PROGRESS REPORT ON PROJECT MERIT

George A Wilkins

Royal Greenwich Observatory

Chairman: Joint IAU/IUGG Working Group on the Rotation of the Earth

The origin, objectives and programme of Project MERIT, which is a special programme of international collaboration to Monitor Earth-Rotation and Intercompare the Techniques of observation and analysis, were described briefly at IAU Colloquium No. 56 (Wilkins, 1981). Further details of the project and reviews of the techniques to be used were published in a special report (Wilkins, 1980). The MERIT Short Campaign of observations was held during the period 1980 August 1 to 1980 October 31 and the preliminary results obtained will be published by the Bureau International de l'Heure in its Annual Report for 1980. The main objective of the campaign was to provide a realistic test of the operational arrangements that will be required during the MERIT Main Campaign in 1983/4. The first MERIT Workshop was held at Grasse on 1980 May 19-21 to review the operational aspects of the short campaign and to continue the planning for the main campaign. Some of the results obtained during the short campaign were presented on the following day at IAU Colloquium No. 63, and are reported in this volume. The proceedings of the Workshop will be published by the Working Group in a report that will also contain the principal results of the short campaign and information about the availability of the observational data.

Many observing stations and computing centres contributed to the short campaign. Observational data were obtained by classical astronomical techniques, by the doppler-tracking of satellites, by satellite and lunar laser ranging, and by connected-element and very-long-baseline radio interferometry. It is clear that the campaign stimulated extra and faster activity in both SLR and VLBI, and that these techniques are capable of providing results of much higher precision and at shorter intervals than those previously available. The campaign has also led to improvements in the quality of the data and in the speed of transmission of the results for the techniques that were already in regular operation. Each dataset has been analysed by at least two groups, and the attempts to understand the

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differences between the results has led to new exchanges of information about both the observational and the processing techniques. As a result the groups are better aware of the strengths and weaknesses of the different techniques and models that have been used. The importance of determining universal time and polar motion together has become apparent. Several groups obtained their first experience of the use of a computer-based communications network for the transfer of data between the coordinating centre (at the BIH in Paris), the operational centres and the analysis centres.

It was decided at the Workshop that the MERIT Main Campaign will take place between 1983 September 1 and 1984 October 31. This allows over two years for the upgrading of current equipment, for the procurement and commissioning of new equipment, and for the development of regular operating procedures for new networks. The campaign will provide an extremely valuable dataset for scientific analysis and a sound basis for recommendations about the future international service for earth-rotation. Special observations may be made to ensure that the terrestrial reference systems used by the various techniques may be accurately linked together to provide a firm basis for a new conventional Terrestrial Reference Frame based on a catalogue of station coordinates.

Project MERIT will continue to be organised through the informal cooperation of the participating groups under the general direction of a Steering Committee on which each technique is represented by a principal coordinator. Information about the further progress of the project will be issued from time to time in the MERIT Newsletter. The project has received the generous support and cooperation of scientists and organisations in many different countries. The progress made and results obtained since the project was first suggested three years ago have clearly demonstrated its value and viability, and, given the continuation and extension of this support over the next few years, there is every reason to believe that the project will achieve its scientific and operational objectives.

References

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GENERAL DISCUSSION

(Chairman : B. Guinot)

Guinot : Today's sessions have been very impressive in terms of the mass of data and results from various techniques, with very small error bars. We have come to the point where it will soon become necessary to add another decimal place in our publications, something that happens seldom in metrology, and especially in the metrology with which we are concerned. One of the important problems in these comparisons will be that of systematic errors affecting the various series. This discussion has been scheduled to address that problem.

Calame : Je voudrais commencer à engager cette discussion générale en faisant deux remarques, l'une sur l'utilisation correcte des sigles définis officiellement, l'autre concernant la compréhension et la modélisation des biais systématiques. Aujourd'hui, on a souvent parlé à tort de UT0; en effet, ce n'est généralement pas UT0 qui est déterminé par ces techniques, car dans les calculs de résidus d'observations les coordonnées du pôle sont introduites, par exemple sous la forme des valeurs du BIH considérées en première approximation. Ceci ne correspond pas à la définition officielle de UT0, qui implique de négliger totalement le mouvement du pôle.

Ainsi, la grandeur déterminée pour le Temps Universel serait en principe UT1. Cependant, pour chaque technique, il y a des corrections empiriques supplémentaires, dont certaines sont largement corrélées avec UT1. Par exemple, dans le cas du Laser-Lune, la grandeur en fait déterminée (que j'ai intentionnellement désignée par UT*) représente la somme de la correction à UT1 et de celle à la longitude de la station et l'ascension droite de la Lune. Dans chaque technique, il y a des choses semblables, qui représentent en quelque sorte des "poubelles" pouvant constituer des erreurs systématiques éventuellement importantes et d'origine différente selon la technique d'observation utilisée. Je suggère donc que nos discussions portent maintenant, non plus sur la comparaison des chiffres obtenus par les différentes techniques, mais sur la comparaison des grandeurs physiques qui sont déduites de chaque technique par rapport au paramètre UT1 réel.

(The first point is that UT0 has a precise and formal definition which implies to neglect totally the polar motion, so that it is not correct to speak about UT0 when the pole coordinates are

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introduced (even with any uncertainty) in the computations.

The second remark is dealing with the fact that, in nearly every technique, the determined parameter may be slightly different from the definition of UT1 because of some additional empirical corrections, specific to the technique, (a sort of a "garbage can"), a part of which is highly correlated with UT1. Thus, some systematic effects may be entered, with different origin and nature according to the used technique. Therefore, it is proposed to discuss about these systematic effects, a comparison of the "garbage cans", rather than the obtained figures themselves.)

Guinot : This problem of systematic effect is, of course, general in metrology. When we express the value of a standard kilogram, for instance, therere some systematic effects, but nevertheless, we call it a kilogram. It is impossible to know the amount of these effects, but it is important to know at least the shape. In classical astrometry, for instance, we know that a longitude error enters fully into the UTO results. It is more complicated for some of the new techniques, but it should be possible to clarify this point, to try to understand or make clear what the systematic errors could be, what their shapes are, the frequencies of periodic terms, for example.

Wilkins : In MERIT, we are trying to adopt a common set of constants and reference frames that will be common to all techniques, in the hopes that things will be easier to compare.

Guinot : I do not think that that will solve the problem entirely, because they enter the different techniques in different ways. It is difficult to see the exact effects of such constants.

McCarthy : A related problem, I think, is the increasing use of the word "model", so that the distinction between systematic errors and empirical models is becoming very fuzzy. The attitude that, if you can model it somehow you have removed the source of systematic error, seems a very dangerous thing. You have not really removed the error, only thrown it into a model.

Mulholland : Thrown it into a "garbage can"...

McCarthy : ... and that "garbage can" is now our model. It affects every technique.

Tapley : There are two types of models that one works with. One is a model derived from a physical phenomenon by means of a mathematical approximation that has a very appropriate place in the phenomenon. The constants of that model may not be known to you, and that may be a source of concern to you. The models that you might equate to a "garbage can" are those purely empirical models for which you do not know a physical phenomenon, if you used those things, it behoves you to try to understand the physics. I think that we should be careful, though, about putting polar motion into the "garbage can" category, even though some use it that way.

McCarthy : I agree. Certainly, there are valid models, nutation and precession, for example. My objection is to excessive use of *ad hoc* modelling.

Fliegel : Part of the alarm taken by classical astrometers over the new techniques surely arises because we have not adequately explained what goes into them. Franckly, the set of systematic errors associated with the astrolabe seems as complicated to me as anything in VLBI or laser ranging. There is no need to invent new symbols, because what we are doing is preparing estimates of a well-defined quantity, UT1. Certainly, the error analyses need to be done, but it is not qualitatively different from what you have been doing for years.

Mulholland : The objection was not so much to the use of UT1, but the incorrect use of UT0. Both UT0 and UT1 have very precise definition adopted within the international Unions. In the new techniques, probably all of us introduce some approximation of the pole coordinates into our calculations, so that what we are determining is indeed an approximate to UT1, not UT0.

McCarthy : UT0 is just the observational UT1 that is suited to the classical techniques. It is what they observe at the telescope.

Mulholland : It is not "observational UT1", it is the apparent longitude shift of the station.

Fliegel : I am not conscious of ever having used UT0. What we produce is the difference between our estimate of UT and someone else's. Even if I use a conventional estimate of x and y, it makes no difference if I call that quantity ΔUT1 or ΔUT0. Due to the linear nature of the equation, the x and y fall out.

Mulholland : They fall out to first order only; the equation is linearized, not linear. The conceptual difference is important.

Fliegel : Yes, to first order only. If UT0 offends people, we will just drop it and say that we are preparing estimates of UT1.

Guinot : I would like to raise also the problem of the duration of the day, as obtained by laser techniques. We have found it rather difficult to compare with Universal Time. It is not clear whether, when we integrate these values, we get an accumulation of the

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random errors in addition to UT1. In particular, with duration of the day only, we cannot do the integration if there is a gap in the data. Is it possible to have the position of the Earth with respect to the LAGEOS orbit, and can we use that to bridge the gaps ?

McCarthy : We also have tried to get UT1 by integrating the Texas length of day and found out that we could not, because of accumulated random error.

Melbourne : Our experience at JPL is that the error buildup in integrations of LAGEOS l.o.d. exceeds the differential error from Lunar Laser ranging after only three days.

Tapley : I think that the answer is that you cannot, if you treat the 5-day intervals as independent. The problem is that you are trying to use an Eulerian integrator on a quantity that itself has stochastic variations. What you might do is to model it as a first order Gauss-Markov process, in the form of a Kalman-filter type sequential estimator, and use the input values of UT1 as random observations. Over a long interval, you might be able to determine a pseudo-measurement process noise that could provide a basis for extrapolation. We have used such an idea in satellite altimetry and it works pretty well there.

YE : For long-term determination of UT1 from laser ranging, we must know the lunar and satellite motion to higher accuracy than is now possible. I think that the geometric determinations from VLBI will be much better. Lunar and satellite laser ranging can contribute much to short-term variations in UT1, but not to the long-term stability.

Melbourne : The current uncertainty in the angular rate of right ascension of the Moon is 0.06 milliseconds per year, and that number is improving rapidly with time. Even though VLBI is a geometric technique, you should not underestimate the power of Lunar Laser ranging to establish a fairly stable frame.

Silverberg : I am not unbiased, but it seems to me that the strength of the lunar method is the economy and speed with which UT estimates can be obtained. We believe that it is possible, and we hope to demonstrate soon, that we can have an estimate of Universal Time from the observing site within an hour after the last observation on any given day. This can be done from every station on the globe, so weather fluctuations should be well covered.

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