DISCUSSION FOLLOWING REVIEW BY S.E. STROM

BOK: Guillermo Haro has asked me to send his greetings to this Symposium. He regrets his inability to be present. He has prepared a lengthy article on T Tauri stars and Herbig-Haro objects. This article surveys his views on the evolutionary status of these stars and objects. It is published in nr.2 of the new series of Tonantzintla Bulletins. He will be happy to send copies of this article, upon request, to anyone desiring a copy.

APPENZELLER: I think that mass inflow (rather than outflow) in T Tau-like objects occurs much more frequently than has been assumed so far. We recently started a spectroscopic survey of T Tau stars which produced definitive spectroscopic evidence for inflow in several well-known T Tau stars where no inflow effects were detectable on the earlier low-dispersion spectrograms. In addition we found one star where inflow and outflow is observed at the same time. Since blueward displaced absorption (i.e. outflow) in this star is observed only in the Ca II H and K lines and since these lines are very sharp, we conclude that the outflow takes place in the cool matter far away from the surface.

BERGEAT: In Kuhi's spectral energy distributions (Astr. Astroph. Suppl. 15, 47, 1974) no feature is present at the Paschen limit in most T Tauri stars. Even the Balmer continuum emission and crowding of the Balmer emission lines are not straightforward explanations for the ultraviolet-blue veiling. For gas envelope models, clumps such as Kuan's (Ap.J.202, 425, 1975) are needed. Does somebody plan to make quantitative models for a gas envelope, including both bound-free and free-free emission?

STROM: Not to my knowledge, although such quantitative models would be quite valuable. I should emphasize again the importance of extracting from the composite "photosphere-envelope" spectrum, the envelope contribution. The discussion in my review suggests several observational programmes which might result in a better understanding of the envelope emission spectrum. Particularly helpful might be further observation of T Tau star spectral energy distributions in relatively unobscured regions such as NGC 2264 and the Orion OB association (see Warner, Strom and Strom, Ap.J. in press).

WERNER: Can you say anything about the number of stars in T Tauri and similar stages, the total mass of these stars, and how this relates to the rate at which stars are thought to be forming locally in the galaxy?

STROM: I don't know these numbers offhand. Herbig (Adv. Astr. Astroph. 1, 47, 1962) has computed such rates. However, as he emphasizes, they are based on the number of T Tau stars observed at the surface of dark clouds and hence are lower limits.

LADA: Is there any difference in total number or number density of T Tauri stars found in a pure T-association as opposed to a T-association which is part of an OB association?

STROM: I don't recall the stellar densities in a typical T-association. However, Herbig has summarized these densities in his comprehensive review (reference cited above).

HERBIG: Many years ago I estimated that within about 1 kpc there are several thousand T Tauri stars brighter than about M(pg) = +11. However, correction for various kinds of incompleteness is very uncertain. On H-H objects, there are perhaps 50 known at the present time, but that number may be very much a minimum, and I could not estimate their volume density.

KUHI: Just to add to the confusion of infall and outflow in T Tauri stars. The Na I D lines in many T Tauri stars show quite a varied behaviour: they show up in emission of variable strength and with absorption components on the red in some stars as well as on the blue in others. The Hydrogen lines however show no evidence for this infall but have symmetrical profiles or even absorption components at velocities from zero to a few 100 km s^{-1} to the blue. Thus there seem to be two different velocity fields present in the same star.

STEIN: Are the Balmer and Sodium lines optically thick or thin?

KUHI: The lower Balmer lines are optically thick; I don't think that the Na D lines are thick but I do not really know.

ZUCKERMAN: If the red-shifted absorption lines seen in young stellar objects are in fact indicative of mass infall, are the implied infall rates comparable to the outflow rates that you have spoken of?

STROM: As I recall from Ulrich's model (Ap.J. 210, 377, 1976), yes.

FIELD: Would you not expect both infall and outflow to occur simultaneously if the stellar wind turns on before the accretion process is completed? It seems to me that the interface between the wind and the infalling gas might be Rayleigh-Taylor unstable, and that some of the cold gas might fall all the way to the surface, thereby acquiring the extremely high positive velocities referred to by Dr. Appenzeller.

LARSON: If a protostellar core begins to generate a hot stellar wind while it is still accreting cooler protostellar gas, Rayleigh-Taylor instabilities might cause the infalling gas to be concentrated into clumps or streams which continue to fall into the core, while in other directions, less dense gas is blown out by the stellar wind. If so, it seems possible that outflow and infall, possibly of a sporadic nature, could occur simultaneously in different parts of a protostellar envelope.

HABING: Another complication could be the presence of double stars, which could lead to very complex in and outflow phenomena.

McCREA: If a star is formed by assembling blobs of interstellar material, the blobs themselves are geometrically very much more extended than the star itself. There is no difficulty about a blob falling into the forming star because of gravitational focusing as it goes in. However, there must, of course, be many fairly near-misses by other blobs besides those that actually fall in. These must at any instant form around the star something like the envelope of a T Tauri star described by Dr. Strom. It will look as though it were composed of in-going and out-going material. Thus we have a model that appears to be in qualitative agreement with the observations. I have tested it also quantitatively in a simple way, and the results seem to be reasonable.

KUHI: I have a question to the theoreticians present about Roger Ulrich's infall model (reference cited above). It seems that he neglects the major contribution to the emission line profile from the infalling material which ends up in the disk. I don't know much more theory to make any legitimate criticism; does anyone here know more about it?

STROM: I am sorry that I cannot summarize Ulrich's model for you. It is my impression that the conditions required to produce a P Cygni profile in his infall model were rather specialized and perhaps a bit contrived. However, lacking the details of his model, I don't wish to dismiss it. I understand that Appenzeller and his colleagues have done some calculations of infall models for T Tau stars. Perhaps he might wish to comment.

COSTERO: I would like to point out that the only direct observational estimate of the mass of a T Tau star is that of V1057 Cyg, classified originally by Herbig as a late-type T Tau star <u>before</u> its 6 mag. brightness increase and subsequent evolution into a B-type star with a cometary-like reflection nebula. This evolution resembles that of FU Ori; the morphology of the nebula and the dark cloud in the surrounding region is very similar to that of a good number of objects (including FU Ori), studied by Herbig some time ago (Ap.J. Suppl. <u>4</u>, 337, 1960), all of them classified as A and B stars. One is tempted to conclude that all such stars are evolved T Tau stars and that possibly all T Tau stars are massive stars, surrounded by some kind of an envelope producing the late-type spectra observed in the earlier stages.

KANDEL: It is generally assumed that T Tauri stars are low-mass objects, but Grasdalen (Ap.J. <u>182</u>, 781, 1973) has found 8 $M_\odot$ for V1057 Cygni, and while many things may be wrong with that estimate, it is probably more "firmly" based than any other mass estimate of a T Tauri star.

APPENZELLER: I would like a comment on the mass of V1057 Cygni. Grasdalen in his mass determination assumed at one point thermal equilibrium of the star. In my opinion this assumption is highly unrealistic

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since we know that a violent event has occured in this star quite recently and since the thermal relaxation time of stars (the Kelvin-Helmholtz time) is very long. An attempt to determine the mass of V1057 Cyg from its spectrum alone has been made recently by B. Baschek and some other people at Heidelberg. Their result was that from the spectrum alone the mass could be anywhere between 0.2 and 15 $M_{\odot}$.

CARRASCO: T Tauri stars were originally classified spectroscopically as dwarf stars by Joy. That is to say that apparently these stars have high gravities. However, later on, Mendoza (Ap.J. 151, 977, 1968) discovered large infrared excesses for these stars. These excesses placed the T Tauri stars as giants in the L $-T_{eff}$ plane. Now, how can we reconcile these two contradictory observations? It seems to me that probably this contradiction can be solved if a large fraction of T Tauri stars are of high mass, say in the 3-15 $M_{\odot}$ range as suggested originally by Grasdalen.

Herbig-Haro objects.

HERBIG: I don't want to say that there may not be a few cases where reflection of a deeply-buried star is involved but as a general explanation of HH objects, I cannot agree with Strom's reflection nebula hypothesis. Let me review my misgivings.
(1) It is supposed that all these objects are peering down tunnels in the dust at the end of which they see a very peculiar "HH star". Unless Strom's idea that HH 1 is in fact an "HH star" is correct, which would make the whole reflection nebula hypothesis unnecessary, it is strange that we never have seen a "HH star" directly: none of these 40 or so tunnels point to us.
(2) In HH 3, the different nuclei presumably all reflect the light of the same source, yet they show different line ratios in their emission spectra.
(3) There is a perfect HH nebula enveloping T Tauri, which, as seen from the nebula, must be a very powerful source of illumination; more powerful than in any conventional HH object. Yet the nebula shows a pure emission spectrum; it refuses to reflect the star's light.
(4) Knapp, Kuiper, Knapp and Brown (Ap.J. 206, 443, 1976) have observed CO in Strom's HH 102 which is also S 239. It shows a broad shifted CO line (displaced 3-4 km s^{-1} from the narrow CO feature in the dark cloud itself) and clearly represents a kinematically and physically distinct glob in the dark cloud. It is not clear to me why just this glob is illuminated by the IR star.
(5) There is at least one HH object having a high radial velocity of -150 km s^{-1}. I think Steve regards this as evidence of mass outflow from the illuminating source. But recall that mass outflow in T Tau stars is exhibited by P Cygni absorptions at the emission lines, not by a shift of the emission structure. No P Cygni absorptions have been reported in HH objects.
(6) As hard as it may be to explain, I would prefer to think that some

of these objects are really moving; two of the HH objects near S239 have well-established cross motions, parallel, of about 140 km s^{-1}.

R.C. SMITH: Are proper motions known for any of these T Tau stars?

HERBIG: Proper motions were determined for many T Tauri stars in the Taurus-Auriga clouds by Pels, at Leiden, in an unpublished study in the late nineteen-fifties. They were also measured by Wenzel, at Sonneberg, at about the same time. These studies also indicated that the random motions were small but, of course, in that case no reference to the motions of the molecular cloud was possible.

I.P. WILLIAMS: Is it possible that the cloud boundary of the dark cloud moves in response to some internal or external pressure gradients, magnetic pinching or what have you, rather than the star being ejected. After all it is very irregular and can hardly be in permanent equilibrium in that shape.

HERBIG: It seems entirely possible to me. Of course, direct observation of the cross-motion of such an indefinite boundary would be difficult. I don't believe that the radio frequency molecular-line radial velocity information, integrating as it does over such a long column, would be very instructive.

THOMPSON: What is the typical luminosity of Herbig-Haro objects?

STROM: The energy emitted between 1μ and 20μ is a few $L_\odot$ (see Strom, Grasdalen, Strom, Ap.J. 191, 111, 1974).

COX: One of my students, John Raymond, has recently made shock spectrum calculations aimed at the conditions appropriate for HH objects. He tells me that he is able to find reasonable agreement, at least for some of the objects.

SOLOMON: What are the actual mass loss rates for the HH stars?

STROM: According to estimates by both R.D. Schwartz (Ap.J. 195, 631, 1975) and myself, about 10^{-6} to 10^{-8} $M_\odot$ yr^{-1} (a factor 10 or so greater than for T Tau stars). From the velocity dispersion of the lines and from the mean velocity of the lines relative to the dark cloud velocity, outflow velocities of 160 to 200 km s^{-1} seem reasonable.

COX: How do you visualize the structure which would give you point-like reflection nebulae? Is it structure of true pinholes in the inner placental cloud or structure in the reflecting medium?

STROM: I imagine "pinholes" in the placental cloud.

LYNDEN-BELL: Have Proper Motions been measured for HH objects? If they

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are reflection nebulae caused by small holes in a screen then the movements of the screen holes would be amplified on the more distant reflection nebulae.

STROM: Note in my review, the proposal to monitor reflection nebulae for indications of change in brightness across the face of the nebula which appear to take place on time scales short compared with the light travel time. Such changes would be an indication of a "lighthouse" effect produced by motions of clumps in front of the star.

COX: In the light-house model of HH objects, the spectra will suffer Doppler shifts due to any motion of the reflecting surface. This will introduce at least some apparent radial motions.

VRBA: I will be making polarization measurement this fall to help to resolve the problem of the HH objects. Thus far only three Herbig-Haro objects have convincing evidence for their being reflection nebulae (those which have the perpendicular to their polarization vectors pointing towards nearby embedded infrared sources). A number of others are suspected reflection nebulae since nearby but displaced infrared sources have been found. All of these objects are either very extended or are contained in associations of HH objects and other nebulosity. None of the HH objects thus far proposed to be reflection nebulae are stellar or semi-stellar in appearance.
On the other hand, there are a large number of very compact HH objects which undoubtedly are illuminated by their own energy sources. There are no known displaced infrared sources associated with these objects. Even objects such as HH 1 and 2 (diameters about 20 arc seconds) have been shown by Schmidt and Vrba (Ap.J. 201, L33, 1975) not to be reflection nebulae on the basis of infrared and polarimetric data.
Thus there are at least two different kinds of objects (those illuminated by their own energy and those not) which have been given the same name under the classification criteria originally set up. Therefore generalizing properties of one object to the entire class will only lead to frustration. Only additional infrared and polarimetric work will reveal to which of these two classes each of the Herbig-Haro objects belong.

Rotation, inflow/outflow and radial velocities of pre-main sequence stars.

KUHI: I would like to present some preliminary results on the rotational velocities of pre-main-sequence stars. In line with our chairman's request to make these contributions spontaneous, the results were received on Monday and plotted up this morning. The long-term goal is to provide some stellar input into the angular momentum problem by seeing how pre-main-sequence stars in clusters and associations of different ages behave with regard to their rotational velocities.
The first effort has been concentrated on NGC 2264 which has a well-

defined pre-main-sequence group of stars. Spectra of stars down to 15th mag. have been obtained with the 4-meter image-tube spectrograph at KPNO at a dispersion of 13A mm^{-1}. These spectra are then digitized, subjected to considerable data treatment, emission lines are removed, and finally Fourier-transformed to obtain the power spectrum. The same spectral region of a standard star of the same spectral type is analyzed in exactly the same way and the difference between the two power spectra is a calibratible function of vsini. If the velocity is large, the transform of the rotational broadening function has well-defined zeroes which can be used to determine vsini as well as the slope. We find that the stars closest to the main sequence have the lowest vsini and that the spread in vsini increases as one moves away from the zero-age main sequence. Also if one distinguishes between Hα and non-Hα emission objects then one finds a greater spread in vsini among the former than among the latter. The highest velocity (114 km s^{-1}) is obtained for the faintest stars measured; it is also an Hα emission object. The distribution for non-Hα stars is very concentrated to low values of vsini whereas the Hα stars are fairly evenly represented at all values of vsini. This seems clear in spite of the small numbers of stars involved.

This dichotomy between Hα and non-Hα stars is also shown by the Ca II K emission which is an indicator of chromospheric activity. The non-Hα stars have Ca II K lines with line strengths consistent with the age assigned to NGC 2264 from the nuclear dating via evolutionary tracks. The Hα objects have much too strong emission which would lead to ages up to ten times younger than the non-Hα stars if interpreted using the Skumanich calibration. In addition the Ca II K emission widths do not follow the Wilson-Bappu correlation of absolute visual magnitude with line width: the Hα objects would have $M_V \simeq -17$ if the K-line widths are taken at face value. This is clearly unacceptable. The alternative explanation is that the extreme activity shown in the T Tauri phase (recall that the vsini measurements can be made only for the less extreme stars) is in some way coupled to the solving of the angular momentum problem. Also that once a regular chromosphere has formed after this phase of violent activity it decays in exactly the same way as the chromospheres of main sequence stars.

The rotational velocities of the non-Hα stars when projected to the main-sequence (where they will become G-type stars) are quite reasonable and consistent with Kraft's measurements of vsini for young clusters such as the Pleiades which already have G and K stars on the main sequence. The Hα emission stars do not: in particular the one star with very large vsini is already close to breakup velocity in its present position (it will have to be more carefully located on the HR diagram to see whether it may actually be exceeding the critical velocity) and would clearly have an enormous velocity on the main sequence (if angular momentum were conserved for the star) which is a few hundred times larger than the observed values. The exact interpretation in terms of timescales for spin down, etc. will have to await the translation of the observed color-magnitude diagram into the theoretical HR plane. Such work is in progress.

Nevertheless a few conclusions are evident: (a) chromospheric activity and rotational velocity seem to die down together with the largest change occurring in the transition from Hα emission to non-Hα objects. The mechanism for spin-down may also be responsible for the activity and the outflow of material. (b) the angular momentum problem is not solved in the protostar phase or even earlier since pre-main-sequence G and K stars are found with very high vsini.

FIELD: Would you remind us what the observations of stellar rotation tell us about galactic clusters?

KUHI: Kraft has presented a nice summary of the rotational velocities in other clusters. For G and K stars on the main sequence in the youngest clusters, e.g. the Pleiades, the velocities (v sin i) are of the order of 15 to 20 km s^{-1}. In older clusters the velocity apparently decays with the same law as expressed by Skumanich, i.e. as the square root of the age. There is no evidence for a preferential orientation of rotational axes as might be expected from differential galactic rotation. The v sin i's are well represented by a completely random distribution.

APPENZELLER: I have two questions: First, since we agree that ordered radial mass motions (either infall or outflow) takes place in the line-forming layers of these stars, can you exclude that the line-broadening which you observe is not entirely or predominantly due to these motions rather than due to rotation? Secondly, in YY Ori stars we observed the He I and He II emission lines to be much narrower than all other absorption lines (and all other emission lines). How can this be explained with your assumption that the broadening of the absorption lines is predominantly due to rotation?

KUHI: The line broadening produced in a photosphere with velocity gradients will be quite different from rotational broadening and will show up as a different power spectrum in the Fourier domain. In addition the line profile produced (in absorption) is asymmetric and has a displaced core. We do not see line profiles that look like this. Also we have determined vsini for a number of stars in the Taurus cloud which have velocities in the range of 40 to 60 km/sec. These results were obtained from the same spectra that Herbig used in determining the radial velocities of stars in the Taurus cloud. As you recall, he found no systematic velocity differences between the stars and the gas, and no difference in velocity dispersions either. Consequently it is safe to conclude that no shifted absorption lines were present and hence that your suggestion for the broadening does not work. The narrow Helium lines present no difficulties if they are produced in a region farther out from the photosphere (and hence we see a stratification in temperature) so that conservation of angular momentum would naturally produce narrower lines. YY Orionis stars may of course be a special case; I have not measured vsini for any such star.

KANDEL: If indeed this is rotational and not some sort of "macroturbulent" line-broadening, then it is very important as a spectroscopic criterion of youth. Spectroscopic activity is not a youth criterion in itself, except by circumstantial evidence; one can imagine "activity" sometimes increasing with age, but it seems necessary that rotation should slow down.

APPENZELLER: Did Dr. Herbig in his determination of radial velocities use exactly the same spectral lines as you did for your analysis?

KUHI: Herbig picked out the absorption lines best suited for his measurements of radial velocity. I used all the absorptions on the same plate and consequently included all of his lines as well. The Fourier analysis is not performed for just a single line but all of them together in order to improve the signal-to-noise.

MESTEL: Can you fill us in with more detail about the discrepancy from the normal Ca K line - age relation, shown by the Hα emission stars? As these show also a much bigger vsini range, I was wondering whether this contains a further hint as to the dependence of a dynamo-built magnetic field on the rotation of the star.

KUHI: The normal Ca II K emission line width for G and K stars correlates well with the age of main-sequence stars. In NGC 2264 the non-Hα emission stars show strong Ca II K emission, which produce an age in agreement with the nuclear age for the cluster as obtained from the upper main sequence. That is, it seems that the decay of the chromosphere has already started in pre-main sequence stars and follows the same law as decay in main sequence stars.
However, the Hα objects are quite different: the Ca II K emission is so strong that the computed ages would make the Hα stars at least 10 times younger than the non-Hα stars. In addition the Wilson-Bappu effect which correlates Ca II K emission line-width and absolute visual magnitude would result in $M_V = -17$ for the Hα emission objects. Such a luminosity is totally inacceptable to everyone.
Consequently it seems most likely that the Hα objects do not yet have normal chromospheres but rather extremely active atmospheres with mass flow and all the other complex phenomena we have heard about at this conference. However, the transition from Hα to non-Hα stars results in a normal chromosphere which once formed decays with the same function of age (and presumably the same physical parameters) as main-sequence chromospheres.
The Hα emission stars do indeed show a much larger range of v sin i than the non-Hα objects. You are quite free to use this fact to develop any theory relating dynamo magnetic fields and rotation that you wish.

HERBIG: Although low-dispersion radial velocities were measured for many T Tauri stars by Joy about 30 years ago, their accuracy is too low to provide the answers to some interesting questions. The problem is that the absorption spectra (one distrusts the emission lines) are difficult to measure in the blue because of "veiling" and confusion by emission lines.

New measurements have now been made for some 50 T Tauri stars at much higher dispersion, and in the red spectral region where the absorption spectrum is better seen. For some 26 stars in the Taurus-Auriga cloud, direct comparison is possible between these new velocities and those from the H_2CO or CO molecular lines at the same point, determined by N.L. Dieter at Berkeley or Gill Knapp at CalTech, respectively.

Briefly the results are that the velocity difference, ΔV, star minus molecular cloud, is symmetrically distributed around a mean of $+0.2 \pm 0.9$ (s.d.) km s^{-1}, with $\sigma = 3.9$ km s^{-1}. A check on standard velocity stars observed with the same equipment gives $\sigma = 4.4$ km s^{-1}. Thus, the scatter in ΔV is due to errors of measurement, and the true dispersion of T Tauri stars with respect to the cloud is not detected; one would estimate that it cannot be greater than about 3 km s^{-1}. The data also show that these stars, in the mean, are not rising or falling with respect to the cloud.

A question of interest is whether any spectroscopic binaries exist among these T Tauri stars. Comparison of 69 spectrograms of 29 stars was made with the large sample of F3-G2 IV and V stars near the sun observed for binaries by Abt and Levy (Ap.J. Suppl. 30, 273, 1976). By scaling their statistics one would expect to find - if the number and kind of binaries in the two samples are the same - about 7 spectrograms whose departures from cloud velocities would have exceeded certain tolerances. In fact, there are 9 such velocities, which is acceptably near expectation. One concludes that the fractions of binaries in the two samples do not differ significantly. One can argue that since the probability of an eclipse is proportional to the stellar radii in units of the separation of centres, a better way to find close binaries in Taurus-Auriga would be to search for eclipsing variables.

Since the velocity of escape from these clouds (on conventional assumptions) is probably around 0.5 to 1 km s^{-1}, one wonders whether these stars are trapped. Unfortunately, these observations do not settle that point, but one notes that a few T Tauri stars in these clouds and elsewhere are found well outside cloud boundaries. One can imagine that such stars can escape by (a) being on the high-velocity tail of their velocity dispersion, or (b) are released when their clouds are reconfigured or disrupted, or (c) there may be points in such irregularly shaped volumes where the velocity of escape is very low.

THOMPSON: I would like to discuss spectra of Lk Hα 101. These spectra show a P Cygni-like profile which has a total width of 1000 km s^{-1}. The absorption appears at a velocity of -400 km s^{-1} with emission blueward of that position. A possible geometrical model to explain the data consists of a displaced dust cloud which provides the continuum in the infrared. Only part of the velocities are seen along the line of sight to the dust cloud. Therefore the absorption component is not at the maximal velocity of the gas. A preliminary mass loss estimate of 10^{-6} $M_\odot$ yr^{-1} has been derived.

KUIPER: Dr. Strom suggested just a while ago that radio astronomers should attempt to look for evidence of the effect of Herbig-Haro stars on the surrounding cold cloud medium. I'd like to discuss briefly whether the Orion CO data might not be showing us just such a case. This is motivated by the observations of Dr. T. Gull that there are a few Herbig-Haro-type patches in the Orion nebula near the Kleinmann-Low infrared nebula.
[The work that I am reporting will appear soon in the Astrophysical Journal (Zuckerman, Kuiper and Rodriguez-Kuiper, Ap.J.209, L137, 1976). We have observed the Kleinmann-Low nebula in CO and we have found that the CO profile extends to at least $\pm$ 75 km s^{-1} from the line centre. Further integration may show it to extend even further. The profile does not resemble the commonly observed case of a "spike" superimposed on a broad "plateau". In this case the wings tail off gradually in a continuous way. We have also mapped the region around the KL nebula and we find that the region which gives rise to the wings beyond $\pm$ 10 km s^{-1} has an extent of 65 x 75 (arc seconds)^2, about the size of our antenna beam. However we do not want to suggest that the source is resolved.]
We consider some possibilities for interpreting this profile in terms of a mass outflow. If we assume the wings to be optically thin, then the amount of CO seen in the wings between $20 < |V| < 75$ km s^{-1} would correspond to a total mass of gas, moving with these velocities, of 2.5 $M_\odot$, where we have taken a CO to H_2 number density ratio of 5×10^{-5}. Now at 75 km s^{-1}, matter moves out of the region encompassed by the beam in 10^3 years. Thus, if we are directly seeing the gas ejected from a central object or cluster, we would have to account for a mass-loss of 10^{-3} $M_\odot$ yr^{-1}.
Another possibility is that we are not seeing the ejected mass directly, but rather its effect on gas in a surrounding cloud. Since Gull reports blue-shifts up to 300 km s^{-1} in his Herbig-Haro-type features, we can do a simple momentum transfer calculation to find that a few times 10^{-4} $M_\odot$ yr^{-1} at this velocity can accelerate the gas observed in the CO line wings. I think that this may still be rather high to explain in terms of a cluster of HH stars, unless the mass-loss rate for the objects in the Kleinmann-Low nebula is higher than Dr. Strom has suggested for HH stars. [.....].

SCHATZMAN: The calculations of Woodward show a cloud with spikes which suggest large outgoing and incoming velocities, the spikes being due to Rayleigh-Taylor instabilities. Would such a velocity field explain the profiles which you observed?

WOODWARD: The velocities of the Rayleigh-Taylor spikes in my calculation relative to the rest of the cloud were only 2 or 3 km s^{-1}, so that I don't see how this mechanism could be responsible for the very broad CO lines Dr. Kuiper has observed.

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STROM: The large velocities you observe imply very short lifetimes for the phenomenon under consideration. Do you have any comment concerning this?

KUIPER: Well, as mentioned, in a uniform homogeneous outflow any given amount of moving material will spend only a few thousand years in the region encompassed by the telescope beam. If all the material present is moved out at the same time, the event would be over quite soon. However, if at any given time, only a small fraction of the CO bearing gas is removed, the phenomenon can last rather longer. Also, if the dominant contribution to the line-of-sight velocity is turbulent, then the net outward motion can be quite slow.

HABING: Why did you take an outflow model and not an inflow model?

KUIPER: We did consider an infall model and have decided that it is not a very attractive explanation. We thought it might be more appropriate to present this discussion after Dr. Larson's review paper.

WERNER: Dr. Kuiper alluded to an interesting and important point which, surprisingly, has not been brought up previously. This is the question of the relationship between the protostars or pre-main sequence objects observed principally in the infrared, which were discussed yesterday, and those which are observed optically, which are being discussed now. It is strange that these two types of object have been described separately with no discussion of their relationship. My feeling is that the infrared protostars are more luminous and therefore probably more massive than the T Tauri and emission line stars discussed today. If this is the case, the mass loss estimates appropriate for the T Tauri objects need not apply to the infrared cluster.

LARSON: If mass outflow from a massive star is taking place within the KL nebula, is there a problem in that no compact H II region is observed there?

KUIPER: If the mass outflow were driven by the classical rocket effect, then there certainly would be a problem. If we want to accelerate the inferred mass to 75 km s^{-1} by ionizing cold cloud material, then we would need to convert 50 $M_{\odot}$ to ionized gas. There is no indication of this much ionized gas. If the outflow were driven by mass-loss from a central cluster, then it is not clear to what degree the expelled material is ionized, and how much ionized gas would actually be present at any given time.

Miscellaneous short contributions.

TAYLOR: I wish to describe briefly some observations which I obtained, in collaboration with Dr. Guido Münch, on three very interesting knots

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of emission located in the vicinity of θ^2 Orionis. The observations were made using a double etalon, scannable Fabry-Perot monochromator at the cassegrain focus of the 1.5 m telescope at Mt. Palomar.
All three condensations have dimensions of the order of 10^{16} cm and appear to be, in part at least, ionized by radiation from θ^2 Ori, with one containing a visible star at its centre. Our Hα and [N II] λ6584 velocity information show that the ionized gas, associated with all these objects, is supersonically blue-shifted by up to 60 km s^{-1} with respect to the background Orion H II region. Furthermore, their radial velocity and angular distance with respect to θ^2 Ori are anticorrelated. Our observations are, in fact, compatible with the idea that all three knots of emission were ejected from the vicinity of θ^2 Ori some 800 years ago, with an ejection velocity of approximately 90 km s^{-1}.
If indeed this scenario is correct, then it is difficult to understand how, if totally ionized, these objects can survive for this period of time while moving through the ambient H II region at Mach 7. However, if, as seems more probable, the knots of emission actually hide a neutral object at their centre, we have to explain their highly supersonic motions. Three processes may be discussed as candidates for the ejection mechanism of a neutral globule from an O star like θ^2 Ori.
(i) The Oort-Spitzer "Rocket Effect". Simplistic calculations of this process have great difficulty in producing the highly supersonic velocities for realistic initial conditions. However, more detailed calculation may be necessary before this process can be ruled out.
(ii) Stellar wind pressure on the condensation. This process may be capable of producing the high velocities provided the neutral globule does not become totally ionized by the O star before appreciable velocities are attained.
(iii) Sling shots, due to the creation of unstable orbits in the fragmentation of collapsing clouds, preceding star formation.
It is interesting to note that, with respect to the third process, the condensations are very close to the strong ridge seen most clearly in neutral Oxygen (Münch and Taylor, Ap.J. 192, L93, 1974) and 10μ emission (as reported by Dr. Wynn-Williams at this meeting) which is almost certainly an ionization front due primarily to θ^1 Ori. As we have heard previously, ionization fronts could well trigger processes of cloud fragmentation. In fact, the two knots closest to θ^2 Ori do appear to have interconnecting links of emission, leading back to this ionization front, which we also observe to have highly supersonic motions. Clearly a full understanding of the objects will have to await further observations. [.....]

LORTET: As far as I remember, these structures are well seen on the [S II] and Hα + [N II] photographs in the Monochromatic Atlas of the Orion Nebula by Wurm and Rosino. Thus they very probably are the ionized skins of neutral globules.

HABING: If the objects you describe have been expelled by θ^2 Ori at the same time, why is the one with the highest velocity the nearest to the star?

TAYLOR: If you assume that all three knots were ejected simultaneously from θ^2 Ori at the same velocity then the objects having the smallest angular distance to θ^2 Ori will have the largest radial velocity vector, as is observed. I should emphasise however that this anticorrelation may well be a chance occurrence. These condensations may have much more to do with the ionization front than with θ^2 Ori, although, as I have said, they do appear to be, in part, ionized by θ^2 Ori, since in all three cases the brightest edge is facing towards the star.

HABING: You measure the radial velocity of the ionized gas. If the main object is neutral, what do you know then about its velocity?

TAYLOR: Nothing. However, I should strongly emphasize that we observe the ionized emission integrated over the entire extent of each knot and the results show that the turbulent broadening of the high velocity component is almost identical to that of the emission from the background nebula. This result is difficult to reconcile with the idea that a neutral globule is at rest with respect to the nebula, while the ionized emission from its interaction with the H II region shows the supersonic velocity. One would have to suppose that the ionized gas streaming away from the neutral globule was both uni-directional and also displayed a turbulence one order of magnitude smaller than its streaming velocity. On the other hand, if we regard these objects as totally ionized, with dispersal times of the order of 100 years or less, we still have to explain why the turbulent motions within them are so small in comparison to their velocity with respect to the ambient gas.
So it would appear to me that the most probable explanation for our observations is that we are seeing high velocity neutral globules surrounded by an ionization front producing the high velocity emission lines observed.

HERBST: I would like to report some results of an optical investigation of the R association Mon R2 by René Racine and myself. This work will appear in the Astronomical Journal. There are four general points which I would like to make here.
Mon R2 is an example of a star-formation region which is intermediate between an OB association and some T associations in the sense that the mass of its earliest-type star (B1) is between that of OB and T associations. This emphasizes an observational fact which was pointed out earlier in this symposium, namely that there is a range of star formation regions which may be characterized by the spectral type of their most massive members. These may be listed as follows:

Association	Example	Earliest SpT
OB	Orion	O
R	Mon R2	B
T	Chamaeleon	A
T	Taurus	F

Of course T and R associations may also be found in OB associations. It is, I believe, an open question as to whether the above sequence is in any sense an evolutionary one.
The study of associations which have not (yet?) given rise to O-type stars is important because these clouds will be more simply related to their initial structures than the OB associations. As Elmegreen has pointed out, one may quickly lose all information on initial conditions when their sequential sub-group formation mechanism turns on. Mon R2, on the other hand, may be an ideal case for testing fragmentation theory. In this regard, let me point out that there are two pieces of evidence for fragmentation in Mon R2. First, there are stars! Second, there are sub-groups of stars, containing up to 8 members and having a characteristic dimension of 2 to 3 pc. The subgroups are aligned along an east-west chain which stretches $\sim$20 pc and is roughly perpendicular to the galactic plane.
The color-magnitude diagram of stars in Mon R2 is surprising in that it shows no "turn-up" of the main sequence such as observed in all the young clusters studied by Walker. This implies that at least some stars in Mon R2 are older than those in NGC 2264 (i.e. $\sim$6 million years). However, there are also regions of Mon R2 (the "core" molecular cloud discussed earlier by Loren) where star formation must have occurred more recently, and is, perhaps, still proceeding.

P.M. WILLIAMS: I would support the emphasis by Dr. Herbst on the R associations for learning about star formation by reporting infrared observations of stars in two other R associations, CMa RI and Vel R2. A number of stars have infrared excesses characteristic of emission from circumstellar matter, strengthening the conclusion that these are regions of recent star formation. If the B stars with infrared excesses in CMa RI have the same masses as the B stars in the OB associations, they may well be much younger, supporting Herbst's suggestion of a second bout of star formation in this region.