II. PROCEEDINGS OF THE JOINT DISCUSSION

I. REPORT OF THE PROCEEDINGS

G. A. Wilkins

INTRODUCTION

The Chairman, W. Fricke, President of Commission 4, opened the Joint Discussion by drawing attention to the purpose and proposed procedure for the meeting. The Joint Discussion had been arranged by the Executive Committee of the Union in order to avoid the necessity for separate discussions by each Commission that was affected by the Report of the Working Group on the IAU System of Astronomical Constants. The Organizing Committee therefore proposed the following resolution:

> 'The members of the IAU at this Joint Discussion recommend to the Executive Committee that the following resolution be put before the General Assembly: "The International Astronomical Union endorses the final list of constants prepared by the Working Group on the System of Astronomical Constants and recommends that it be used in the national and international astronomical ephemerides at the earliest practicable date."'

The Chairman stated that the resolution would be put to the meeting after the invited papers had been read and discussed; he pointed out that only Members of the Union were entitled to vote, but all participants were free to express their views on the report, which was included in the introduction to the Draft Reports and which is printed at the beginning of this Discussion. The invited papers were presented in the following order:

W. Fricke: Arguments in favour of the revision of the conventional system of astronomical constants.

G. A. Wilkins: Presentation of the Report of the Working Group.

A. H. Cook: Parameters of the Earth's gravitational field.

G. M. Clemence: Masses of the principal planets.

I. I. Shapiro: Radar determinations of planetary motions.

B. Guinot: Rotation de la Terre et constantes astronomiques.

Unfortunately Shapiro was unable to be present, but his paper was read by G. H. Pettengill. The full texts of these papers, except that by Wilkins, are given separately below. The Report of the Working Group is printed here above on p. 593. We have not attempted to give a verbatim account of the discussions, but we hope that this Report includes all significant points. In a few cases we have added in parentheses a few additional remarks in order to clarify particular points. We are grateful to R. L. Duncombe and J. Kovalevsky for allowing us to use the notes that they made during the discussions.

THE PRESENTATION OF THE REPORT

Before presenting his paper, *Fricke*, pointed out that he was originally opposed to any change in the system of constants, and that he had accepted the Chairmanship of the Working Group with some trepidation, but as a result of his subsequent studies he was now firmly convinced of the necessity for change. He then outlined the arguments that had led him to this conclusion, and he indicated some of the considerations that had influenced the Working Group in its choice of numerical values.

In presenting the Report of the Working Group, *Wilkins* first of all reviewed the stages that had preceded the Joint Discussion. IAU Symposium no. 21 on the System of Astronomical

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Constants was held at the Paris Observatory on 27–31 May 1963; it was held under the Chairmanship of G. M. Clemence and was attended by some 30 astronomers, including several who were primarily concerned with the new techniques of radar astronomy and space research. The Proceedings of the Symposium will be published in full in *Bull. astr.*, **25**, nos. 1–3, but there have been unexpected delays at the printers. A brief report on the Symposium has, however, been given by Wilkins in *Quart. J. R. astr. Soc.*, **5**, no. 1, 23–31 March 1964. After considering papers on the new determinations of particular constants and then on the system as a whole the Symposium passed (with only one or two objections) a series of resolutions calling for a new system and the setting up of a Working Group to specify the system in detail. The Symposium itself recommended a list of fundamental constants and so the task of the Working Group was clearly defined. The Group originally consisted of D. Brouwer, A. Danjon, W. Fricke (Chairman) and A. A. Mikhailov, with Wilkins as Secretary, but owing to the severe illness of Danjon, J. Kovalevsky was co-opted in his stead.

The first step taken by the Group was to send a circular letter and copies of the Paris resolutions to all persons, some 80 in number, who were thought to be likely to be able to help the Group or who might be affected by the introduction of a new system. The Group met in January 1964 at the Royal Greenwich Observatory, Herstmonceux Castle, and, after considering all the replies that had been received, drew up the list of values for the system. The Group did not depart in any significant way from the Paris resolutions, although it did introduce the names 'defining constants' and 'primary constants', rather than the single name 'fundamental constants.' The final details of the Report were settled by correspondence and then duplicated copies were distributed widely in March, in advance of the distribution in the Draft Reports. The Group has not received any adverse comment on the form of the system; nor has anyone suggested other values for particular constants; nor has any fresh evidence been published that would justify any change in the Group's recommendations.

Wilkins then reviewed briefly the reference list of recommended constants, and pointed out that the constants defining the radius and gravitational field of the Earth were in close accord with the recommendations of the Working Group appointed by the International Association of Geodesy. The new system of primary constants necessarily implied a new value for the mass of the Earth but otherwise the system of planetary masses was not to be changed at this time. Following a recommendation of the Paris Symposium the Group had not suggested any change in the constants of the precession and nutation even though more likely values were now available. He concluded by remarking that the new system should be sufficiently close to the truth to provide an adequate basis for new ephemerides and star places that are intended for prediction and comparison with observation, i.e. first-order corrections should be adequate if the best possible values are required in the future. The new system should be introduced as soon as possible, and preliminary discussions in Commission 4 had indicated that star places based on the new system could be introduced directly into the almanacs for 1968, but for the planetary and lunar ephemerides it would be necessary to give the required corrections separately.

GENERAL DISCUSSION

D. H. Sadler opened the general discussion by congratulating the Working Group, and particularly the Secretary, on the speed and efficiency with which it had produced its Report. He had no comments on the system as a whole but mentioned three criticisms of the Report. Firstly, as pointed out by N. C. Lahiri, there is no reference to the fact that the recommended value of the constant of precession does not even lie within the range given for the true value. Secondly, there does not appear to be sufficient explanation in the notes on constants 19 and 24 of the serious inconsistency between the recommended mass ratios $(E + M)/S$ and M/E and the values used in the current theories and ephemerides. Thirdly, speaking on behalf of those who will have the

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responsibility of implementing the recommendations of the Report, he regretted the absence from the Report of any treatment of the effects of the changes on the ephemerides of the Sun, Moon and planets. In amplification of this third point he later mentioned that, ideally, changes should also be made to the elements of the orbits in order that the agreement between the ephemerides and past observations should be maintained. *Fricke* commented that it was realized that there would be a transitional period between the initial introduction of the system and the time when all the planetary theories were on a completely self-consistent basis. (It will not be worthwhile making any but the most direct corrections to the current ephemerides since the theories on which they are based are not of adequate precision, and the system of planetary masses has yet to be revised.)

H. G. Hertz questioned whether it would have been advantageous to use the epoch of 1950 rather than 1900 in the definition of the system, but *B. G. Marsden* pointed out that Newcomb's constants were determined for a mean epoch of about 1850 and a lengthy extrapolation was not justified. The continued use of 1900 appears to be the most appropriate choice.

Cook welcomed the simplicity and self-consistent structure of the new system; he thought that the new system was a great improvement on the old system and it would make the study of the physics of the Earth much easier.

CONSTANTS FOR THE EARTH

Cook then presented his paper on the parameters of the Earth's gravitational field; he dealt mainly with the difficulties that arise in their determination from observations of the motions of close artificial satellites and from surface observations of arc-length and gravity. Although he gave $1082{\cdot}65 \times 10^{-6}$ as the most likely value for the coefficient J_2 in the expression for the gravitational potential he indicated that the value of 1082·7 recommended by the Working Group was adequately close. After thanking Cook for his paper, *Fricke* thanked him and his colleagues on the IAG Working Group for the help and advice that they had given.

J. A. O'Keefe then commented as follows: 'The meaning of the new value of the Earth's flattening which is proposed for adoption today is fundamentally different from the meaning of the old value of 1/297 which we are dropping. The older value could be regarded, within a a reasonable range of error, and in view of the older techniques, as being at the same time the actual flattening of the Earth and the flattening which the Earth would have if in fluid equilibrium. Deviations from it could properly be regarded as deviations from fluid equilibrium.

'When we adopt the value 1/298·25 for the Earth, we admit that the Earth is not in fluid equilibrium; if this figure is combined with the precessional constant, it turns out that the Earth's polar moment of inertia can be deduced; and from the latter the flattening corresponding to fluid equilibrium can be found; it is near 1/300.

'Hence when we astronomers furnish to our geophysicist colleagues data on the deviations of the Earth's gravitational field from fluid equilibrium, these data should *not* start from 1/298·25, but from the value of fluid equilibrium. If plots are made from the flattening of 1/298·25, there is a danger that geophysicists may use these to interpret the internal constitution of the Earth, overlooking the dominating role which is played by the non-hydrostatic part of the second harmonic. The danger is not hypothetical; during the past year there have been several such papers.

'I therefore propose that we add to the remarks on the flattening a clear indication that the new ellipsoid is not that of fluid equilibrium.'

There followed a lengthy discussion on possible amendments to the last sentence of note 16 of the Report in order to cover O'Keefe's point. The original wording was 'The new values of these constants [f and g_e] are not intended for geodetic use', and the remark was added to emphasize that no change in the International Gravity Formula, or other similar constants used by geo-

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desists but not by astronomers, was contemplated. Since the Report defines precisely the significance of f, several participants felt that no change was required, but eventually it was agreed that a positive statement should be given; the sentence has therefore been amended to read 'The new value of f is given here only for astronomical use (parallax corrections, etc.)' (The Report does not give the numerical value of g_e, although it gives an expression for g_e for use in the derivation of f from J_2.)

Y. Kozai suggested that the limits within which J_2 is believed to lie should be altered to read: 0·001 082 6 to 0·001 082 7.

THE SYSTEM OF PLANETARY MASSES

Clemence introduced his paper by remarking that he had not set out to exhaust the astronomical literature on the masses of the planets but he had intended to exhaust the observational evidence. He concluded that it would be premature to make an immediate revision of the conventional values of the masses of the principal planets but a revision would be justified when the work now in progress, or planned for the near future, has been completed. One of the major uncertainties was in the mass of Mars, but observations of the perturbations of a suitable space probe would give a more accurate value. *Marsden* pointed out that the use of a large mass of Mars was not the only way of reconciling the radar and dynamical methods for the determination of the solar parallax; he would favour a reciprocal mass near 3 080 000, and not as low as 3 050 000. *Rabe* suggested that observations of a suitable space probe could also be used to determine the combined mass of the Earth and Moon. *Schubart* was of the opinion that the Report should draw attention to the large uncertainty in the mass of Pluto.

SCALE OF THE SOLAR SYSTEM

Pettengill, in presenting Shapiro's paper, reviewed the determinations of planetary distances and motions that had been made over the past few years from radar measurements of range and Doppler velocity. Observations of radar echoes from Venus had established the relationship of the astronomical unit and the metre with very high precision; preliminary analysis of the 1964 observations confirmed the value of 149 598 000 km per A.U. with a precision of $\pm$ 100 km. It also appeared that, relative to the Earth, Venus and Mercury were ahead in their orbits by $0''.5$ and about $1''$, respectively, but new ephemerides with one or two more extra decimal places that are mathematically correct are needed if full use is to be made of the observational material. The spread in the Doppler shift of the echoes indicates that the rotation period of Venus is 247 $\pm$ 5 days and the axis of rotation is inclined at $-84^\circ \pm 2^\circ$ to the orbit of Venus.

Duncombe congratulated Shapiro and the other workers at M.I.T. and J.P.L., and then commented as follows: 'The truncation effect in the published ephemeris of Venus, remarked about in Dr Shapiro's paper, is due to the fact that this ephemeris is derived from tables based on Newcomb's theory rather than directly from the theory itself. End-figure consistency in the coordinates of Venus may be obtained from numerical integration fitted to the Newcomb-based ephemeris. Such numerical integrations of the orbit of Venus have been generated by the Nautical Almanac Office, U.S. Naval Observatory, the Jet Propulsion Laboratory of California Institute of Technology, and the Lincoln Laboratory of the Massachusetts Institute of Technology.

'In a comparison of observations with theory, some investigators have attempted to attribute the entire difference to physical causes whereas, in fact, part of the discordance may be due to arithmetical inadequacies of the comparison ephemeris such as the truncation error mentioned by Dr Shapiro. While the analysis of the presently available radar data to obtain corrections to the elements of Venus is of interest, it would also be desirable to determine the corrections to the elements of the Earth in the same solution. Due to the commensurability of the mean motions of the Earth and Venus this may be most feasible after eight or more years of radar data become available.

'To aid in the interpretation of radar observations of the rotation of Venus, the Nautical Almanac Office will produce comparison ephemerides based on provisional values of the rotational elements as provided by Dr Shapiro or other investigators.'

D. O. Muhleman then gave a brief report on the results of a recently completed comparison of the J.P.L. radar observations of Venus for the years 1961, 1962, and 1964 with an ephemeris without the Duncombe-corrections; they had found significantly different corrections to the elements; they had also attempted to combine the radar and optical observations by using some of Duncombe's equations of condition. The estimates of the measure of the A.U. varied by only 100 km about 149 598 400 km.

Mikhailov pointed out the Doppler spread due to the rotation of Venus depends on the law of reflection but *Pettengill* replied that this difficulty was overcome by selecting the reflection from a particular part of the surface.

Clemence emphasized that it would be of considerable value to have observations extending over several years in order to measure more accurately the motion of the perihelion of Mercury, and so make possible a critical test of the various theories of relativity.

Yaplee queried whether it would not be better to use the light-second rather than the metre. *Fricke* said that the Working Group had considered the point again but had decided to follow the quite definite recommendation of the Paris Symposium. *Cook* commented as follows: 'Nowadays geodetic measurements of arc lengths are mostly made with electromagnetic waves in some form so that, in common with measurements of the distances of the Moon and of planets, the fundamental data are light times. If all our data were geometrical, there would be a strong case for expressing distances in light times and not in metres. But, one other measurement also connects terrestrial and celestial mechanics, that of the absolute determination of gravity. In such measurements we use a wavelength of light as our standard of length, just as we define the metre in terms of a wavelength of light, and we use the frequency of a caesium standard to obtain our unit of time. It would of course be possible to express the value of gravity in terms of light times, but the uncertainties of the laboratory measurements of the velocity of light and the acceleration due to gravity are not so very different and some uncertainty would be introduced by expressing the one in terms of the other. It seems therefore preferable to continue to express celestial distances also in terms of the terrestrial metre.'

OTHER CONSTANTS AND FINAL DISCUSSION

Guinot then presented his paper which was primarily concerned with the effect of the new constants on time and latitude determinations. He concluded that the new system is quite satisfactory but that for very precise studies it will be advisable to take into account the small discontinuities introduced by the change in the value of the constant of aberration.

R. O. Vicente discussed the reasons why he considered that the Group were right not to suggest any changes in the constants of nutation and precession. He pointed out that the classical theories of nutation were imperfect because they were based on the assumption that the Earth is a homogenous rigid body, and the modern theories, which endeavour to take account of the internal constitution of the Earth including the complex motions in the liquid core, cannot yet be used to compute the values of the constants. The adopted values must therefore be based on observation, but, owing to the difficulties in eliminating possible systematic errors over the period of 19 years, the constant of nutation cannot be determined to better than $0''.006$. Hence there is no justification for changing from the adopted value of $9''.21$ even thought it is at the upper limit of the likely

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range. The principal uncertainty in the constant of precession is due to uncertainties in the planetary masses and hence in the planetary precession; the constant should therefore not be changed until an improved system of masses has been adopted.

Vicente also suggested that it would be better to adopt 20″50, rather than 20″496, for the rounded value of the constant of aberration, but *O'Keefe* pointed out that the recommended rounded values of the solar parallax and of the constant of aberration are self-consistent to a higher precision than is apparent from the number of figures given.

Finally, at about $17^h\,45^m$, the Chairman put the main resolution to the meeting and it was carried unanimously by the 50 Members of the Union that were still present.

2. ARGUMENTS IN FAVOR OF THE REVISION OF THE CONVENTIONAL SYSTEM OF ASTRONOMICAL CONSTANTS

W. Fricke

The system of astronomical constants conventionally used in the computation of ephemerides rests upon observations made in the nineteenth century. At the international conferences held in Paris 1896 and 1911 a set of values of various constants was adopted, and agreement was reached about the procedure for their introduction into the national ephemerides. The adopted values of the constants resulted from measurements of the maximum accuracy possible at the time of their adoption.

Several of the constants are not independent of each other, and it has always been known that the adopted values do not form a consistent set. If one keeps in mind that the main purpose of the ephemerides is to enable the comparison of observed positions of celestial objects with those predicted by theory in order to contribute to the improvement of the theories, then inconsistencies are only tolerable in so far as they do not obscure the comparison between theory and observation. At the time of Newcomb, the inconsistencies in the system of constants were harmless in this respect, since the accuracy of the observed positions of the stars and of the bodies of the planetary system did not seem to require more accurate values of the basic constants. Consistency for itself, however desirable it is from the logical point of view, appears to me not to be an indispensable condition for a system as long as no discrepancies arise in the discussion of observations. On the other hand, consistency becomes indispensable when the accuracy of the observations is sufficiently high to reveal discrepancies caused by an inconsistent set of constants.

I consider the fact that no changes have been made in the most important astronomical constants for many decades as a great advantage, because frequent changes in the foundations of the ephemerides may render the analysis of the differences between observed and computed values difficult or even impossible.

Some arguments put forth in the past in favor of a revision of the conventional system of constants seem to me not strong enough to justify a change. These are, first, the inconsistency of the system under the condition mentioned above, and second, the availability of improved values of certain constants. I would like to give an example of circumstances under which one is better advised to continue using an adopted incorrect value of a constant than to make a change in the foundations of the ephemerides. One of the best examples is probably provided by the constant of precession. It has been realized for a long time that Newcomb's constant of precession needs a correction of the order of plus 1 second of arc per century, and the reasons for Newcomb's error are known. However, it has also turned out to be extremely difficult to determine the correction itself with a better accuracy than 25 per cent. Now, the mistake in the adopted value of precession