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NONGRAVITATIONAL FORCES ON COMETS

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I. EARLIER INVESTIGATIONS OF NONGRAVITATIONAL EFFECTS

The study of the nongravitational effects on comets began slightly more than a century and a half ago. As is well known, Encke (1819) demonstrated that comet 1819 I had a revolution period of not more than a few years and that the same comet had also been observed in 1786, 1795 and 1805. The observations clearly required that the revolution period be about 3.3 years, and Encke went on to remark that, after approximate allowance had been made for the perturbations by the planets, the average revolution period seemed to be 1207.9 days between 1795 and 1805, but only 1207.3 days between 1805 and 1819. As a result of a more refined computation of the planetary perturbations, the following year he (Encke 1820) was able to confirm these figures and find in addition that the average period between 1786 and 1795 was as much as 1208.1 days. It seemed rather clear that for some unknown reason the period was decreasing at a rate of about 0.1 day per period, and further confirmation of this was provided by the observations of the comet--now known as P/Encke--at its first predicted return in 1822.

Utilizing the mean motion n rather than the revolution period, we have

$$n = n_o + (\text{planetary perturbations}) + n_1\ (t-t_o), \qquad (1)$$

where for P/Encke and an epoch t_o in the first part of the nineteenth century, $n_o \simeq 1075''\ \text{day}^{-1}$ and $n_1 \simeq 0''\!.10\ \text{day}^{-1}\ \text{revolution}^{-1}$. In order to explain this secular acceleration term n_1, Encke (1823) postulated that the comet moved under the influence of a resisting medium, the impeding force being

$$-\ Bv = \mu U v^p r^{-q}, \qquad (2)$$

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where r and v are the comet's heliocentric distance and velocity, μ is the product of the gravitational constant and the mass of the sun, and the coefficient U is to be determined from the observations; Encke also assumed that $p = q = 2$. The equations of motion of the comet in rectangular coordinates thus become

$$\ddot{x} + \mu x r^{-3} = \partial R/\partial x + B\dot{x}\,, \tag{3}$$

with $x \to y, z$, dots denoting differentiation with respect to the time, and R being the disturbing function for the planetary perturbations. On application of the method of variation of arbitrary constants (or orbital elements) it follows that

$$n_1 = 3n\, C_{pq}\, M_{pq}, \tag{4}$$

where

$$C_{pq} = Un^p a^{p-q+2} \tag{5}$$

(a being the semimajor axis of the orbit), and

$$M_{pq} = \int_{E_0}^{E} (1 + e \cos E)^{\frac{1}{2}(p+1)} (1 - e \cos E)^{-\frac{1}{2}(p+2q-1)} dE, \tag{6}$$

e being the orbital eccentricity and E(t) the eccentric anomaly.

Encke's actual calculations were made, not in terms of rectangular coordinates, but in terms of orbital elements. He determined n_1 empirically using Eq. (1) and found that it was essentially constant, at least until his own investigations terminated with the apparition of 1858 (Encke 1860).

Asten (1878) then established that the observations of P/Encke up to and including those in 1868 could be satisfied with the constant value $n_1 = 0\overset{''}{.}1044$ day^{-1} revolution^{-1} (giving $U = 1/862$), and he also found that there appeared to be a secular variation in the eccentricity--or more specifically in $\phi = \arcsin e$. If we denote this secular variation by ϕ_1, defined by an expression analogous to Eq. (1):

$$\phi = \phi_o + (\text{planetary perturbations}) + \phi_1\,(t-t_o), \tag{7}$$

it follows from the resisting-medium hypothesis that ϕ_1 is given by an expression analogous to Eq. (4), namely,

$$\phi_1 = - \cot\phi \; C_{pq} \; (M_{pq} - N_{pq}), \tag{8}$$

where

$$N_{pq} = \int_{E_o}^{E} (1 + e \cos E)^{\frac{1}{2}(p-1)} \; (1 - e \cos E)^{-\frac{1}{2}(p+2q-3)} \; dE. \tag{9}$$

Hence

$$\frac{\phi_1}{n_1} = - \frac{\cot\phi}{3\,n} \; (1 - \frac{N_{pq}}{M_{pq}}). \tag{10}$$

The integrals in the expressions for M_{pq} and N_{pq} may conveniently be worked out in terms of elliptic functions, and for p = q = 2, it can be shown that

$$M_{22} = \tfrac{1}{3}[32E\,(1 + e^2)(1 - e^2)^{-2} - 4K\,(5 + 3e^2)(1 - e^2)^{-1}], \tag{11}$$

$$N_{22} = 8E\,(1 - e^2)^{-1} - 4K, \tag{12}$$

where K and E (modulus e) are the complete elliptical integrals of the first and second kind, respectively. For P/Encke, with e = 0.8463, we have K = 2.100, E = 1.232, and with Asten's empirical value of n_1 it follows from Eq. (10) that $\phi_1 = - 3''.68$ revolution^{-1}. Since this is in fact almost precisely equal to Asten's empirical value of ϕ_1, it seemed that the resisting-medium hypothesis had been amply demonstrated as the correct explanation for the nongravitational effect. Backlund (1884) showed that the observations were compatible with any resisting-medium law in which $p + q \not> 3$, and that as $p + q \to \infty$ the theoretical value of ϕ_1 tends toward $- 3''.86$ revolution^{-1}.

Furthermore, although the resistive coefficient U determined by Möller (1861) in the case of P/Faye was some two orders of magnitude larger than that found for P/Encke, the ratio ϕ_1/n_1 again seemed to be consistent with the resisting-medium hypothesis. Möller (1865) withdrew his result, however, and he was subsequently unable to detect any significant nongravitational effects on P/Faye; but then Oppolzer (1880) established for P/Pons-Winnecke that although ϕ_1 could not be measured, the value of U was the same as for P/Encke.

Asten (1878) also found, however, that it was impossible to represent the 1871 observations of P/Encke in the framework of his study of the motion during 1819-1868, and after much deliberation he concluded that this was perhaps due to perturbations by the minor planet (78) Diana. Backlund (1884) showed that the observations of P/Encke during 1871-1881 were in fact compatible with the resisting-medium hypothesis, but that n_1 (or U) was substantially smaller than before. Later he claimed (Backlund 1910) that relatively sudden changes had taken place, with n_1 decreasing from $0\overset{''}{.}13$ to $0\overset{''}{.}08$ day^{-1} revolution^{-1} in 1858, to $0\overset{''}{.}06$ in 1871 and to $0\overset{''}{.}04$ in 1895; there was possibly another jump in 1904, and furthermore, there seemed to be evidence for a small periodic variation in n_1 during 1819-1858. While retaining the basic idea that the secular acceleration was due to a resisting medium, Backlund felt that the comet should be regarded as experiencing collisions of short duration, perhaps with a ring of meteoric material orbiting the sun. Backlund's investigations were continued by Matkevich (1935) and Idel'son (1935), and more recently Makover (1955) assumed that the nongravitational perturbation took the form of an impulse acting on the comet exactly at perihelion. The most recent calculations of this type (Bokhan and Chernetenko 1974) show that n_1 has now decreased to only $0\overset{''}{.}01$ day^{-1} revolution^{-1}.

Similar computations during the nineteenth century on other comets were not at all conclusive. The conflicting results for P/Faye have already been mentioned, and in a later study on P/Pons-Winnecke, Haerdtl (1889) suggested that there was no secular acceleration. Leveau (1877) did not seem to think it necessary to introduce any secular variations into the orbit of P/d'Arrest; and Rahts (1885) fitted the 1858, 1871 and 1885 observations of P/Tuttle, and Gautier (1887) the 1867, 1873 and 1879 observations of P/Tempel 1, by gravitational theory alone. Schulhof (1898) suspected a slight secular acceleration in the case of P/Tempel 2, but he later found his calculation of the perturbations to be erroneous (Schulhof 1899). After P/Encke, P/Biela seems to be the first comet for which a reasonably convincing secular acceleration was established (Hepperger 1898). Maubant (1914) found a secular acceleration for P/Tempel-Swift. Lamp (1892) confirmed the calculation by Schulze (1878) of the perturbations on P/Brorsen during 1873-1879 and was forced to conclude that the error of 0.6 day in the predicted perihelion time in 1879 implied that this comet had experienced a large secular <u>deceleration</u> (i.e., n_1 was negative); this could certainly not be explained on the basis of the resisting-medium hypothesis. The 3-day error in the predicted perihelion time of P/Halley in 1910 (Cowell and Crommelin 1910) was also suspected as being due to a nongravitational secular deceleration, in spite of Brady's (1972) conclusion that this comet was instead being perturbed by a planet having the mass of Jupiter and traveling in a highly inclined orbit at twice the distance of Neptune. (The existence of such a planet can be ruled out on several grounds: e.g., Klemola and Harlan 1972, Goldreich and Ward 1972, Seidelmann <u>et al</u>. 1972, Kiang 1973.)

One of the difficulties with the older calculations is that the investigators frequently made solutions for the planetary masses at the same time. In his investigations on P/Encke, Backlund used values of the sun : Mercury mass ratio ranging from less than 3 000 000 to almost 10 000 000; and Haerdtl's dicussion of P/Pons-Winnecke led to 1047.1752 ± 0.0136, rather than a value closer to 1047.35, for the sun : Jupiter mass ratio. More recently, Rasmusen (1967) found that one could eliminate the need for nongravitational effects on P/Olbers (and apparently also on P/Halley) by changing the latter mass ratio to 1051 (!). In any case, approximations were made in the old calculations, and there are obvious problems with the observations of diffuse comets; many astronomers have therefore been skeptical of the results (e.g., Roemer 1961). Nevertheless, modern calculations confirm the general correctness of the old results for P/Encke, P/Pons-Winnecke (Oppolzer's figures), P/Biela, P/Tempel-Swift, P/Brorsen and P/Halley, and they confirm that the nongravitational effects on P/Tempel 1 and P/Tempel 2 are very small. Nongravitational effects ought to have been detected in the nineteenth century for P/Faye and P/Tuttle, and particularly for P/d'Arrest, although examination of the motion of this last comet was rendered difficult by a close approach to Jupiter in 1861, by the missed returns of 1864 and 1884 and the severe discordances among the observations in 1877.

In more recent times Recht (1939) established that P/d'Arrest has a definite secular deceleration, and Kamieński (1933) found a slight deceleration in the case of P/Wolf--at least before a close approach to Jupiter in 1922 caused a substantial increase in the perihelion distance of this comet. Dubyago (1950) found a large secular acceleration in the case of P/Brooks 2, and Evdokimov (1963) a large secular deceleration for P/Giacobini-Zinner. Sitarski (1964) derived a slight acceleration for P/Grigg-Skjellerup and more recently (Sitarski 1970) a larger acceleration for P/Wolf-Harrington.

As for comets having somewhat longer periods, Schubart (1968) found it necessary to assume a secular deceleration in order to fit the prediscovery observations of P/Tempel-Tuttle in 1699 and 1366; and Herget and Carr (1972) confirmed that the error in the prediction for P/Pons-Brooks at its 1954 return (Herget and Musen 1953) must have been due to a nongravitational secular acceleration.

II. MODERN METHODS FOR THE STUDY OF NONGRAVITATIONAL EFFECTS

Although the equivalent of Eq. (3) was written down long ago (Encke 1831), all the studies mentioned in Sec. I were done essentially by considering Eq. (1), and in some cases by considering also Eq. (7) and possibly also even similar equations in other orbital elements: for a further discussion on this point see Sekanina (1968). Our own initial study (Marsden 1968) was made basically in the same manner and differed from the earlier investigations only in that it included a treatment of as many as 18 comets in as uniform and rigorous a manner as possible. The possibility of using Eq. (3) directly was briefly considered at that time, but before any computations were actually done we decided (Marsden 1969) to generalize it to

$$\ddot{x} + \mu x r^{-3} = \partial R/\partial x + F_1 x r^{-1} + F_2(r\dot{x} - x\dot{r})h^{-1} + F_3(y\dot{z} - z\dot{y})h^{-1}, \quad (13)$$

where $h^2 = (y\dot{z} - z\dot{y})^2 + (z\dot{x} - x\dot{z})^2 + (x\dot{y} - y\dot{x})^2$, and the F_i obviously represent three rectangular components of the additional nongravitational force, with F_1 directed along the radius vector, F_2 also in the orbit plane (along the velocity vector at perihelion or aphelion and if the orbit is circular), and F_3 normal to the orbit plane.

It is important to point out that it was not our desire to favor any particular theory concerning the true physical nature of either the nongravitational forces or of comets generally. We felt that Eq. (13) was of a sufficiently general form for deriving useful information about the nongravitational forces whatever their cause, and that even if the forces were entirely impulsive and discontinuous, this fact would become apparent from our studies.

Some experimentation was carried out as to the dependence of the F_i on r. It was quickly established that a simple r^{-2} or r^{-3} law was unsatisfactory, for beyond some 2 to 3 AU from the sun the nongravitational forces diminished very considerably. This was evident both from attempts to link successive apparitions of a number of comets and from the fact that the motions of the short-period comets of largest perihelion distance (notably P/Oterma and P/Schwassmann-Wachmann 1) seemed to be entirely unaffected by any nongravitational influence. Somewhat at random, we selected the form $r^{-3} \exp(-r^2/2)$, with r measured in AU, and the fact that solutions made using this law for various comets and over various timespans yielded results that were generally very regular and consistent (Marsden 1969, 1970; Yeomans 1971; Marsden and Sekanina 1971) gave us some confidence that the extraneous forces with which we were dealing were basically continuous, rather than impulsive, in nature.

Among the regularities was the fact that the radial component was generally directed outward from the sun and perhaps an order of magnitude larger than the transverse component, which was equally likely to be in either direction; the normal component was never significant. There were changes with time, but particularly in the case of the better-determined transverse components, it seemed that these changes were generally very smooth and uniform: contrary to Backlund, we concluded that a sudden change in the nongravitational influence on a comet was a relatively rare phenomenon.

Interaction with a resisting medium, or indeed any kind of collisional process, would therefore seem to be ruled out, at least as the basic cause of the nongravitational forces. The nonrandom form, and the absence of any nongravitational influence, not only on the orbits of comets of large perihelion distance, but also on those of some particularly stellar-looking comets of smaller perihelion distance (e.g., P/Arend-Rigaux and P/Neujmin 1), make it extremely difficult to argue in favor of any kind of "sandbank" model for comets. Of the cometary models that have ever been seriously proposed this leaves only the icy-conglomerate model (Whipple 1950) and the supposition that the nongravitational forces are reactive in nature (as in fact was first suggested by Bessel 1836). The general predominance of the radial component, and the fact that it usually acts outward from the sun, are definite points in favor of this model. This finding is actually an added bonus, for it implies that the angle by which the direction of maximum mass ejection from the comet lags behind the subsolar point would generally be very small, a point that is certainly not obvious when one considers the general problem of heat conduction through a rotating icy cometary nucleus (Whipple 1950).

Several other, completely different avenues of cometary research have led in recent years to the strong likelihood of the correctness of some kind of icy-conglomerate model. Delsemme and Miller (1971) have examined the problem of the variation of the rate of sublimation of possible ices in comets with heliocentric distance, and Delsemme and Delsemme (1971) were able to fit the function

$$g(r) = \alpha \left(\frac{r}{r_o}\right)^{-m} \left[1 + \left(\frac{r}{r_o}\right)^{n}\right]^{-k} \qquad (14)$$

to their sublimation curve for water ice within ± 5 percent over the range of heliocentric distance 0.1 to 4.0 AU. This formula, and their numerical values ($m = 2.15$, $n = 5.093$, $k = 4.6142$, $r_o = 2.808$ AU, together with the normalizing coefficient $\alpha = 0.1113$), have been used in all the more recent calculations of cometary nongravitational parameters A_1 and A_2 (Marsden et al. 1973, Marsden and Sekanina 1974, Yeomans 1974), these parameters being defined by

$$A_i = F_i\, g(r) \qquad (i = 1,\ 2) \tag{15}$$

and determined from observations covering spans of time during which they can be treated as constant. Delsemme and Miller (1971) assumed that the visible absorptivity κ and the infrared emissivity ε of the water ice were each 0.9, and it can be shown that the results would be essentially the same whenever κ and ε have identical values. Changes in absorptivity and emissivity, as well as to other ices, can be handled, very simply but to a good degree of approximation, by retaining the above values of m, n and k and changing r_o to

$$r_o = 2.808\ (\kappa/\varepsilon)^{\frac{1}{2}}\ (L_o/L)^2\ \mathrm{AU}, \tag{16}$$

where L is the vaporization heat of the ice in question and L_o that of water ice. The significance of r_o is that beyond that distance from the sun most of the solar radiation incident on the comet is reradiated. Our fits to the observations of actual comets suggest that $1\ \mathrm{AU} \lesssim r_o \lesssim 4\ \mathrm{AU}$, and since for all other postulated cometary ices $L \ll L_o$, these other ices can be considered as dominant constituents only if $\kappa \ll 1$ (i.e., if the albedo is almost unity).

III. RESULTS FOR INDIVIDUAL SHORT-PERIOD COMETS

Fig. 1 shows the results of the computations of A_1 and A_2 for several comets, the units of these accelerations being AU per $(10^4 \text{ days})^2$. Converted to mass-loss rates these nongravitational parameters suggest that something like 0.2 to 0.3 percent is lost from each comet in one revolution (Sekanina 1969). For most of the comets several points are shown: they were derived from observations covering successive timespans, and the arrows indicate the direction of increasing time. The problem with this plot is that A_1 is usually very badly determined in relation to A_2, and even though the A_1 scale is only one-tenth the A_2 scale, there appear to be large discontinuities in the curves for several of the comets. Only the changes in A_2 with time are really significant, and it can be seen that whereas P/Brooks 2, P/Schwassmann-Wachmann 2, P/Encke and P/Forbes have values of A_2 that decrease (in absolute value), A_2 remains almost constant for P/d'Arrest, P/Tuttle and perhaps also P/Grigg-Skjellerup; in the cases of P/Pons-Winnecke and P/Kopff there are smooth transitions of A_2 through zero, whereas the A_2 values for P/Finlay, P/Comas Solá and P/Tuttle-Giacobini-Kresák show much greater irregularities. The rather anomalous result for P/Jackson-Neujmin is somewhat suspect because there are only two apparitions (in 1936 and 1970); nevertheless, it is certainly not possible to link these two apparitions by gravitational theory alone. The relatively large values of $|A_2|$ for P/Brooks 2 and P/Schwassmann-Wachmann 2 might be an artifact of the model (specifically the adoption of $r_0 = 2.808$ AU), for these comets have perihelion distances of 1.8-2.2 AU. On the other hand, the majority of the comets (those having $|A_2| \lesssim 0.1$) have perihelion distances ranging all the way from 0.3 to 1.8 AU; and it could be that in P/Brooks 2 and P/Schwassmann-Wachmann 2, which are "new" comets only recently perturbed in from much larger perihelion distances, we are seeing the result of the vaporization

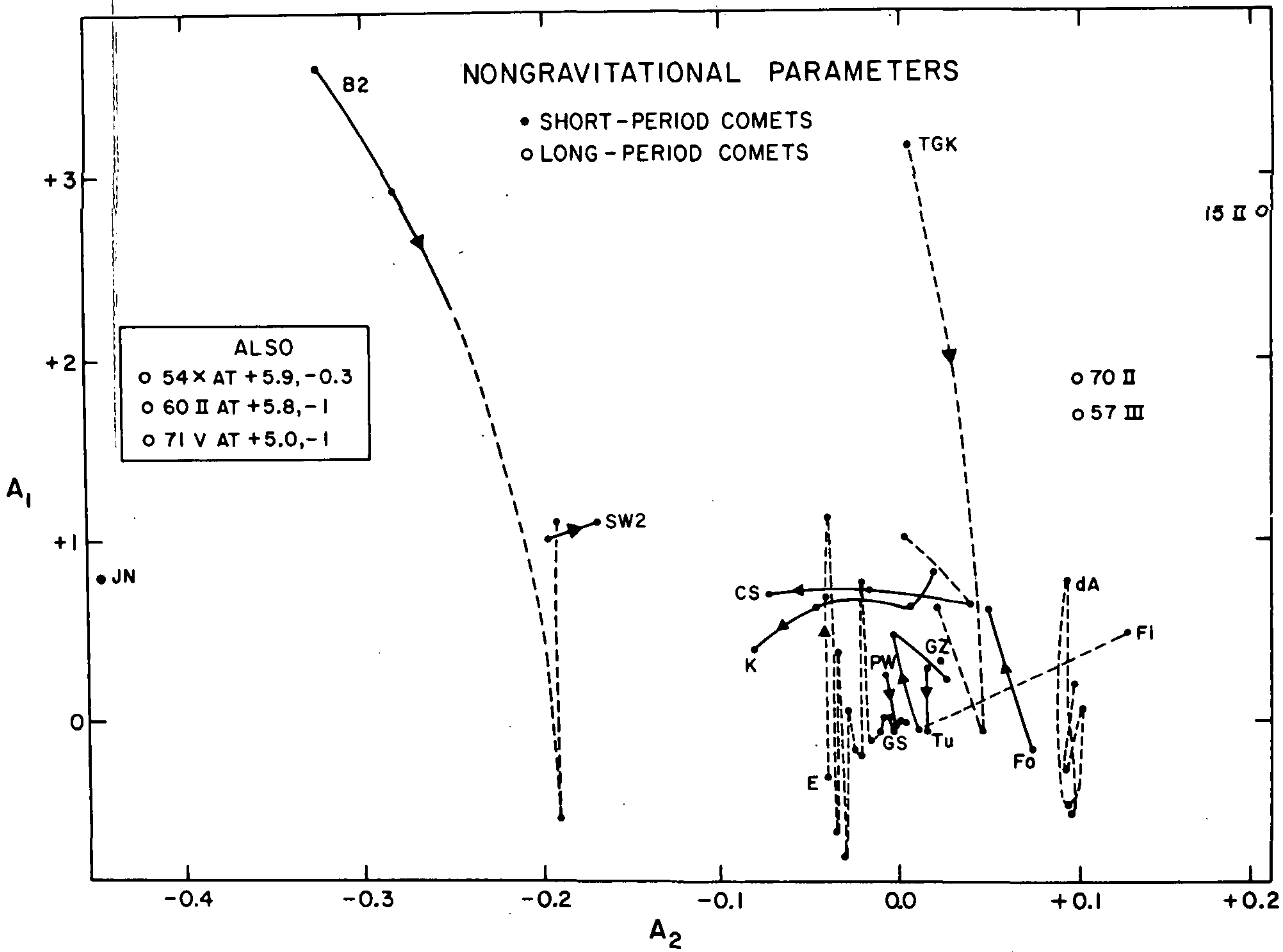

Fig. 1. Nongravitational parameters $\underline{A}_1$ and $\underline{A}_2$ for the short-period comets B2 (Brooks 2), SW2 (Schwassmann-Wachmann 2), JN (Jackson-Neujmin), CS (Comas Solá), K (Kopff), E (Encke), PW (Pons-Winnecke), GS (Grigg-Skjellerup), Tu (Tuttle), GZ (Giacobini-Zinner), TGK (Tuttle-Giacobini-Kresak), Fo (Forbes), dA (d'Arrest) and Fi (Finlay); and for the long-period comets 1915 II, 1954 X, 1957 III, 1960 II, 1970 II and 1971 V.

of a rather limited supply of free material that is much more volatile than water ice (Marsden et. al. 1973).

More useful for studying the variations of the nongravitational parameters with time is Fig. 2, which shows A_2 for P/Encke over the past two centuries (Marsden and Sekanina 1974). The decrease in $|A_2|$ (or in n_1) is seen to persist only since about 1820, before which time $|A_2|$ was increasing. We must leave it to our descendants to establish whether P/Encke acquires a secular deceleration during the twenty-first century, but there does seem to be a suspicion (first indicated, in fact, by Michielsen 1968) that the variation of A_2 can perhaps be represented by a damped sinewave. The sinewave could arise, for example, from slow changes in the direction of the axis of rotation of the comet (Marsden 1972, Sekanina 1972), whereas the damping could be a consequence of the core-mantle nuclear model (Sekanina 1969), in which the remaining icy content of the nuclear core is redistributed after each revolution without any shrinkage in the radius of the core. Eventually the comet could become inert [like (944) Hidalgo and perhaps some of the Apollo asteroids; P/Arend-Rigaux and P/Neujmin 1 appear to be comets that have almost reached this stage], although exact predictions of death date will be complicated by any continuing sinusoidal oscillations.

A few comets show large irregularities in their nongravitational parameters. To the ones already mentioned we can add P/Perrine-Mrkos, P/Schaumasse and P/Giacobini-Zinner, as well as the lost comets P/Biela, P/Brorsen, and perhaps also the other two lost comets (of more than one appearance) P/Tempel-Swift and P/Neujmin 2. (The last-named comet has been observed only twice, but a gravitational orbit solution is not completely satisfactory. To the list we could perhaps also add P/Westphal, the two

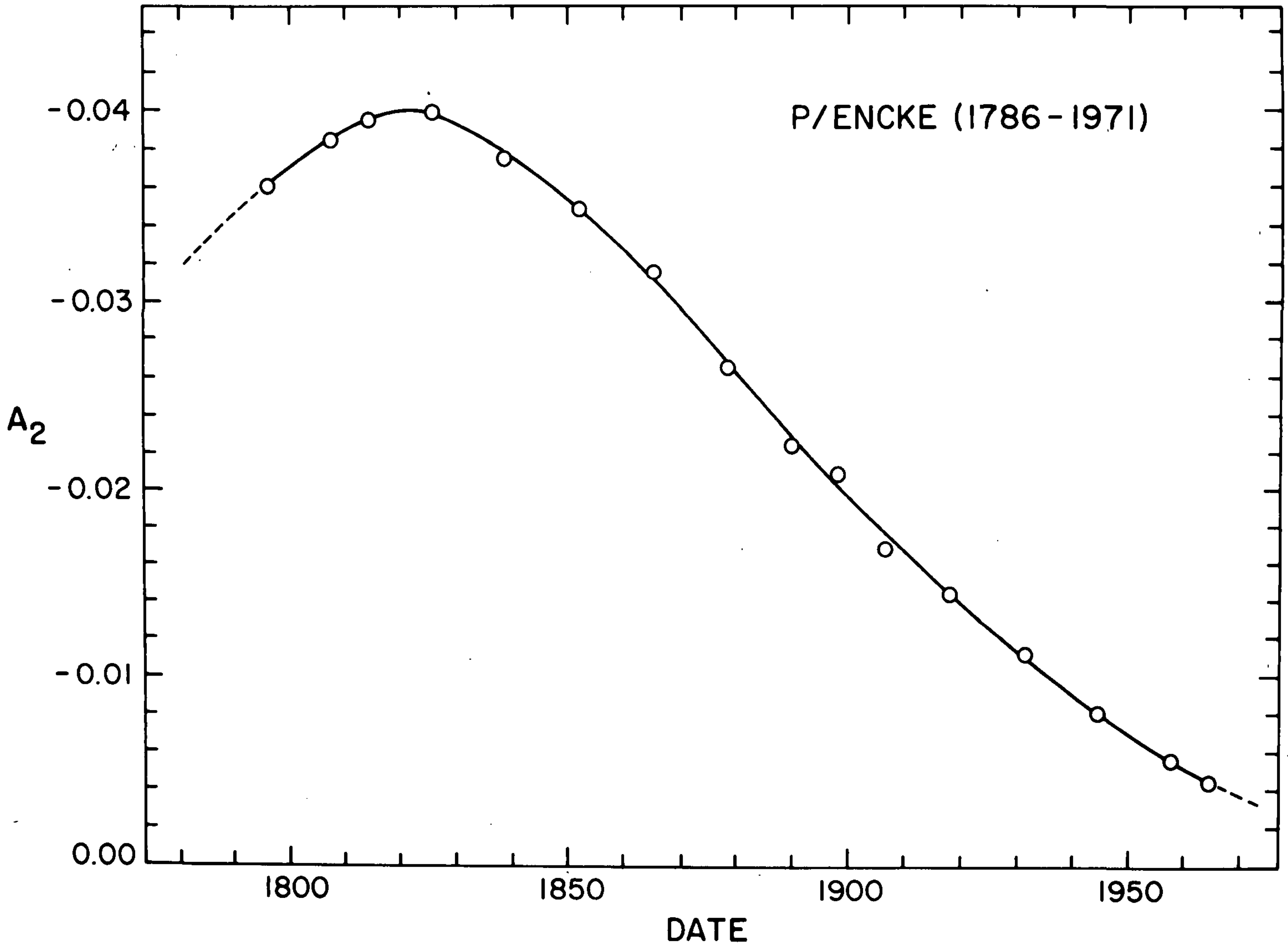

Fig. 2. The nongravitational parameter A_2 for P/Encke during 1786-1971.

apparitions of which cannot be linked at all well: this comet faded out before perihelion in 1913, and it is somewhat questionable whether it can be reobserved at its forthcoming return in 1975/76.) The possibility that these irregularities are due to collisions of the comets with small interplanetary boulders has been discussed in some detail (Marsden and Sekanina 1971), and it has been suggested that some comets are much more prone than others to collisional damage because their nuclei consist basically of low-density, high-albedo, snow-dust "mantle" material. On the other hand, it seems not at all out of the question that the irregularities in the nongravitational parameters are due to sudden changes in the directions of the axes of rotation of the comets (Marsden 1972), such changes arising when solar radiation has reduced the nucleus to an unstable shape. More theoretical and perhaps experimental (along the lines initiated by Kajmakov et al. 1972) work is needed on this problem.

IV. LONG-PERIOD COMETS

Fig. 1 also contains points for six long-period comets, each observed at a single perihelion passage. For a one-apparition comet A_1 is the better determined of the nongravitational parameters, although it is nonetheless very uncertain. It is encouraging, however, that positive values of A_1 have been obtained in each case (see also Marsden et al. 1973). As one might expect, there seem to be no cases of one-apparition comets of large perihelion distance that show the effects of nongravitational forces. On the other hand, these six comets are the only "nongravitational" cases encountered among several dozen suitable candidates for which new orbit determinations have been made.

Why should comet 1960 II, a comet strikingly lacking in dust, have a large A_1, detectable even though the observations cover less than six months, when the spectacular "dusty" comets 1957 III and 1970 II have only moderate A_1 values? And why don't well-observed comets like 1962 III and 1973f, which had particularly small perihelion distances, show any nongravitational effects at all? Little progress has been made toward answering these questions, and it is clear that more data on the possible detectability of nongravitational parameters for long-period comets are needed.

The unknown nongravitational forces acting on long-period comets must modify the derivation by Oort (1950) of the extent of the cloud of comets that is believed to surround the solar system: if only the comets of large perihelion distance are considered, it seems that the outer extreme of the cloud can scarcely be distant more than 50 000 AU, which is only about one-quarter of the result obtained by Oort (Marsden and Sekanina 1973).

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DISCUSSION

L. Biermann: I am just a little bit troubled by the absence or smallness of the non-gravitational normal forces in several well studied cases. For comet Humason the exceptional value of the surface to volume ratio might play an important role. The other cases seem to require or at least to support almost isotropic emission of the gases from the nucleus, such that no net reaction force remains. What are your ideas on this?

B. G. Marsden: I think that the gases that are being emitted from the comet Humason are not the principal constituent. Inside there you've got a lot of water-ice and not a great deal of it would vaporize in that case. The perihelion distance is only about 2.1 astronomical units. You get a bit of it, but even though it's a very active comet, really, in relation to the total mass of the comet, not very much was coming out.

The question as to whether it comes out in all directions, of course they would cancel out on the forces and this has been suggested in the case of comet Schwassmann-Wachmann 1. These outbursts of comet Schwassmann-Wachmann 1 may be isotropic but I think Dr. Roemer's observations of the outbursts of 1 show that they really aren't very isotropic.

D. J. Malaise: Did you ever try to estimate what is the amount of energy involved in the orbital change and how it would compare with the amount of energy in the rotational motion of the nucleus?

B. G. Marsden: No, we haven't actually done that. I should have. Dr. Sekanina determined the mass loss but that's about as far as we have gotten.

D. J. Malaise: I mean if the anisotropic evaporation gives you changing orbits and things like that, you might think that this is a process which is not directed. You might expect some equipartition of these effects on the translational motion of the orbit and on the rotational motion of the nucleus. So if you know what energy is involved in the translational motion of the orbit change, you can estimate how the rotation of the nucleus would change.

F. L. Whipple: I'll speak to that a bit.

Of course the total energy calculated in 1950 for the angular momentum about the sun with regard to rotation, these forces are fairly large. It wouldn't take very long if you could get all of the gas ejected on one side to give you a moment about the center to change the period very quickly.

I think that just a revolution or two would do for these comets. It's rather large. But of course you don't expect that.

The asymmetry comes from the sublimation of the material asymmetrically about the equator which then gives you a small component which can produce a procession effect in the axis. And I think that's what happens to Encke. It's a little tricky to work it out.

I think I may get back to it again with numerical integration.

E. Gerard: How could you explain that $A/\hat{2}$ could go from positive to negative values?

B. G. Marsden: This is just the axis of rotation going through the orbital plane. If the axis of rotation is in the orbital plane, you wouldn't get an $A/\hat{2}$ component.

Voice: Can you elaborate perhaps on the last point you made about the Oort cloud shrinking because of the non-gravitational forces, and also the possible evolutionary effects on a long-period orbit caused by the non-gravitational forces?

B. G. Marsden: We haven't been able to come up with any real theoretical reason as to why there is this shrinkage. All of the arguments that have been made by various people on this subject have some fallacy in them. They just are violating some factor in celestial mechanics.

This was entirely established numerically when you use that law, Delsemme's law. Then we found that 1/a was changed in a systematic manner for positive values of a/1, but we don't know why. I'm sorry.

The change in 1/a, yes. Well, we did try and study the contribution of the non-gravitational effects on that, but of course the problem here is really with the transverse components we don't know at all well in the case of the long-period comets and they could have quite a considerable change.

F. L. Whipple: I think your point was that there's a slight change in 1/a induced by the non-gravitational forces, which therefore affects the calculated outer dimension of the orbit.

B. G. Marsden: This was in Oort's own figures. With a large perihelion distance you don't have this problem, you see.

M. Dubin: On the rotation of the comets which you require to explain the over-all perturbations, have you had enough information to give a classification of the speeds of rotation and the directions of rotation?

DISCUSSION (Continued)

B. G. Marsden: It's not only rotation; it's the conductivity as well, which is tied in with this inextricably. One can't separate them.

M. Dubin: How can you possibly allow that the speed of rotation will not increase indefinitely and keep adding angular momentum by this process — a clear acceleration process? Wouldn't it be required that comets spin up to extremely high speeds, as with the Kopsky-Rajiski effect of meteorites?

B. G. Marsden: Since we sometimes observe the same value of the lag angle λ for a long period of time, I wouldn't have thought that there was any very obvious increase in rotation rate.

F. L. Whipple: We have yet to find the period of rotation of any comet. There is one case in which it looks as though there might be a 1.4-day period, but I question that one. This was for comet Bennett.

B. G. Marsden: It would be very nice if somebody would determine a good rotation period for a comet. There should be an opportunity to do this in 1978, when comet Arend-Rigaux returns. This comet will be no brighter than sixteenth magnitude, but it is very stellar, and with image tubes and modern techniques I think a good light curve and hence the rotation period could be determined.

G. H. Herbig: I would like to ask two questions — first about the significance of what I believe is the fact that the major axes and the inclinations of the long-period comet orbits indicate a rather isotropic distribution around the sun. This has been compared to the similar situation in the globular cluster orbits, high velocity star orbits around the center of the galaxy indicating a spherical halo around the galactic system which, in this case, is believed to reflect the shape of the pre-galactic cloud. Now my question is: in the case of the comets, perturbed as they must be by planets moving in the direct sense, what can you say about the analogy? What can you say about the cosmogonical significance of this isotropic distribution of cometary orbits about the sun?

B. G. Marsden: I don't know that one can say very much, except that there is a complex interplay of both planetary and stellar perturbations.

F. L. Whipple: Oort answered that, and I think Öpik probably did, too, back in 1932. With these large aphelion distances. I'm talking about 20,000 or more astronomical units, that order of magnitude, the passing stars very quickly disturb the inclination so that there is no expected orientation; even though there had been originally a plane, the passing stars destroy the evidence.

G. H. Herbig: So it could be then that these were all ejected from a flattened solar nebula disk, and the isotropy now just reflects stellar perturbations?

F. L. Whipple: That's what Oort said.

G. H. Herbig: Is that true?

B. G. Marsden: We can't say. This goes back to our discussion yesterday afternoon. You can't say whether the comet originated out there at the great distances or just beyond the orbit of Neptune and were thrown out by Neptune, or maybe Neptune was made from comets and thrown out a little bit and then stars would start taking over.

All it takes, really, is one star coming along in an appropriate manner and you can do anything. There is a nice movie showing that sort of thing. Of course it applies to globular clusters.

G. H. Herbig: Well, my second question was those pictures of the eruption of the Schwasmann-Wachmann comet that we saw yesterday in which the expanding cloud had a corkscrew or a spiral form, this sort of looks like conservation of angular momentum in radially ejected material.

If you do interpret that structure in that way, is it consistent with cometary rotations and so forth that you infer from your theory?

B. G. Marsden: Well, we don't observe any non-gravitational effects on that comet. This is the trouble.

The spiral cloud you just mentioned, this was comet Bennett, wasn't it?

F. L. Whipple: No, it was another one, another one in the late '50's. I can't remember the name of it. It had a four-day period in oscillation and brightness. Malaise studied it.

B. G. Marsden: The wagging tail?

D. J. Malaise: Burnham, there were two of the others which were like that.

F. L. Whipple: Nobody can prove that that was rotation.

By the way, it takes a mass of micron size particles about the size of this room to produce the burst on Schwasmann-Wachmann. It's a very small amount of material.

B. Donn: With regard to rotation, there is the phenomena of jets that have been occasionally observed coming out of comets in a rather narrow angle and persisting in the same direction for relatively long periods of time. If you talk about Schwassmann-Wachmann showing some sort of a spiral action, here you have a case where there is no indication of any change over periods, I think in some cases, of several days when these jets persist, which is very hard to explain, but that's what has been seen.

E. Roemer: In reference to the outbursts of P/S-W 1, the diffuse envelope gives the appearance of a filled shell of non-uniform surface brightness in which the orientation in position angle of structural features seems to be preserved during a radial expansion for time intervals of more then a month. I don't think that any information can be extracted from the surface brightness distribution as to the rotation of the nucleus on the basis of conservation of angular momentum of the ejected particles.

L. Biermann: Concerning these screw-type structures, I believe these are plasma structures. Of course the rather old explanation is that these are a consequence of almost force-free magnetic fields, and at least one needs magnetic fields anyway for different reasons. They are something which might be considered, and doesn't seem to be far-fetched.

H. U. Schmidt: May I come back to Professor Biermann's first question? I wonder whether you have answered the question really because as far as I understood the question, the big comets like Humason were excluded in that question and the question went to those smaller objects where one would really worry if there are no non-gravitational effects.

F. L. Whipple: Yes, that's the thing with the large comets, one revolution at a long period. Most of them are rather large bodies and it is difficult to get enough force to do very much while the small ones in large measure show it, except these that do not show any coma, the one or two cases.

B. G. Marsden: What about Kohoutek?

F. L. Whipple: Yes. But if it is not rotating, it is very difficult to pick up the change due to a radial force.

F. L. Whipple: (?): Would you be able to determine the radial component of the nongravitational force on comet Kohoutek?

DISCUSSION (Continued)

B. G. Marsden: If it were as large as that on comet Arend-Roland, I certainly think so. I don't know why some of the long-period comets show nongravitational forces and others don't.

F. L. Whipple: There is always the worry that the optical center of light that you measure may not reflect the center of the actual mass, namely, the nucleus, I don't know how to resolve that problem. It could upset these calculations on the single-apparition comets.

B. G. Marsden: I agree, although in some cases there are residuals of up to five or six seconds of arc, and for the long-focus observations I think it unlikely that the difference between center of light and center of mass could be so large. However, if the results from single-apparition comets were all the evidence we had on the existence of nongravitational forces on comets, I should be very skeptical.

D. A. Mendis: I would like to ask a question about the effect of the evaporating gases on the spin of a comet if the comet is not very regular. In that case, the axis of the expanding gas need not necessarily pass through the center of mass of the comet and this would give it a kick, not only in linear momentum but also in angular momentum, and this could probably either spin up or spin down the comet.

F. L. Whipple: Yes. I have spent quite a bit of time worrying about why comets split, particularly in the case of the new comets. The most rational explanation seems to be that they spin up.

It is almost impossible to find any other solution that will cause a comet to split except asymetric ejection of material that will cause a spin up. And you can very easily postulate shapes and conditions under which that effect would be very marked.

So it is quite possible, but how do you prove it?

W. Jackson: I thought Opik once wrote a paper saying that splitting of comets could occur through gravitational – breakup inside the Roche limit of the sun, I think is the word.

F. L. Whipple: That of course is in the sun-grazing comet families—the sun-grazing comets. There you do split off pieces. But the fact that the nucleus remains has been one of the strongest arguments for a discrete nucleus because if you had a gravel bank, nothing would remain.