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Being As a Classical Observer of Mars

By

Masatsugu MINAMI

In the preceding issues of the CMO (see #405 and #406), Bill SHEEHAN interestingly discussed the present status of the "classical" observation of Mars which has been made from the terrestrial stations. His "night thoughts" first picked out an outstanding case of the Classical Mechanics which experienced a great vicissitude at the beginning of the last century and looked finally hushed because of the great advent of the new kind of mechanics, i.e. Quantum Mechanics: He suggested then as an analogy that the classical observation of Mars might have lost its meaning and might not afford to give any new perspective in the future because of the advent of the era of the modern means of investigations by the use of a variety of space probes including Mars orbiters and surface rovers: We should so admit that we might be at the "post-frontier" era in which we might not be able to find any new findings from the Earth by means of the "classical" system of the observations. We might belong thus to a group of "loners" who should be aware that already "the world we knew has moved out from under us." And hence we should be patient if someone would suggest that we might just be doing "something fabulous but useless."

However, does not this view sound too impatient? Can we say that the classical physics is dead since the time of the arrival of quantum theory? As well, can we say that the classical method to observe Mars from the basis on the Earth should be dead because of the rising of the Space Era?

We should however recall any modern physicist does not think of the vanishment of the classical mechanics. Just he knows that the classical mechanics has its own "*application limit*": Really the classical mechanics or the Newtonian mechanics is still useful even at this era if we treat the matters macroscopically, or if we treat the world where Planck's constant h can be considered to be vanishing. To launch any rocket or spacecraft to space, nobody does use the quantum mechanics. In case, one may be forced to use the relativity theory, but the relativity theory here is quite "classical."

The Wave Mechanics (another formalism of Quantum Mechanics) was put forward in 1926 by Erwin SCHRÖDINGER (1887-1961). So the following wave equation is called the Schrödinger equation for the wave function $\psi(q)$:

$$i\hbar\partial\psi(q)/\partial t = H\psi(q) \qquad (1)$$

where $\hbar$ is $h/2\pi$, q is a set of the position coordinates, t is the time and H is an Hermitian operator called the Hamiltonian and has the dimension of energy of the system. The Planck Constant h was named after Max PLANCK (1858 - 1947) who discovered it in 1900.

Here Hermitian is named after Charles HERMITE (1822-1901), French mathematician, and Hamiltonian is after William Rowan HAMILTON (1805-1865) who was the founder of Hamiltonian Classical Mechanics. He is also known as the discoverer of the *quaternion* in mathematics.

SCHRÖDINGER first regarded $\psi(q)$ as expressing a *real* wave in our 3 dimensional world and in this sense he was very "classical" in spite of the fact he had reached the very new kind of wave equations. In order for a wave to exist in the *real* space, it must be localised in a three dimensional space, whereas the Schrödinger equation should be treated to be a more mathematical object: It holds really in a much higher dimensional space: It turned out the wave is not any real wave, but it is more highly *probabilistic* as shown by Max BORN (1882-1970), Werner Karl HEISENBERG (1901-1976), Paul DIRAC (1902-1984) et al who all made every remarkable contribution to establish the Matrix Mechanics. Roughly speaking, $|\psi(q)|^2$ just describes the probability density of finding a particle in the q space when the position of the particle is measured in a classical sense.

In Matrix Mechanics, any observable operator (or simply Observable) $A(q, t)$ satisfies the following equation in the framework of HEISENBERG:

$$dA(q)/dt = (1/i\hbar)[A(q), H] + \partial A(q)/\partial t, \qquad (2)$$

where $[a, b] = ab - ba$ is a commutation relation which holds in the matrix algebra. It is not so difficult to show that Eq. (2) is mathematically equivalent to Eq. (1) in the Schrödinger picture. The last term in Eq. (2) will be erased if the observable $A(q)$ does not depend *explicitly* on the time. In matrix mechanics, the generalised canonical *positions* q_i and canonical *momentums* p_i (both are operators or matrices) satisfy the following commutation relations:

$$[q_i, p_j] = i\hbar\delta_{ij}E, \qquad (3)$$

where δ_{ij} is Kronecker's delta named after Leopold KRONECKER (1823-1891) and satisfies $\delta_{ij}=1$ (or 0) if $i=j$ (or if $i\neq j$) respectively, and E is the identity operator of the system. The set of operators are not necessarily commutable, and when q and p are not commutable as in (3), there holds the Uncertainty Principle (or Principle of Indeterminacy) between coordinate (position) q and momentum (velocity) p.

Werner HEISENBERG is the very person who established the matrix mechanics and the uncertainty principle. Max BORN was a mentor of HEISENBERG, and it is said it was BORN who first pointed out ingeniously that the ingredients HEISENBERG treated were possibly the elements of a "matrix", so that it was so due to BORN's nice sense of mathematical physics that the matrix mechanics was born. It is widely known that Olivia NEWTON-JOHN (1948~) is one of grandchildren of Max BORN.

The fact that the matrix mechanics and the wave mechanics are equivalent was pointed out by SCHRÖDINGER and others, while the formalism put forward by DIRAC can be said most elegant.

Here we would like to notice that the canonical formalism of classical mechanics holds quite similarly as in the framework of quantum mechanics, where we can really find the canonical variables of classical mechanics, that is, the canonical variables q, and p in a higher n-dimensional space. Corresponding to the commuting bracket [,] in quantum theory, we can define the following bracket, called the *Poisson bracket* named after Siméon Denis POISSON (1781-1840). Let U and V be some classical functions depending on the canonical variables q_i, p_i. Then we can define the Poisson bracket by

$$\Sigma_i((\partial U/\partial q_i)(\partial V/\partial p_i)-(\partial U/\partial p_i)(\partial V/\partial q_i)) = [U, V]_{Poisson} . \quad (4)$$

This $[U, V]_{Poisson}$ can be proven to satisfy the same algebra as the bracket commutators of operators or matrices. If we choose such that $V=q_i$ or p_i, we obtain

$$[U, q_i]_{Poisson} = -\partial U/\partial p_i$$

or

$$[U, p_i]_{Poisson} = \partial U/\partial q_i .$$

Here if we choose $U = q_j$ we know that the following relations hold:

$$[q_i, q_j]_{Poisson}=0, \quad [q_i, p_j]_{Poisson}=\delta_{ij}.$$

If we compare these with (3) of quantum mechanics, we are led to the view that the following correspondence holds:

$$[q_i, p_j]_{Poisson} \longleftrightarrow (1/i\hbar)[q_i, p_j]_{Quantum}. \quad (5)$$

On the basis of the above algebraic relations, if we further introduce the following classical Hamilton Canonical Equations of motion:

$$dq_i/dt=\partial H/\partial p_i , \quad (6.1)$$

$$dp_i/dt=-\partial H/\partial q_i , \quad (6.2)$$

the system becomes *dynamical*. If we employ the Poisson equations, these turn out to take the forms

$$dq_i/dt=[q_i, H]_{Poisson}, \quad (6.1)'$$

$$dp_i/dt=[p_i, H]_{Poisson}. \quad (6.2)'$$

If we assume A is a function of (q, p, t), then we have

$$dA/dt=\Sigma_i\{(\partial A/\partial q_i)(dq_i/dt)-(\partial A/\partial p_i)(dp_i/dt)\}+\partial A/\partial t$$

which takes the following form in view of (6)

$$dA/dt = [A, H]_{Poisson}+\partial A/\partial t. \quad (7)$$

Note that these have been shown in the framework of *classical mechanics*.

If we here employ the preceding correspondence like (5):

$$[q_i, H]_{Poisson}\longleftrightarrow ((1/i\hbar)[q_i, H]_{Quantum}$$

or $$[A_i, H]_{Poisson}\longleftrightarrow((1/i\hbar)[A_i, H]_{Quantum} , \quad (8)$$

and then Eq. (7) proves to be nothing but the Heisenberg type equation (2) of quantum mechanics.

We have thus shown that the quantum theory has no large difference in the framework from the former classical theory.

It is important to know that the classical formalism including the Hamiltonian as well as the Poisson brackets had been already prepared much before the advent of Quantum Theory. The pioneers of quantum systems must have been taken aback at the developments of situation, but the scrutiny of the classical formalism helped them much. It was only necessary to trace the way the classical formalism was established and further develop classical formalism in a much more modern way.

It sounds somewhat strange for us to be able to understand the content of quantum theory more deeply if we do know exquisite history and the neat formalism of classical theory. This however does not imply that the mathematical formalism, if it is similar, reflects in it the physical requirements: In the 19th century there was no internal requirement concerning the physical world.

We should say thus that the advent of the new quantum theory was not so overwhelming to the classical physicists: We might here recall Arnold SOMMERFELD (1868-1951) at Königsberg to Göttingen, and at Munich. He rose earlier and was established in the world as a *classical physicist*. He was quite senior to HEISENBERG, even to BORN. So it must have been shocking, but not to say overwhelming, to him to see the rapid development of quantum theory. However it is well known that he had nevertheless educated several numbers of quantum students (including HEISENBERG et al) and really contributed himself to Quantum Theory, first by the work of the Bohr-Sommerfeld quantisation condition around the 1910s, and later by introducing the *azimuthal* quantum number and the *spin* quantum number.

We here add that quantum mechanics is still "classical" in the sense that it is *not relativistic*. Just a relativistic wave equation has been the one introduced by DIRAC, that is the Dirac equation which treats the half spin particle (that is, electron, and anti-electron=negaton). Here *time* is exactly conjugate to the Hamiltonian (energy), but in the original Schrödinger equation, the time is linear but the Hamiltonian is quadratic.

Now we are in a position to return to the world of Mars Observation. First of all, however, we must here neatly define the *classical* observations of Mars. Since Bill SHEEHAN put forward an antithesis between "the classical visual telescopic studies of Mars" and the "ever-more hurtling era of reconnaissance orbiters and surface rovers," it is appropriate for us to define the classical observations as the observational studies of Mars by means of telescopes from terrestrial bases. We here consider the telescopic observations by the use of CCD to be classical.

We should then cast the question: Is the classical observation of Mars said to be out of date? And so should it be abandoned?

We are of course of the opinion that it is overhasty to give up the classical observations. It is true and unavoidable that most of visual observations have been proving to be ambiguous and vulnerable, and so it could not be trusted by professionals and we could not become business-like as the professionals.

It will not be allowed for any business-like scholar to miss the chance to get a share of the government budget in the era of space: The latter has been enormous to glorify the national prestige.

Such an apostasy of the Lowell Observatory, as was pointed by SHEEHAN, must have been raised, if not by those persons who might have been interested in the budget, but by those persons who did not understand what the Mars observations should be. We should say however such an attitude of Lowell Observatory originated in the personality of LOWELL himself, and in fact he did not prepare any philosophy to be able to cope with the possible developments in the coming era.

As far as we see, not only LOWELL, but also several observers have been there who could scarcely try to pursue the *classical* observations of Mars in a thoroughpaced way. We may say every classical observer could not even be called *classical*. We should say there have been few, not to say none, who established the true observation method. G V SCHIAPARELLI and E M ANTONIADI may belong to the exceptional cases and contributed a lot to the classical observational world, while LOWELL and followers cannot be said even to be classical. These persons tried to find something which did not exist.

To observe Mars, we will need to prepare a careful, minute and comparative plan to be pursued if said classical. Mars rotates every day with a period which is akin to the case of the Earth, and hence it must be watched from every corner of the Earth every night to cover all of the surface of Mars. We know there have been several associations of Mars observations hitherto, but they do never look to have worked well. If the data were treated closed or confined in a certain corner (country), the data would imply much less than the case where a communication network is fully used world-widely. Furthermore the observations should be durable for a long period. We suppose they all knew, for example, the fact that Mars had a constant axis of rotation, and because of it the planet was governed by the four seasons within a Martian year. Nevertheless we seldom, if ever, come across the data of the areocentric (or heliocentric) longitude of the Sun in the old documents. Nobody could say that the foregoing systems, if ever classical, did reach the bottom of classical observations.

We should add here that several trials of several orbiters and surface rovers cannot also be said modern: To seek the evidence of life on Mars near the surface is never any new trial, but nothing but a mere extension of the dream of LOWELL et al. The detailed images of the surface are not revolutionary or instructive by themselves: They must become meaningful just when and only when they are presented in a sequence under some philosophies of observations.

Any great charm of the scientific observations lies in the case we have succeeded in predicting the possible truth of Nature with a right foresight before the realistic closer proof and substantiation. A suitable example we ever know is the good case of Christiaan HUYGENS (1629-1695) concerning his classical discovery that the Saturnian *ring* should exactly be a thin ring. He did not end with such an idea of something like ear-lobes or arms of Saturn. Though the shape of the something near Saturn was not exactly clear to him through his refractor, he instead tried to repeatedly watch the something near the planet until he reached the idea enough to grasp the true shape. He just came to the conclusion that the situation could be much clearer if the something was a ring. That is, if it is definitely a ring, various results which were obtained by the long-term observations of declinations and positions, became quite understandable, and the results did not contradict the idea of a ring.

Similarly, though it was an easier task, HUYGENS also discovered the satellite Titan and as well determined by the repeated observations from 1655 to 1659 its orbital period to be around 16 days.

As to the ring, he checked the "arm" disappeared in early 1656, and in October of the same year it did not recover a jot, but in December 1657 he conceived that the arms looked like a surrounding object: In February 1659, he checked that it appeared wider. He always imagined the correlation of the ring with the ecliptic.

Il va sans dire que such a research obtained by a series of observations and made under a methodological way should be called "scientific". Afterward, since then, the orbital period of Titan was made detailed and full, and a lot of beautiful photographs of the ring were provided, but these are all no more than refinements of the "classical" contents.

As to the nearest neighbour celestial object, i.e. the Moon, there have been issued a lot of *detailed* observations. Even detailed, some proved slightly *lunatic*. Here some were beyond the "application limit."

For example, Giovanni Domenico CASSINI (1625-1712) depicted a face of Lady at the western edge of the bay of Sinus Iridum, and Johann Hieronymus SCHRÖTER (1745-1816) and Franz von Paula GRUITHUISEN (1774-1852) suggested the presence of some advanced inhabitants on the Moon.

It must have been certain that it was possible even in those times to point out to them that such a view was foolish beyond the "application limit."

Concerning Mars, it is well known that there have been made a lot of silly statements; though we skip here citations.

The defects connoted in such silly statements may be because of the shortage of advanced information, but it would be better to think that these were due to a lack of appropriate improvement of information.

In the case of HUYGENS, he squeezed a right route of thinking just depending on no more than a minimal kind of information. We should therefore, at the present-day situation of Mars observations, seek the way to grasp the real characteristics and circumstances of the real Mars without depending on any preconception: These ways are still classical, but apparently there must exist several or many ways to pursue.

Thus we consider that the classical approach to Mars observation has never been obsolete, as was the case of the classical mechanics. Rather within the framework of the classical observation, there are lots for us to do; and for instance, we should say, further improvement of the method and reinforcement of the communication are waited. Without advancement of methods and communications with other places and other observers, such an isolated employment of a single large aperture telescope, for example, does not necessarily lead to any improvement. This is apparent if we recall the fact that, as far as the planet Mars is concerned, the great telescopes in the 19th century did not leave any results of special importance.

We have insisted thus that the classical programme of observations of Mars is not out of date. Nevertheless, we should admit that it is impossible for anyone on the Earth to catch telescopically any debris or regolith on the Martian surface. It is not our purpose, however, to catch the details on Mars. It does not belong to astronomy to try to dig the Martian surface. It is not our purpose for us to become constructors. We should just want to be astronomers who make models of exact inference.

We finally move on thus to a decisive point which has been discarded in the discussions so far. This is a consequence of our belief that we should be *classical* astronomers. We should here declare paradoxically that it is quite hasty if one would like to readily conclude that the observations from the surface of the Earth are inadequate. Contrarily we believe that it implies no defect by any means but rather it is an advantageous point to observe the planet from the distance, strictly rejecting to stand on the surface of Mars.

We thus believe that a decisive key to give a breakthrough in this problem lies in discarding our attitude to seek the silly details on the Martian surface. That is, we should first abandon the method to *directly* contact with the Martian surface and turn to adopt such a standpoint to use the means of a *remote sensing*. In other words, instead of standing on the Martian surface, we would like to obtain from the remote district a rich series of results covering the surface based on lesser information. This is the way HUYGENS did the good work.

As an example, let us pick out some cases on the Earth. To know how wheat is growing in *a hectare* of cropland, we have only to examine on foot closely at the field. However in the case of a *huge* area of cropland, where any rover cannot investigate or predict in detail how rich the crop or harvest is, or will be, it is more effective to use a balloon high up in the sky and acquire information of wheat field depending on the remote sensing. This kind of sensing is also far more effective to know how much or in what part of the sea or lake is polluted. Observations from the sky will also give a further better answer to the question how the devastation of a mountain side is artificially advancing, and how it is affecting the lower parts of the rivers starting from the mountain and emptying into sea or lake. For instance, it has been supposed that the prediction of the crop how it is rich or poor at Siberia has been made in quite a precise way at the American continent every year by the use of the spy airplanes high up in the sky or the artificial satellites. Remote sensing has also been an important means for the military and speculation sides. The spy programme from sky is much more effective if made from a lot of different angles than what the spy groups on the ground could bring about.

As for the space activity on Mars, which is however not inside of our interests, we may say that the orbiters are much superior to the surface rovers on Mars. This is quite proved from the unique thing that the former discovered some spring waters at the walls of craters.

The observations of the great dust clouds hitherto obtained from the Earth do not look yet to be inferior to the data obtained from the orbiters in the space age. Just they are waiting to be analysed. Even if many more spacecrafts could be launched to the direction of Mars, the remote sensing from the Earth will remain to be more timely (as far as the surface is observable from the Earth) and comprehensive from the macroscopic point of view.

In concluding this essay, we would like to repeat again that we should be proud of the classical observations. The human brain has no ability to describe anything that is very unfamiliar. Any description is to be classical. Nowadays we know that the electron is a particle obeying the quantum mechanics and behaving quite differently from the mere classical particle. We know the probabilistic waves pass through the diffraction gratings, but eventually we *observe* and *describe* them to be classical particles for example imaged on the films or the plates. There is no image of the wave on the film. Just like this, the human brain has a tendency to think everything on a classical basis: And so, to reduce the results into a classical story is not any shortcoming and is always affirmative in the world of observations. □

ISMO 11/12 Mars Note (9)

Trend of the Elysium Orographic Cloud at Late Northern Spring

Christophe PELLIER

In the 8th Note of the 2012 apparition we reviewed the evolution of the Tharsis orographic clouds throughout the Martian day. This 9th Note will review the same phenomenon but for the Elysium volcanic shield to see whether it's possible to spot similarities or differences. The Elysium summit is much less high than any of the Tharsis bulge (14 km against 20/27), and we do not observe the "poking effect" through a morning mist that is anyway duller and thinner than over Tharsis.

The first Figure is an image taken by Jim PHILLIPS at λ=082°Ls shortly after opposition; the Elysium cloud is shining almost at the centre of the disk.

I - Hourly evolution of the cloud

Figure 2 depicts the evolution of the Elysium region in blue from early morning to early afternoon (the reason why blue light is preferred here to colour images is that there is a bright albedo arc in the ground next to Elysium, that confuses the aspect of the cloud in RGB or LRGB). The cloud looks to appear around 11H LMH in Don PARKER 3H24 UT's B image from 14 March 2012, and so it looks safe to say that the cloud forms from 10H30-11H LMH. This looks a bit later than for Olympus and Ascraeus (refer to CMO #406 in

http://www.hida.kyoto-u.ac.jp/~cmo/cmomn4/CMO406.pdf

for a detailed analysis of the Tharsis orographics), but the clouds are much smaller and then could begin to develop even earlier. But this looks coherent with the northern summits of Tharsis.

The cloud then gets brighter by the hour, but remains small throughout the afternoon until a point where it looks to widen quickly, from mid afternoon, and then it occupies the whole mountain and not only the summit itself. This is visible on ↙

Fig.2 : The grow of the Elysium cloud during the Martian morning. The cloud is easy to see at 12H12, but just barely at 11H LMH. From left to right, B images from Tomio AKUTSU (*Ak*)(31 March), Jean-Jacques POUPEAU (13th March), Don PARKER (14th March, two frames) and Efrain MORALES (10th March). Ls range is 082°Ls~083°Ls except for *Ak* (λ=091°Ls).

↙Damian PEACH's set of 29 February to 1st March (Fig. 3). Note that there are still a few hours of possible evolution as at the season, sunset occurs over Elysium around 19H LMH, but the late hours at the best season where not visible from the Earth due to the angle of view.

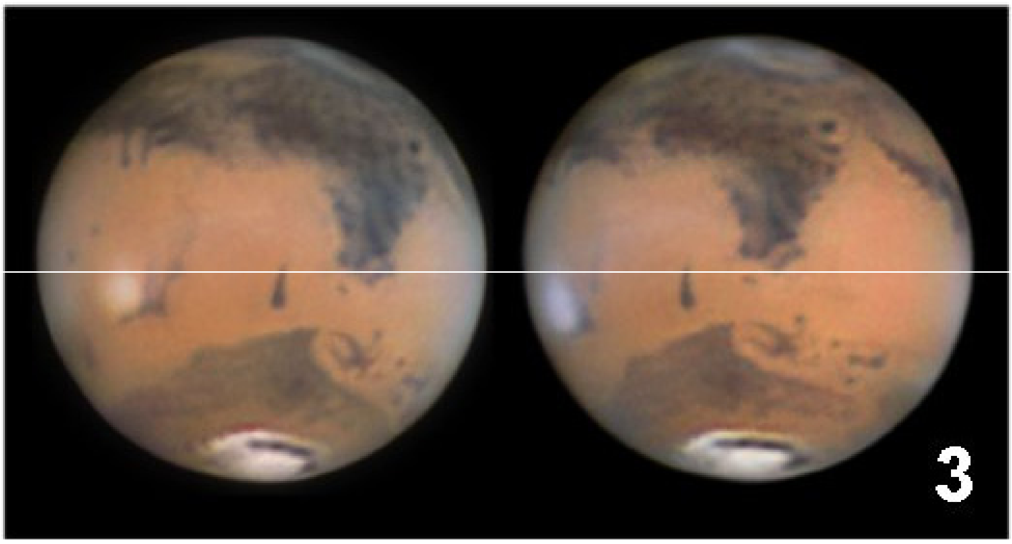
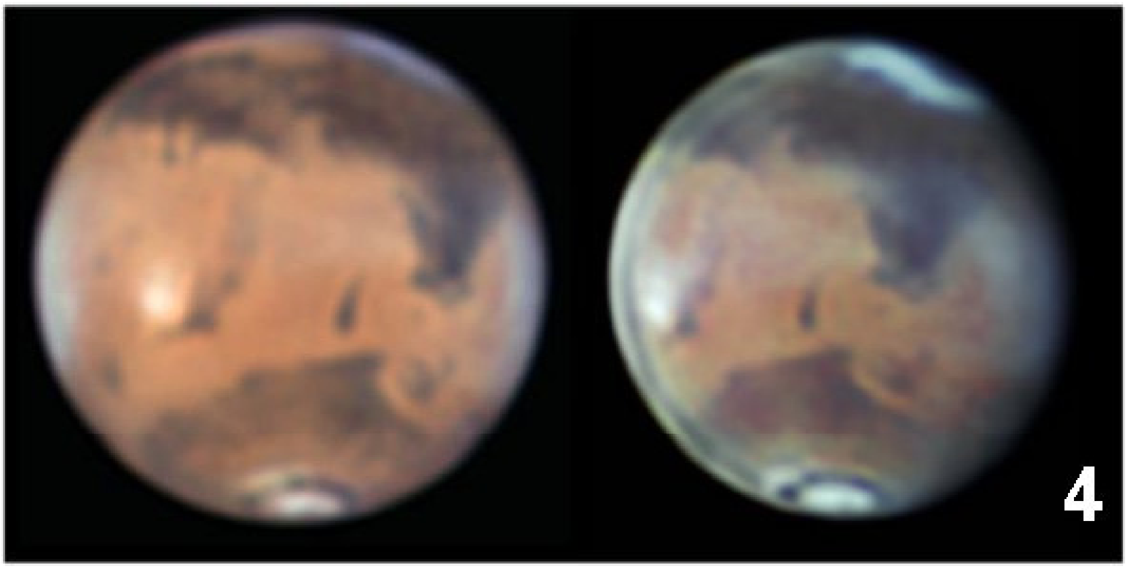


Fig. 3: Expansion of the Elysium cloud in the afternoon, as seen on PEACH's images from 29 Feb. to 1st March, 2012.
Images are taken 1H36 mn apart (15H12 to 16H48 LMH)

II - A brief seasonal evolution

The season observed in 2012 at opposition (late northern spring) is maybe not the best one for the Elysium cloud. A comparison between two images with the cloud again at the same Martian hour but after one (terrestrial) month shows that it looks bigger at early summer.

Fig.4: The Elysium cloud caught at same Martian local hour (around 14H), but at different seasons.
Left: λ=081°Ls, right: λ=096°Ls. Images by Efrain MORALES (10th March and 12th April).



III - Is there a "trailing effect"?

One of the most salient features observed with the Tharsis orographic clouds is their strong tendency to develop westward as the hours go by during the afternoon (read CMO #406). This is certainly due to a wide circulation cell, the warmer air located on the lee side having a tendency to rise in altitude. It is difficult to see the effect over Elysium, where as written above, the tendency of the cloud at late hours is more to get puffy than trailed. The difference can be due to topography, the trailed effect being too small to detect on amateur images as the mountain is much smaller than Olympus (this is also the opinion of Reiichi KONNAÏ - personal communication). But the latitude is another possible difference; Elysium is located at 25°N, 7° higher than Olympus. For any reason, the macroscopic circulation cell effect could be not active, or too weakly active, at 25°N. Topographical reasons look more plausible though.

Conclusion

The Elysium cloud is a similar phenomenon than the Tharsis ones; however it has its own proprieties possibly due to the topography. In the next 2014 apparition, we will have the opportunity to observe the late hours of a better season before opposition, and we will ask the same questions to see if more elements can be picked up. □

Letters to the Editor

●·····*Subject: question*
Received; 15 January 2013 at 03:24 JST

Dear Masatsugu, I hope you are well--I hope to write the essay you requested in the next day or two.

Meanwhile, I am wondering what you can tell me about Sizuo Mayeda and his June 4, 1937 observation of the bright point near Tithonius Lacus, which I think is one of the classic observations of its kind. I don't recall if he is one of the Japanese astronomers you discussed in your interesting talk at Meudon in 2009.

○····*Subject: Mars essay*
Received; 17 January 2013 at 07:12 JST

Dear Masatsugu, Here's another essay. It may be too long, in which case you can cut it in half like an earthworm and it may even grow two heads.

I will send it to Christophe and Reiichi as well.

Sorry to hear of your continuing health problems. I am having a few procedures done here too; tomorrow I have pins put in two toes I damaged cross-country skiing with bad equipment two years ago, and have decided on the ablation surgery for A-fib which will be performed in February. I am trying to get well in advance of my May trip to Flagstaff, where I hope to do some climbing of cinder cones, meteor craters, and the San Francisco Peaks. Kind regards,

○····*Subject: Re: Mars essay*
Received; 19 January 2013 at 03:30 JST

Dear Masatsugu et al., I sent a version of my essay to my good friend Dale Cruikshank, and he responded as follows; he also sent some interesting essays from 1964 and 1966 which I've attached.

http://www.hida.kyoto-u.ac.jp/~cmo/cmo/1964Mars_Rocks_DPC_ABB_CommLPL.pdf
http://www.hida.kyoto-u.ac.jp/~cmo/cmo/1966Mars_surface_Icarus.pdf

>-----Original Message-----
From: Cruikshank, Dale P.
Sent: Thursday, January 17, 2013 5:05 PM
To: Sheehan, William P (DHS)
Subject: RE: Mars essay

Hi Bill, thanks very much for your Mars essay--I read it with both nostalgia and close interest, and the limonite issue brought back memories of some work that Al Binder and I did while we were grad students in Kuiper's lab in the 1960s. I'll attach the two papers we wrote about our measurements of red-stained rocks in the Sonoran desert in which Tucson is embedded, and comparisons with our measurements of the near-infrared reflectivity of Mars. As we point out limonite is only one of several iron-oxide minerals, and indeed the Mars surface seems to have a number of these, as well as some others.

I'm sitting in a Pluto mission meeting in Boulder just now, listening to details of the spacecraft performance and planning exercises for the flyby in July 2015. Best regards,

Dale

Dr. Dale P. CRUIKSHANK
NASA Ames Research Center

○····*Subject: RE: essay for CMO and ISMO*
Received; 21 January 2013 at 01:02 JST

Dear Masatsugu, It is my mistake--Sithonius Lacus was a site I did not know about. I think that de Vaucouleurs meant Saheki's observation as he refers to a 5" disk. The fact he received no response from Saheki (if he wrote to him indeed) I cannot explain. Saheki seems to have known French, as he did have Flammarion's *La Planète Mars* as we saw during our visit in 2004.

Perhaps we need to look into what the mineralogy of Sithonius Lacus is now. This will be a good project for our readers.

Pardon my short response--I am temporarily rather disabled from surgery on my foot. Best,

○····*Subject: RE: essay for CMO and ISMO*
Received; 21 January 2013 at 01:33 JST

Dear Masatsugu, I was in at my office--returning home, I checked the draft of that article; it can be corrected if Tithonius Lacus is changed to Sithonius Lacus, and then if the "also" referring to Saheki's observation is deleted. Thus the sentence should read: … an intense point of light suddenly appear near Sithonius Lacus; it scintillated like a star and disappeared after five minutes; a similar observation, at Tithonius Lacus, was reported by Tsuneo Saheki on Dec. 8, 1951," etc., etc.

Perhaps Reiichi Konnaï would be so kind as to investigate and write something about Sithonius Lacus--is this also an area of plagioclase and calcium-rich pyroxene, as the other two areas (Edom and Tithonius Lacus) seem to be?

I sent Dale Cruikshank the draft of the article, and he responded with some interesting reminiscences about the spectral studies of Mars vs. minerals he and Alan Binder carried out as G. P. Kuiper's grad students in the 1960s. Perhaps this is something to write up in another article. Best wishes,

○····*Subject: CMO 406*
Received; 26 January 2013 at 01:16 JST

Dear Masatsugu, I have downloaded the latest CMO, with the article I wrote; it looks good—the section on Sithonius Lacus is now corrected. This gives us a new project: to try to examine what might have been happening at Sithonius Lacus when that observation was made. Best wishes,

Bill SHEEHAN (Willmar, MN, the USA)

●·····*Subject: Re: CMO/ISMO 406/32 uploaded /email address and location change.*
Received; 26 January 2013 at 16:57 JST

Hi, Thanks for sending the report. Please do take note of my new email address,

I will be moving to a new location, but will be active as soon as my setup is complete in a few months.

Freddy WILLEMS (Waipahu, HI, the USA)

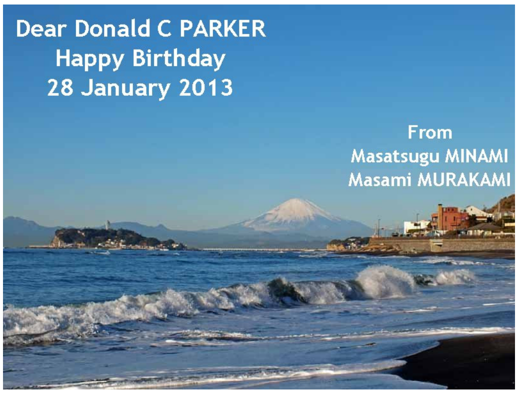
●·····*Subject: Re: Happy Birthday*
Received; 29 January 2013 at 02:54 JST

Dear Masatsugu and Masami, Thank you for the birthday greetings and for the kind words! Best,

Don PARKER (Coral Gables, FL, the USA)

●····*Subject: Happy Birthday*
Sent: Mon, January 28, 2013 6:23:35 AM

Dear Don PARKER, one of the most impressive Mars observers on the Earth. Wishing you a Happy Birthday! on 28 January 2013, and hoping you will have many more happy years. With best wishes



Masatsugu MINAMI and Masami MURAKAMI, Japan

☆ ☆ ☆

TEN YEARS AGO (214)

---*CMO #269 (25 February 2003) pp3535~3558* ----

http://www.hida.kyoto-u.ac.jp/~cmo/cmomn2/cmo269/index.htm

The first announcement was about "the 11th Meeting of the CMO Observers" which was planned to be held in May 2003 at Fukui. In the period the Fukui City Museum of Natural History was also to hold a Lecture Meeting on "Mars, this summer" opened to the public: Kunihiko OKANO and some CMO members were nominated as the speakers.

The 4th Observational Report of the CMO treated the latter half of January 2003 and the first half of February: The disk diameter δ was only 4.9" to 5.7". The Martian season proceeded from λ=124°Ls to 138°Ls. The tilt φ was from 10°N to 2°N. And the phase angle ι was from 31° to 36°. The observers recorded were 5 persons domestically, and two observers from abroad. In Europe they observed from around S Meridiani to Elysium, and to our eyes the morning Hellas to the evening Hellas after one round faced toward us. M Acidalium was not so dark compared with the southern markings.

http://www.hida.kyoto-u.ac.jp/~cmo/cmohk/2003repo/04/04.html

The retrospective 2001 Mars CMO Note (16) was entitled "Great Yellow Cloud with Water Vapour on 30 June 2001" where picked out were the B images of the dust taken timely at Okinawa by Tomio AKUTSU (*Ak*) at an early season, and the vast expansion of water vapour at the morning side was on the agenda. It was pointed out that the water vapour must have played an important role at the first period of the dust rising at the morning area. As July 2001 came in, the water vapour decreased as the dust expansion began:

http://www.hida.kyoto-u.ac.jp/~cmo/cmohk/269Note16/index.html

"Great 2003 Mars Coming (8)" was "Recommendation of the Use of the Wedge-Prisms" written by Teruaki KUMAMORI (*Km*): When the planet was under 60° above the horizon, the resolution rate decreases because of the atmospheric dispersions and hence he showed how to cancel by means of a small-angle wedge prism. However in the case of the Newtonian the image rotates inside the field so that we need to adjust the angle of the prism as time goes by:

http://www.hida.kyoto-u.ac.jp/~cmo/cmohk/coming2003/08.html

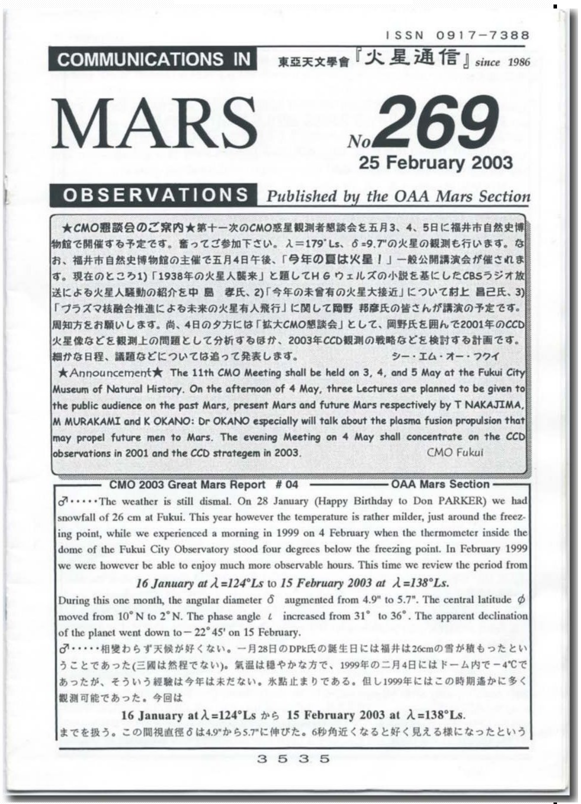
"Great 2003 Mars Coming (9)" was "Ephemeris for the Observation of the 2003 Mars. III" written by Akinori NISHITA (*Ns*) and it dealt with the ephemeris for the period from April to August 2003:

http://www.hida.kyoto-u.ac.jp/~cmo/cmohk/coming2003/09.html

In LtE, contained are those from abroad: Sam WHITBY (VA, the USA), Mario FRASSATI (Italy), Don PARKER (FL, the USA), Carlos HERNANDEZ (FL, the USA), Richard McKIM (the UK), Damian PEACH (the UK, at Tenerife), Dave MOORE (AZ, the USA), David GRAHAM (the UK), Bill SHEEHAN (MN, the USA), Eric NG (Hong Kong), W. Y.-LAI (Taiwan), Konrad DENNERL (Denmark). Domestically we received from H ISHADOH (Okinawa), Tohru IWASAKI (Kita-Kyushu), Miyuki UMEDA (Fukui City Museum of Natural History), Yukio MORITA (Hiroshima), Kunihiko OKANO (Tokyo). Kanehiro OSA (Ishikawa), Toshiaki HIKI (Nagano), Teruaki KUMAMORI (Osaka), Yaichi

MAKINO (Toyama); some were concerned with the coming Meeting in May and also with the Lowell Conference at Noto to be held in 2004.

Mk wrote in his 9th essay "Shin-Saijiki-Mura" about an old *Ume* tree (Japanese apricot tree) which was seen near the Station in the neighbourhood of his home. After ten years, the tree is declining and looks to die from the distance. But a close investigation proves that several young branches are still alive: He expects so the white *Ume* flowers will bloom as the warm air in spring governs soon.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OBSERVATIONS *Published by the OAA Mars Section*

★CMO懇談会のご案内★第十一次のCMO惑星観測者懇談会を五月3、4、5日に福井市自然史博物館で開催する予定です。奮ってご参加下さい。λ=179°Ls、δ=9.7"の火星の観測も行います。なお、福井市自然史博物館の主催で五月4日午後、「今年の夏は火星！」一般公開講演会が催されます。現在のところ1)「1938年の火星人襲来」と題してH G ウェルズの小説を基にしたCBSラジオ放送による火星人騒動の紹介を中 島　孝氏、2)「今年の未曾有の火星大接近」について村上 昌己氏、3)「プラズマ核融合推進による未来の火星有人飛行」に関して岡野 邦彦氏の皆さんが講演の予定です。周知方をお願いします。尚、4日の夕方には「拡大CMO懇談会」として、岡野氏を囲んで2001年のCCD火星像などを観測上の問題として分析するほか、2003年CCD観測の戦略などを検討する計画です。細かな日程、議題などについては追って発表します。　シー・エム・オー・フクイ

★Announcement★ The 11th CMO Meeting shall be held on 3, 4, and 5 May at the Fukui City Museum of Natural History. On the afternoon of 4 May, three Lectures are planned to be given to the public audience on the past Mars, present Mars and future Mars respectively by T NAKAJIMA, M MURAKAMI and K OKANO: Dr OKANO especially will talk about the plasma fusion propulsion that may propel future men to Mars. The evening Meeting on 4 May shall concentrate on the CCD observations in 2001 and the CCD strategem in 2003. CMO Fukui

CMO 2003 Great Mars Report # 04 —— OAA Mars Section

♂·····The weather is still dismal. On 28 January (Happy Birthday to Don PARKER) we had snowfall of 26 cm at Fukui. This year however the temperature is rather milder, just around the freezing point, while we experienced a morning in 1999 on 4 February when the thermometer inside the dome of the Fukui City Observatory stood four degrees below the freezing point. In February 1999 we were however be able to enjoy much more observable hours. This time we review the period from

16 January at λ=124°Ls to 15 February 2003 at λ=138°Ls.

During this one month, the angular diameter δ augmented from 4.9" to 5.7". The central latitude φ moved from 10°N to 2°N. The phase angle ι increased from 31° to 36°. The apparent declination of the planet went down to −22°45' on 15 February.

♂·····相變わらず天候が好くない。一月28日のDPk氏の誕生日には福井は26cmの雪が積もったということであった(三國は然程でない)。氣温は穏やかな方で、1999年の二月4日にはドーム内で−4℃であったが、そういう經驗は今年は未だない。氷點止まりである。但し1999年にはこの時期遙かに多く觀測可能であった。今回は

16 January at λ=124°Ls から 15 February 2003 at λ=138°Ls.

までを扱う。この間視直徑δは4.9"から5.7"に伸びた。6秒角近くなると好く見える様になったという

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In TYA #090 corner, Toshiaki HIKI (*Hk*) wrote about CMO #129 (10 Feb 1993) and CMO #130 (25 Feb 1993) of twenty years ago: The opposition day already passed, and at the end of February 1993 the apparent diameter went down to δ=11.6" and the season proceeded to λ=040°Ls; φ was shallow so that it was difficult to check the thawing npc. The observations were not so much made because of weather, while, among 11 numbers of domestic observers, Tohru IWASAKI (*Iw*) and Yukio MORITA (*Mo*) were active in the observations around Propontis I. CMO #130 showed "COMING 1992/93 MARS (5)" where "The North Polar Cap in Spring" was written by *Mn*, citing the thawing curve made from several references: Here a detailed description of *Baum's Plateau* was made which shows a temporary halt of thawing of the npc at about λ=010°~040°Ls.

M MURAKAMI (*Mk*) & M MINAMI (*Mn*)

★ We sincerely thank Takeshi (Ken) SATO (458) for his kind donation to CMO/ISMO. (*CMO Fukui*)

Bulletin: Kasei-Tsûshin CMO (http://www.mars.dti.ne.jp/~cmo/ISMO.html)

CMO #407/ ISMO #33 (25 February 2013)

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