# THE FIRST 50 YEARS OF PLUTO–CHARON RESEARCH

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This chapter reviews the study of the Pluto system from a historical context, from the time of discovery until the year 1976. Significant contributions from the entire gamut of investigations are reviewed more or less chronolgically. The chronological approach emphasizes the genetic relation between various researchers, and the cross-fertilization which inevitably occurs in any prolonged scientific investigation. The recognized "milestones" in the study of the planet are recounted. However, special efforts have been made to uncover motivations behind individual endeavors, to recount unsuccessful or unpublished studies, and to emphasize the symbiotic nature of relationships between Pluto study and attempts to answer other astronomical questions of the day.

## I. INTRODUCTION

The flurry of excitement caused by the discovery of a ninth planet has been reviewed in the chapter by Reaves. The first order of business following the announcement of Pluto by the staff of Lowell Observatory was to seek out pre-discovery plates of the object, and to refine its orbit.

The cast of characters involved in the early photographic and orbit work reads like a *Who's Who* of twentieth century astronomy: H. N. Russell, S. B. Nicholson, N. U. Mayall, V. M. Slipher, H. Shapley, A. C. D. Crommelin, G. Struve, W. Baade, M. Humason, and G. Van Biesbroeck, to name only a few of the more well-known, but less-remembered, contributors. It is an interesting historical note, however, that a disproportionate number of the people conducting Pluto research from the 1930's through the present were graduate students and postdoctoral researchers. For some reason, Pluto traditionally has attracted more than its fair share of the junior component of the astronomical community. Most move on to other branches of astronomy, clearly demonstrating that not everyone is up to the task of Pluto research.

The discovery of Pluto came at 4 p.m. on February 18. It was officially announced on 1930 March 13, at midnight Eastern Standard Time, in the form of a telegram from V. M. Slipher to the Harvard College Observatory. In the interim, Slipher had taken only a few people into confidence. Among them was the celestial mechanician J. A. Miller, then Director of Swarthmore's Sproul Observatory and a former professor of Slipher's at Indiana. Miller was invited to Lowell (Slipher 1930) to supervise the preliminary orbit computation. Miller arrived in Flagstaff on March 22. By April 12, the Lowell computers announced their results. The Lowell orbit was neither very accurate (e.g., derived semiaxis major and eccentricity 216.721 AU and 0.909, respectively), nor was it the first. Table I presents the six earliest orbit determinations.

By August 1930, the orbital elements had become pretty well-established, after the inclusion of prediscovery positions dating back to 1915. In his review of that month, F. C. Leonard (1930) illustrated an orbit derived by a young graduate student at Berkeley named Fred Whipple. As Leonard summarized,

> "The following facts concerning the orbit of Pluto are taken from the results of the closely concordant investigations at the Students' Observatory, Berkeley, by Mr. E. C. Bower and Mr. F. L. Whipple, under the supervision of Director A. O. Leuschner and (independently and almost simultaneously) at the Mount Wilson Observatory by Dr. Seth B. Nicholson and Mr. N. U. Mayall."

FIGURE 1a,b HERE (full page)

Whipple's orbit, contrary to many of the orbits announced in the immediate post-discovery melee, is strikingly accurate (Fig. 1). The semiaxis major, inclination, and eccentricity all are consistent with modern values, and even the time of perihelion—1989—is in agreement.

Few were as persistent in their orbital calculations as Bower (1931*a*) who, in addition to writing a Ph.D. thesis, authored at least 11 other works on the orbit of Pluto. With Whipple, Bower took some 17 photographic plates at the Students' Observatory to aid in their orbit calculations. Bower's orbits are numbered up to XIX, no mean feat in the days when calculators were human. Bower endeavored to determine Pluto's mass through study of its orbital interactions with Uranus and Neptune, and deduced an upper limit of 0.7 $M_\oplus$. With the orbit—and therefore ephemeris—of Pluto thus constrained, earnest study of the planet as an astrophysical object could begin. These early efforts are described, after a brief diversion to consider some of the preliminary orbit work done in the spring of 1930.

## II. FACT AND FOLKLORE IN POLAND

In addition to the six orbits listed in Table 1, there is a putative seventh early orbit which is of considerable interest, supposedly calculated by Thadeusz Banachiewicz of the Craków Observatory. Wiesław Wiśniewski (1993, personal communication) initially suspected it might lay claim to being the first one calculated. Considerable folklore in the literature exists regarding the resourcefulness of this orbit and its timing relative to the others (see, e.g. Levy 1991, p. 73; Hoyt 1980. p. 206). Witkowsi (1955) claims, "Having in vain asked Flagstaff for recent observations of the transneptunian object, Banachiewicz helped himself about by using a Flagstaff photograph of Pluto published in the *Illustrated London News*." The only way to try to resolve the ambiguity is to go back to the original sources.

The newspaper article (Crommelin 1930*a*) was published on April 5. Therefore, Banachiewicz could not have calculated his orbit prior to this date. A radiogram was sent from Banachiewicz to the Harvard College Observatory on April 7 (Shapley 1930, dated April 8) which includes a set of circular elements matching those in Banachiewicz (1930). The latter article is signed April 08, and in it are 3 mean positions. The first one is an average of two from Yerkes (Copenhagen Cir. No. 265); the second is an average of one each from Heidelberg, Neubabelsberg, and Bergedorf (Wolf 1930); the third is an average of one each from Pulkowo and Neubabelsberg (Wolf 1930). Therefore, Banachiewicz had his requisite 3 reliable observations by April 07, and would not have needed to resort to so crude a technique as "newspaper astrometry" after that date. If Banachiewicz used the newspaper position for his first circular orbit (which remains unpublished), the orbit could have been calculated no earlier than April 6. First conclusion: the Whipple and Bower orbit was indeed the first.

What does Banachiewicz have to say regarding the matter? A paper signed on April 22 (Banachiewicz and Smiley 1930) begins:

> "At the time of the determination of the Transneptunian body was undertaken, 37 observations from 1930 March 2 to April 4 were available, not all of which were complete... It was possible to use also the incomplete observations, namely the position of the body, without the time of the observation, determined from the *Illustrated London News* reproduction of one of the plates of the Lowell Observatory, as given in *Nature* No. 3154, pg. 577 [Crommelin 1930*b*], and the apparent position for March 12.14 (*Lowell Observatory Observation Circular* dated March 13, 1930), with the right ascension given only approximately."

The 35 "complete" observations follow, beginning with March 16.065. The 2 "incomplete" observations were employed because they extended Pluto's arc backward by an additional four days.

I assert that Banachiewicz' acknowledgment of the April 12 *Nature* article is strong circumstantial evidence for one of two things. Most probably, Banachiewicz never personally measured the photograph in the *Illustrated London News*. It is less likely the following sequence occurred: Banachiewicz measured the photograph to get a third position. Then, guessing at the time the plate was exposed, he determined a 3-point circular orbit on April 6. Finally, something occurred between April 6 and 22 which gave him reason to believe the *Nature* coordinates were more accurate than his own. This second path seems much less tenable.

True, Banachiewicz did incorporate the Pluto position from the *Illustrated London News* into a circular solution for Pluto's orbit. However, he never did publish a *3-position, circular orbit* using this datum. In the April 8 paper, he states, "Determinatione de orbita presenta punctos dificile, causa exiguo mutatione de positione." This reflects his doubts regarding the robustness of a 3-point solution along a short arc which used only "complete" data. Banachiewicz was universally regarded as being a very resourceful and careful investigator. It seems rather out of character to attempt a solution with less than the minimum required data. Conversely, it seems rather typical of his resourcefulness to include *those components* of an incomplete datum which might enhance an already overdetermined solution, and thereby obtain maximum accuracy. Second conclusion: the Banachiewicz orbit listed in Table 1 was his first.

## III. EARLY RESEARCH

Research on Pluto as an astrophysical object began immediately following its February 18 discovery. Although the sky was overcast that night, an additional position was obtained on February 19, in spite of intermittent clouds. Over the next few weeks, C. O. Lampland (1930) used the Lowell 42-in reflector whenever possible to obtain additional positions. Additionally, a pair of deep, one-hour exposures were made specifically to search for satellites (Tombaugh 1960). Discovery of a satellite would have been doubly useful. It would bolster the claim that Pluto was indeed a full-fledged planet, and would yield an immediate mass estimate via Kepler's Third Law. Lampland's plates (one blue, one yellow) reached a limiting magnitude of about 19. Charon was not detected.

For the remainder of the discovery apparition, E. C. Slipher concentrated on trying to determine Pluto's diameter, with an experiment which may be considered the "precursor" to Kuiper's diskmeter work. Tombaugh (1961) described these efforts:

> "E. C. Slipher continued to examine the new planet visually with the 24-inch refractor on nights of very steady seeing, but he could not detect a disk. He set up a test target with illuminated graduated apertures about 1 mile away and observed them with the 24-inch refractor. As a result of these comparisons, he concluded that the new planet, under such faint illumination, could have a disk nearly $0''.5$ in diameter and not be detected by the best observations.

As summarized in Leuschner (1932), there were other early attempts to discern a visible disk for Pluto, all of them unsuccessful. In particular, R. G. Aitken and H. Wright of the Lick Observatory (Bower 1931*b*) report Pluto's apparent diameter at $\leq 0.3''$, equivalent to an upper limit of 8730 km, or 0.68 $d_\oplus$.

Nicholson and Mayall (1930) were among the first to furnish post-discovery mass estimates for Pluto, based on its supposed perturbations on Neptune. In November they reported a value of $1.08 \pm 0.23$ $M_\oplus$ when Lalande's 1795 observations were used. However, in January of 1931 they warned (Nicholson and Mayall 1931) that without the Lalande data, "Values of the mass $m$ ranging from 0 to 1.5 represent the modern observations almost equally well. . . For values of $m$ between 0.9 and 1.1, the representation of all observations remains practically unchanged."

Upon learning about Lick's upper limit to the diameter, and the Earth-like mass estimates, Pickering (1931*a*) wrote:

> "The mass has not yet been accurately determined, but if the above value is nearly correct, the density is evidently considerable, and may be even greater than that of platinum, suggesting in that case that Pluto may turn out to be a piece of a star approaching the white dwarf type."

Nicholson and Mayall (1931) reported a blue minus yellow color index: "Preliminary observations with a yellow color-screen indicate that the color index is at least one magnitude." These observations were made at the Mt. Wilson 60-inch telescope. V. M. Slipher (1933) reported slightly less vague colorimetry:

> "Pluto is too faint for study of its spectrum with our equipment at Flagstaff. Instead, we early sought to learn something of the colour of its light by comparing it and near-by stars visually at the 24-inch refractor. . . It soon appeared evident that Pluto was brighter visually than photographically. Then Dr. Lampland made exposures to the same end, with the 42-inch reflector, through an orange filter on panchromatic plates and on blue plates without filter, for a check. . . Thus Pluto's light in the yellow and red is apparently somewhat stronger than normal sunlight, and so he is a yellowish planet, possibly standing between Mercury and Mars in color. Higher precision is attainable in these colour tests, and these observations are being repeated with that objective in view.
>
> It is quite beyond the spectrograph to deal with the diurnal rotation of Pluto. But this is not quite hopeless of detection by photometric study if the reflectivity from different faces of his disc differ enough to give an appreciable light variation, and the matter is having attention at the Lowell Observatory."

Whether or not the matter had the attention of the Lowell staff, none of this light curve work, nor the "higher precision" colorimetry, ever was published. The first serious attempt at photographic photometry of Pluto was by Baade (1934). Realizing the inherent inaccuracies of what he was attempting to do, Baade took photographic plates of Pluto near the two stationary points during the 1933 apparition. In this way he was able to avoid frequent changes of the comparison stars. Eight plates of 5-min exposure were obtained at the Mt. Wilson 60-inch (stopped down to an aperture of 40 inches) on 19–20 March 1933. In October–November, another dozen were obtained, five at the 100-inch and seven at the 60-inch. The Baade data are plotted as Fig. 2.

FIGURE 2

Baade recognized the hopelessness of trying to extract from his data any information regarding rotational modulation, and combined them to derive a mean photographic magnitude of $m_{pg} = 15.58 \pm 0.02$. Reduced to mean opposition, this corresponds to $m_{opp} = 15.41 \pm 0.02$. All Baade could say about the *amplitude* of the light curve was that it *could* have been as much as 0.2 mag.

Additionally, Baade obtained four photovisual measures of Pluto to determine the planet's color index. His result, $m_{pg} - m_{pv} = 0.67 \pm 0.02$, corresponds to a star of spectral class G4—clearly redder than the Sun. The photovisual magnitude, 14.91, was in good agreement with visual observations made in Germany by K. Graff (1930), which yielded a mean magnitude of 14.88. Other early magnitude estimates for Pluto are summarized in Andersson (1974, Table IX).

No doubt disappointed by his results, Baade would have been pleased to know that his photometric data would prove useful half a century later. For although no light curve could be extracted, the data provided a valuable constraint to the static "spot models" of Pluto generated in the 1980's (cf. Marcialis 1983,1988; Buie and Tholen 1989). The amplitude of Pluto's light curve, as predicted by the spot models, is approximately that of the vertical bar in Fig. 2. It is due to Baade's data that the idea of polar caps, or at least a higher-than-mean polar albedo distribution, came to be considered seriously.

Henry Norris Russell contributed a monthly column to *Scientific American* magazine from 1927–1943. The columns for 1930 December and 1931 February were about the new planet (Russell 1930,1931), and in the latter Russell notes:

> "Meanwhile we may note that the new period of 247.69 years, which should be much nearer the truth than any previous value, is also much nearer to being 1½ times Neptune's period of 164.77 years. After three revolutions of Neptune and two of Pluto, the former is only one years' motion ahead . . . "

Russell was tantalizingly close to putting his finger on the Neptune–Pluto resonance discovered 33 years later (Cohen and Hubbard 1964). However, realizing the formal errors in the known periods of the two planets, he did not commit to the final step of suggesting its possibility and analyzing the consequences. At the time, the word "resonance" was not in common usage, and it is difficult to ascertain whether Russell truly believed the near-comensurability to be anything more than coincidence. Actually, Russell's suggestion was predated by similar speculation in Europe by Wilkens (see Mineur 1930). Besides the approximate 3:2 commensurability, Wilkens also noted the lines of apsides for Neptune and Pluto were nearly opposite one another, approximately $178°45'$ apart. Wrote Mineur, "Wilkens remarque que ces deux conditions. . . son parmi celles auxquelles on est conduit lorsqu'on cherche une solution périodique du problème de trois corps. Le système Soleil, Neptune, Pluton constituent-il une telle solution de ce problème?"

Yamamoto (1934) published a short paper speculating on the origin of Pluto. After noting several common characteristics of Pluto and Triton (e.g., $V = 14.9$ at Pluto's distance, angular diameters both $<0.3''$, masses $\lesssim 0.1\mathrm{M}_{\oplus}$), Yamamoto concluded the two had a common origin as satellites of Neptune. Yamamoto, then-Director of the Krasiki Observatory, ends his paper:

> "...it is highly probable that in some remote time a massive body (perhaps a fixed-star) made a peripheral encounter with the solar system; in its descending (or ascending) node where it approached Neptune's system with at least two satellites; consequently Neptune's mean orbit was considerably reduced, one of the satellites of the system becoming an independent member around the sun. The retrograde motion of Triton (the only satellite of Neptune at present) might be a still more effect [*sic*] of the encounter. The fact that Neptune's system was so considerably affected and yet the other planetary systems were practically unaffected by this encounter is not incomprehensible from some possible configurations of planetary bodies at that instant."

Unfortunately, due to Japan's activities external to the astronomical arena at about the same time, and also owing to its publication as an Observatory Circular rather than a "mainstream" journal article, the work slipped *almost* unnoticed, into obscurity.

Two years later, R. A. Lyttleton, then at the Princeton Observatory, published a paper (1936) entitled, "On the possible results of an encounter of Pluto with the Neptunian system." This work is one of the most widely-cited yet most misunderstood pieces of Pluto research ever to go to press. Lyttleton, too, realized that although the orbits of Neptune and Pluto do not intersect at present, precession of the nodes and apsides due to perturbations by the other planets implied that at some time in the distant future or distant past, the two bodies could suffer a close encounter. The main purpose of this paper was to consider the results of such an encounter on Neptune and Pluto, and to place upper limits on how much mutual scattering might possibly occur.

Lyttleton showed that Neptune could not scatter Pluto sunward with a perihelion of less than 7.3 AU. Thus the Earth was "safe" from Pluto, unless further inward scattering by Saturn were to occur, a contingency he considered "...of little interest on account of its extreme unlikelihood." If instead the scattering were outward, Pluto's aphelion could not exceed 598 AU. "Hence at no time may Pluto be ejected from the system and in all probability is therefore an original member."

Next considered were future interactions between Pluto and Triton. Six possible interactions were enumerated and evaluated in turn. At the end of the paper a seventh, admittedly improbable, possibility was admitted. For the sake of completeness, Lyttleton states that through just the right gravitational interaction with Triton, Pluto could be captured into a bound orbit around Neptune, in the process reversing Triton's orbit from retrograde to prograde. As the orbital equations hold when the direction of time is reversed, he realized this situation to be equivalent to one where Pluto might be an escaped direct satellite of Neptune.

On top of the already improbable geometric constraints on this probability, Lyttleton realized, "...if Pluto's mass exceeds that of Triton the result is more readily accomplished." Until about a decade ago, this was by no means a certainty and only served to make the house of cards one story higher. Very few members of the astronomical community at large accepted the "escaped satellite" theory at the time (Tombaugh, 1989, personal communication). Based upon the words used in his Summary, Lyttleton himself thought the idea rather unlikely:

> "(iii) If Pluto should encounter Triton several interesting cases arise, but these are of slight interest owing to their improbability.
> (iv) Nevertheless it appears from these considerations that Pluto may originally have been a direct satellite of Neptune, ...thus giving a second, though rather speculative, possible explanation of the retrograde motion of a true satellite."

A mere four years later, in a popular article appearing in *The Sky*, Lyttleton (1940) appears to have convinced himself absolutely of the incontravertibility of his "proof" pinpointing Pluto's origin.

Lyttleton gave absolutely no acknowledgment of Yamamoto's earlier contribution in either of his papers. However, from the structural organization of the first paper, I contend that he was aware of the earlier work. First, the escaped satellite of Neptune idea is appended to the end of the paper, almost as an afterthought or addendum might be. Second, rather than receiving the option number (vii) as one would expect, it receives a separate section number of 5. Why would such a well-organized paper, where even the concluding remarks are enumerated in outline form, depart from such regularity? Third, although the work is mathematically rigorous, the equations mysteriously disappear during consideration of this final option. Fourth, Yamamoto's paper had achieved distribution throughout the United States well in advance of Lyttleton's submittal date. (Lowell Observatory's copy is stamped as received on 1935 August 02; Lyttleton's paper is dated 1936 October 30.) It is unknown whether the fifth section was added due to a suggestion by Russell, by an unknown referee, or on Lyttleton's own initiative, but it does seem probable in light of the evidence that the escaped satellite idea belongs not to Lyttleton, but rather to Yamamoto. The only new "twist" which Lyttleton contributed to the problem was to eliminate the need for an interloper (Yamamoto's "fixed star") in the dynamics.

All of the above doubts and disclaimers notwithstanding, the "escaped satellite" idea is perpetuated in textbooks published as recently as 1994 (cf. Consolmagno and Schaefer), and the validity of such an improbable occurrence is emphasized over much more plausible theories of origin in almost every beginning astronomy class taught today!

Perhaps one of the reasons is that Kuiper (1957) revived the escaped satellite idea, although for entirely different reasons. The abstract of his paper reads:

> An earlier hypothesis [Kuiper 1956, p. 172], that Pluto originated as a satellite of Neptune which was released during the stage of protoplanet evaporation, is confirmed by a study of the Jacobi constant of Pluto in the system sun–Neptune. The present value of the constant implies that the evaporation of the interior major protoplanets was not complete at the time of Pluto's release."

Kuiper had earlier shown that Pluto could escape from a previously bound orbit around proto-Neptune if the gas giant's mass were reduced by a factor of ~40 as the proto-solar nebula dissipated. However, he realized that the proto-Sun, as well as all the planets interior to Neptune would have lost mass as well. (For purposes of this treatment, the planets could be thrown into the Sun.) Kuiper put the total solar system mass loss at about 0.05–0.06 $M_\odot$.

In a letter to *Science*, W. J. Luyten (1956) publicly accused Kuiper of plagiarizing Lyttleton, but it is clear that Kuiper's line of reasoning was totally independent, part of a much broader theory of planetary formation. An escaping Pluto was a consequence of what Kuiper thought to be a natural process, not an afterthought by Lyttleton which invoked special *ad hoc* geometry. Of course, the escaped satellite idea was effectively put to rest by the discovery of Pluto's satellite Charon (see Lin 1981). In Kuiper's (1961) own words, "If the hypothesis that Pluto originated as a satellite of Neptune is correct, this body should have no satellite of its own."

Devising a plausible hypothesis for the origin of Pluto, while interesting, was (and perhaps remains today) an extremely underconstrained problem due to the lack of knowledge of the planet. Such hypotheses are of little value unless they can be tested, and ignorance of Pluto's two most fundamental parameters—its mass and radius—continued to be gaping holes in the overall picture. By 1935, the number of papers about Pluto had relaxed to the "background" level of about 12 per year (see Appendix I). The great majority dealt with either astrometric positions of the planet, or interpretations of these positions in the form of orbit theories. It was hoped that improved knowledge of the orbits of Pluto and the outer 3 Jovian planets eventually would reveal an unambiguous gravitational signature. Most of the observatories reporting positions of the planet had relegated Pluto to the status of asteroid in their photographic observing programs. The task generally was viewed as a "long-term" project, more for posterity than for the observers' own immediate benefit. Of particular note, Tombaugh (1961) reported, “Lampland started a long series of short-exposure plates of Pluto with the 42-inch [Lowell] reflector for positions. He secured a plate in every possible month until his death in December, 1951.”

## IV. ONTO THE BACK BURNER

At least for the short term, it proved more fruitful to search archival plates for prediscovery sightings than to extend incrementally the time base with new observations. Practically every major observatory in the world contributed to this process. By 1 June 1930, the total number of prediscovery sightings reached 136. The earliest seems to be from Heidelberg Observatory on a plate exposed by M. Wolf, dated 23 January 1914. A reported position from 1903 on a plate exposed in South Africa later was shown to be spurious. According to Leuschner (1932), the search is essentially complete back to 1890. An extensive list of photographic positions, from prediscovery through the 1965 apparition, while incomplete, is given in Cohen et al. (1967). A more complete list can be compiled from sources listed in the PLUBIB database (see Appendix I).

Unfortunately, investigations other than astrometric which yielded marginal or negative results prove much more difficult to exhume, as they were rarely reported in the mainstream journals of the day. As an example, the first known spectrogram of Pluto was obtained almost immediately after the discovery announcement. In a letter dated 1930 May 29, Walter Adams (then-Director of Mt. Wilson Observatory), noted in passing to Slipher that M. Humason had obtained a low-dispersion spectrogram. Humason visited Lowell Observatory on May 30, spectrogram in hand. Slipher wrote to J. A. Miller the following day that, “Their spectrogram. . . shows a solar type spectrum.” (Hoyt 1980). None of the details of Humason’s work was ever published.

Humason’s efforts continued at some level for over a decade, although lack of documentation in the published literature makes it difficult to assess the frequency or intensity of his work. Adams (1941) briefly reports in the *Annual Report of the Mt. Wilson Observatory:*

> “A spectrogram of Pluto was obtained by Humason on the night of March 23, 1941 at Mt. Wilson, with the 2-prism spectrograph and 3-inch Schmidt camera showed a typical solar-type spectrum. In the region between $\lambda3700$ and $\lambda5000$ no observable differences from the solar spectrum could be detected with the dispersion employed (230Å per millimeter at $H\gamma$).”

Tombaugh (1980) makes a passing reference to the Mt. Wilson spectrum work, but claims the data were obtained at the 100-inch telescope. Presumably he was referring to Humason’s earlier result. To date, none of these spectrograms have reached the literature.

At Mt. Wilson, R. S. Richardson (1942) realized that Pluto and comet

P/Halley would be making their closest approach to one another in about 1,000 years in April of 1942. Their minimum separation was 14 AU, of which about 5 AU was displacement normal to the ecliptic. Halley was nearly at aphelion from 1940–1960, while Pluto's ecliptic longitude matched that of the Comet in 1940. Perhaps if Pluto induced noticeable perturbations on the orbit of Halley, reasoned Richardson, the mass problem might finally be resolved. Assuming a Pluto mass equal to that of Earth (best estimates at the time placed it at 0.5–0.7 $M_\oplus$), calculations showed that Halley's time of perihelion passage was unaltered to within a "small fraction of a day." This was tiny compared to the 2.7-day discrepancy during its previous perihelion passage, and therefore probably undetectable. Likewise, the longitude of Halley's ascending node was altered by only one arc minute. After considering all the other sources of perturbations on the orbit, Richardson concluded, "...it is doubtful if the planet's influence can be detected."

Richardson's technique was by no means a new idea. It resurfaces many times throughout history as a means of detecting "new" planets, at least once per generation. Pickering (1928,1931*b*) had tried it. The first known suggestion of the idea, in fact, was advanced by the Frenchman A. C. Clairaut (1758), fully 23 years before the discovery of Uranus! More recent incarnations of the idea appear in Schütte (1950), Öpik (1971), Brady (1972), and Guliev (1992). None of these attempts met with the slightest success.

Concurrent with Richardson's work, L. R. Wylie of the USNO (1941,1942) analyzed all known astrometric positions of Neptune (including Lalande's 1795 observations) in order to improve the mass estimate of Pluto via its perturbations. His result (Sun/Pluto = 360,000, or $0.91M_\oplus$), stood through World War II and was not challenged for a decade. Eckert et al. (1951) incorporated this value of the mass, and the 1795 Lalande observations of Neptune (Mauvais 1848), into their next revision. The result was not very satisfactory, as the predicted longitude of Neptune very quickly diverged from new observations. Brouwer's (1955) analysis of Uranus observations showed the presence of a systematic trend in its latitude residuals could not be removed by changes to either Pluto's orbit or its assumed mass. It therefore became clear that a *simultaneous* solution for the five outer planets was the proper course to navigate. This rather unwieldy analytical problem rapidly precipitated the era of numerical techniques in ephemeris determination in the latter half of the 1950s.

FIGURE 3

Figure 3 is a compilation of the various estimates of Pluto's mass, plotted as a function of time. While the sources are too numerous to cite, all data were culled from the literature in the PLUBIB compilation (Appendix I).

## VI. SECOND-GENERATION RESEARCH

Gerard Kuiper's decade-long investigation of Pluto began in 1947–1948. As was typical of his approach to scientific problems, his attack was multifaceted. Using the McDonald Observatory 82″, he obtained (Kuiper 1949, Plate XIII) a low-dispersion spectrum covering the wavelength region $\lambda\lambda$ 3300-8800Å. The spectrum was intended to search for a possible atmosphere on Pluto, and was part of an overall atmospheric detection program for the Galilean satellites, Titan, Rhea, and Triton. Kuiper wrote (p. 369), "No evidence of a $CH_4$ atmosphere is found from these data, or from better spectra obtained more recently."

These "better spectra" remain unpublished. The only positive detection of an atmospheric absorption was for Titan, where methane was identified (Kuiper 1944). Dunham (1949) mentions an unsuccessful attempt to obtain a photographic spectrum of Pluto at Mt. Wilson with a 3-inch $f/1.0$ Schmidt camera, but cited problems in guiding during the exposure.

Kuiper recognized that inert species, such as argon, might be present in the atmospheres Triton and Pluto in spectrally undetectable amounts. However, their presence might be betrayed by an excess of flux near 3000Å, the short-end atmospheric cutoff, owing to $\lambda^{-4}$ Rayleigh scattering. The absence of such an ultraviolet excess led Kuiper to put a limit of 0.1 bar total pressure for Pluto's atmosphere. However, this upper limit was not very satisfying. Kuiper (1949) wrote, "...Spectrophotometry on small [point source] spectra is not very accurate, and steps are being taken to repeat this work photoelectrically." Unfortunately, successful photoelectric spectrophotometry of Pluto was not to be obtained for nearly two and a half decades (Fix et al. 1970).

Better known than Kuiper's spectroscopic work on Pluto are his visual attempts at determining the planet's diameter (Kuiper 1950):

> "On repeated occasions, during 1948 and 1949, the writer has attempted to measure the diameter of Pluto with the 82-inch telescope. A disk meter, designed to produce a small artificial luminous disk of controllable brightness, color, and diameter, was placed at the Cassegrain focus. With the aid of small diaphragms the F ratio of the lens forming the artificial image is always made equal to the F ratio of the main telescope. Diffraction effects in the two images to be compared are then identical so that they are eliminated in the comparison. The limits of the instrument are set, of course, by the size of the Airy disk itself, 0″.06 for the 82-inch telescope; and by obvious intensity requirements (very faint disks, smaller than about 0″.5, look like stars).
>
> Owing to the faintness of Pluto no reliable results were obtained, in spite of very considerable efforts. The nearest approach to an actual measurement was made on November 4–5, 1949, when seeing was 9 on a scale of 10 for several hours and the mirror was essentially free from distortions. The writer then derived 0″.4 for the diameter, and A. Shatzel, who was called in because of the exceptional conditions, got the same value, independently. But it was obvious that the measurement was on the very threshold of the capabilities of the

> telescope (if not beyond it), even with the exceptional conditions prevailing on that night. It was concluded that further efforts would be futile and that the observation could be successful only with the 200-inch telescope—which would give six times the intensity."

At the meeting of the American Astronomical Society (AAS) in December 1949, Kuiper gave an oral presentation of his 82-inch diskmeter results. During the meeting, he had consulted with W. Baade regarding the feasibility of adapting the diskmeter for use on the 200-in. Thoroughly convinced it would yield a more definitive result, publication of the abstract was withheld.

The diskmeter actually was invented by Camichel (1944), who used it quite successfully to measure the diameters of the Galilean satellites and Titan, obviously much less taxing observations in all respects. Kuiper often is erroneously attributed with its invention. In order for his device to fit inside the prime focus cage of the Hale telescope, the optics had to be folded. I suggest that it was this need for additional redesign and machining which caused those not involved with the project to attribute invention of the device to Kuiper.

With the aid of M. Humason, the diskmeter was fitted at the prime focus of the $200''$ on March 14. An $f/3.6$ Ross corrector, which Humason used in his photographic program, was left in the optical train. On March 22, 04:00 UT, Pluto was observed at an airmass of 1.07. Kuiper and Humason took turns measuring the diameters of Pluto and a nearby 11th-magnitude star. The values they got were the same, and were $0''.23 \pm 0''.01$ for Pluto, and $0''.11$ for the star (presumably with comparable uncertainty). The derived diameter for Pluto ($0.46\ d_{\oplus}$) was intermediate between those of Mercury and Mars. At this point two unresolved issues, not previously addressed in the literature, deserve some attention.

First is the effect of the Ross corrector. "It is at present uncertain whether the uncorrected value, $0''.23 \pm 0''.01$, should be used;" writes Kuiper, "or whether the maximum correction found for the presence of the corrector lens should be applied, making Pluto's observed diameter $0''.20 \pm 0''.01$." These words show the corrector was known to degrade the image by about $0.03''$, a small but noticeable effect. Unaddressed by Kuiper is that the apparent diameter of the star, too, should have been affected in a similar manner. In the end, Kuiper chose to adopt the larger value, perhaps because everyone "knew" Pluto was earth-like in mass and density.

What is not apparent, however, is why Kuiper did not assume the star's image to be the impulse response of the system, and Pluto's "measured" diameter to be the convolution of this impulse response with the planet's actual angular diameter. Had Kuiper done this, he would have obtained Pluto's angular diameter as $0.23'' - 0.11'' = 0.12'' \pm 0.02''$ (3,100 km), *independent* of the effect of the corrector. To my mind, the only acceptable "explanation" of this apparent error, however unlikely, is that Kuiper actually believed he was resolving the star's disk. (The diffraction limit of a 5-meter telescope is

$0.029''$.)

A second point which should be discussed is the effect of Charon. We now know that at the time of the observation, Charon lay at a separation of $0.67''$ to the northeast of Pluto. The Hale telescope gave an intensity advantage over McDonald of $(200/82)^2 \approx 6$ for point sources. Therefore, Charon should have been both resolved and bright enough to see had the angular sensitivity of the diskmeter experiment been anything close to what Kuiper claimed.

FIGURE 4 (full page, several panels)

In order to investigate the effect, if any, of Charon, Marcialis and Merline (1996) recently began a series of simulations of the diskmeter experiment. The "realistic CCD model" of Merline and Howell (1995) was modified to address the problem.

Figure 4 summarizes our preliminary results. A single star (left column) and a binary (right) corresponding to Pluto and Charon's apparent separation, position angle, and apparent magnitudes are shown. The seeing has been progressively degraded, in increments of $0.2''$, from $0.2''$ in the top row to $1.2''$ in the bottom. As expected, Charon cannot be clearly resolved when the seeing is $\gtrsim 0.6''$.

However, examination of the sequence reveals something new. For seeing $0.6''$ or worse, the Pluto–Charon binary in row $n$ is easily confused with a single source at a seeing value intermediate between rows $n$ and $n+1$. That is, Pluto–Charon appears to the eye as a single source $\sim 0.1''$ wider than the actual seeing. To test this conjecture, a double-blind experiment was performed. Under the assumption of a radially-symmetric point-spread function (i.e., a single source), FWHM widths of the simulated images were determined using the IRAF reduction package. Indeed, the extracted widths were $0.11''$ to $0.05''$ wider for Pluto–Charon than for the star at the same seeing.

This result explains why Kuiper and Humason measured a diameter for Pluto of $0.23''$, but only $0.11''$ for the nearby 11th magnitude star. It also explains why each observer got the same answer for Pluto, even though it was incorrect. (At the time, Pluto's diameter was $0.045''$; Charon's was $0.025''$.) No longer is it necessary to invoke systematic changes in seeing during the interval between Pluto and star measurements. We conclude that, although Charon was not resolved, it did indeed have an unintended, but understandable, effect on Kuiper's measurement. Complete details of this simulation will be presented in a later paper.

Regardless of the sensitivity and interpretation of the diskmeter experiment, the result as published had significant impact on the veracity of the estimates for Pluto's mass and density. Wrote Kuiper (1950): "With the diameter of Pluto 0.46 times the earth's, the volume is 0.10 earth and the probable mass slightly below 0.10 earth. If the mass derived from the Neptune observations were correct, the density of Pluto would be nearly ten times the earth or 50 cgs. This does not seem physically possible. The mass determination is therefore not regarded as real..."

About 1950 Kuiper had obtained a photoelectric determination of Pluto's $B-V$ color index which confirmed Baade's earlier (solar) value of 0.67. He took this as evidence for a frost-covered surface:

> "The albedo may be computed from Baade's value of the photovisual magnitude...The resulting albedo is 0.17...
> Such a body must have some atmosphere, though most of its original atmosphere will have frozen out owing to the low equilibrium temperature for Pluto, 40°-50°K. Both the atmosphere and the condensation products will prevent the albedo from being extremely low: nearly all snows are white, the crystals being small ($H_2O, CO_2, CH_4$, *etc.*). On the other hand, the albedo need not be that of freshly fallen snow, 0.7-0.8, because of several effects, including grit deposited by comets and meteors, will darken snows over the ages. However, the rocky surface of Pluto would be expected to be invisible, which may explain why its color index is only slightly different from the sun, quite contrary to the results for Mercury, Mars, and the moon. For Pluto...is consistent with a gritty snow surface.

This view of Pluto has proven to be way ahead of its time. Nearly three decades elapsed before the community as a whole embraced it, for the old, earth-like mass estimates proved a difficult tenet to renounce. But the discrepancy was apparent; at least indirectly Kuiper's work spawned several creative experiments over the next 15 years. These included another satellite search, early attempts to observe stellar occultations, reanalysis of some 700 Lowell astrometric plates and, on the theoretical side, new determinations for the masses of the outer planets.

There were a few notable attempts proffered to reconcile the large (Earth-like) mass estimates of Pluto with the small diameter determination. In the diskmeter paper, Kuiper summarily dismissed the possibility that Pluto might be a white dwarf: "Since for a mass smaller than the earth, degeneracy cannot set in..." Dinsmore Alter (1952), who taught Tombaugh during the latter's attendance at the University of Kansas, recounted an idea proposed about twenty years earlier:

> "There is, however, another explanation. It started as a suggestion by Sir James Jeans, quoted by Crommelin, in 1934, that perhaps the surface of Pluto reflects light more or less as does a mirror. Perhaps that we could place a big, polished ball, perhaps far larger than our own earth, out at the distance of Pluto. We would, from the earth, observe the sunlight which was reflected by the ball as coming only from a small area near the centre of its surface. No matter how carefully we might measure the diameter it would be far less than the diameter of the ball, since we would receive no light at all from the edge of the ball.
> Let us now suppose that we had a similar ball out there, except that it was covered with patches, both of polished and of rough surface.

> From the polished parts we could receive light only from patches close to the centre of the surface, but these rough patches would reflect some light even from the edge. If the rough patches were big enough, we would measure the true diameter of the ball from the earth. If too small, we would not obtain enough light from the edge to observe it and would measure the ball as being smaller than it actually was."

Alter went on to describe a simulation of this proposed experiment, and images from this simulation (Alter et al. 1951, Plate IX) appear as Fig. 5. Here they conclude:

> "...It must be remembered that if at some time in the past Pluto had an extensive atmosphere which liquefied or froze, the surface of the former atmosphere must be entirely smooth, for there could be no wind to stir ripples on its surface. It appears reasonable to assume that Pluto may have specular frozen pools of such a one-time atmosphere. However, this assumption is not vital to the conclusion made as a result of this experiment."

FIGURE 5 (full page photograph)

Kuiper (1949 p. 387) disagreed with this conclusion even before Alter published it:

> "Bodies devoid of an atmosphere are expected to have little or no limb darkening, since the surfaces almost certainly are rough. Even if icy surfaces ($CH_4$, $NH_3$, or $H_2O$) had formed initially, they would have since become frosty and rough, for several reasons. An icy surface, in order to be level, must have been deposited as a liquid ...After the liquid froze over, the atmospheric temperature continued to fall until the present temperatures were attained. During this cooling period, sublimation products (snows) must have precipitated. If certain parts of the ice remained exposed, they must have evaporated and have been eroded by temperature changes and meteoritic impact until they became rough in turn. In the case of Pluto, an additional deposit of cometary debris is expected to have been collected. It is concluded, therefore, that the visible disks of Pluto and Triton correspond to the real diameters..."

The idea of specular reflection from the subsolar point, or some other strange limb darkening law, served to inspire several new attempts to address the Pluto mass/size paradox. Alter (1952) suggested, "The only apparent hope of making a true [diameter] measurement is by the extremely tedious method of watching for it to occult stars."

Alter's suggestion was being acted upon at the Crakǒw Observatory. There, the veteran Pluto orbit calculator Banachiewicz had pointed out that Pluto occulted an average of one star of magnitude 15 or brighter per year. The task of identifying these candidate stars fell upon J. Kordylewski, who results appeared in a series of papers in *Acta Astronomica* (Kordylewski 1956*a*, *b*, 1957*a*, *b*, 1958). Of particular interest was an appulse of Pluto to NGC 3162, an elliptical galaxy measuring 1.1 by 1.4 arcmin; if the outline of Pluto could be seen obscuring the galaxy, then a direct diameter determination could be made. None of the predicted occultations ever occurred (Pluto's ephemeris was in error by approximately 2.5″), and the only fruits of Kordylewski's efforts were tables of astrometric positions of Pluto.

The occultation approach was later championed by I. Halliday (1963). An appulse of a star was observed in 1965, and although the occultation never occurred, the event allowed a more stringent upper limit to Pluto's diameter to be determined (Halliday et al. 1966). A single occultation chord through Charon was observed from South Africa in 1980 (Walker 1980). Brosch and Mendelson (1995) reported a single atmospheric occultation chord observed during horrendously nonphotometric conditions on 19 August 1985. But it was not until 1988 that the first successful stellar occultation by Pluto's limb finally was seen (Millis 1988), 32 years after the initial Crakо́w predictions.

At least two satellites searches were initiated about this time. The first, by Kuiper at the 82-inch McDonald reflector in January of 1950, consisted of five 103*a*-O plates. Three plates on January 24 were 30 minute exposures; two from the following night were one hour exposures, with the telescope masked down to 54 inches in order to improve the seeing. The limiting magnitude of these plates was about 18 or 19, and two of them were published by Kuiper (1961, Plate XII).

Humason undertook a similar photographic study April 15 and 16 of the same year, with the 200-inch Palomar telescope. Two plates of 15-minute exposure had limiting photographic magnitudes of about 22.4 exterior to 2″ from Pluto's photocenter (52,460 km). A third plate was exposed for 5 minutes to search for satellites as close as 0.5″ from Pluto; it had a limiting photographic magnitude of 18 to 19.

It is interesting to note that both searches were made under the assumption of a very faint satellite—much fainter than Charon's apparent blue magnitude of ~17.54—and Charon's disk was part of the saturated core of Pluto in all of these plates. When the discovery finally was made in 1978, it was with a telescope of 1.55-m aperture. Images showing clear asymmetry due to Charon have been obtained by this author with the 24-inch telescope of Dyer Observatory (Hardie et al. 1985). In any case, Charon's maximum elongation from Pluto during the 1950 apparition never exceeded 0.86″.

Robert Hardie was another researcher whose thinking was influenced by the idea revived in Alter's papers (Hardie, 1981, personal communication). A stellar photometrist by training, Hardie realized that specular reflection from a mottled surface would in all likelihood result in a strange rotational light curve, perhaps punctuated with spiky, delta function-like features. Such features would serve as "longitude markers" for Plutonian albedo patterns. While a graduate student at the University of Chicago, Hardie was well aware of Kuiper's Pluto researches.

Kuiper had obtained photoelectric photometry of Pluto in 1952 and 1953 with the 82-inch McDonald reflector. None of these data have been published, although while the author was a graduate student at Vanderbilt University, Hardie (1982) confided that most are of poor quality. While the 1952 data were essentially useless, the 1953 points showed some evidence for variability in Pluto's brightness. M. Walker, then a Carnegie Fellow at the Mt. Wilson and Palomar Observatories, obtained an additional six points, in blue and yellow, using the 60- and 100-inch reflectors of Mt. Wilson. But it was not until Hardie's dedicated assault on the problem in March and April of 1955 that the lightcurve determination was properly addressed (Walker and Hardie 1955). The Lowell Observatory 42-inch was so badly in need of realuminization that the telescope sat unscheduled through the spring. Hardie jumped at the opportunity, and undertook photometry of Pluto in an essentially unfiltered mode; subsequent observations were used to tie the observations to the Johnson-$B$ scale. A typical Pluto integration was about 8 min in duration.

Over five nights, Pluto was observed continuously for anywhere from four to seven hours, in order to rule out rapid periodicity. Seeing no such variability, nightly means were determined for the remainder of the program. Walker and Hardie write:

> "With the possibility of short-period variation eliminated, it became clear from the observations on successive nights that a periodic variation in the light does indeed occur, with a range of about 0.1 mag and a period between 6.0 and 6.5 days. Returning to the 1954 observations, we found that they could be combined with the 1955 observations according to a number of possible periods...
> In order to obtain a definitive value of the period, Kuiper has kindly turned over to the authors a portion of his 1953 observations for inclusion here [6 points]...Using his data, we may select a period from Table III which fits all the observations from 1953 to 1955; this value is 6.390 days with an estimated accuracy of ±0.003 day."

Walker and Hardie's determination of Pluto's period caused quite a stir, judging by the press coverage their results received at the time. Finally, someone had obtained a concrete observational result about the planet. Scientific publications about Pluto began to be published, for the first time, at a sustained rate of several per year, every year. Yet, for all the excitement generated, additional photoelectric photometry of the planet was not attempted by anyone, until Hardie decided to revisit the problem a decade later.

By 1964, Hardie had joined the faculty at Vanderbilt University and the A. J. Dyer Observatory in Nashville, Tennessee. Although always small in number, members of the Department had included other prominent astronomers such as E. E. Barnard and C. Seyfert. The Observatory consisted of a 24-inch cassegrain located on a dark site off-campus. Living on the Observatory grounds, Hardie had almost unlimited time to observe. After constructing three photoelectric photometers around 1P21 photomultipliers, Hardie began his most productive scientific years. Although he had obtained excellent *UBVRI* photometry of the Sun and all its planets (cf. Harris 1961) Pluto was the only planet to hold his interest throughout his career.

During Pluto's 1964 apparition Hardie obtained his second lightcurve. Fourteen points comprised the lightcurve, obtained over fourteen nights with the Baker 24-in. The data were presented in Montréal at the December meeting of the AAS. A plot appeared in the 1965 March issue of *Sky and Telescope.* The actual data went unpublished, however, until the completion of Marcialis' Master's thesis and subsequent paper in the *Astronomical Journal* (Marcialis 1983,1988).

Hardie was a friend and mentor to the author, and he confided several details about the Dyer photometry in conversations from 1980 to 1983. He has said that these observations were some of the most technically difficult he ever attempted, and required sitting in the Observatory's photo lab for extended periods before being able to locate and guide on faint Pluto. The plot published in the *Sky and Telescope* article was drafted by his son and shows fifteen, not fourteen, data points. While the original strip chart recordings no longer exist, a xerox of Hardie's original tabulation of final results does. Apparently the datum at rotational phase 0.6 is spurious, as it neither appears in the tabulation, nor was Hardie able to verify its reality at the author's request. (Neff et al. [1974] reported they were given only 14 measures, as well.)

It is a testament to Hardie's skill that this light curve was the most photometrically accurate data set obtained, until Tholen and Tedesco (1994) used the University of Arizona's 90-inch in the early 1980s.

The new data allowed Pluto's synodic period to be refined to 6.38673 days, with a formal error of ±26 s. Additionally, Hardie noticed three changes in the light curve: the amplitude and asymmetry had increased, while the mean level had decreased by 10%. In the *Sky and Telescope* article, Hardie is quoted:

> "...The shape of Pluto's light curve is now known with considerable confidence. During each rotation, the planet's light increases for about four days, then drops to minimum again in about two. If Pluto is assumed to have a more or less round disk, evenly illuminated out to its edge, the light curve's asymmetry can be explained only by an extremely unlikely pattern of dark and bright surface markings.
> Therefore... Pluto's disk is brightest at its center and darkened toward its rim.
> As a result, Pluto must be larger than hitherto believed, by an as yet unknown amount. The diameter that G. P. Kuiper determined, 5,800 kilometers, evidently refers to the central bright spot, not the entire disk...We may now dispense with, in all likelihood, the need to suppose a pseudo-degenerate-matter core to the planet, which is required by the current but improbable estimates of Pluto's mass and radius."

Hardie alluded here to Pickering's (1931*a*) white dwarf statement. Still, emphasis should be placed on the words "current but improbable." Hardie wanted proof of his conjecture invoking a "bigger" Pluto. This explains his long association with fellow Canadian I. Halliday of the Dominion Astrophysical Observatory. Halliday (1963) had published a paper on the formalism of determining the diameter of Pluto via a stellar occultation, and predicted such events happened about once every 1 to 3 years. Hardie's sabbatical (1964–1965 at the DAO and 1965–1966 at the USNO in Washington, D. C.) was in part sparked by an interest in astrometric observations of the planet for the purpose of predicting such occultations.

Halliday's search for candidate occultation stars was extensive. Results of the search were announced, not in an IAU Circular as might be expected, but rather in a *Sky and Telescope* article (Halliday 1965):

> "At the Dominion Observatory, we have been following the path of Pluto among the stars for two years, in order to predict occultations... With the help of several plates taken for us with the 48-inch Palomar Schmidt telescope and the 24-inch Baker corrector-reflector at Dyer Observatory [by Hardie and R. L. Sears], we have measured the positions of several dozen faint stars near the path of Pluto. By far the most interesting prediction is for the night of April 28th.
>
> The star in question lies at right ascension $11^h23^m12^s.1$, declination $+19°47'32''$ (1950 coordinates). Its visual magnitude has been measured photoelectrically at Dyer as 15.3, while Pluto is to be 14.1 on April 28th. Thus the combined light of the star and Pluto will be magnitude 13.8, and if an occultation takes place this will suddenly drop to Pluto's 14.1. This 25 percent change in light should be observable in telescopes whose apertures exceed 20 inches. Photoelectric records are desirable... The expected time of the occultation is about 05:00 Universal time on April 29th..."

Conditions for the event were nearly ideal: it was to occur over a great many large telescopes in N. America, during darkness, with Pluto fairly high in the sky, and the moon's phase nearly new. By and large, even the weather cooperated. The appulse was observed from about a dozen sites, including Victoria B. C., Lick, Flagstaff, Kitt Peak, and Fort Davis. No event was seen at any of the sites. Still, the null experiment yielded important results (Halliday et al. 1966):

> "The minimum photocentric separation for the most southerly observatory (McDonald) is reduced to $0''.125$, corresponding to a diameter of 5800 km for a grazing occultation. The circle represents the projected disk of the planet as seen from the McDonald Observatory if Pluto were 5500 km in diameter. Considering the mean error, estimated at $0''.013$, one can assign an extreme upper limit of 6800 km to the diameter with a confidence of 95 percent...
>
> It appears that any conjecture, such as Alter's or Hardie's, which

> attempts to explain the density discrepancy by assigning a diameter significantly larger than about 6000 km can now be eliminated... From consideration of its apparent magnitude and distance, the diameter must be at least 2000 km, the diameter corresponding to unit albedo...On the basis of our extreme upper limit of 6800 km for the diameter of Pluto, an extreme lower limit of 0.1 for its albedo can now be assigned...it is clear that the mass determined from the orbital perturbations must be greatly in error...These assumptions [earth-like density and 6800 km diameter] yield an upper limit of 1/7 of an earth mass...A value of 4 gm/cm$^3$ for the density would appear to be more plausible, and this, combined with a possible value of 5500 km for the diameter, would lead to a mass of 1/17 of an earth mass (5,700,000 reciprocal solar masses) or about five times the mass of the moon. Such orders of magnitude for the mass would not account for the perturbations in the motions of Uranus and Neptune that were used in deriving the mass of and predicting the existence of Pluto. We interpret the results of this investigation as substantiating independently Kuiper's conclusions that Pluto's mass has heretofore been greatly overestimated. There appears to be no escape from a mass estimate as small as 0.1 earth mass or smaller..."

Minimum separation of the Pluto–Charon photocenter from the star was $0.125'' \pm 0.008''$, as seen from Kitt Peak. Sanders' (1965) independent determination (from Lick), when properly reduced, gave a closest approach of $0.163'' \pm 0.020''$.

Figure 6 is a result of very recent re-analysis of the event by Marcialis (1996, 1997). At the mid-time of the appulse, Charon's circumstances were $\rho = 0''.79$, $\theta = 137^\circ.5$. Under the simple assumption that what Halliday et al. measured was the approach of the *photocenter* of the (unresolved) Pluto–Charon binary to the star, instead of the approach of a lone planet, one obtains several new results. With a Pluto:Charon light ratio of only 4:1 or 5:1, it is *impossible* for all the stations observing the event to have missed seeing an occultation event. In order for Pluto's limb to have passed entirely to the south of the star, the ratio had to be at least 6:1. If one assumes a ratio of 8:1, a very consistent picture results. While my analysis is ongoing and incomplete as of this writing, the following conclusions seem firm.

First, at 8:1 the problem is essentially dominated by Pluto. Charon sits on the periphery. Nowhere on earth was there a Charon occultation, and the photocenter and barycenter lie in very close proximity. Second, the derived impact parameters are essentially the same as what all the original observers deduced for their observing stations, Third, this brightness ratio is consistent ($\sim$8.2:1) with what one obtains under the assumption that Charon's magnitude has remained constant from 1965 up until it was recently measured directly with the Hubble Space Telescope (Buie et al. 1994; Buie, 1996 personal communication).

If we assume a constant Charon, this implies that Charon's southern hemisphere probably holds no new "surprises" in terms of very high or very low albedo features. More importantly, it is the *first observational evidence that the secular dimming of the system is intrinsic to Pluto*. Thus, the early spot models of Pluto (Marcialis 1983, 1988; Buie and Tholen 1989) overestimated Charon's contribution to the total flux, explaining the difficulties they encountered in reproducing the proper lightcurve amplitude during the 1950s. No longer are regions approaching (or exceeding) unity albedo required.

FIGURE 6 (at least 1/2 page; several panels)

## VI. THIRD GENERATION: INFORMATION EXPLOSION

The early to middle 1960s was an extremely fertile era for Pluto research. Escalation of the Cold War fueled the Space Age, and caused many young people to be hired into technical positions at Universities and government laboratories. For the first time since the early 1930s, the assault on Pluto's secrets no longer was a series of isolated skirmishes. Prior to the 1960s, the occasional researcher would perform an experiment, publish its result, and then move on to more tractable problems. Now, independent groups addressed different aspects of the Pluto problem simultaneously, often with knowledge of what others were up to. Collaborations still were rare, but the breadth of techniques being applied to study Pluto increased and became strongly diversified.

Sharaf (1955) published an improved orbit for Pluto. His method was to start with the elements of Bower's orbit XIX, and to calculate differential corrections to these, which were afforded by an additional 28 apparitions of observations. A followup study (Sharaf and Budnikova 1964) included second-order corrections and a still longer arc of observations. NASA authorized the translation into English of these two documents in 1969, no doubt in connection with the planning of outer solar system missions. These solutions were among the last analytical ones attempted. Much smaller residuals result from numerical techniques (see, e.g., Fig. 1 of Cohen et al. 1967). It became quite obvious about this time that when Pluto dives into the Uranus-Neptune region (and out of the ecliptic) near its perihelion, complicated perturbations from all the jovian planets occur. To predict Pluto's position with accuracy, either the epochs of osculation must be much more tightly spaced or, more simply, completely numerical techniques adopted.

In light of the observational results of Pluto's stellar appulse, the celestial mechanicians were feeling increased pressure to justify their large mass estimates for Pluto. As discussed earlier, the need for a simultaneous solution which incorporated all five outer planets became clear in the 1950s. Even the celestial mechanicians came to doubt the previously derived results. The early 1960s armed them with a powerful new tool with which to attack the problem—the electronic computer. Brouwer (1966) was in the process of initiating a new investigation of Pluto's orbit in 1965, which unfortunately was cut short by his death.

Brouwer (1955) had suggested, "The three outermost planets have mean motions so nearly in the ratio 6:3:2 that it is not excluded that their orbits are connected by a resonance relationship." Acting upon this idea, Cohen and Hubbard (1964,1965) decided to explore interactions between Neptune and Pluto via long-term, numerical integration of the equations of motion. The problem was a good early test of the new way to study celestial mechanics: the results are not very sensitive to the actual mass of Pluto, so long as it could be considered to be much smaller than that of Neptune.

The orbits of Neptune and Pluto were run backward in time some 750 and 500 revolutions, respectively. Checks on the accuracy of the calculations were provided by monitoring the total angular momentum and energy of the system; changes in these quantities by more than small, threshold amounts were what dictated the end of the experiment. Simulating 120,000 yr of solar system history required about 100 hr of CPU time on the 13-place, NORC machine of the U. S. Naval Weapons Laboratory. The results were rather startling, and suggested additional numerical simulation would be a fruitful course to pursue. (Today, a similar simulation could be done on a workstation or personal computer during one's lunch break.)

The primary finding was that Neptune and Pluto were in a libration resonance—a conclusion which Russell *almost* had stumbled upon 30 years earlier. As a result of this resonance, Neptune and Pluto never could collide as a result of orbital precession. Furthermore, Pluto and Neptune were shown to approach one another most closely (approximately 18 AU) when Pluto is at *aphelion*, not at perihelion. The two planets tended to avoid one another. In fact, Pluto approaches Uranus more closely than it does Neptune. This unexpected orbital stability placed another nail in the coffin of the "escaped Neptunian satellite" idea of Yamamoto and Lyttleton, although Cohen and Hubbard did not state this explicitly.

The Pluto–Neptune libration is characterized by an oscillation of the angle $\delta$:

$$\delta = 3l_P - 2(l_N + \varpi_N - \varpi_P) - 180^\circ \, , \tag{1}$$

where $l$ and $\varpi$ are the mean anomaly and longitude of perihelion, and the subscripts $P$ and $N$ are Pluto and Neptune. The best way to visualize this resonance is in a Sun-centered coordinate frame which rotates at Neptune's mean rate of motion about the Sun (Fig. 7).

Long-term secular changes in all 6 of the classical orbital elements were revealed by the calculations, and ascribed to periodic oscillations of order $10^6$ to $10^7$ yr. Later studies (see, e.g., Cohen, et al. 1967,1968) improved the orbital elements of Pluto, and thus the amplitude and period estimates for the libration. As computing power grew, the simulation interval was extended first to 300,000 yr, then 1 Myr. Williams and Benson (1971) verified the conjecture that the long-term secular changes in elements were indeed periodic, by extending the simulation interval to 4.5 Myr. Also uncovered were even longer-period oscillations and two more resonances.

The year 1966 was an interesting one in the study of Pluto, as two additional disciplines entered the arena of Pluto study—one observational, the other theoretical. Kellermann and Pauliny-Toth (1966) attempted the first radio observations of Pluto. Their result was part of a study to measure the 1.3-cm emission from Uranus and Neptune. Although the detection of Neptune at this wavelength was a first, Pluto was well below the sensitivity of their observations. The formal result for Pluto, $4 \pm 17$ mJy, translated to an effective temperature of $300\pm$ 1200 K, assuming a diameter of 6000 km. While a somewhat less than satisfying result, it demonstrated how researchers in areas other than the "classical" observational disciples of astrometry and photometry were becoming interested in the study of Pluto. A second attempt, by Webster et al. (1972) at 3.2 cm, was only slightly more encouraging: $T_{\mathrm{surf}} < 162$ K.

The theoretical paper (Flandro 1966) has slipped largely into anonymity, although it should be required reading for all beginning students of celestial mechanics. "Fast reconnaissance missions to the outer solar system utilizing energy derived from the gravitational field of Jupiter" appeared in *Astronautica Acta*, a journal comprised largely of contributions from the engineering disciplines. The abstract reads:

> "Contrary to popular belief, indirect ballistic trajectories involving close approach to one or more intermediate planets need not require longer flight duration than is characteristic of direct transfer orbits. In fact, significant reduction of both required flight time and launch energy results if efficient use is made of the energy which can be gained during a midcourse planetary encounter... This paper describes the application of energy derived in this fashion, utilizing gravitational perturbations from Jupiter, for reduction of required launch energy and flight duration for exploratory missions to all of the outer planets of the solar system. The latter half of the next decade abounds in interesting multiple planet opportunities due to the similar heliocentric longitudes of the major planets during this time period. Trajectories to Saturn, Uranus, Neptune, and Pluto using the midcourse energy boost from Jupiter are best initiated in the years 1978, 1979, 1979, and 1977 respectively. Flight time reduction ranges from one half the required direct trajectory duration for Earth-Jupiter-Saturn missions to as much as 85% of the direct transfer time for Pluto flights via Jupiter. Many multiple-target trajectories are also possible. Of particular interest is the 1978 Earth-Jupiter-Saturn-Uranus-Neptune "grand tour" opportunity which would make possible closeup observations of all planets of the outer solar system (with the exception of Pluto) in a single flight."

Planetary scientists everywhere owe a huge debt to this contribution; it was the father of what was to become the Voyager Project.

At the time of the paper's publication, Venus and Mars were the only planets already visited by spacecraft, and these only via brief flyby reconnaissance missions. In studies of a possible mission to Mercury (what was to become the Mariner 10 mission), trajectories using Venus to slow the spacecraft had substantially longer flight times than even a direct Hohman transfer orbit which, as a result of being minimum-energy, also maximizes cruise time between launch and destination. Using inner planets to reach the outer ones, such as the circuitous route Galileo was doomed to pursue, also increased transit time. Thus, most spacecraft trajectory designers of the day assumed multiple encounters always increased travel time, thereby complicating mission planning.

Unfortunately, two factors contributed to Pluto being deleted from the agenda of the "Grand Tour." First, spacecraft designers were not sure they could build a spacecraft which would survive the rigors of a 2530-day trip to Pluto, or even the 1957-day Jupiter-Saturn-Uranus mission. Second, the optimum Jupiter-Pluto trajectory had the earliest launch window, by more than a year, of all the possible missions. It was decided to play the billiard game conservatively, using two spacecraft for redundancy. Jupiter and Saturn became the primary mission, with options of pressing on to Uranus, then Neptune, should they be successful. (Note that serious talk of the Pluto Fast Flyby mission began one Pluto-Jupiter synodic period after Flandro's paper. Perhaps the mission will come to pass in a second synodic interval.)

It is hard to doubt the wisdom of the decision, since an entire generation of planetary scientists owe their careers to it, and it seems likely that a second seems doomed to subsist largely upon the leavings of the first. Nevertheless, no one in the Pluto community can help but think, "What might have been..."

Rollan I. Kiladze began his study of Pluto's rotational properties in 1965. A dynamicist, Kiladze (1965) derived a semi-empirical formula supposedly relating a planet's rotational angular momentum to its mass and orbital semi-axis major. The validity of this formula aside, its application was problematic given the mass and radius estimates available at the time. A second paper (Kiladze 1966) reanalyzed Hardie's original photometric data and tried to make a case for a rotational period of 1.1819 days. This was despite Hardie's strenuous objections to the contrary (Hardie 1982).

In the spring of 1966, Kiladze (1967) became the first person to challenge Hardie's decade-long monopoly on the Pluto photometry market. A five-point photoelectric lightcurve was obtained with the Crimean Astrophysical Observatory Southern Station's 50″ reflector. Kiladze concluded the photometric period and epoch, as determined by Hardie, were indeed correct after all. However, examination of these data shows them to be substantially brighter than Hardie's 1964 light curve, and the shape of this curve really does not match the earlier ones particularly well. The Crimean data have been universally rejected by all those attempting to model Pluto's albedo as being inconsistent with surrounding data sets.

Now that Kiladze (1967,1968) had at least convinced himself of the 6.3867 day period determination, he turned around the application of his semi-empirical mathematical argument. Substituting the value for the rotation period constrained the "allowed" limits for the density, radius, mass, and albedo of the planet. When Kuiper's diskmeter diameter also was adopted, Kiladze obtained values $\rho = 0.72\ \rho_\oplus$, $m = 0.07\ \mathrm{M}_\oplus$, and $A_V = 0.17$ for density, mass and visual albedo, respectively.

Most questioned the robustness of these results, but they nonetheless are important for two reasons. First, they admit that Pluto's mass might be more lunar-like than earth-like. Kiladze's formula shows it might be as low as 0.03 $\mathrm{M}_\oplus$ if Pluto's light curve were double-humped (corresponding to a period twice that found by Hardie). Second, because the formula supposedly arose as a result of accretion processes in the solar nebula, Kiladze claimed, "...the compatibility of inequalities...demands for the rotation period of Pluto a value greater than three days. Consequently, the rotation period does not contradict the conception of Pluto's origin, consistent with the rest of the planets, as an independent planet at its present distance."

It is interesting to note in passing that Kiladze (1967) states, "Examination of only circular orbits, however, leads to the conclusion... corresponding to a counter rotation of the planet...." This is the first appearance in the literature which admits the possibility that Pluto might be a retrograde rotator.

Brosche (1967) also contrived a semi-empirical argument which led to a small mass estimate for Pluto. Based on the premise that the ratio of the angular momenta of comparably-sized planets scales approximately as the square of the mass ratio, he obtained an estimate of 0.011 $\mathrm{M}_\oplus$ for Pluto. Adopting the Halliday et al. upper limit for Pluto's radius led to a (then) rather unpopular density estimate of 0.80 g $\mathrm{cm}^{-3}$. Brosche assigned factors of ~2 for the uncertainties in these estimates.

For his analysis, Brosche used Pluto/(Earth + Moon) and Pluto/Mars for his ratios. With hindsight, however, the choice of Earth and Mars appears questionable. Earth is not an "isolated" planet—a lot of the system's momentum is in the Moon's orbit, even if one chooses to ignore momentum transfer over the age of the solar system. How it got there is a strong function of origin and evolutionary scenarios. Besides, Pluto then was thought to be devoid of significant satellites. Had Brosche used Venus instead (it was rejected due to "solar tides" possibly affecting its primordial spin rate), a much larger value for the mass would result. (Would the retrograde rotation give Pluto a negative mass?) A vitally important question not considered was: A density for Pluto of ~0.80 g $\mathrm{cm}^{-3}$ is decidedly unlike those of the inner planets. Therefore, is not such a direct comparison between the rocky planets and Pluto suspect in the first place?

While the methods of Kiladze and Brosche may not have stood the tests of time, their results served to highlight a need to reexamine the system of planetary masses as it related to the Pluto problem. Kuiper had argued that the mass determination was in error a quarter of a century earlier. Now, a new generation was searching for ways to be "comfortable" with a less-massive Pluto.

And come down the mass estimates did. In a pair of papers by Duncombe et al. (1968*a, b*), these celestial mechanicians described their experiments with simultaneous numerical integration of the orbits of the five outer planets, as had been suggested by Brouwer a decade earlier. They discarded the 1795 Lalande observations (Mauvais 1848), instead using the very reliable USNO transit circle observations of Neptune obtained annually from 1960 through 1968. Clearly an inverse solar mass for Pluto of 360,000 could not explain Neptune's residuals, given the then-accepted masses for the jovian planets. Their search criterion was to find the value of Pluto's mass which minimized longitude residuals in the observations of Neptune for the interval 1846–1968. A reciprocal mass of 1,812,000±50,000 ($M = 0.18$ $M_\oplus$) best fit their data—down by nearly a factor of 5 from what Brouwer (1955) had found. Assuming the upper limit of 6400 km provided by the stellar appulse (Halliday et al. 1966), a simple calculation shows that Pluto's bulk density was about 1.4 times that of Earth, i.e., 7.7 g $cm^{-3}$. While still rather high, at long last this estimate approached the realm of plausible building materials; a pure iron planet with a thin veneer of frost would have a bulk density of about 7.4.

These results led Halliday (1969) to suggest that perhaps the true value of Pluto's mass was being masked by small errors in the accepted mass values for the four jovian planets. Halliday claimed that the issue of Pluto's high density might be resolved, say, if Neptune's mass were reduced by 0.1 $M_\oplus$. While this was a substantial decrease for Neptune or Uranus, similar revisions for Saturn or Jupiter easily could be accommodated. Examining this possibility, Duncombe et al. (1970,1971) found slight revisions to the masses of the jovians indeed were in order. However, the effect on their derived mass for Pluto was only a very slight decrease, from 0.18 to 0.17 $M_\oplus$. Two decades later, armed with the very accurate Voyager flyby mass determinations for these bodies (in particular Neptune) and additional earthbound astrometry, Standish (1994) finally was able to show this indeed was the case. (Furthermore, there was absolutely no evidence in the residuals to suspect a tenth planet.)

The year of the first moon landing leaves us with a mystery. Hardie (1969) claimed that Pluto's secular dimming was not permanent. Hardie speculated the dimming might be due a change in the physical state of the surface, such as frosts melting or subliming as the planet fell towards perihelion. The exact motivation for this claim has been lost. It might be simply that Hardie reasoned the dimming could not go on forever. But this is a rather trivial deduction, and it was very uncharacteristic of him to make speculations, particularly in the press, without supporting evidence. So, it likely appears that Hardie had some direct knowledge that the rate of dimming was slowing. Where did he learn this? When asked by the author if he had done any additional Pluto photometry since the 1964 light curve, he replied in the negative. Had someone else obtained photometry? "No." The Kiladze data, were they believable, showed the opposite trend. The Iowa group did not begin their photometry until 1971. Photographic plates of Pluto obtained at the A. J. Dyer Observatory for astrometric purposes (Hardie et al. 1985) covered the interval, but were neither calibrated nor of sufficient accuracy for photographic photometry to show the trend, much less a deceleration in its rate. Besides, the plates (typically 2 or 3 per apparition) were obtained at random photometric phases.

The only evidence I have been able to find that Pluto's brightness was being monitored during the latter 1960s is in a paper by Moseley (1969), an experienced visual observer of variable stars. Using the 25-cm refractor of the Armagh Observatory, Moseley noted there was a discrepancy between the 1968 *Handbook of the British Astronomical Association* (magnitude 15.0) and the values he obtained on 1968 March 26, 28, and 29: 14.1, 13.9, and 14.0, respectively. Moseley's paper was published simultaneously with Hardie's comments; I speculate that Hardie knew of it. Nonetheless, the visual observations certainly are no more accurate than Hardie's photographic plates, and Hardie was not one to trust visual photometry of even much brighter objects to more than 10 to 20% in any case. So the motivation and/or evidence for Hardie's claim may never be known.

What *is* known, however, is that he still clung to the "melting surface" explanation upon my arrival at Vanderbilt a decade later. He was convinced Pluto's lightcurve amplitude was indicative of a low-obliquity spin axis, and had to be sold on the idea that a static albedo pattern, coupled with changing aspect angle, could explain the so-called orbital lightcurve.

## VII. THE 1970s

### A. How low can you go?

The 1970s were to be a decade where Pluto's secrets were revealed more by telescopic study of the planet as an astrophysical object than by modeling astrometry and generating ephemerides. However, two contributions by the dynamicists in 1971 deserve mention. Seidelmann, Klepczynski, Duncombe, and Jackson continued to model Neptune's motion, in search of the "Holy Grail" of Pluto's gravitational signature. Their technique was virtually the same as in the previous effort, but this time the old astrometric data were augmented by Jackson's (1974) extremely accurate observations. These were obtained primarily with the USNO Six-Inch Transit Circle in Washington, D. C. Another slight modification to their techniques was afforded by increased computing power: each of the 5,426 individual observations could be modeled individually. The older method of first computing normal positions tended to over-weight defective observations.

Minimization of residuals in Neptune's longitude for the interval 1939 to 1968 gave 2,848,000 for the reciprocal mass of Pluto, while the subset of data obtained between 1960 to 1968 (primarily transit circle data) yielded an inverse mass of 3,473,000. The authors chose to adopt a rounded, intermediate value of 3,000,000±500,000 for their final result. They conclude, "Assuming 6400 km as an upper limit to the diameter of Pluto (Halliday et al. 1966; Kuiper 1950), this determination of the reciprocal mass of Pluto (3 000 000 or 0.11 earth masses) gives a density of 4.86 gm/cm$^3$ or 0.88 times the Earth's density. If we assume a density equal to that of the Earth, this would give a diameter for Pluto of 6112 km." For however brief a time, Pluto once again appeared more Mars-like than Earth-like.

T. Van Flandern suggested improvement might be possible through use of the FK4 system, in lieu of the older FK3. Indeed, the latitude residuals were reduced by over 30%, but some periodicity in them still remained.

The "Mother of all Solutions" was attempted by M.I.T.'s Ash, Shapiro, and Smith (1971) about the same time as the USNO effort. Simultaneous solutions for the masses of all nine planets and the Moon were found, incorporating essentially all published solar system astrometry. Additionally, radar observations of the inner solar system were folded into the mix. The extreme value of these data is apparent: "Interestingly, the radar data alone yielded a value of 1047.46±0.06 for the inverse mass of Jupiter. The precise Mars data obtained at Haystack during the 1967 and 1969 oppositions primarily are responsible for the small standard error. Thus, modern inner planet radar data spanning only 2 years can almost compete with several centuries of outer planet meridian circle observations in the estimation of Jupiter's mass." While the perturbations of the terrestrial planets on Neptune or Pluto are not sensible, those of Jupiter mostly definitely are.

Other innovations in the M.I.T. work included parameters to account for systematic differences in the reference systems used in the optical data and, for the inner planets, allowance for possible errors introduced by differences between photocenter and barycenter. With regard to Pluto, the authors reported:

> "We find, however, that the data don't seem to warrant more than the placement of a lower bound of about 2,000,000 on the inverse mass. When all the outer planet data were analyzed and solutions obtained only for the initial conditions and masses of these planets, we found for Pluto's inverse mass 1,500,000 ± 210,000 (formal standard error); when the equator, equinox and declination biases were added to the parameter set, the corresponding value was $-4,200,000 \pm 260,000$! Further, when all the data were analyzed simultaneously or broken down into single-planet subsets, solutions for some reasonable parameter sets also yielded negative values for Pluto's mass. These differential corrections, being relatively large, may not correspond to the converged least squares solutions...The

> extreme sensitivity of the differential correction to Pluto's mass to the choice of data and parameter sets indicates the difficulty of extracting a meaningful value for it from the existing outer planet observations.
>
> The fact that the sum of squares of the residuals is decreased by the addition of Pluto's mass to the parameter set is, in itself, not significant. Any additional parameter, whether or not is has physical meaning, may reduce the residuals. In Pluto's case, the conspicuous asymmetry about the minimum on the graph of the sum of squares of residuals plotted against planet mass obtained at the USNO indicates that the value at the minimum may represent more of an upper bound on the mass than a reliable determination....
>
> These results and inferences, when coupled with our ignorance of the mass environment [Kuiper Belt] past Neptune's orbit, lead us to the conclusion that Pluto's mass, and hence its average density, cannot be determined reliably from existing data."

With this effort, the answer to the 40-year-old question of Pluto's mass appeared to be that *there was no answer*, at least with current techniques, technology, and observational baseline. Once again, it seemed that time would be the only way to supply one of the needed improvements before another attempt was justified. Meanwhile, the discovery of Charon (Christy and Harrington 1978), revealed the inverse mass of the system to be 200,000,000, or 0.0017 $M_\oplus$. (An unweighted mean of all determinations since from 1980–1996 is 140,500,000±6,600,000 inverse solar masses, or 0.00237±0.00011 $M_\oplus$.)

For the balance of the decade, the efforts of dynamicists as related to Pluto were split down three paths. The first studied resonances between Pluto and Neptune over longer timescales and tried to assimilate the numerical results into semi-analytic form (Williams and Benson 1971, Nacozy and Diehl 1974, 1978*a, b*, Lobkova 1975). Second, there was trajectory planning for the Grand Tour Missions and the resulting deletion of Pluto as a port of call for Voyager (Ledbetter 1971, Odom 1971, Penzo 1971, Long 1971, Friedman et al. 1972). Third was the revival of a massive trans-Plutonian planet from modeling its perturbations on P/Halley (Brady 1972), Neptune (Rawlins 1970), or Uranus (Gunn 1970), and the immediate and vehement response of the community to these works (Goldreich and Ward 1972; Klemola and Harlan 1972; Seidelmann 1972; Seidelmann et al. 1972).

Dryer et al. (1973) undertook one of the first studies of how the outer planets might interact with the solar wind. The motivation for their work logically was connected to mission planning for the various Grand Tour trajectories being considered at the time. Clearly, lack of data about the outer solar system softened their conclusions ("With the exception of Jupiter, we do not know if the outer planets possess intrinsic magnetic fields.") Pluto was included on their list for completeness, but necessarily treated in a rather cursory manner. For Pluto, their model assumed neither an internal planetary magnetic field nor an atmosphere; the ionopause was fixed at the surface. We now know conditions at Pluto are such that the scale height of the atmosphere is a substantial fraction of the planet's radius (Millis et al. 1993), and the Dryer et al. model probably is more appropriate to Charon than to Pluto.

## B. The Third Telescopic Crusade

A new generation of young scientists took the challenge of Pluto observations in 1970, initially from the University of Iowa. Two young professors, J. D. Fix and J. S. Neff, served as the ringleaders of the first real "group" to study Pluto since the original Lowell observers. Their collaborators were graduate students W. A. Lane, Larry A. and Linda A. Kelsey, and postdoctoral researcher A. A. Lacis. These six people, plus L. E. Andersson of Indiana University, revived interest in observational study of the ninth planet. Schooled mainly as photometrists, these individuals expanded their "tool kit" of research skills to include spectroscopy, polarimetry, and theoretical modeling. Beginning with their various contributions, prying loose the secrets of Pluto and Charon has become virtually a continuous process. Exposing Pluto's secrets would no longer be occasional random shouts in the darkness, but a chorus of many voices.

On 1970 May 3, 5, 6, and 8, Fix, Neff, and Larry Kelsey obtained spectrophotometry of Pluto with the 24-inch reflector of the University of Iowa at Hills. This work came 21 long years after Kuiper's (1949) claim that "steps were being taken" to acquire photoelectric spectrophotometric data. Observations were obtained at a resolution of 128Å, in 21 equally-spaced channels between 3400 and 5900Å. A total of 34 scans were obtained during the run, of which 11 were rejected as unsatisfactory. Due to the faintness of Pluto, the remaining scans were co-added to improve the signal-to-noise ratio, and the resulting spectrum (Fig. 8) represented an average over a complete rotation. Fix et al. summarize their significant findings:

> "...albedo of Pluto generally increases toward the red. The normalized $(B - V)$ index derived from our curve is about +0.18, in good agreement with the observed value of +0.17 reported by Harris (1961). As the probable errors indicate, our results become less reliable as the ultraviolet and infrared parts of the spectrum are approached. Nevertheless, it appears that there is a genuine peak or upward trend below about 3800Å. The most definite evidence of structure in the curve occurs at about 4900Å, where there appears to be a real but shallow depression. There also appears to be a decrease in the slope of the curve at a wavelength of about 4500Å."

The color index referred to is that obtained after removal of the Sun's intrinsic $(B-V)$ of 0.67 (cf. Hardorp 1980). Restoring it gives $(B-V) = +0.84$, in good agreement with more modern results (see, e.g., Marcialis 1990). While Pluto/Charon is indeed "red" the spectral sense, it would be difficult to convince one's eye of this. For example, Mars (Johnson and Gardiner 1955) has $(B-V)=+1.36$, (1.6× the slope of Fig. 8). The ice-covered Galilean satellites range from 0.83–0.87, bracketing the Pluto–Charon value. Yet, these moons appear essentially white to the eye.

The most interesting part of the Fix et al. spectrum is its behavior near the short-wavelength end. Alternatively interpreted as either an upturn extending beyond the Earth's atmospheric cutoff (about 3000Å), or as an absorption at about 3800Å, Barker et al. (1980) since have shown the structure here to be spurious: the red spectral slope actually continues at a more or less constant rate. Nevertheless, the Fix et al. spectrum triggered several feral anser pursuits by theoreticians and at least one mineralogist.

Two months following publication of the spectrum, Manning (1971) proposed in a Letter to *Nature*:

> The most prominent feature in the Pluto spectrum at 3780Å might therefore be considered to be an absorption band. Its half-width (~1,700 $\mathrm{cm}^{-1}$) compares favourably with the half-width of the 3700Å (27,000 $\mathrm{cm}^{-1}$) grossular band (~1,000 $\mathrm{cm}^{-1}$), hence the Pluto band is likely to be of solid state origin. It seems reasonable to suggest further that the bands have a common origin, namely they mark the transition $^6\mathrm{A}_1 \rightarrow {}^4\mathrm{E}(D)$ in $\mathrm{Fe}^{3+}$ ions in oxide or silicate environments.
>
> Although the assignment of a single band in a spectrum cannot be considered conclusive, there are other features in the Pluto spectrum that correlate well with known bands of $\mathrm{Fe}^{3+}$ ions in minerals. For example, there seems to be a significant absorption at ~4300Å (23,300 $\mathrm{cm}^{-1}$) in the Pluto spectrum, which could mark the lowest field-independent transition. . . . The Pluto spectrum also shows definite absorptions at 4900Å (20,500 $\mathrm{cm}^{-1}$) and possible absorption at 4020Å. . . . There would seem to be sufficient correlations of energies and half-widths of bands in spectra of Pluto and terrestrial crystals to justify the suggestion that the surface of Pluto, at least, is iron-rich."

If nothing else, this shows the danger in handing telescopic data in an unsupervised manner to someone versed only in laboratory analysis. In Manning's defense, neither the Seidelmann et al. (1971) nor the Ash et al. (1971) papers had as yet been submitted, and the latest mass estimate (Duncombe et al. 1970) still placed Pluto's bulk density around 7.7 gm $\mathrm{cm}^{-3}$. Later that year, Fix (1972) submitted a short paper to *Icarus* in which he constructed the first simple models of Pluto's interior. Fix determined the fractional mass of terrestrial core ($F$) and mantle ($1-F$) materials for a suite of assumed Pluto radii and masses, apparently convinced existing estimates for these quantities were valid. His bottom line was, ". . . Thus, while it is possible that Pluto and the Earth have roughly the same gross chemical composition, it is considerably more likely that Pluto has a greater share of heavy materials than does the Earth."

The alternative interpretation of the spectrum's upturn toward the ultraviolet was that it was due to Rayleigh scattering by a clear atmosphere. As discussed earlier, Kuiper (1949) had searched unsuccessfully for such behavior in photographic spectra of Pluto. The idea was revived by Hart (1974), "It is unlikely that any gas other than neon will be present in quantity. Most common substances, like $CO_2$, $NH_3$, and $CH_4$ are solids, with negligible vapor pressures, even at the highest temperatures which might exist on Pluto. The second most abundant gas is probably argon... Even if free $N_2$ exists, its vapor pressure is only about 0.04 atm at 42.9K."

Hart went on to consider the possibility of neon oceans, both permanent and seasonal, and how much neon would have to be present for diurnal and seasonal thermostating to occur. He states in conclusion, "... although the observations are compatible with a surface pressure as large as 1.0 atm, a surface pressure as large as 3.0 atm seems unlikely. It follows that... neon oceans should not exist on Pluto during any part of the year."

In a paper submitted only 26 days following Hart's, G. S. Golitsyn (1975) also considered a neon atmosphere, perhaps with components of argon and methane. The Soviet preferred a somewhat thinner atmosphere. The abstract reads, "Even a very rarefied atmosphere could materially influence the temperature distribution on Pluto. An analysis of observational evidence indicates that the atmospheric pressure at the surface is probably not more than 0.1 atm." Golitsyn's paper concentrated on the global behavior of the Hadean atmosphere, and its implications for weather on the planet: "... the large thermal inertia of the atmosphere at low temperatures... insures effective heat transfer into regions of the planet poorly warmed by the sun despite low wind velocities... atmospheric circulation will be symmetric, as is true on Titan, that is, a single direct circulation cell extends from the equator to the pole. The small value of the Rossby number also implies an insignificant diurnal temperature variation."

Golitsyn even considered, somewhat presciently, the existence of nitrogen frost patches and polar caps on Pluto:

> "In order for there not to be polar caps at the pole [*sic*] where $T \approx 42^\circ K$, $p_{N_2}$ should be less than 0.4 mb. Even if we suppose that the 20% decrease in albedo observed during the 1955-1972 period occurred because of thaw of preexisting $N_2$ frost patches on the surface of the planet as it approached the sun (rather than because the orbital motion of Pluto turned darker equatorial regions of the planet to face the earth), an upper limit on the $N_2$ abundance in the atmosphere would still be 1 mb. Nevertheless, even so rarefied an atmosphere would fully control the thermal conditions on Pluto's surface, and in principle it might be detectable, for example, from radio waves emitted by the planet."

Although the above speculations were based upon what amounted to 3 spurious data points, we can see in them the roots of many "modern" ideas regarding the conditions and behavior of Pluto's surface and atmosphere.

Other Pluto-related investigations at Iowa were being conceived in the early 1970s. A. A. Lacis, who completed his Ph.D. thesis on the structure of white dwarfs in 1970, teamed up with Fix (Lacis and Fix 1972) in an analysis of the Walker and Hardie (1955) and Hardie (1965) photometry. The two light curves were combined, an obliquity of 90° assumed, and Fourier techniques employed in an attempt to invert the data for the longitudinal distribution of bright and dark albedo patches. While the results of this investigation were not very conclusive, the paper essentially was a "road map" laying out future University of Iowa collaborations:

> "On the basis of presently available photometric observations of Pluto, models which are consistent with the observed light curve may be constructed entirely, or in part, from diffusely or geometrically reflecting material...the limitations imposed on the allowed albedo combinations are not very strict. Although it is clear that a sizable difference in albedo must exist between the brighter and darker areas, it cannot be ascertained whether the light variation is due to dark spots or to bright spots, or whether the spotted areas are large or relatively small.
>
> It is possible that a more fruitful approach for distinguishing the principal type of surface material lies in searching for an opposition effect...Also, if the two types of surface areas have different polarization characteristics, then the change in polarization with phase, if it can be measured, could shed considerable light on the nature of Pluto's surface.
>
> Otherwise, it is hoped that additional photometric observations of Pluto will improve the reliability of higher-order Fourier terms to the point where it is possible to distinguish the definite presence or absence of the two types of reflecting areas considered. When this is the case, a more precise model of the surface of Pluto may be constructed."

Lacis and Fix realized the 90° obliquity assumed in their lightcurve analysis was suspect, but it had been forced upon them by the paucity of data. Fix soon teamed up with L. Andersson, to address these two items (Andersson and Fix 1973). Andersson obtained new *UBV* observations as part of his Ph.D. research on 16 nights in 1972. These were supplemented by one night each during the 1971 and 1973 apparitions. The $U$ and $B$ data were used to derive mean color indices of $B-V=+0.81\pm0.01$ and $U-B=+0.31\pm0.03$, and the $V$ data yielded a third Pluto light curve (dismissing Kiladze's as spurious). The trend of increasing amplitude and decreasing mean magnitude uncovered by Hardie (1965) indeed was continuing; the amplitude of the light variation had increased to 0.22 mag.

The Fourier analysis techniques with which Fix had become familiar during his collaboration with Lacis proved valuable when applied to the photometric data in a slightly different way. Armed with a third light curve and the wider range of orbital longitude through which Pluto had been observed, Andersson and Fix were able to

> "...interpret the gradual decrease in the mean brightness and increase in the amplitude during the past few decades as being due to a change of aspect of the planet because of its orbital motion and the large obliquity of the planet's rotational axis. In other words, we propose that the planetographic latitude of the sub-Earth point has changed considerably during the period since the discovery of Pluto and that Pluto's axis of rotation is oriented in such a way that the sub-Earth point has moved toward the planetary equator during the past 40 yr. In order to account for the decreasing brightness of Pluto it is proposed that the polar regions of the planet have higher albedo than do its equatorial regions.
> The existing photometric data for Pluto have been analyzed in an attempt to determine the orientation of Pluto's axis of rotation ...Accordingly, the right ascension, declination plane has been searched for the locations of those poles which give, in terms of the adopted model, satisfactory agreement with the photometric data...The region encompassing the various domains is shown in [Fig. 9]. It is apparent from [Fig. 9] that the obliquity of Pluto is large, probably greater than 50°...(There is of course another domain of pole positions lying 180° from the domain shown in [Fig. 9].) It will be very interesting to repeat these calculations after the light curve of Pluto has been measured again 5 or 10 yr from now. Much of the present domain of possible pole positions will prove to be incompatible with the new light curve and it should thus be possible to determine the coordinates of Pluto's pole with considerably greater precision than is attainable at the present."

The actual pole position of the rotational axis of the Pluto–Charon system turned out to be just off the edge of the solution domain. Andersson and Fix also reanalyzed the Walker and Hardie (1955) observations, employing trial values of a linear phase coefficient. They found the rms deviation from a smooth curve was reduced to 0.006 mag for a phase coefficient $\beta = 0.05$ mag deg$^{-1}$. (Improved estimates for $\beta$ since have been obtained by various methods and authors; it has been found to be closer to $^3/_4$ the initial determination. For details, see Marcialis 1983, Binzel and Mulholland 1984, Tholen and Tedesco 1994).

The Iowa group repeated their spectrophotometric observations of Pluto over 10 nights in April and May of 1972. These new data became the basis of Linda Kelsey's M.A. thesis (1972). This time, 85 equally-spaced wavelengths from 3400Å to 5700Å were sampled. Concurrent with these new observations, Larry Kelsey and Fix (1973) obtained the first polarimetric measurements of Pluto, using the Kitt Peak 1.3-m. Their unfiltered observations had a bandpass approximately that the detector, an S-20 photomultiplier, and were obtained over an entire rotation:

> "Pluto has been observed at a solar phase angle of 0°.8 and found to have a degree of polarization of 0.27 percent (probable error 0.2 percent) and a position angle of polarization of 156° (probable error 2°). The observations indicate that Pluto has a polarization curve with a deep negative branch and, therefore, that its surface is likely to be microscopically rough. The observed polarization is compatible with the small geometric albedo of Pluto. No convincing evidence of variable polarization associated with Pluto's rotation has been found."

Similar polarization measurements were obtained in 1979–1981 by Breger and Cochran (1982), with essentially the same result: the plane polarization coefficient was 0.29±0.01. There seems to be no dependence of the polarization coefficient on rotational phase, sub-Earth latitude, solar phase angle, or lightcurve amplitude. The most obvious explanation for this is a surface of relatively high-albedo, where multiple scattering dominates. A deep, opaque atmosphere might work as well, but it fails to explain the large lightcurve amplitude.

We note that even though evidence for a leaner, smaller, higher albedo Pluto had been in print for well over a year (Seidelmann et al. 1971; Ash et al. 1971), the Iowa group still clung to the notion of a larger, darker planet. This appears to be a common thread in all the Iowa contributions. Perhaps the reason for the Iowa group's preoccupation with a terrestrial-like composition for Pluto can be traced to William Lane's (1975) M.S. thesis, and the paper which followed (Lane et al. 1976). Iowa's 60-cm telescope was used for a total of 16 nights during the 1974 and 1975 apparitions to observe Pluto with a chopping, GaAs PMT photometer. The motivation for the experiment was as follows:

> The *UBVRI* measurements of Pluto made by Hardie (and reported in Harris 1961) are somewhat useful for comparing Pluto's reflectance with that of other solar system bodies and meteorites. However, Hardie's *I* filter was located at a wavelength just shortward of the blue side of the one micron absorption that occurs in the spectra of many asteroids and meteorites so that a measurement of Pluto's reflectivity redward of Hardie's *I* filter should be valuable for comparing Pluto to other objects. We have, therefore, measured Pluto's relative reflectance at...an effective wavelength of 8620Å and bandpass of 400Å...
>
> When combined with the *UBVR* measurements of Hardie, the $(V - I)$ reported here shows that the reflectance of Pluto continues to increase beyond 0.8 micron. Linearly sloping reflectance curves, increasing toward the red, are characteristic of metals such as iron

> and nickel. We have compared the reflectance of Pluto to spectra of a number of asteroids and to meteorites... These comparisons show that there are solar-system objects whose reflectances resemble that of Pluto between 0.3 micron and 0.86 micron. The best matches are to the asteroid 5 Astrae, an iron meteorite with low nickel content, and a stony iron meteorite."

Had the instrument's response been shifted just a bit more toward the red, Lane would have stumbled upon the 8900Å methane absorption band, and the spectrum would have turned downward. The supposed "match" with stony/iron objects would not have been so close in this case. One can only speculate as to what candidate materials and analogs Lane and collaborators would have considered for this case. Unfortunately, the small aperture of their telescope and the relatively low response of their detector compared to modern CCDs precluded them from obtaining more than one wavelength measure in the near-IR. The true character of Pluto's spectrum would have to wait for another couple of years.

J. Neff ventured to Kitt Peak to obtain additional photometry of Pluto during April of 1973 with the old No. 1 36-inch telescope. On four consecutive nights, 28 measurements were made in Johnson $V$. Neff iet al. (1973,1974) used these data to show that the short period originally proposed by Kiladze (1966) indeed was spurious, and to refine Pluto's synodic period to 6.38737±0.00019 days. The times of mean light crossing were used to do this, dating back to the original Kuiper observations (Walker and Hardie 1955). Marcialis (1983) demonstrated there could be systematic phase drift in this quantity as sub-Earth latitude changed. This in turn would affect the derived period, amounting to a few hundredths of a rotation. However, the increased temporal baseline provided by the new observations afforded refinement over Hardie's (1965) value. This value for Pluto's synodic period went unchallenged until the early 1980s. In fact, upon the discovery of 1978P1, Christy and Harrington (1978) adopted it as their provisional period for Charon about Pluto.

In his Ph.D. thesis entitled "A photometric study of Pluto and satellites of the outer planets," Andersson (1970) presented $UBV$ photoelectric photometry of Pluto and 12 satellites of the jovian planets obtained from 1970–1973. In most cases, rotational light curves and linear phase coefficients resulted. Additionally, Andersson observed photoelectrically the comparison stars used by many early investigators, allowing more reliable conclusions to be drawn about the long-term behavior of the planet's brightness. The document is a classic for outer solar system observers. In general, the data are unmatched until CCD technology arrived more than a decade later.

The rotational period of Hardie (1965) was verified. In 1972, Pluto was observed from McDonald Observatory over nine nights in January, five nights from Arizona's Catalina Observatory in May, and three more nights (KPNO) at the end of the year under conditions which were photometrically suspect.

What resulted was the third complete light curve of Pluto, and the first since Hardie's in 1964. A linear phase coefficient of 0.05 mag $\mathrm{deg}^{-1}$ was derived from these data, consistent with, but independent of, the value Andersson and Fix (1973) had determined by minimizing the scatter in the Walker and Hardie data.

Pluto's light curve was continuing its long-term evolution, with amplitude, asymmetry, and mean magnitude all increasing. The "secular dimming" was found to extend back to the discovery apparition of the planet. While less certain, based upon his new data Andersson also suspected that Pluto might be reddening slightly as it dimmed. Andersson's new photometry was pivotal to the pole determination discussed earlier (Andersson and Fix 1973).

Upon completion of his graduate research, Andersson moved on to a position at the University of Arizona's Lunar and Planetary Laboratory, where his contributions to the study of Pluto continued. There he influenced many of the people who would later take up the challenge of Pluto observations: D. P. Cruikshank, W. Hubbard, D. Hunten, U. Fink, C. Benner, L. Lebofsky, E. Tedesco, and George and Marcia Rieke, to name a few. These people formed the core of Pluto researchers in the 1980s, and their students in turn comprise a substantial component of today's active investigators. Very shortly after the discovery of Charon (Christy and Harrington 1978), Andersson (1978) was the first to realize that a satellite implied mutual events in his DPS abstract:

> "Attempts to fit an orbit...to the Pluto satellite...indicate that although the orbit orientation ambiguity is not resolved, the $\Omega = 170°$ solution (satellite approaching at northern elongation) fits the observations better than the $\Omega = 170°$ one. If Pluto's equator and satellite orbit plane coincide the photometric determination of the rotation axis by Andersson and Fix...also slightly favors the $\Omega = 170°$ alternative. In the $\Omega = 350°$ case, Sun (and Earth) passed through the satellite orbit plane in 1968±5 yrs. Eclipse phenomena would occur during five or six apparitions around the time of passage through he plane. Thus the probability is high (~50%) that eclipses etc. will occur during the 1979 apparition...Photometric observations of Pluto in 1979 are very strongly urged. Wide distribution of observers in longitude is desirable."

This lays the framework for what became the Pluto–Charon Mutual Events Season Campaign, largely established by Tedesco and Tholen, whose years at LPL began about this time. Unfortunately, Andersson never lived to see the Pluto–Charon mutual events. He succumbed to lymphoma in May, 1979 at age 35.

## VIII. ENTER THE MODERN ERA

It is fuzzy and somewhat arbitrary as to what delineates the beginning of the "modern" era in Pluto research. I would put it at the time of Charon's discovery in 1978. Others would define it earlier, at the time when scientific papers began to appear annually, i.e., when Pluto began to be scrutinized at every apparition. In actuality, there is no hard demarcation in the continual process of scientific inquiry. The year 1976 certainly falls within the window of possible dates, and is the year chosen by the Editors of this book. For 1976 was the year methane was discovered on Pluto.

Almost overnight, this discovery precipitated nearly universal alignment with Kuiper's (1949,1950) ideas of the ninth planet: a small, icy body of relatively high albedo and low mass, distinct from both terrestrial and Jovian planets. Equilibrium and disequilibrium chemical models by Lewis (1972) gave new weight to Kuiper's opinion. Cruikshank, then at the University of Hawaii, was a disciple of Kuiper's while at LPL; he teamed up with D. Morrison and C. Pilcher and returned to Tucson to put these theoretical models to the test.

Its intrinsic faintness, combined with the relatively low efficiency of infrared detectors of the time, made spectroscopic measurement of Pluto at anything near laboratory resolution an impossibility. To circumvent the problem, a set of broad- to medium-resolution filters was selected. These filters effectively comprised a "truth table" through which one could distinguish between the various ices expected to be abundant in the outer solar system. Cruikshank et al. (1976) report:

> "...We observed Pluto with the 4-meter Mayall telescope at Kitt Peak National Observatory in March 1976, measuring the reflectances of the planet with standard $JHK$ filters and with two specially made narrow filters centered at 1.55 and 1.73 $\mu$m, (designated $H1$ and $H2$, respectively), the appropriate positions of the diagnostic absorptions of $H_2O$ and $CH_4$ frosts. The measured broadband colors for Pluto, expressed as magnitude differences, were $J - H = 0.2 \pm 0.1$ and $H - K = -0.4 \pm 0.1$. Although the $H - K$ color is that expected for a frost, the $J - H$ value is considerably more positive than that expected for an object covered with water frost. The $H1/H2$ reflectance ratio for Pluto is also inconsistent with water frost. From laboratory spectra, this ratio should be about 0.5 for a body covered with pure $H_2O$ frost and illuminated by the sun, while pure $CH_4$ frost should yield $H1/H2 \sim 2.5$. A solar-illuminated surface should yield a value of 0.99. The reflectance ratio obtained for Pluto was $H1/H2 = 1.6 \pm 0.1$. Rhea, a satellite of Saturn known to be largely covered with water frost, yielded $H1/H2 = 0.6 \pm 0.1$, the expected result.
>
> ...On the basis of Pluto's $H1/H2$ ratio, its $J - H$ color, and the restriction imposed by the other observational and theoretical studies

> cited above, we conclude that $CH_4$ frost is probably the dominant reflecting material on Pluto's surface. The fact that the $H1/H2$ ratio is less for Pluto than or a pure laboratory sample of $CH_4$ frost suggests that the frost on Pluto's surface either is mixed with other materials (such as silicates, carbonaceous material, or other frozen volatiles), or has a different grain size distribution than the laboratory sample. . . If so, the mean density is likely to be in the range of 1 to 2 gm $cm^{-3}$.

Figure 10 shows the relative reflectances for laboratory samples of potentially abundant frosts, with the approximate bandpasses of the five filters superposed. This "truth table" technique has been used several times since the original Cruikshank et al. observations to diagnose surface compositions of outer solar system bodies (e.g., Triton; Cruikshank et al. 1977), and even in the first diagnosis of the dichotomy in surface compositions between Pluto ($CH_4$) and Charon ($H_2O$) (Marcialis et al. 1987).

## IX. SUMMARY

I have attempted to review most of the significant contributions in the first half-century of Pluto–Charon research. Many of the unpublished results were dredged up through careful reading of the published materials and, in some cases, conversations with the original players. It is my hope this review will encourage those knowing the whereabouts of the unpublished results, original notes, and old equipment discussed in this chapter might share this knowledge, and that they will bring these historical artifacts to my attention.

Rather than strict adherence to the "official" literature of the subject, much effort has been expended in trying to uncover the "human" aspects of the characters involved. In many cases, I have used the original investigators' own words, hopefully to good effect. I have tried to "get into their heads," interpolating between the lines in a reasonable manner. I have attempted to preserve the theme of temporal continuity whenever possible. The result is, I hope, a unified picture of how our understanding of Pluto has grown, and how it relates to progress in other branches of astronomy.

Subdividing advances into their component disciplines would have been much a simpler tack (see Stern's [1992] review for such an approach), but knowledge of how the parts integrate, interact, and influence one another leads to greater comprehension. Additionally, the ties between Pluto and the study of other planets have been discussed where appropriate.

Many "modern" ideas regarding the Hadean system are in fact recurrent ones. Our ancestors were much more clever than we tend to think. It has been said that those who don't understand the past are doomed to repeat it. I prefer the view that those who do understand the past may succeed where earlier attempts have failed.

*Acknowledgments.* This chapter is dedicated to the scientists whose labors made its subject possible. In particular, thanks to a couple of Bobs no longer with us: Hardie and Harrington. Each rejuvenated the field at crucial times when it had become stale. Both were friends and role models. Both are missed. I acknowledge the Libraries and staff of Kitt Peak, the Lunar and Planetary Laboratory, and the Lowell Observatory for allowing me to access their wealth of resources. In particular, I thank my friend J. Spencer, who checked Lampland's diary to resolve some discrepancies which exist in the literature, and for locating some (now) hard-to-find references. W. Merline ran the simulations to produce Figure 4, and made helpful suggestions to improve this chapter. D. Levy's constructive review of the chapter was very helpful, as he had independently addressed many similar issues during the preparation of his excellent biography of Tombaugh. Most especially, deep appreciation and regards to my friend Clyde Tombaugh on the occasion of his ninetieth birthday; without his tireless and careful efforts, this book would not exist.

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## FIGURE CAPTIONS

Figure 1. (*a*) The earliest orbits for the new planet were very inaccurate, as this diagram from Crommelin (1930*c*) shows. The original caption reads: "Diagram of the published orbits for Planet X. The full curve represents an orbit by the Lowell observers based on two months' observations. The planet is at 1 at the present time, and the slight elongation in the dot indicates the motion in two months. Uranus is at 3, and Neptune at 2. According to this orbit, the planet passed perihelion about 1897, when Uranus was at 5 and Neptune at 4. The lighter broken curve represents the parabolic orbit from the Berkeley observatory, California, and the dotted circle represents an orbit published by the Crakow observatory. All three orbits agree in placing the planet at the present time at about 40 astronomical units. They also agree in the node and the inclination of the orbit plane. The other elements are very uncertain, due to the limited time covered by the observations, and no importance should be attached to the wide divergence of the ellipse, parabola, and circle."

Figure 1. (*b*) By the early summer of 1930, the true nature of Pluto's orbit had been determined. This diagram was prepared by F. Whipple, a graduate student at Berkeley, for an article by Leonard (1930): "...To make the representation of Pluto's orbit more realistic, the plane in which it is drawn should be rotated from left to right along the dotted line AB, through an angle of 17 degrees to the plane of the printed page."

Figure 2. The first attempted light curve of Pluto, by Baade. The data have been phased according Pluto's known rotation period and epoch of minimum light. The vertical bar at the upper left shows the approximate "postdicted" amplitude of Pluto's light curve at the time, according to the spot models of Marcialis (1983, 1988).

Figure 3. Estimated mass of the Pluto–Charon system vs. time. Since the discovery of Charon, the estimates have stabilized, and even slightly increased. Contrary to the prediction of Dessler and Russell (1980), Pluto did not disappear in 1984.

Figure 4. Kuiper's diskmeter "measurement" of Pluto's diameter, made at the 200-in Hale telescope. The line drawings depict his result, and the actual angular configuration and sizes of Pluto and Charon at the time. Image scale in the drawings is very nearly twice that of the simulations, for clarity. Subsequent panels depict simulations of the effects of seeing and other noise sources, for a single source (left) and Pluto–Charon (right), using the "realistic CCD model" of Merline and Howell (1995). "Exposure times" were 1 sec, and the image stretch is logarithmic, to approximate the eye's response. For values of seeing worse than about $0.6''$, Pluto–Charon is easily mistaken for a point source $\sim 0.1''$ wider than the actual seeing. This explains why Kuiper and Humason consistently measured $0.23''$ for Pluto, but only $0.11''$ for a nearby star. Even though Charon was not resolved, it did skew the measurements. (Figure from Marcialis and Merline 1997).

Figure 5. Alter's attempt to reconcile the discordant mass and diameter estimates for Pluto. He performed a simulation demonstrating how specular reflection of collimated light from steel spheres could lead one

to underestimate the sizes of the objects. The upper panels show the experimental setup in diffuse light (left), and diffuse+collimated light (right). In the lower panels, the diffuse source has been removed. The lower left photograph was taken through a 12-in telescope. Alter posited that the real Pluto had an extreme limb darkening law, caused by an extremely smooth surface. (Figure from Alter et al. 1951).

Figure 6. Representations of the 1965 stellar appulse to Pluto. The upper left panel depicts what Halliday et al. thought they were seeing—a 5500-km diameter planet passing $0.143''$ to the south of the star. "×" indicates the photocenter of the Pluto–Charon system, which forms the origin of the (moving) coordinate system. The arrowed line indicates the relative trajectory of the star over 20 minutes, centered on closest approach. The size of the star symbol approximates the quoted uncertainty in its position. Length of the line segments labeled "N" and"E" show the diffraction limit for a 1-m telescope in visible light, while each panel is sized to match a typical seeing disk from the Earth's surface. The remaining panels are from a reanalysis by Marcialis (1996, 1997), with the "light ratio" (Pluto:Charon) indicated in the lower right. In order for there to have been no event, this ratio must have exceeded 6:1, and more likely was 8:1. The dotted circle concentric with Pluto's disk shows the half-light radius observed during the 9 June 1988 stellar occultation.

Figure 7. The Neptune–Pluto libration resonance, as depicted in Cohen and Hubbard (1965). A coordinate frame is chosen which is centered on the Sun, and rotates about it at a rate commensurate with Neptune's mean motion. In this frame, Pluto circulates around the Sun and Neptune in a double-looped curve. Neptune librates around its mean position, much like a pendulum, with an amplitude of 76-deg.

Figure 8. Wavelength dependence of the relative albedo of Pluto. The upturn shortward of 3800Å, and the sawtooth pattern to its right are now known to be spurious. At the time, they were interpreted as evidence for a substantial atmosphere, and absorption features by $\mathrm{Fe}^{+3}$-bearing minerals, respectively. Unfortunately, the wavelength span was not broad enough to include methane absorptions at 7250 and 8900Å.

Figure 9. Andersson and Fix used fourier techniques to try to determine the direction of Pluto's rotational axis. Their solution "footprints" are indicated on this Mercator projection of the sky, along with the (●) North (angular momentum) and (○) South poles as determined from the mutual events. (■) marks the angular momentum pole of Pluto's heliocentric orbit, while the Sun's annual path around the sky—the Hadean ecliptic—is shown by the dashed curve. Very nearly on the ecliptic, (×)s indicate the positions of Earth at those times when Pluto's rotational light curve was measured. Beginning at $\alpha = 19^h$, and moving counter-clockwise around the sky, these epochs are: 1932, 1954, 1964, 1972, 1980, 1986, 1996, and (hopefully) 2000. As Pluto is near perihelion, the Sun (and Earth) have moved nearly halfway around Pluto's sky in just 65 yr. Coordinates are J2000.

Figure 10. Near-infrared spectra of cosmically abundant ices (from Fink and Sill 1982), along with the passbands of standard $JHKL$ filters. Cruikshank et al. (1976) used $JHK$, along with two custom-made filters, $H1$ and $H2$, to discriminate between these (and other) candidate materials on Pluto. Methane was identified in the experiment, reviving Kuiper's idea of a smaller, ice-covered, high-albedo Pluto.