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TREADING CAREFULLY: V.M. SLIPHER, C.O. LAMPLAND, E.C. SLIPHER AND THEIR AMBIVALENT RELATIONSHIP WITH PERCIVAL LOWELL'S MARS

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Abstract: V.M. Slipher joined Lowell Observatory as an assistant in 1901, C.O. Lampland the following year, and E.C. Slipher, V.M.'s younger brother, in 1907. All three were to remain loyal to the observatory's founder Percival Lowell, and thus avoided the fate of Andrew Ellicott Douglass, who was dismissed in 1901 for having developed independent views (questioning the reality of the markings Lowell had seen on Mars and especially Venus). Because of Lowell's tendency to generate controversy, the observatory was largely ostracized by professional astronomers during Lowell's lifetime, though his assistants—especially V.M.—managed, by eschewing controversial views and publishing rather sparsely—to gradually build up respect for the observatory in the years after Lowell's death in 1916. After a draining legal battle with Lowell's widow, the observatory's finances were stabilized and in 1927 Roger Lowell Putnam took over as Sole Trustee, marking a new era of relative prosperity for the observatory. Putnam's priorities were to revive the founder's unfinished legacies: to update Lowell's Mars theories, and to search for Planet X. The first priority called on the three senior staff members to write what I have called the "Great Lowell Observatory Mars Book," and considerable effort was expended. For reasons explained here, it was never completed, though rather belatedly, E.C. Slipher did produce a book on Mars in 1962 that comes as close as anything to its fulfillment.

Keywords: Percival Lowell, Mars, Venus, A.E. Douglass, Lowell Observatory, V.M. Slipher, C.O. Lampland, E.C. Slipher, Mars photography, spectrograph studies of Mars, W.W. Coblentz, radiometry of Mars, Roger L. Putnam, Clyde Tombaugh, Pluto, Arthur Adel

1 PROBLEMS WITH VENUS

Today, of course, Percival Lowell (1855–1916) is remembered mainly for his work on Mars and his theory of intelligent life on that planet (Dick, 1996). What is not often realized is that during the first several years of his astronomical career, he became embattled in defending not so much his theories about Mars which, though controversial, were regarded as plausible by many astronomers and won over the general public but about Venus—in particular, a curious hub-and-spoke system which he and his staff at Flagstaff were almost alone in seeing and defending against unusually harsh and bitter criticisms from other astronomers. Indeed, many of the most consequential decisions he took, which would later produce such startling results as those on which the expansion of the Universe would be inferred, were taken, first, with an eye to defending his Venus observations.

At the end of his first campaign of Mars observations in 1894, for which he had borrowed an 18-inch Brashear refractor (see Taibi, 2019), Lowell was pleased with his success and decided to make what had originally been planned as a temporary observing station a permanent institution. He could not, however, satisfy himself that Flagstaff was the best site for it. The elevated situation on Mars Hill, originally vaunted for the superior quality of its air for planetary studies, began to look increasingly problematic based on reports from his assistant, Andrew Ellicott Douglass (1867–1962; Webb, 1983), during the winter of 1894–1895. (Lowell, of course, had gone back to Boston rather than winter over.) Douglass' employer, therefore, set about pursuing a two-pronged strategy. He would order a permanent replacement for the Brashear, a 24-inch refractor from the firm of Alvan Clark & Sons (with specifications for a short focal ratio of f/16.0 in order to be able to fit the telescope in the same dome that had been used for the 18-inch), while at the same time testing seeing conditions elsewhere to see if a better site could be found than Flagstaff.[1]

In August 1896, Lowell and telescope-maker Alvan Graham Clark (1832–1897) arrived in Flagstaff to accept delivery of the 24-inch Clark telescope (Figure 1). The weather that summer had been monsoonal, and by late September Lowell would order Douglass to resume considerations of the Mexico option. He also had commissioned two local handymen, the Sykes brothers, Godfrey and Stanley, who operated a bicycle repair shop in Flagstaff but advertised themselves as "… makers and menders of anything …", to construct a unique 42-foot wooden cylindrical dome to house the new telescope (for details, see Hoyt, 1976; Putnam, 1994; Webb, 1983). They did so without any plans—and did not even know what the diameter should be; Percival, who expected self-reliance in the "… makers and menders of anything …" was actually somewhat peeved when Godfrey asked for direction in the matter. Lowell's previous plan to have the 24-inch Clark built to specifications that would allow it to fit into the dome used in 1894 now went for naught, since the dome was replaced anyway, but as a result of it, the Clark—though optically as good as any instrument the Clarks ever made—would be handicapped by serious problems of chromatic aberration

With Mars still a long way from a not very favorable opposition, Lowell put the telescope through its paces on Mercury and Venus. He was joined by his secretary Wrexie Louise Leonard (1867–1937, who had accompanied Lowell to Algeria and the Sahara), Douglass, and three new assistants, Wilbur A. Cogshall, Daniel Drew and Dr Thomas Jefferson Jackson See.

A veteran of only several months' observing time at the 1894 Mars opposition, Lowell may have been a fast learner, but in fact was still very much a neophyte when it came to planetary observation. Already, at the end of 1894, James E. Keeler (1857–1900; Campbell, 1900) of the Allegheny Observatory (and co-founder of the *Astrophysical Journal*) had written to his colleague George Ellery Hale (1868–1938; Adams, 1939):

> I dislike [Lowell's] style ... it is dogmatic and amateurish. One would think he was the first man to use a telescope on Mars, and that he was entitled to decide offhand questions relating to the efficiency of instruments; and he draws no line between what he sees and what he infers. (Keeler, 1894).

After receiving several (in their opinion) substandard articles submitted by Lowell for the *Astrophysical Journal*, Keeler and Hale took the rather dramatic step of completely banishing Lowell from the publication, forcing him to establish other outlets for his work that he could control—the observatory *Annals* and, later, the *Bulletins* of the Lowell Observatory.

Lowell had always been a great admirer of the Italian astronomer Giovanni Virginio Schiaparelli (1835–1910), who had not only first called attention to the 'canals' of Mars but had concluded, from his long-term studies of the difficult detail visible on Mercury and Venus, that those two planets were in isosynchronous

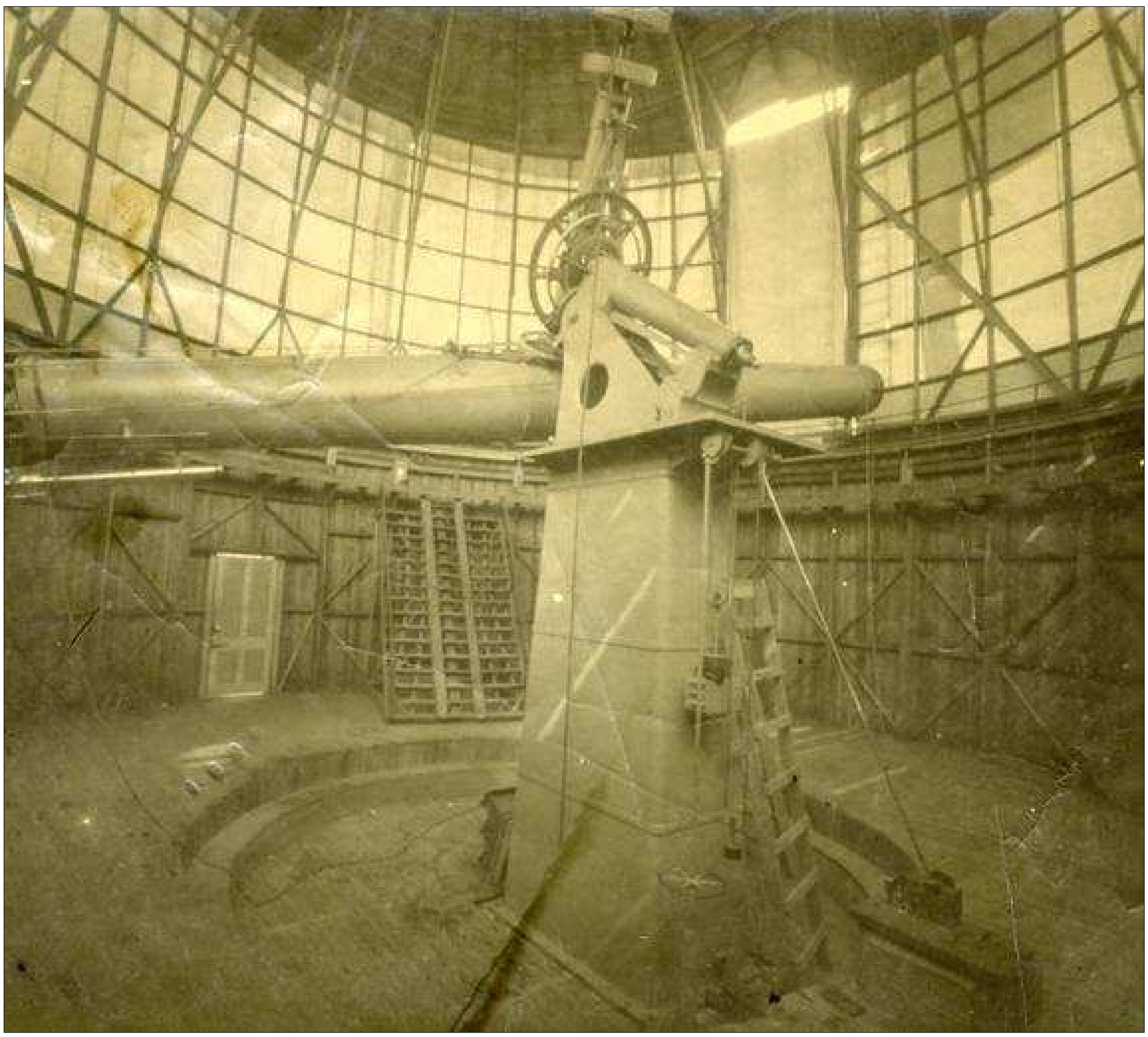

Figure 1: In the autumn of 1896 the 24-inch refractor is mounted in the dome built by the Sykes brothers on Mars Hill (courtesy: Lowell Observatory).

rotation, that is, their periods of rotation and revolution coincided, so that they kept (except for a small wobbling due to librations like those of the Moon with respect to the Earth) the same face always turned toward the Sun. Schiaparelli had observed the inner planets by daylight, and Lowell did the same. He also immediately confirmed Schiaparelli's results.

Lowell's drawings of Mercury were certainly rather bizarre. Even Schiaparelli later commented on his memoir about the planet, "It terrifies me, that's the word! Might it be the case that Mercury possesses a nearly polyhedral structure, regular and symmetric, like a cut diamond?" (Schiaparelli, 1909). However, it was Lowell's work on Venus that really set the astronomical establishment on its ear. After only two months at the telescope (see Figure 2), Lowell shock-

Figure 2: Percival Lowell observing Venus by daylight, 1897 (courtesy: Lowell Observatory).

ed the astronomical world by depicting markings on that planet and drawing inferences from them that were quite unlike anything seen before in over three centuries of observation.

In order to reduce the chromatic aberration in a telescope, it is helpful to stop down the aperture. In the case of Mars, Lowell frequently decreased the aperture to between 12 and 16 inches. For Venus, which because of its brilliance is greatly bothered by such effects, he adopted a much more extreme remedy, habitually stopping down to only 1.6- to 3-inches while employing his 140× 'comet-seeker' eyepiece. This resulted in comparatively high magnifications of 47× to 88× per inch of aperture and tiny

Figure 3: Lowell's secretary, Wrexie Louise Leonard, takes her turn at the eyepiece (courtesy: Lowell Observatory).

exit pupils only 0.29 to 0.54 millimeters in diameter. The ability of such narrow exit pupils to render visible defects in the lens of an observer's eye would be documented by Douglass in a breath-takingly insightful article, "Atmosphere, telescope, and observer", published in the June 1897 issue of *Popular Astronomy*:

> Perhaps the most harmful imperfection of the eye is the lack of homogeneity within the more dense transmitting media, either the lens or membranes, presumably the former. Under proper conditions the lens displays irregular circles and radial lines, the whole resembling a spider-web structure … These become visible when the pencil of light entering the eye is extremely minute and of the proper brilliancy, by the casting of their own shadows, as it were, on the retina and the absence of light from other parts of the pupil to drown them. (Douglass, 1897: 80).

Observing under these conditions, Lowell (1896) was able, beginning with his first look at Venus with the new refractor on 21 August, to glimpse "… many markings …" on the disk, though nothing of the polar caps which had been seen by the French astronomer Trouvelot and others. The following gives a flavor of the entries in the log book preserved in the Lowell Observatory Archives:

> August 24. 2 hrs 52 mins. Plenty of markings on disk but none sure.
>
> 2 hrs 59 mins. Wish to add to polar-cap remark … that Mars' polar caps under similar seeing would have been evident.
>
> 3 hrs 13 mins. There seem to be certain markings—one black spot in special situated thus.
>
> 5 hrs. Apparently many markings but illusive.
>
> August 30. 3 hrs 41 mins. Thought to see same spot that I saw on the 23rd together with many less dark markings.
>
> 4 hrs 10 mins. There is no doubt, I think, markings but they are too elusive to map.
>
> 4 hrs 40 mins. Spot again, the spot.

Next a band-like polar collar, then various spots and streaks emerged until, by late September and early October 1896, they had arranged themselves into a persistent pattern strikingly like the spokes of a wheel radiating from a central hub.

In addition to Lowell himself, his assistants also tried their hand at sketching the planet, and made out traces of what he was seeing, though only his loyal secretary, Miss Wrexie Leonard (Figure 3), drew the hub and spoke system with a boldness that approached that in his own drawings (Figure 4).

Given Lowell's strong authoritarian personality and expectation that assistants do what they were told to do and not have ideas and initiative of their own, combined with Wrexie's admiration and affection for him, one wonders about the role of suggestion—the influence of one observer's seeing on another's. It would have taken a very strong character indeed to resist that influence. Douglass (1898) touched on the matter:

> I had never closely studied Mr. Lowell's map—merely glanced at it casually, and although I knew it resembled the hub and spokes of a wheel, I did not know what position the centre held with respect to the phase … in order to give even a remote resemblance to Mr. Lowell's work. (Douglass, 1898: 383).

But this is hardly convincing.

The mental susceptibility that, in William James' words, makes us "… yield assent to outward suggestion, affirm what we strongly conceive, and act in accordance with what we are led to expect …" is probably common to us all (James, 1890(2): 598). In closed settings such as convents and hospitals it is known to have a tendency to produce, from time to time, episodes of mental contagion referred to as mass hysteria. In the famous hospital of the Salpêtrière in Paris, headed by Jean Charcot—a man described by one who knew him as the "… most authoritarian man I have ever known …"—it produced dramatic but, as we now know, fictitious convulsions in patients who were hypnotized, providing in the process striking evidence for Charcot's particular theories (Ellenberger, 1970: 92). Salpêtrière was also a closed system, one in which:

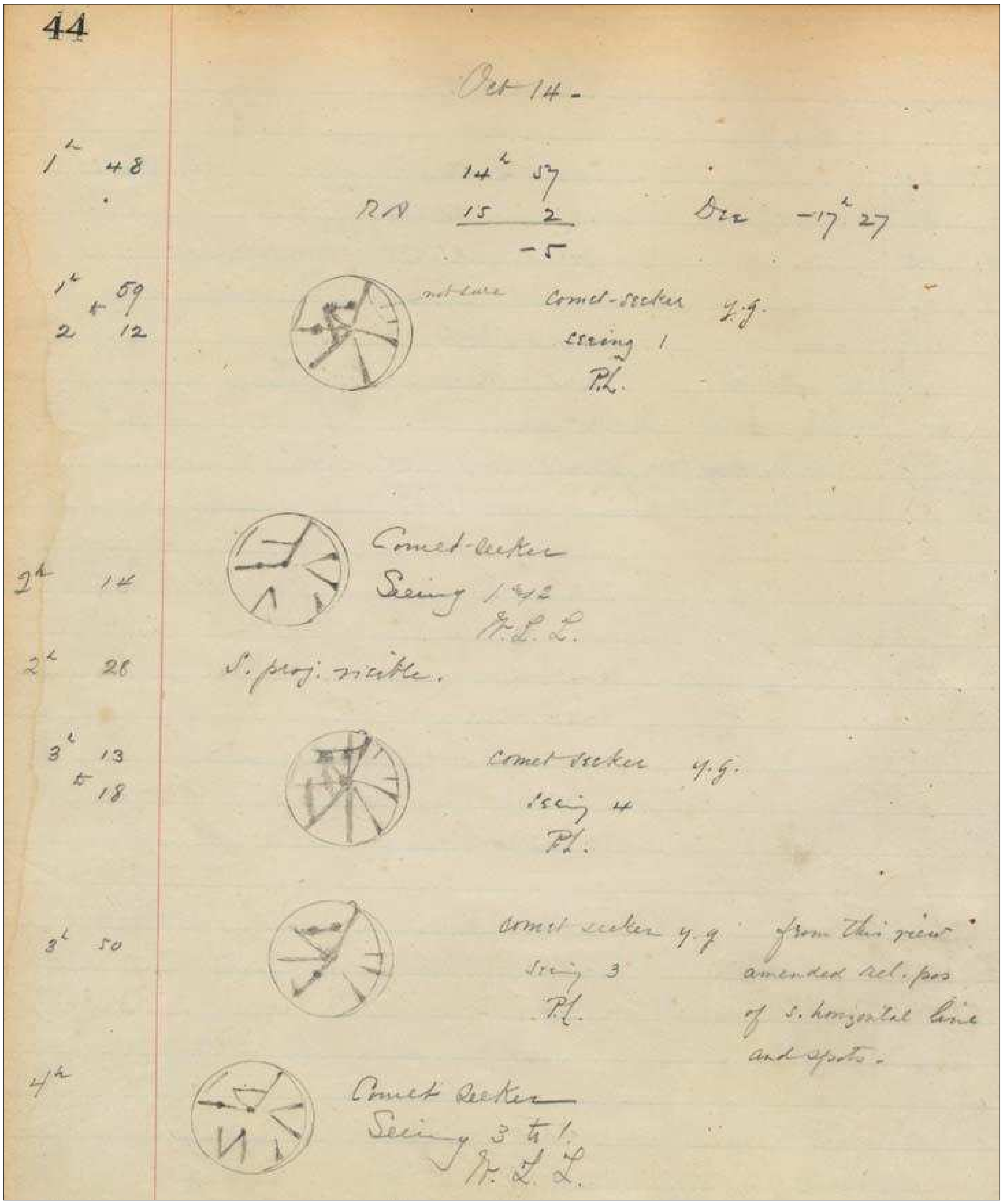

Figure 4: A page from Lowell's observing log book from 1896, showing the spoke-system on Venus. The first, third and fourth drawings are by Percival Lowell, and the second and fifth are by Miss Leonard (courtesy: Lowell Observatory).

> Because of Charcot's paternalistic attitude and his despotic treatment of students, his staff never dared contradict him; they therefore showed him what they believed he wanted to see. After rehearsing the demonstrations, they showed the subject to Charcot, who was careless enough to discuss the case in the patients' presence. A peculiar atmosphere of mutual suggestion developed between Charcot, his collaborators, and his patients, which would certainly be worthy of an accurate sociological analysis. (Ellenberger, ibid.)

Lowell himself had witnessed what was in some ways an analogous phenomenon himself, in the case of the 'fox disease' of Japan,

> … a species of acute mania supposed by the people to be a bewitchment by the fox. As the person possessed so regards it and others assist in keeping up the delusion by interpreting favorably to their own views, it is no wonder the superstition survives. (A.L. Lowell, 1935: 9).

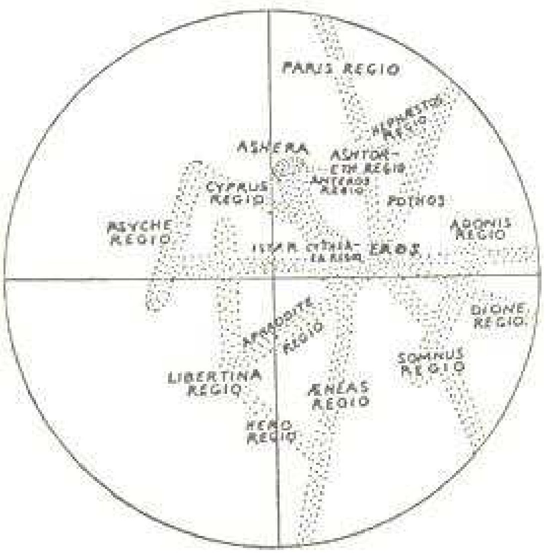

Figure 5: Lowell's map of Venus' surface markings, showing named features. He believed that the planet's rotation was equal to that of its period of revolution, and one side—this side— always faced toward the Sun, while the other was hidden in eternal night (after Moore, 1961: 91).

To what extent was there such a 'peculiar atmosphere of mutual suggestion' at Flagstaff during these years? To what extent were his assistants, consciously or unconsciously, accommodating themselves to Lowell's views, and suffering from an astronomical equivalent of fox disease?

2 LOWELL RUSHES IN WHERE ANGELS FEAR TO TREAD

As he had done with Mars in 1894–1895, Lowell blitzed the media with the results of his Venus observations, seeing through the press a series of articles, each largely rehashing the same results in similar terms, in the *Boston Evening Transcript*, *Atlantic Monthly*, *Popular Astronomy*, *Astronomische Nachrichten* and *Monthly Notices of the Royal Astronomical Society*. His drawings, clumsy and harsh, were accompanied by verbal descriptions that suggested that Lowell had seen on Venus markings quite unlike what had been reported by all previous observers of the planet:

> The markings proved to be surprisingly distinct; in the matter of contrast, as accentuated, in good seeing, as the markings on the Moon and owing to their character much easier to draw; in the matter of contour, perfectly defined throughout, their edges being well marked and their surfaces well differentiated in tone from one another, some being much darker than others. They are rather lines than spots … A large number of them, but by no means all, radiate like spokes from a certain centre. In spite of this curious system there is about them nothing of the artificiality observable in the lines of Mars. They have the look of being purely natural. (Lowell, 1897: 361).

The markings, he rather surprisingly added,

> … prove to be not only permanent but permanently visible. Nothing but our own air, and it must needs be bad at that, suffices to obliterate them. Unless the seeing is very poor, they are always seen and always seen in the same place. (ibid.).

So definite were they, indeed, that Lowell offered a map of the planet's surface, devising for the various features romantic names drawn from mythology—Eros, Adonis Regio, Aeneas Regio (Figure 5). It had been the first map purporting to show the planet's surface since that published by the papal astronomer Francesco Bianchini (1662–1729) in 1728, though in contrast to the earlier astronomer's, which featured large diffuse spots, Lowell's markings were stripy bands.

Lowell's deductions from the observations were at least as bizarre as the markings themselves. First, he concluded from the fact that the markings were stationary that the Schiaparellian rotation period was correct—the period was the same as that of the planet's revolution around the Sun, 224.7 days. Next, the fact that the markings were always visible showed that there could be no clouds on the planet. The planet's lustre, "… as if a bright veil of some sort were drawn over the disk …", was owing to the presence of an atmosphere, as had been established by previous students of the planet; it was evidently transparent, and allowed features of the surface to be seen through it. As to the nature of that surface,

> There appears to be no sign of water or of vegetation upon the planet. This is shown by the absence of color in any part of the disk. The disk is simply a design. In black

> and white over which is drawn a brilliant straw-color veil. Compared with the pronounced and beautiful tints of Mars, the white, blue-green and reddish yellow, Venus is a very drab-like thing.
>
> Such appearance is what the probably meteorologic conditions prevailing on the sun-lit side of the planet would lead us to infer a priori. Being exposed in perpetuity to the full blaze of the Venusian sun, it would seem that a funnel-like indraught of air from the dark side to the bright and then an umbrella-like return of it, would necessarily result and that the eventual outcome of this would be the depositing of almost all the water upon the unilluminated side where in the form of ice it would substantially remain. (ibid.).

Lowell's idea that the night side was covered in ice suggested to Godfrey Sykes the idea that this might explain the long mysterious 'Ashen Light', a glowing of the night side of Venus occasionally reported that made it seem like the 'New Moon in the Old Moon's Arms'. It might, Sykes suggested, be the effect of starlight dimly reflected off the icy surface. It was as good an explanation as any.

No sooner had he concluded these observations (and the dozen or so publications that came of them) than he decided to move his base to Mexico for Mars. So, at the end of November 1896, dome and telescope were disassembled, loaded on flatcars, and shipped to Mexico, where they were reassembled by local workmen under the supervision of Douglass, who had been sent on ahead, at Tacubaya, outside Mexico City and not far from the National Observatory. Though Lowell was irritable and impatient to have everything in readiness for the 1 December opposition, and was regularly telegraphing instructions to "… hurry things please …", installation took longer than expected—mainly because of difficulty adjusting the double track upon which the Sykes dome would revolve. Not until the end of December were these problems finally resolved, and Lowell appeared from Boston with the precious lens in hand and Mars already nearly a month past opposition. Though the observing campaign continued until the following March 1897, there was little new to report about Mars, though Lowell claimed that the conclusions from the 1894–1895 opposition at Flagstaff had been confirmed. In fact, better results were obtained by astronomers elsewhere.

If his Mars work was rather blasé, the Venus work caused a huge stir. As Lowell biographer William Graves Hoyt (1976: 110) put it:

> … while Lowell could claim that other observers had seen the canals on Mars, no other astronomer had, or indeed has, ever seen anything like the 'surprisingly distinct' features he described on Venus.

At an 1896 meeting of the Royal Astronomical Society of London, Captain William Noble (1828–1904) said of Lowell's chart: "I do not know whether Mr. Lowell has been looking at Mars until he has got Mars on the brain, and by some transference transcribed the markings to Venus."(Anonymous, 1896: 420). Lowell defended his work as he usually did, by claiming the vast superiority of the air in Flagstaff for planetary observations. Thus, he wrote,

> For skepticism, of course, I care nothing; people are naturally prone to disbelieve what they have not seen, and the distinctness of the markings in good air raises them above criticism. It is merely a question of time when they and their consequences shall be generally recognized. (Anonymous, 1897: 113).

The French popularizer of astronomy, Camille Flammarion (1842–1925), though broadly sympathetic to Lowell's work on Mars, noted that where Venus was concerned Lowell's observations were "… entirely at variance with all that has gone before." Edward Singleton Holden (1846–1914), then Director of the Lick Observatory and a long-time student of the planet, helpfully suggested that perhaps the spoke-like markings might be owing to a strain on the glass, induced by an overtight condition of the adjusting screws or of the objective lens in its cell—an idea that the lens-maker, Alvan Graham Clark, indignantly rejected. The most highly respected observer of the era, Edward Emerson Barnard (1828–1904; Sheehan, 1995), cared nothing for Lowell's results, and said:

> This rotation controversy [regarding Venus] has raged for upwards of two centuries, with fitful periods of quiescence—after some observer more combative than the rest had definitely "settled the question"—only to break out again with renewed virulence when a new champion for rotational honors entered the field…. [The] discrepancies are due in the main to the difficulty … of seeing the markings which really exist on the surface of Venus. (Barnard, 1897: 299).

One of Barnard's drawings is shown in Figure 6.

But perhaps the most unkindest cut of all was inflicted by Camille Flammarion's assistant at the latter's private observatory at Juvisy and a leading member of the British Astronomical Association, Eugène Michael Antoniadi (1870–1944; McKim, 1993), who seems to have regarded Lowell as little more than a wealthy crackpot. Antoniadi agreed with Flammarion's verdict, that what Lowell had published was "… at variance with all that had gone before …" and added sarcastically:

> It is to be hoped … [the] canals of Venus, though negatively advancing our scanty Aph-

> roditographical knowledge, will advance optical science in a positive manner, and enable us, in a near future, to have a clearer grasp of the canaliform illusion which so violently agitated of late the public mind. (Antoniadi, 1898: 474).

So stinging were Antoniadi's criticisms of Lowell that communication between the two men was effectively broken off for many years, and only resumed in 1909.

3 THE MACHINE BREAKS DOWN

On his return from Mexico in April 1897, Lowell suffered a mental breakdown, which was to largely sideline him from astronomical activities for the next four years. The official diagnosis was the then fashionable (but vague) 'neurasthenia', to which 'brain-workers' (as opposed to manual laborers) were supposed to be especially susceptible. Of course, it is no longer possible to be sure what the exact diagnosis was, though manic-depressive illness ran in the family, especially in the less wealthy Russell-Spence line (Jamison, 2017). The medical historian Janet Oppenheim, in her book *Shattered Nerves*, points out that among well-to-do young Victorian and Edwardian men, depression was frequently interpreted as owing to potential conflict between the son and parents, particularly the father (Oppenheim, 1991: 167). Lowell's own ambivalence toward his father is well documented. On the other hand, he was always excessively devoted to his mother (and she to him). Percival's sister Katharine said that Lowell was

Figure 6: The usual view of Venus, by Edward Emerson Barnard in 1889, using the 12-inch refractor of the Lick Observatory (after Barnard, 1897: Plate 19).

> … peculiarly fond of his mother … [and] his tender solicitude for her was unfailing … we used to say that he could do anything with her and that she spoiled him. (Strauss, 2001: 14).

She had died, after a long illness, two years previously (in April 1895), and one can hardly overestimate the blow—or the possibly significance of the second anniversary of her death, which occurred at about the time he began to sink.

Lowell was a highly egotistical and self-centered individual. As is typical for extreme narcissists, he was exquisitely sensitive to criticism, and Lowell's most psychologically informed biographer David Strauss infers from the timing of the breakdown that criticism of his Venus work likely played an important and perhaps even the main role in precipitating, in the psyche of someone already vulnerable, one of those "… instabilities of mood and nerve …" that tended to complicate Lowell's life at important junctures (Strauss, 2001: 21–23). This seems right, especially as Lowell never forgot or got over this criticism. Admittedly, that criticism was unusually harsh even by late-Victorian standards, and may have led Lowell, as Strauss suggests (2001: 173), to entertain private "… doubts about the validity of his own observations and his future prospects in astronomy."

Whatever the underlying causes of Lowell's emotional collapse, a long and uncertain recovery followed. Indeed, the next four years were marked by periods of anxiety, melancholy, sleeplessness, and mental torpitude. A working-class patient suffering from such symptoms would have been quickly relegated to a public asylum, but someone with Lowell's resources was able to command the social position, domestic arrangements, and economic resources needed for recovery, and when he did return to full activity, to salvage from this potential wreckage a good measure of success. Lowell spent the summer of 1897 in seclusion, apparently in a state of complete exhaustion, at his Beacon Hill house, then moved in for several months with his father in the Sevenels mansion on Heath Street, under a physician's strict orders of 'absolute rest'; neither visitors nor work were allowed, a tactic which Lowell would come to believe only compounded his misery and prolonged his illness. Like many depressives, he next tried the opposite: giving up on rest, he forced himself to be in constant motion, and in the company of his doctor, embarked upon a regimen of travel, exercise, and sunshine, which included forays to Bermuda, the Southern United States, Maine, New York, England, the French Riviera, and even Algeria again, this time traveling with Mabel and David Peck Todd on an expedition to observe an eclipse of the Sun in 1900.

In Lowell's absence, the Observatory continued its existence through the efforts of at least one—and sometimes several—staff astronomers. Douglass was mainly left in charge of the astronomical research, though during a brief period when he himself fell ill, the direction of the Observatory was taken up by Dr T.J.J. See (1866–1962), who however so alienated the rest of the staff, and Douglass in particular (who used such terms as 'vermin' and 'reptile' to refer to him), that he was soon sent packing by Lowell's brother-in-law, William Lowell Putnam II, who was left in charge of managing the business side of things in Lowell's absence.

During this fraught time, Douglass did his level best to defend Lowell's work from criticism. Thus, in 1898, he wrote of the markings on Venus:

> No matter how difficult to obtain, a just hearing is our right. No one is entitled to cry out against us until he can show that his atmosphere is approximately as good as the one through which Mr. Lowell discovered these markings. Let our dubious friends … devote a portion of their valuable time to work at the telescope under better atmospheric conditions, and no one will misinterpret the silence which will follow (Douglass, 1898: 320).

Not long after Douglass wrote this an eminent astronomer did indeed attempt to do as he suggested. This was none other than Edward Emerson Barnard, who came to Flagstaff to see for himself. He arrived on 30 May 1898, and remained until 4 June. Though this was the season at which See had predicted the best seeing, the results were disappointing in the extreme. Typical of the entries in Barnard's observing notes (Barnard, 1898) are the following:

> May 31 … "The jumping [of the image was constant so that nothing was seen." June 2: "Image jumping excessively. High S.W. wind. The seeing continued very variable, running from a blurred mass to a fairly well-defined limb, but the image was jumping badly." June 3: "The image was quite steady; I saw no markings. Mr. [G.A.] Waterbury, however, saw the usual Flagstaff markings." June 4: "Fearful high N.E. wind … Mr. Waterbury … saw the usual Flagstaff markings easily—I could not see them."

In addition to seeing virtually nothing on Venus, Barnard—a veteran of severe intrapersonal strife at Lick Observatory during his seven years as a staff astronomer there, from which he had recently fled for the apparently serener haven of Yerkes Observatory—witnessed evidence of similar intrapersonal conflict and dysfunction on Mars Hill. Morale seemed to be very poor, in large part because of the presence of the detestable See.

Douglass, whose comments defending Lowell's Venus observations had clearly reflected pre-rehearsed views, confided to his distinguished visitor his growing skepticism about the Lowellian markings on the planets, which had been growing since at least 1894–1895, when he expressed doubt about the existence of 'double canals' on Mars, one of Schiaparelli's most startling findings and one which had come to acquire, for admirers of the Milan astronomer, something of the status of an article of faith. Douglass noted that, while the double canals were infrequently visible in steady air, they quite commonly presented during periods of poor seeing. By the time of Barnard's visit, Douglass had gone so far as to begin a series of experiments with artificial planet disks which led him to suspect that such markings were entirely illusory, and this was above all true of the hub-and-spoke system of Venus. This was sure to rile his employer.

The dismal results during Barnard's visit can hardly have shored up Douglass' confidence, and subsequently he redoubled his efforts with the artificial planet experiments. They continued for another year, when Lowell, who had at first been indulgent (or perhaps only distracted), abruptly ordered him to break off, since they "… cast doubt on some observatory publications." A year later Lowell was inclined to relent, and even instructed Douglass to see if there was any tendency for lines drawn on an artificial planet to appear as double. But by then, Douglass was venturing into dangerous territory. Most of the surface detail recorded in his artificial-planet experiments was, he found, completely fictitious, leading him to suspect that much of the detail so prominent in the Observatory's renderings of actual planets was fictitious as well. He was still working this angle at the beginning of 1901, when he confidentially queried a psychologist specializing in optical illusions, Joseph Jastrow (1863–1944) at the University of Wisconsin in Madison, on these matters.

By then, Lowell had recovered sufficiently to return to the observatory and resume its direction. He had had Venus on his mind during much of the four years of his slow convalescence, and was planning a counterattack against his critics. He appreciated the challenge of Douglass' artificial planet investigation. Indeed, one of the first things he did on his return to Flagstaff was attempt to sketch one of Douglass' artificial planets through the telescope. He drew a double line in the position of a shaded area!

Douglass had enjoyed four years of independence from Lowell's authoritarian personality, and had begun to develop his own ideas. He fretted about Lowell's return, complaining, privately, to W.H. Pickering (1858–1938), whose assistant he had been at Arequipa in Peru for the 1892 Mars opposition, about Lowell's "… strong personality, consisting chiefly of immensely strong convictions." While allowing Lowell a portion of literary talent, he found him completely lacking in 'scientific instinct' (Douglass, 1901). Pickering shared Douglass' views in the matter, and assured him that Lowell was generally held in low regard by the astronomical profession. Nevertheless, he advised his former assistant, on practical grounds given the scarcity of paying positions in astronomy, to stay put, "… unless the loneliness is too oppressive." (Pickering, 1901). Whether Douglass could have managed to work under these conditions is doubtful, but in any case, he never had the chance to find out. On the same day he wrote to Pickering, he had also written to the Observatory's trustee during Lowell's absence, his brother-in-law William Lowell Putnam II (1861–1923):

> Lowell devotes his energy to hunting up a few facts in support of some speculation. I fear it will not be possible to turn him into a scientific man (Douglass, 1901).

Figure 7: V.M. Slipher, 1875–1969 (courtesy: Lowell Observatory).

Though the letter was meant to be "… between ourselves only …" Putnam eventually showed it to Lowell, who reacted as might be expected. Douglass was dismissed in early July 1901.

It would later be said that Lowell's decision to fire Douglass was the best thing that ever happened to astronomy in Arizona, since Douglass went on to a stellar career which included founding the Steward Observatory of the University of Arizona in Tucson. It is interesting, by the way, that Douglass' drawings of Mars while under Lowell's thumb include a parade of canals, up to the point of his anti-canal epiphany, but thereafter they were conspicuously absent. He had emerged from his heretofore hypnotic trance and liberated himself from the Lowellian spell.

4 VENUS IN 1903

For Lowell, the battle continued—at first single-handedly. Alone in the redoubt of his Observatory, he resolved to take no more Assistants. "I am so much at home here … and yet no one I know knows it …" he confided to his sister Elizabeth (Lowell, 1901a). Having previously lived in town in a house just north of the inter-section of Aspen and Beaver, he confirmed his intention to stay permanently on Mars Hill by expanding the observer's quarters, where Douglass and the other astronomers had been living, into the 'Baronial Mansion', which eventually developed into a rather random and haphazard 18-room residence before it was more or less abandoned—except when his widow was in town—after his death. He returned with a will to observing Mars at its mediocre February 1901 opposition, while cogitating a response to the Venus criticisms.

The latter had been made urgent, even before he returned to Flagstaff, by the April 1900 announcement by the Russian astronomer A.A. Belopolsky (1854–1934), at the Imperial Observatory of Pulkova, of spectrographic observations suggesting a short rotation period. This was, of course, completely inconsistent with the isosynchronous rotation period Lowell had deduced from the spoke observations, and Lowell, deciding to fight fire with fire, made one of the most consequential decisions of his career, ordering in late 1900 his own spectrograph from Pittsburgh optician John Brashear.

It was not ready for delivery until September 1901. By then, Lowell had a new Assistant to replace the departed Douglass, Vesto Melvin Slipher (Figure 7; Hoyt, 1980), a graduate of Indiana University. Lowell had previously promised his erstwhile Assistant Wilbur A. Cogshall, now a Professor at Indiana University, to hire Slipher to operate the spectrograph. Still smarting from Douglass' departure, Lowell initially rebuffed Cogshall, telling him "I have at present all the assistants I need." In fact, however, it is as likely that Lowell would have undertaken to operate the spectrograph on his own as that he would have swept the floors or done the dishes. Cogshall's persistence overcame his hesitation, and Lowell, rather peevishly, agreed to take Slipher on, "… only because I promised to do so; and for the term offered. What it was escapes my memory." (Lowell, 1901b). The following year, Lowell added a second Assistant, another Indiana University graduate, Carl Otto Lampland. Both proved to be remarkably loyal and durable. Slipher remained for 53 years, until his retirement in 1954; Lampland for 49 years, until his death in 1951.

Possessed of the skill in tinkering and making do with whatever happens to be at hand of many farm hands, Slipher quickly learnt how to manage the spectrograph, and as a first trial succeeded in obtaining spectrograms of Jupiter showing its rotation. He did not, however, get to Venus until 1903, and then was a little late, as Lowell a year earlier already published a retraction of his Venus observations—suspecting they might be illusory after all—in the German journal *Astronomische Nachrichten* (Lowell, 1903b).

His retraction was, however, soon retracted. The following March, Slipher had obtained encouraging spectrographic results, about which Lowell said, "… without any bias from me and with every precaution of his own to prevent unconscious bias on his part and to eliminate systematic errors …" the findings were "… completely confirmatory of Schiaparelli's period and of my visual work here in 1896-97." (Lowell, 1903a). Giving his own first-hand account in a *Lowell Observatory Bulletin*, Slipher (1903: 18) more cautiously declared that he had found no evidence of a short rotation: "Rotation in twenty-four hours would incline the planetary lines about one-third of a degree, a quantity too large to have escaped detection." This is not quite the same as Lowell had claimed. At any rate Lowell was gratified that the 'hard nut' of Belopolsky's short rotation period had been cracked.

Lowell made visual observations with the Clark from February into July that year, taking, as he said, every precaution to rule out illusions:

> Nothing was set down without a caveat until I had assured myself of the certainty of its non-subjective existence. Two points I examined specifically: one the objective assurance of the psychical perception; the other the space-prolongation of an impresssion by movement of the eye. With regard to the first point, experiments on the visibility of a wire show that it is possible by direct consciousness to part the true from the spurious. Although it is possible to see illusory lines, it is also possible to become cognizant of the fact. If one pays attention, an hallucination of the sort may be found to differ from a presentation of fact by the absence of the sense of reality … With regard to the second kind of illusion, the transference of a perception from one point to another by motion of the eye, experiments have led me to believe in the possibility of its production at times near the limit of vision. Whether the transference is due to continuity of impresssion, for the eye retains an impression for the twentieth of a second, and might thus conceivably superpose a first image upon a second part of the field, or whether it be an ideo-sympathetic effect, I am not aware, nor is it for purpose vital to inquire. That it may be produced, and that it may also be precluded by holding the eye still is sufficient. This plan I adopted. It is not so easy one might imagine; for bent on detection, the eye has a roving drift hard to hold in check (Lowell, 1904: 184–185).

Both of his points are questionable, but as to the second, specifically, holding the eye still, given its tendency to have a roving drift, is easier said than done, and despite what Lowell claimed, it is unlikely that he succeeded as far as he thought.

Indeed, it is probable that Lowell was doing nothing more than observing shadows cast onto the retina (see Figure 8), through the process Douglass had intimated in the 1897 paper, "Atmosphere, Telescope and Observer." Moreover, by drastically stopping down the aperture of his telescope (to 1.6- to 3 inches), he had, as Tom Dobbins and I argued a number of years ago, turned it into a giant ophthalmoscope, so that as likely as not he was merely observing an angioscotoma, i.e., the shadow of his own retinal blood vessels, projected onto Venus and perceiv-

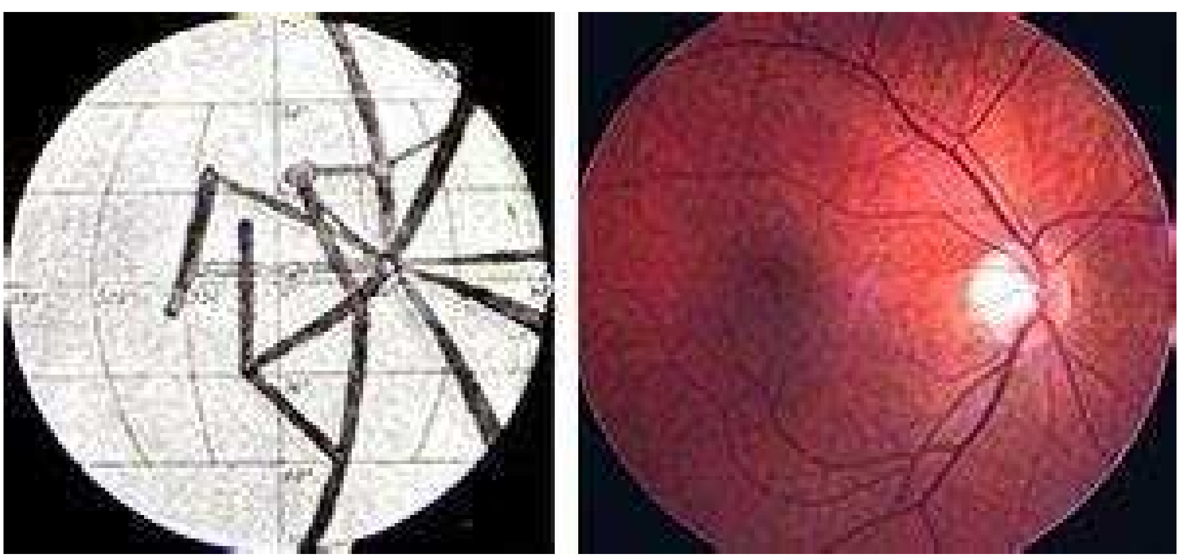

Figure 8 (left): Lowell's spoke system on Venus, and (right) a photograph of the human retina, showing blood vessels (photograph: William Sheehan).

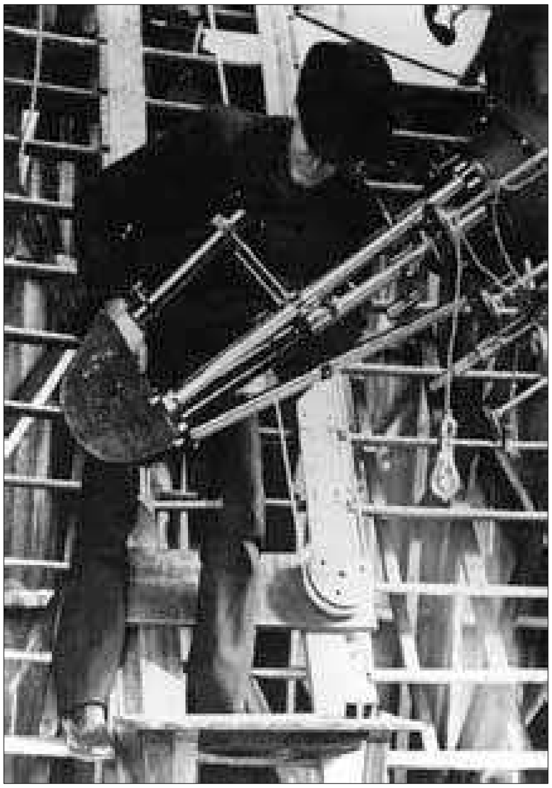

Figure 9: V.M. Slipher operating the Brashear spectrograph (courtesy: Lowell Observatory).

ed as the peculiar hub-and-spoke pattern (Sheehan and Dobbins, 2003).

Be this as it may, there can be no doubt that, as Strauss says, "… the skepticism about the Venus markings clearly changed the direction of research at Lowell." (Strauss, 2001: 220). Nothing did more damage to Lowell's reputation than his Venus observations, and one of the lessons his staff learned was extreme caution. They were reluctant to publish anything unless they were absolutely sure of their work.

Figure 10: Young Carl Otto Lampland, 1873–1951 (courtesy: Lowell Observatory).

As soon as Slipher had finished with Venus, he moved on to other Lowell priorities. One was, of course, Mars, where he was at pains to try to detect bands of water-vapor in the far-red part of the spectrum in service to Lowell's Mars theories, and the Giant Planets, where his spectra revealed strong spectral bands that neither he nor anyone else could immediately identify. Most significantly, by 1913–1914 Lowell pointed him to the technically difficult feat of obtaining spectrograms of the spiral nebulae (Figure 9), in the hope of obtaining confirmation of Lowell's hunch that they were planetary systems in formation along Laplacian lines. Famously, Slipher instead discovered the very high velocities that would prove them to be extragalactic and provide an underpinning of the theory of the expansion of the Universe (see Bremond, 2009). His first announcement of velocity shifts of multiple galaxies was presented at the August 1914 AAS meeting, and lists twelve redshifts and three blueshifts, the latter being of NGC221 (M32), NGC 224 (M31) and NGC 598 (M33), all belonging as we now know to the 'Local Group' (V.M. Slipher, 1915). This was the most important discovery ever made at Lowell Observatory, and earned Slipher the respect of the astronomical community that had proved elusive when he had been identified with Lowell's initiatives.

5 OBSERVING AND PHOTOGRAPHING MARS

Lampland is less well known than Slipher, as he made no discoveries like that of the redshifts of the nebulae. However, he had a long and productive career, and his contributions are only now being fully appreciated. He had been born, the third of 11 children, of Norwegian parents in a log cabin near Hayfield, Minnesota, on 29 December 1873, grew up on the farm, attended country schools, clerked in the local store, played clarinet in the village band and received his Bachelor of Science from Valparaiso Normal School in Indiana (Figure 10). Desirous of further education, he went next to Indiana University and took up the study of astronomy. After graduating in 1902, he accepted a job as principal of the High School in Bloomfield, Indiana, but had hardly learned his way about town when, on the recommendation of J.A. Miller, W.A. Cogshall, and V.M. Slipher—the latter obviously well beyond the probationary stage of his employment—he received from Percival Lowell the offer of a position at Lowell Observatory. Needless to say, he too accepted, and would remain at the Observatory until his sudden death in 1951.

The noted Flagstaff historian Platt Cline (1994: 179) described Lampland as

> … tall, lean, conservative in dress and manner, quiet, reserved and courteous, and walked rapidly, never strolling. He wore a constant expression of preoccupation. His associates appreciated his dry humor and stock of literary quotations. Always a student, he carefully collected and organized data on many subjects …

including on his colleagues, whose daily activities, in addition to his own, were kept track of, not always without critical comment, in the daily diary he kept from 1902 to 1951. This in itself tells us much about the man. He was of a type not uncommon in scientific research—an obsessive-compulsive, who was a perfectionist in everything he did (Duncan, 1952).

With Slipher busy mastering the spectrograph, Lampland received a different assignment, being charged with designing a planetary camera for use on the 24-inch Clark. Frustrated with criticisms about the subjective nature of planetary detail, Lowell wanted to buttress his arguments with the incontestable public testimony and apparently unobjectionable objectivity of photographs. As Slipher had done with the spectrograph, Lampland made rapid progress, and by 1903 used his planetary camera to obtain photographs of Mars that were not only superior but far superior to any obtained previously. Two years later, he achieved a real *tour-de-force*, succeeding, apparently, in recording some of the canals, something which even Lowell would not have regarded as possible. He received for this achievement a medal from the Royal Photographic Society and an unexpected MA from Indiana University.

Also in 1905, Lampland became involved in another of Percival Lowell's projects, taking charge of the first photographic search for Lowell's postulated trans-Neptunian "Planet X." At first he rigged up a camera from equipment to hand to be used on the 24-inch refractor, and exposed a few plates before the enormity of the task—and the unsuitability of the extremely narrow field provided by the 24-inch telescope—led to its abandonment. The search was subsequently taken up, though no more successfully, with other instruments, with the grind of taking the plates falling to three Lawrence fellows from Indiana University, John C. Duncan, who spent 1905–1906 at Lowell, Earl C. Slipher and V.M.'s younger brother, in 1906–1907, and Kenneth P. Williams, 1907. Of these, only E.C. Slipher (1883–1964), who distinguished himself as a photographer of Mars as a member of the Lowell Observatory Expedition to Alianza, Chile, during the 1907 opposition, would remain.

Meanwhile, in 1909–1910, Lowell installed a 40-inch reflector—mounted underground in the hope that this would provide stable seeing. It has been said that Lowell made this investment principally for Lampland's use (Putnam, 1994: 101). Be this as it may, the greater part of Lampland's subsequent observing time would be devoted to this instrument. For the most rapid work, it required the cooperation of three men, but Lampland often used it alone. In addition to the 'transneptunian' fields, his thousands of direct photographs would include comets, the planets and their satellites, and the principal nebulae of the NGC. He discovered several variable stars and novae; the remarkable nebular appendage of R Aquarii; and changes in the Crab Nebula and Hubble's variable nebula. Most of these photographs were never published, and as late as 1948, at the Zurich meeting of the International Astronomical Union, the Commission on Extragalactic Nebulae adopted a resolution pointing out the desirability of having them published. Characteristically, the resolution had still not been acted upon by the time of Lampland's death. The most innovative work done by Lampland with this telescope involved radiometry of the planets during the 1920s, which will be described below.

It is notable that after E.C. joined the staff, the three men who were to become senior staff astronomers—V.M., Lampland, and E.C.—each established the specialization that he was to continue to claim for the rest of his career. Lowell astronomer Henry Giclas, who became a member of the staff in the early 1930s, would later recount:

> After a few years at Lowell, it became very apparent that each of the older men had their field defined, and became extremely covetous of it and would brook no interference from each other or especially any outsider. It must be obvious to even a casual observer that there was never a sharing of observing facilities between the older men. There never was a spectrograph built for the 42-inch reflector despite its superior light gathering power compared to the 24-inch refractor, even though Lowell, while he was alive, had V.M. put the Brashear spectrograph on the 42-inch for trials. No one ever used the 42-inch except Lampland … Lampland kept his nose out of anything spectrographic, and even though he designed the planetary cameras and received the medal from the Royal Photographic Society of England for his efforts, he had nothing to do with planetary photography after E.C. Slipher came in 1907. In other words, each staff member had his own narrow field and dared not stray into another's domain. There was much ill feeling among the three older men; but let an outsider come in and try to do or suggest something and the three operated like one against any invasion. (Giclas, 1991).

In addition, neither V.M. nor Lampland showed any aptitude as visual observers of Mars, and their drawings hardly resemble Percival's at all, showing only a few diffuse bands and streaks. It was in E.C. that Lowell found for Mars what Miss Leonard had been for Venus. E.C.'s drawings from 1907 onwards are almost indistinguishable from Lowell's. This, of course, raises the interesting question, well-known to art historians (Gombrich, 1961) of the extent to which observers imitate others' drawings as much as represent the view in the eyepiece. The process is a little like the striving to work out the method of solving a math problem when knowing the answer given in the back of the book: Lowell imitated his hero Schiaparelli, as E.C. in turn imitated Lowell who was both his employer and an extraordinarily strong and controlling personality. Thus,

> Once a definite expectation is established, it is inevitable that one will see something of what one expects; this reinforces and refines one's expectations in a continuing process until finally one is seeing an exact and detailed—but ultimately fictitious—picture." (Sheehan, 1988: 85).

In each case, one sees something of that yielding of the will to another that one sees in subjects under hypnosis, or surrender of the acolyte to the dogmas of a religious faith.

6 THE DIRECTOR AND HIS UNDERLINGS

The search for Planet X, begun in 1905, and later based on exhaustive calculations in which Lowell attempted, with the assistance of several human computers as they were known in those days, to calculate its position from perturbation theory as a means of directing a telescope to the right part of the sky, absorbed an enormous amount of Lowell's time and effort during the last decade of his life, as well as that of his staff.

Always impatient of results, high strung, and prone to overwork, Lowell became increasingly so as the years went by and disappointments mounted. The Planet X search, despite all the effort he and his computers had expended, failed to turn up a planet, and was, according to his brother, "… the greatest disappointment of his life." (Sheehan, 2016: 226). His failure to win over his professional peers to his Mars theories was also the source of much frustration and unhappiness. Though his staff, V.M. Slipher, Lampland, and E.C. Slipher, were completely loyal, and did not hint any tendency to go the Douglass route into apostasy—then or later—they were well aware of the fact that they were his employees, technicians charged with providing data in support of his projects rather than in pursuit of their own initiatives. For the most part they managed to stay on his sunny side.

At a lower level of the social scale were his servants, and it was their lot to suffer more directly from his violent outbursts of narcissistic rage. Coming from an aristocratic background in which servants were expected to "… minister … with unconscious service all the time, and with no … arrogant independence" (Gould, 112/1996-1/1997:22) Lowell tended to notice them only when they fell short. According to Platt Cline,

> He had always been demanding and impatient with servants and assistants, [in later years he] became noticeably more so." (Cline, 1994: 179).

In 1913–1914, after suffering yet another breakdown, he seems to have been especially demanding and impatient. Two assistants were hired that year to work in the 24-inch dome, a Mr Hanway who was assigned to help E.C. with the photographic end of the work, and W.H. Spaulding, who assisted V.M. with the spectrograph. At first things went well (apart from some differences Spaulding had with the younger Slipher,

> … owing to his vain attempts to prove himself possessed of good sense when it was patent that he is 'not all there' … [but] with the winter came Lowell …

and things rapidly deteriorated. According to a letter Spaulding (1914) wrote to Douglass requesting employment in Tucson,

> With Lowell came abuse, brutality, arrogance, conceit and insolence. It was my lot to work in the dome with him, and the night I was with him he knocked a light out of my hand, jerked a box out of my hand without asking me for it, pushed me backward across a pile of boxes and stepped over me before I could rise. Every order that he gave could have been heard a half a mile away. The next morning I told Mr. Lampland that I would quit if there [were] any more such tantrums. He communicated my ultimatum to Lowell and for a couple of nights things went somewhat easier, only to begin again worse than before. He would rush out when I was setting the telescope, and grabbing hold of the end, swing it here and there, while I was vainly trying to get the object into the field. At length I deliberately walked out of the dome … and went down and demanded my time from Mr. Slipher—I mean V.M. Slipher, E.C. being a mere nonentity. V.M. prevailed upon me to stay by agreeing to put Mr. Hanway in the dome. The first night that they were working together Lowell became enraged over Mr. Hanway's apparent slowness in starting the dome; so he seized the switch rope and gave it a terrific pull, locking the switch so tight that it was impossible to reverse it and stop the dome. Mr. Hanway was unacquainted with the expedient of running over and throwing out the big main switch. So they—he and Lowell—started getting things out of the way

> of the ladder which was coming at them like a juggernaut. Most of the tables and chairs they dumped into the pit, but the big astronomical chair was the main problem. They could by great exertion keep it out of the way of the dome. Round and round they went until finally, when they were pretty nearly played out, Mr. Lampland came in. They had been working about twenty minutes. Mr. Lampland thought they were trying to follow a meteor.

Needless to say, neither Hanway nor Spaulding remained long at the Observatory after that, and when they left, both had difficulty collecting the money that was owed them.

In 1914 Lowell also turned to an old favorite, Venus, and made some daytime observations of the planet (Figure 11). It is significant that at this time his views were still the same as enunciated in 1896–1897.

In January 1915 Lowell presented his "Memoir on a Trans-Neptunian Planet" to the American Academy of Arts and Sciences in Boston. It was later published at his own expense. By then, he had clearly given up on 'Planet X', and his views about Venus and Mars had by this time completely hardened into dogma and would never be changed. Because the professional astronomers, whom he regarded as 'troglodytes' and accused of 'old-fogeyism' for refusing to embrace a promising new idea, had held out against him, he decided to turn his efforts toward younger minds, which were still capable of taking on new impressions, and set out in September 1916 on an exhausting lecture tour of the Pacific Northwest and West Coast, in which he visited the State College of Washington in Pullman, the University of Washington in Seattle, Reed College in Portland, the Agricultural College in Corvallis, the University of Oregon in Eugene, and Stanford University in Palo Alto and the University of California in Berkeley. Each venue was overflowing, and the audiences enthusiastically heard him speak on "Mars-Forecasts and Fulfillments," "Mars and the Earth," "Great Discoveries and Their Reception," and "The Far Horizon of Science." On returning from the West Coast on 19 October he immediately threw himself into another round of nightly telescopic observations, observing Amalthea and the satellites of Jupiter in pursuit of his latest idea, that the interiors of the planets had a

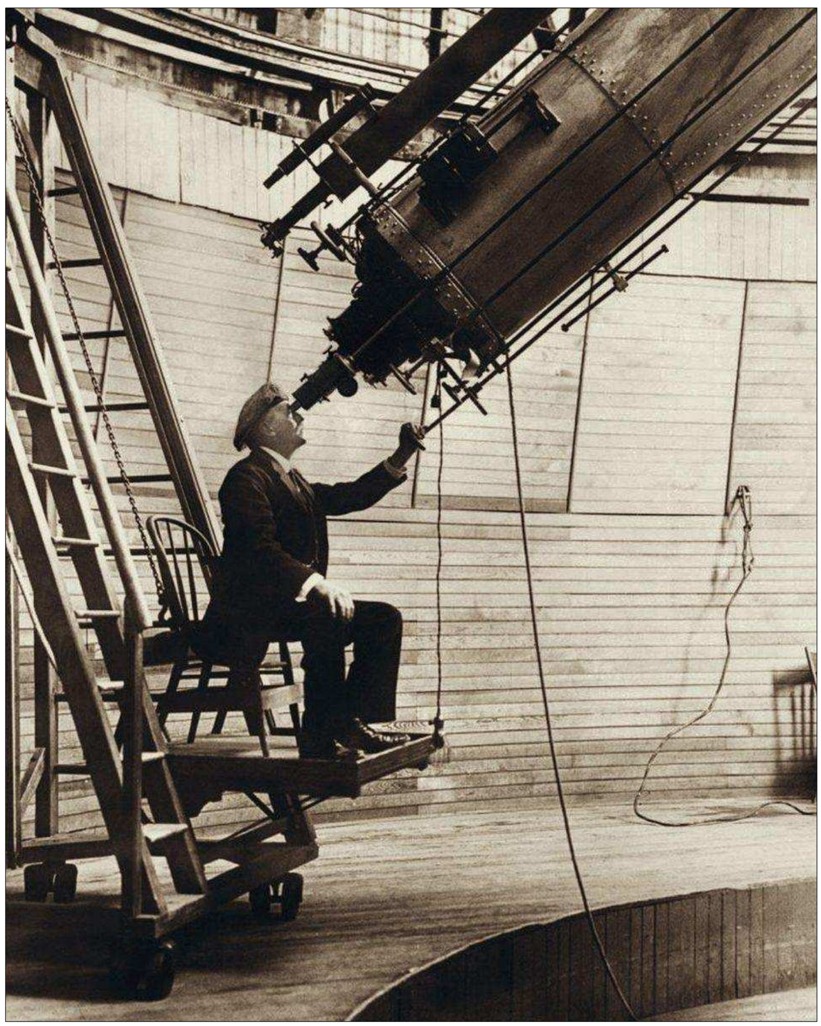

Figure 11: A photograph by Phillip Fox of Percival Lowell observing Venus by daylight on 17 October 1914 (courtesy: Lowell Observatory).

layered structure, like that of onions "… in partitive motion." Then in early November he ventured with Lampland on a field trip to Walnut Canyon, where he caught a cold, and was still nursing it on the night of 11 November, when he observed as usual with E.C. The following morning he was in a bad mood, and had a serious altercation with one of the drivers or some other household employee; soon afterward he was in his bedroom in the Baronial Mansion (Figure 12) and suffered a massive stroke (Cline, 1994: 174). Without ever regaining consciousness, he passed away just before 10 o'clock that evening.

Lowell's unexpected death left the staff rather disoriented. Without him, the Observatory resembled Hamlet without the prince. Lowell had always teemed with ideas and, at least between his periodic breakdowns, had the energy and will to drive them forward. His staff had been somewhat satellitic to his central orb, generally serving as technicians assigned to specific tasks with the spectrograph or camera in pursuit of data to support Lowell's initiatives. Lowell was a somewhat dictatorial director, and had not, as a general rule, encouraged a great deal of independence in his staff (remember Douglass)!

The one individual who was beginning to emerge somewhat from Lowell's shadow was V.M. Slipher, who had managed the highly difficult technical feat of obtaining spectrograms, first of the Andromeda Nebula, then others, which had led to the discovery of the very large velocity shifts that showed them to be extragalactic systems. V.M. received credit from astronomers elsewhere—indeed his presentation at the Seventeenth Meeting of the AAS in Evanston, Illinois in August 1914 was met with a standing ovation, something almost unprecedented at the time. But he published in rather obscure places (Marcus, 2013). Rather than in the *Astrophysical Journal*, which would have been the logical place for it to appear (but from which Lowell had been banished almost from the beginning), Slipher's Andromeda Nebula results were published in a *Lowell Observatory Bulletin* and in the lightweight *Popular Astronomy* (Slipher, 1915). According to Joseph N. Marcus (2013: 187),

> An audit of the Lowell Observatory archives and Astrophysical Data Service (ADS) documents that the publication records of Slipher and Lampland, especially after Lowell's death, were fairly sparse. Much of what they did publish was confined internally to the *Lowell Observatory Bulletin*, an arguably insular and suboptimal route for obtaining wider recognition for their work …

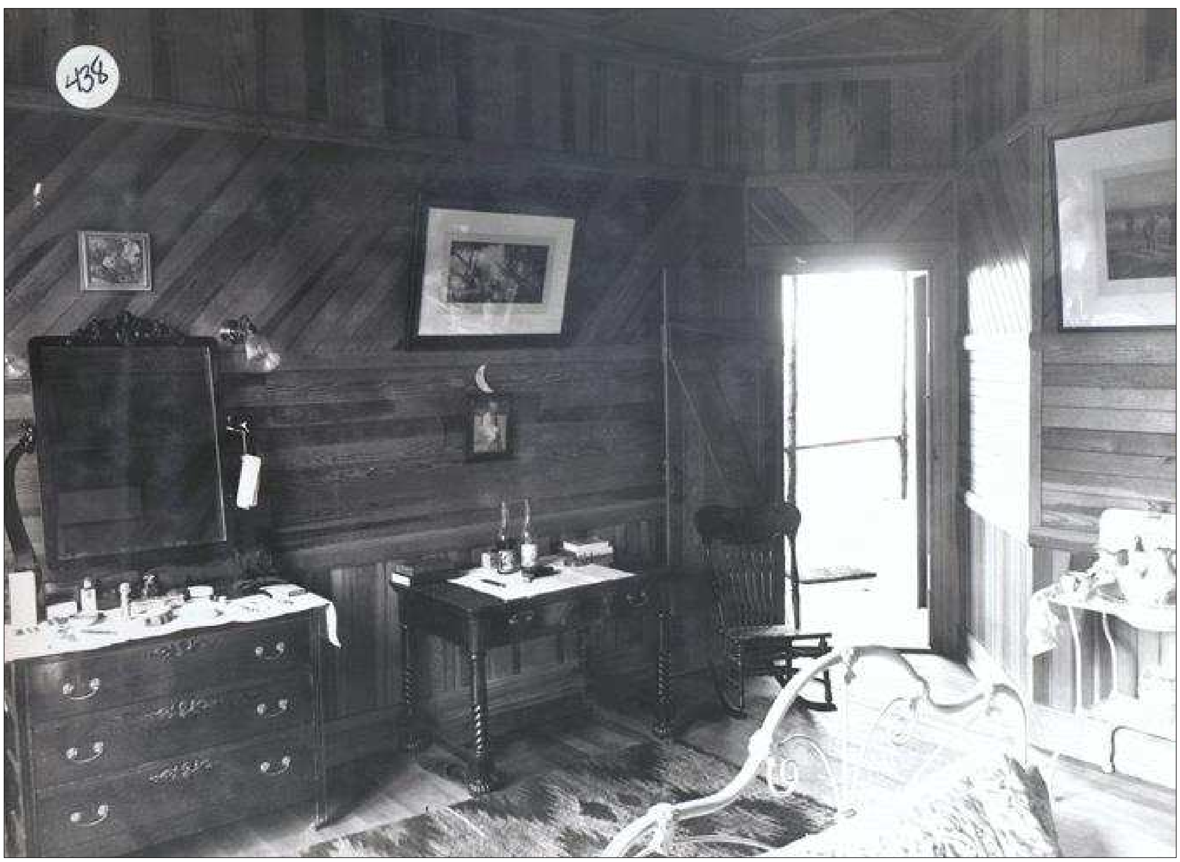

Figure 12: Lowell's bedroom in the Baronial Mansion on Mars Hill. This is the room in which he died, on the 'green couch' (which is not shown here), on 12 November 1916 (courtesy: Lowell Observatory).

But at least in this case Lowell knew a good thing when he saw it, and supported Slipher, who had hitherto been regarded as little more than 'Lowell's Man Friday' (in Lowell arch-critic W.W. Campbell's words), in encouraging the continuation of this important work and in building up his reputation by attending astronomical meetings and meeting with other astronomers.

As Slipher began to emerge more in his own behalf in the years before Lowell's death, he did so against what had become a rather concerted effort among the American astronomical establishment to isolate the Lowell Observatory and turn Lowell into a kind of pariah among astronomers. Lowell biographer David Strauss has argued:

> Lowell's apparent success in photographing the canals, providing evidence of water vapor on Mars, and attracting the support of prestigious intellectuals like Lester Frank Ward and Edward S. Morse galvanized the American astronomical establishment to launch a counterattack to refute Lowell's claims for the canals. Leading mainstream astronomers used their positions as power brokers within the discipline to put to rest what they regarded as a serious threat to the public's faith in science … Their collaboration took the form of experiments and expeditions designed to discredit claims that Lowell had made, efforts to close avenues of publication to him and his assistants, domination of key posts in honorary organizations, and systematic campaigns in the media to undermine faith in Lowell's work … (Strauss, 2001: 220).

It was this concerted effort to discredit his claims that Lowell reacted to on his lecture tour of the Northwest and the West Coast. After Lowell's death, his staff astronomers devoted themselves to other work than Mars. Slipher studied the spectra of lightning, aurorae, the night sky, the Zodiacal Light, Venus, Mars, the giant planets, the solar corona, nebulae and comets, while Lampland took deep photographs of nebulae and comets with the 40-inch reflector. Thus, they began to differentiate themselves from their founder, and to escape what had almost been guilt by association. They also—both by temperament and inclination—concentrated on getting good data and letting the results speak for themselves. Understandably, both men were shy of the kind of publicity Lowell had thrived on, and were rather parsimonious in publishing.

That is not to say that they entertained any doubts about their Director's views on Mars and the other planets. They both remained loyal to Percival Lowell, in Lampland's case, 'excessively loyal', according to John C. Duncan, who had gotten to know him as a Lawrence Fellow in 1905–1906 (Duncan, 1952: 293). Though Duncan says that "… this did not prevent him from holding his own in their many arguments …" he does not say what the nature of those arguments were, apart from one example, which did not involve astronomical ideas at all:

> I remember an occasion early in 1905, when, upon opening his mail, he became taciturn and depressed for several days, a very unusual state for him. At last I found out what was troubling him: Mr. Lowell had announced an increase in his salary which he felt unworthy to accept. He never told me the outcome; perhaps it was a compromise between Lowell's fierce generosity and Lampland's firm humility. (ibid.)

Lampland's attitude toward Lowell is attested in a rather touching letter he wrote to Charles Sprague Sargent, Professor of Agriculture at Harvard since 1879 and author of the *Catalog of the Forest Trees of North America*, a month after Lowell's death:

> In the death of Dr. Lowell the world of science has lost an able fearless investigator and leader, but the sense of loss is especially heavy to those who knew him intimately—his many beautiful and lovable traits of character, his whole-souled enthusiasm and wonderful energy in everything he undertook, the lofty example of his life of devotion to science because it was to him a labor of love. (Putnam, 1994: 192–193).

This was generous.

7 TARNISHED LEGACIES

Percival had, of course, intended the work of the Observatory to continue after his death, and left generous provisions in his will for its continuation, but he was scarcely cold in the ground when his widow, Constance Lowell (Figure 13), who during the years of their marriage (1908 to 1916) had from all indications shown herself to be a devoted, agreeable and compatible companion, showed herself after his death to have an unpleasant, argumentative, contentious, grasping and pathologically litigious personality. According to William Lowell Putnam III,

> The consequence of this would be that seen in hindsight … [the] marriage was a disaster of the first magnitude for the pursuit of astronomy. (Putnam, 1994: 101).

After seeing to her first order of business—sending Lowell's secretary Wrexie Leonard (Figure 14) packing—Mrs Lowell proceeded to exert a stranglehold on her husband's estate that was resolved only after a costly legal struggle dragging on for more than ten years, during which the Observatory was forced to keep its operating budget to a bare minimum while seeing the resources of the estate progressively sapped through court costs and what V.M. Slipher, appointed Director after the founder's death, called 'very excessive' attorney's fees. Not only were

Figure 13: Constance Lowell, dressed in the mourning black that she always wore after her husband's death. His loss seems to have completely unsettled her (courtesy: Lowell Observatory).

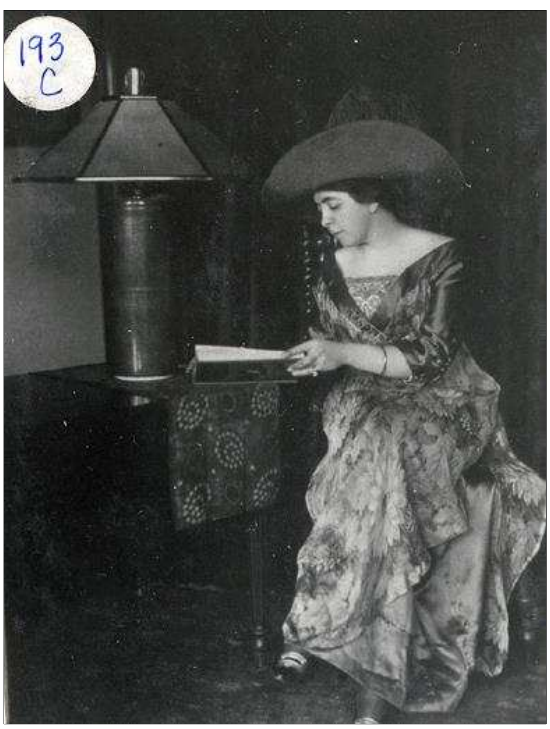

Figure 14: Wrexie Louise Leonard (courtesy: Lowell Observatory).

the Observatory staff bereft of a dynamic, inspiring, and decisive leader, they found themselves operating under a cloud of financial uncertainty in which the very survival of the Observatory was in doubt. Much of the burden of keeping things going under these difficult circumstances fell upon V.M. who had a mind for business and who, quite apart from continuing his research, often had to postpone payments to the staff and borrow against their salaries for the Observatory's ongoing expenses. Instead of the Observatory being so well endowed that it could, as E.M. Antoniadi once quipped to Camille Flammarion's widow Gabrielle, "… talk for a long time about canals on Mars …" (see McKim, 1993: 225) the Observatory was famously broke by the time the lawsuit was finally settled at the end of 1925. By then, V.M.—who was later described, probably accurately, by Flagstaff historian Platt Cline as "… slow, reserved, cautious, phlegmatic and a very thorough plodder …", lacking in empathy though formally courteous to all—was spending more and more of the time in his Observatory office hedging his bets in pursuit of his own private business interests. "Careful with his modest salary," Cline (1994: 97) adds, "he invested in livestock, real estate and securities and [eventually] amassed an estate of more than $1 million."

Under the circumstances, it is hardly surprising that the Observatory's research programs slowed and publications almost ground to a halt. By 1921, when V.M. published results for the distant elliptical galaxy NGC 584 in Cetus, he had pushed the spectrographic observations of nebulae with the 24-inch refractor as far as the instrumentation could bear, and then returned to the planets—publishing during Mars' very favorable opposition in 1924 a new set of spectra of Mars which, however, did not add anything new to his 1908 findings except the negative finding of not detecting chlorophyll in the spectrum (V.M. Slipher, 1924).

E.C. Slipher had, meanwhile, taken over the photography of the planets and especially Mars, using the planetary camera that Lampland had earlier used to photograph the canals to establish a reputation as the Observatory's virtuoso planetary photographer and leading in-residence expert on Mars. His photographs—which eventually numbered 200,000—would add enormously to the understanding of seasonal effects such as polar caps, clouds, and dust storms. However, being often away in Phoenix during legislative sessions beginning in the late 1920s, he also published very little. Only six papers written by E.C. appeared during the entire decade of the 1920s, all concerned with planetary photography and mostly of Mars.

Remarkably, Lampland, who was a notoriously perfectionistic and slow worker, published (according to ADS) twenty-seven papers or abstracts during that decade, which was almost double the number of the Sliphers combined. This was largely owing to the fact that during this decade, Lampland engaged in a highly productive collaboration with another scientist, William W. Coblentz, Chief of the Radiometry Section of the U.S. National Bureau of Standards, who did much of the data interpretation and writing for their joint publications.

At the oppositions of 1922, 1924 and 1926, Coblentz and Lampland worked in concert to produce the first direct measurements of thermal radiation from the surface of Mars. Coblentz, a founder of astronomical infrared astronomy, had previously developed stellar radiometers based on the thermoelectric effect, and with the assistance of Seth B. Nicholson, had used them in 1914 at Lick Observatory to measure IR radiation from 110 stars. His most important result was that these stars radiated as black bodies. At Lowell, Coblentz (1923) adapted these stellar radiometers for work with the optically excellent but mechanically unwieldy 40-inch (and later 42-inch) reflector (Figure 15).[2] These fascinating instruments have recently been studied in detail by Joseph N. Marcus, who found them exquisitely designed and cutting edge but temperamental—making them ideal for Lampland's technical skill and fussy attention to detail (Figure 16). Shorn of technicalities. The

Figure 16: Lampland holding one of the radiometers used to determine heat emitted from the surfaces of Mars and the other planets (courtesy: Lowell Observatory).

Figure 15: The dome of the 40-inch (and later 42-inch) reflector. The small structure in front of the dome was where Coblentz made the galvanometer readings as Lampland centered the thermocouple on the image of the planet (courtesy: Lowell Observatory).

basic idea was this: the telescope focused light and heat emanating from the surface of a planet onto a tiny thermocouple junction in a glass vacuum tube, eliciting a small current that was amplified and measured in a Thompson iron-clad galvanometer at the base of the dome. By interposing various filter screens or a 1-cm water cell that cut off radiation transmission beyond certain wavelengths, the spectrum could be parsed into its visible and infrared components, from which temperatures could be deduced.

Radiometry was a two-person job. As Coblentz (1951: 158–159) later recalled:

> Lampland usually operated the telescope and I made the galvanometer readings in a little underground room adjoining the great dome housing of the 42-inch reflector … To the casual visitor … who wandered by at night, it sounded monotonous, as we called the signals ('On', 'On'; 'Off,'—'Off') as Lampland set the thermocouple receiver "on" the star or planetary image, after which I read the galvanometer deflection, and called 'off' as I recorded the measurements.

By today's standards, Hearnshaw (1996: 68) notes that

> … the Coblentz radiometers' absolute efficiency in converting thermal into electrical energy was dismally low (around 10^{-6}), and worse than the efficiency of selective detectors in the form of selenium photoconductive cells and potassium photoelectric cells then becoming available to astronomers, nevertheless the very broad band (non-selective) nature of the thermocouple still made it an indispensable instrument for the IR region between about 1 and 10 microns …

Lampland was aware of the fact that calibration of the results for planets would be improved if it were possible to determine the transmissivity of the atmosphere to infrared radiation at different wavelengths, though this was something that would not become possible until the 1930s.

What Coblentz and Lampland (1923) actually found was measured planetary radiation as percentages of the total radiation, but this (misleadingly, in Lampland's view) was converted into actual thermometric degrees by the young Princeton astronomer Donald Menzel, so as to suggest a degree of accuracy greater than was experimentally warranted. Though to a purist like Lampland this was unforgivable, the thermometric degree results certainly made the data more digestable to the public—and even fired the imagination a little. Reported in this form, the two collaborators' results were found to be, as they wrote, "… in harmony with other observations of Mars …", presumably meaning Percival Lowell's 1906–1907 papers on the temperature of the planet which had yielded a maximum temperature of 22℃ (72℉), which, as Lowell himself had noted, made the Martian temperatures as comfortable as those of the south of England. Moreover, the temperate zones of Mars were "… not unlike those of a cool bright day on Earth, with ranges from 8° to 18℃ or 45° to 65°F." (Coblentz and Lampland, 1924a). So precise was the positioning of the thermocouple on the image that it was possible to obtain readings from individual albedo features on the Martian disk, which showed the dark areas to be warmer than the desert areas. From this Coblentz, in a paper that was not co-authored with Lampland, suggested it might be due to "… the existence of plant life in the form of tussocks, whether grass or moss." (Coblentz, 1925). Radiometric observations in 1924 by Edison Pettit and Seth B. Nicholson using vacuum thermocouples with the 2.50-m reflector at Mt. Wilson Observatory led to similar results: they found that the temperature rose to as high as 20℃ for the dark areas and ranged from –10 to 5℃ for the desert areas at the equator (Petit and Nicholson, 1924). But unlike Coblentz, these investigators did not comment on the relevance of their data to the existence of Martian vegetation—grass, moss, or any other.

As recently discovered by Marcus, the absorption efficiency of the blackener on the thermocouple junction of one of the radiometers used by Lampland and Coblentz was lower than expected in the infrared. This produced somewhat higher temperatures than those later obtained by spacecraft radiometry at the same sites. Sometimes lost in the discussion was the fact that the temperatures calculated by Lowell and measured by Lampland and Coblentz were ground temperatures. In Lowell's time, Alfred Russel Wallace had gone to great length to point out that air temperatures, which are those usually referred to by meteorologists, would be much lower. Lampland was, of course, well aware of this, and in an unpublished note found in the Lowell archives admitted that

> … means of ground temperature near the surface and the air temperature a foot or more above the ground on the Painted Desert [of Arizona] may give a difference of 46℉ (25℃), and for the middle of summer would no doubt greatly exceed this value. (Lampland, n.d.).

On Mars, this meant that even if ground temperatures were as high as Lowell calculated, air temperatures would be much lower, one's feet might be quite warm even as one's head was freezing. Moreover, though daytime temperatures might be warm on Mars, the thinness of the air provided little ability to retain heat after sunset, and so even in midsummer, nighttime temperatures were likely to be bitterly cold. All in all, the radiometric measurements gave a misleadingly benign reading of the Martian environment, but this was allowed to stand because it dovetailed nicely with Lowell's theories.

8 ORTHODOXY REIGNS

Lowell's ideas about Mars continued to reign supreme in the minds of his three loyal staff astronomers. V.M. wrote to a botanist in 1923 that he was a staunch believer in the probability of 'extra-mundane' life:

> If life be thought to result from the substance and forces of nature then we could expect from what we have observed of distribution of life on the earth to find it in the main where favorable conditions repeat themselves; or if life be thought of as due to Divine intelligence it is for me just as difficult to think of it being confined alone to the earth; for if life has importance in the Creator's mind it would seem strange that all the vast universe (with doubtless many fertile and genial fields for life) should be left barren … except this earth. For me to think of life as limited to the earth would mean either that we minify God and magnify man, or we would need largely to discard the results of astronomical research ...
>
> A thousand million rose bushes and one rose! It is neither according to religious or scientific teaching, but reminds us of the time when the earth was regarded as the center of the astronomical system. (V.M. Slipher, 1923).

Taking stock of the flurry of activity during the planet's very favorable opposition of August 1924, V.M. saw the results as largely vindicating Lowell. In a letter written at the end of 1926 he said:

> Years ago most astronomers looked with suspicion upon reports of observations concerning the planet and rather looked upon such work as unscientific and injurious to the reputation of those engaged in it. The Pendulum now seems to have swung oppositely. (V.M. Slipher, 1926).

Lampland was also in complete agreement with the founder's ideas. Well aware of the often bitter controversies the Lowellian spiderwebs had aroused, and especially aware of the criticisms launched at the 1909 opposition by Lowell's chief nemesis, E.M. Antoniadi, who on the basis of work with the 83-cm Henry brothers refractor at Meudon Observatory had dismissed the 'canals' as illusions, Lampland rose vigorously to their defense:

> The reality of the 'canals' has been a much-discussed question. Over a long period of years the matter was indeed a highly controversial subject. But the advent of the photographic plate … fully confirmed the results obtained with the eye and quite removes the basis of the arguments of some who attributed these strange markings to the imagination of the observer or optical illusions. Having established the reality of these markings it seems without profit to attempt to explain and evaluate the personal equation of different visual observers in their delineation of these markings. The differences in the draftsmanship and rendering of landscape views by different artists is recognized and all the more might one expect marked differences on so difficult a subject as planetary observations ...
>
> The so-called Lowellian type of planetary draftsmanship has not escaped severe criticism from some observers. In answer to these criticisms there is perhaps no more convincing evidence than the work of observers like [René] Jarry-Desloges and very recently [Robert J.] Trumpler who fortunately combined visual and photographic observations with the great Lick telescope. The narrow and definite forms of the canal markings by these investigators confirms Lowell's work. (Lampland, n.d.).

E.C., of whom we shall say more later, was also an orthodox Lowellian—and indeed remained so right up until his death on the eve of the first spacecraft mission to the planet.

Though Lowell's ideas about Mars survived intact, the radiometric observations of the other planets tended to undermine the late founder's theories. Regarding the giant planets, Lowell had, in concert with many other astronomers of the nineteenth and early twentieth centuries, subscribed to the view that Jupiter was a world arrived at a half-stage between a Sun and a planet. Fiery and chaotic in its interior, it was overlain by clouds caused by

> … vertical currents from the heated core and strung out in longitudinal procession by Jupiter's spin ... Yet Jupiter emits no light, unless the cherry red of his darker belts [and Great Red] be considered its last lingering glow. (Lowell, 1909: 167).

The inference was, then, that the Jovian clouds ought to be hot. Coblentz and Lampland made observations of the giant planets in the summer of 1924. When later Menzel calculated the temperature from their data, he found that the visible clouds of the upper atmosphere of Jupiter were frigid at –130℃. The temperatures of the other giant planets, Saturn and Uranus, were even colder (at –150℃ and –170℃). Lampland must have been surprised. The paper publishing these results, on which Lampland appeared as third author, stated only: "The temperatures of Jupiter, Saturn, and Uranus are low … There is little evidence of internal heat." (Menzel, et al., 1926: 177). This rather momentous result is not called out.

This may seem rather strange to us, but it is also characteristic of Lampland. He was much more comfortable gathering data and letting it 'speak for itself'. He was much more reluctant to interpret it. Adel (1987) said that Lampland,

> … should have been a librarian. If you asked him a question, his face would light up and he'd rush into the library and come back with a stack of books like this dealing with the subject. He missed his calling. He wasn't a research person. He was afraid to express himself, to take a stand. Percival Lowell once said of Lampland, he said, 'If you show Lampland a sheep and you ask him, what is it? Lampland will say, well, it looks like a sheep from that side. And really he was that way. He would never—how to put it? He would never—well, express a definite opinion, take a stand ... Lampland … was afraid to say, "These observations mean thus and so." Even if he knew something was wrong, you see, he would never do that, never say it. And that was his great failing. As far as Percival Lowell was concerned, it didn't matter, because all he wanted was assistants to do the things he told them to do. He wasn't looking for people who had their own ideas and initiative.

Another, even more notable case of Lamplandian cognitive dissonance, involved Venus. After V.M.'s spectrographic observations of 1903, Lowell never changed his views about the planet. In his book *The Evolution of Worlds* in 1909, he set forth his conclusions in much the same terms—and in the same florid prose—as he had done in 1896–1897. A single observation by Lampland and Coblentz in 1924 would, however, be enough to disperse these battenings of the imagination. Coblentz later recalled the circumstances:

> The first morning when Lampland set the thermocouple on the dark, unilluminated cusps of Venus, not thinking of the implications involved, I called to him that "a lot of heat" was radiated from the dark portions of the planet, and that it was of different intensities from the tips of the two cusps. Jumping down from the observing platform he came into the galvanometer room, saying that he thought I must be mistaken. Telling him to make the measurements himself, I climbed the ladder to the great reflecting telescope and made the thermocouple settings on the image of the planet, while he read the galvanometer scale, —and verified the measurements. He then told me how it might affect the "P.L. theories." I admired him for his self-sacrificing loyalty to Lowell. (Coblentz, 1951: 159).

Despite his initial discomfort with overthrowing one of Percival Lowell's cherished notions, the detection of infrared radiation from the dark side of Venus was a discovery of the first importance, and Coblentz and Lampland published it in several different places (see: Coblentz and Lampland, 1924b; 1924c; 1925). The two researchers clearly stated that the spectral selectivity of the emitted heat on the day and night sides was about the same indicated the day and night temperatures were about equal. Their competitors in planetary radiometry, Edison Pettit and Seth B. Nicholson at Mt Wilson, likewise published dark side observations showing the heat from the dark and illuminated hemispheres was about the same, both in 1924 and, in more detail, in a 1955 paper (Pettit and Nicholson, 1955). Inexcusably, they failed to cite the Coblentz and Lampland work. Nor did Carl Sagan (1960) mention them in a widely read paper. Coblentz and Lampland had published in as well-known and respected journals as Pettit and Nicholson, but perhaps the lingering odor of the Martian canal controversies prevented them from getting the same hearing as their rivals. Whatever the case, they lost out on credit for what was one of the most important discoveries ever made about Venus, and one that was notable, moreover, in complete opposition to 'P.L.'s theories'.

9 THE 'GREAT LOWELL OBSERVATORY MARS BOOK' GETS UNDERWAY

Lampland and Coblentz were in the thick of all this important pioneering near-infrared work when, in 1925, the long-running legal contest of Lowell vs. Lowell, which for a while must have looked likely to swallow up the entire estate of the late Percival Lowell like that of Dickens' Jarndyce vs. Jarndyce, was finally settled in a court room in Boston. The Estate had dwindled to less than half its original size by 'wastage', legal fees, taxes and executor's fees. When the resolution came, it had been largely owing to the heroic efforts of the Observatory's first Sole Trustee (and noted New England architect) Guy Lowell. Perhaps in part owing to the strain of overseeing the Observatory's legal affairs, Guy Lowell died prematurely at age 57 not much more than a year after the lawsuit was settled, suffering, in February 1927, while vacationing in the Madeira Islands, a stroke (the common Lowell family malady, which had also taken off Percival himself and his younger sister Amy). His body was relinquished to the deep. Before his death, he had had the foresight to nominate Percival's cousin, Roger Lowell Putnam (1893–1972; Figure 17), to succeed him in the role of Sole Trustee. Shortly after taking on the role, Putnam (1927a) wrote to V.M.:

> I presume that the reason Mr. Guy Lowell appointed me … was due to the fact that I specialized in mathematics in college, and have always been very fond of it.

He added, a few days later,

> I am looking forward to coming out and meeting you and your associates and working together to carry out Uncle Percy's wishes and to carry on his work. (Putnam, 1927b).

Putnam who had trained in mechanical engineering, proved to be as hands-on in his role as he could, allowing for his geographical distance (he was based in Springfield, Massachusetts), and after the long legal hiatus in which the staff astronomers had necessarily put the observatory into a state of semi-hibernation, he was intent on waking things up. In particular, he was eager to see the Observatory follow-up with vindicating, if possible, two of Percival's most cherished, though disappointing, legacies: 1. Mars and 2. The Search for Planet X.

The search for Planet X—which W.G. Hoyt calls the "Third Search"—has been often described, and in meticulous detail. Needless to say, getting a suitable telescope funded (the 13-inch Abbott Lawrence Lowell astrograph) and made operational cost the staff astronomers an enormous amount of effort, though it had to take turns with a number of other projects already underway, including conversion of the 40-inch reflector to a 42-inch, and the completion of a new spectrograph that V.M. Slipher would use to carry out extensive observations of the light of the night sky and aurorae, both from Mars Hill and at the 11,500-foot mountain station on the San Francisco Peaks north of Flagstaff. The latter was approved in 1926 as one of Guy Lowell's final services for the Observatory. V.M.'s heavy involvement with the business of the Observatory—keeping accounts, etc.—as well as his private business affairs (which had included investing in the Hotel Monte Vista which opened in January 1927) and E.C. being in Phoenix for months at a time on legislative business helps explain a slowdown of the Observatory's research production. It may also explain why the staff astronomers were so eager to hire the Kansas farmer and self-made astronomer Clyde Tombaugh. Tombaugh, though without formal training in astronomy and possessed only of a high school diploma, was quickly assigned to the grinding tasks of exposing the plates and examining them on the blink comparator.[3] Tombaugh's willingness to take up these burdens and his competence in carrying them through freed up more time for the staff astronomers to pursue other interests, astronomical and non-astronomical.

Another major commission imposed by Putnam involved what I refer to as the "Great Lowell Observatory Mars book". In contrast to the X search, which was at least a qualified success with Tombaugh's discovery of Pluto, the Mars book, despite occupying a great deal of the three staff astronomers' time and causing them much anxiety and worry, proved to be a distraction and ultimately a dead end.

On the same day Putnam had written to V.M. about "… working together to carry out Uncle Percy's wishes and to carry on his work …" V.M. had replied:

> If you have been following the trend of developments in regard to Mars, you no doubt have been pleased as we have with the fact that the results of the more recent oppositions [especially those of 1922, 1924 and 1926] … have yielded here and elsewhere results that have more or less confirmed Dr. Lowell's conclusions as to the conditions on Mars. (V.M. Slipher, 1927).

In a passage written somewhat later, Lampland acknowledged the way that the results of the instrumental triumvirate of camera, spectrograph and radiometer had largely borne out Lowell's conclusions:

> Carrying on and sustaining the splendid traditions of his predecessor [Schiaparelli], Lowell advanced greatly our knowledge of the planet by the vast store of systematically and carefully gathered observational data and their very complete interpretations. No student of Mars has so comprehensively attacked the many problems that must be answered to give reasonable interpretations of the phenomena that were seen to take place upon that distant world. It is a keen satisfaction to be able to record that a continuation of the investigations on Mars with other means of attack which have come into use in more recent years have completely confirmed many of his results that were openly scoffed at by the more conservative astronomers of his day. (Lampland, n.d.).

Figure 17: Roger Lowell Putnam (courtesy: Lowell Observatory).

Somehow these rather glittering generalities would be translated into plans for a multi-authored Mars book. The exact history does not seem to have been recorded in the Lowell Observatory Archives, but it is entirely possible that the idea of a book on Mars was discussed during Putnam's first visit to Flagstaff the following month. In any case, it quickly captured Putnam's imagination (if not those of the three staff astronomers) and became his highest priority. In later correspondence he would often ask for progress reports or copy in his subsequent correspondence with V.M. Slipher and Lampland, who usually responded that, though committed to making progress, they were seriously pressured by other work. This was no doubt true, though one can think of several other reasons that might have made them reluctant to don the mantle of authorship.

The first was that, after years of avoiding 'startling telegrams' and of 'letting results speak for themselves', they were now being forced to take up the very subject, Mars, which had embroiled the Observatory in so much controversy from the professional astronomical community during the late founder's lifetime. Though they were, to a man, true believers in the Lowellian view of the planet and convinced, as V.M. had told Putnam, that the results at recent oppositions had more or less confirmed Lowell's conclusions about conditions on Mars, they can hardly have been eager to reopen old wounds. After arriving at Lowell in 1929, Clyde Tombaugh recalled:

> A few times the two Sliphers and Lampland poured out to me the anguish of their souls. One time, Dr. V.M. Slipher told me, 'The lot of the older men here is not a happy one,' because of the ostracism from the astronomical community. (Tombaugh and Moore, 1980: 101).

Tombaugh assumed that, at least in part, the ostracism was related to the long controversy over the canals of Mars. Writing a book on the very subject that had led to that ostracism can hardly have been an endearing prospect.

Another problem was that, although a significant amount of the Observatory's research after the founder's death had been devoted to Mars, each of the three men had his own well-defined specialist interest, which had been established early on. V.M. knew spectroscopy, Lampland radiometry, and E.C. photography. None of them dared cross over into the field claimed by another. The result was that none of the three had the fully comprehensive and global view of Martian research that Lowell himself had had and that E.M. Antoniadi, Lowell's nemesis, possessed by the 1920s.

In addition to the lack of overlapping expertise, all three men were rather halting and unconfident writers. Lowell had been a fluent writer with a highly literary if somewhat dated, overwrought and flowery style. The Sliphers and Lampland experienced composition as a very slow and laborious process. Extended book-length composition was almost an impossibility. Describing Lampland's difficulty in presenting his ideas in talks at meetings, Adel (1987) said:

> Lampland was notorious for being unable to speak coherently on any topic if he were presenting a paper. He didn't know where to begin and he certainly didn't know where to end. But he was held in great esteem by astronomers, possibly because he was at Lowell, had worked with Percival Lowell, and because he was an older person, and was working in a difficult field, radiometric observations of the planets. It wasn't easy. So he was given a generous amount of time for the presentation of his paper … 40 minutes maybe, and he got to the end and still hadn't said anything. Someone in the audience suggested he be given additional time, and he was, and he didn't get any farther along then.

Some of the same difficulties can be noted in Lampland's drafts for the Mars book. Lampland's mind did not run linearly from point A to point B, obviously, and thus was rather challenged when it came to straightforward exposition.

At least in broad conception, the way forward should have been clear enough. There was never any question that the book was to be thoroughly Lowellian in point of view. Indeed, the authors set forth their intentions and plan by simply copying the Table of Contents from Lowell's *Mars and Its Canals*. They agreed with Lowell in regarding as the most important question about the planet as the possibility that conditions on the planet were favorable to the development of life, probably of a lower order (though they were careful not to rule out the possibility of intelligent life). The reality of the canal system that Lowell had described (see Figure 18) was not doubted, although as noted earlier, only E.C. had personally seen the canals in anything like the Lowellian fashion, as narrow straight lines (Figure 19). V.M. and Lampland rarely made visual observations of Mars or any other planet, and so they had to take the Lowellian system rather on faith.

In addition to drafts preserved in the Lowell Archives, documentation of the Mars book includes numerous marginal notes in pencil, scattered across journals and books in the Lowell library, that show that an effort was made to keep up with the recent literature on the planet.

The drafts produced by the three men show differences in their interests and literary style

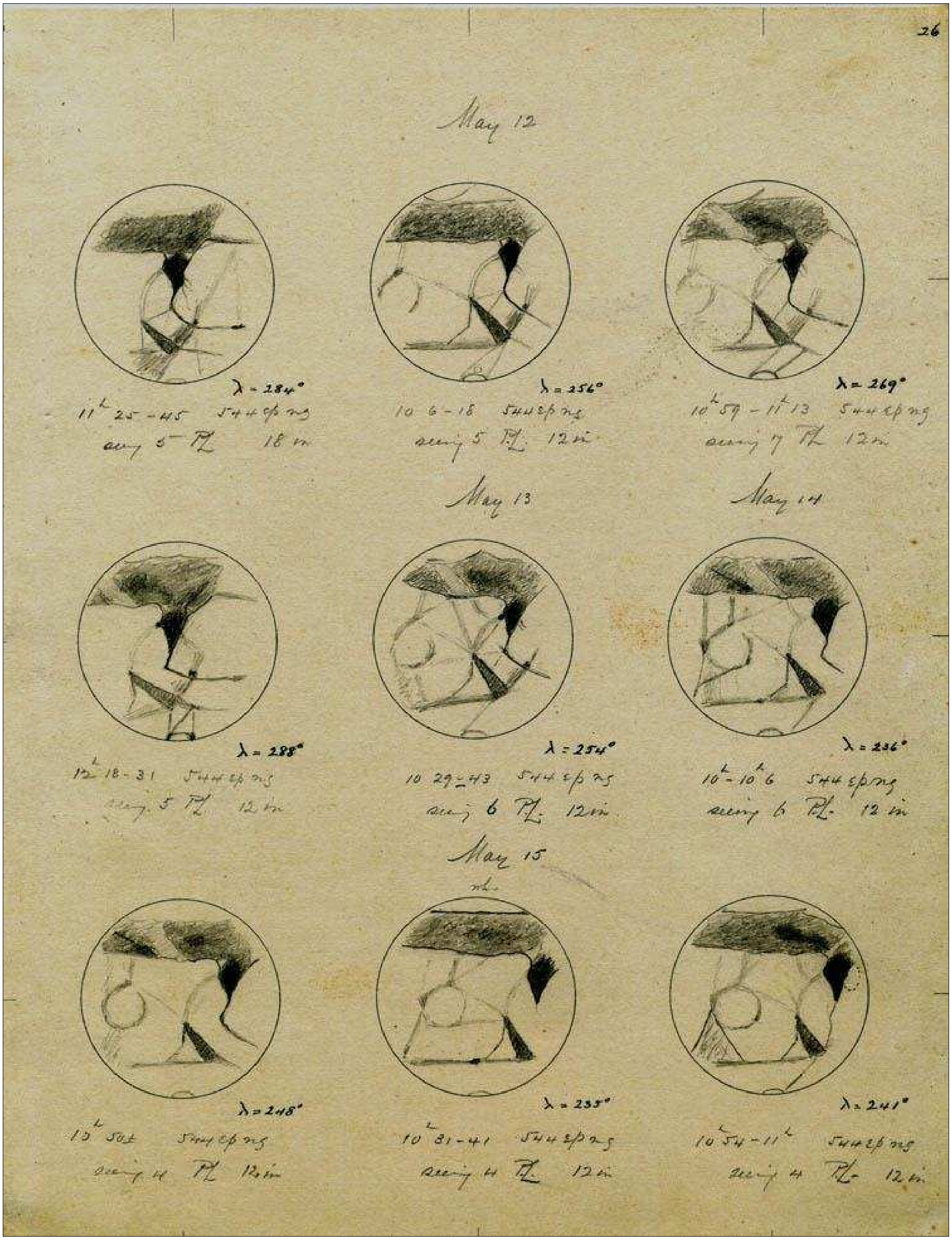

Figure 18: The power of suggestion. Logbook page from 1905, showing Percival Lowell's sketches of Mars from May 1905; compare with Figure 19 (courtesy: Lowell Observatory).

V.M. produced the following as an overall argument for the book:

> Briefly … to support organic life the three essentials are, air, water, and sufficient warmth. Now these matters are so closely related to one another in the life history of a planet that the proof of the existence of one either proves the coexistence of the other or requires in the main the same kind of evidence of its presence. To make accurate observations from which we may arrive at

> satisfactory determinations of these three properties is more difficult and cannot be arrived at by direct observation so easily, but rather requires much information to be gained only from observations by different instrumental means and modes of attack. To get the utmost information for the ultimate solution of these questions requires much time, that certain observations may be extended over long periods of time, in order to examine the variations brought about by the planet's seasonal changes … For this reason, and for just such evidence as these require, the Lowell Observatory … has been amassing observations of the planet now extending back over a period of forty years. Possessed of this information and experience, they should, it would seem, be able to proceed with a clear[er] comprehension of results [than can others] and arrive at a [more] authoritative conclusion about Mars than those who have made no special study of the planet. (V.M. Slipher, n.d.).

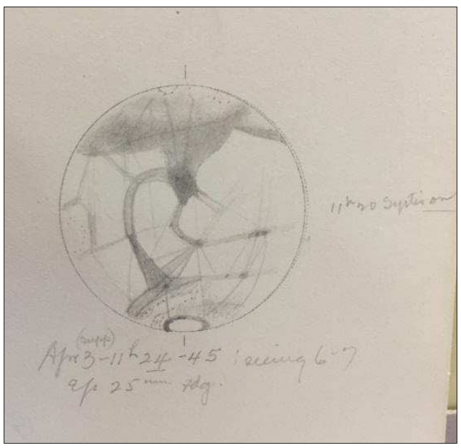

Figure 19: A drawing of Mars by E.C. Slipher, showing a close resemblance to Lowell's. The two men often observed together, and E.C.'s were the only drawings that closely resembled those made by his employer (courtesy: Lowell Observatory).

In the same rather clunking style (it is, remember, only a draft) he goes on to discuss the way that observers of Mars had originally believed in the existence of seas and lakes on the planet's surface, but that later observers had failed to verify this. Eventually, however, the pendulum had swung back, he says, as the spectroscope had definitively established the existence of water vapor. Alluding to his 1908 studies (he says nothing about W.W. Campbell's negative result from Mt Whitney in 1909), he writes:

> The darkening of spectral lines in the red end of the spectrum of Mars, photographed at the place and time that the earth's air was particularly dry revealed water vapor in the Martian atmosphere and in an amount comparable with that which exists on the earth at points of high altitude. The same kind of observations disclosed the further important but not surprising fact that there is more water vapor present above the melting polar caps of the summer hemisphere than existed at the same time in the air above the equatorial regions of Mars. Not only has the presence of water in the Martian atmosphere been detected but the actual amount of it disclosed by the photographs through the spectrograph has been measured so that we have quantitative as well as qualitative results to reason from. (ibid.).

Lampland seems to have been the most conscientious of the three, and as he had done since he first joined the Observatory, tended to obsess (and complain) not only about his own heavy workload but on his colleagues' perceived slackness. In his diary he kept tabs, on a daily basis, on what everyone else was doing (sometimes even in Norwegian runes, where he entered his most severe censures). To take a rather random entry, on 26 January 1926 he wrote of E.C.:

> Not much observatory work today. Came late this morning sat around visiting most of afternoon. Nearly 4 pm. I came to shop and he was there visiting with workmen … Not in office when I returned from 40". (Lampland, 1926).

Such entries exhaustively tallied would occupy many pages. Later things got even worse with E.C. getting elected to the State legislature and spending much of his time in Phoenix when the legislature was in session. In addition, V.M. was increasingly preoccupied both with the Observatory's business (accounts, etc.) and his own private business affairs (including launching the Monte Vista hotel, in which he was an investor). This could not but take a toll on his own research productivity, and of course it did. But though Lampland clearly disapproved of his colleagues' slackness, there is no evidence that he ever confronted them directly, and so his entries come across at times as those of a passive-aggressive busybody. Nevertheless, it must have given the rather self-righteous and rigid Lampland a great deal of satisfaction in being able to congratulate himself for his superior dedication to the cause of the Observatory, though the large and eclectic library he assembled shows that he did not entirely lack for leisure time.

Lampland's drafts, of course, were mainly centered on his radiometry data, and his inferences from them regarding planetary temperatures. He began by addressing the 'general reader' about the question of a planet's surface temperature, but proceeded almost immediately into 'brambles and quicksand', adding complications to complication as more and more qualifications and subtleties introduced themselves. One can already sense that this is headed in the same direction as his presentations at meetings, without a clear sense of where to begin or where to end and what to include in between.

Here is Lampland's mind at work:

> The general reader who has given little or no attention to such subjects as insolation, albedo, planetary radiation, climatology etc may find such topics difficult or unintelligible upon first meeting with the words. For that reason the discussions and illustrations will be of a concrete nature, making use of expressions and examples that fall within the experience of those not occupied with the technical aspects of the problems.
>
> Even to the most cosmopolitan individual or the world wide explorer familiar with the greatest extremes and diversities of climate and weather the determination of climatic conditions and the temperatures of the surface of the earth from observations from a distant point in space through the intervening medium of our atmospheric ocean would certainly involve complexities not anticipated in the casual consideration of the problem. This state of affairs is brought strikingly home to the investigator occupied with radiometric researches on the planets in attempting to interpret the meaning of the results of his measures … (Lampland, n.d.).

An outline of some of the topics Lampland planned to discuss gives some idea of the complexities he was feeling it necessary to present to the reader. If he had accomplished all that is outlined below, he might have completed a work that would have anticipated by a quarter of a century Gerard de Vaucouleurs' *Physics of the Planet Mars*:

> Planetary Heat and Planetary Radiation. Definition.
> --The solar constant; its measurement and value.
> --Seasonal variation (due to inclination of planet's axis of rotation) and its effects on planetary temperature and climate
> --The atmosphere, land, and oceans—their influence on planetary temperatures and climates.
> Planetary Temperatures
> On the Earth
> On the planets:
> --inferred from observed activities. (Mars). Melting of polar caps, clouds, seasonal changes.
> --calculation of, by means of measured value of solar constant, the measured albedo, and the application of the laws of radiation.
> --calculation from radiometric measurements of the intensity and spectral distribution of the planetary radiation. {Stephan's [sic.] Fourth Power Law of Total Radiation; Planck's Law}
> Modern radiometry: advent and development, recent applications to planetary research
> Radiometers. Historical and short descripttions of various types.
> Thermocouples, in vacuum cells.
> References:
> Solar energy curves (See: Smithson. Miscell. Coll. 74 no. 7, 15).
> (Annals Astrophys. Obsy vols II, III, and IV)
> Atmospheric transmission (See Fowler's intensities work and Astrophys. Jour. and elsewhere.)

The most satisfying of the drafts produced by the three men are by E.C., presumably because planetary photography was a more immediately accessible subject than either spectroscopy or radiometry. He wrote what amounted to a primer on the techniques of planetary photography which would be largely reproduced—although needing to be supplemented by his pioneering techniques of using the Kodak Dye transfer process to produce color images of Mars from tri-color black-and-white negatives taken through blue, yellow-green, and red transmission filters—in his book *Mars: The Photographic Story* (E.C. Slipher, 1962). To give some flavor of what he was doing, Slipher wrote in a late-1920s draft:

> The image[s] of the various planets at the principal focus of the telescope objective are too small to show planetary detail on the photographic plate sufficiently well to be of the most practical value for subsequent study. Therefore it is necessary that this image be amplified, and to do this a lens is inserted at a proper place near the focus of the big object glass … This lens magnifies the image for the photographic plate, under proper manipulation, just as the ordinary eye-piece does for the visual observer. It allows, by adjustment, any magnification desired.
>
> Next we must correct for the chromatism of the telescope, because the visual refracting telescope does not bring light of different wavelengths to the same focus… The out-of-focus light does not interfere with eye observations but in the photograph it is ruinous especially because [the out-of-focus light] is the color [blue] to which the photographic plate is most sensitive … To rid the planet's image of this stray light … [a] yellow-orange [filter is used].
>
> Next, we must find a photographic plate which is sensitive to this color, for ordinary plates are strikingly insensitive to orange light. Therefore plates of special sensitiveness are required … While their [sensitivity] is well suited to the color curve of the telescope objective, yet their lack of speed and coarseness of the silver grains of the emulsion are serious obstacles … [This is because] even on the fastest photographic plates a time exposure is necessary to get a highly magnified picture of a planet … In the case of Venus the time is only about one-third second, Mars one and three-quarters to two and three-quarters seconds, Jupiter averages about five seconds, while in the case of Saturn the exposure time ranges from twenty to thirty-five seconds … [The

> fact that] our air waves cause a motion of the image, which in time exposures obviously causes blurring … is the chief obstacle that militates against securing the sharpness of delineation required for registering the most minute planetary details. (E.C. Slipher, n.d.).

Though work on the Mars book was obviously somewhat sporadic, and nothing more than preliminary drafts were produced, at least occasionally the task intruded into the three men's consciousness. Thus, for example, in January 1928 Lampland wrote to E.A. Fath at the Goodsell Observatory in Northfield, Minnesota, that

> … work on it is in progress but we have been completely swamped with work and it has not been possible to devote as much time to it as we might wish. We certainly hope that the thing will be finished before autumn." (Lampland, 1928a).

Similarly, he wrote to John A. Miller of Swarthmore College in February 1928, to apologize for not being able to accept an invitation to present a paper before the American Philosophical Society. "It is curious how things work out," he told Miller:

> The coming months [find] me so completely buried in work that I must get done and out of the way in some fashion.
>
> The radiometric work alone planned for the remainder of the year would more than keep me going at a strenuous pace, with new apparatus under construction to be used in extending some of the problems with which we have been occupied. The method promises new ways of getting at certain problems otherwise inaccessible at the present time. As you probably know, radiometric work is most laborious and attacking new lines is inviting grief in abundance. But it all very interesting … This past summer I completed a large radiometer adapted for measures over the entire lunar surface … and this work will I believe be of much value when the observations are carried over the full lunation.
>
> A large amount of difficult work needs to be done on terrestrial problems, as well as in the laboratory to enable us to interpret more clearly the planetary measures. Albedos at present refer mostly to the visual region of the spectrum … Also, we should know more about emissivities of various substances, rocks, soil etc. Here also it will be necessary to employ radiometric methods in the laboratory and the field. The encouraging part of it is that the region about us here offers excellent opportunities for field work—the mountains, desert, volcanic cones and lava fields, and cliffs are not far away. A heavy part of the work is the design and construction of suitable apparatus.
>
> As I mentioned in the telegram the other evening, we have on our hands just now the writing of a book on Mars. It is a joint undertaking of V.M. and E.C. Slipher, and myself. It has been planned that I am to attend the International Astronomical Union at Leiden, and that means that I must finish my part before leaving Flagstaff. Writing on a subject so filled with difficulties is something that takes time—for me at any rate—and the job will keep my nose on the grindstone the coming months. (Lampland, 1928b).

That autumn, when Lampland had optimistically hoped the Mars book would be done, he apologized on his way back from Leiden for his failure to stop at Northfield (even though it was close to his home of Hayfield, Minnesota, which he did briefly visit), appealing as usual to the burden of his research work: "I felt that I could not spare another minute as my work had been entirely at a standstill during my absence …" he complained to Fath at the end of October. Now he expressed the hope that the Mars book would be done by Christmas, but he hardly inspired confidence in adding,

> It is a job we must make every effort to get out of the way but we are rather severely handicapped by shortage of help and with a very heavy observing program on our own hands. (Lampland, 1928c).

In fact, in addition to ongoing observing programs, the Observatory was now launching the 'Third Search' for Lowell's Planet X, though the senior astronomers were relatively unencumbered by it, thanks to the January 1929 addition to the staff of Clyde Tombaugh, the "… young man from Kansas …" who despite lacking formal training in astronomy (which was seen to be an advantage) was highly motivated, a self-starter, and used to hard work and meager and ascetic living conditions (such as the shambolic apartment in the second floor of the Administration building which was uncomfortable and barely inhabitable at the time).

10 THE TWILIGHT OF AN OBSERVATORY

As everyone knows, that search succeeded not in finding Lowell's Planet X but Pluto, which was at first hailed as the ninth planet but is now acknowledged to be, more interestingly, the prototypical object of the Kuiper Belt. After the discovery of Pluto, the Mars book disappears from the Archives. By then its somewhat meandering and lackadaisical course had apparently become something of an inside joke. Whenever the wives of the three men could not locate their husbands, they were said to have been working on the Mars book. Clearly, the deadline slipped past Christmas 1928 and the book was no closer to completion by Christmas 1929—even though Putnam was still occasionally inquiring about it. With the discovery of Pluto, Lampland

(Figure 20) especially, found his time largely taken up with chasing the planet along its path with the 42-inch reflector in order to obtain positions for its refined orbit. Putnam saw this, and granted the three collaborators a temporary reprieve. As with the temporary nature of V.M.'s hiring by Percival Lowell in 1901, the temporary part of the reprieve seems to have gradually been forgotten.

By then, the book would have been increasingly beyond reach of the three authors anyway. Spectroscopy and IR studies of the planets were now largely taking shape within the increasingly complex field of modern physics. When Arthur Adel, trained in infrared spectroscopy at the University of Michigan and a dedicated researcher, began to collaborate with and eventually to take a staff position at Lowell (publishing no fewer than fifty papers on the research he did there), he found the three older men woefully out of date:

> Talks were never given by invited astronomers. There were no such things as colloquia or seminars, nothing of the sort. The older people, you see in point of fact, had long since departed from active astronomy. They weren't doing anything. Lampland would putter. He'd putter, he and possibly his wife would go out to the telescope and make some measurements, without ever intending to do anything with them. V.M. Slipher [Figure 21] was busy with his properties and E.C. Slipher [Figure 22] with politics or doing something else in town, or suffering his terrible tooth aches. He had awful tooth aches. I felt sorry for him. (Adel, 1987).

Though Lampland continued as usual his ambitious course of self-study, reading widely (and not just in astronomy), his specialized knowledge of his own field lagged farther and farther behind. At a time when Adel was preternaturally productive, making a series of important discoveries in rapid succession, including working out from V.M. Slipher's spectra the harmonics of vibration of methane and ammonia molecules to show how they gave rise to absorption bands observed in the atmospheres of the giant planets, the profile of atmospheric transmission as a function of water-vapor content from 5 to 14 microns, the observation and preparation of the first grating map of the solar-telluric spectrum, and the discovery of the 20 micron IR window in the Earth's atmosphere—the latter one of the foundational discoveries leading to modern astronomical IR spectroscopy—Lampland affected the world-weariness of the man who has already seen all there is to see. He told his old friend John C. Duncan, now a Professor of Astronomy at Wellesley College:

> We have lived through an interesting period in the history of astronomy, and it is at the moment difficult to see any unexplored frontiers, unless the two hundred inch [at Palomar] should unexpectedly show greater penetration than one might anticipate at this moment. (Lampland, 1937).

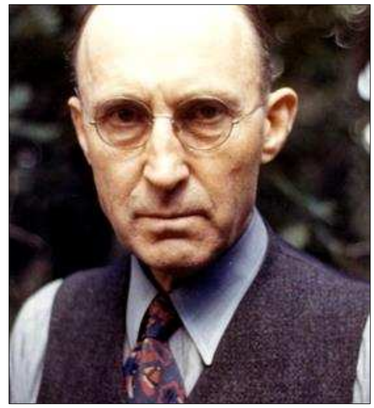

Figure 20: A later image of C.O. Lampland, in 1940 (courtesy: Lowell Observatory).

This was a remarkably pessimistic view.

Indeed, rather like the decrepit and deteriorating Baronial Mansion, which had become more of a fire hazard than a livable dwelling, intellectually the Lowell Observatory was increasingly running down as the three senior astronomers went from being middle-aged to geriatric.

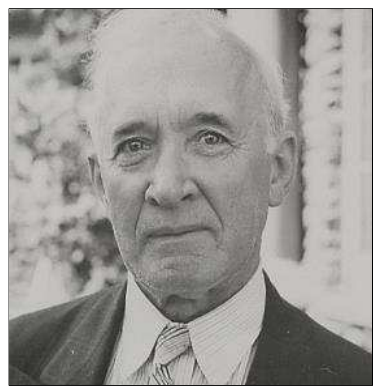

Figure 21: V.M. Slipher in 1940 (courtesy: Lowell Observatory).

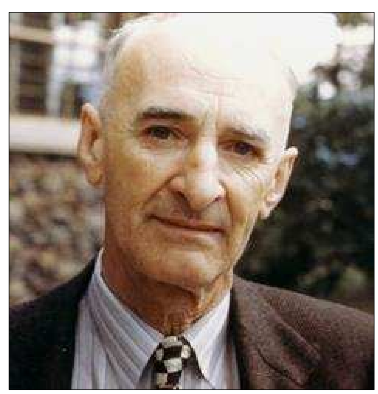

Figure 22: A later image of E.C. Slipher, in 1940 (courtesy: Lowell Observatory).

Figure 23: Art Adel (left), one of the undoubted geniuses associated with Lowell Observatory, shown here with Robert Krause at the Arizona State College 24-inch reflector, the first fully dedicated IR telescope (courtesy: Lowell Observatory).

Adel, rather bitterly, felt that his work was not appreciated and was resented as a threat by his seniors, and with the outbreak of World War II he was rather ham-fistedly forced into the Army by E.C. Slipher, who served on the local draft board. Under regrettable circumstances, he never returned to Lowell, but ended up establishing an infrared laboratory and dedicated infrared telescope at Northern Arizona University (Figure 23), where he rebounded from the Lowell debacle and went on to have a distinguished career.

Clyde Tombaugh (Figure 24), who came to Lowell Observatory without a degree, took leave of absence from the Observatory in 1932 to complete his Bachelor's degree at the University of Kansas. On one occasion he described to Lampland a discussion at the home of Dinsmore Alter, the Professor of Astronomy. At the time, Tombaugh was obviously a true believer in the Lowellian orthodoxy and

Figure 24: Clyde Tombaugh in 1940 (courtesy: Lowell Observatory).

> [Alter] had obtained some ice cream packed in 'dry ice'. We were having some fun playing with this novel substance, when suddenly he remarked, 'This is what I think your Martian polar caps are made of. I took up the matter in good humor and asked him to explain the blue band bordering the cap after the melting of the latter was under way, and which (the blue band) behaved like a free liquid … Then I asked him to explain the great seasonal changes observed in the maria if they were not due to vegetation … At this point he changed the subject of conversation. But I was determined to get him to admit that life still existed on Mars, at least vegetational life, from the standpoint of observational evidence (for my feelings were pricked at his effort to change the subject when I was about to corner him in the argument). I again introduced the subject in a courteous manner, but he would not discuss it. I will forgive a man for not believing a fact or truth, especially on the Mars question, but when he will not allow me to present my argument or proof, I deem it a discourtesy and offence. The Mars question has not been discussed since nor even introduced. And I regret to say I do not have the best feeling toward him. (Tombaugh, 1932).

In 1938, V.M. hired Tombaugh's brother-in-law, James Edson, also an astronomer, to do some darkroom work. However, Edson had ideas for his own researches. He used a 16-inch reflector that was personally owned by V.M. to photograph the cusp extensions of Venus near inferior conjunction, and engaged in some innovative research doing photometry on some of E.C.'s stacked images of Mars in which, among other things, he showed that canal-like markings registered on the images were narrower than the resolution limit of the images, that the 'ground' on one side of the canals tended to be darker than the ground on the other, and that the canals were 'lumpy', varying in darkness from one point to another, whereas Lowell and others had shown them as smooth. Edson later noted that the trend of his research

> … seemed to be more or less at variance with current ideas at Lowell and elsewhere … The prospect seemed to be for more new conflict as further estimates. (Giclas, 1991).

Looking back long afterward, Edson realized that although the Slipher brothers had been reluctant to allow him to do anything besides routine darkroom work, they had tried to be accommodating. At the time the accommodation was not quite so evident, however. Henry Giclas (ibid.) remembered on one occasion overhearing

> … a vehement and acrimonious encounter between V.M. Slipher and James Edson half way up the east basement stairs one evening early in the year. From what I could not help from overhearing at my office half

> the building away was a terrible tirade about the young people trying to take over. (ibid.).

Edson felt that Lampland was the one "… member of the senior staff who understood and actually helped …" what he was doing, despite the "… potentially upsetting implications." (ibid.). Lampland continued to 'putter' until his sudden death on 14 December 1951, while V.M.—despite Putnam's attempt to replace him with Adel's fresh knowledge and dynamism as a researcher—kept the 'young people' from taking over until, in 1954, he finally retired at the age of 79. By then the U.S. Weather Bureau had begun to provide the first outside funding in the history of the Observatory (see Figure 25)—funding that V.M. had fought tooth and nail to oppose—for research on planetary atmospheres, and a brilliant instrument-man named Harold Johnson joined the staff to lead the Observatory into a new era in which photoelectric photometry was to be developed into a major tool of astronomical research. Mathematician Albert Wilson, who had been leading the Palomar Sky Survey for Caltech, was brought in as the Observatory's third Director (after only Percival and V.M.). One of his qualifications appears to have been that he was acceptable to V.M. and E.C. (Tenn, 2007). However, Wilson proved to be rather unpopular, but he did start the Lowell Observatory "… on the road to modernity." (Tenn, 2007: 65). But only after Sputnik, when abundant Federal resources began to flow in support of American science, was the Observatory fully revived and brought into the late twentieth century under Director John Hall, whose tenure began in 1958.

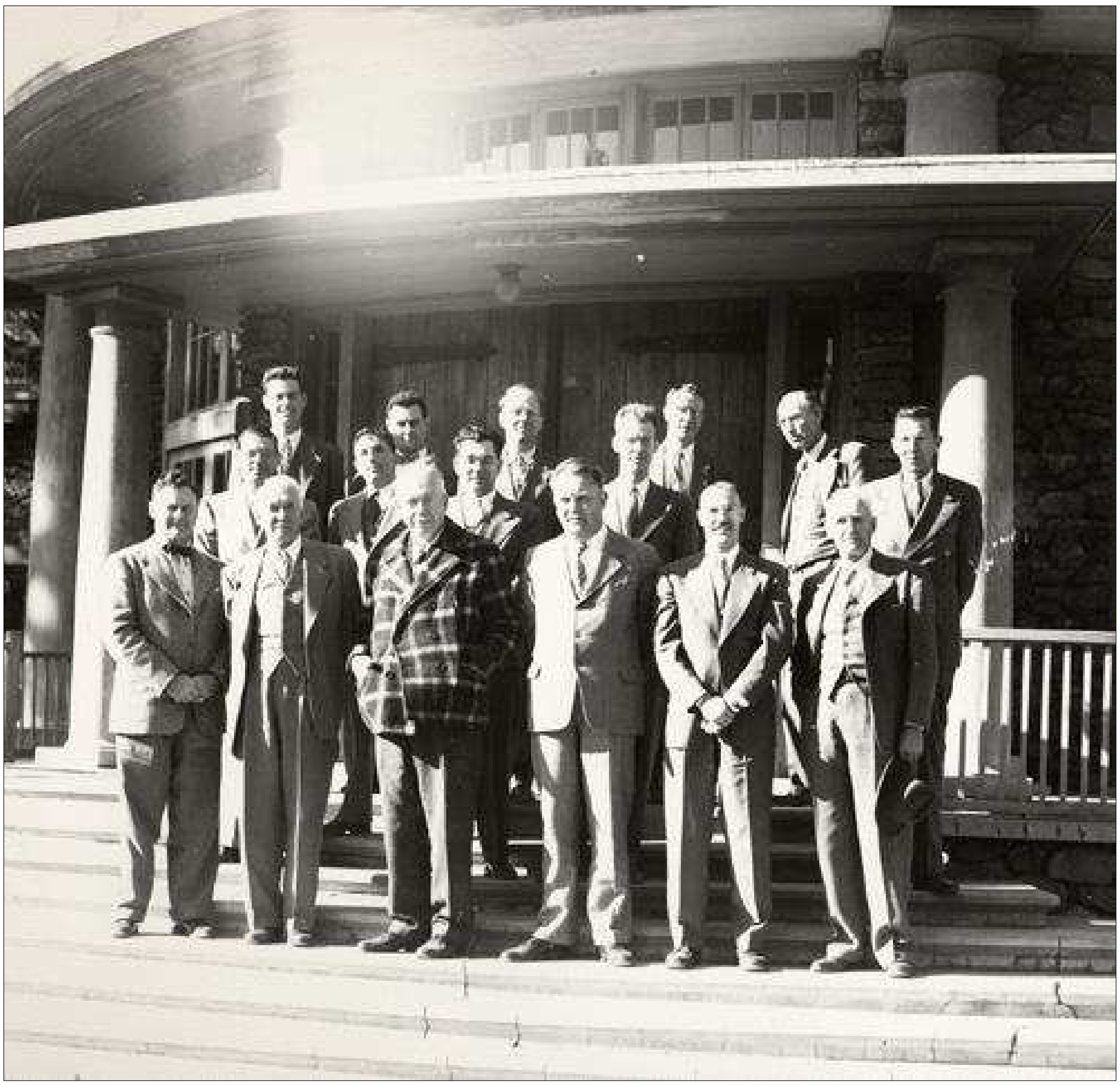

Figure 25: A new era begins. With funding from the Weather Bureau Lowell Observatory hosts a conference on planetary atmospheres in March 1950. In attendance were: (front row) Roger Putnam, V.M. Slipher, Edson Pettit, Gerard Kuiper, Seymour Hess, and E.C. Slipher; (middle row) Rudolph Penndorf, Arthur Adel, James Edson, Rollin Gillespie, and C.T. Elveg; (top row) William L. Putnam III, Roger L. Putnam, Jr., unidentified, Henry Giclas, and C.O. Lampland (courtesy: Lowell Observatory).

Figure 26: E.C. Slipher with the planetary camera attached to the 27-inch refractor at the Lamont-Hussey Observatory in Bloemfontein, South Africa, imaging Mars during the opposition of 1954 (courtesy: Lowell Observatory).

11 WHERE HAVE ALL THE CANALS GONE?

E.C. continued to be widely recognized as one of the leading experts on Mars, but he became more slapdash and haphazard over the years. Admittedly, he spent more and more of his time with his politics and other non-astronomical activities, but the decline is noticeable: the nice A4 sheets with a printed title and circular disks that he had used in earlier years seem to have disappeared after the 1930s, while in 1943 several Mars drawings are on the backs of envelopes, as though he had gone to the telescope unprepared. He published only a fraction of all of his photographs, and there are still many excellent ones that remain unpublished. They are occasionally consulted by researchers. E.C. should have written them up.

Figure 27: E.C. Slipher comparing globes of the Earth and Mars in 1957 for "Mars and Beyond", a telecast for Walt Disney's "Tomorrowland" series (courtesy: Lowell Observatory).

E.C. went to South Africa to photograph Mars in 1939 and again in 1954 (at the age of 71—see Figure 26). These expeditions were widely publicized, and attest to the continuing public interest in Mars. But E.C.'s views about the planet had hardly changed since Percival Lowell's time. In an article on the 1954 Mars expedition written for *National Geographic* (which helped fund the expedition), he referred to the dark markings on Mars as "… blue-green areas …" and maintained that the development of a new dark area "… the size of Texas …" which he called "… the greatest change observed in the geography of the planet since its surface was first mapped 125 years ago …" seemed to indicate the annexation by plant life of a vast area of the desert (Slipher, 1955: 430). Furthermore, "Such green areas bear eloquent testimony to the fact that Mars is not a dead world …" (Slipher, 1955: 431).

As for the canals, E.C. still believed as he had done in the first decade of the twentieth century:

> Most astronomers now agree on the existence, if not the nature, of this strange network of faint lines which interlace the green areas and the desert regions as well. They do not meander like normal stream drainage. One runs for 1,500 miles without a bend—half the distance across the United States. Sometimes one canal will run right through another, something no sensible river would do. (Slipher, 1955: 434–435).

Clyde Tombaugh taught navigation to naval personnel during WWII but was never rehired by Lowell Observatory. By the early 1950s he was working at White Sands Missile Range and proposed that the 'oases' on Mars might be craters formed by meteorite impacts and the canals cracks along which primitive vegetation might grow. University of Michigan astronomer Dean B. McLaughlin maintained that the canals were neat windrows of volcanic ashes spread from volcanoes. However, E.C. would have none of this: "To me they suggest lines of vegetation along watercourses." (E.C. Slipher, 1955: 436).

E.C. was approached as an authority on Mars for the film "Mars and Beyond" (Figure 27), presented in 1957 on Walt Disney's "Tomorrowland" series.[4] The following year he was commissioned by the U.S. Air Force to prepare its official Mars map; it is a maze of spidery canals (Figure 28)! His final word on Mars was the book *Mars: The Photographic Story*, published in 1962. This was the closest the "Great Lowell Observatory Mars Book" ever came to fruition, and draws on E.C.'s drafts for the project written in the 1920s. He then followed up two years later with another book, *A Photographic Study of the Brighter Planets*, which while repeating much of the Mars material in his earlier book, does

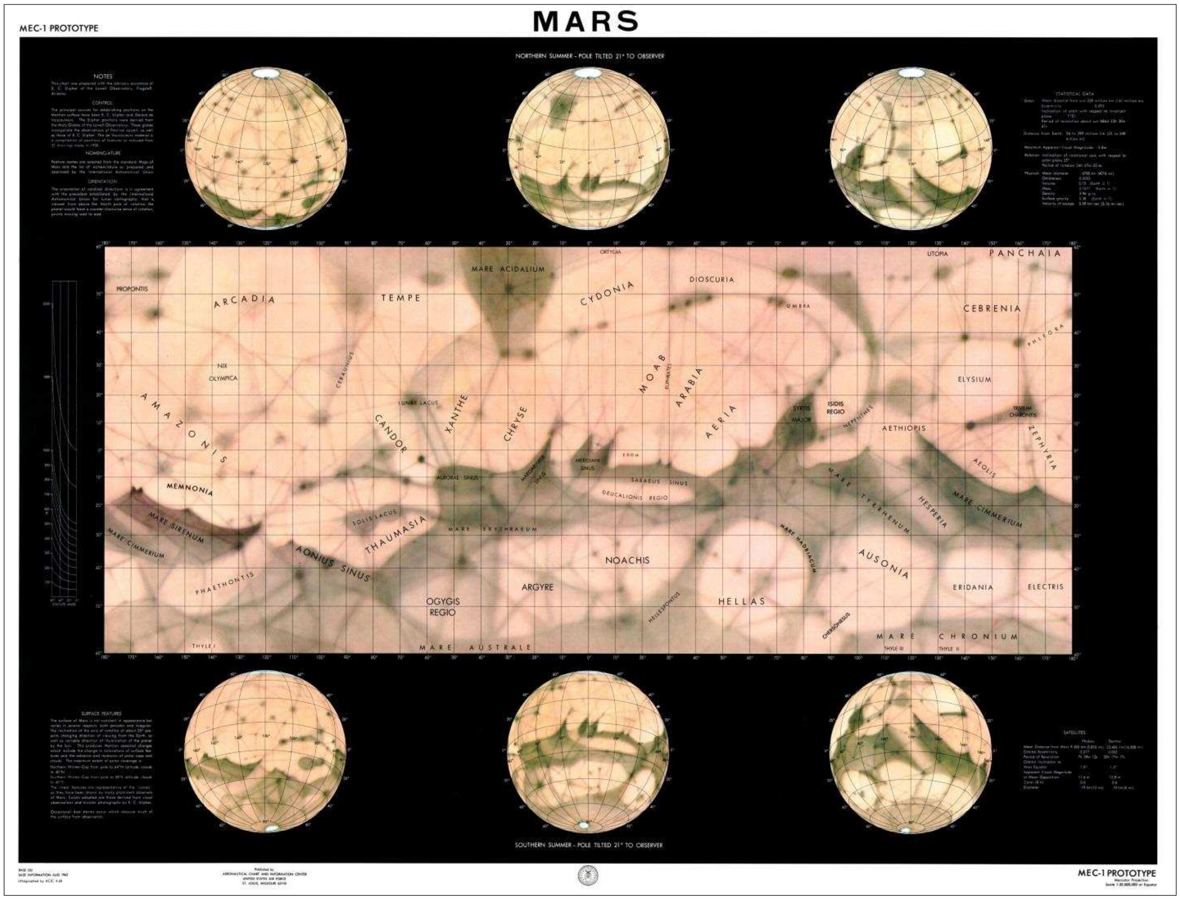

Figure 28: Official US Air Force map of Mars, based on E.C. Slipher's Mars observations, 1958 (courtesy: NASA).

include examples of his photographs of the other planets from Mercury to Saturn. It is the final summing up of a lifetime of work (see E.C. Slipher, 1964).

Of *Mars: The Photographic Story*, one might well say—paraphrasing William Shakespeare—Percival Lowell, thou art mighty yet! E.C. not only published an image of Lowell's globe from 1907 and his canal-strewn map from 1905, he presented a list of bullet points about Mars that follow, in many cases almost word for word, the conclusions Lowell had enumerated in *Mars and Its Canals*. Lowell, in one of his last lectures, had said in 1916:

> I have said enough to show how our knowledge of Mars steadily progresses. Each opposition as it comes round adds something to what we knew before. It adds without subtracting. For since the theory of intelligent life on the planet was first enunciated 21 years ago, every new fact discovered has been found to be accordant with it. Not a single thing has been detected which it does not explain. This is really a remarkable record for a theory. It has, of course, met the fate of any idea, which has both the fortune and the misfortune to be ahead of the times and has risen above it. New facts have but buttressed the old, while every year adds to the number of those who have seen the evidence for themselves. (Lowell, 1916: 427).

Slipher summarizes his conclusions in these words:

> Our knowledge of Mars steadily progresses. Each opposition adds something to what we knew before. Since the theory of life on the planet was first enunciated some fifty years ago, every new fact discovered has been found to be accordant with it. Not a single thing has been detected which it does not explain. Every year adds to the number of those who have seen the evidence for themselves. Thus theory and observations coincide. (Slipher, 1962: 70).

Two years later, on 7 August 1964, E.C. Slipher died. A few months later the American spacecraft *Mariner 4* (Figure 29) set out across interplanetary space on a journey to the Red Planet, made a close sweep by the planet on 14 July 1965 flyby (Figure 30), and sent back a series of television images that demolished the Lowellian Mars once and forever. Just seven years after Director John Hall revived Lowell Observatory and brought that historic institution fully into the late twentieth century, the modern era of Mars studies also began.

Figure 29: Mariner 4 launch, 28 November 1964 (courtesy: NASA).

12 NOTES

1. After finishing his book *Mars* in November 1895, Lowell himself tested conditions at Boghari and Biskara, and even on the northern fringe of the Sahara desert, but did not find that they were better than Flagstaff had been the previous summer. He also kept open the option of moving the observatory to Mexico in order to observe the winter Mars opposition of 1 December 1896.
2. The 40-inch was converted to a 42-inch in 1925.
3. The blink comparator was an indispensable piece of equipment that Lampland had talked Percival into acquiring for the X search years earlier.
4. See the following web site:
https://www.youtube.com/watch?v=dk7lf2D848I

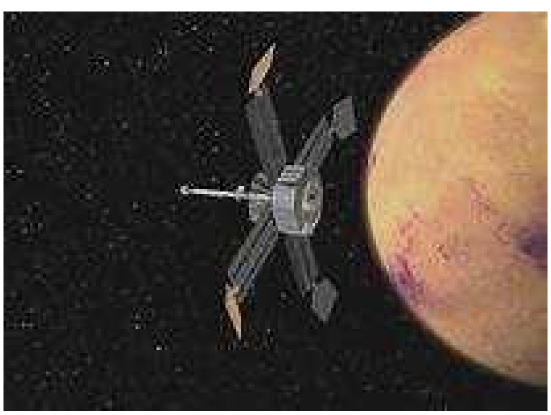

Figure 30: Mariner 4's flyby of Mars, 14 July 1965: the end of an era (courtesy: NASA).

13 ACKNOWLEDGMENTS

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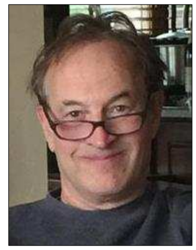

Dr William Sheehan is a retired psychiatrist who researches the history of astronomy, and is based in Flagstaff, Arizona, USA. His books include *Planets and Perception* (1988), *Worlds in the Sky* (1992), *The Immortal Fire Within: the life and times of Edward Emerson Barnard* (1995), *The Planet Mars* (1996), *In Search of Planet Vulcan* (with Richard Baum, 1997), *Epic Moon* (with Thomas A. Dobbins, 2001), *Transits of Venus* (with John E. Westfall, 2004), *Galactic Encounters* (with Christopher Conselice, 2015), *Celestial Shadows* (with John E. Westfall, 2015), *Discovering Pluto* (with Dale P. Cruikshank, 2018), *Jupiter* (with Thomas A. Hockey, 2018), *Mercury* (2018), and *Saturn* (2019). He is a past Fellow of the John Simon Guggenheim Memorial Foundation, a recipient of the Gold Medal of the Oriental Astronomical Association, and a member of the IAU Working Group on Planetary System Nomenclature. The IAU has named asteroid 16037 after him. He is currently working on a new book on Mars with Jim Bell and co-editing (with Robert W. Smith) a multi-authored volume on Neptune.