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X-Tract from the "X" files: Part I

W H PICKERING and Percival LOWELL: from Collaborators to Rivals

By

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2015 has been dubbed the "Year of Pluto" at Lowell Observatory. Fittingly, I welcomed the New Year in Flagstaff, and spent time doing research into Percival Lowell's Planet X calculations in the new Collections Center at Lowell Observatory, background research to a book project on Pluto.

My own interest in Pluto was stimulated fifty years ago this April when I got a book out of the downtown Minneapolis Public Library, "***The Search For Planet X***," by Tony Simon. This year-January 13-marks 100 years since Percival Lowell presented a brief synopsis of his "X" calculations at the American Academy of Arts and Sciences in Cambridge, and in September, we will mark the centennial of the publication of his now-famous "***Memoir on a Trans-Neptunian Planet***."

It will be 85 years ago on January 21, 23, and 28 that Clyde Tombaugh exposed the plates on which Pluto's image registered, February 18 was the date he discovered the planet "blinking" way off beyond Neptune, and March 13 the anniversary of the announcement of the planet discovery.

Then on July 14, 50 years to the day after Mariner 4 flew by Mars, New Horizons, launched on January 19, 2006, will arrive at Pluto, completing mankind's first broad-brush survey of the Solar System.

A Year of Pluto indeed.

William Henry PICKERING (1858-1938) was a great astronomer, though he was always overshadowed by his brother, Edward Charles, a dozen years his senior and renowned as the director of the Harvard College Observatory, as well as, later, by his sometime colleague and collaborator Percival Lowell, whose work on Mars and the search for a trans-Neptunian planet are much better known.

Like Lowell, Pickering was born in Boston into a distinguished family (indeed, during the brief period of time before the Civil War, the Lowells and the Pickerings lived on the same street, Mt. Vernon Street, on Beacon Hill). On receiving his Bachelor of Science degree from MIT in 1879, he stayed on as a teacher of physics until

1883, when he was called to Harvard by his brother to teach astronomy.

From the first, William was an inveterate tinkerer. He was an instrument- man whose early interest was in photography. He captured a horse in motion along the lines of Eadweard Muybridge, discovered that photographic emulsions differ in their sensitivity to daylight, a first step toward the development of indoor and outdoor films, and discovered that photographic film was sensitive to infrared light. More pertinently here, he pioneered the use of gelatino-bromide dry plates for astronomical photography. On February 21, 1883, he successfully photographed the Orion nebula with a small Voigtlander lens, and proceeded to show that the Orion Nebula was only part of a much larger cloud of gas covering the entire constellation of Orion. (In fact, he was the real discoverer both of "Barnard's Loop" and the Horsehead Nebula.) On the basis of these successes, he suggested to his brother the project of producing a large-scale photographic atlas of the sky —a suggestion that led to the great Harvard survey of over 400,000 plates taken at the Harvard Stations in both hemispheres (the plates of the survey are currently in the process of being digitized). William also undertook numerous photometric investigations, which helped support his brother's long-term interest in and pioneering contributions to that field.

In 1889, the Pickering brothers went to California to photograph the New Year's Day total eclipse of the Sun. Afterwards, in search of a better conditions than Cambridge offered for a new station of the Harvard College Observatory, they visited Pike's Peak in Colorado and Mount Wilson in Southern California. Pike's Peak left something to be desired, but the favorable conditions on Mount Wilson were established. Using the Boyden 13-inch refractor, the younger Pickering began work in astronomical photography on Mount Wilson, and established the favorable conditions on the peak which were later to lead to George Ellery Hale's decision to establish his solar observatory there in 1903 — the beginning of the future world-famous Mount Wilson Observatory.

Already, William's fateful interest in the Moon and planets had begun. In 1888, he had obtained some of the earliest photographs of Mars, on blue-sensitive plates taken through the 13-inch refractor. In 1890, back at Harvard, he used the 15-inch Merz refractor at Harvard to observe the Moon, and made experiments into sensitivity of the eye with regard to Martian colors. And a year later he was in the Peruvian Andes, serving as acting director of the Boyden station of the Harvard College Observatory and setting up the Boyden refractor above Arequipa. Despite his brother's insistence that stellar spectroscopy was to take priority, William spent most of his time noting what he regarded the apparent ellipsoidal deformations of the Galilean satellites of Jupiter and, in 1892, visually observing Mars at its perihelic opposition. He had long been interested in the planet. As far back as 1877, he had written to Edward: "I'm so much interested in Mars. I always thought it the most interesting planet in the system, because its climate so nearly resembles our own. And now it's more interesting than ever."(WHP to ECP

August 26, 1877; Harvard College Observatory, Director's Papers, Harvard University Archives) At Arequipa he confirmed the existence of linear surface features corresponding to Schiaparelli's *canali* (though neither then nor later was able to make out their doublings or geminations), and telegraphed reports to the *New York Herald* which included sensational discoveries such as the canals, mountain ranges near the south pole, evidence of melted snow water flowing northward, clouds in the Martian atmosphere and the existence of "forty lakes" around Solis Lacus.

His Martian credentials established, Pickering's advice was sought out by Percival Lowell, irrepressibly captivated by the Red Planet, in January 1893. A fateful meeting between the two Brahmins took place toward the end of that month at Harvard College Observatory. Lowell announced his intention to set up his own observatory in time for the next favorable Martian opposition, in October 1894, and Pickering agreed to take an unpaid leave of absence of one year from Harvard to join the expedition as an advisor. For the best seeing conditions, Pickering advocated the deserts of the Southwestern United States, and helped Lowell borrow two telescopes, an 18-inch Brashear that eventually ended up at the Flower and Cook Observatory in Philadelphia and a 12-inch refractor from Harvard. He also designed a pre-fab dome that was to be shipped out west as soon as a site had been settled. Andrew Ellicott Douglass, a recent graduate of Trinity College of Hartford, Connecticut who had assisted Pickering at Arequipa, was hired to test seeing conditions at various sites in the Arizona Territory with the 6-inch Clark refractor Lowell had taken with him to Tokyo. In April, Lowell, increasingly impatient with Douglass's peregrinating route and eager to get on with the siting of his observatory, chose the hill above the lumber town of Flagstaff — it was conveniently located on a rail line, had the greatest elevation of any of the sites Douglass tested and, as Douglass said before his death, also had the best saloons. At the very end of May Lowell arrived by train at the platform of the station which still stands, and joined Douglass and Pickering on the hill — which Lowell dubbed "Mars Hill" — to begin observations of the planet of their predilection. The great Mars campaign of 1894 was on.

In some ways, Pickering and Lowell were too alike to get on well with one another. Boston Brahmins both, they ambitious, egotistical, fiercely independent and self-willed. After the press misconstrued Lowell's observatory as a Harvard venture, Lowell let it be known in no uncertain terms that this was not so; he was in charge. "Simply call it the Lowell Observatory," he admonished the press. He made it clear that Pickering and Douglass were both on loan from Harvard. However, for present purposes, they were his employees. Pickering, who chafed under his older brother — "my father didn't want to feel dependent at all on Uncle Edward," his daughter explained to Howard Plotkin — did not intend to be thrown into a similar role with a man without any formal training in astronomy and who was of roughly the same age as himself. Though there are no records of any specific unpleasantness, they split up after the 1894 opposition. Douglass stayed on as Lowell's chief assistant for several years — though his relationship with Lowell, too, ended bitterly. Pickering, though encouraged by his brother to return to Lowell Observatory, "since your skill seems to lie especially in such work as can be done there," rejected the idea. He preferred to return to Harvard, where he became a consistent critic of Lowell's ideas about Mars. After the turn of the century, their rivalry came blossomed fully into the open as it emerged that both were in the hunt for a trans-Neptunian planet.

The possible existence of planets — either inside the orbit of Mercury, or outside that of Neptune — was very much in the air at the end of the 19th and beginning of the 20th centuries. There were a number of mathematical investigations, and one or two telescopic searches, none very rigorous and all rather speculative. Perhaps the most notable was that carried out in 1877 by David Peck Todd, then at the U. S. Naval Observatory and later astronomer at Amherst College.

Todd's investigation was adapted from a graphical method used by Sir John Herschel to elucidate the circumstances of the discovery of Neptune (in *Outlines of Astronomy*; 1849 and later editions). Since the graphical method was favored by several searchers for trans-Neptunian planets, including Todd, Pickering, and Lowell (the last before 1910), we ought to say a bit more about it here.

Herschel began with a plot of the residuals of Uranus, where a residual is defined to be *the difference between the observed position and the position of a planet as predicted by means of the initially assumed values of the constants*. Herschel showed, by means of some ingenious diagrams, that "the increase of longitude of Uranus from 1800 to 1822, the cessation of that increase in 1822, and its conversion into a decrease during the subsequent interval" were due almost entirely to the effects produced by the tangential force of Neptune on Uranus; the normal force had, by comparison, no comparable effect. Neptune's discovery in 1846 occurred, he said, at almost the best time for its detection, and he argued further that even without guessing at the elements of the disturbing planet, the planet's direction could have been ascertained from the maximum of its residuals around the time of the 1822 conjunction.

Todd claimed that the graphical method had "suggested itself" as early as 1874, even before he had encountered Herschel's account. In 1877, plotting the residuals of Uranus according to Le Verrier's 1873 theory, Todd derived elements for a planet at mean distance 52 AU, with period of 375 years, and magnitude 13+.

(These elements will sound familiar to Pickering's Planet O; for one reason: they were both based on the same method — that of graphical analysis — and data, Le Verrier's 1873 residuals.)

Todd supposed the planet's longitude for 1877.84 was 170 degrees plus or minus 10 degrees — this put it in Gemini — and estimated its angular diameter as 2.1 seconds of arc, which would have made it a planet as large as Neptune. He also found an inclination of 1 degree 24 minutes, and the longitude of its node at 103 degree. Using the U.S. Naval Observatory's 26" refractor, he searched around this position for thirty nights, between November 3, 1877 to March 6, 1878, for thirty nights—without success. In fact—in tacit acknowledgement of the speculativeness of the search—he published nothing about his search for three years, and when he did, it was only after another planet-hunter, Professor George Forbes of Glasgow, announced his own set of planets, lurking at 100 and 300 AU, and having periods of about 1000 and 5000 years, respectively.

Meanwhile, another clue as to the whereabouts of trans-Neptunian planets had appeared in Camille Flammarion's 1879 best-selling *Astronomie populaire*. There Flammarion says (Gore translation, p. 471):

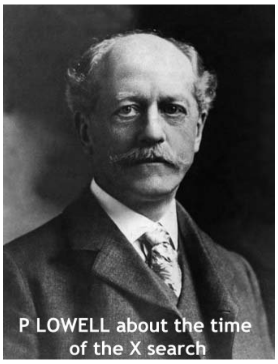
> We are about to treat of comets, and we shall see that in all probability, the periodical comets owe their presence in our solar system to the influence of the planets. In fact, every comet which, coming from the outside, passed sufficiently near a planet to be subject to its attraction and be captured by it, would continue its voyage to the vicinity of the sun, and would afterwards return to the point where it had been diverted from its primitive course, and would thus continue to revolve round the sun. All the periodical comets have their aphelia near the orbit of a planet. Now, the third comet of 1862 and the swarm of shooting stars of August 10 follow an orbit of which the aphelion is at the distance 48 [AU]. There should exist there a large planet….

After Flammarion wrote, another comet was found by Barnard (1889 III) with an aphelion distance near 48 AU. The "finger-posts" were pointing more and more insistently toward Flammarion's "large planet" at 48 AU. Indeed, Percival Lowell would trace his own interest in the possibility of unknown trans-Neptunian planets to his perusal, in 1901, of Johann Gottfried Galle's catalog of the orbits of all observed planets. Lowell found from this perusal that "up to about 100 astronomical units from the Sun the comet aphelia fell into groups between which were vacant gaps. The distances of these groups were significant, for each lay wholly or substantially just outside the orbit of a major planet…. Outside of the Neptunian group was a gap and then another group at 49 astr. units consisting of comet 1889 III, with its aphelion at 49.8, comet 1862 III with its aphelion at 48.1 and the Perseids. There came a second gap and a second group at 76 astr. units. Such an arrangement hinted strongly at two as yet undiscovered planets, the one circling round the Sun at about 48 astr. units, the other at about 74." ("*Trans-Neptunian planet,*"unpublished manuscript, ca. 1909. Lowell Observatory, Planet X calculations, Box 21.)

{Note: Comet 1862 III, Swift-Tuttle, was shown by Schiaparelli to be the source of the debris that give rise to the Perseid meteor swarm, so this represents a kind of double-entry. According to the most recent orbit, its perihelion is at 0.9595 AU and aphelion at 51.225. It was rediscovered at its most recent return in 1992. Comet 1889 III was discovered by Barnard; it was long believed to be lost, but was rediscovered in 2006. Its perihelion is at 1.077 AU and aphelion at 47.232

AU.}

Lowell's interest, at first almost casual, in the trans-Neptunian planet he would call "Planet X" became more and more serious (not to say obsessive) over the years, perhaps as a potential way of redeeming himself in the wake of the severe criticisms of his Mars— and especially Venus— work by professional astronomers. Thus in 1903, when the Venus work came to something of a crisis as he reobserved the disputed "spoke" system he had been on the verge of abandoning, in the face of professional ridicule, a year before— he hired a young Hoosier astronomer Vesto M. Slipher to develop expertise in the use of a spectrograph to vindicate the synchronous rotation of Venus which the observations of the spokes had suggested. At about the same time he hired another Hoosier astronomer, C. O. Lampland, to embark on the photography of Mars. In anticipation of results he expected to be obtained at the favorable opposition of May 1905 he remarked to Lampland, "We will get out something to make them sit up!" It is hard to resist the idea that the search for the trans-Neptunian planet was another tactic in the same campaign of vindication, for the discovery of another major planet of the Solar System, whatever its intrinsic interest, was certainly something that would cause his colleagues to "sit up." Lampland much later told Clyde Tombaugh, "Lowell wanted desperately to improve his credibility among other astronomers. So, Lowell thought, if he could predict the location of a ninth planet, beyond Neptune, and then find it, it would surely improve his status."{Clyde Tombaugh; Mercury, May-June 1986, pp. 66-72}


P LOWELL about the time of the X search

For obvious reasons, this time Lowell, in marked contrast with his very public other astronomical activities, was careful to keep a low profile. Information about the search for Planet X was shared discretely— and at times, even with members of his own staff, on a "need to know" basis.

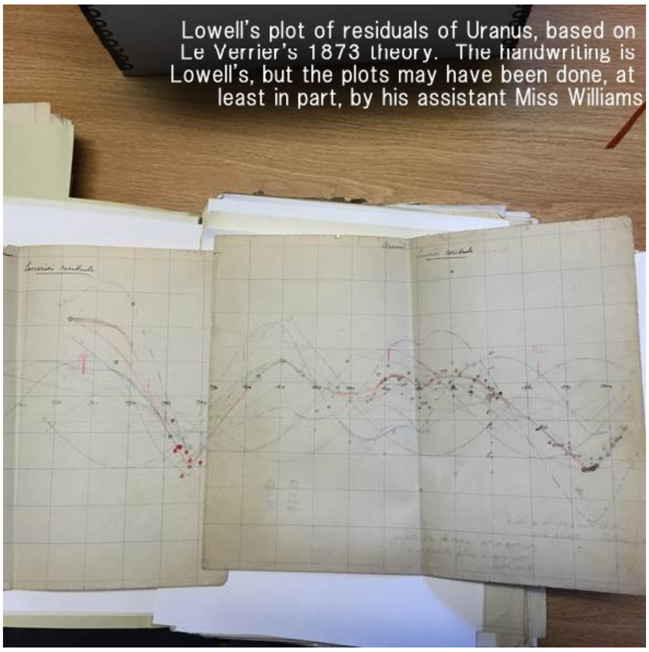
In early 1905, Lowell took two steps. First, he began planning for a photographic search, writing in February to E. C. Pickering at the Harvard College Observatory and to Camille Flammarion in France asking for astrographic charts of star fields along the "invariable plane." Reckoning that he and his assistants would be kept too busy with Mars for several years to take on these additional (and doubtless time-consuming) duties, he sought to find qualified observers. Thus he founded (in April 1905) the Lawrence Fellowship program at Lowell Observatory. Coordinated with Indiana University, the program was ostensibly aimed at giving selected Indiana University astronomy graduates experience with practical observing and a chance to pursue their thesis research for a modest stipend ($50/month). The Lawrence fellows hired on this basis were, successively, John C. Duncan in 1905-06, Earl C. Slipher in 1906-07, and Kenneth P. Williams in 1907. In practice, they devoted almost all their time in Flagstaff photographing the "invariable plane." After some test exposures were by Lampland with the 24-inch Clark (the field was too small), a 5-inch Voigtlander lens and a Roettger 6 3/8-inch lens were both tried. For various reasons, they too proved unsatisfactory. Only in January 1906 did the observatory secure an instrument that was deemed good enough for his purpose. This was a 5-inch Brashear doublet, able to reach a limiting magnitude of 16, with a sharply defined field of about 5 degrees. The Brashear was used to obtain pairs of plates along the invariable plane, and from then until the end of the search, in September 1907, several hundred such plates had been exposed, turning up asteroids *en masse*, as E. C. Slipher informed Lowell. The invariable plane survey finally ended in September 1907, under the third Lawrence fellow, Williams, but Lowell was by then preoccupied with analyzing the results of that summer's expedition to Alianza, Chile, to photograph Mars-led by his friend and fellow planet-searcher David Peck Todd, most of the photographs were taken by E. C. Slipher, and were of such outstanding quality as to lead to his being hired on as a permanent member of the staff. With the completion of the invariable plane survey, the Lawrence fellowship also ended.

This first search was little more than a preliminary effort. It was not the Main Chance by any means. After all, Lowell himself— with many distractions— was still learning in the fly. He rarely sought advice from experts elsewhere—on photography, spectroscopy, celestial mechanics or anything else. On one occasion, he had asked E. E. Barnard of Yerkes Observatory for advice about suitable telescopes for conducting a wide-angle planet search, but that was the exception. It took time for him and his assistants to work things out for themselves. Thus the photographic survey squandered several months of valuable time before the 5-inch Brashear was decided upon. The means of examining the plates obtained was also primitive; Lowell simply placed one plate on top of the other and examined them with a magnifying glass. Lampland, as early as March 1906, suggested the acquisition of a stereo-comparator from the Zeiss company, but Lowell hesitated. "I question whether the advantage of it is worth the price but I will look into the matter," he replied. When he did look into the matter on his trip to Europe that summer, he found the experience "not reassuring."

Simple bad luck also played a role. As Clyde Tombaugh pointed out after the discovery of Pluto, during 1905-07, Pluto's highly inclined orbit placed that planet far from the ecliptic and outside the belt covered by plates with the 5-inch Brashear, and at the time it was only a sixteenth magnitude object —thus at the very limit of the search plates. It was much fainter than Lowell expected Planet X to be.

Inevitably, the first photographic search provided, as Hoyt notes (p. 95) "some sound lessons on how to conduct a search…. It also showed the practical futility of searching at random through trans-Neptunian space for a body whose presence was merely presumed and about which, if indeed it did exist, nothing whatever was known. What was needed, Lowell was well aware, was some indication of where to look, however general in nature, some ‘finger-post’ that would point out the more probable places where a hitherto unseen planet might be hiding among the millions of stars in the sky.”

Lowell was avidly pursuing such fingerposts. Indeed, at almost the very moment he was initiating the photographic survey, he was embarking on a mathematical investigation, which would be based on the


Lowell’s plot of residuals of Uranus, based on Le Verrier’s 1873 theory. The handwriting is Lowell’s, but the plots may have been done, at least in part, by his assistant Miss Williams

plotting of residuals in longitude of both Uranus and Neptune in order to use Herschel’s graphical method to find the direction of any putative perturber. In March 1905, he wrote to Walter S. Harshman, director of the Naval Observatory’s Nautical Almanac Office in Washington, D.C., asking him to recommend “a capable computer” to systematically reduce the residuals in longitude of Uranus and Neptune. Harshman recommended William F. Carrigan, who despite spending all his time during office hours doing grinding computations for the *American Ephemeris and Nautical Almanac*, was eager (or desperate enough) for the extra income to devote as such time as he was able in his spare time to additional computations, and agreed to Lowell’s proposal. Unfortunately, I have found out very little about Carrigan (he died in 1922, though I am not sure of his age at the time). He would probably have made a good subject for a Theodore Dreiser novel. Certainly he deserves to be called, as Hoyt calls him, the “forgotten man in Lowell’s Planet X” search, and there is a tragic aspect to his heroic efforts, for as we shall see, they proved ultimately for naught. (Hoyt, *Planets X and Pluto*, p. 96)

Carrigan found, for the forty-year period from 1780 to 1820, found 151 observations of Uranus made at Greenwich and 171 made at the Paris Observatory. For Neptune, there were only the two prediscovery observations by Michel Lalande in 1795, after which there were no others until Challis’s observations of August 4 and 12, 1846. As he began to get underway, Carrigan sought guidance from Lowell and asked him “with what degree of rigor you desire to have the residuals computed. I ask this in view of the necessity of re-reducing the observations if the utmost accuracy is required.” Lowell’s answer has not survived, but presumably asked for Carrigan to proceed as rigorously as possible, since that is what Carrigan. The work went slowly. Carrigan sent Lowell a first batch of computations in August 1905, together with a letter in which he hinted that Lowell did not, perhaps, fully appreciate just how laborious and time-consuming the work he had asked for could be (Lowell Observatory Archives, Aug. 16, 1905):

I judge from your letter that you are inclined to underestimate the magnitude of the task I have undertaken at your request. As I informed you in a letter written before I begun the work, there are at my command 294 observations of Uranus made during the designated period, viz:— 1780 to 1820. The complete computation required for the comparison of each observation averages somewhat more than three hours. This of course includes duplicating and checking. In the first place I computed a complete heliocentric ephemeris of Uranus at intervals of 192 days, afterward interpolated to an interval of 96 days. From this ephemeris the heliocentric coordinates are interpolated to the date of each observation, the interpolation being made forward and back so that the agreement of the results should be a guarantee of their accuracy. Then the sun's longitude, latitude and radius-vector, with the nutation and the obliquity of the ecliptic are computed from Newcomb's tables of the sun; this computation being in every case duplicated. With these two sets of quantities, viz:— the coordinates of Uranus and those of the sun, a computation of the planets distance is made, which distance is used to determine the aberration. Then with the new time corrected for aberration the places of the sun and of Uranus are again computed and the geocentric longitude and latitude of the planet derived. Since the Greenwich observations in R.A. and Dec. have been reduce to long. and lat. by means of the obliquity derived from Carlini's tables of the sun it is necessary to apply a correction to bring them into accord with Newcomb's obliquity. The computation of the geocentric from the heliocentric places has also been duplicated.

You will understand from the above outline what a mass of work will be required for the comparison of the 294 observations. The work which I sent yesterday covers the period from 1781 to 1790 inclusive, and weighs two pounds and one ounce….

Eight months later, with nothing more forthcoming from Washington, D.C., Lowell followed up to ask Carrigan what was going on. Carrigan responded that he was having serious trouble with his eyes (eye-strain?). At this point Lowell was apparently still happy to let the project proceed in leisurely fashion, since he wrote to Carrigan "I am extremely sorry about your eyes and in consequence do not want to hurry you in the least with my work. I will therefore ask you to send it from time to time …drawing attention if you kindly will by writing in red the discrepancies in the longitudes of both Uranus and Neptune."

Over the next six months, Carrigan's computations (as well as batches of invariable plane plates from Flagstaff) arrived sporadically at Lowell's office at 53 Bay Street, Boston. For Lowell himself the "X" search was not necessarily a priority. He was putting the finishing touches on *Mars and Its Canals* in June, and preparing the series of lectures on Mars and planetary evolution which he would give in October to capacity audiences in Boston's Huntington Hall (the

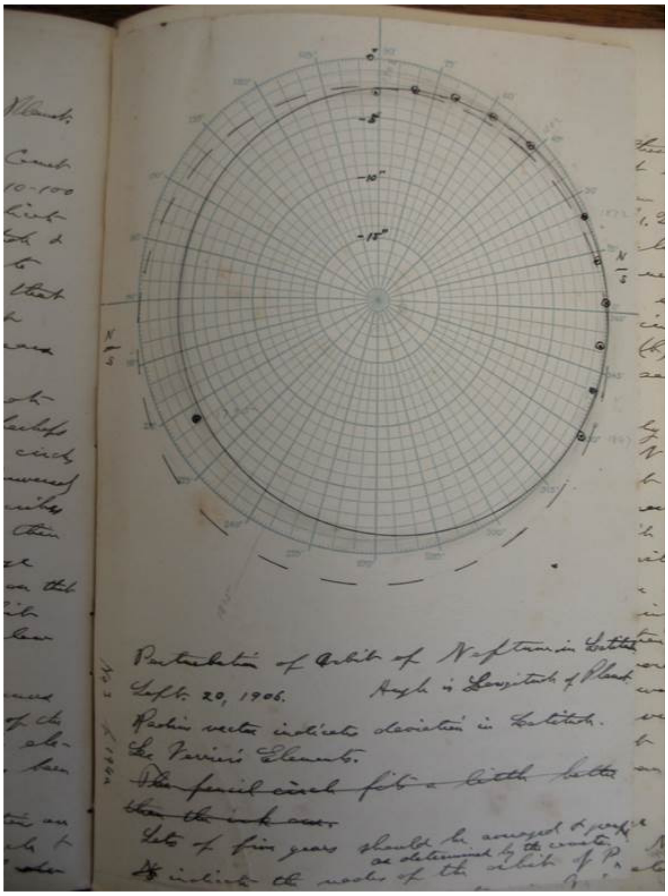
53 State Street Boston, where Lowell had his office and where he and Miss Williams did the calculations for Planet X.

lectures were serialized in *The Century Magazine* and later published in book form as *Mars as the Abode of Life*). As noted earlier, the summer of 1906 he was in Europe, where he checked out the Zeiss stereo-comparator and found it wanting. He did not even acknowledge Carrigan's latest computations until after the Huntington lectures were over, in late October 1906. He then sent Carrigan a check for $228.75 for his work, and told him "I shall take pleasure in looking over your computations when I get a moments time-up to now I have been extremely busy."

Lowell and Carrigan parleyed back and forth into 1908. By March 1908, Carrigan had finally completed the tedious and laborious work on the residuals of Uranus from 1780 to 1820. Lowell was eager to press on; he asked Carrigan to continue from 1820 to the turn of the century, but also advised a more streamlined approach. "I should not take every observation but a few of the best ones only for each [year]," he wrote.

Lowell, of course, was stretched thin. He was as busy with other projects as he had been in 1906-07, including publishing the results of his 1907 Mars observations, publicizing V. M. Slipher's January 1908 spectrographic determination of water vapor in Mars's atmosphere, and preparing the Lowell Institute lectures for publication as Mars as the Abode of Life. He was also negotiating with Alvan Clark and Son for a 40-inch reflector that he hoped would finally vindicate the Martian canals and, in June, went from bachelor to benedict by marrying his Boston neighbor and Constance Savage Keith. The couple set sail for a summer honeymoon in Europe. Hoyt notes that when Lowell returned from Europe in early October, "he made no move to reopen his theoretical trans-Neptunian exploration. Clearly there was nothing urgent about the X search."

A month later, however, the same author writes, "he plunged into the investigation again with furious energy." Why the change?



On November 11, 1908, at a meeting of the American Academy of Arts and Sciences in Cambridge—the same venue in which Lowell would present a summary of his own "Memoir on a trans-Neptunian Planet" a few years later—Lowell was in the audience as William H. Pickering summed up the results of his own parallel investigation of trans-Neptunian space, also underway since 1905. For Lowell was not the only one lured by the prospect of the glory of finding a third major planet, which, as Philip M. Sadler remarks, had been "dangling in front of the astronomical community since 1846." Wanting to avoid the laborious calculations of Le Verrier and Adams, Pickering decided to adapt Sir John Herschel's graphical approach. He began by plotting residuals from Le Verrier's 1873 theory for Uranus— something that was very convenient and labor-saving, since Le Verrier himself had supplied the residuals. (It should be noted, however, that Le Verrier's theory was already out of date, and had been supplanted by revised theories of Uranus by Gaillot and Newcomb, which gave a rather different set of residuals.)

According to Sir John Herschel, the angular speed of Uranus would change in proportion to the perturbing force from the gravitational attraction of the unknown planet and this force would be registered in the residuals. Pickering began with the assumption that a trans-Neptunian planet existed, and basically applied Herschel's analysis to Le Verrier's residuals by finding the best linear and sinusoidal fits to the data to correct Uranus's elements—mean distance, perihelion and eccentricity— from the residuals. Herschel's method was quite satisfactory if the purpose was to correct Uranus's orbital elements from the residuals. Pickering, however, pushed it further: drawing a sinusoidal line in ink upon glass and then superposing this drawing upon the data for the residuals, he attempted a "goodness of fit" match to the graph of the corrected residuals, and by a process of successive curve-fitting— an approach Philip Sadler calls "so subjective that it could produce virtually any curve," and which even Pickering himself admitted later was very "rule of thumb"— isolated a "hair-pin turn" that Pickering believed was caused by the perturbation of the unknown planet on Uranus. From this he deduced the existence of at least one planet, "O" (so-called because O was the next letter in the alphabet after N, for Neptune). Since Pickering used the same residuals as Todd had used in 1877 —those of Le Verrier's 1873 theory of Uranus —there can be little surprise that his result was almost exactly the same. On the assumption of a circular orbit for "O," Pickering deduced that its mean distance was 51.9 AU, its period 373.5 years, and its mass twice that of the Earth. He also gave for its approximate position (for epoch 1909.0): R.A. 7h 47m, Dec. +21°. This would have put it (had it existed) in eastern Gemini at the time. Incidentally, Pluto was then one constellation over, in Taurus, but Neptune was in Gemini, not far away from the position of the putative "O."

Pickering had come up with this position for his planet (as is evident from papers and calculations in the Tulane University archives) earlier in 1908, and had delayed announcing it until after unsuccessful photographic searches had been undertaken at Arequipa with the 24-inch Bruce doublet at the Harvard College Observatory's Boyden station—the same instrument used in 1898 to obtain the plates that led to Pickering's most celebrated discovery, that of Saturn's satellite Phoebe, the first satellite discovered photographically— and by the skillful amateur telescope-maker Rev. Joel Metcalf with a 12-inch refractor at Taunton, Massachusetts. Now Pickering even dropped Lowell a note, inviting him into the game. Lowell's response (on Nov. 16, 1908) was cool, as might be expected. "I am looking up the whole subject myself," he wrote to his long-time Beacon Hill rival, "and have not yet got far enough along to undertake any visual search. When I get a position I will let you know."

In retrospect, it's clear that Lowell owed Pickering's investigation — published in the *Annals of the Harvard College Observatory* early the next year as "A Search for a Planet Beyond Neptune," a 162-page memoir — more than he ever acknowledged. He now began barraging Carrigan with almost daily letters for new data and residuals, as he resolved to try the graphical method for himself. A large number of graphs of residuals can be found in the Lowell Observatory Archives — not only for Uranus, but also for Jupiter and Saturn. On one graph — showing the residuals of Jupiter from Bouvard's tables for that planet — there are three peaks, corresponding to conjunctions with the then-unknown Neptune, leading Lowell to exult to Carrigan (Nov. 19, 1908) that "Neptune could have been found apparently from Jupiter's residuals." (In fact, as Lowell would find out when Pickering published his "*Search for a Planet Beyond Neptune*" memoir, Pickering had already thoroughly covered this ground)

It's obvious that at first Lowell appreciated the importance of having them calculated with the utmost rigor. Thus his patience with Carrigan's time-consuming but extremely rigorous calculations. Later, however, this changed, and Lowell became more careless about the residuals — and I think, at least in part, this was a direct result of Pickering's publication of the results of his graphical analysis of the residuals of Uranus and Neptune that brought investigators like Gaillot and Lowell himself up short and led them to believe that perhaps the signature in the residuals was more apparent than had been supposed.

Lowell wrote in an early manuscript (n.d.; however, presumably after 1908, when Carrigan was cashiered as no longer needed. Probably 1909, when Lowell was beginning his own graphical analyses after reading Pickering's paper. The drift of the paper shows that he still believed that a conjunction of Uranus with an unknown took place around 1794/95.)

"It is a matter of first importance to be sure that the present residuals between observation and calculation are correct. As far as their reduction goes Le Verrier himself reduced them to 1873 and we could have no better guaranty of their accuracy as his detection of the errors in Bouvard's tables sufficiently shows. Since 1873 they have been reduced by the Greenwich Observatory force which also vouches for the correct reductions. So far as the theoretic positions are concerned those may be affected by errors in the elements of the other planets. With regard to those of Jupiter and Saturn the present theories apparently leave little to be desired. With reference to Neptune the theoretic perturbations have been computed, both according to Le Verrier's theory of 1873 and according to Newcomb's of 1900… The divergence between them is due to the different masses assumed for Neptune, that of Le Verrier being 1/18907 and that of Newcomb 1/19300.. "The effect of this can be seen in the diagram of the present observed residuals. The differences due to altered eccentricity and apsidal positions of Uranus assumed are also at once deductible. They do not affect the curve of observed residuals appreciably between 1790 and 1820, but after this they alter it in the direction of rectification till nearly 1900… {direction of rectification: the residuals are decreasing} This is because Newcomb altered them on purpose to conform his theory of 1900 to observation. {well, duh…} It will be seen, then, that they do not destroy the dip in the curve toward 1785-1787 with the rise in toward 1810.— The observed residuals still point to a conjunction of Uranus with an unknown in about 1794.5. We are therefore justified in assuming the existence of such a body." (*To be continued*)

On the Local Dust near Hellas Observed by Maurice VALIMBERTI on 27 August 2014 (λ=186°Ls)

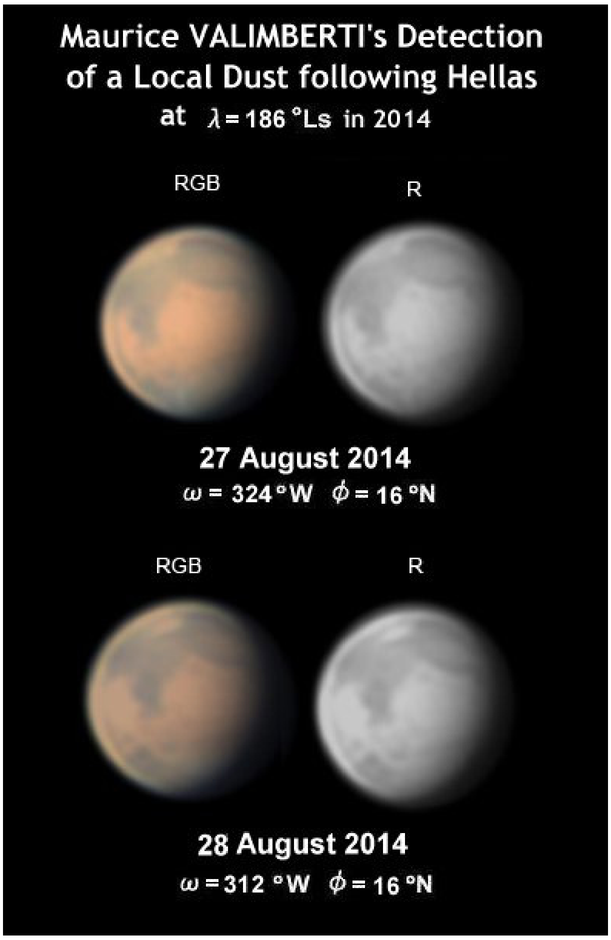
By

Masatsugu MINAMI

ISMO 2013/14 Mars Note (#03)

A local dust patch was conveyed on the images nicely taken by Maurice VALIMBERTI (*MVl*) in Melbourne on 27 August 2014 (λ=186°Ls) at ω=324°W. It was located quite near Hellas which was now shown up near the preceding limb. On the following day at ω=312°W, *MVl* proved that it was following Hellas but was clearly present detached from Hellas.

We here would like to try to consider what position we should give to this dust. We will not be surprised if Maurice must have taken aim at a certain dust event every evening once or twice from 25 August（λ=184°Ls）to 30 August. We should say without his observations the present dust must have been forgotten from our history. The observing conditions were not preferable, since the apparent diameter was going to be under 7 arc seconds and the planet was going to be lower and lower in the evening sky. This apparition, several Australian observers joined more than expected and produced a lot of excellent results. However in August almost all of them stopped observing. We cannot make out what happened, but *MVl* also did not issue any observation in July (as well as other Australian observers), but he resumed again training his telescope to Mars from 11 August. At any rate he was really successful



since his images on 28 August at ω=312°W and ω=320°W surely proved a presence of a definite dust adjacent to Hellas, thus leaving a precious evidence.

From the view-point of the Martian season, since the global dust storm in 2001 was entrained at λ=184°Ls (on 24 June 2001), some important season could come after August, and so we should say Maurice nicely performed an appropriate response. Different from the 2001 case, we should say the dust this time did not prove to have metamorphosed into a great dust storm. On 29 August (λ=187°Ls) the dust surely existed, while on 30 August, it looked dispersed. Since no other member than *MVl* joined the chasing race, it is impossible to say about the aftermath of the dust. Just the atmosphere looked generally dirty, but we regard that no evidence was there to say a thick dust storm governed. However some further considerations will be instructive to the observations in the coming apparitions in 2016 and 2018.

Talking of the dust storm concerning Hellas, it was often said as if the Hellas basin could be the source of the dust disturbances, or numerous dust storms have their origin inside Hellas. However this must be a false idea. The basin is enormous and it's about 7000m deep. Hence the bottom floor is always under a high pressure atmosphere (and its sky may be almost always clear from an air made of slightly heavier particles), and so a bit of inner dust disturbances in the bottom cannot seep out toward the outside. Refer to the following site in CMO #256 for more about our opinions concerning the possible outbreak mechanism:

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmomn0/01Note02/index.htm

The great dust storm in 1956 (entrained at λ=250°Ls) and the more furious storm in 1971 (occurred at λ=260°Ls) started from the area near Hellas but not from the inside. The 2001 case (entrained at λ=184°Ls) was from Hesperia somewhat far from Hellas. The season around λ=250°Ls has been considered to be appropriate for any great dust storm to occur since λ=250°Ls is located on the orbit near the perihelion. Any dust germ needs an enormous energy from the Sun to grow up to ascend higher in the sky to govern the wider area of the upper atmosphere, and hence the perihelion may be a milestone. In this sense, the case in 2001 (λ=184°Ls) is quite extraordinary. Otherwise, E M ANTONIADI reported that in 1924 the Martian surface was covered by a large storm having a creamy colour (like the Jovian surface) at λ=236°Ls. On the other side, it is also well known that the 1973 beautiful dust storm was emerged at λ=300°Ls. The so-called great dust storm does not necessarily occur every year. Thus it is not easy to anticipate the time to aim at the dust outbreaks: It may deeply depend on the activity of the Sun, and hence on the variable effect of the solar wind. The positions of the occurrence places have a variety though some hints can be shown. Some other causes will exist for the variability of the timing. At any rate it is difficult to cope with the problems concerning the emergencies of the greater dust storms.

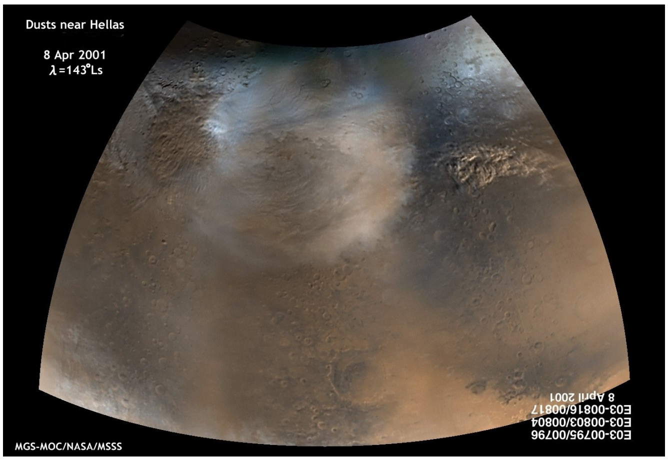
We should here move on to the items of the dusts which are not great, but may work as a trigger to produce some following bigger dust disturbances. In 1971, before the grand storm started at λ=260°Ls, there was observed in Europe a bar-like dust which started at λ=214°Ls near Iapygia down toward NW direction. This soon deflated, but it must have been a sign of the following grand one.

In the case of the 2001 global storm which started much earlier than expected, there occurred some small dust disturbances detected by the MGS-MOC beforehand: Concerning this the following site is given in CMO #268, entitled *Precursory Phenomena Leading to the Early Rise of the 2001 Yellow Cloud*,

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmohk/268Note15/index.html

which describes that "In 2001, MGS-MOC detected two minor dust disturbances near Hellas on 8 April 2001 where λ=143°Ls. This image remains still uploaded:

http://www.msss.com/mars_images/moc/extended_may2001/weather



(published on 24 May 2001). One is seen on the east coast of Hellas, and the other is at Noachis."

At least, in 2001, some small dusts thus occurred earlier at λ=143°Ls quite near Hellas.

Any grand dust storm may be said it will need some local dust disturbances as forebears in order to accumulate the dusty components.

However there are cases where some considerable dust storms do not work as any trigger. In fact, in 2003 at λ=215°Ls it was observed a beautiful and particular dust storm which showed up to cut S Sabæus into two. This place is located not necessarily so far from the Hellas area, while this was not followed by any great dust storm within the 2003 great apparition period. As to this dust the present writer was observing in Okinawa prefecture, and on 4 July at 15:10 GMT (λ=215°Ls), he dispatched the following notice by email to Masami MURAKAMI at Kanagawa prefecture who then published it in "Director's Notes in 2003"

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmomk/DN1.html

which can be read even now from the Façade of the CMO Web: "Clear Dust in Deucalionis Regio: ISHADOH and MINAMI observed at ω=331°W that the eastern half of S Sabæus was clearly covered out by a dust from Deucalionis R. Western half of S Sabæus and S Meridiani looked however very darker in a chocolate colour than usual." This was written before the accomplishment of *Mn*'s first observation (which was made at 15:30 GMT) of the day. This dust however remained of mid-sized and did not metamorphose into a great dust. Further this could not work as a trigger to summon a final possible grand dust storm. On 4 July 2003, the sky of the western part of S Sabæus (+ S Meridiani) was clear because the area was under a high pressure air in contrast with the dusty eastern part of S Sabæus. To see our opinions concerning this dust, refer
to the following site in CMO #288:

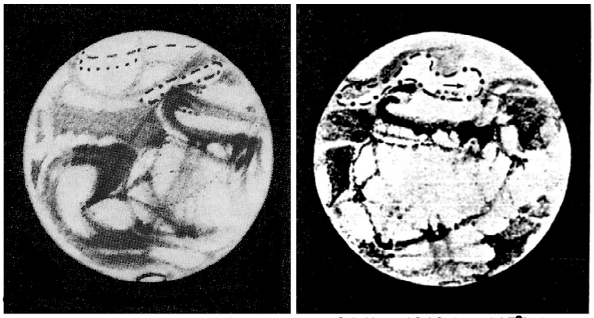
http://www.kwasan.kyoto-u.ac.jp/~cmo/cmomn3/288Note01_03/index.htm

We are now in a position to describe a case in 1969 where near Hellas a local dust streak was observed by C F CAPEN.

C F CAPEN was observing Mars by the use of a 208cm reflector of the McDonald Observatory, and left drawings. The first drawing here was taken on 29 May 1969 (λ=164°Ls). There is indicated a presence of a dust streak along the west coast of Hellas.

This clearly corresponds to the present *MVl* case though the season is earlier than the case of CAPEN. This dust was also

C F CAPEN's Drawings in 1969 by the Use of a 208cm Refl



29 May 1969 (λ=164°Ls) 31 May 1969 (λ =165°Ls)

sketched on 31 May 1969 (λ=165°Ls) on the day when the planet Mars was at opposition. This dust looks to show a westward movement, while we should stress here that the dust is not the one that gushed out from the inside of Hellas. As far as the present writer remembers, there was no occurrence of any great dust storm in the 1969 apparition.

> As to the observations by CAPEN in Texas in 1969, Bill SHEEHAN commented in his Book "*The Planet Mars -A History of Observation and Discovery*", The University of Arizona Press, 1996 at p168 as follows (based on personal communication between C F CAPEN and W SHEEHAN on 24 April 1983
>
> ----In May 1969, with Mars near opposition, Capen enjoyed a series of splendid views with the 82-inch (2.08-m) reflector at McDonald Observatory in Texas. "When Mars was first brought into focus on the nights of May 29, 30, and 31, its globe appeared to be draped in a dark gray spiderweb, resembling the shade and texture of iron-filings. When 1,000× was employed the global network . . . resolved into dark circular features and parallel aligned streaks, some of which were fortuitously aligned into canal-like lineaments." These details he afterward identified with large craters and composites of dark blotches and streaks shown on the spacecraft photographs.

We shall close this Note by referring finally to a local dust in 2003 occurring this time far from the Hellas area but at the middle latitude region. We note that VALIMBERTI and others detected in 2003 a local small dust around Xanthe at λ=214°Ls (the same season as the first dust streak appeared in 1971). This was explained in CMO #289:

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmomn3/289Note02_03/index.htm

In this Note, some data are introduced which are compiled from the article by B CANTOR and others entitled "*Martian dust storms: 1999 Mars Orbiter Camera observations*" (*JGR*, **106** (2001) 23653). For example, they found a total of 783 small dusts during the seasons λ=109°Ls~273°Ls in 1999 based on the images taken by the MGC-MOC. The seasons related are at λ=135°Ls, 150°Ls, 161°Ls, 162°Ls, 165°Ls, 187°Ls, 193°Ls, 203°Ls, 210°Ls, 219°Ls, 220°Ls, 223°Ls, and 227°Ls. In the period λ=160°Ls~165°Ls, there were found dusts at the northern higher latitude regions (just from the time of the smallest north polar cap appearance), and during λ=210°Ls~227°Ls the dusts emerged from the mid-latitude region. The season λ=214°Ls hence belongs to this period. Note however this classification was due to the results in 1999.

Finally we comment that Clyde FOSTER (*CFs*) in the present apparition detected a vivid local dust at the northern end of Xanthe at the season λ=209°Ls on 6 October 2014 (cf CMO/ISMO 2013/14 Mars Report #16 in this issue).

CMO/ISMO 2013/14 Mars Report #16

2013/2014 Mars Observations in October 2014

♂・・・・・The planet Mars moved from Sco to Sgr in October, and on 26 October 2014 and its apparent declination took the maximal degree -24°57' (nearly 25°S). The angular diameter went down from δ=6.1" to δ=5.6". The Martian season proceeded from λ=206°Ls to λ=224°Ls and so the season of the southern dust disturbances came. The local dust was reported on 6 October at the northern end of Chryse-Xanthe and so on. The tilt quite moved from φ=6°N from φ=3°S, and hence the southern hemisphere began to face a bit largely to us. The south polar cap (spc) was so visible. The arctic cloud was variable, depending on the activity of the airborne dust. The phase angle ι decreased a bit from ι=39° to 36°. On 19 October, an Oort cloud comet C/2013 A1 (Siding Spring) passed Mars very closely.

♂・・・・・This period, we have received no more than 21 reports from three observers. Among them, Clyde FOSTER (*CFs*) from South Africa was performing solo almost every evening. Just Maurice VALIMBERTI (*MVl*) observed twice left alone from Australia, and Yukio MORITA (*Mo*) in Japan had no more than a chance: Conditions from the Northern Hemisphere remained worse.

♂・・・・・The following are the observers names and data of the days they performed the observations.

FOSTER, Clyde *(CFs)* Centurion, SOUTH AFRICA

17 Sets of *RGB* + 1*R* + 17 *IR* Images (4, ~8, 11, 12, 14, 15, 17,~20, 25, ~29 October 2014)
36cm SCT @*f*/*33* with an ASI 120MM

MORITA, Yukio (*Mo*) Hatsuka-ichi, Hiroshima, JAPAN

1 Set of *RGB,L* + 1 *LRGB Colour* + 1 *L* Images (19 October 2014) 36cm SCT with a Flea 3

VALIMBERTI, Maurice *(MVl)* Melbourne, AUSTRALIA

2 Sets of *RGB* + 2 *IR* Images (5, 20 October 2014) 36cm SCT @*f*/24 with an ASI 120MM

♂・・・・・We shall now give a short review to each observation in October chronologically:

4 October 2014 (λ=208°Ls, δ=6.0")

Clyde FOSTER (*CFs*) obtained a set composed of three primary colour ingredients + IR742 component at ω=069°W from which he composed an RGB image. This combination is his standard one recently employed. The present images are considerably excellent to the effect that the south polar cap (spc) is well visible and its preceding end has a bright core. The central part of the spc at present should be shadowy (on the rear side, since the season around λ=200°Ls) and the peripheral part must be made of a series of bright beads. Especially the spc part which will deviate from the centre of the pole from around λ=230°Ls (the area which surrounds Hypernotius Mons) must be much brighter and so the area stands now out conspicuously as a bright core at the preceding area seen from the Earth side. Solis L is caught as a dark isolated big patch (darkest on IR) and the area beyond Thaumasia is also well dark. The area around Aurorae S is also dark, and this *CFs* R image shows well the linkage of Aurorae S with Tithonius L. Ophir-Candor is evident in IR. On the northern hemisphere, the densities of Lunae L, Nilokeras, and the southern part of M Acidalium (maybe the area of Niliacus L) are well rendered delicately. The northern part of M Acidalium is misty: The misty part is bright in G, and the arctic cloud part is large and bright in B, though fainter in IR. The spc is not particularly depicted brighter. We however worry about the bluish tonal structure of the dark markings here employed.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141004/CFs04Oct14.jpg

5 October 2014 (λ=208°Ls~209°Ls, δ=6.0")

Maurice VALIMBERTI (*MVl*) gives a set of three primary images together with an IR image and composed an RGB image at ω=310°W. On the RGB image, Syrtis Mj appears among a well tinted surface, shown also R and G, and S Sabæus is faintly seen near the morning terminator. Hellas does no more show the white colour. The npc is of a favourable shape in B. Markings, including Syrtis Mj, are not clear cut and hence the RGB is not clear. However the colour tone is soft and is so appropriate to the planet Mars. The tint of the deserts is nicely subdued.

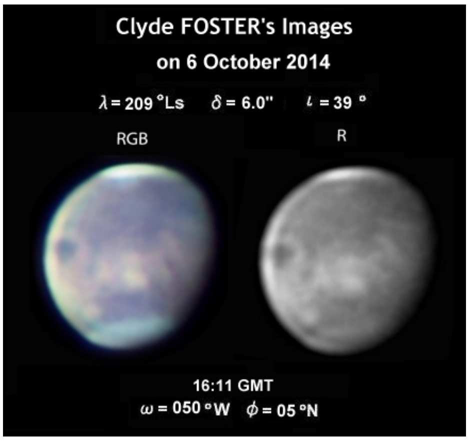
CFs obtained a set of images (including IR) at ω=059°W. The R image is good: Solis L went to the neighbourhood of the morning terminator, while the nails of S Meridiani appeared as if one nail near the preceding limb. Margaritifer S is also well visible. The spc is much brighter at the eastern side, and sends a misty matter to the area of Argyre. M Erythræum is irregularly fainter, while Auroræ S is dark up toward south. This dark segment and Margaritifer S pinches the fainter area of Eos and others. Ophir-Candor is bright. The arctic area is light in G, as well as in B, and a bit seen also in R. On the RGB image the cloudy matter is seen on the left so that M Acidalium does not make a usual shape. Nilokeras is visible.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141005/MVl05Oct14.jpg

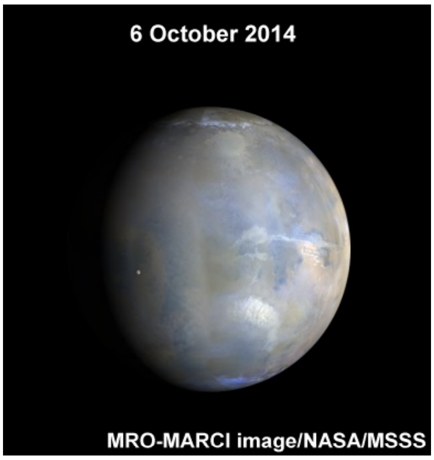
http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141005/CFs05Oct14.jpg

6 October 2014 (λ=209°Ls~210°Ls, δ=6.0")

CFs gives a set of images as well at ω=050°W. Preceding side of the spc is brighter, and a mass of water vapour looks like going down toward Argyre. The mass may also be slightly going upward as seen in B. S Meridiani is now darker while Solis L became fainter near the terminator. Ophir-Candor is visible. Auroræ S is still dark. Ganges and Nilokeras are definite. There is seen a dust patch located near Nilokeras to the north of Chryse-Xanthe. This is also light in G, and in RGB shows a brighter colour than the desert. Maybe a typical local dust. It may be also possible to identify another weaker dust between Oxus and M Acidalium. These disturbances must have been caused when the areas passed the dawn line where they received a severe change in temperature. Another characteristic of the arctic cloud area on the day is that the core of the cloud is located, in contrast to the cases on the preceding days, near to the morning terminator. This may imply an occurrence happened of a severe ascending disturbance of a low-pressurfe air mass at the terminator line. The results of the disturbances may maintain throughout the daytime until the night.



Here is shown for comparison an image of the case of 6 October extracted from the MRO-MARCI Weather Reports (see 6 October 2014 - 12 October 2014 released: 15 October 2014). It gives an idea how



a closed-up dust image can be caught on the terrestrial bases. This case given by *CFs* well verifies the fact that the dust phenomenon can be caught on the Earth even if the apparent diameter δ of Mars is merely 6 arcsecond under some decent conditions. It is also interesting to notice that another dust seen around Agathodæmon on MRO-MARCI is not so evident on *CFs*'s image. It will also be instructive to see how differently every day the MRO-MARCI images on 4 October, 5 October and so on show the dust phenomena. This kind of disturbance can be found even in mid-October.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141006/CFs06Oct14.jpg

N.B.: Reiichi KONNAÏ already made a comparison between *CFs*'s images on 6 Oct with the MRO-MARCI correspondent. See the line Received on 18 Oct:

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmo/ISMO_LtE428.htm

N.B^{bis}.: By the way, in 2003 at λ=214°Ls, not so different from the present season, some CMO members as VALIMBERTI, PAU, NG, KUMAMORI, ISHADOH, and MINAMI detected a local dust in Xanthe near Nilokeras on 2 July 2003. This was summarised in CMO #289 and so please try to refer to

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmomn3/289Note02_03/index.htm

7 October 2014 (λ=210°Ls, δ=6.0")

CFs brought a set of images as usual at ω=038°W. The images are excellent including R and G as well IR and RGB. The spc is bright. S Meridiani is further dark. Margaritifer S is definite, and together with the dark Auroræ S, they surround the area of Eos and others. M Acidalium is a little too short because of the large arctic cloud, while Niliacus L takes form and from there Oxus runs eastward. The area between Oxus and M Acidalium is slightly more reddish than other deserts. The large arctic cloud looks declined toward east seen in G which is in contrast to the case of the preceding day. The bright dust on the preceding day looks to have dispersed if we depend on the MRO-MARCI.

These configurations of markings are reminiscent of the surfaces seen in 1954, 1969, and 1986. For example, in 1954, the season λ=210°Ls hit the day 7 Aug 1954 where the tilt was φ=4.2°N. However concerning the angular diameter it was a world of difference and it read δ=18.4". Incidentally one of the present writers (*Mn* at the age of 15) used to observe at the Fukui City Observatory equipped with a 15cm refractor. However on the day he observed just around ω=312°W, ω=330°W, and the angle ω=042°W was already obtained on 4 August 1954 (λ=209°Ls). In the case of 1969, on the day 17 Aug 1969 (λ=210°Ls), the tilt was slightly deviated to φ=7.9°N while δ=12.9". In the case of 1986, the tilt is different as φ=4.4°S on the day 21 July 1986 (λ=209°Ls). The diameter was however enormous as δ=23" since Mars approached most on 16 July 1986. Similar configurations are expected in 2016. (In 2001, the configuration was broken because of the global dust event).

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141007/CFs07Oct14.jpg

8 October 2014 (λ=210°Ls~211°Ls, δ=6.0")

CFs gave a similar set of images at ω=027°W. S Meridiani quite came into the disk, and suggested the two nails. Margaritifer S was conspicuous in IR. Auroræ S is dark near the morning terminator. Others were ordinary. Somewhat a misty streak from the spc is seen down to the area of Argyre which is clear in R and G. It looks like Niliacus L is separated from M Acidalium. Oxus is definite in RGB. The arctic cloud is thicker at the eastern part.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141008/CFs08Oct14.jpg

11 October 2014 (λ=212°Ls~213°Ls, δ=5.9")

CFs obtained a similar set of images at ω=359°W. S Sabæus is totally visible. From M Serpentis a dark band runs southward. Depressiones Hellesponticæ looks dark. Aram is slightly lighter. Margaritifer S shows to be dark. In IR, Oxia Palus shows up. The spc is not so clear, while it looks to swell in B. No core is seen inside the arctic dull mist.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141011/CFs11Oct14.jpg

12 October 2014 (λ=213°Ls, δ=5.9")

CFs gave a set of images at ω=351°W: The configuration of S Sabæus and others is reminiscent of some 1954 Mars images. In R, the twin nails of S Meridiani are visible. In R and G, Syrtis Mj is apparent near the preceding limb. In R the southern part of Syrtis Mj is fainter. There is seen a dark band from M Serpentis to Depr Hellesponticæ. The dark fringe of the spc is explicit. There may be a misty projection of the spc along the terminator. The arctic cloud expands weakly.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141012/CFs12Oct14.jpg

14 October 2014 (λ=214°Ls, δ=5.9"~5.8")

CFs proceeded to obtain a set of images at ω=330°W: The northern part of Syrtis Mj is well captured, but the procedure around the preceding limb is not good enough. S Meridiani moved to the morning side. The area of Depr Hellesponticæ looks complex and connected with the dark fringe of the spc. In R and IR, Boreosyrtis is visible. The arctic cloud is conspicuous in B, but looks dull in other ingredients.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141014/CFs14Oct14.jpg

15 October 2014 (λ=214°Ls~215°Ls, δ=5.8")

CFs has got a set of images at ω=332°W similar to the angle on the preceding day. The colour balance is however different and this one is quite better: The RGB image shows a natural balance. It is thus appropriate to see Hellas. Its inside is full of dirty air and independent from the spc. R's procedure is also better, and S Meridiani and S Sabæus are nicely depicted. The area of Depr Hellesponticæ is complicated. In R and IR, the dark fringe is conspicuous. The area of Boreosyrtis is similar to the previous one. The north polar cloud is docile.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141015/CFs15Oct14.jpg

17 October 2014 (λ=216°Ls, δ=5.8")

CFs took a set of images at ω=302°W. The images look slightly coarser. Hellas came in the central area of the disk. The western part of the basin is lighter maybe due to the inner dusty air. The aspect of Hellas and its surroundings suggests that the atmosphere has become largely dusty. The neighbourhood to the east of Syrtis Mj looks light, but not particularly irregular on the MRO-MARCI images. The arctic cloud has shrunk (due to a presence of dust?).

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141017/CFs17Oct14.jpg

18 October 2014 (λ=216°Ls~217°Ls, δ=5.8")

CFs gave a set of images at ω=294°W. Seemingly Syrtis Mj is at the centre of the disk. Inside Hellas, there is seen a shadowy band. Hellas' perspective looks narrower than usual because of a deformation of the northern part. On MRO-MARCI's image there is seen a singular disturbance at the area of Iapygia Viridis. There is a lighter patch at Libya to the east of Syrtis Mj on *CFs*'s image, while it is not so evident on the MRO-MARCI image. The latter image shows a strong disturbance near the Nepenthes area instead. The area of Hellas at this season will be a point to be chased anyway in 2016 and so this *CFs* image should be remembered. Incidentally, the area of Xanthe is now out of scope of *CFs*, while the MRO-MARCI proves the presence of a carbon copy of the dust detected on 6 October at the same place.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141018/CFs18Oct14.jpg

19 October 2014 (λ=217°Ls, δ=5.8")

Yukio MORITA (*Mo*) took a set of R, G, B, and L images at ω=186°W and composed RGB and LRGB images. The spc is well suggested, but the L image is not good enough and so the LRGB image does not work. Rather IR image is recommended if the dark markings are wanted. Just M Sirenum is suggested. The arctic cloud looks weak.

CFs obtained a set images at ω=282°W. Syrtis Mj moved westward by about ten degrees compared with the images on the preceding day, and so Hellas is situated better to be watched. The disturbance around Iapygia Viridis looks very singular and so interesting. Unfortunately the moving images of MRO are broken at these areas, though just the dust disturbance at western border of Hellas is well shown. *CFs*'s images, especially R and G images, shows a bar-like bright dust streak to WN direction from Iapygia, but it is not so evident on MRO images (where the Huygens crater is apparent). At the area to the east of Syrtis Mj there may exist a spread of dust. The whole atmosphere looks dirty in general (due to the floating of dusty matter).

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141019/CFs19Oct14.jpg

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141019/Mo19Oct14.jpg

20 October 2014 (λ=217˚Ls~218˚Ls, δ=5.8"~5.7")

Maurice VALIMBERTI (*MVl*) gave a set of R, G, B, and IR images and composed RGB at ω=171°W. The spc and the dark markings are shown best by the IR image, while the R image best shows a lighter band adjacent to the dark fringe of the spc. The northern polar cloud is shown on G and B.

CFs shows a set of images at ω=279°W. Syrtis Mj is shown on the morning side: The shape of the northern part of Syrtis Mj is nicely depicted in R and IR. The inside of Hellas looks complicated, and the contour of Hellas is also not smooth due to the dusty atmosphere. The spc is the brightest in G and shows a faint swelling in B. The bar-like streak observed on the preceding day looks to remain. The southern part of Trinacria shows definitely a dusty aspect which looks complex in R. M Serpentis takes a good shape, but its north is disturbed by dusts. The arctic cloud is rather invisible.

The MRO-MARCI image on 20 October is unfortunately almost broken, but the one on the following day (21 October) shows the Hellas to Æria area more clearly: The disk is quite dusty and especially the singular dust complex observed on 18 October near Iapygia looks to stay on the 21 October disk and this must be reflected in a bar-like dust streak on *CFs*'s images..

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141020/MVl20Oct14.jpg

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141020/CFs20Oct14.jpg

25 October 2014 (λ=221°Ls, δ=5.7")

CFs gives an R image at ω=215°W. The spc and M Cimmerium feature on the disk, and the area of Elysium is suggested. The corresponding MRO image is almost broken, but the light streak along the Ætheria Dark patch is finely depicted.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141025/CFs25Oct14.jpg

26 October 2014 (λ=221°Ls~222°Ls, δ=5.7"~5.6")

CFs obtained a set of images at the same angle as the one on the preceding day at ω=214°W. M Cimmerium largely occupies the central area of the disk, and the aspect in R and IR is good but the ant-leg like projection is obscure. The southern end of Hesperia is clearly bounded by a dark marking, and the southern Ausonia is light. The spc on the RGB image is whitish bright. Elysium is obvious. The arctic cloud is visible light, but not large, not tightened even in B. On the corresponding MRO image, adjacent to the dark fringe of the spc, seen is a light belt as if a sandy matter is delivered.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141026/CFs26Oct14.jpg

27 October 2014 (λ=222°Ls, δ=5.6")

CFs obtained a set of images at ω=208°W. Still M Cimmerium lies largely, and Elysium is seen on the morning side. The arctic cloud looks to have much shrunk on this day.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141027/CFs27Oct14.jpg

28 October 2014 (λ=222°Ls~223°Ls, δ=5.6")

CFs gave a set of images at ω=196°W. M Sirenum is visible, and the area of Symplegades Insulæ is lightly vacant. A small dark spot is visible near Læstrygonum S. This may be a part of Valhalla.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141028/CFs28Oct14.jpg

29 October 2014 (λ=223°Ls~224°Ls, δ=5.6")

CFs got a set of images at ω=184°W. The spc is bright (especially in G) and in R it shows a brighter spot at the lhs (evening side). On the contrary the arctic cloud has shrunk and just shows a small core. M Sirenum is the main dark marking.

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141029/CFs29Oct14.jpg

Note: To tell our thoughts when reviewing in this issue, we should like to make a pursuit of some global watching on the occasion of the next apparition after λ=180°Ls of the local dust disturbances on the Martian southern hemisphere so frequently as *CFs* performed this apparition. The apparent declination will be quite low from our Northern Hemisphere, but we now feel keenly it is quite necessary to organise a collaboration to chase the phenomena globally.

Masatsugu MINAMI & Masami MURAKAMI

Letters to the Editor

●·····*Subject: Mars 2 December*
Received: 3 December 2014 at 03:08 JST

Images from this evening. Mare Cimmerium very prominent. .... Best regards

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141202/CFs02Dec14.jpg

○····*Subject: Mars 29 December*
Received: 30 December 2014 at 04:39 JST

Good evening all, After a number of weeks of bad weather (which fortunately coincided with the first few weeks of the recovery from my operation), conditions were clear this evening, and I thought I would see if I could pick up any detail on Mars. Managed to capture a few of the major features: Hellas, Syrtis Major, Sinus Sabeus, Hellespontus and Mare Australe. SPC small, but clear. Down below 5" and at a distance of just over 182 million miles. A belated happy xmas to you. Best regards

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141229/CFs29Dec14.jpg

○····*Subject: Re: Mars 29 December*
Received: 31 December 2014 at 14:00 JST

Thanks, Jim, Must admit the weather the last few weeks this year has been terrible. Normally in Gauteng we have stunning weather during the day, sometimes thundershowers in the afternoon and clear evenings. Anyway, the bad weather coincided with my recovery period, so that helped keep my frustration levels down...!! However, I certainly cannot complain. You will note that over the last few months that work commitments have been the reason for me not capturing my patrol images, rather than weather..... To capture any form of blue image, I have to wait now at least until about 19.00 local time. At that time the elevation is dropping and seeing has deteriorated. One way to at least continue monitoring is to take R and IR images a bit earlier (almost daylight conditions). Would appreciate any comments as to whether this is of value. Best regards and I hope you all enjoy your

New Year celebrations!

Clyde FOSTER (Centurion, SOUTH AFRICA)

●·····*Subject: New DeTeCt: planetary software*
Received: 9 December 2014 at 16:30 JST

Dears, With Jupiter higher in the morning sky, contribute to planetary science by using this simple software on your videos for detecting Jupiter impacts by sending me your results (no detection also counts, we already have more than 23 days of analyzed !). New version 2.0 of DeTeCt software is now released, with the following changes:

- More robust version (no crashes) compiled under Visual Studio with candidates estimation
- Support for all latest Firecapture 2.4, Genika 2.7.x, Sharpcap 2.1 and PLXCapture 2.3 logs
- Better duration and datation calculation (FITS, Firecapture, ...)

Thanks to Xavier Dupont, Michel Jacquesson, Paul Rolet, Marc Patry, Alan Coffelt and Thomas Ashcraft for tests before publication!

To download this software, go through the web page of the Jovian impact frequency project here :

http://www.astrosurf.com/planetessaf/doc/project_detect.shtml

Regards,

Marc DELECROIX (Tournefeuille, FRANCE)

●·····*Subject: Re: CMO 429*
Received: 19 December 2014 at 02:34 JST

Dear Masatsugu, The second note reviewed the seasonal evolution of the cloud front activity in 2014. Curiously, it looked to fade completely after λ=135°Ls. I have an intention to give some coming Notes: a) Identification of particular fronts on images, trying to explain the observed shapes. b) Front movements (daily evolution, wind measurements if possible). And then back to other topics :

This early winter in Nantes is typical, warm and wet: 2014 will be the warmest year on record in France, in my garden some kind of vegetation is still alive when it should have been destroyed by morning freezes many weeks ago! Take care with the cold, hopefully it brings clear skies as well...

Best wishes,

Christophe PELLIER (Nantes, FRANCE)

●·····*Subject: Mars Alerts and Recent Observations Page*
Received: 26 December 2014 at 14:08 JST

Hi Mars enthusiasts. Click on link below (Mars Current Apparition Highlights) :

http://www.alpo-astronomy.org/marsblog/2013/10/20/201112-features-observations/

Jim MELKA (Chesterfield, MO)

●·····*Subject: Happy new year 2015.*
Received: 31 December 2014 at 22:54 JST

Have a happy new year!

Zlatko F. KOVACEVIC (Virovitica, CROATIA)

●·····*Subject: Mo23Dec_14.*
Received: 1 January 2015 at 17:34 JST

Happy New Year! Snowy on this new year day. Recently the seeing has been terrible. Just on 23 Dec 2014 the air looked stable, though the B image was very poor. Best regards,

http://www.kwasan.kyoto-u.ac.jp/~cmo/cmons/2013/141223/Mo23Dec14.jpg

Yukio MORITA (Hiroshima, JAPAN)

●·····*Subject: Planet X article.*
Received: 13 January 2015 at 07:35 JST

Dear Masatsugu, (I have started writing a piece on P vs. L as calculators of the trans-Neptunian planet.) Here is an image of Bill Putnam, who was Lowell's great nephew (son of Roger Lowell Putnam) and died recently, with me standing in front of the 6-inch Clark at the time of the transit of Venus in June 2012

○····*Subject: Pluto article*
Received: 22 January 2015 at 07:09 JST

Dear Masatsugu, No time at all today; I don't know about tomorrow or Friday, but I promise something by Monday. Thanks for your patience.

Bill SHEEHAN (Willmar, MN)

☆ ☆ ☆

☆ *Any e-mail to CMO/ISMO including the image files is acknowledged if addressed to*

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