DISCUSSION FOLLOWING REVIEW BY R.B. LARSON

SPITZER: In your three-dimensional collapse computations, what behaviour was shown in the direction parallel to the axis of rotation, and perpendicular to the projection plane used for the illustrations which you showed?

LARSON: This depends on the assumed temperature. If the cloud is initially nearly supported by thermal pressure, it retains overall a nearly spherical shape, except near the accreting core where it becomes relatively flattened and disc-like. As the cloud temperature is reduced, the cloud collapses by a larger factor parallel to the rotation axis and the system ends up with a more flattened shape. In the lowest temperature case which I tried, in which the initial ratio of thermal to gravitational energy was only 0.04, the resulting system of dense condensations and residual gas crudely forms a flattened pancake with thickness about one quarter of its diameter.

DE JONG: In the sequence of 3-dimensional models that you showed as a function of temperature I wonder whether the fraction of mass that goes into what you call stars depends in some way on temperature?

LARSON: This work is only preliminary and I haven't yet investigated such questions in detail. Based on a few cases, I have the impression that the total mass in all of the condensed objects does not vary greatly but may increase somewhat with decreasing temperature.

LYNDEN-BELL: Does your 3-D scheme accurately conserve angular momentum?

LARSON: It approximately conserves angular momentum; the results are only preliminary and I haven't yet checked how accurately angular momentum is conserved. I doubt whether the results are very sensitive to accurate global conservation of angular momentum, since they seem to be dominated by internal viscous transfer of angular momentum.

AMES: If you start with a cloud having the angular momentum corresponding to galactic differential rotation what periods do you obtain for the binary condensations in your 3-D calculations?

LARSON: I believe that Bodenheimer has made some estimates based on varying assumptions and will report these later. A problem is that such predictions depend sensitively on what one assumes about fragmentation and on what portion of a rotating cloud is allocated to a binary system.

TARTER: The three-dimensional calculations show rather spectacular fragmentation into two or perhaps three gravitationally bound clumps. To what degree is the fragmentation scale dependent on the somewhat limited nature of the numerical calculation, i.e. roughly 100 particles? For example, is the number of such fragments correlated with the number of "particles" in the calculation?

LARSON: For fragmentation into a relatively small number of large condensations containing many particles, the result does not seem to depend on the total number of particles used in the simulation, at least insofar as I have been able to check this with simulations using 50 and 100 particles. In the case of fragmentation into many small condensations containing only a few particles, it is more difficult to be sure that the results are not affected by the small number of particles.

WOODWARD: We are probably all familiar with N-body calculations for clusters of 100 stars, and there seems good reason to believe that the computed evolution represents fairly well what 100 stars would actually do. However, you are modelling a continuous system with what must resemble a particle-in-cell approach. Therefore you must have implicit in the calculation some underlying computational grid, which may nevertheless be free to move. Particle-in-cell calculations demand at least 2 particles per zone in order to give a meaningful result. This indicates that your underlying grid would contain at most 50 zones, or a zoning of 4 x 4 x 3. What then can the calculation on so coarse a grid mean?

LARSON: My calculation uses no grid of any type; gravity forces are calculated directly for all pairs of particles, and pressure and "artificial viscosity" forces are calculated between neighbouring pairs of particles. The method may be regarded as a type of Lagrangian scheme which instead of a grid uses a number of representative point particles which move with the fluid. I agree that a simulation with only 100 particles is very crude, and one can only gain confidence in the results by experimenting with different numbers of particles and by making comparisons with the simple hydrodynamic problems having known solutions. I have made two such checks in preliminary fashion for the cases of a massless accretion disk and for a non-rotating isothermal collapse. In these cases the results of the finite particle scheme seem to roughly agree with analytic or other numerical solutions.

DEKKER: I am always very much impressed by the pains taken by observers to indicate the limitations of their instruments. I would appreciate a similar attitude from those who perform numerical experiments. And thus inform us for instance which scales are suppressed and what is smoothed out by viscosity in their calculations. Is it not possible to provide the information about the limitations of the calculations in a more or less systematic way?

LARSON: You have raised a good question, but it is difficult to give a good answer because numerical hydrodynamics, especially multi-dimensional, is still more of an art than a science. One can only gain confidence in the correctness of the results by extensive experimentation and experience. The ultimate test of the validity of numerical results is provided by comparing the results of different investigators using quite different techniques; those results which are reproducible between different investigators can be considered as fairly reliably established. In my review I attempted to emphasize those qualitative features which I consider to be most general and most convincing results of numerical calculations. I also mentioned some respects in which there is disagreement between different calculations, and these disagreements will be discussed at more length later and you can make your own assessment. In most cases the reasons for disagreement are not yet completely understood; therefore it is difficult to specify systematically the limitations of any particular calculation, since these limitations are just not sufficiently well understood.

TEMESVARY: Ever since we obtained the first numerical results of a stellar evolution model (20 years ago) we realized that now we are in the same position as the observer. Although we know the physics we have put into the programmes we look with utter astonishment on the results that we ourselves cannot understand in all detail. It would be therefore quite justified - and I advocate - that we put error bars on our results representing the uncertainties of input parameters and numerical methods used. Reading a numerical paper, as reading an observational paper, requires some expertise to evaluate the art and mastery of the author. Here the art of astronomy approaches the art of gastronomy and becomes a matter of confidence and appreciation. I can assure you, what I have read so far by Dr. Larson has pleased my tongue.

MESTEL: I am not clear whether the fact that you do not find hierarchical fragmentation is in conflict with simple analytical arguments. If your rotating systems are not collapsing or cooling, it is not clear that the Jeans mass is decreasing.

LARSON: The results seem to be quite consistent with simple analytical arguments. In cases where the cloud as a whole does not contract much, being supported by a combination of rotation and pressure, fragmentation occurs on a scale governed by the initial Jeans mass. In some cases with smaller pressure and/or rotation such that an initial phase of roughly uniform contraction of the cloud is possible, the scale of fragmentation is governed by the Jeans mass at the end of this initial phase of roughly uniform collapse when the cloud begins to develop a markedly non-uniform structure.

McNALLY: How did you treat pressure in your three-dimensional calculation?

LARSON: Pressure is simulated by a repulsive force which acts only between particles and their nearest neighbours. Dimensionally the magnitude of this force, as can be shown in various ways, is given by the square of the sound speed divided by the distance between the particles in an interacting pair. At present I have not attempted to go beyond this simple dimensional expression with a constant of proportionality equal to unity.

STEIN: With all the talk of fragmentation, I would like to remind people that gravity can also produce coalescence. As an example of this coalescence process, Hohl has produced a movie of a computer simulation of gravitational instability in a thin sheet. He found that clumps of the Jean's mass formed in a free-fall time and then these clumps coalesced into larger clumps in another free-fall time.

P.M. WILLIAMS: What fraction of the initial energy is dissipated by the viscosity and does this have an observable effect on the properties of the cloud.

LARSON: I don't have a quantitative answer, but it is clear that dissipation is essential for the formation of dense condensations, and one can get an idea of the amount of dissipation from the fact that something like half of the mass goes into dense condensations.

BOK: During the next few years, many optical astronomers will make studies of isolated dark nebulae in blue light, in the red and gradually farther out in the infrared. We shall be working in close collaboration with molecular radioastronomers. All of us together want to give you the initial conditions for your model building. What specific parameters would you wish us to provide? I presume that you will want to have information on any observable rotational effects, even though these may yet be small. I presume also that you will be asking for high-resolution surveys indicating clumpiness and the presence (or absence) of a core. And finally you will probably be asking us to provide you with density gradients in the objects. Please let us know what precisely you want.

LARSON: All of the types of information which you mention would be quite valuable. High-resolution information on the density distribution will be important to test model calculations for the early stages of collapse and to provide information on how fragmentation takes place. Observational data on the properties of individual protostellar condensations, if such can be identified, would eliminate the guesswork presently involved in choosing the initial conditions for protostar calculations. Data on the rotation and internal motions of dark clouds would also be very important because the results of collapse calculations are quite sensitive to the initial rotational velocity.

I.P. WILLIAMS: If there is a thick blanket of dust surrounding a star, there will be "back heating". If the blanket is very thick, this may considerably lengthen the life-time of a protostar.

LARSON: Backwarming will occur to some extent, although I would be surprised if it could lengthen the lifetime of a protostar by a large factor.

BODENHEIMER: The theoretical calculations of collapsing spherical protostars that are assumed to start at the Jeans limit, predict that in the mass range of a few $M_\odot$ the stars do not "emerge" from their collapsing envelopes and become visible as normal pre-main-sequence hydrostatic objects until their apparent ages, as measured along standard equilibrium tracks, are in the order of 3×10^6 years. However the observational evidence that Steve Strom presented yesterday, shows that a fairly large number of observed objects (T Tauri stars, FU Orionis stars, Herbig emission objects) have apparent ages, measured in the same manner, that are much shorter (10^5-10^6 years). There appears to be a discrepancy.
As regards the question of the occurrence or non-occurrence of a ring in two-dimensional axisymmetric collapse calculations, this apparently does not depend on the initially assumed distribution of angular momentum. I have made calculations with several distributions and a ring appeared in each case, although the properties of the ring varied. Ring formation is not affected by the <u>initially assumed</u> angular momentum distribution, but rather by the degree to which angular momentum is <u>transported</u> outwards during the collapse.

MICHEL: For the star collapse models presented here, similar starting conditions have been used as for galaxy formation. Still, the results seem to be quite different. What is the difference in the physics?

LARSON: The main qualitative difference is that in the galaxy models one has stars which do not collide or dissipate energy; therefore a stellar system does not develop the very dense condensations which are characteristically obtained in a dissipative gas.

McNALLY: Calculation of the collapse of interstellar gas clouds has produced a divergence of result. Despite initial agreement on the cause of the collapse of axi-symmetric gas clouds, there is disagreement about the later evolution. Some workers (Black and Bodenheimer, Larson, Nakazawa et al., Tscharnuter, for references see Larson's review) find that ring structures develop while we have found that collapse proceeds in such a way that the density maximum remains at the centre of the cloud.
It is of great importance to understand why some workers find ring structures and others do not, since ring structures could hold a key to the resolution of the problem of how protostars cope with excess angular momentum. In a complex non-linear numerical problem it is not immediately obvious that the ring structures are a consequence of difference in (a) physical state, (b) heating/cooling mechanisms, (c) dynamical constraints, (d) transport of radiation, (e) initial conditions, (f) boundary conditions, (g) numerical procedures.

To investigate the problem I reviewed the published work of all groups known to me to find a common factor. Because of the broad initial agreement about the form of collapse it was probable that differences (a)-(e) only played a minor role. Differences in boundary conditions and/or numerical procedures remain.

It became apparent that those groups who find ring structures all shared a similar boundary condition or conditions whereas the group that did not find ring structures had a quite distinct boundary condition. Ring structures develop if collapse is assumed to occur for either constant volume or constant boundary pressure. Rings did not form if at the boundary the density and the temperature (and thus the pressure) was assumed to be zero.

If a constant volume or constant boundary pressure is assumed, a rarefaction wave propagates into the collapsing cloud. The effects of such a wave are most clearly evident in the collapse of those clouds which are initially of uniform density and isothermal. The collapse of an initially uniform, isothermal cloud can be described in terms of linear wave flow, the cloud remaining uniform (but with increasing density) and isothermal. The boundary conditions may be a trifle artificial but a perfectly well defined algebraic problem can be established.

In the numerical case, the discontinuities in pressure, density and temperature which occur in the algebraic model, are replaced by a situation in which the boundary pressure is maintained at the initial value or a constant volume assumption is made. A rarefaction wave is then allowed to propagate from the boundary so that the density distribution within the cloud becomes non-uniform. In the case of spherically symmetrical collapse the locus of these points reached by the rarefaction wave is the surface of a sphere.

To investigate what happens in the case of axial symmetry, J. Settle of our group looked at the shape of the surface defined by the advance of a rarefaction wave through a collapsing, axi-symmetrical gas cloud. The cloud was assumed to be initially of uniform density, isothermal, rotating and spherical in shape <u>or</u> non-rotating and spheroidal in shape.

At first the locus retained a quasi-spheroidal shape. The locus became increasingly flattened as collapse proceeded and ultimately a cusp formed in the equatorial plane. This event was unsuspected.

The conditions under which the cusp formed mimic the conditions under which rings form. We have compared the formation of the cusp with the formation of rings as described by Nakazawa <u>et. al</u> as this group gives the most complete information on the starting conditions of the ring structure. [.....]. We suggest that the interaction of the cusp with the collapse calculation plays some role in triggering the situation which leads to ring formation.[....].

TSCHARNUTER: There are two notions of a "ring" structure:

(1) Rings which are gravitationally unstable. This type has been described by Larson, Black & Bodenheimer and the Japanese group.

(2) Rings which are essentially a pressure or density wave. They may or may not become gravitationally unstable depending on how angular

momentum is transported either by numerical or physical processes. In my calculations I found both types, however, the discussion of the numerical errors has not yet been finished. Further investigations are necessary to find out which type is of physical significance.

BODENHEIMER: Tscharnuter and I have made 2-D collapse calculations with the same boundary condition. In the case where his difference representation of the angular momentum equation is similar to mine (but not exactly equivalent) he has obtained a ring with the same general characteristics as those obtained by Larson, Black and Bodenheimer, and Nakazawa, Hayashi and Takahara. (Density maximum and potential minimum away from the centre, central density decreasing with time). With an alternative difference representation for the same equation, he does not obtain a "standard" ring but rather one of the "pressure" type that he has just mentioned. The boundary condition does not appear to play a significant role in this problem.

I would like to discuss two more questions. First, are the axisymmetric rings found in two dimensional hydrodynamical calculations stable to non-axisymmetric perturbations? Recent three-dimensional calculations performed at Livermore by Wilson and Norman, starting from an equilibrium isothermal ring calculated by Black and Bodenheimer provide some information on this question. A fluid hydrodynamic calculation with self-gravity but no energy transport was performed on a 20 x 20 x 10 grid. The results show that if a slight ellipsoidal perturbation is initially introduced, the ring starts to break up into two distinct subcondensations on a time scale that is less than one rotation period. If no initial perturbation is introduced (other than numerical) four condensations result.

The second question concerns the possible consequence of the instability. Does the successive fragmentation of an interstellar cloud rotating initially with galactic angular velocity result in main-sequence stars in the observed mass range that are rotating slowly enough to be stable, and does it result in binaries with observed orbital properties? Mouschovias this morning indicated that the answer to this question is "No". I would like to argue that the answer is indeed "Yes".

Suppose we take the point of view that the most likely end result of ring formation is the production of a binary system. In a semi-quantitative way, based in part on simple assumptions and in part on the detailed 2-D calculations, we can estimate the evolution of an interstellar cloud, starting with 10^4 solar masses, T = 75 K, $\rho \simeq 10^{-23}$g cm^{-3}, through a sequence of fragmentation stages. Once the ring has formed, it is assumed to break up into 2 fragments of equal mass with conservation of total mass and angular momentum of the ring. The radius of the fragment, as estimated from the 3-D calculations, is about half the distance from the ring centre to the rotation axis. The fragment is assumed, in most cases, to rotate in synchronism with its orbital motion. This assumption implies that dissipative processes, that are not specified, act during the condensation process to transfer angular momentum from spin to orbital motion. Further 3-D calculations will be required to

clarify these transport processes. The evolution of a succession of such fragments has been calculated, during the early low-density isothermal phases as well as during the later optically thick phases during which effects of heating, ionization and dissociation have been included. Fragmentation ceases when (1) the temperature is high enough so that hydrostatic equilibrium is possible and (2) the ratio of rotational to gravitational energy at the expected main sequence radius is less than the critical value of 0.25, above which point fission would occur in the equilibrium phase.
Treating the ratio of ring mass to cloud mass at each stage as a parameter, the results have been checked in the limiting cases of large (close to 1) and small ratios. The final fragment masses turned out to be about 300 and .01 $M_\odot$, respectively. A number of calculations have been made for intermediate cases. As a simple example, suppose that the ring fragments when its mass is 1/4 the cloud mass and that the fragments rotate in synchronism. The end result of this sequence is a close binary system of period 3.5 days and with component masses of 2.44 $M_\odot$, which itself is a component of a wide binary system with a period of 9.57 years. These orbital properties are not too different from those of the observed triple system κ Peg. Clearly the old idea of resolution of the angular momentum problem by successive fragmentation resulting in transfer of spin angular momentum to orbital angular momentum at each stage deserves a close theoretical look, with the aid of improved 3-D calculations.

WOODWARD: You mentioned a grid of 20 x 20 x 10 for the 3-D calculations performed by Wilson and Norman. Although your figures display an entire ring, the asymmetry of the problem would allow a computation of only one quarter of the ring. Were the grid points spread only over this quarter, or over more of the ring?

BODENHEIMER: The grid points in the azimuthal direction were spread out over half the ring (π radians). More recently the Livermore group has done a calculation over the full 2π radians.

MOUSCHOVIAS: Let me stick to the non-magnetic cases discussed because, once the magnetic field is introduced, many conclusions will change. I would like to submit that ring formation is a direct consequence of the imposed axial symmetry. If that is relaxed, I submit that you will end up with a cigar-shaped object.

LARSON: I agree that ring formation is most probably an artificial result of the imposition of axial symmetry, and will probably not occur in real clouds except perhaps in exceptionally symmetrical cases. Many calculations which consider non-axisymmetric effects, including those which I described, suggest strongly that the first thing to happen in a rotating system is the formation of a bar-like structure.

MOUSCHOVIAS: I presume that we both used Newton's second law in estimating the period of a binary "star", that results from the collapse of a blob rotating with the galactic angular velocity and having a typical interstellar density (15 cm^{-3}). Yet, the periods we get differ by 7 orders of magnitude! Did you put in some new physics? Or, did you consider only a portion of the total mass as going into a binary system? Because your angular momentum has to go somewhere - the binary stars you form carry a very small fraction of the total angular momentum.

BODENHEIMER: The physical difference between your period estimate and mine is simply that you considered only one fragmentation stage starting from an interstellar cloud rotating with galactic angular velocity. I considered a sequence of fragmentation stages with transfer of angular momentum from spin to orbit at each stage. The first fragmentation takes place at a distance from the centre given by the distance of the first ring. Subsequent fragmentation stages occur at roughly the same distance from the centre of the original cloud but on successively smaller scales.

TARTER: To reconcile the published differences between the results of Westbrook and Tarter and of Larson and of Appenzeller and Tscharnuter, we have begun running our computer code with much greater spatial and temporal resolution. Unfortunately, the results are not yet in, so I can't really comment beyond what is in the published literature. I would like to point out, however, that all these 1-dimensional calculations lead to density changes of many decades, and this makes the numerical computation very difficult to do properly (as compared with most 1-D problems which are quite easy). Consequently, a detailed resolution of the differences will require a rather precise analysis of all of the computed physical quantities.

APPENZELLER: The differences between Tarter and Westbrook on the one hand and Larson and Tscharnuter and myself on the other hand are in the sense which is to be expected from the differences in the initial conditions. However, whether the different initial conditions can explain the differences in the numerical results quantitatively can only be determined by test computations.

WINKLER: The discrepancies between the results of Larson, Appenzeller and Tscharnuter on the one side and of Westbrook and Tarter on the other for the final radius of a 1 $M_\odot$ protostar seem to arise from the following two approximations:
(1) Differing degrees of artificial viscosity one introduces in order to compute the shock-front numerically.
(2) Radiation transport, solved by the diffusion approximation on the one hand and by a flux-limited method on the other.
Why this affects the results is as follows. The increase of artificial viscosity decreases the temperature gradient in the vicinity of the shock-front. In the diffusion approximation the radiative flux is directly proportional to the temperature gradient. Thus, the radiative flux goes

down when the amount of artificial viscosity has been increased. If one is aware of the conservation of the total energy, the energy which is not radiated away must clearly show up in the internal energy of the protostar. Also, the flux-limited method seems to reduce the amount of energy that can be radiated away. Therefore it is not surprising that Westbrook and Tarter got a fairly large radius because they used the largest amount of artificial viscosity and the flux-limited diffusion approximation. Unfortunately, we cannot judge a priori which numerical procedure is closer to the truth.

In my thesis (University of Göttingen, July 1976) I calculated the first stages of the evolution of a 1 $M_\odot$ protostar. There, the shock-front is treated as a discontinuity. No artificial viscosity is used, which is certainly closer to reality. Furthermore the radiative flux is calculated exactly using the spherical transport equation even across the shock-front. My preliminary results indicate that the radius of the final protostar should be fairly small and comparable with the one Larson and Appenzeller and Tscharnuter obtained.

Short contributions

YORKE: Recently I have solved numerically the detailed radiation transfer problem in collapsing protostellar clouds. I used the density distribution of dust as calculated by Yorke and Krügel (1977, Astr. Ap. 54, 183) at various evolutionary stages of a 50 $M_\odot$ and a 150 $M_\odot$ cloud, starting from the time of initial gravitational collapse and continuing up to (but not including) the formation of a compact HII region. For the radiation transfer problem, the spectral and angular distribution of the radiation as well as the temperature of the dust grains had to be solved simultaneously. Basically, the spectral appearance of the 50 $M_\odot$ and 150 $M_\odot$ protostellar clouds (which produced main sequence stars of 17 $M_\odot$ and 36 $M_\odot$ respectively) evolved from an extended far-infrared source whose radiation peaked at about 100μ and which embedded a highly obscured warm spot to a double-peaked infrared spectrum. The far-IR radiation was still present but the hotter component, which appeared as a "point" source of temperature $T \simeq 1000K$, became increasingly visible as the evolution proceeded. Similar calculations for lower mass protostars are being conducted at present.

THOMPSON: From your results, at what wavelength should a survey for protostar candidates be made?

YORKE: At present I can discuss this question only for massive protostars in the process of evolving to an O star. The hydrodynamic calculations of Yorke and Krügel indicate that the evolving protostar is embedded in dusty gas in a double cocoon configuration. My calculations show that the dusty gas in the immediate vicinity of the central accreting core acts as a false photosphere (radius $\sim 10^{15}$ cm), converting all stellar photons into the near-infrared. In the outer regions of the cloud, where at about 10^{17} cm an outwardly moving shell with a density $n_{H_2} \simeq 10^7$ cm^{-3} develops, the dust is warmed to $T \simeq 100$ K by the near-infrared radiation. The near-infrared radiation, however, may not itself be clearly visible during the early evolutionary stages, because the extinction is great. Thus, the far-infrared radiation is probably more typical of massive protostars during all evolutionary phases. Nevertheless, a survey at about 8μ will reveal embedded protostars of a late stage when the central cores have reached their final mass. At 2μ these objects should also become increasingly more visible at this late stage.

GEZARI: Cocoon stars such as the one modeled here would be extremely difficult to detect at far-infrared wavelengths. They would almost certainly be located in large molecular dust clouds where a point-like source (as compared to the ∿30" spatial resolution at 100μ) of this luminosity would be practically indistinguishable from the strong extended background source. On the other hand, several unresolved cocoon stars could make a significant contribution to the 100μ flux from smaller extended dust clouds.

WHITWORTH: I have tabulated - for the contemporary gas, and for the primordial gas - mean, equilibrium values of the isothermal sound speed, $u = (kT/m)^{\frac{1}{2}}$ (where k is the Boltzmann constant, T the gas kinetic temperature, and m the mean gas particle mass) on the (n,N)-plane (where n is the space-density, and N the column-density, of Hydrogen nuclei in all forms). Combining this with the Jeans' criterion for instability against gravothermal contraction, $N^2 > 45nu^2/16\pi Gm_{eff}$ (where G is the universal gravitational constant and m_{eff} is the mass of material per Hydrogen nucleus) we obtain the most efficient possible path of fragmentation in the (n,N)-plane. This shows that protostars with mass $M \simeq M_{\odot}$ can condense out of the contemporary gas from a relatively low background density $n \lesssim 10^4$ cm^{-3}; but that much higher background densities were required for such protostars to have condensed out of the primordial gas. The above conclusions are supported by an evaluation of relaxation times to the equilibrium isothermal sound speeds; the relaxation times are always smaller than the corresponding free-fall collapse times. However, since the Jeans' criterion only says that contraction is energetically feasible on a gravothermal count, it is by no means certain that the gas destined to form a star follows the proposed path.

SILK: The mass fraction of heavy elements relative to Hydrogen has to be less than about 10^{-5} before star formation from the "primordial" gas at 10^4 K, as advocated by Hoyle, becomes important. Otherwise the gas can cool, fragmentation being dominated by grain cooling and opacities. Hence I believe that globular clusters which generally contain more than this threshold value of heavy elements, must have originated via a mode of star formation similar to that found in younger stellar systems.

APPENZELLER: Recent medium and high dispersion spectroscopic observations of the T Tau star S CrA give physical parameters which are in excellent agreement with the model computation described earlier in this session by R.B. Larson. In detail Dr. B. Wolf and myself found the following main properties of the spectrum: most spectral lines show an "YY Orionis" profile, i.e. emission with redward displaced absorption components. However, there are also pure emission and pure absorption lines present. The pure emission lines are undisplaced. With the exception of two very weak Ca I lines, all absorption lines are always red-shifted by 300 to 400 km s^{-1} indicating a continuous infall of matter. Although strong time variations of the strength and structure of the strong emission lines are observed, no variations of the absorption line red-shift could be detected. On high-dispersion (20 A/mm) spectrograms the Balmer lines usually show a complex structure which consists of a broad undisplaced emission component with a redward displaced absorption component and superposed a redward displaced emission component which shows strong time variations. The variations of these redward displaced emission components are apparently the cause of the temporary disappearance of the Balmer absorption components on low dispersion spectrograms. All observed properties are in good agreement with those predicted from the model computations for a 1.7 $M_\odot$ protostar. A detailed description of our results will be published in Astronomy and Astrophysics (1977, Astr.Ap. 54, 713).

COHEN: Kuhi and I, at Berkeley, are trying to determine the mass spectrum of protostellar fragmentation in different associations. That necessitates deciding whose theoretical tracks are relevant to the conversion of locations in the HR diagram to masses and ages. The program involves optical scanner spectra to determine T_{eff} and infrared photometry to determine L_{bol}. The spectra of some 400 stars have been obtained thus far. The first association to be studied is the rather small, faint Cepheus IV group. The lower mass stars occupy a domain of the HR diagram not crossed by Larson's tracks for dynamical evolution. It is possible to locate some of the higher mass stars near Larson tracks but only at such an early age that the stars would still be entirely shrouded in dense dusty cocoons and hence optically invisible. We conclude that it is necessary to fall back upon convective-radiative evolutionary tracks, such as those computed by Iben. Within this framework the youngest star in Cepheus IV is younger than 10^5 yrs but already reveals a well-defined photospheric spectrum, apparently not veiled by gas or dust. This may provide evidence for the existence of a stellar wind in T Tauri stars at a very early phase.

GLASSGOLD: Gerola and I have developed a one-dimensional hydrodynamic program for studying the evolution of interstellar clouds which includes thermal and chemical phenomena associated with interstellar molecules. One objective of this work is to understand the relationship between the various chemical, thermal, and dynamical time scales. A second objective is to analyze the effects of different boundary and initial conditions and of external perturbations on the overall evolution of a cloud. Another objective is to correlate observed properties of interstellar clouds with the predictions of the model calculations.

Earlier hydrodynamic studies of interstellar clouds generally ignored chemical changes. Schwarz, McCray, and Stein (1972, Ap.J. 175, 673; Ap.J. 177, L 125) and Mansfield (1973, Ap.J. 179, 815) did include the time-dependent ionization of H^+, and showed that thermal instability enhanced density perturbations in the intercloud medium. The chemical and thermal evolution of interstellar clouds offer even richer possibilities, especially if they contain molecules. In addition to their relevance to observed clouds, the hydrodynamic studies may also shed light on the initial conditions for protostellar evolution. [....].

At the instant the evolution is started, the assumed initial temperature is too large for the given density and external fluxes, and so the temperature rapidly relaxes at constant density to a mean value of about 50 K. The heating is inhomogenous, and the temperature is somewhat non-uniform, ranging from 45 K in the center to about 60 K at the surface. Nonetheless, the pressure gradients are small and the hydrodynamics is governed by gravity. The density and velocity distributions are qualitatively similar to earlier calculations, e.g., NcNally et al. (1969, MNRAS 146, 123). They show a uniform core (constant density and velocity gradient) with a non-uniform envelope. As time evolves, most of the mass of the cloud is in the envelope. The temperature decreases slowly at first, and toward $t \simeq 2 \times 10^{14}$s a substantial dip occurs in the core. From the dynamical point of view, the thermal properties of the cloud are such that the collapse is somewhat retarded in the envelope and somewhat accelerated in the core by pressure gradients.

The most novel aspect of these calculations is the inclusion of a chemical as well as a thermal model appropriate to intermediate densities. [...]. An interesting result of our calculations is that most of the Hydrogen becomes molecular in about 10^7 yr because of the high densities produced by the gravitational collapse. Another consequence of the almost complete conversion to molecular hydrogen at $\sim 2 \times 10^{14}$s, is the core temperature drop. [...].

The development of the CI distribution is consistent with the time scale for recombination, which is in the range 3-5 x 10^5 yr. Thus, the CI adjusts itself quickly to instantaneous values of the density and the optical depth (for the UV radiation which ionizes CI). Thus, the evolution of CI is strongly influenced by the gravitational collapse. Complete recombination in the core does not occur until about 7 x 10^6 yr. The development of CO is also slow, and sensitive to the high densities resulting from the dynamical evolution. Abundance levels > 10^{-5} are not achieved until after 5 x 10^6 yr. These time scales for CI and CO are consistent with earlier estimates by Langer and Glassgold (1976, Astr.Ap. 48, 395). [....].

The model calculations indicate that the evolution of important neutral species such as H_2, CI and CO can be strongly influenced by the dynamical evolution of an interstellar cloud. Calculations of column densities and emissivities are in progress, as are hydrodynamical studies of other clouds and physical situations.

DE JONG: In the calculation of the gas temperature in your model did you take into account the quenching of the cooling at great depths in the cloud, in particularly of CO, due to radiation trapping of the cooling protons?

GLASSGOLD: Yes, we did.

DE JONG: In this connection I would like to point out that the so-called "Chemical-Thermal" instability that you and Dr. Langer invented recently (1976, Ap.J. 204, 403) disappears if one takes into account the quenching of the CO cooling be radiation trapping.