ORBIT DETERMINATION AND EVOLUTION OF COMET C/1995 O1 (HALE–BOPP) *

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Abstract. The situation leading to the determination of the Hale–Bopp orbit is discussed, largely in terms of a procedure that generates two sequences of parabolic orbits. The comet is also considered in relation to the problem of the possibility of impact on the earth. The placement of its orbital nodes near the orbits of the earth and Jupiter is clearly an intriguing feature. The role of the prediscovery observation in 1993 is described, as it appeared both as a boon and a burden. Although evidence has been put forward that the Hale–Bopp nucleus is unusually large, it seems likely that nongravitational forces are noticeably affecting the comet's motion. While discussion of the comet's future long-term motion may be amenable to the usual treatment as a problem of diffusion, it is not entirely improbable that the present situation arose from a recent dramatic approach to Jupiter. It is shown that such a Jupiter encounter in June -2215 is not inconsistent with the non-existence of records at the comet's last perihelion passage, which could then have been the first to occur as close as 0.9 AU to the sun. Of course, the Jupiter encounter might also have given rise to the possible large satellite to Hale–Bopp discussed by Sekanina.

1. Introduction

Whenever a new comet is discovered, our lack of knowledge about it is such that, for a fleeting moment, we can*not* say that the comet will *not* collide with the earth. One of the many interesting points about the comet discovered on 1995 July 23 is that, despite the availability of several dozen excellent astrometric observations, there existed for at least a day or two a small – though decidedly nonzero – probability that such a collision would occur. Binzel (1997) has suggested that every discovery should be categorized according to an "impact hazard scale", in the perhaps rather naïve expectation that a nonhazardous score will prevent the appearance of misleading assessments in the press. I have strongly questioned (Marsden, 1997a) the utility or practicality of such an exercise. Nevertheless, despite the fact that the possibility of a collision was not advertized, at least one press story (Matthews, 1995), commenting on the uncertainty of our knowledge of the orbit of this comet a full week after the discovery, *did* suggest that one could occur, adding – perhaps rather extravagantly – that the nucleus of this particular comet could be as much as 1000 km in diameter.

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There was a certain irony in the fact that comet C/1995 O1 was discovered visually by amateur astronomers when as bright as tenth magnitude and only 22° from opposition at ecliptic latitude $-9°$. The opposition region is, of course, the prime target of professional searches for NEOs, “near-earth objects” that might be a threat to the earth. Largely in anticipation of the conversion to CCD surveys, both wide-field photographic surveys for NEOs – by Helin and by Shoemaker – with the 0.46-m Schmidt at Palomar had been terminated just months earlier, while the Anglo-Australian Near-Earth Asteroid Survey at Siding Spring was also, regrettably, heading toward its demise at the end of 1996. At the time, Spacewatch was the only CCD search game in town, but its rate of sky coverage is less than one-tenth that of the old photographic surveys. The essential simultaneity of the independent discoveries (Hale and Bopp, 1995), coupled with an independent detection by another amateur astronomer the following night, was at least partly due to the comet’s proximity to a popular globular cluster in the Messier catalogue, but there was also justifiable speculation (e.g., Sekanina, 1995) that the comet had been found because it had experienced an outburst.

In the following sections I discuss in more detail the initial questions involving the establishment of the comet’s orbit, the early demonstration that the comet was not in outburst – and that it could rather confidently be expected to become, more than a year and a half later, one of the greatest comets of the century. There is consideration of the possibility that the orbit is noticeably affected by nongravitational forces, and some conclusions are drawn about the comet’s long-term motion.

2. Initial Deductions Concerning the Orbit

With an apparent daily motion of 9 arcmin westerly in ecliptic longitude and 2 arcmin northerly in ecliptic latitude, the comet might easily have had the orbit of a typical asteroid. Although the tendency may be to avoid such orbit solutions for comets, it is worth remarking on the discovery a year later (Elst and Pizarro, 1996) of what from its orbit was obviously an asteroid, now catalogued as (7968), in the Themis family – yet it clearly exhibited a tail, causing it *also* to be catalogued as comet 133P/1996 N2. While that tail was, as anticipated, a temporary phenomenon (Boehnhardt and Offutt, 1997), the complete absence in that instance of any coma obviously represented a clear physical difference from any normal comet. In any case, the availability of near-simultaneous astrometric observations on July 24 from both Australia and Japan (Garradd and Nakano, 1995) made it immediately clear from the absence of differential parallax that comet C/1995 O1 had then to be at a geocentric distance $\rho \geq 2.0$ – and thus at a heliocentric distance $r \geq 3.0$ AU. These July 24 observations also showed that the *minimum* possible values for the orbital semimajor axis and inclination to the ecliptic were $a \sim 3.4$ AU and $i \sim 6°$. Although the minimum eccentricity for $\rho = 2.0$ AU was $e \sim 0.15$, smaller eccentricities could be achieved if the object were somewhat farther away, the circular solution corresponding to $\rho \sim 2.6$ AU, $a \sim 3.6$ AU, $i \sim 7°$.

To allow $i \geq 10°$ required $\rho \geq 3$ AU. While the likelihood that the object was an “active asteroid” could be rejected, the persistence of the low inclination at these distances *did* mean that one should at least consider the possibility that the comet was a short-period one of small to moderate eccentricity. However, if ρ were even moderately greater than the circular value, the object would have had to have come significantly closer to the sun in earlier years (unless it had just been thrown inward as the result of a passage near Jupiter), and at some point the occurrence of an outburst far from the sun becomes less probable than the failure to discover a fainter comet during a prior perihelion passage. In any case, as the observed arc increased, so did the minimum value of ρ that was consistent with the observations, and by the time the accurate positions covered some two days, it was clear that $\rho \geq 3$ AU, and the possibility that the comet was of short period could be abandoned.

So it was appropriate to concentrate on parabolic orbital solutions. Nevertheless, the range of possible parabolic solutions was considerable. In a recent paper about the problem of securing adequate orbital data for Kuiper Belt objects (Marsden, 1998a), I demonstrated a procedure that can very conveniently be adapted for the purpose of exploring possible parabolic solutions. Given, as we were, only four independent pieces of information (the comet’s elongation from opposition and ecliptic latitude, its daily motion in ecliptic longitude and latitude), we require one more piece of information in order to compute a parabolic orbit. A common practice in any orbital calculation that is clearly indeterminate is to compute a series of orbits on the assumption that the object is at perihelion or aphelion (Väisälä, 1939), i.e., to assume that the heliocentric radial velocity $\dot{r} = 0$. The parabolic “Väisälä orbit” from the July 24 observations of the Hale–Bopp comet required $\rho \sim 8.7$ AU, which in turn meant $i \sim 151°$ and that the perihelion distance would have been $q \sim 9.7$ AU. While this would have set new records for both perihelion distance and intrinsic cometary brightness, further interest in the comet would have been relatively minor. As ρ was increased beyond the “circular” value at 2.6 AU, the Väisälä computations yielded a long series of aphelic solutions with increasing i; these reached a maximum eccentricity of almost 0.9 near $\rho = 5.9$ AU (with $i \sim 75°$), beyond which there was a retrograde circular solution near $\rho = 7.9$ AU.

This orbit-exploration procedure is set up in such a way that any choice of ρ immediately yields values for all three components of the object’s heliocentric position vector and two of the components of the heliocentric velocity vector. If $\dot{r} = 0$, the remaining velocity component – call it $\dot{x}$ – is therefore also defined. But $\dot{x}$ is in reality a second parameter for which we should select a series of values. At one extreme, we can take $\dot{x} = 0$. This choice will therefore yield the smallest possible heliocentric velocity for the selected ρ, and by the conservation of energy, it also yields the smallest possible a. Increasing $\dot{x}^2$ will thus increase a, so that if ρ in fact allows elliptical solutions, there must also be two parabolic solutions, one for positive and one for the corresponding negative $\dot{x}$. Beyond the ρ that yields the

TABLE I

Parabolic orbital solutions

	Postperihelion			Preperihelion		
ρ (AU)	T (yr)	q (AU)	i (deg)	T (yr)	q (AU)	i (deg)
2.0	1995.03	1.21	6.1	1996.07	1.99	7.6
2.5	1994.89	1.28	7.0	1996.23	2.09	9.2
3.0	1994.76	1.25	9.0	1996.39	2.06	11.4
3.5	1994.65	1.11	12.7	1996.54	1.90	14.6
4.0	1994.54	0.90	18.8	1996.68	1.64	19.4
4.5	1994.44	0.68	29.1	1996.79	1.33	26.7
5.0	1994.34	0.50	46.4	1996.89	1.04	38.1
5.5	1994.21	0.46	72.0	1997.00	0.83	55.6
6.0	1994.01	0.64	98.7	1997.15	0.83	78.9
6.5	1993.72	1.16	117.6	1997.39	1.15	101.9
7.0	1993.35	2.15	129.2	1997.73	1.94	118.8
7.5	1992.97	3.75	136.3	1998.12	3.37	130.0
8.0	1992.88	6.10	140.9	1998.31	5.64	137.5
8.5	1994.66	9.25	143.7	1996.80	9.08	143.0

perihelic parabolic solution, most possible orbits will rapidly become hyperbolic, there being a limiting ρ with a single parabolic orbit having $\dot{x} = 0$.

Table I shows the two series of parabolic solutions for comet C/1995 O1, one generally postperihelion at the time of discovery and the other generally preperihelion, as a function of ρ. For each entry the values of q and i are accompanied by the perihelion dates T. These particular orbits are derived from observations spanning 1995 July 24–26. The results are qualitatively similar if somewhat longer or shorter arcs are used, although in the latter case the errors associated with the data are – understandably – increased. When $\rho = 2.0$ AU, the parabolic solutions yield perihelion dates some six months before and after the comet's discovery. As ρ increases, each T moves further from the discovery date, i increases – and initially so does q, by up to ~ 0.1 AU around the ρ that also yields the circular solution. Technically, smaller values of ρ, all the way down even to $\rho \sim 0$, yield parabolas that have T values that converge on the time of the observations, but these orbits can be ignored, because it was already clear from the July 24 observations alone that $\rho \geq 2.0$ AU. Given the July 24–26 arc, we can also remove the first two, or maybe three, entries in Table I.

As ρ increases beyond 2.6 AU, the values of q *decrease*. The comet's perihelion point would come *inside* the earth's orbit at $\rho \sim 3.8$ AU on the postperihelic branch and $\rho \sim 5.1$ AU on the preperihelic branch. Minimum values of $q = 0.45$ and 0.80 AU occur near $\rho = 5.4$ and 5.8 AU, while the perihelion point finally recedes beyond the earth's orbit near $\rho = 6.3$ AU on each branch. After that, q increases very rapidly, and extremes of T, roughly 33 months on each side of the discovery date, occur around $\rho = 7.9$ AU. The single limiting parabolic orbit in this particular calculation is around $\rho = 8.54$ AU, at which point $T = 1995.75$, $q = 9.47$ AU, $i = 143.6^\circ$. (The fact that I previously placed the discovery-perihelic parabola at a slightly larger $\rho \sim 8.7$ AU is merely a manifestation of the uncertainty in the computation over the shorter arc.)

Many of the postperihelic parabolas, particularly those corresponding to small q, can obviously be eliminated on the grounds that the comet was not previously observed. The small-q preperihelic parabolas were potentially of great interest. The preperihelic parabolas corresponding to $5.4 \leq \rho \leq 6.2$ AU all mean that there would be an approach within 0.10 AU of the earth's orbit, with the comet near its descending node and roughly one month after perihelion. The minimum distance from the earth itself, given by the preperihelic parabola corresponding to $\rho \sim 5.2$ AU, would be 0.22 AU around 1997 Jan. 10. By the time the observations extended over a two-day arc, it could be stated with some confidence that an approach significantly closer than this distance would not occur. However, such was not the case on July 24, when the uncertainty in the circumstances that corresponded to $\rho \sim 5.2$ AU was such that a collision was certainly possible. The parabolic orbital elements in Table II (with also the usual notations of ω and Ω for the argument of perihelion and longitude of the ascending node), which are very nicely consistent with a series of accurate measurements extending over some ten hours, would nominally bring the comet to within some 0.0005 AU of the earth on 1997 Jan. 9.7 UT. Given the inevitable deviation from a parabola, as well as the 0.01-AU cumulative effect of planetary perturbations over the intervening year and a half, a collision could clearly be contrived. Some of the individual residuals increase beyond 2 arcsec if the arc is extended over 16 hours, while the two-day arc increases the residuals systematically to more than 5 arcsec. Of course, this last situation is unacceptable, given that we were dealing with modern CCD observations of high quality – most of them made, incidentally, by amateur astronomers. But this is just a development of the last few years. Given both the general inferior accuracy and minimal immediate availability of the old photographic observations, the conclusion that there could be no collision would have been much less clearcut even a decade ago.

Principally in order to provide an ephemeris – but also to allow the comet's name to be announced as "Hale–Bopp" – a set of preperihelic parabolic orbital elements was published from the two-day arc. This set (Green, 1995), the minimum-q solution corresponding to $\rho \sim 5.8$ AU in Table I, was labeled *highly uncertain* and might in fact have turned out to be *much* further from the truth than it did! After all, it took a change of more than 1 AU (in either direction) in ρ to increase the mean residual from 0.5 to 0.6 arcsec, and another 1-AU change was needed to increase it to 0.7 arcsec. As it happened, the mean residual of the postperihelic parabolas

TABLE II

Parabolic collision orbit (equinox 2000.0)

T	1996 December 10.10 TT
ω	128.09 deg
Ω	289.49 deg
i	48.93 deg
q	0.7949 AU

TABLE III

Estimated uncertainties

τ (days)	D (arcsec)	$\Delta\rho$ (AU)
3.0	3.0	1.4
4.0	4.5	0.87
5.0	7.0	0.51
6.0	10.5	0.31
7.0	15.0	0.20
8.0	20.5	0.14
9.0	27.0	0.09

increased from 0.7 arcsec at $\rho = 6.0$ AU through 1.0 arcsec at $\rho = 4.5$ AU to 1.3 arcsec and more at $\rho \leq 3.5$ AU, but as already noted, these solutions could be rejected as implausible anyway.

So there remained the question of how long an arc of observations would be required in order that the range of ρ permitting them to be satisfactorily represented could be reduced to, say, within ±0.1 AU. At that point, rather good values for the more interesting orbital elements could be judged from Table I – provided, of course, that the orbit really was approximately parabolic. The principal quantity governing this uncertainty in ρ is the amount by which the comet's apparent motion in the sky deviates from a great circle. For the two-day arc this departure was at most 2 arcsec, comparable to the total range of the random observational errors, and as noted, the range in ρ was perhaps ±2 AU. Table III extrapolates the departure D from a great circle and the $\Delta\rho$ range in distance as functions of the increasing time span τ of the observations.

It therefore seemed likely that the first really meaningful orbit could be computed on Aug. 1, and the orbit published at that time (Marsden, 1995) is reproduced in Table IV. The resulting ρ value was only 0.02 AU larger than the value (6.20 AU) we now know to be correct, and the agreement with Table I can also be noted.

TABLE IV

Parabolic orbit from 8-day arc (equinox 2000.0)

T	1997 April 1.810 TT
ω	129.956 deg
Ω	282.339 deg
i	89.779 deg
q	0.92819 AU

Because there was still the question of whether the comet was in outburst, and indeed, worry whether the comet would reasonably brighten as it approached the sun – some of the spectacular cometary failures of the past half century came to mind! – nothing was immediately said about what might be expected of the comet. Nevertheless, bold use was immediately made of the magnitude formula $-2.0 + 5 \log \Delta + 10 \log r$, at least for the purpose of computing a then-current ephemeris. The comet's minimum distance from the earth would be a rather large 1.3 AU about one week before the 1997 Apr. 1 perihelion date, although with $r = 1.1$ AU at the descending node, an early-December perihelion date would presumably have allowed the comet to be a stupendous sight, quite dominating the night sky, a month or so later. Less anticipated was the fact that the ascending node was at $r = 5.2$ AU, placing the comet virtually on Jupiter's orbit about one year before perihelion passage. The closest approach to Jupiter itself was destined to be about 0.8 AU.

3. Orbital Improvements

After an initial orbit computation for a new comet is known to have errors that are tolerably well confined, the continuing acquisition of further observations makes the repeated computation of further refinements very necessary, if normally nowadays quite routine. At some stage, a meaningful deviation from the assumed parabolic motion usually becomes evident, at which point it may be essential to incorporate the perturbative effects of the planets into the solution.

In the Hale–Bopp case, a very important early development, within hours of the dissemination of the Table IV orbit, was the identification by McNaught (1995) of a likely image, only 9 arcmin from the expected position, on a plate obtained with the U.K. Schmidt in New South Wales on 1993 Apr. 27, when the comet would have been near $r = 13.1$ AU. If real, this slightly diffuse eighteenth-magnitude image, which McNaught had apparently previously marked, but not reported, was of significance for two reasons: it enabled the immediate determination of an orbit that was no longer just a parabola, and it demonstrated that the comet's discovery had not been occasioned by an outburst that would surely subside. A general orbit solution, fitting the 1993 observation perfectly, indicated that the comet had a revolution period of only 3–4 millennia, and with an aphelion distance of some hundreds of AU, C/1995 O1 must therefore have passed 0.9 AU from the sun at least once before. Unlike most of the spectacular cometary failures, it was not on its initial approach from the Oort Cloud, and its previous exposure to strong solar radiation made it likely that it would not disappoint on this occasion. Many long-period comets that are not “new” in the Oort sense are known to have varied in brightness by something approximating an r^{-4} law. My confidence in the aforementioned magnitude formula was increased, and despite the frequent extreme vacillations of other prognosticators, my early conclusion that Hale–Bopp would brighten to a total magnitude of -1.7 turned out not to be an exaggeration by more than half a magnitude. The best previous case with which C/1995 O1 could be compared was the great comet of 1811, which had been a naked-eye object for as long as ten months.

Nevertheless, there was some concern about the 1993 image, because there was no other early image to confirm it. There was some reassurance in the fact that, as the days and weeks went by, the deviation of the 1993 position from orbits based on the 1995 observations alone decreased from 9 arcmin to 4 arcmin to 2 arcmin to 1 arcmin and less. But after three months or so, this deviation refused to decrease below some 10–15 arcsec. Worse, whereas solutions that *did* incorporate the 1993 observation had initially represented it well, it was proving impossible to satisfy it to better than 4–5 arcsec. Orbital elements were also being published by Nakano (e.g., 1995) and by Yeomans (1995), the latter remarking that the “1993 observation of position and magnitude cannot be ruled out but it must be treated with considerable caution”, otherwise “a significant unmodeled perturbation must have been operative over a relatively short time interval”. This prompted McNaught to remeasure the plate, with essentially the same result (within 1 arcsec) as before; furthermore, his measurement nearby of the Phocaea-type asteroid (3343) showed that there could be no mistake in the timing of the exposure.

Although a short-term “unmodeled perturbation”, perhaps due to nongravitational outgassing, had been discussed in connection with attempts to reconcile the 1862 observations of comet 109P/Swift-Tuttle (Marsden et al., 1993), the large heliocentric distance made it seem unlikely that the Hale–Bopp problem could be explained in this way, despite early suggestions (e.g., Matthews et al., 1995) of the possible dominance of highly volatile carbon-monoxide ice, rather than the usual water ice, in this comet’s composition.

By early December 1995, when the last detection was made before the eight-week hiatus around solar conjunction, some 800 astrometric observations of C/1995 O1 had been accumulated. But the 1995 observations were confined to a region of the sky no more than 8° across. By giving each observation unit weight, it seemed to me that the failure to fit the 1993 observation was principally due to the resulting extreme magnification of the systematic errors in the positions of the reference stars (which were then generally from the *Guide Star Catalogue*) in the part of the sky where the comet was in 1995. The situation could presumably therefore be ameliorated, either by giving excess weight to the 1993 observation, or by restricting the 1995 observations to a small but representative sample. We took the first course (e.g., Marsden, 1996a), finding that it was necessary to increase the weight of the 1993 observation to perhaps some 10–12. After postconjunction observations became available in 1996, it was possible to reconcile the 1993 and postdiscovery observations by assigning the former a weight of only 5. This reduction demonstrated the probable correctness of the star-catalogue hypothesis, as the postdiscovery observations extended over a greater area of the sky. The necessary weight continued to decrease as the observed arc increased, and an orbit solution using 1522 observations through December 1996 (Marsden, 1996b) included the 1993 observation with only unit weight and gave an (O$-$C) right-ascension residual for it of -1.1 arcsec, the mean error of the complete solution being 0.7 arcsec. Although statements continued to appear in the World Wide Web to the effect that occultations of stars by the comet's nucleus could be predicted more accurately using orbital solutions that exclude the 1993 observation, the fact is that by this time the 1993 observation had become irrelevant, and it really did not matter whether one included it in the calculation or not.

By the time comet C/1995 O1 reached perihelion more than 2500 astrometric observations had been made. During the four months following perihelion passage, however, only some 100 observations were added, and there were scarcely 100 observations during the month *preceding* perihelion. The latter, in particular, may seem surprising, until one understands that most of the these 100 were made by the more *inexperienced* observers. The observers who are normally the most productive appreciated that Hale–Bopp was so large and bright that it would be essentially impossible to obtain high-quality measurements that could be associated with the comet's nucleus. And so it was! In mid-May, of course, observations were briefly interrupted because of the comet's small elongation from the sun, and subsequent observations have been mainly confined to the southern hemisphere.

Difficulties again arose with early attempts to utilize postperihelic observations in the orbit solutions. It was not immediately clear whether the problem was due to the preponderance of poor observations near perihelion or to the need to consider the influence of nongravitational forces. Certainly, the measured high water-production rates indicate that nongravitational activity was occurring, but the record large sizes proposed for the Hale–Bopp nucleus (Weaver et al., 1997; and especially Sekanina, 1998) made it unclear whether the relative mass-loss would be large enough to yield a measurable nongravitational effect on the motion. After all, Yau et al. (1994) used the argument of possible large nuclear size for the apparent lack of detectable nongravitational effects on comet 109P over more than two millennia, and that large size seems to have been confirmed photometrically (O'Ceallaigh et al., 1995). A gravitational solution (Marsden, 1997b) for C/1995 O1 was able to constrain the mean residual to 0.8 arcsec by the expedient of giving substantially increased weight to the observations in July and August 1997.

TABLE V

Latest general nongravitational solution (equinox 2000.0)

T	1997 April 1.13722 TT
ω	130.58860 deg
Ω	282.47068 deg
i	89.42993 deg
q	0.9141436 AU
e	0.9950791
$1/a$	+0.0053831 ± 0.0000017 AU^{-1}
A_1	+1.04 ± 0.03
A_2	+0.1759 ± 0.0093

A later gravitational solution by Nakano (1997), with observations extending to mid-October, had to accept a mean residual of 1.0 arcsec.

Beginning during the last months of 1997, nongravitational solutions were computed for C/1995 O1 by both Nakano and myself, but we were rather reluctant to publish them, for the reasons outlined above. Nevertheless, it has been becoming rather evident that we do have to accept the nongravitational solutions, which give consistently comparable nongravitational parameters, as well as good fits over the comet's complete observed arc (even to many of the data near perihelion). The total number of available observations is now more than 2900. The latest nongravitational result (Marsden, 1998b), satisfying 2551 unit-weight observations through 1998 Feb. 8 with maximum residuals of less than ±2 arcsec in each coordinate (the right-ascension O$-$C residual of the 1993 observation being -1.8 arcsec) and a mean residual of 0.8 arcsec, is given in Table V. The last four lines show the eccentricity e, reciprocal of the semimajor axis and the radial and transverse nongravitational parameters A_1 and A_2, mean errors being shown for the quantities of particular interest. The nongravitational parameters, in particular the positive A_1, are quite comparable to those determined for several other well-observed long-period comets.

4. Long-Term Motion

For a long-period comet, the standard procedure for examining the long-term motion is to integrate the heliocentric elements for the 20 years or so needed to take the comet somewhat beyond Neptune – in both the past and the future – and to modify the resulting elements so that they refer to the barycenter of the solar system. In doing this, it is usual to restrict one's interest to the values of $1/a$. The significance of such computations was first pointed out by Strömgren (1914), and it was, of

TABLE VI

Original and future orbits

MPC	r (AU)	Error (AU^{-1})	$(1/a)_{\text{orig}}$ (AU^{-1})	P^- (yr)	$(1/a)_{\text{fut}}$ (AU^{-1})	P^+ (yr)
25714	6.57	±0.0000285	+0.003894	4115	+0.005663	2347
26723	5.21	±0.0000060	+0.003834	4212	+0.005603	2384
27079	4.64		+0.003848	4189	+0.005618	2375
27541	3.67		+0.003833	4214	+0.005602	2385
28052	2.70	±0.0000018	+0.003839	4204	+0.005609	2380
28557	1.95	±0.0000013	+0.003837	4207	+0.005607	2382
30428	2.27	±0.0000004	+0.003810	4252	+0.005579	2400
30738	3.03	±0.0000003	+0.003809	4254	+0.005579	2400
31204	4.33	±0.0000017	+0.003835	4211	+0.005572	2404

course, the observed distribution of the preperihelic barycentric $1/a$ values that led to the concept of the Oort Cloud.

The whole point is that elliptical barycentric orbital elements obtained in this manner are – in the absence of perturbations by stars, giant molecular clouds and the Galactic center – essentially constant until the comet returns to (or since it previously was at) a comparable distance from the sun. What the comet actually does during the course of its next or its previous perihelion passage depends on the particular configuration of the planets at those times. For a long-term study of a long-period comet that remains within, say, 1000 AU of the sun, we are therefore dealing with numerous intervals of several millennia when the comet is following near-perfect Keplerian ellipses, punctuated by essentially random, impulsive changes each time the comet passes inside the orbit of Neptune. That said, one may question the need to perform an actual numerical integration of the orbit of a comet like Hale–Bopp for, say, five million years. Nevertheless, such a computation has been published (Bailey et al., 1996), use being also made of small variants of an early determination of the heliocentric orbit at an epoch near the 1997 perihelion passage. As is well known (e.g., Shtejns, 1961), we are dealing with a diffusion process, and for a study over an extended interval of time, it is probably sufficient to consider it as such.

Table VI shows the "original" and "future" values of $1/a$, together with the formal mean error and the corresponding periods P^- and P^+, for several of the orbits computed by Nakano and myself, the references to the relevant *Minor Planet Circular* being shown. The heliocentric distance r applies to the last observation used in the solution. As already noted, the first entry confirms the 3–4-millennium period found as soon as the 1993 prediscovery observation was recognized. The difference between the P^- and P^+, or between $(1/a)_{\rm orig}$ and $(1/a)_{\rm fut}$, partly represents the changing relationship of the barycenter and heliocenter, but mainly the direct effect of the comet's moderate approach to Jupiter in 1996. The first six entries show a steady improvement, with the values apparently converging as the error in $1/a$ decreases with increasing arclength and the comet approaches the sun.

The next two entries, after the gap, include postperihelic observations (with r therefore shown as increasing); the error in $1/a$ continues to decrease, but $1/a$ and P are somewhat different from before, even though there again appears to have been convergence. But we know these solutions to be suspect, because of the failure to allow for nongravitational effects, and experience with other comets suggests that these may also influence preperihelic computations extending to $r < 3$ AU. The last line in the table is from the nongravitational solution shown in Table V. Although the meaning of the empirical model that produced this solution can be questioned, one might take some solace in the agreement of the nongravitational $(1/a)_{\rm orig}$ with that of, say, the fourth entry. The discordance in the $(1/a)_{\rm fut}$ values is understandable, and there is obviously not yet a well-determined gravitational solution exclusively from postperihelic observations with $r > 3$ AU.

While a statistical approach is presumably appropriate for the study of the future motion of comet C/1995 O1, one can argue, of course, that the present situation was brought about in some quasideterminable way in the past. Since this comet presumably *is*, after all, one of the largest to have come inside the earth's orbit in recent times, it may be fair to say that it has *not* done this *very* many times before. In fact, I might be so bold as to speculate that it has done so only *once* before, just 4211 years ago, in the year -2214 (consistent with the last line of Table VI). Interestingly, if perihelion passage had occurred around -2214 July 7, there would have been a near collision with Jupiter around -2215 June 6. Of course, it would be impossible to say what Hale–Bopp was doing before then: the most likely situation would be that the comet's previous period was much longer, corresponding to capture then from the Oort Cloud. With a minimum distance from the earth of about 1.4 AU in -2214, the comet's performance would have been comparable to that in 1997. It is really not unreasonable that there are no records of it, for the date corresponds roughly to the time of the earliest, very sporadic, cometary observations that have been handed down to us.

Speculation is often the ideal way to end a paper. Of course, a devastatingly close approach to Jupiter is a very good way of breaking up a comet, and this -2215 scenario would therefore be ideal for the generation of the 30-km satellite that Sekanina (1997–1999) proposes may accompany the 70-km primary body in an orbit that is still closely bound.

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