SUMMARY OF THE CONFERENCE: OBSERVATIONS

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I have no plan of trying to sum up or to abstract the observational aspects of this conference. It would seem more profitable to discuss a few points that deserve special praise, or seem in need of critical attention, and to point to a few things I found puzzling and to tell you how I tried to explain them to myself.

Let me say at first, rather naively, how struck I am by the delicate symbiosis that exists between the stars and the interstellar medium, how each is nourished by the other, and how the Galaxy as we know it is entirely a consequence of that balance and interplay. It is interesting to speculate how, if one were able to tinker with just one parameter of this beautiful machinery, how the whole ecosystem (as it is now fashionable to call such things) might find a new balance point and become unrecognizable to us.

1. CRITICALLY NEEDED OBSERVATIONS

Too often in astronomy, issues seem to reach a stalemate, and are in effect decided by the exhaustion of one of the proponents rather than by critical experiment. We must do all we can to avoid this situation. Some examples where it seems to me that resolution of an important issue could turn on specially designed new observations:

(a) It appears quite reasonable that the compact H II regions do contain O-type stars, but the evidence is indirect, from inferred Ly_c fluxes and infrared (IR) luminosities. But it would be comforting to observe some O-type stellar line features: there might be some surprises in store. And, specifically, it seems incredible that the nature of the Becklin-Neugebauer star still remains open. Only IR line spectroscopy of adequate resolution can settle such questions.

(b) If collapse models like Yorke and Krügel's are to be tested, and since we cannot wait long enough to see what will happen to the objects in W3, somehow Doppler measurements must be made at high angu-

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lar resolution of these sources. It is of course easy to measure the emission lines of the surrounding H II region, but one needs the motion of the gas + dust mixture very near the object. Perhaps there is an outside chance that the radial velocity of the dust is measurable directly. We know that at shorter wavelengths there are a set of diffuse absorption lines that behave as if they are intrinsic to the interstellar dust; can there be some similar features, adequately sharp, in the infrared? Another speculative possibility is that of detecting the gas mixed with the dust in the inner shell of the Yorke-Krügel double-shell models. If there is a level where gas is thermalized by this dust at T ≈ 1000 K, one should be able to predict what kind of emission spectrum is to be expected in the infrared. Such a spectrum would be unmistakably different from that of the H II region. What appears to be such an emission spectrum has been detected at shorter wavelengths in the dusty M supergiant VY CMa (in ScO). Only if such hypothetical features can be found at longer wavelengths would Doppler measures seem possible. But, make no mistake, this will be a most difficult observation: at W3, the diameter of this region would be only a few tenths of an arc second.

(c) I want only to mention another area where some decisive observations are overdue: the question in T Tauri stars of outflow vs. outflow followed by return vs. infall. The considerations are complex, the spectroscopy is as yet somewhat ambiguous, and the proponents of the various views are both ingenious and firm in their convictions. There is no time for me to go through the pros and cons here, except to offer the opinion that aside from that minority species called YY Orionis stars, a convincing case has not yet been made for pure infall. At the most, both outflow and infall must be operating in conventional T Tauri stars, although possibly in different proportions in different stars. Whether the infall comes from far outside or is simply returning outflow, cannot yet be decided. I do believe that what is needed most, short of a fully convincing spectroscopic test, is a modern theory of outflow at a level of sophistication and detail that is competitive with the infall theories of Ulrich, of Appenzeller and Wolf, and of Bertout. I am sure none of these investigators will disagree if I say that the proponents of infall have been working harder and longer on their models than have the opposition.

(d) And a plea to the theorists: as Larson himself suggested here, it is surely time for a new generation of collapse models to replace those of Larson, vintage 1972. The proliferation of directly relevant observations, such as has been presented here by Cohen, urgently demands some firm predictions to confront!

2. INTERESTING NEW IDEAS

(a) A fascinating feature of Woodward's calculations was that under compression, a dense surface layer appeared in his idealized cloud, and presumably it is here that new stars would form, not in the deep interior as I had always thought before. I am not clear how, once formed, such stars would move but at first glance this offers the possibility of explaining a puzzling feature of T Tauri stars in nearby clouds: the lack of large numbers of very faint examples. I had supposed that the large numbers that should exist in the interior were simply obscured, but can it be because they are largely a surface phenomenon? Surely, it would be possible to decide whether real molecular clouds have such a surface crust: the modellists usually assume a $\rho(r) \propto r^{-n}$ (n positive) density law in representing observed column densities, but could they do equally well if a density peak existed at the surface?

(b) Elmegreen and Lada's ideas on the sequential formation of OB associations, like the ignition of a chain of firecrackers, are equally interesting. Everyone who has looked at the Palomar Sky Survey has come across examples of a star cluster sitting beside an H II region or a dark cloud, and I find their explanation very appealing. I wonder if some independent support for the idea might be found, in the case of the Orion association, from the departure times and points of origin of the three runaway OB stars that came out of that region at high speeds about 2.6×10^6 and 5×10^6 years ago. Whatever the phenomenon that ejected them (such as a Type II supernova event), it could well have marked the time and place of a new Elmegreen-Lada cycle.

3. MYSTERIES

(a) It seems that much is to be learned about the lifetimes of the dense molecular clouds. Are they simply compressed and then relaxed in passing through a galactic shock, retaining their identity and their contents in the process? If collapsing molecular clouds are common, as I have heard suggested here by Loren and others, then they have to be replaced somehow. Let me mention an observation that I find very difficult to understand, but that may contain a hint as to the history of individual clouds, not of the massive, dense molecular kind we have heard about here, but the diffuse clouds that produce the optical interstellar lines. It has long been known that there is from cloud to cloud a major spread in the Ti, Ca, Al/Na, K ratio, and the most plausible explanation is that the former have been taken up in the dust by the formation of highly refractory compounds like Al_2O_3, $Ca\,Ti\,O_3$ and $Ca_2Al_2Si\,O_7$. But the only circumstances in which this pattern of selective leaching-out takes place is by cooling the gas down from about 2000 K at high particle densities: up to 10^{15} cm^{-3} have been mentioned. The only known place for this to occur is in circumstellar shells around either pre- or post-main sequence stars. But how can one explain a major depletion of Ti and Ca throughout a whole interstellar cloud unless a major fraction of that material has been cycled through stars, or protostars? Not only is there the large question of whether stars ever form at all in such low-density clouds, how is there time between passages of these clouds through the galactic shock to process and mix all that material via star formation? It seems impossible.

One explanation is that these diffuse clouds in our neighborhood must be fragments split off more massive molecular clouds that have very long lifetimes, and have had long star-forming histories. The optical evidence is for a major spread in the degree of depletion, but these are tales told by children. Cannot the radio astronomers ask the parents directly: is there any way to determine the depletion of these critical heavy elements in the dense molecular clouds, and thus to see if this picture checks out?

(b) Surely a central question of this whole subject is: where did all that interstellar dust come from? There is a tendency to discuss the dust as though it was provided just to make H_2 and to shield the other molecules and then perform other useful services for star formation. There is no time to discuss all the suggestions that have been made as to whence it comes. These include at last count red giants and supergiants, carbon stars, planetary nebulae, novae, supernovae, supernova shells, and "solar nebulae." Feelings run high among the proponents of these various scenarios, and I shall not attempt an assessment of the claims. But I do want to call to your attention the advantage of manufacturing the dust near where it is seen, and near where its services are required: namely in the dense molecular clouds themselves. I do not see how this can be done in the gas by mildly compressing the clouds. Very high densities are required, at least for a short time, and as far as we know, this is possible only around protostars or very young stars. The basic idea is that the molecular clouds may begin with low dust opacities but as more stars are formed within, the byproduct dust makes the cloud increasingly more opaque, star formation proceeds at a more rapid pace, until I presume the cloud is restructured or rearranged by the formation of a sufficiently energetic O star or stars. I would urge that people working in this field keep this proposal in mind: that the dust is being made before our eyes around young stars. There must be evidence accessible to us that would settle this very fundamental issue one way or the other. It must not be allowed to become a stagnant stalemate of the kind mentioned earlier!

4. NOTED IN PASSING: CAUTIONS AND CONCERNS AND CURIOSITIES

I want to note a few miscellaneous matters that have arisen either implicitly or peripherally here, and on which brief remarks seem to be in order.

(a) On the concept of a molecular cloud as a smooth, well mixed medium: in a few places there are nearby bright, but not too hot stars which shine on the dust, and make its fine structure visible. Perhaps the most interesting is in the Taurus-Auriga dark nebulae, where one can see that those dark clouds have a fibrous structure as if painted with a coarse brush. These long slender (perhaps several 10^{-3} parsec wide) filaments form a single parallel system at 72 Tauri, but around the Pleiades cluster, some degrees away at the edge of the same cloud, there is a greater area illuminated, and more than one system of these fibers are apparent. How typical such structure is one doesn't know: some other nebulae seem more amorphous. Perhaps an infrared technique could be devised to detect such fine structure in any dust cloud, whether illuminated or not.

(b) I want to note that we should be prepared, when looking at molecular clouds for young objects, to encounter an occasional interloper. The statistics of ordinary stars in the solar neighborhood show that many such innocent objects must be passing through the clouds. I do not know what one would see at a deeply imbedded white dwarf accreting H-rich interstellar material, but the possibility of encountering such infrared herrings among the young objects in the clouds should be kept in mind.

(c) There has been some mention here of the FU Orionis stars (or Fuors), as well as of the 1720 MHz OH maser line that one of them exhibited temporarily. These are faint, otherwise inconspicuous stars, located in molecular clouds, which have been observed to rise in brightness by a large factor, and to remain bright for a considerable time. There are only 3 examples known, but since the phenomenon is very rare and no one knows when another example will turn up, one is emboldened to risk some generalizations on the basis of this tiny sample. It appears to me that this spectacular flareup, which apparently is a phenomenon limited to T Tauri stars, does not represent a permanent transformation of the star: I suspect that after decades or perhaps centuries, the object probably returns to whence it came and resumes the appearance of a T Tauri star. I estimate that, averaged over the whole T Tauri population, this instability can strike a given star perhaps every 10^4 years, and that it can recur. In fact, if the sun in its youth underwent such a flareup, it would have remelted most of the dust in the inner solar nebula, and thus provided a natural explanation for the chondrules found in stony meteorites, which clearly represent some dust that was remelted long after most of the solar meteoritic solids had condensed from the circumstellar gas.

But the organizers of this Colloquium have, quite properly, excluded meteoritical matters from discussion. I only want to note that it would be amusing if the answers to some problems of the T Tauri stars literally lie not far from here, on museum shelves.

5. SCENARIOS

I think that Mezger's suggested scenario for the OB stars is a very reasonable one, and accounts for the essential facts as they are known today. But these are all massive objects, and I want to say a little about low-mass objects in the same spirit. Let me point out, as Herbst did here, that between the T Tauri domain — perhaps up to 2 or 3 solar masses — and Mezger's there is a serious lack of information on the early history of stars of intermediate mass. These objects in time presumably become the Be/Ae objects with IR excesses that Strom has studied, but I think nothing whatever is known about their early development as IR or radio sources. Possibly LkHα-101, discussed here by Thompson, is such a creature. Possibly so little is known because there is as yet no efficient way of searching for them. This is an area well worth someone's attention.

Since there was some discussion at the time of Mezger's review of the order in which stars form, let me explain the evidence on this matter. No one of course claims that, all other things being equal, low-mass stars must form first. In the real sky, for some reason things are not equal. The proposal is that the formation of the first massive OB star in a molecular cloud is controlled by local circumstances, and there may be a long wait before somewhere in that volume, conditions are exactly right for such an event to take place. But in the meantime, smaller-mass stars are apparently able to form more easily, and since most of these (one suspects) cannot escape, the cloud slowly fills up with such stars having a small velocity dispersion, characteristic of the neutral cloud. We see exactly this situation in Taurus-Auriga today.

But what happens if a massive star finally forms near the surface of the cloud? (Mouschovias has pointed out the difficulty of disrupting the whole cloud from the inside in this way.) We must then see something like the Orion Nebula today: a hot ionized pocket in the front of the cloud, the ionized gas having a high velocity dispersion, and around the pocket and in the cold cloud beyond, a large number of low-mass stars having a low velocity dispersion. These objects must have been there for a long time; their velocities reflect that of the cold gas that preceded the appearance of θ^1 and θ^2 Orionis. And probably star formation at low masses is then at an end in the H II region. So here is a system of older low-mass stars mixed with younger massive ones.

Examples can also be found where the massive stars formed early; a good example is the cluster NGC 2362, in which there seem to be no members less massive than about middle B type. There are many examples of clusters lying between these extremes. Lest anyone think that these statements on the heterogeneity of the mass functions of clusters are new, I must point out that the basic ideas were put forward by Walter Baade in his Harvard lectures of 1958.

One wonders what will be the ultimate fate of massive molecular complexes like Taurus-Auriga: perhaps piecewise disruption over a long period of time by the formation of successive OB stars. Those clouds are thus a promising hunting-ground for traces of such events in their early stages, as well as for very young intermediate-mass objects. A very few deeply imbedded IR stars are already known there, such as Allen's source near IC 2087. But to reiterate, we shall learn little from such objects until their intrinsic line spectra can be observed, as distinct from the extinction law of the foreground dust.

Lynden-Bell raised here the question of what can be said about the origin of stars in the very low-mass domain, less than about 1 solar mass. Of course, the only reason we know that low-luminosity stars exist at all is through the sample very near the sun. There has long been the suspicion that the dMe stars in that sample represent the younger fraction, which on account of the very long Kelvin times at low masses, have had the time to leave their parent interstellar clouds and reach our vicinity and still show evidence of their youth through chromospheric activity and flaring. There are some difficulties with this idea, however, notably the existence of some high-velocity flare stars. As far as I know, the only direct evidence that low-mass stars form in interstellar clouds as do their more massive counterparts is the presence of a large population of very faint "flash" variables in the Orion Nebula, and the existence of a smaller group around NGC 2264. The faintest of these former have $M_{pg} \approx +13$ or fainter (uncorrected for interstellar extinction), but membership in the Orion association is only statistically demonstrable for the objects as a group, and so one cannot in most individual cases rule out the possibility of a field star. A similar "haze" of very faint flash variables surrounds the Pleiades. One presumes that the great number of M dwarfs in the field represents the residuum of all such clusters and associations that have dissolved since the beginning of star formation in the Galaxy, plus a component originating in minor molecular clouds that were never able to generate much else. But this is the purest speculation.

6. FINALLY...

On 'Star Formation' as a subject, it would seem almost a hopeless task to reconstruct the whole motion picture from our one snapshot. The subject suffers from such a mish-mash of soft information, of material that no one can say is relevant or not, of theoreticians looking hopefully at observers for support only to find the observers looking wistfully back.

One day I heard Schatzman ask a speaker: "Yes, but what has that to do with star formation?" And I heard Thaddeus say that despite the fact that his molecular clouds were the place for stars to form if anywhere, he saw no signs that stars are forming there now. I have the feeling that if a lawyer had sat here among us this week, he would probably have shaken his head and said: "Gentlemen, this is all terribly interesting but,by the rules of evidence, you have no case."

The only way we can become convincing is to be hard-nosed, to be extremely critical, to pursue those tests that will decide between one alternative or the other. And one must not feel injured if his favorite idea is demolished. After all, when the historians of science look back on our times with the perspective of the years, all that we do today will certainly be seen to have been either wrong, or irrelevant, or obvious.