To summarize, the main difficulties in the way of determining parameters of the external potential of the Earth from surface measurements arise from the presence of the higher harmonics and the fact that, on account of the poor distribution of observations and consequent inadequate averaging, the higher harmonics lead to errors in the estimates of J_2, a_e and g_e. None the less, in the last 20 years, very considerable advances have occurred in our knowledge of these quantities, mainly on account of observations of artificial satellites, of the extension of networks of geodetic survey and on account of the great development of gravity measurements at sea. Coupled with radar measurements of the distance of the Moon and new determinations of the mass of the Moon from space probes, we now have a set of data on the external potential of the Earth that appears to be consistent to within a few parts in a million.

4. MASSES OF THE PRINCIPAL PLANETS

G. M. Clemence

INTRODUCTION

The Working Group, appointed by the Executive Committee of the Union to consider revision of the conventional system of astronomical constants, decided not to recommend any changes in the conventional values of the masses of the principal planets. The remarks that follow are intended to explain why I think the decision of the Working Group was wisely taken, and to indicate some work that should be done before recommending a revised set of planetary masses.

I begin by attempting to estimate the most probable value of the mass of each planet separately, using observational evidence that is as much as possible independent of any assumptions about the masses of the others. Then I consider the problems that are encountered in attempting to combine the separate determinations into a consistent system.

In view of the quantity and diversity of the observational material, a substantial portion of which is known to be affected by systematic errors of obscure origin, the task is a difficult one. I do not think that another person, working independently, would be likely to arrive at the same numerical values as are given here. Therefore, although I have been obliged to give numbers, I do not strongly defend any of them. I hope only that they are sufficiently exact to justify the general conclusions.

MASSES OF THE PLANETS

After the name of each planet Table 1 gives the conventional value of the ratio of the mass of the Sun to the mass of the planet, including atmosphere and satellites. The conventional value is, in general, the value used in the planetary theories that serve as the basis of the national and international ephemerides. The only exception is the value for Jupiter, which in the theories of the four inner planets is 1047·35; the discrepancy is completely trivial, since the relevant perturbations consist of only four significant figures. Below the name, numbered serially, are the results of the principal determinations of the mass-ratio with the probable errors assigned by the authors, authors and dates, and indications of the observational data.

As a rule, I have excluded the older determinations that were based on observational material included in later determinations. The principal exceptions are as follows:

The masses of Venus derived by Spencer Jones and by Morgan and Scott both include Greenwich observations of the Sun 1900–23, but the material common to the two determinations

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contributes a relatively small portion of the weight of either; therefore they are treated as independent determinations. A similar remark applies to the two determinations of the mass of the Earth from observations of Eros.

The determination of the mass of Saturn by Hill includes most of the observations used by Gaillot; both are of interest on account of the discordance between the two results, which presumably is the consequence of some error in one or both of the theories with which the observations were compared. The observations slightly overlap those used by Hertz, which in turn are mostly included in my own determination.

In forming the weighted means, shown after the separate determinations, I have included only the determinations based on independent, or nearly independent, observational data, and I have excluded determinations that, in my judgement, are certain to be affected by systematic errors. The weights assigned are inversely proportional to the squares of the stated errors, excepting the rejected determinations, although in some cases the stated errors are clearly too small. A discussion for each planet follows.

Table 1.

				Mercury, 6 000 000	
1.	5 970 000	±	455 000	Duncombe, 1958	Venus, 1750–1949
2.	6 480 000	±	350 000	Brouwer, 1950	Secular perturbations
3.	6 120 000	±	43 000	Rabe, 1950	Eros, 1926–45
4.	5 980 000	±	170 000	Makover, Bokhan, 1961	Comet Encke, 1898–1954
	6 110 000	±	40 000		
				Venus, 408 000	
5.	409 300	±	1 400	Clemence, 1943	Mercury, 1765–1937
6.	404 700	±	800	Jones, 1926	Sun, 1836–1923
7.	407 000	±	500	Morgan and Scott, 1939	Sun, 1900–37
8.	408 645	±	208	Rabe, 1950	Eros, 1926–45
9.	408 000	±	800	Brouwer, 1950	Secular perturbations
10.	408 539·5	±	12·0	Anderson, Null, Thornton, 1963	Mariner II, 1962
11.	408 945	±	300	Duncombe, 1963	Mars, 1750–1955
	408 539	±	12		
				Earth, 329 390	
12.	328 390	±	103	Witt, 1933	Eros, 1893–1931
13.	328 452	±	43	Rabe, 1950	Eros, 1926–45
14.	328 906	±	6	—	Radar, 1961–3
15.	329 330	±	100	Jones, 1941	Solar parallax, 1930–1
	328 906	±	6		
				Mars, 3 093 500	
16.	3 088 000	±	5 000	Van den Bosch, 1927	Satellites, 1877–1909
17.	3 110 000	±	7 700	Rabe, 1950	Eros, 1926–45
	3 050 000	±	See discussion		
				Jupiter, 1 047·355	
18.	1 047·39	±	0·03	Clemence, 1961	Newcomb's material
19.	1 047·40	±	0·03	de Sitter, 1915	Satellites
20.	1 047·4	±	0·4	Kulikov, 1950	Jupiter VIII, 1908–46
21.	1 047·558	±	0·40	Samter, 1910	Egeria, 1850–1906
22.	1 047·57	±	0·06	Osten, 1928	Valentine, 1899–1918
	1 047·41	±	0·02		

Table 1—*continued*

				Saturn, 3 501·6	
23.	3 502·20	±	0·53	Hill, 1898	Jupiter, 1750–1888
24.	3 499·9	±	1·18	Gaillot, 1913	Jupiter, 1750–1907
25.	3 496	±	3	Van den Bosch, 1927	Satellites
26.	3 497·64	±	0·27	Hertz, 1953	Jupiter, 1884–1948
27.	3 494·8	±	1·3	Jeffreys, 1954	Satellites, 1924–37
28.	3 499·7	±	0·4	Clemence, 1960	Jupiter, 1779–1941
	3 499·6	±	0·4		
				Uranus, 22 869	
29.	22 934	±	6	Harris, 1950	Satellites (Photographic)
30.	23 239	±	89	Hill, 1898	Saturn, 1751–1888
31.	22 530	±	50	Van den Bosch, 1927	Satellites
	22 930	±	6		
				Neptune, 19 314	
32.	19 094	±	22	Gaillot, 1910	Uranus, 1690–1955
33.	18 889	±	62	Van Biesbroeck, 1957	Nereid, 1949–55
	19 070	±	21		
				Pluto, 360 000	
34.	400 000	±	40 000	Brouwer, 1955	Uranus and Neptune, 1712–1941

Mercury. The four results are completely independent, and agree with one another much better than the separate probable errors would lead one to expect. Probably the excellent agreement is partly accidental. There seems to be no reason for doubting the general mean of results 1, 3, and 4. Result 2 is excluded for a reason to be mentioned later.

Venus. The remarkable thing about this tabulation is the outstanding results 6 and 7. At first sight it is tempting to ascribe them to some defect in Newcomb's theory of the motion of the Earth (Kulikov, 1965, Duncombe and Clemence, 1958) but it is difficult to think of any that would not similarly affect results 5, 8 and 11, since all of them depend at least as much on Newcomb's theory of the Earth as on the theories of the planets observed. In any case all such questions lose their practical importance if it is agreed to accept result 10 at its face value. The question whether to do so is, I think, the most difficult of all questions concerning the masses of the planets. All previous experience with the constants of astronomy teaches that formal probable errors are seldom to be believed in; in many cases it has turned out that they must be doubled or trebled in order to arrive at results consistent with later independent evidence. We have no previous experience with a result like result 10, which is derived from the observed deflection of a space probe passing near Venus (the deflection is not directly observed, but is inferred from Doppler shifts in the frequency of electromagnetic waves transmitted from the Earth to the probe and back again). I am inclined to mistrust any result of this sort, that is obtained by observation of a single event; the liability to systematic error is much greater than in the case of a repetitive phenomenon, which may be observed again and again, as often as it recurs. Furthermore, our experience with Doppler shifts applied to celestial mechanics is very limited; so far as the masses of planets are concerned we have only this single example.

The mean of the other results, excluding result 10, is 408 360 ± 160, or if we exclude 6 and 10, 408 500 ± 160; there is nothing here to lessen confidence in result 10.

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After much hesitation I adopt the general mean of results 5, 6, 7, 8, 10, 11, excluding result 9 for the same reason as result 2, although I would much prefer to wait for further observational evidence.

Earth. The difficulty with the mass of the Earth + Moon is second only to the difficulty with Venus. Here, however, the new evidence is much more extensive, being based upon distances from the Earth to Venus inferred from radar echoes observed at several places over a considerable range of distances. Also, Newcomb's theories of Venus and the Earth are not relied upon, but special orbits have been calculated for the purpose.

It has several times been remarked that result 14 falls about midway between results 13 and 15, but of course this coincidence is no reason for increased confidence in result 14, in view of the probable errors.

Atkinson has pointed out the liability of result 15 to systematic error on account of the flexures of the telescopes used, those in the northern hemisphere being usually pointed south of the zenith, and those in the southern hemisphere north of the zenith, while the result depends mainly on a comparison between the two. The bias thus introduced has long been known to observers of stellar parallaxes, some of whom participated in the program for the solar parallax, and it is very curious that the results were not controlled by special experiments, as they might easily have been, but it is now too late for that. Therefore, in my judgement, result 15 must be rejected.

Marsden (1965) has pointed out that result 13 (and hence, possibly, result 12 as well) may be brought near to result 14 by forcing the reciprocal of the mass of Mars down to about 3 020 000, a value that is not necessarily inconsistent with result 16 in view of possible systematic errors in observations of the angular distance between Mars and its satellites. The gravest objection to forcing the mass of Mars in this way is the resulting increase in the observational residuals for Eros 1926–45; the sum of the squares is increased from 7·55 to 13·73, which is far from satisfactory.

Evidently, however, result 14 is the only one of the four that is not suspect on account of known liability to systematic error. Further study along two different lines is much to be desired: (*a*) a rediscussion of all observations of Eros from 1893 to the current epoch, with particular attention to the positions of the comparison stars and possible defective illumination of the planet, and a comparison of the observations with a new orbit; (*b*) a derivation of the mass of Mars that does not depend on the measured distance from a satellite to the planet, nor on observations of Eros. Probably a space probe passing near Mars is the most promising experiment.

In the meantime I think we cannot do better than to adopt result 14.

Mars. In view of the discussion in the preceding paragraphs, little if any confidence can be placed in results 16 and 17, and both of them are rejected. It is not possible to state any value as the most probable one, nor to assign any probable error. All I am able to conclude is that the value lies between 3 000 000 and 3 100 000 with rather high confidence (perhaps 80 per cent), but with little preference for one or the other. Thus, if a single number is insisted upon, I would choose 3 050 000, in the middle of the stated range.

Jupiter. The five independent determinations are remarkably consistent, and there is no reason to doubt the general mean.

Saturn. Results 23, 24, 26, and 28 are all derived from overlapping observational data, and hence are not independent of one another. Furthermore, results 23 and 24 have been obtained with the aid of general theories of the motion of Jupiter, while 26 and 28 have been obtained with an orbit calculated by step-by-step numerical integration (Eckert, Brouwer, Clemence, 1951). Other things being equal, result 28 would be the preferred one, being derived from the longest span of observations, but it remains to explain the discrepancies with results 23 and 26. Concerning result 23 it is important to recall that Hertz (1953) obtained 3496·69 ± 0·27 by comparing observations of 1884–1948 with Hill's theory; the important difference between this result and result 23 is an indication of an error in Hill's theory, causing it to yield different masses of Saturn at different times, and is sufficient reason for rejecting result 23. At first sight, a similar remark might appear to be applicable to results 26 and 28, which in view of their probable errors cannot both be correct. But although both results have been obtained with the same numerical orbit of Jupiter, in deriving result 28 I have corrected the numerical orbit for the effect of the secular perturbations by the four inner planets, and have also corrected Newcomb's value of the general precession in longitude. Whether these corrections are sufficient to account for the difference between results 26 and 28 is not certainly known, but at any rate it is reason for preferring 28.

Accordingly, results 23, 24, and 26 are rejected, giving the result shown.

Uranus. Both of the results 30 and 31 disagree with 29 by amounts that are difficult to reconcile with the stated error, and I think all visual observations of satellites referred to the planet are liable to systematic errors. But there seems to be no other valid reason for rejecting them, and accordingly they are included in the general mean.

Neptune. No discussion appears to be necessary.

Pluto. Brouwer and Clemence (1961) conclude that no reliable gravitationally determined mass of Pluto is available. Nevertheless it seems advisable to use the value given until a better one becomes available.

SECULAR VARIATIONS

Having provisionally adopted values of the masses of the principal planets, the next step in logical order is to attempt a mutual adjustment of at least the inner four of them, using the observed secular variations of the eccentricities, inclinations, perihelia, and nodes of the inner planets for the purpose. Sixteen equations of condition may be formed, the right-hand members being the excess of the observed change in the value of an element over its calculated amount. The values of the masses of the four inner planets here stated yield four more equations, and external evidence about the precession yields another, making 21 in all. The equations may be solved by least squares, yielding corrections to the assumed values of the masses and to the constant of general precession in longitude. Results 2 and 9 have in fact been obtained by such a method, which is one reason for excluding them from the mean values given in Table 1. If the process described can be carried through without doing violence to the probable errors already estimated, we might adopt the resulting masses as the basis for future work, and introduce them into the national and international ephemerides of the planets.

It would be useless to attempt such a mutual adjustment at present.

The calculated secular variations of the elements of Mercury, Venus, and the Earth are not known with sufficient accuracy. In the case of Mars I have found that Newcomb's calculated value of the motion of the perihelion requires a correction of approximately

$$3''\!.5\, T + 0''\!.9\, T^2,$$

T being reckoned in centuries from 1850. It may reasonably be supposed that similar corrections, somewhat smaller in size, will be found for Mercury, Venus, and the Earth when the theories of their motions are revised, since Newcomb's theories of the four inner planets are all defective in the same respects. In calculating the secular perturbations of the second order, he neglects the effect of the periodic perturbations of the first order, and he entirely neglects most of the periodic and mixed terms of the second order. These omissions would destroy the value of any mutual adjustment of the masses.

CONCLUSION

It seems to me that any immediate revision of the conventional values of the masses of the principal planets would be premature. If it were to be done now, it might well require even more urgently to be done again within a few years, when work either now in progress or planned for the near future will have been completed. Work that is to be desired includes:

1. Improvement of the general theories of Mercury, Venus, and the Earth.
2. Discussion of the motion of Eros from 1893 to the present epoch.
3. Determination of the mass of Mars.
4. Use of space probes to determine the masses of Mars and Mercury, to confirm the mass of Venus, and if possible to determine directly the mass of the Earth.

Many other investigations could be proposed that would contribute importantly to our knowledge of the masses of the principal planets. I have mentioned only the ones that appear to me to be most urgent.

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