THE COMA: PANEL DISCUSSION

D. Malaise

Well, I just said that I disagree with nearly all that was said in the review paper. Now I have to illustrate this point a little, so I will first write something on the board.

What we are looking for in the coma, in fact, is data about the nucleus because this is the only important part in comets, and the coma is something evanescent. So what we are trying to identify by observing the coma is essentially what I would call a "source function" - not a source. This function represents the intensity of the source in number of molecules of species M emitted per second in the direction α, ϕ and with the velocity v, per steradian and per unit velocity. It can be written : $I(t, \alpha, \phi, v, M)dtd\Omega dv$

Then, in order to identify and build up a good model of this source, we have just observation of a few fragments that we happen to see in the coma. And we just try to identify some characteristic of the source by making a model fitting. That means that we are putting a lot of ourselves, of our thought, of our dreams, between the observation and the result.

Starting from the nucleus, what we need first is hydrodynamics, then we need chemistry near the center with all the physical data which enter into it. And then we need what I would call physics when we are sufficiently far away from the center, and have to deal essentially with photo-dissociation and radiation pressure.

Now, this is an extremely complicated situation, the models on which conclusions are drawn concerning the source are utterly simple. Of course, science is not simply materializing your dreams ; it consists rather in solving contradictions.

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And now I would like to illustrate some contradictions between the accepted picture and the observations. These are, by no means, exceptional cases; on the contrary, I hardly see any case that fits the simple image of photo-dissociation processes to build up the coma.

Fig. 1 is extracted from my early work (Malaise 1966). It shows the log-log diagrams of the photometric profiles of comet Burnham. Four radicals were observed on five nights. The night number and the corresponding heliocentric distance is indicated at the left of the figure. The plain lines are the profiles in the direction of the sun and the dashes are on the tail side. I have to point out here the distance scale : this comet passed very close to the earth (about .2 a.u.) so that the resolution was very good : 500 km for 1, 3 and 5 and 1000 km for 6 and 9. But we observe here only a very limited part of the inner coma, something between 1500 and 7000 km, that is well inside the production zone of the radicals.

If we consider the symmetry of the profiles, viz : the relative intensity of the sun and the tail sides, we notice large differences : CH is in general quite symmetric, CN is symmetric except on the first night; but C_2 and C_3 are very dissymmetrical and this dissymmetry varies rather fast. On the fifth night both C_2 and C_3 are 25% brighter on the sun side, while the next night both C_2 and C_3 are about 30% dimmer on the sun side. The relative variation of C_2 and C_3 is markedly parallel, while there is no correlation with the variations in the profiles of CN and CH. These large intensity variations are clearly due to variations in the number of radicals in the line of sight. This cannot of course be explained by a steady state model. It could be explained by a source whose strength, ejection velocity, angular distribution and composition (relative amount of species) varies with time. But of course it is not clear whether we have to trace back these observed variations all the way down to the source.

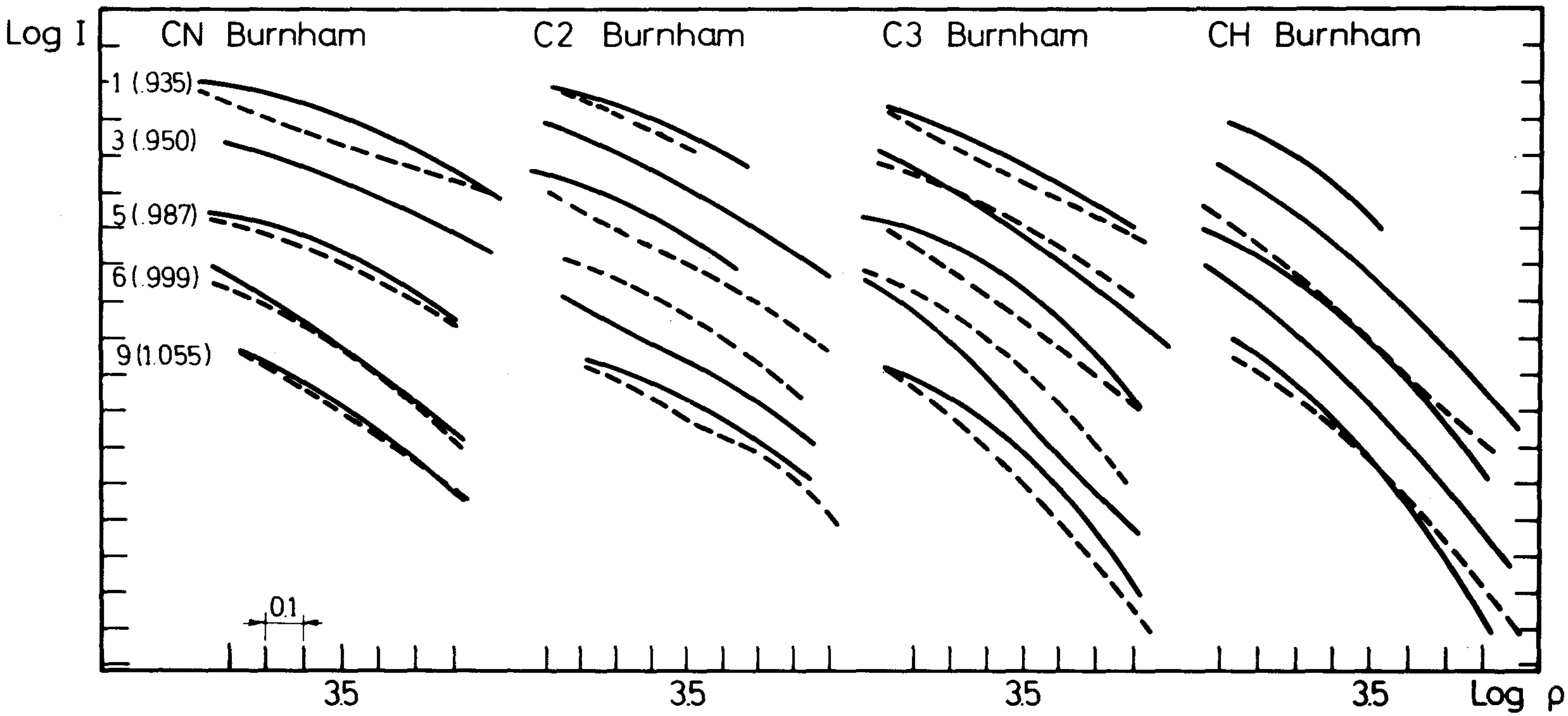

Figure 1 : Log-log photometric profiles of comet Burnham. Intensity in arbitrary units.

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Another thing you may notice here is that the gradient, if you insist on finding a mean gradient in these curves, is rather constant with heliocentric distance for C_2 and CH, but for CN and C_3 it shows a trend towards increasing when the distance to the sun increases. This very fact, that you get a flatter profile when you approach the sun is at complete variance with any model where the production of the radical is by photodissociation with a source which has a constant strength. So this can be due either to the fact that we have wrong physics in the model or to the fact that we have the wrong source.

These are not small effects and I must stress that Burnham was a very quiet comet with no dust, faint tail and a nice symmetric amorphous coma.

Now I want to show you a more recent observation of profiles of Comet Bennett (fig. 2). These were done with a photo-electric photometer, six channel polychromator. That means that all six channels are taken at the same time, so that not only can I compare the bands within themselves, but the position of the bands with respect to each other are completely respected.

These are log-log graphs and you have the tail side of the comet on the top each time, and the sun side of the comet at the bottom. On the 6th the angle ψ between the radius vector and the photometric cut was 45° ; on the 9th ψ = 16° and on the 10th ψ = 17°. On the 10th the diaphragm had a diameter of 4000 km ; on the other dates it was twice as large. The ordinate scale is absolute: for the continuum it gives the intensity in ergs cm^{-2} s^{-1} A^{-1} $sterad^{-1}$ while for the bands the intensity is integrated over the band.

These are a few examples to illustrate further my point about the activity of the source : you see that the profiles are not symmetric and that practically we never observe

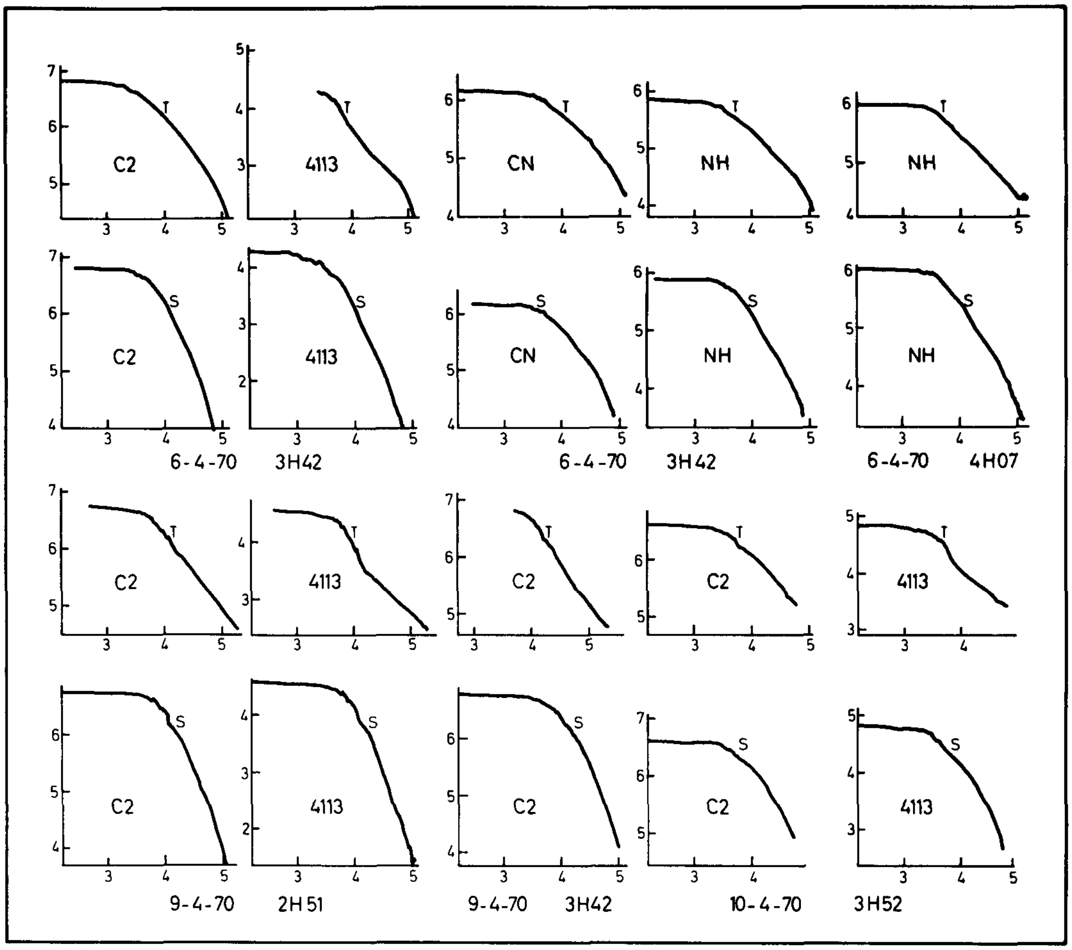

Figure 2 : Log-log photometric profiles of comet Bennett. Intensity in absolute units. T : tailward side, S : sunward side.

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an expansion zone with a constant slope equal to - 1. In the model of photodissociation, this is typical for a ratio of lifetime which is not much smaller than 0.1. That means the lifetime of the parent and the lifetime of the observed radical are not more than one order of magnitude different. If we turn to the continuum, you see that on the tail side, the profile shows a typical concavity. This shape is given by no model based on a constant source of dust escaping at constant velocity : this always gives a kind of expansion zone which has a constant slope on a certain region, but the slope never increases. What we observe here, on the tail side is the formation of an envelope. On the sun side, the continuum has a steep nearly constant slope (- 3). This high gradient is also difficult to explain with a simple theory. If we compare the continuum on the 9th and on the 10th, we see that the envelope has shrunk on the tail side, but on the sun side we have now a profile typical of a molecule. This case is unique in our observations.

We notice also that the intensity of C_2 has slightly increased (1%) from the 9th at 2h.51 to the 9th at 3h.42 and then has dropped by 2.7% on the 10th at 3h.52. At the same time, the continuum has first increased by 5% on the 9th and then has kept the same value on the 10th. Observe also how the central intensity of NH varies on the 6th between 3h.42 and 4h.07 (25 min !) thereby changing completely the shape of the profile : no doubt that if one should try to deduce time of flight for these two profiles, the resulting values would have astonishingly different values which would certainly be related to nothing else other than the activity of the source. At any rate, we observe a highly variable behavior among these profiles. We nearly never observe an expansion zone in the three radicals, and when some part of the diagram has a constant slope, this slope is not 1 ; it is rather 1.25 or so and it varies from night to night. But any simple

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model gives a slope equal to 1.00 to the second decimal. The best one can suppose is that the intensity of the source varies with time. But if one supposes this (and one is lead to this assumption when one sees these observed profiles), all the conclusions about the time of flight and the scale length are becoming doubtful, because you can change a profile in any way, and you can simulate any time of flight just by varying the strength of the source with time.

Fig. 3 illustrates something I found with the six channel photometer. As I told you, I scan the head of the comet with a small diaphragm by scanning the telescope. Behind the diaphragm I have a concave grating and six exit slits for the bands. So the six channels correspond exactly to the same part of the comet and the displacement of the profiles with respect to each other are meaningful. The left part of fig. 3 shows the displacements of the center of luminosity of the bands for the different dates. Remember that the scans of the 6th were made at an angle of 45° while the others were made at 16 or 17° of the radius vector. The sun is to the left of the figure. You see that NH is always relatively displaced towards the sun ; the splitting of the continuum is not real ; it shows you the uncertainty in the displacements. CN and C_2 on the other hand are systematically displaced towards the tail.

The right part of fig. 3 gives more details about these displacements for the night of the 10th. Each curve corresponds to a different radical or to the continuum ; it represents the central point of the isophotes, so that the higher the point in ordinate, the brighter the isophote to which it corresponds and the lower part corresponds to low isophotes, that is to parts of the comet which are far away from the nucleus. The ordinate axis has been made to coïncide with the position of the center of luminosity of the continuum. The scale in thousand km is the distance of the corresponding isophote along each curve.

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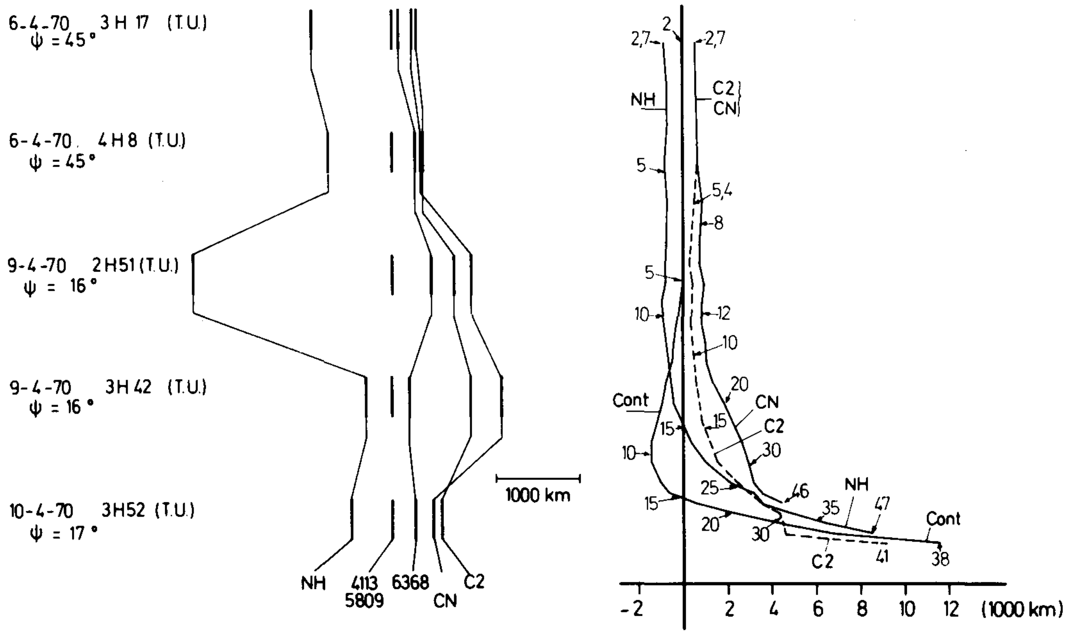

Figure 3 : a. (left) relative displacements of the center of luminosity of the radicals with respect to the continuum for comet Bennett.

b. (right) relative displacements for various isophotes (distance of isophote in thousand km indicated by arrows). Center of luminosity of the continuum has been taken as reference.

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The top of the figure corresponds to the brightest part of the comet, near the center. One sees that as close as one comes to the source i.e. 2700 km for the radicals, there are displacements of the isophotes relative to the reference. For NH, this displacement is 800 km to the sun and for C_2 and CN it is 600 km to the tail. These displacements are constant to a distance of about 5000 km (C_2), 7000 km (CN) or 10.000 km (NH). Then the center of the isophote is slowly pushed back to the tail for the three radicals. Off hand, only the fourth model of Haser (Haser 1966) can account for this general behaviour. It consists of a lambertian source ejecting molecules in the direction of the sun with a velocity function which is gaussian about a mean speed v_0 ; the molecules are pushed back by the radiation pressure and are finally destroyed by photodissociation or a similar process. Note that a maxwellian distribution of velocity would not fit since in this case the center of the lower isophotes is never pushed back to the tail. The gaussian case does not fit very well either since in this case an envelope is formed in the direction of the sun ; but it is possible that our profiles do not reach the envelope. In any case, if we take the initial velocity to be the same for the three radicals, we can compare the distance by which each radical has been repelled for the same isophote ; these distances should be roughly proportional to the acceleration. For the 24000 km isophotes, these distances are 1890 km (CN), 2870 km (C_2) and 2950 km (NH). These figures do not fit the know values of the acceleration. At any rate, we have to infer from these observations and from the Haser model that the source coincides with the luminosity center of C_2 and CN or that it lies on the tail side of it. In the former case, the source of these two radicals should be isotrope (and not the source of dust and of NH). In the latter case, all sources should be lambertian-gaussian. In this case, it is noticeable that the displacement of the continuum is larger than that of C_2 and CN which means that the acceleration of the dust is much lower than that of the radicals.

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The shape of the center of the isophotes for the dust is very special i.e. the position is constant between 2000 and 5000 km, then it is first displaced towards the sun to a maximum of 1450 km for the 10.000 km isophote ; thereafter it is swept back very rapidly to 11.500 km for the 47.000 km isophote. It is easy to see that this is related to the formation of an envelope on the tailside in the continuum, but no theory, to my knowledge, can account for this. Note also the large irregularity in the curve of C_2 and CN and the smaller one in NH.

These results are very preliminary and were given mainly to illustrate my point about the complexity of the source. I am now going to dwell on building models to try to extract as much information as possible from these profiles. My biggest frustration is that since this instrument has been in operation (1967) I have had in all less than ten hours of observing time on comets. This is due to the fact that to obtain good profiles, I need to work at the cassegrain focus of a large telescope (at least 2 m) and that the big observatories have their observing program planned six months or one year in advance. Anyhow, these observations show at the least that we have to be very careful when we speak about the symmetry of the profiles particularly when we try to fit the models.

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R E F E R E N C E S

Malaise, D., 1966 XIII Coll. Liège, p. 199

Haser, L., 1966 XIII Coll. Liège, p. 233

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F. L. Whipple: I mention the effect of "blanketing" and absorption near the nucleus only because it has not been mentioned so far in the discussion. Three results of several are worthy of mention here: (a) reduction in sublimation rate of the nucleus; (b) spatial effects on ionization and excitation phenomena; (c) opacity in the line-of-sight of observations. Blanketing and opacity may well account for some of Malaise's difficulties.

W. Jackson: In a paper published in ICARUS (Vol. 8, p. 270 (1968)), B. Donn and I tried to take into account the effect of optical depth on the center of luminosity of the observed radical emission. The figures in that paper are theoretical estimates for radicals and ions. In all cases there will be a displacement of the center of maximum radical density some 100 to 1000 km toward the sunward side.

Finally, I wonder if the coupling between C_2 and C_3 can be explained by the photodissociation of C_3 to yield C_2. e.g.

$$C_3 + h\nu \rightarrow C_2 + C$$

or possibly

$$R_1C_3H + h\nu \rightarrow R_1 + C_3H$$

$$C_3H + h\nu \rightarrow C_3$$

$$C_3H + h\nu \rightarrow C_2 + CH$$

Z. Sekanina: Effects of opacity from the dust particles released following a massive outburst of gas are apparently responsible for a feature occasionally observed in tails and generally known as a "shadow of the nucleus." In some cases the screening of the nucleus might be so efficient that the vaporization from the surface virtually ceases for a while—until the surplus of particles in the atmosphere is dispersed out into space.

Voice: What was the time scale?

Z. Sekanina: That was a very short time scale. I guess it was five hours or something like that. And you simply have trouble to explain that by any other mechanism except by stopping the influx of the solar radiation to the surface.

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I think this is a very effective mechanism.

A. H. Delsemme: Yes, I just wanted to mention that I had prepared a lengthy discussion on the different causes for departures of circularity of the isophotes. But I had no time to run through it in my short expose. I will submit it for publication on this occasion. I think that is the best way to handle it, because it was prepared but I didn't read it. It was too long.

But, of course, I am quite aware of all these difficulties and my discussion will cover not only the things which have been mentioned, but some other ones that have not been mentioned to date.

I would like to emphasize that Malaise's observations are very important because those are the only clues we have on the different positions of the different isophotes. We have theoretical reasons to believe that it should happen, and I am going to mention them in these notes.

But it is important to observe them. And I would like to encourage him to publish these data that were taken four years ago, in that I have already seen them two years ago in Liege.

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