $(P,\varrho)$ diagram? Can one understand how there could be a whole range of densities?

**Field:** I do not know a satisfying answer to Weymann's question; I have not dealt with the problem yet. Goldsmith (1969) has considered some linear calculations of stability and to some extent a few non-linear calculations of the development of the thermal instability after being induced by a shock wave. In a few shock calculations Goldsmith found a transition from the unstable to the stable phase in $10^6$ yr. One expects that on shorter time scales intermediate phases will indeed be found. This time scale is only a few percent of the time required by a gas element to go through a spiral arm. I think we may well find that the proper resolution of the problem is a dynamical cycle in which we find compression, then condensation; then, following the passage of the arm, expansion again, only to be followed by recompression. And the dynamical aspect of this problem, when fully explored, will show that 10 to 30 percent of the gas is in intermediate phases. That is one new problem to study which I will take home from this Symposium. (Goldsmith, D. W.: 1969, Thesis, University of California, Berkely.)

**Habing:** I would like to point out a possible difficulty in the cycle that Field described. The difficulty is with the expansion phase. The option is that at the rear of a spiral arm the clouds evaporate, because the surrounding pressures become so low.

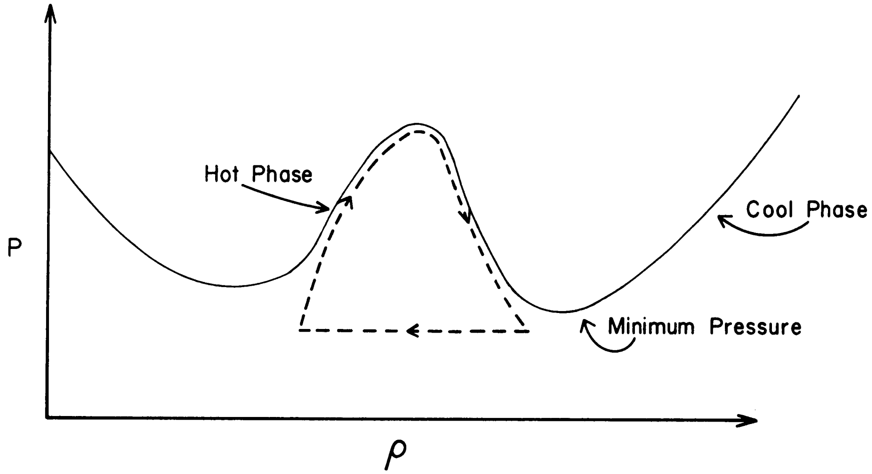

Fig. 5. (See the remark by Habing.) ($P$, $\varrho$) curve similar to that of Figure 3 at p. 55, but extended (schematically) to lower densities and higher temperatures. There is a second minimum in the curve at the lower densities. If this minimum is less deep than the other one, phase transitions at minimum pressure cannot occur in the direction indicated by the broken line. The broken line indicates a possible cycle, as discussed by Field.

One would guess then that evaporation takes place at constant pressure. But if one calculates the extension of the ($P$, $\varrho$) curve toward higher temperatures, it turns out that there is no thermal equilibrium point available at the high temperature end (see Figure 5). The ($P$, $\varrho$) curve has to increase (for decreasing $\varrho$) because of bremsstrahlung-cooling. Figure 5 is only schematical; we still have to make more detailed calculations.

**Field:** In addition to Habing's remarks, I would like to say that the situation at the high temperatures is still a little unclear. The reason is that in order to study the thermal equilibrium in this high-temperature region, one must have the ionization equilibria (in the presence of cosmic rays) of C, N, O, and perhaps Fe, through many stages of ionization. Since all these elements contribute to the cooling at the high temperatures, I think it is too early for us to say that we understand that branch of the curve.

**Sunyaev:** There exist calculations by Cox and Tucker (1969) of the cooling rate for a plasma with normal cosmic abundances. I know that several people present calculated the thermal equilibrium in the intergalactic medium for H and He. Why can you not use these calculations? (Cox, D. P. and Tucker, W. H.: 1969, *Astrophys. J.* **157**, 1157.)

**Field:** I think those calculations are not directly applicable to this situation, since we are talking about cosmic-ray heating and cosmic-ray ionization. The ionization equilibrium in Cox and Tucker was for thermal collisions only and did not include cosmic-ray ionization.

**Sunyaev:** I understand. But if the thermal electron collisions in the plasma give a degree of ionization higher than the cosmic-ray flux, the cooling rate is the same as in the collisional case. And the degree of collisional ionization increases very rapidly with temperature.

**Field:** I do not think that you are necessarily right. In thermal equilibrium the only ions present in significant numbers are those with ionization potentials of the order of 10 kT. However, in the presence of cosmic rays, ionization can take place to a very much greater degree, and it works out that the ionization equilibrium can in fact include all the stages more or less equally. The ionization equilibrium with cosmic rays may be completely different from that without cosmic rays.

**Pikel'ner:** Field mentioned that the Rayleigh-Taylor instability collects the gas and that it is one of the mechanisms of formation of the denser clouds. After the formation of hot stars in these complexes, H II regions appear and push the remnants of the gas complex out into space. Very probably part of the gas will not flow out at all, but will stay at the same place. Ultimately the magnetic field will remain slightly curved there. After the H II region disappears, the gas clouds can collect again in this place. We should therefore observe the formation of stars at the same place in time intervals of $10^7$ yr. For example, in the Orion region one finds older stars with ages of about $10^{10}$ yr and very young stars with ages much less than $10^6$ yr. I think we observe here the recurrent appearance of hot stars within time lapses of about $10^7$ yr.

**Field:** Dr. Parker, there is an interesting study by Kippenhahn and Schlüter (1957) about confinement of cool solar matter in a magnetic well. They studied a two-dimensional situation. You said earlier that you expect the Rayleigh-Taylor instability to develop with sharp variation in the third dimension. Can you explain to us the difference between this Kippenhahn and Schlüter picture, which ended up with what is observed in the Sun (namely, a long filament suspended in the magnetic field perpendicular to the field), and your picture, where you have sheets along the field? (Kippenhahn, R. and Schlüter, A.: 1957, *Z. Astrophys.* **43**, 36.)

**Parker:** I will draw it on the board (Figure 6). The model was meant for solar prominences in which material is supported on magnetic fields coming up from the Sun. The material is in its cool, dense phase due to thermal instability. I was stating a different situation for the galactic field. When it becomes unstable, it tends to slice itself in the third dimension, giving sheets parallel to the plane of the paper. The situation studied by Kippenhahn and Schlüter was different since in their case the lines of force are rooted in the Sun, whereas you have no such stabilizing effects in the Galaxy.

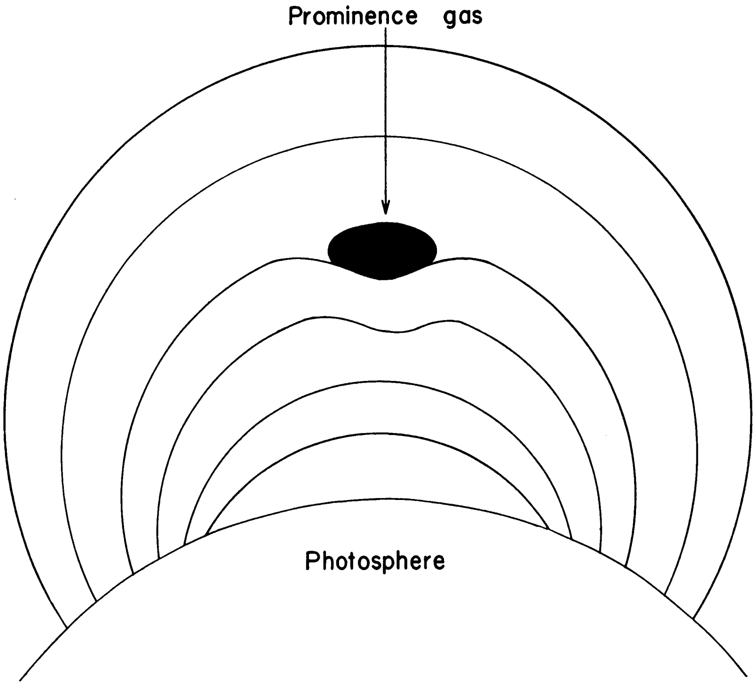

Fig. 6. (See the remark by Parker.) Schematic drawing of the Kippenhahn-Schlüter model for cool solar material suspended in a magnetic well in the solar corona.

**Lüst:** To me the rooting of the magnetic lines in the Sun is an essential feature of the Kippenhahn-Schlüter model.

**Mestel:** Kippenhahn and Schlüter found that their model was unstable if the gas is supported by a local dipolar field. The gas tends to slide down into the Sun if the field is given a slight asymmetric disturbance. For stability they required a local quadrupole field.