PHOTOMETRY OF THE COMETARY ATMOSPHERE: A Review

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1. INTRODUCTION

Photometry and polarimetry of the cometary heads still constitute one of the most important sources of information about the physical processes in comets. For instance, most of the present estimates of molecular lifetimes are based on the observed distribution of molecules in the cometary head and the assumption of a particular kinematical behaviour of the matter in the cometary atmospheres.

The study of kinematics and dynamics of cometary heads and tails has been based upon the analysis of the forms and apparent motions of well-defined envelopes, halos, knots in tails and streams. Direct inspection of a large number of photographs (or drawings from the last century) of several bright comets demonstrates that the cometary head is generally a complicated object. The heads consist of nearly circular diffuse patterns with superposition of different features, particularly of curved streams. This is illustrated by the Atlas of the Cometary Forms compiled by Rahe, Donn and Wurm (1970).

Comets of small apparent dimensions exhibit few features which could be observed directly and could be used for the interpretation of physical processes. Therefore, for many comets the information available for comparison with theories of the mechanism and tail or head formation was obtained mostly only from the study of distribution of the surface intensity.

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It is, however, essential that the observations should refer, as far as possible, to the radiation emitted or reflected by different kinds of particles (dust, C_2, C_3, CN, CO^+, etc.). It is, therefore, evident that the interpretation of the structure of comets requires monochromatic observations.

Direct unfiltered photography is still valuable for the continuous monitoring of the rapidly changing cometary phenomena—as, for instance, of some features in the tail. It is almost useless for other information about the processes in cometary bodies. The amount of useful monochromatic observations of comets has been still lamentably poor in the past decades—in contrast to the photometry of stars and nebulae, where rapid progress has been achieved. The number of comets observed with adequate modern techniques is small and limited mostly to bright objects observed since 1956. A most dissatisfying circumstance is the fact that photographic and photoelectric observations do not usually lend themselves to the transformation of the absolute photometric scale into isophotes which can be obtained with high angular resolution only from large-scale photographs.

The best discussion of this problem is in a short review by F. D. Miller in the Appendix to "Report on Planned Programme for Comet Kohoutek 1973f" by Brandt, Rahe and Vanysek (1973).

Table 1

Narrow-Band Filters for Standard Cometary Photometry
and Photography (Recommended)

Cometary Emission	λ_{max}(Å)	FW(Å)
CN	3880 4738	70 to 80 50 to 60
C_2	5170	50 to 60
CO^+ (tail)	4267	<50
Na	5893	<50

λ_{max} = wavelength of the maximum transmission
FW = full width at half maximum

Table 2

Sample of the Narrow-Band Filters Used for Photometry
of CN, C_2 and Continuum*

Author	CN	C_2	Continuum
Bappu et al. (1967)	3859(163)	4720(71)	4310 4860(65) 5875(97)
Vanysek (1969)	3880(240)	4740(90)	4860(180)
Miller (1969)		5136(96)	4850(64)
Konopleva et al. (1970)		4740(190) 5225(480)	4380-4470 4750-4840 5640-5700
Borra et al. (1971)	3878(95)	5117(95)	4870(95)
Kohoutek (1974)	3892(42)	4747(58) 5180(78)	5306(73)

*Four digits are peak transmission wavelength and in parentheses the width at half maximum; both in Å.

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2. PRESENT STATE OF COMETARY PHOTOMETRY

Even though the narrow-band photometry is being used more extensively, the available photometric observations of comets suitable for the study of the different compounds' distribution are still lacking, and only a few homogeneous sets of observations have been obtained. The paucity of accurate photometric observations of comets in monochromatic light is due merely to the fact that it is very difficult to reconcile the needs of cometary photometry with those of stellar photometry.

The results obtained from the wide-band photometry must be regarded as tentative only, unless it is quite evident that either continuum or emission bands were absent in the spectral region studied. There is only one exception: the photometry and photography obtained with red filter (e.g., Schott RG 1) provide data for the dust part of the coma or tail. But in other visual spectral regions the situation is more complicated. One can, for instance, hardly make some reasonable conclusion about the dimension and shape of the CN or C_2 coma because of overlapping with CO^+ features.

The acceleration of CN and C_2 molecules due to the light pressure estimated from the oscillator strength for typical bands is 0.3 to 0.5 cm sec^{-2} at 1 AU and leads to some deformation of the CN and C_2 isophotes by shifting them slightly into the tail direction. However, the kinematics of CO^+ ions required obviously larger accelerations thus the typical "onion-like" form of the isophotes obtained

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from measurements near the CN emission pass-bands is due to the overlap of the CN and CO^+ emission and, of course, also to the scattered light on the dust particles. This effect can easily be demonstrated in many direct photographs or even on the isophotometry charts.

The colour photography is one of the very efficient methods for a direct inspection of dust and gaseous forms in comets. Photographic colour emulsions with very low reciprocity failure are available and show promise in the study of cometary structure and morphology. An example of the possibilities was shown by Dr. J. C. Brandt by a colour photograph of Comet Bennett which showed the dust tail as yellow and the ion tail as blue. A black-and-white photograph of the comet taken at approximately the same time did not permit the two tail types to be easily distinguished.

In the visual region the UBV colour system used in routine stellar photometry is inadequate for cometary photometry. The unusual intensity distribution in cometary spectra means that the colour of the comet cannot be transformed to any conventional colour system. The U-filter covers practically only CN bands (3880 Å) while the V- and B-filters include the most prominent band sequences of C_2. The C_3 emission and most of the CO^+ lines are in the range of the B filter. Only from the U-B colour, which is more sensitive to the behaviour of cometary spectra, the relative contribution of CN to C_2 may be qualitatively estimated.

Somewhat more suitable for cometary photometric studies is the uvby system combined with the H_β narrow pass-band filter. The H_β and b filters can be used for the determination of C_2 $\Delta v = +1$ emission band and continuum flux near the H_β wavelength. The region close to 4860 Å is not strongly contaminated by molecular emission and, therefore, the H_β photometry combined with a wide-band filter seems to be the best "two-colour system" for routine photometry of faint comets. Also measurements in the near infrared—i.e., R and I—may sometimes be contaminated by molecular emission, particularly by the CN red system.

For practical purposes the colour difference D may be introduced, defined by

$$D = (U\text{-}B)_{comet} - (U\text{-}B)_s$$

where $(U\text{-}B)_{comet}$ is the colour corresponding to the comet, and $(U\text{-}B)_s$ are the colours for the Sun or stars with the continuum distribution similar to the distribution in the cometary continuum. When D = 0 no emission of CN is present. The value of D increases with molecular emission up to the maximum value which depends on the filters' transmission at 3880 Å.

A very serious problem is the fact that most parts of the available photometric data of comets have been usually obtained from observations which were made at large zenith distances. Therefore, their accuracy cannot be compared with those achieved by routine photoelectric methods, and absolute flux values are about 10% or more uncertain in contrast to the relative intensities of nearby

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passbands which may be precise enough when, for instance, a tilting filter technique is applied.

This method was used recently by Barbieri et al. (1974) for the determination of the continuum flux at 8560 Å and 8748 Å of Comet Kohoutek 1973f, by means of a narrow Fabry-Perot filter. The advantage of solid etalons in wavelength scanning by tilting is an extensive exploit in atmospheric studies and even weak emission can be identified. By a tilting method it can be easily demonstrated that the continuum of Comet Kohoutek was free of any molecular emission around 8750 Å. However, such observations are unique and limited to bright comets.

Valuable observations were obtained by a photoelectric spectrum scanner by O'Dell and Mayer (1968) for Comet Rudnicki (1966e), and by Gebel (1970) for Comets Ikeya-Seki (1967a), Honda (1968c) and Thomas (1968b). A similar observational method was applied by Babu and Saxena (1972) to Comet Bennett (1969i) and by Babu (1974) to Comet Kohoutek (1973f). Unfortunately, these observations had relatively low angular (and space) resolution.

From poor space resolution suffer, to some extent, also the photographic measurements which were used for studying the molecular density distribution in the cometary heads. (See Vorontsov-Velyaminov (1960), Vanýsek and Žáček (1967), Dewey and Miller (1966), Borra and Wehlau (1971, 1973).) The best material of this kind with high angular resolution (about 14"/mm) in monochromatic light has been obtained by Rahe et al. (1974).

Perhaps the most important photometric data (with regard to the photometric profiles of cometary neutral atmospheres) have been obtained by Malaise with the aid of his six-channel photometer with adjustable wavelength and passbands (Malaise, 1970); but a considerable amount of his observations were still recently being reduced. The instrument itself was recently attached to the 2-meter Ondrejov telescope, but very bad weather conditions in January 1974 permitted only one incomplete observation of Comet Kohoutek made by Malaise and the author of this report.

3. RECENT RESULTS

From preliminary reports obtained by many observers an unusually great number of photoelectric observations of comets has been obtained very recently. Both bright Comets Kohoutek 1973f and Bradfield 1974b were observed so extensively that the observations are only partly reduced and the following summary represents only a small sample of the results.

A very large and homogeneous set of photoelectric and infrared measurements before the perihelion passage of Comet Kohoutek was published by Rieke and Lee (1974). Their results are important for the interpretation of infrared radiation (particularly the 10μ "bump") of the dust in the cometary atmosphere. The data for UBVRI colours may, however, provide only rough information about the behaviour of the continuum radiation of the comet in the first half of October 1973, when the contribution of the emission bands was negligible. After October

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16, the emissions of CN and C_2 bands in the spectrum of the comet were apparent and only observations in narrow pass-bands might provide exact data for the determination of albedo of the dust particles, by a comparison of the integrated surface brightness in the infrared and surface brightness of the scattered light in the visual spectral region. Therefore, the numerical expression involving albedo derived by Rieke and Lee should be considered to be only very preliminary. Their data in the UBVRI system for different diaphragms indicate that the colour index B-V of the inner part of the coma was almost the same as that of the Sun while the outer region shows a decrease of the index U-B which was probably due mainly to the CN band.

One of the sets of pre-perihelion observations in the narrow pass-bands was obtained by Babu (1974), with the spectrum scanner (Babu, 1971) measuring the intensities in the pass band about 35 Å in the range 3700 to 6400 Å. His results indicate that absolute fluxes of CN emission at 3880 Å varied approximately with r^{-2} while C_2 and C_3 bands increased with r^{-4} in the interval of heliocentric distances $r = 0.73$ to 0.52. But because the change of geocentric distance was very small and the radius of the coma measured with a fixed diaphragm was almost constant (about 4×10^4 km), a fast increase of the C_2 and C_3 intensities with decreasing r was due, partly at least, to a decrease of the length scale of the parent particles rather than to an increase in the abundances of these molecules. This effect was caused by a shrinking of the gaseous coma which, for small diaphragms, is more pronounced for C_2 emission than for CN.

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This fact was confirmed by the photometric data submitted by Cowan and A'Hearn (1974) which provided the total flux in the C_2-band sequence at 4700 Å measured in a very large diaphragm 116 and 193 arcsecs corresponding to the coma diameter about 10^5 and 1.8×10^5 km in the interval from December 1 to December 7. The luminosity of the C_2 (0, 1) band remains essentially the same—about 2×10^{19} erg sec^{-1}—in the time interval between December 2 and December 7 and was slightly lower than on December 1.

The results obtained by Babu for the continuum energy distribution in the head of Comet 1973f indicate some reddening of the scattered light with respect to the Sun, decreasing with phase angle ϕ (in the interval $\phi = 51°$ to $57°$) and with heliocentric distance so that the reddening disappeared on December 17.

The positive colour excess has been confirmed in several comets (Walker, 1958; Bappu and Sinvhal, 1960; Liller, 1960; Vanysek, 1960; Kharitonov and Rebristyi, 1974). Some spectrophotometric results lead to the conclusion that the colour of comets resembles the spectral distribution of G8 V stars and this reddening may be attributed to selective light scattering on small dust particles. However, the results obtained by Gebel (1970) for Comets 1968 I, 1968 V and 1968 VI show that the spectral distribution of continuum was "grey"—i.e., it coincided with the colour of the Sun.

In the case of Comet Kohoutek, measurements of the continuum spectral distribution were made at very large zenith distances where some uncontrollable influence of anomalous extinction must be expected. Therefore, it is not quite certain that the differences with respect to the solar continuum are real.

Post-perihelion photoelectric observations have been made by Kohoutek of his bright comet in the UBV system as well as in the pass bands near λ(Å) = 3880 (CN); 4267 (CO^+); 4738, 5172 (C_2); 5300 (continuum) and one centered on the sodium doublet.

This set of observations covers the range of heliocentric distances r from 0.65 to 1.0 AU. Kohoutek reported that measurements in the 4267 Å pass-band indicated a negligible intensity of CO^+ bands in diaphragms 40 and 80 arcsecs and the measured intensity virtually refers only to the continuum radiation. Emission of the sodium doublet was detected only on January 15 and 16, but at the heliocentric distances 0.7 to 1.0 AU the sodium lines (if any) were very weak. It must be noted that the intensity of NaI emission before perihelion passage was obviously also low, as follows from the above-quoted measurements made by Babu. If the results obtained by Kohoutek for 4267 Å, 5300 Å and 5890 Å are interpreted as intensities for the continuum, then solar radiation scattered on the dust particles exhibited some red excess, which is in agreement with the selective "reddening" of the cometary continuum observed in several previous comets studied photometrically and spectrophotometrically. The estimated contribution of the C_2 $\Delta v = 0$ band to the continuum in the V-colour was about 1:0.7, and about 1:1 in the B-colour where, of course, $\Delta v = +1$. The C_2 band as well

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as the CN band dominates in the U-colour where the band/continuum ratio was about 1.66 (for a heliocentric distance r = 1 AU).

The dust coma, according to these measurements, was more concentrated toward the nucleus than the CN and C_2 atmospheres. The "colour effect" described by Vanysek (1960, 1966)—i.e., an increase of the colour index with diameter of the diaphragm, is quite evident in B-V from Kohoutek's measurements. The absolute colour indices in the 40 and 80 arcsec diaphragms increase slightly in BV from 0.85 to 0.94. The magnitude difference Δm of the measurements in two diaphragms with the radii ρ = 40 and 80 arcsec indicates a deviation from the surface intensity law ρ^{-1} for a spherically symmetric coma. The deviation can be expressed by ρ^{-n} where $n < 1$, and is due to the "flatness" of the photometric profile of the inner part of the coma where visible radicals are produced from parent particles. This means, of course, that the "zone of production" for C_2 and CN was traced at least up to 4×10^4 km from the nucleus.

Kohoutek found that the comet's brightness decreased after the perihelion passage more rapidly in the inner part of the coma (with $r^{-4.6}$ to r^{-5}) than in the outer one ($r^{-3.6}$ to $r^{-4.2}$). A very rapid change in surface intensity was observed photoelectrically by Mrkos and Vanysek in Comet Bradfield 1974b. This is merely a well-known effect due to the coma expansion with increasing heliocentric distance r—i.e., a reversal of the pre-perihelion shrinking of the cometary head.

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Although the results discussed here and obtained by Babu, Kohoutek and Cowan and A'Hearn represent not quite homogeneous sets of observations, the pre-perihelion and post-perihelion total luminosity of the C_2 (4734 Å) Swan-band can be compared. If the available data are reduced to the heliocentric distance $r = 1$ AU and to the diameter 5.5×10^4 km then the post-perihelion luminosity decreases by a factor of about 10:

pre-perihelion $F_o = 2 \times 10^{18}$ erg sec^{-1} (from Cowan and A'Hearn's observations);

post-perihelion $F_o = 1.5 \times 10^{17}$ erg sec^{-1} (Kohoutek).

The post-perihelion decrease of the luminosity of CN seems to be not so sharp. The relative intensities of the CN band in December 1973 obtained by Babu are considerably lower than those of the C_2 $\Delta v = 0$ band but the post-perihelion results reported by Kohoutek indicate that the CN emission was slightly more luminous than that of the C_2 main band. Therefore, the luminosity of CN (reduced again to $r = 1$ AU and the same area) was lower after the perihelion passage only by a factor of about 2.5 to 3. A considerable diminution in luminosity occurred in the continuum, as follows from almost all available observations.

One can believe that Kohoutek 1973f was a "normal" comet and the relatively high brightness at large heliocentric distances shortly after discovery till the beginning of October 1973 may be attributed to dust clouds surrounding the central condensation (or nucleus), which diminished slowly when the comet was

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approaching the Sun. This means that at least this particular comet may be described as a nucleus surrounded by a swarm of dust particles from which the very small (and volatile) ones were expelled and evaporated beyond $r \geqslant 0.8$ AU. Barbieri et al. (1974) concluded from the near-infrared observations at 8560 and 8748 Å that the dust production rate decreased by a factor of 10 relatively to the gas production in the post-perihelion period.

Although the above discussed results are somewhat incomplete, it is evident that C_2 emissions are more sensitive to a change of dust content than the CN band. Unfortunately, the luminosities of bands of molecular origin are not sufficient for the determination of the production rate without knowledge about kinematics and lifetime scale of the respective compounds. However, there is strong indication that the C_2 production rate depends on the dust contents in the cometary atmosphere (and, consequently, on dust production) more than CN and perhaps other molecules.

4. POLARIMETRIC MEASUREMENTS

The available polarimetric data of Comet Kohoutek are only few and must be considered only as preliminary. Michalsky (1974) reported polarization measurements made by Avery, Stokes, Zellner, Wolstencroft and himself at three observatories in Hawaii, Arizona and Washington State. Pre- and post-perihelion observations were made with broad and narrow filters, which included or excluded emission lines and/or bands. All measurements were centered on the

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coma condensation with apertures ranging from 15-40 arcsecs in diameter. As for Comet Bennett 1969i, higher linear polarization was observed in the red than in the blue. Rayleigh scattering is excluded because of the colour of the comet.

The maximum of linear polarization was found by Avery on January 9—26%—in B colour, and Zellner (also 26%) on January 16 in the close area (15 arcsecs) around the central condensation.

Measurements made in adjacent spectral regions when emission was included and excluded indicate that the magnitude of the polarization is higher in emission—this unusual effect has not been reported previously. Other measurements bear out this behaviour after perihelion passage as well as before. Because the polarization of the molecular bands should be only 8 to 10% this effect must be analyzed again very carefully. All measurements showed the direction vector to be rigidly perpendicular to the scattering plane.

Michalsky noted that the light scattered from nonspherical aligned particles should show a small circularly polarized component. A search for this component led to a value of 0.02 ± 0.06% showing that no large effect is present, but observations indicated an increase of linear polarization with decreasing aperture, which may imply alignment possibly contradictory to the previous discussion.

The most important results concerning the polarization of the cometary light are those reported by Weinberg; however, these did not concern the comet's

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head but the tail. A multicolour photoelectric polarimeter was used at Mt. Haleakala Observatory to observe the tail of Comet Ikeya-Seki (1965 VIII) on 4 nights following perihelion on 21 October 1965. Observations were made at six continuum wavelengths and with two different filters centered at the 5577 Å emission of OI. From preliminary results only the observations at 5400 Å on 28/29 October 1965 are available. Measurements were made by scanning at 0.5 deg/sec over a 9 x 20 deg section of the sky containing the comet: in azimuth, from 105 to 114 deg (90 = east), and in elevation, from 0 (horizon) to 20 deg in steps of 1.0 deg. This method of scanning provides considerably more information in the direction normal to the axis of the tail of the comet and the intensity can be easily derived from the total brightness (radiance) of background plus comet for different zenith angles.

Of particular interest is the change in polarization between 6 and 7 deg elevation (approximately 11 deg from the nucleus). Since the background (primarily zodiacal light) and comet radiations are independent, their Stokes parameters are additive. The polarization of zodiacal light in this area is positive—i.e., the electric vector is perpendicular to the scattering plane. Only negative polarization at distances greater than 11 degrees from the nucleus can produce the observed net decrease in total polarization in the direction of the comet tail.

The comet was ideally positioned with respect to the main cone of the zodiacal light, and the separation of the comet from the smooth fall-off in total brightness is easily accomplished. The sharp change of orientation of the polarization

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plane (orientation of the electric vector) with the phase angle is very typical for the polydispersed optically thin cloud containing particles with very low imaginary part of the refractive index. Therefore, the polarization data obtained by Weinberg are compatible with infrared results at $\lambda = 10\mu$ where the emission-like peak (observed in spectra of Comets Bennett and Kohoutek) may be ascribed to dielectric silicate particles (Maas et al. (1970), Ney and Ney (1974), Kleinmann et al. (1971)).

Moreover, negative polarization (with respect to the orientation of the electric vector) as in the case of zodiacal light, requires the presence of dielectric or irregularly-shaped particles. The use of additional observations at several wavelengths at different times, as Weinberg suggests, may single out a rather small family of permissible solutions for the size distribution and chemical composition of the particles in the tail of the comet if the particles are spherical or have large-volume shapes.

The possibility that elongated particles are dominant in the cometary dust is supported by some earlier measurements. Clarke (1971) showed that the plane of polarization for Comet Bennett 1970 II deviated significantly from one of the two possible orthogonal positions to the scattering plane. This effect can be explained by scattering on the aligned elongated particles. Harwit and Vanysek (1971) proposed the bombardment of dust particles by solar wind protons as efficient alignment mechanism. Because the rate at which the alignment occurs

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depends also on the gas flow from the nucleus, the polarization near the nucleus would be more arbitrarily oriented than in the tail where the solar wind predominates.

The elongated form of the particles can be expected if the crystalline formaldehyde polymers are present in the cometary dust. Vanysek and Wickramasinghe (1975) have recently discussed the possibility that the polymers $(H_2CO)_n$ are one form of formaldehyde in the comets. The polymerization process may produce polymer chains with variable length helically wound into a stable crystal. These particles would grow as long whiskers and possess optical properties in the visual and infrared region similar to those of the silicate grains.

5. PHOTOMETRIC PROFILES AND THE LIFETIME OF PARENT MOLECULES

The photometric profiles of the coma in monochromatic light are still used for the determination (or, better, estimates) of the lifetime of the parent molecules or precursors for the observed radicals, mainly CN and C_2.

The lifetime τ is defined as a reciprocal value of dissociation probability

$$\tau^{-1} = \int \sigma_\nu F_\nu \, d_\nu$$

where σ_ν is the photodissociation cross-section and the flux at a frequency ν is defined as $F_\nu = cu_\nu/h\nu$ where u_ν is the density of solar radiation (c = light speed, $h\nu$ = photon energy).

The value of σ_ν is about 10^{-18} to 10^{-17} cm^2 for the most common compound.

Results concerning the prospective parent molecules for cometary radicals (Potter and Del Duca, 1964) show that τ derived from the known cross-section and F_ν is for most compounds estimated longer than 10^5 seconds. These results, however, were not comparable with the scale-length for parent molecules determined from the polarimetric profiles of cometary heads.

The lifetimes derived from the early measurements on comets (a summary of these results is in Vanysek's paper [1972] in Nobel Symposium No. 21) suggest $\tau_p \sim 10^4$ sec. But the lifetimes determined for some components which could be possible parent molecules are $\tau_p = 10^{5.5}$ to $10^{6.5}$ seconds (except for NH_3 as a source of NH_2, with $\tau_p \sim 10^3$ sec).

The differences between laboratory and astronomical results were so striking that the hypothesis for the production of observed neutral molecules in comets via photo-decomposition processes was almost (but prematurely) abandoned and other theories were proposed (Wurm, 1961; Opik, 1963; Herzberg, 1964; Jackson and Donn, 1968).

The decomposition of parent molecules was ascribed to the predissociation or to the chemical reaction in the innermost part of the coma, or in the nucleus, or to the presence of free radicals in nuclei.

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Delsemme and Swings (1954) considered that free radicals may be embedded in ice in the form of clathrates. This idea has been modified by Delsemme who assumed that small fragments of ice of submillimeter dimensions expelled from the nucleus into the surrounding halo contain considerable amounts of clathrate hydrates formed in the cavities in the water ice lattice where different molecules, even unsaturated, can be bounded by van der Waals forces. By a destruction of the lattice by solar radiation the encaged molecules are liberated into space and ejected isotropically from the cometary head. If the molecules are free radicals, or very short-lived precursors of such radicals, the ice particles play the role of parent molecules.

However, the problem of the precursors of the observed radicals is still the problem of the methods used. The lifetime of parent molecules τ_p and of the produced radicals τ_r can be estimated, in fact, only indirectly by determining $v_p\tau_p$ and $v_r\tau_r$ (where v_p and v_r are the expansion velocities, and supposed to be constant) from the intensity distribution in the cometary head. For the interpretation of the intensity distribution only a relatively simple model (Hazer, 1957) is usually applied in which the expansion velocity has no significant distribution.

However, the radiation energy absorbed by the molecule during the dissociation processes may be higher than the dissociation energy and may lead to a significant increase of the velocity distribution of dissociated compounds. For instance, if the difference between the absorbed energy and dissociation, $\Delta h\nu$, is only one or a few eV, then the velocity distribution $\pm\Delta v$ around the mean expansion velocity $\bar{v}_p$ for particles of molecular weight 20 may increase up to some km-sec^{-1}. Then a considerable number of the produced daughter molecules flow back into the "zone of production" up to some distance toward the nucleus where the collisions with expanding parent molecules and others increase above some critical limit. Only from this rough qualitative description does it seem to be evident that the simple coma model is invalid and the actual density of daughter molecules—radicals—should be considerably higher at distances, say 5×10^3 to 10^4 km, from the nucleus.

Moreover, recent results concerning the determination of lifetimes of the parent molecules from monochromatic isophotes with high angular (and consequently also spatial) resolution indicate that the scale-length $v_p\tau_p$ should be longer than 10^4 km (Rahe and Vanysek, 1974; Delsemme and Moreau, 1973; Kumar and Southall, 1974). Rahe and Vanysek found for the scale length of CN the parent molecule of Comet Bennett 1970 II $v_p\tau_p \simeq 5 \times 10^4$ km and about the same for C_2. For the virtually "dust-free" Comet Tago-Sato-Kosaka the results are: (CN) $v_p\tau_p \sim 8 \times 10^4$; ($C_2$) $v_p\tau_p \approx 4 \times 10^4$ km.

Kumar and Southall revised the isophotes of Comet Tago-Sato-Kosaka used by Rahe and Vanysek and applied a new correction of the sky background. The new results are: (CN) $v_p\tau_p = 1.7 \times 10^4$ km; (C_2) $v_p\tau_p = 2.5 \times 10^4$ km. (All values are for heliocentric distance r = 1 AU.) The average value for $v_p \tau_p$ from recent results is equal to about 2 to 6 x 10^4 km, and if we assume the expansion velocity as derived from the radioastronomical detection of methyl cyanide v_p = 0.4 km sec^{-1}, then $\tau_p \sim 10^5$ sec ~ 20 hours.

Most important results have very recently been obtained by Delsemme and Moreau (1973) from the spectra of Comet Bennett (1970 II) who determined the profile of the C_2 and CN bands from the distribution of brightness of the emission perpendicular to the spectrogram dispersion. It was proved that the scale-length of CN as well as of C_2 varied with r^2; the scale-length reduced to r = 1 AU was found to be 1.4 x 10^5 km for CN, and 0.9 x 10^5 km for C_2. The corresponding values for the parent particle scale-lengths are: (CN) $v_p \tau_p$ = 5 x 10^4 km and for C_2 = 2 x 10^4 km. Delsemme and Moreau noted that the scale-length for parent particles grew with increasing heliocentric distance r somewhat less rapidly than would be expected. However, the increase in geocentric distance was almost exactly the same as the increase in $v_p \tau_p$, and the effect of the variation of space resolution on the determination of parents' scale-length in this case must be taken into account. Moreover, these results may be affected by the kinematical behaviour of CN and C_2 molecules because the measurements provide profiles across the coma along the radius vector Sun-comet only.

Even if the solid hydrates of gases (clathrates) in icy grains are the source of some observed molecules in comets, the problem of other prospective parent

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molecules remains substantial; and there is no reason for excluding them as possible constituents in the cometary nuclei. One of the arguments for the "clathrate" model arises from the short lifetime of parent particles exposed to the solar radiation field. However, the scale-lengths of hypothetical precursors have been derived from the photometric profiles of cometary heads by an inaccurate method. Moreover, the expansion velocities of parent particles cannot be directly described and the real value of $v_p \tau_p$ remains highly uncertain and can easily be underestimated.

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DISCUSSION

W. Jackson: I don't know whether I heard you correctly or not, but did you say that you could not explain the free radicals by photodissociation ?

J. Rahe: I think one can.

W. Jackson: Yes, because the lifetimes that you get from the photometry are of the order 10^4 to 10^5 seconds.

J. Rahe: Yes. Up to rather recently the lifetimes derived from cometary measurements seem to be rather short. Only after you had observations with high resolution, both angular and space reolutions, could you get a better determination of the lifetime of parent particles. This value increased considerably and the results of Delsemme and Moreau, Kumar and Southall, and Vanysek and myself are consistent.

W. Jackson: I see.

But the only point I was trying to make was that the lifetime measurements you are getting now are about the same as the lifetime measurements you would estimate for, say a typical parent molecule of CN.

J. Rahe: I think they are very close now. The new results obtained from high space resolution measurements are larger than follows from older observations. Low angular resolution and contamination of band measurements with the continuum background means most likely an apparent increase of the ratio ρ_r / ρ_p

G. Herbig: What is the reason that the polarization in the tail of comet Ikeya-Seki was negative ?

I didn't understand your explanation.

J. Rahe: I think Dr. Michalsky is here later. Isn't he giving a paper ?

You see, I received this paper only last night, and I didn't have any chance to check on this.

W. Jackson: Does anybody have a comment in terms of Dr. Michalsky's paper ?

J. L. Weinberg: The polarization reversal refers to a crossover from positive to negative polarization, i. e. , the electric vector flips by 90 degrees—

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from perpendicular to the scattering plane to parallel to the scattering plane. We have preliminary results for Comet Ikeya-Seki that I have worked out versus scattering angle and we find along the axis of the tail that the polarization at 5300 Å goes from plus 22 percent to minus 45 percent with the neutral point at around 125-1/2 degrees, and it falls very sharply across the line of zero polarization. It goes, for example, from 8 percent positive to 8 percent negative in 1-1/2 degrees of the phase angle.

We also found the history of the neutral point, that is the position of the crossover changes with time at the same color and it also changes with color. That is the neutral point moves toward the head (toward smaller scattering angle) with increasing wavelengths. The observations off the axis at different colors are still being reduced.

These results strongly suggest the presence of dielectric particles. (See contributed paper by Weinberg - ed.)

D. J. Malaise: I don't understand how improved space resolution can result in getting longer formation times for the observed radicals. It seems that if the true photometric profile is worked out near the center of the coma by the space resolution, you would get an upper limit for the time of formation and that any improvement in space resolution would lead to shorter times of formation. And I remember when I measured the profile of molecules in '65 on Comet Burnham (1959k). The resolution was between 500 km and 1000 km on the comet; this is the best space resolution I know of, just because the comet passed at 0.2 AU from the earth; in this case the formation distances of CN and C_2 were in the range of 2×10^4 km.

J. Rahe: What time did you get?

D. J. Malaise: I did not get time, I got scale length. It was about 20,000 kilometers.

J. Rahe: Oh, I see.

D. J. Malaise: And I still don't understand why you say that when you increase your resolution you get longer lifetimes.

J. Rahe: The lifetime of hypothetical parent particles of the observed radicals have been estimated from the observed photometric profiles of the cometary head and expressed in the scale length, ρ ($\rho = \tau v$; τ = lifetime; v = expansion velocity) in which the particle decay takes place. The scale length, ρ_r of the observed radicals can be determined with fair accuracy directly from the

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surface brightness decrease near the edge of the coma. But the scale length, ρ_p for the parent particles depends on the accuracy of the ratio ρ_r / ρ_p which can be derived from a comparison of the "flatness" of the observed photometric curve in the central part of the coma with the theoretical curve calculated, e. g. using Haser's model. This method is thus rather sensitive to the space resolution: measurements made with low angular and space resolution give higher values for the ratio ρ_r / ρ_p and, consequently, relatively shorter scale lenghts ρ_p.

D. J. Malaise: Yes, that is my second point. Haser's model does not describe any data close to the nucleus, so either the model is wrong or, I think, the model is too simple. I am very doubtful whether mean times of formation of free radicals can significantly be deduced by fitting a profile on both ends (center and edge of the head). The main reason is that in most cases the profile does not fit in the intermediate part. This is, in my opinion, due to the fact that the theoretical profile used for the fitting is based on an oversimplified model of the source. In particular, the yield of the source is assumed to be constant which is hardly tenable over periods of the order of 10^5 seconds. In fact, the shape of the profile is more likely to reflect the time variations of the source yield than the lifetimes of the particles. The profiles are usually quite asymmetrical and vary from day to day. The lifetime computations always rest on the assumption that we deal with steady state situations. This may of course happen but to my knowledge it is quite exceptional.

J. Rahe: Yes, you have to improve the model, there is no question about that as was pointed out in Vanysek's paper.

W. Jackson: I don't believe that.

I don't agree, because I think that Kumar and Southall took the data that you measured, here at Goddard, the monochromatic isophotes and fitted them both at the edge and at the nucleus.

They could get a fit only when they went back and reexamined the plates to remeasure the sky background. When they took out the sky background, they got a fit over the whole curve.

But you said, I thought, you could only get a fit at the ends—you didn't get a fit in between.

D. J. Malaise: My argument is that there is selectivity in the source of molecules which depends on temperature and depends on time.

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Of course you can get examples where the coma is symmetrical and you can get a fit over a range that is quite wide.

And if it is quite unsymmetrical a fit from the direction of the sun gives a very different scale length than a fit in the perpendicular direction.

J. Rahe: Well, for instance these results from Kumar and Southall for Tago-Sato-Kosaka showed rather symmetrical profiles, I suppose.

Voice: Yes.

J. Rahe: But Tago-Sato-Kosaka was a dust-free comet.

W. Jackson: So one would say that if you started out with a symmetric profile, of the radical, say, Haser's model worked pretty well.

But if you get a situation where you get asymmetry in the photometric profile, then you know that Haser's model is not going to work very well for that particular comet.

D. J. Malaise: My contention is that whenever I observe a comet, in most of the cases I get asymmetric profiles.

B. Donn: I would like to point out that you get better spectral resolution if you measure the profile along the lines the way Delsemme does with a spectrograph than you get with a filter.

On the other hand, you only get the profile in one direction and you do not know then if there is any asymmetry. This is a problem one has to take into account.

D. J. Malaise: Yes, but this is exactly what I said in 1965, you know. It was on high resolution spectra. When you are looking at long range, usually your spectra do not reach far enough unless your spectrograph is very, very fast.

A. H. Delsemme: For reasons of spectral contamination discussed in Delsemme and Moreau (1973 Astrophys. Lett. 14, 181) photographic plates exposed through properly chosen filters give poor values of the exponential scale lengths for the decay of the emitters of light. This explains why Kumar and Southall's adjustment of Haser's model remains poor, as demonstrated by their Table 1. Therefore their comparison given by Table 2 does not make sense because they put very accurate measurements of many spectra and poor measurements of one photograph on the same footing. Malaise has just reminded us why his early profiles also give poor values for the same scale lengths. This is because they were not extended to the outer coma, since the exposure times would have been prohibitive.

Because of the use of an image intensifier, a large fraction of the 81 spectra of Comet Bennett used by Delsemme and Moreau (1973) reached those distances in the outer coma where the decay of CN (or of C_2) is already very large, showing for instance slopes of -3 and more for the photometric gradient. This made the fitting of Haser's model extremely accurate and provided for the first time a reliable dependence on distance for the scale lengths in the outer coma, also helping a better assessment of the parent's scale lengths because they are interdependent in Haser's model.

We had also the problem of the small dissymmetry between the two photometric profiles, sunwards and anti-sunwards; this dissymmetry was rather easily discounted by using a very simple formula based on constant acceleration coming from the sun.

I wish to add a few words about the "activity" of the comets. The word "activity" conveniently hides our ignorance. This activity may come from some variation in the excitation from the sun, or from a variation in the production rates.

Sometimes we have, indeed, variations which show up as humps in the brightness profiles: They seem to be originated by time variations in the production rate of the comet. In principle, the expansion velocity of the humps could be measured by their displacements. So far, nobody has ever succeeded in identifying these humps from day to day, as Bobrovnikoff did for halos observed in Comet Halley.

For our observations of Comet Bennett, we had many days where the "activity" of the comet was not apparent, and the photometric profiles were very smooth. The observed profiles could then be accurately fitted to Haser's model with two parameters.

A serious disadvantage of Haser's model is however that the two scale lengths of the parent molecule and of the light emitter, can be switched because of the symmetry of the formula.

Everybody always accepts that the shortest of the two times is the parent's lifetime, and the longest, that of the dissociation of the light-emitter. This is probably true but has never been proved for each radical and its parent molecule. In particular I think of OH which seems to decay with a lifetime not too different from that of its probable parent: H_2O.

J. C. Brandt: I am not clear whether dissociation can explain the parent molecules. In my notes, I thought you said in your review it could not, but I thought you said in response to Dr. Jackson's question, it could.

There are a lot of arguments both ways, but since this is a crucial point, you know for the clathrate model, I would like somebody to state whether or not this is the actual situation.

Can photodissociation account for lifetimes observed for parent molecules?

J. Rahe: It seems according to this review, that lifetimes are very similar. Assuming of course, a spatial velocity, an expansion velocity.

And then this seems to be able to be accounted for by photodissociation.

J. C. Brandt: Professor Delsemme, do you agree?

A. H. Delsemme: Yes, I would like to add some more words in this respect.

I believe that the work which has been done in the laboratory is incomplete and therefore inconclusive.

The work of Potter and del Ducca was very important. It has been used so far repeatedly, but has never been reproduced; besides, they have never given the details of their integrations, and it is unclear whether they have neglected to include some predissociation bands of some molecules. Therefore, it has not yet been conclusively proved or disproved whether the observed lifetimes can be explained by specific parent molecules, in particular since we have the alternate hypothesis of the existence of a halo of ice grains.

W. Jackson: They didn't miss many molecules, but they did give one detail. They used for the absorption coefficient, in order to obtain the lifetime, only the absorption of the continuum.

As Professor Herzberg pointed out, (Trans. I. A. U., 12B, 1964. p. 194) you have to include the possibility of predissociation.

All of the large molecules, in all likelihood, will predissociate even though they may have a reasonably sharp band. If you integrate under the total band including the line structure, you end up with photodissociation lifetimes that are of the order of 10^4 or 10^5 seconds for cyanogen for example. HCN is particularly long, unfortunately. It is of the order of 9×10^4 second. Cyanogen is of the order of 1×10^4 second. And cyano-acetylene is of the order of 1×10^4 seconds.

I have recomputed them myself. And I will show those results Wednesday.

In answer to the question about which scale length you used, which one is the shortest, the parent or the radical, for C_2 and CN in all likelihood, the parent has to be the shortest.

The bond strength of CN is almost as strong as CO and N_2. There is no evidence from the spectra that CN predissociates in the region where it absorbs in the visible. So in all likelihood, those have a long lifetime.

There is a question about OH. You can get predissociation in OH in the higher rotational levels. However, these probably aren't excited, since OH has cooled rotationally before it can reabsorb.

Now on the other hand, some of the OH is produced in highly rotationally excited states from the photodissociation of H_2O. So some of those radicals may be lost through predissociation.

But that is still a small portion compared to the total amount of OH that is produced from the photodissociation of water.

So I think for most of the radicals it is a reasonable approximation that the radical has the longer lifetime as compared to the parent molecule.

The person who could probably answer that question best is Professor Herzberg, who is sitting back there, since he knows more about the spectra of radicals and molecules, than most of us. At least more than I do.

J. C. Brandt: So could I summarize the last 10 minutes by saying that photodissociation can account for the lifetime of the parent molecules, and in that sense the clathrate grains are not demanded as the source?

W. Jackson: Yes. That is true.

If you want to say that the clathrate model was needed to hold the radical, true. But that was not the reason the clathrate model was introduced.

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DISCUSSION (Continued)

A. H. Delsemme: I would like to straighten out ideas about the gas hydrates (clathrates) hypothesis, because it has been sometimes distorted in the literature, and this distortion has appeared in the present discussion.

The hypothesis was introduced by Delsemme and Swings (1952) mainly to explain why all molecular emissions appear at short heliocentric distances only. The explanation is that: most of the carbon and nitrogen compounds are imprisonned in the lattice of the water clathrates, and are liberated in proportion of the vaporization of water. The hypothesis is reinforced if thermodynamic equilibrium is reached within the nucleus, because the clathrates are implied by it. Delsemme and Miller (1970) have also shown that the clathrates are the limiting case of gas absorption in water shows, and cannot really differentiate from gas absorption. Of course, in order to have clathrates, a prerequisite is to have large amounts of water. Delsemme and Rud (1973) list eight different arguments proving that water is a major constituent controlling the vaporization of several comets. This is easier to defend since the discovery of H_2O^+ in Comet Kohoutek and H_2O in Comet Bradfield. Now, an entirely different line of arguments stem from Delsemme and Wenger's (1970) laboratory experiments on clathrate ices. They have shown that the clathrate-hydrate of methane comes as a granular powder. When it vaporizes in vacuum, the vaporizing gases drag some of these grains away from the main body of snow From their sizes and velocities, the building up of a halo of icy grains surrounding the cometary nucleus is predicted. Delsemme and Miller (1971) propose that the extended source of light emitters deduced from photometric profiles, could come from the size of the icy halo, and therefore does not give any information whatsoever on the parent molecules, that are therefore not detected through the photometric profiles. This proposal is shown to be consistent with the photometric profiles of the C_2 emission and of the continuum of Comet Burnham. The existence of the parent-molecule is not really disputed, but their scale lengths are not automatically given by the photometric profiles and the use of Haser's model. Another approach which seems to suggest the existence of the halo of ice grains comes from the work of Delsemme and Moreau on the dependence on heliocentric distance of the assumed scale length of the parent molecules of CN and C_2, which vary, not like parents should vary (inverse square law) but like the icy halo should vary (simple inverse law).

Now, there is still room for some leeway. The scale lengths of the CN and C_2 parents are not yet very accurately known, their dependence on distance still is disputable and may come from other causes. Only comet Burnham was used to check the existence of the halo of ice grains, and some numerical coincidences although difficult to justify are not to be totally excluded. Other comets (like Bennett) often are too dusty to be used for the same purpose. However, the ice grain hypothesis has also been used with success by Sekanina to explain the tail orientation of the faraway comets. To summarize, the hypothesis of the halo of ice grains has not yet been sufficiently confirmed, but seems to stand on a firm basis. The ice grains must not necessarily be made of clathrates. Gas absorption on snowflakes of water would give the same result. Pure water ice grains would also satisfy Sekanina's explanation of the tails, as well as the photometric profiles of the continuum if we have an approximate numerical coincidence of the radius of the ice grain halo and of the scale length of the C_2 parent, near 1 AU.

D. J. Malaise: Yes, I would like to remind you that what we measure is scale lengths and what we are discussing is lifetimes, and both are independent variables. If the molecule doesn't move in a straight line, that is if collisions increase the time spent in the inner coma, there is not a simple relation between scale length and lifetime.

W. Jackson: Right.

It can collide many times before it gets into free flow.

D. J. Malaise: This could explain the observation that the photodissociation times do not agree with laboratory data.

W. Jackson: I disagree.

Because, what happens is, if you get enough collisions near the nucleus, you quickly go into a fluid flow. And once you have the expansion into the vacuum, the the stream lines pull everything off, and it doesn't take long before the random motion is converted into directed flow.

So it really doesn't make any difference. If you raise the pressure higher, you are not going to appreciably change the residence time of the parent molecules, even though that was what I said a long time ago.

At least that is what I thought a long time ago.

D. J. Malaise: You have hydrodynamics where the molecules are produced and you have free flow where the molecules are observed. You don't know how it goes from one to the other.

W. Jackson: Once you get the hydrodynamic flow, the stream lines are the same as the free expansion. I would say you go from hydrodynamic flow to free expansion in a very continuous fashion.

You don't go into a discontinuous situation.

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M. K. Wallis: I would agree with Dr. Jackson, that in simple hydrodynamic flow the velocity quickly reaches that of uniform radial expansion (within 100 km). Except, that if you have photodissociative heating, or some other heating process such as collisions with dust from the nucleus, the pressure can be increased enough to reduce the velocity of the gas, the hydrodynamic flow near the nucleus and you will get — it seems backward, but you will get lower velocities nearer the nucleus than you would get further out on the free molecular expansion.

So you still have to be careful. With no heating, you are right. The velocity soon gets up in the hydrodynamic flow — say within 100 km. But if you have got heating, as we all believe, then you can't stay up any longer.

W. Jackson: Well, if you have got heating, are we talking about a 10 percent change in the residence time of the parent?

The point is that when you get heating, you have got the daughter formed already because you can only get heating through the photodissociation. There is a possibility of getting heating by some other mechanism that I -

M. K. Wallis: Photodissociation, or collisions with the dust.

W. Jackson: Or, some people might argue for electrons heating up the molecules by the plasma coming in.

I don't understand that, so I am not going to get into that.

M. Dubin: Let me ask Jurgen Rahe a question that relates to this.

I thought I heard you say about comet Kohoutek that it was a normal comet. I didn't finish the question yet, because I want to know what a normal comet is.

You further indicated that the comet, in terms of its general brightness after perihelion in mid-January, late January was 10 times dimmer than at the same distance from the sun before perihelion. And that is a normal comet?

You indicated that sodium was observed later, only on a few days, for this normal comet. And then you also indicated that you thought that the early observations were a result of a swarm of dust evaporating.

And as we heard also in terms of the clathrate argument, a swarm of particles would change the nucleus model from the solid nucleus considerably, and these same particles have already been reported to probably be vaporizing to try to explain the anti-tail on the comet.

So please, would you explain what a normal comet is?

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J. Rahe: First I should like to point out that when I said normal I was just reading the paper by Dr. Vanysek. And doing justice also to the question Dr. Brandt asked I think I should perhaps later, make a Xerox copy of his statement and distribute this. But this question probably wasn't quite clear due to my German English, not a question of text.

Now the question, what a normal comet is, of course, is very difficult to answer. But the brightness development of comet Kohoutek was not so surprising if you look back now.

(Laughter.)

I don't know whether you read one article in Sky and Telescope written by Dr. Jacchia. He compared the brightness development of comet Kohoutek with many other comets, and compared with those comets, Kohoutek didn't do really that badly.

M. Dubin: But compared with many other comets it did do badly. There are a class of comets that did do better than Kohoutek. But, which is the normal group?

J. Rahe: Yes, this is very difficult to say.

M. Dubin: Now what does this do to your model here?

J. Rahe: According to what Dr. Vanysek says here, he seems to change the nuclear model of Dr. Whipple a little bit. But the thing might be easier if you comment on it, Whipple, because it is very difficult to change a nuclear model just from these observations of comet Kohoutek, I would say.

F. Whipple: I don't think I have any special comments on that point. I am not exactly sure what he did to the nuclear model and — just from mere reading of the papers, I don't believe I can comment on that.

I think that since I first visualized it, the only real change has been the introduction of the clathrates, and otherwise the whole picture is very much the same.

But then we have the chemistry which is very involved.

But I am not sure how he changed it, so I don't think I can answer the question.

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J. Rahe: He is talking about the clouds of dust particles surrounding the nucleus of the comet. And this cloud is supposed to be responsible for the large brightness of comet Kohoutek from the time of discovery, until the beginning of October.

M. Dubin: I raised that question, because I say that it has already been observed that probably the cloud of dust particles around the comet were not a simple cloud, they were vaporizing or sublimating. They are also possibly clathrates and this would change the distribution of the parent molecules in the coma region, which in turn would relate to the discussion we have had in the last few minutes.

J. Rahe: All the measurements Dr. Vanysek was referring to in his paper were made when the comet was much closer to the sun. These observations were made the beginning of January, and he is talking here about the dust cloud here until the beginning of October, October and earlier.

And what you said about this sodium observation, we were able to observe sodium emission, (observations made in Chile at the European Southern Observatory) up to about 0.7 astronomical units — but not further out.

This is very common behavior, that you observe sodium closer to the sun and not further than 0.7 or 0.8 astronomical units.

H. U. Schmidt: Dr. Dubin just said that Kohoutek behaved differently than some comets. What photometric observations do exist for a new, undisturbed comet with a small q (perihelion distance) at large distances before perihelion besides Kohoutek? It seems to me that comet Kohoutek showed just the photometric behavior which is in line with a statement by Oort in his theory 23 years ago. He stated that a genuine new comet must have a chance of being detected which is much larger than its chance to be detected at later returns. He concluded this directly from the statistics of very small values of 1/a. Since Kohoutek was much brighter before perihelion than afterwards, it followed this prediction by Oort.

DISCUSSION

M. Dubin: In reply to Dr. Schmidt's point – I was just questioning what a normal comet was, initially because of the brightness difference observed of about a factor of 10 as reported by Vanysek and Rahe.

There is, in fact, one observation that has been made and reported earlier, that showed an anomaly on Kohoutek. This was reported at the Huntsville workshop and further defined by Page, Carruthers and others, on the hydrogen emission of the comet.

They find that comet Kohoutek had possibly an explosion of hydrogen in early December, where the rate of generation of hydrogen was considerably greater in that period than even at perihelion.

Now I don't understand comets too well —

(Laughter.)

— but that is why I asked the question.

E. Ney: I am a little amazed by the factor of 10 before and after perihelion of Kohoutek, because within one astronomical unit, all the visual and infrared observations do show that it is dimmer after perihelion passage, but only by about a magnitude.

So the factor of ten if it does exist must be at more than an astronomical unit, or it could be because of the diaphragms that were used, or that the reduction wasn't exactly the same.

I only know of a factor of two to three that Kohoutek changed before and after perihelion, which was also the same amount that Ikeya-Seki changed.

J. Rahe: This observation was made by Kohoutek in a B filter with an aperture of 80 arc seconds.

E. Ney: At what distance from the sun?

Voice: It must have been about one astronomical unit, but it takes a very short time, I found that out.

D. A. Mendis: I would like to make a comment regarding the time scale and the length scale.

If you have a distributed source model rather than a simpler source model, whatever way the distributed source is brought about, either by a clathrate model, with icy grains around it, or in the form of a cluster of grains, then the time scale,

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or the length scale that you measure must necessarily be larger than the dissociation length scale.

It will mimic a much larger dissociation length scale. In other words, the scale length is almost completely meaningless in such a situation.

You see, because you can get the molecules out to 10^4 kilometers before they are released from the grains.

B. Donn: We have some preliminary results on this distribution of parent molecules, it seems to me, from the radio observations which have detected them. As I see these observations in the case of the water in Comet Bradfield, the evidence is that the water is concentrated toward the nucleus.

There is a problem here that it may be not just a density, but also an excitation process, and this we have to work on trying to understand.

The same thing, as I recall, is true with the methyl-cyanide, that is if you move your beam off the nucleus a short way, then the density, or at least the antenna temperature drops.

And this observation suggests that from the signal we are getting, the excited molecules are very closely concentrated to the nucleus.

Unfortunately both the water and the methyl-cyanide we observe in excited states, and therefore there is an excitation process that needs to be interpreted.

I would like to say one other thing about a somewhat different aspect of this problem, and that is that for comets at distances beyond 3 AU, almost the only features we have observed are the continuum; the emission spectrum has not been strong enough to be detected, I think with the exception of one that Dr. Dossin took of comet Humason and that was a strange beast anyway.

And so we have the problem that at large distances any comet, at 3 AU and beyond — in the case of Halley at 3 AU and the outbursts of Schwasmann-Wachman at 5 AU, all that has ever been observed is a dust continuum in which we have seen the Fraunhofer spectrum of the sun.

So it is generally characteristic at large distances that we are seeing dust, no matter what the comet is.

Now among the important pieces of observational data that are missing are intensive studies of these comets at larger distances to get data on the spectra, and on the luminosity variations at large distances so we can have more data for

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interpreting the sort of work, for example, the Marsden and Sekanina have been doing on these distant comets.

These are observational data that we need to get, and I was just thinking of the comet observers, could they put more emphasis on these faint distant comets—okay, you don't get as much data, but it would be very important for studying the evolution of the nucleus as it approaches the sun.

W. F. Heubner: I would like to get back to the question of: what is a normal comet?

First of all, I think we agree that the brightness of the comet drops by a factor of ten in the visual from before to after perihelion.

However, in the infrared, I think the drop is much smaller. The reason for it is that the particles which scatter the light in the infrared are the larger particles, and they happen to hang around the comet nucleus for a longer time. You are quite right when you say that you probably only see a brightness change of a factor of two in the IR. That comes from the scattered light on the solid particles, the largest solid particles.

In the visual we see molecular emission from fluorescence and the scattering from the smaller particles superimposed. Here the change in brightness from before to after perihelion can be much greater.

Secondly, I would like to comment on the brightness of the comet at very large distances: it seems to me that if one wants to have a cloud of particles around the nucleus at very large distances—and we are talking now distances of Jupiter and further out—then one needs a shell of gas, of frozen gas on the outside, which is extremely volatile. Something like CH_4 that can propel these dust particles to form a coma (unless they happen to be already in a coma, which I don't quite believe).

CO does not seem to be a likely candidate. It does have the right latent heat of vaporization, but one would then have to see a tail. But CH_4 does not leave a tail, at least not a visible one. I think it is likely that there was a shell of frozen CH_4 on the outside of the nucleus. Whether that makes it a normal comet or not, I don't know.

F. L. Whipple: For those that haven't lived with comets for so many years, I think the observations of the orbits that probably will be discussed later by Marsden and Sekanina are very important here.

The peculiarity of these comets, ones with extremely long periods and perihelion distances out between 3 and 5 AU, is that they seem to be

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coming in new; you don't find the returned ones. They should be coming back with much the same orbits with somewhat smaller perihelion distances, which indicates very strongly that the first time a comet comes in there is this outer layer of methane or whatever, that throws out dust and gives it this halo to make the comet bright at the distance of 3 to 5 AU.

But the comet loses it after it makes one passage, and as Marsden points out, there is a real dearth of these comets that come in with somewhat shorter orbits. There should be a lot of them returning if you look at it statistically over the eons. And you have comets with extremely long period orbits, going out to 20- 30,000 AU.

Z. Sekanina: I would just like to say that a study of the tails of the distant comets as I made about a half a year ago, suggested that there is a very strong evidence that the activity of these incoming comets of large perihelion distance, tends to decrease as the comet approaches perihelion, which is something that would be normally regarded as very un-normal.

It certainly is not valid for typical comets of a small q.

Now there is an indication that the comets are more active, up to as far as I would estimate 10 to 15 AU before perihelion, and as the comet approaches the perihelion, which in this case is on the order of between 3 and 5, the activity already starts dropping down.

The evidence is extensive from the tail.

From the orientation of these tails, you can rather confidently say at what time the tails were formed, and for all the directions that a tail would cover corresponding to ejection times near the perihelion, there are simply no tails in those directions, while there is a beautiful tail in the direction corresponding to ejection at 10 to 15 AU before perihelion.

W. Jackson: Are you saying that the orientation of the dust tail is an indication of when the activity started?

Z. Sekanina: That is right.

W. Jackson: And from a study of the orientation of the dust tail, it looks like the activity started at large astronomical distances.

Z. Sekanina: That is right and one sees the tail production decrease as the comet approaches perihelion.

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Voice: This is a question to Sekania.

Do you mean today that you observe comets at 10 to 15 astronomical units?

Z. Sekanina: Not necessarily. We observe comets at, say, 4 AU and these have tails, and we observe that they are straight and you can say, under simple assumptions, from the direction that the matter was released at 10 to 15 AU.

Voice: So you deduce the activity between 10 and 15 AU from these tails that you observed orientations of.

J. T. Wasson: I would like to speak briefly to the point raised by Huebner about the possibility of methane ejecting the dust.

It is improbable that pure methane is an important cometary constituent. It is highly volatile and condenses out of a nebula having a pressure of 10^{-3} atm at temperatures of 40°K.

If comets formed within the solar system and temperatures were as high as or higher than those at present, methane would not have condensed inside 30 AU.

It is possible to condense methane far away from the sun, out in the Oort cloud, but then you run into problems of condensing out very much material at the low gas density there.

And so I think you are probably going to have to look for a volatile other than methane to eject the dust.

Secondly, I would like to ask how well you know that there is no CO^+ at 5 AU and beyond?

How accurate are the observations and what kind of limits can one give on carbon monoxide abundance?

W. F. Huebner: I think if CO were present then we would have seen it in the tail just as we saw it in Humason, and as far as the CH is concerned, I don't mean to imply that this is the only possibility.

Certainly there are other molecules which do not radiate in the visual, and are highly volatile. I just mentioned that the CH_4 might be a possibility.

But I do disagree with you about the condensation temperature. We have made calculations which indicate that CH_4 will condense out at about 100 degrees.

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Voice: Is it pure or as a clathrate?

W. F. Huebner:

It is pure, that is right. In a mixture of other gases.

W. Jackson: I would like to make a comment about whether or not you would observe CO^+ at 5 AU. Humason is probably one of the most unusual comets rather than a reasonably normal comet, because the amount of material that had to be produced to observe that amount of CO^+ out there would suggest that comet Humason was an extremely large comet.

Now a "normal comet" with a radius of the order of 5 kilometers, would not show any CO^+ or any molecular emission unless you use very sensitive techniques.

You possibly — I don't know, with image intensifiers, maybe there is a possibility of observing something way out there, but I think it would be extremely difficult.

E. Roemer: I should like to call attention to the fact that comet Sandage, 1972h, is currently observable with an asymmetric coma 25 arc sec (100,000 km) in radius at a heliocentric distance of 6.8 AU. The perihelion distance is a bit beyond 4 and the magnitude of the nuclear condensation is about 20. A photograph from the September 1974 dark run will be shown tomorrow as part of my review presentation on astrometry and luminosity.

B. Donn: I will make a brief comment here on the place of origin of comets and will discuss it in more detail in the session on comet origins. It is not necessary to assume that comets could only form within about 30 AU of the sun because the density of matter was much to low everywhere else. We know that stars tend to form in clusters and it may be that is the only way they can form. If the sun originated as a member of a cluster there was appreciable density over a volume of several parsecs. Within 50 to 100 thousand AU of sun small sub-clouds may well have had densities large enough for bodies as small as comets to accumulate in the available time. Cameron has published a scheme of interstellar comet formation in sub-clouds that separated from the collapsing cloud which formed the sun. In his theory comets could readily form in such regions. By means such as these comets may have formed in the region of the Oort cloud.

L. Biermann: In connection with this question of the relative merits of CH_4 and CO for explaining the appearance of comets at very large distances from the sun, beyond 5 AU, I would just like to make reference to a paper which I gave last year at the conference in Barcelona. I started using some figures given by Arpigny on the emission rate of CO^+, about ten years ago, and I used laboratory work on ion-molecular reactions to show that CO was likely to be produced in larger quantity than I would adduce from the CO^+, largely because a sizeable fraction of the observed CO^+ is removed, not by flowing out into the tail, anyhow in a fairly typical, ordinary comet, the typical bright comet, but by being transformed into different kinds of ions.

And so the observed intensity of the CO^+ might be misleading concerning the quantity.

Now I suppose at some later sessions we could take up this question; maybe some of us can get together and make an estimate for showing — my feeling is, what was expressed already — what you would have to expect from a typical comet, at distances beyond 5 AU would turn out to be invisible as a CO^+ tail; it would turn out to be invisible with available optical means — anyhow. For past comets you would not expect to have seen anything. For future comets, using advanced techniques, things are, of course, different.

H. U. Schmidt: I have a short question to Dr. Huebner. You spoke about a thin layer of volatile material formed at lower temperature than in the solar nebula which would be needed to produce the bright image of Kohoutek at large distance before perihelion. Would you think it is possible that this thin layer might have been formed on an old nucleus originating in the solar nebula but suffering additional accretion in interstellar space at lower temperature for a longer time?

W. F. Huebner: I think I would agree with that in general, that one can accrete highly volatile materials. I'm not quite sure where or what they would be, but it wouldn't be very much. And I think the amount would have to be significant if one really wants to account for a large dust coma around the nucleus at that distance.

(Simultaneous discussion.)

F. L. Whipple: It was part of my comment. I didn't want to enlarge on it, but —

W. F. Huebner: It's thin, but it must be big enough to bring out the dust.

F. L. Whipple: I think that Bert Donn and his group and I have been thinking about the same sort of thing, that if you do make these comets at

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great solar distances early in the game, you are probably in an extremely dense nebula, and there's no reason why you couldn't get a large accumulation of interstellar dust there that might amount to say a meter possibly, but certainly centimeters, which would probably be enough to do this for a one time affair as the comet comes in to perihelion at, say 4 astronomical units. I think that's the whole point. Or we might have gone through a dense cloud later on at a relatively low velocity, and collected a lot of material.

At the present time you wouldn't collect enough. If you have one hydrogen atom or a tenth per cubic centimeter you're not going to collect a significant amount, but in these dense clouds, which we might have gone through when the solar system originated, comets at great distances could have accumulated a lot. It might have been a part of the original accumulation of the nucleus.

B. G. Marsden: With regard to this question of normality of comet Kohoutek, I can think offhand of four comets, three other ones in addition to comet Kohoutek, which seem from the orbital evidence to be new, although we never know for sure. They just seem to be coming from large distances. But of course, the perturbations over several times around could throw them back so that they still appear to be coming from those distances.

Among these four comets also they all have small perihelion distances and all were observed for some time before perihelion.

In addition to comet Kohoutek they are comet Arend-Roland (1957 III), comet Cunningham (1941 I), and comet Seki-Lines (1962 III). Now, as of last October, when comet Kohoutek was now only 2 astronomical units from the sun, it was very easy to compare it with comet Arend-Roland. The comets look very similar. Comet Kohoutek seemed to be then intrinsically a magnitude brighter, and so that is why confident predictions were made that it would be a bright object in January.

It seemed very reasonable to compare these two comets, the only difference being comet Kohoutek did go rather less than one half the distance from the sun than comet Arend-Roland did.

Comet Arend-Roland behaved perfectly well, comet Kohoutek seems to have been, I would say, about 1-1/2 magnitudes fainter after perihelion than before, and I don't see any way in which that could have been predicted.

Comet Cunningham, which Dr. Jacchia mentioned in his paper in Sky and Telescope, is often labelled as the bête noir among comets that have failed. Again we had somewhat the same problem that two ephemerides were provided, in this case one going according to an r^{-6} law and one r^{-4} law, and everybody

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forgot about the r^{-4} law as time went by, and were still expecting comet Kohoutek to be -10 at perihelion, just as they were expecting comet Cunningham to be very bright.

Indications are, in the case of comet Cunningham, that it was very little fainter, a very little amount fainter after perihelion than before. It was badly placed for observation. There was only one observation made in Argentina.

And finally, comet Seki-Lines. This comet had the perihelion distance of 0.03 of an astronomical unit, and it seems from the orbital evidence, again, to be a new comet. This again, was a perfectly spectacular comet after perihelion, in spite of going so close to the sun; it doesn't seem to have suffered at all.

So I would deduce from these statistics that comet Kohoutek is in the 25 percent minority among normality of comets.

(Laughter.)

D. D. Meisel: I'd like to follow up what Brian said about the normality of comet Kohoutek.

Now, observations that Bortle and Morris gathered on comet Kohoutek show that before perihelion and after perihelion the heliocentric distance index was 2.5 in both cases, but that Kohoutek on the average, by the least squares solutions, dropped one full magnitude after perihelion.

Now, there are only about 15 percent of all the sample of comets that have had reasonable photometry done, which have indices around 2.5. So in that way, Kohoutek, at least photometrically in the crudest way we know, which are the comet magnitudes, was normal for this group with very low heliocentric index.

But again, it's only a sample of 15 percent, so how good's the weatherman?

M. A'Hearn: I just wanted to offer a further comment on the question of the normalcy of comet Kohoutek, in two respects. The first is, that if you look through the abstract booklet, at least one of the abstracts of papers that's not being presented (by L. Brown) indicates that Kohoutek underwent flaring; we've also heard about the flaring in hydrogen. I, too, have photoelectric data (A. J. in press) that indicates, for example, that it underwent a flare at least in the C_2 band on the first of December, lasting through the 2nd of December, and it may be that Kohoutek has undergone a large number of these flares, which would make it rather difficult to interpret any of the photometric data in terms of simple pictures of the production of the molecules.

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I also wanted to comment on the pre- and post-perihelion differences. My photometry indicates that in the C_2 Swan band ($\Delta\nu = +1$), the fluxes are the same before and after perihelion to within a factor of 2, although the visible continuum is way down. So the continuum in the optical is down a great deal, perhaps as much as a factor of 5 or 10, but the C_2 band is comparable to what it was before perihelion, provided you use diaphragms that isolate approximately the same linear area in the comet.

H. Keller: The observation of Skylab and the rocket observation of the hydrogen production rate indicate that there is a factor of about 3 in production rate in the difference; that means the production rate was about a factor of 3 higher, preperihelion than after perihelion at a typical distance of 0.5 AU. That alone can account for the 1.5 magnitude difference in visual brightness.

W. Jackson: But evidently if the production rate of hydrogen went down, the relative production rate of the parent that's responsible for the C_2 must have gone up, if the brightness is going to remain the same pre- and post-perihelion.

So that, again, indicates some variability in at least the layering of the comet.

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