DISCUSSIONS

Part I

(Lutz)

L. H. Aller: Did you examine the effects of changing the mass of the nebular shell upon the distance estimate? When Minkowski and I applied the basic principle of what is commonly called the Shklovsky method, we felt we could only give limits on the nebular distance because we thought the mass might lie anywhere between 0.05 and 0.5 solar masses. Shklovsky took the mass of the shell as 0.2 solar masses for all planetaries. Me may be right; nobody knows. The error in distance is relatively insensitive to that in mass but is *not* negligible in your treatment and may affect results as much as by changing temperature.

J. H. Lutz: If the mass chosen were too large by a factor of five the derived value of R would be too large by roughly 2.0.

Part II

(Bromage and Nandy)

Anon.: It has been shown by Williams and McIntyre that the λ4430 band could be caused by Ca°-atoms in grains, if the atoms can be inserted into holes corresponding to about the size of Ca^{++}-atoms, which is the form of Ca in interstellar space. I would like to know if there is data on the relation between the depletion of Ca and the strength of the λ4430 band?

Do you have any data on the possible anticorrelation between the strength of the λ4430 feature and the excitation of the H II regions that you have looked at?

J. M. Greenberg: An interesting anti-correlation of the sort you seem to be implying was found indirectly a number of years ago. Stoeckly and Dressler (*Astrophys. J.* **139**, 240, 1964) showed that the λ4430 feature in high velocity clouds was weakened. Lichtenstein and I (*Astrophys. J.* **68**, 74, 1963) attempted to connect this with the fact that in such cases the Ca to Na abundance ratio approached 'normal' values (Routly, P. and Spitzer, L., Jr.: 1952, *Astrophys. J.* **115**, 227).

We assumed that the λ 4430 was due to Ca imbedded in the grains and that high velocity clouds implied H II regions which evaporated the mantles of the grains. Implicit in this argument was the assumption that Ca atoms accreted more really than Na on to the grains.

W. W. Duley: I believe that McIntyre and Williams calculate that Ca atoms are not likely to give a band at 4430 Å when trapped in hydrocarbon matrices. Their calculation is in error as we see a band due to the Ca 4226 Å line at 4430 Å in solid C_6H_6.

T. Snow: Is it possible that the reddening and diffuse bands which you and Bromage have correlated in the Cygnus OB association are not necessarily circumstellar, but perhaps intracluster instead? That is, could they be occurring in dust associated with the star group as a whole, rather than dust clouds surrounding each individual star?

K. Nandy: The reddening is definitely local, but we can't say for sure whether that means close circumstellar clouds, or something less closely attached to each star.

J. M. Greenberg: In a paper Dr Wang and I gave at the Liège Symposium last year (Greenberg, J. M. and Wang, R. T.: 1972, *Mem. Soc. Roy. Sci. Liège*, 6ème, tom **III**, pp. 197–207). It was shown that the strength of an absorption band is amplified relative to the extinction if the dust is truly circumstellar. This, of course, assumes that the circumstellar dust is the same as the interstellar variety.

If, on the other hand, the λ4430 is contained in mantles we expect the band to be depressed if the dust in the circumstellar region consists only of the cores on which mantles form.

P. J. Treanor: You mentioned that the reddening of No. 10 of the 6 Cygnus association seems to be too small. I have also recently had some doubts about the published data on this star. A preliminary polarization measure in the red region suggested a value a good deal higher than that published for the blue. If wondered if there could be some identification error. It is quite a bright star – about $m = 9.8$.

Greenberg and Van de Hulst (eds.), Interstellar Dust and Related Topics, 575–584.

(Nandy and Seddon)

M. F. A'Hearn: I have narrow band filter measurements (in the current A. J.) of the polarization across the λ4430 band in several stars. Despite a wavelength resolution of only 20 Å, the high polarimetric accuracy places severe limits on the possible amplitude of a dispersion-like polarization curve as described by Greenberg and by Nandy – certainly no more than a few thousandths of a magnitude.

J. M. Greenberg: In view of the importance of polarization shape of the λ4430 band in helping to define the optical properties of grains I would like to propose that we should make more observations at better spectral resolution in order to be sure of the result.

(Hayes *et al.*)

R. K. Honeycutt: I have two comments on this interesting paper. First the knee in the reddening law at ~4300 Å appears to persist independently of the presence of the 'window' between 1.6 and 2.0 μ^{-1}. This conclusion is based on photoelectric scans of reddened early-type supergiants at 20 Å resolution taken at Indiana University which show the knee in the reddening curve of most stars even if one considers only the spectral region blueward of 2 μ^{-1} (that is, excluding the window).

Secondly, photoelectric scans of VI Cyg No. 12 (soon to be published by R. S. Chaldu, M. V. Penston, and myself) support the reality of the red window. The feature appears clearly in the reddening law of VI Cyg No. 12 in about the strength expected from its color excess of $E_{B-V} = 3.3$. The window looks essentially featureless at 40 Å resolution.

K. Nandy: Why use Whiteoak's data for only 28 stars, when others have published measurements on more stars at higher dispersion?

D. S. Hayes: Whiteoak's data were the first I found, and the only ones I know of yet, which have enough photometric accuracy and wide enough wavelength range. I haven't seriously tried looking for more data because Whiteoak's is adequate and this is only a preliminary analysis.

Anon. 1: In our spectrophotometry of Cyg IV No. 10, the feature you have discussed stands out quite strongly. But also, when we look at wavelengths shorter than 5000 Å, the knee at about 4300 Å is quite well defined.

D. S. Hayes: I am familiar with other attempts in the literature to locate a knee around 4300 Å while looking at a wavelength range of a couple of hundred Å on each side. In my opinion, although there may, in fact, be a knee at about this wavelength, it is not what most people mean by the 'knee', when looking at the entire interstellar extinction curve. Any 'knee' at 4300 Å must be much smaller, in the sense of having a smaller change in slope.

Anon. 2: You said 1% of the normal polarization is about the size of the polarization feature you will be looking for. Do you mean that if the polarization is 5%, that you are looking for a polarization difference of 0.05%?

D. S. Hayes: Yes. This is, as I recall, on the same order as the size of the polarization feature being searched for in the case of the 4430 Å band.

D. R. Huffman: Do you know of any material which has such a window in the absorption?

D. S. Hayes: No. Do you? Dr Greenberg and I have not yet really tried to ascribe this window to a particular material but have limited our thoughts to whether it is a possible phenomenon.

D. H. Harris: It may be that the feature you observe at $\lambda^{-1} = 1.8\ \mu m^{-1}$ is in reality the space between two weak broad absorptions.

S. J. Shawl: What particle size and size distribution junction were used in your calculations?

D. S. Hayes: Perfectly aligned cylinders of radius a distributed according to a Greenberg distribution, $n(a) = e^{-5(a/a_0)^3}$: $a_0 = 0.225\ \mu$.

(General)

S. van den Bergh: Could you give the arguments for and against the assumption (to M. Greenberg) that the λ4430 feature is related to the band at λ2200.

J. M. Greenberg: There is, to my knowledge, no *theoretical* argument which supports this hypothesis. I believe that a paper by Sneddon discusses the observational correlation. The λ2200 feature has been variously attributed to graphite (carbon) and to silicates, but I do not recall that either of these substances has been directly used to produce the λ4430 band.

D. H. Harris: Small quantities of impurities in graphite markedly change the strength of the 0.22 μm feature. Optical constants for less pure carbon can be found in the literature on coals.

A. N. Witt: In answer to the question whether λ 4430 is produced by scattering. I have examined

https://doi.org/10.1017/S0074180900054759 Published online by Cambridge University Press

spectra of various reflection nebulae in this respect. When compared with the spectra of the illuminating stars, no evidence for excess light at λ 4430 can be found in reflection nebulae. The diffuse galactic light is unfortunately much too faint to obtain sufficient spectral resolution to answer the question as to whether there is λ 4430 excess in the DGL.

Part III

(Zellner)

A. N. Witt: Do you consider that the lower polarization in the UV is due to the breakdown of the single scattering assumption, which would occur first in the UV?

B. H. Zellner: Yes. It is also true that in the UV we see to a smaller geometrical depth and hence observe a smaller range of scattering angles. It does not seem advisable, however, to invoke 2nd order mechanisms in order to explain phenomena which are already nicely reproduced by single scattering Mie calculations.

(General)

W. W. Duley: If the observations of UV surface brightness of reflection nebulae require that dielectric grains are present and that these grains are transparant to extremely short wavelengths $\lambda^1 \rightarrow 10\ \mu^{-1}$ then the only solid material that could satisfy this requirement is solid neon which is transparent to $\lambda \simeq 900$ Å.

Part IV

(Serkowski)

Th. Schmidt: It seems that the helical interstellar magnetic field model of Mathewson and Ford can no longer be taken as the only possibility. Among other things there seems to be no real indication from the most recent faraday and pulsar mesurements for a regular magnetic field inversion between northern and southern galactic longitudes, as would be necessary for the helical field.

(Treanor)

K. Serkowski: The superiority of the ring method, as compared to taking photographs through a pair of calcite plates, is not clear to me. Are not the photographic adjacency effects affecting the results obtained with the ring method? Maybe the best solution would be to use a pair of calcite plates and a rotating tilted glass plate in front of photographic plate. This tilted plate would increase the size of Schmidt stellar images slightly. In this way the advantages of both methods may be combined.

P. J. Treanor: The question of the relative merits of the ring and calcite polarimeter is a complicated one. Undoubtedly, one has to take percautions to minimize adacency effects in the ring, particularly in relation to the calibration step I have introduced (I explain this in the full text). Also there is some loss of limiting magnitude. However, the rings present a complete analysis of the polarization and the fact that the rings extend over many grains should increase this information content in comparison with methods which rely on single or multiple star images.

(Martin *et al.*)

T. Gehrels: The circular polarization of VY Canis Majoris (T. Gehrels, *Astrophys. J.* **173**, L23–L25, 1972) has the wavelength dependence as you described it, namely, with a rise from about 0.6 μm, maximum near 1 μm and down again at about 3 μm. (K. Serkowski, review in this book, p. 145). It seems therefore that you have succeeded in interpreting this phenomenon as well.

M. Harwit: If the grains in the interstellar medium produce a quarter wave or circular dichroic effect, a net angular momentum transfer takes place from the starlight to the grains. Since the starlight is preferentially directed within the galactic plane, this effect should affect grain alignment.

Anon: You have assumed that the position angle of interstellar polarization is independent of wavelength. But Coyne and Gehrels (Ref.: *Astron. J.* **71**, 355, 1966) reported earlier a remarkable wavelength dependence of the position angle of interstellar polarization. How would your results be affected if there is intrinsic wavelength dependence of position angle.

(Kemp)

K. Nandy: Do you think that it is just a coincidence that the circular polarization band occurs near 4300 Å wave and a change of slope occurs in the extinction curve?

R. D. Wolstencroft: I think it is probably unrelated. The theory of Kemp requires that the wavelength dependence of circular polarization obeys a dispersion-like relation which is the first derivative of the absorption curve. This would require the maximum absorption at the point of zero circular polarization, namely at about 5500 Å rather than 4300 Å.

D. R. Huffman: I would like to report a preliminary laboratory discovery that raises a different possibility for explaining these circular polarization results. In doing so, I want to credit Dr Kemp for the use of one of his photoelastic modulators in the experiment. We have detected circular dichroism in small particles of a magnetic iron oxide oriented preferentially along the line of sight. (These are the kinds of particles suggested by the Purcell-Spitzer theory as necessary to obtain magnetic alignment in 10^{-6} G fiields). This circular dichroism, which is a difference in the imaginary part (absorptive part) of the complex optical constant, would give rise to circular polarization in originally unpolarized light. I am suggesting this only as another possibility which should be considered.

(King and Harwit)

K. Serkowsky: How strong a photon alignment may be expected in the envelopes of cool stars?

J. Harwit: Perhaps you could deduce this from my article in *Bull. Astron. Inst. Czech.* **21**, 204, 1970.

(Lloyd and Harwit)

T. Gehrels: I would like to address a remark to theoreticians. About 20% of the stars observed for interstellar polarization have a remarkable wavelength dependence of the position angle. When we first found it (T. Gehrels and A. B. Silvester, *Astron. J.* **70**, 579, 1965) we made a qualitative interpretation in terms of the light traversing individual clouds that have various particle sizes as well as various orientations of the galactic magnetic field.

We now know that some of these stars may have intrinsic polarization, caused by circumstellar shells, for instance. In face, we make the rather astonishing conclusion that nearly all stars have at least some intrinsic polarization and that this is so especially when emission lines are observed.

However, the interstellar contribution to the wavelength dependence of the position angles is real (G. V. Coyne, *Astron. J.*, in preparation) and your attention is drawn to this effect.

Anon: Ireland *et al.* (Reference: *Nature* **212**, 990, 1966) tried to explain the wavelength dependence of the position angle of interstellar polarization by considering the possible rotation of the interstellar magnetic field.

(General)

M. Harwit: I would like to point out how little we actually know about the mechanism which aligns grains in interstellar space. Both the work of Spitzer and Purcell, and the calculations by King and myself show that none of the mechanisms postulated thus far align grains adequately, unless very unusual material properties are invoked. It is therefore important to look for new effects which would lead to alignment, and these effects will have to be strong, particularly in view of the existence of several strong damping mechanisms presented in the paper Leon King just read. We should also keep in mind that some alignment effects will work in certain regions of interstellar space, while others may dominate elsewhere. This will complicate the interpretation of observations to a serious extent. It also argues against interpreting the grain alignment as a reflection of the magnetic field direction; these two parameters may be quite unrelated or may be weakly related, but we have no observational evidence of a strong interrelation.

J. M. Greenberg: I am not nearly as convinced as Dr Harwit that a magnetic alignment mechanism fails, particularly since only a factor of about two in the magnetic field is at issue. In any case it is clear that no other mechanism proposed is nearly as effective (Greenberg, 1971, *Proc. of Symp. on Interstellar Molecules*, Charlottesville, Va. p. 94–124.)

T. Gehrels: I wonder if theoretical work is progressing on the effects of surface waves on the interstellar polarization and reddening phenomena. Van de Hulst devoted a chapter (Ch. 17, *Light Scattering by Small Particles*, John Wiley & Sons, Inc., 1957) to this topic. I once wrote a note (*Astron. J.* **71**, 62, 1966) in order to try a qualitative explanation of the characteristic curve of the interstellar polarization.

K. Serkowski: While the shape of the wavelength dependence of interstellar extinction around discrete absorption features such as, e.g., λ 2200 of λ 4430 seems to be more or less independent of the size of dust grains, the shape of this wavelength dependence in the red and the infrared is determined mainly by size of dust grains. Therefore it seems reasonable to represent it by plotting

$$\frac{A(\lambda) - A(\lambda_{max})}{A(\lambda_{max}) - A(k\lambda_{max})} \quad \text{vs} \quad \lambda_{max}/\lambda,$$

where λ_{max} is the wavelength of maximum polarization proportional to grain size and k is a constant such that $k-1 << 1$. After normalizing in this way, the wavelength dependence of extinction in Scorpius and Orion, in the red and infrared spectral regions, becomes identical with that in Perseus and the rest of the sky.

Part V

(H. M. Johnson)

P. J. Treanor: McCarthy has recently reported an analysis of the distribution of supernovae in spiral galaxies which shows a continuous increase in the frequency of supernovae per pc^3 as the nucleus is approached. Perhaps this frequency is associated with dust clouds near the nucleus as in your photograph of the M31 supernova?

(Cahn and Nosek)

L. H. Aller: In order to get improved data on interstellar extinctions for planetaries we need more reliable Hβ and radio-frequency fluxes. Many planetaries in the Perek-Kohoutek catalogue have not yet been observed. It is a matter of telescope time for the optical photometric work, particularly in the southern hemisphere where so many are located. In the radio frequency range it seems most practical to work at some wavelength near 6 cm, where you can still use a large dish (such as the Parkes instrument) and beat the confusion problem.

(Shu)

C. Heiles: (1) Observing the 1665 and 1667 MHz lines of OH allows a determination of optical depth. Mark Gordon and I have observed several points in Cloud 2, the dark Taurus cloud. We find that the OH velocity is correlated with optical depth. The sense of this correlation is what would be expected from a contracting cloud. The velocity of contraction is about 0.4 km s^{-1}.

(2) Verschuur and Turner published, several years ago, an unsuccessful attempt to detect Zeeman splitting of the OH lines in Cloud 2. They were able to place a limit of about 10^{-4} G. This is much less than would result from flux conservation during the previous contraction process. It therefore appears that magnetic fields need not be considered as inhibitors to contraction in the early stages.

(Philip)

M. P. Savedoff: On what do you (or Crawford) base the origin of your $E(b\text{-}y)$. What is its astronomical significance?

A. D. Philip: The four-color system was calibrated by Crawford by means of photometric measures of stars in the Hyades and other open clusters for which the distance moduli are known. The color excess $E(b\text{-}y)$ is then defined as the difference between the observed $(b\text{-}y)$ index and the ZAMS $(b\text{-}y)$ for a star of that effective temperature. Using ratios derived by Crawford, one can then calculate the color excesses in c_1 and m_1 and the total absorption A_v.

A. N. Witt: How do you explain the conflict between your results and those obtained from galaxy counts, which seem to indicate a much higher optical thickness of the galactic extinction layer?

A. D. Philip: The conflict between optical measures of stars at high galactic latitudes and counts of galaxies has been known for several years. I believe that the color excesses derived from photometric measures of stars are the most accurate.

(General)

C. Heiles: The Taurus dust cloud (Cloud 2) shows strong H I 21 cm self absorption. Mrs Knapp and I have observed this using the NRAO 300-ft telescope, which provides high angular resolution (HPBW = 9′). We find that the self-absorption exists not only in the cloud, but also very strongly in a

https://doi.org/10.1017/S0074180900054759 Published online by Cambridge University Press

narrow region *outside* of the cloud. We have not yet attempted to derive the relative amounts of cold gas inside and outside the dust cloud itself. It is not inconceivable, however, that the majority of the gas lies just outside the cloud. In this connection it is interesting to note that the CO molecule exists outside the cloud as well as inside, according to recent observations by Solomon.

F. Gardner: W44 has also been investigated with the Parkes' 64 m telescope with a 4′ beam. The 43 km s^{-1} H_2CO absorption has been found to arise in a cloud that is centered in direction near the edge of the continuum distribution and has halfwidths of $7' \times 4'$, when corrected for beam broadening. The location and velocity are similar to those of the OH absorption which are believed to be associated with the supernova remnant, although the OH cloud is considerably larger.

Part VI

(Huffman and Stapp)

T. Gehrels: I am rather intrigued by your remarks on the size distributions being narrow, because for the interstellar polarization we also concluded, tim eand aging (T. Gehrels, *Astron. J.* **73**, 641, 1967; K. Serkowski, T. Gehrels, W. Wisniewski, *Astron. J.* **74**, 89, 1969), that the size distribution is narrow.

(Day)

W. W. Duley: Do you have a monomolecular H_2 layer or does H_2–H_2 physical adsorption occur?

K. L. Day: Since it was not possible to outgas the filament, it is likely that a certain amount of H_2 gas was temporarily absorbed during the experiment, leading to impinging H_2 striking previously attached molecules of this gas. From a pure physics standpoint this is not desirable, but might actually be a better approximation to the situation in space than a scrupulously clean graphite surface.

(Watson)

H. J. Habing: I have two comments:

(1) Observations of the 21 cm line in absorption and in emission show clearly the existence of two gas components, hot rarified gas and cool clouds. Judging from the temperatures you get, your model works only for the cool clouds, although the clouds should not be too dense, because then your radiation field is too weak.

(2) You assume $n_e \approx 0.05$ cm^{-3}. If you have less electrons, then your mechanism will become less effective because the charge on the grains goes up. However, to get 0.05 in a cloud of $n_H = 10$ cm^{-3} requires $n_e/n_H = 5 \times 10^{-3}$, this means that in addition to the 912–1500 Å radiation field, another ionizing mechanism must be present. I am therefore doubtful whether your mechanism can replace the still hypothetical heating mechanisms of low-energy cosmic rays and soft X-rays. But maybe that is not what you want?

W. D. Watson: I agree with your first comment. However, I should point out that my mechanism is proposed for intermediate density clouds. There seems to be less difficulty in heating the intercloud medium. However, the conditions in denser clouds are poorly known. Hence any additional heating mechanism would be useful in pur understanding.

In reponse to point 2, I agree that electron densities which would be larger than given by ionization of carbon would imply an additional heating mechanism. However, in fact, my calculation is based on electron density due only to carbon ionization.

(Wickramasinghe)

T. Snow: I would like to make the comment that if the optical properties of grains change as you suggest when the grains reach large distances from their 'parent' star, perhaps this can at least partially explain our result that the diffuse interstellar bands do not seem to be formed in circumstellar dust shells. Grains which are still close to a star would not yet have the dislocated atoms in them which may be responsible for the diffuse bands.

N. C. Wickramasinghe: This is possible.

J. M. Greenberg: What I believe is more likely than the dislocated atom explanation for λ 4430 is that the grains which are initially ejected from the stellar atmospheres are essentially the cores on which mantles subsequently develop in interstellar space and that is the *mantles* which contain the '4430' ingredient.

M. P. Savedoff: You must be careful to include electrical charges. At such velocities (temperatures) appreciable charges may result and the electrostatic cross-section may be larger than geometric.

R. F. Willis: The statement that the optical properties of grains, which may have been bombarded by high velocity H atoms in circumstellar space, will be altered due to the high concentration of possible defects 'frozen-in' crystalline materials, is not absolutely true. For highly disordered graphite or any other electrically conducting crystal, the optical absorption spectrum will remain unaltered. For a glassy material, such as silicate, the optical spectrum is, of course, insensitive to further disorder effects. Some dielectric crystalline materials could show additional features, the strength of which will depend on both the nature of the impacting atoms (or ions) and their concentration. Nevertheless, the overall spectrum will still be closely similar to that of the undamaged crystal. The accretion of a mantle of damaged crystal plus ions plus a thin film of a dielectric would, of course, give a different optical spectrum to that of any single component grain.

Part VII

(Greenberg and Yencha)

W. W. Duley: I know of experiments which involve the trapping of radicals from microwave discharges and that in this case explosions in the condensed solid often occur. However, I know of no corresponding experiments done with UV light. Have you seen this effect experimentally?

J. M. Greenberg: In direct answer to your question: No. The UV light is used in our experiment to produce the free radicals. What triggers an explosion does not seem to me to be critical. I would expect that a warming up of the material which already has a high free radical concentration would be sufficient. I recall seeing mention (in the volume on Free Radicals by Bass and Broida) of a sudden explosion of a solid with frozen in free radicals when it is allowed to warm up. In this connection I should like to mention that this phenomenon may be the cause of the production of jets in comets as they approach the Sun.

B. N. Khare: We have performed experiments on the long wavelength UV irradiation of a frost of formaldehyde, ammonia, ethane, and water deposited on a quartz cold finger under high vacuum at 77 K. Products produced at low temperatures, as determined by gas chromatography/mass spectrometry and infrared techniques, include the organic molecules methanol, ethanol, acetone, acetylene, acetonitrile, acetaldehyde, methyl formate and possibly formic acid. Many of these product molecules have been identified in the interstellar medium. As in your case we also found a range of unidentified very high mass number products. These results, presented at the *Symposium on Interstellar Molecules*, Charlottesville, Virginia, 4 October 1971, have been published as 'Experimental Interstellar Organic Chemistry: Preliminary Findings', by B. N. Khare and Carl Sagan (in *Molecules in the Galactic Environment*, M. A. Gordon and L. E. Snyder, editors, Wiley, 1973).

J. M. Greenberg: I should point out that there are several essential distinctions between your experiment and ours. In our experiments which were first reported at the AAAS meeting in Chicago, December 1970, and at the Liège Symposium in April 1971, as well as in Charlottesville we have restricted ourselves generally to lower temperatures and also to using a mixture of water, methane (or ethane) and ammonia so that we start with much smaller molecules. We believe that both of these conditions may represent better the chemical evolution of the interstellar dust. We extended our radiation down to 1400 Å in the UV. I do not believe that the mass spectrograph/gas chromatography gives the correct identification of the molecules as they exist in the *low temperature solid* but rather provides information on what molecules may be formed in the solid as it is warmed up in analogy to an interstellar grain blow-up. It may well be that somewhere during this process the grain may contain all sorts of amino acids and other very large molecules. Molecular weights of well over 350 were indicated in our gas chromatograph.

B. N. Khare: I agree that the composition of the solid may be amino acids or their precursors. Amino acids have been found in many experiments on the primitive simulation of the Earth's atmosphere, by Stanley Miller, Sidney Fox and recently by Carl Sagan and myself where we used long UV energy. However, it is now pretty well determined that whether one uses heat, shock, UV or gamma radiation, the formation of amino acids and their precursors is to be expected.

(Heiles)

M. W. Werner: I would like to suggest the possibility that molecules are not located deep inside dust clouds, where Salpeter's parameter has a large value, but are instead concentrated in a thin shell at the

https://doi.org/10.1017/S0074180900054759 Published online by Cambridge University Press

surface. If this were true, the edges of the cloud might show more intense molecular lines than the center does.

J. M. Greenberg: Perhaps this would be the kind of thing which would happen to grains as they migrate outward from UV shielded interior regions of a cloud and are then subjected to a higher probability of being triggered to explode and release large numbers of molecules.

S. van den Berhg: Observations of Cas A with the 200-in. show that [O II] λ 3727 has a very similar distribution to that of [O II] $\lambda\lambda$ 7320, 7331 over the surface of the remnant. This suggests that the absorption over the face of this object is probably *not* very non-uniform.

(Morris *et al.*)

T. Snow: The frequencies of the four lines of cyanoacetylene which you have observed are near the peak of the 3 K blackbody radiation field. Have you considered the blackbody radiation as a source of excitation of the lines?

M. R. Morris: This has not been considered but it should be in a complete analysis of the emission from this molecule. However, I do not think that the 3 K background would be an important excitation mechanism.

(General)

F. Gardner: The detection of the $l_{10} - l_{10}$ transition of formaldimine (C_2NH) by P. D. Godfrey, R. D. Brown, B. J. Robinson, and M. W. Sinclair is reported. Laboratory measurements showed that the two strongest peaks of the multiplet have rest frequencies of 5289.84 and 5290.78 with an intensity ratio of 1.6. In observations of Sagittarius B2 with the Parkes 64 metre telescope both these lines were detected with intensities of 0.1 K and 0.05 K with a peak-to-peak error of 0.025 K. The linewidths were 20 km s^{-1} and the radial velocity 63 + 6 km s^{-1}.

R. F. Willis: Speculating on mechanisms for forming complex, large molecules there is one well-known effect involving graphite which may be of interest. Graphite consists of layers of C atoms, the spacing between which is large ($\sim$ 3.5 Å) so that molecules and atmos can easily become incorporated in the lattice. Such 'chelate complexes' are well-known. Also, the edge-faces are much more reactive due to dangling bonds and have a higher accommodation coefficient than the basal plane. It is possible that interstellar graphite grains could accrete atomic and molecular species between the atomic layers so facilitating the formation of more complex molecules. To a certain extent, such molecules would be 'shielded' from photodissociation by UV quanta. We have observed that Cs deposited on the basal plane diffuses between the layers and is trapped there. The optical properties are only slightly modified in such a system.

Part VIII

(Davies)

C. G. Wynn-Williams: One of the remarkable properties of many of the 1612 MHz OH/IR stars is that there is a symmetry between the high velocity and low velocity parts of the microwave spectrum. Does your model account for this?

(Wynn-Williams *et al.*)

M. W. Werner: It is interesting that some of your small infrared sources have no associated radio emission. This suggests that arguments based on a comparison of wide angle infrared and radio continuum measurements of H II regions may be misleading. Since much of the far infrared ($\lambda \sim 100\ \mu$) radiation may come from knots which have no radio emission, it will be necessary to compare IR and radio measurements of a given condensation before drawing any conclusions about energetic problems.

P. E. Palmer: I would like to briefly remark on the generally excellent correlation of far IR emission with millimeter-wavelength molecular line emission (HCN, CS, X-ogen, etc.). Dr Wynn-Williams kindly communicated the 20μ map of W3 to us. We observed HCN at three distinct positions in W3. Unfortunately, with the 36 foot radio telescope it is not possible to resolve all of the $20\ \mu$ sources. In NGC 7538 we also observed HCN and CS emission at the $20\ \mu$ position. NGC 7538 is also unusual in that it is one of the 6 or 7 objects in the galaxy showing maser emission in the rotationally excited states of OH.

These results are measurable if we believe that the molecular lines arise in gas mixed with the dust emitting the far IR: the large extinctions derived serves to protect the molecules and the relatively high (for H I clouds) temperatures and densities allow these molecules to be excited above the background temperature and become detectable. Alternatively, the molecules may be 'outside' the hot dust, but still in a dense region.

(General)

H. J. Habing: Dr Wynn-Williams has shown the coincidence in W3 (cont.) of the H_2O point source and an IR source that probably is a protostar. In this connection I should like to mention the work by Dr T. de Jong at Leiden, who recently constructed a model of an H_2O maser, in a protostar cloud. He considers a semi-infinite plane parallel atmosphere of uniform temperature and density. Deep in the atmosphere the H_2O molecule is in LTE, whereas close to the outer boundary LTE breaks down. The H_2O line at 1.35 cm arises in the transition of the 6_{16} to 5_{23} level, at 447 cm^{-1} above the ground state. Approaching the surface of the protostellar atmosphere from the inside one finds that a whole series of levels, among which is the 6_{16}, stays in LTE considerably longer than other, similar series, one of which contains the 5_{23} level. It follows that in the outer parts of the atmosphere a layer exists in which the 6_{16} level is inverted with respect to the 5_{23} level. A ray of 1.35 cm photons will be amplified tangentially to the stellar atmosphere.

The model has some features in common with an earlier proposal by Dr Richard Hills from Berkeley and contains an idea of Dr J. Jefferies. Today I learned from Dr W. Klemperer that Dr Phil Solomon has arrived independently at similar conclusions.

Part IX

(Woolf)

D. P. Gilra: (1) For late N-type carbon stars Gilra and Code (*Bull. Am. Astron. Soc.* **3**, 379, 1971; see also my paper on 'Dust and Molecules in the Extended Atmospheres of Carbon Stars') had predicted emission due to circumstellar SiC grains in the 10–13 μ region. The infrared observations of these stars you have shown confirm our predictions very well. It seems the existence of SiC grains in circumstellar shells around these stars should be considered established.

(2) As I indicated in my paper on 'Collective Excitations and Dust Particles in Space' there are strong theoretical problems in identifying the observed features (in M supergiants etc.) in the 10 μ region with silicates. The dielectric constant of silicates is not available in this spectral region. So the absorption and scattering cross-sections cannot be calculated and we cannot investigate the shape, size and coating dependence of the strong resonances that will be present. Therefore the identification should be taken with reserve.

Part X

(Taff and Savedoff)

K. Nandy: What drives the cloud motions?

L. G. Taff: It is not our intention to account theoretically for the observed velocity distribution of the clouds. Possible mechanisms might be turbulence, galactic rotation on time scales $\gtrsim 10^8$ yr, supernovae creating expanding H II clouds.

B. D. Donn: What is the time scale of the evolution of the mass function?

L. G. Taff: Using as a basis the collision time for the smallest clouds a solution that is not an equilibrium one will decay as $e^{-t/\tau}$ to an equilibrium solution where τ is 5 to 15 times the above collision time.

L. H. Aller: Have you considered the effects of rotation?

L. G. Taff: We have not, although magnetic fields are included in some of the discussions. Rotational effects will not affect the cooling aspects of the problem because the cooling is so rapid ($\sim 10^{3.5}$ yr) but may, through shearing motions, affect the formation of very small mass fragments at the interface between clouds as they separate. For this to be very important the periods of cloud rotation would have to be much smaller than the cloud-cloud separation times.

B. J. Bok: What do you compare against?

L. G. Taff: Radio observations.

(Cameron)

J. Hackwell: We have already started to compare the infrared absorption spectra of carbonaceous chondrites with the infrared excesses of M-stars and find a surprising agreement between them. It is interesting to note that neither the M-stars nor the carbonaceous chondrites appear to show the secondary absorption bands typical of many silicates. In the case of the carbonaceous chondrites this may be due to their peculiar 'stacked' structure which is dissimilar to that found in any of the terrestrial silicates.

(McNally)

C. E. Heiles: Radio astronomers have found many molecules in regions such as the galactic center, M51, and Orion A. I believe these are just those places where matter is collapsing under its own gravity and where pressure is unimportant, similar to collapsing models published by yourself and others in past yers.

Some molecular transitions require much higher densities for their excitation than others; these always have smaller velocity widths than ones which can be excited at the lower densities. This is just what one would expect if the linewidths result primarily from large-scale collapse motions because the theoretical models show density increasing and collapse speed decreasing towards the center of the cloud. The correspondence between observation and theory can be checked to increasingly high degrees of precision in the near future by observation of strategically chosen transitions and molecules using high angular resolution obtainable with interferometers, one of which has just been constructed at the Hat Creek Observatory.