Annex to Newsletter 3 of the subgroup T5

Individual answers and comments to Newsletter 2

(7 February 2000)

FROM P.S. MATHEWS , 16 August 1999 in ageement with V. Dehant

It seems to me that the first question to be decided is whether the definition should be such that it can be implemented uniformly for all techniques.

A. If this is not necessary, and the current IERS practice of estimating celestial pole offsets is to be continued, then I would stand by my proposal of Journees 1998 for the definition of the CEP. Then my answer to Question 3 of your Section 6 would be: C1

B. On the other hand, if the answer is yes - and that appears to be a reasonable view to take - then the definition of the CEP will have to be by convention, on the basis of some model. In that case :

(i) My proposal (Journées 1998) will have to be modified on the lines of the suggestion made by Christian in his email of the 30th July (see below). The note appended below elaborates on this suggestion.

(ii) My answer to Question 2 of your Section 6 would be : A1, but with the phrase "in the CRS" left out. (Any part of the motion can be assigned arbitrarily to either the CRS or the TRS according to one's whim; so I believe it to be not meaningful to talk of "part of the motion in the CRS" or "part of the motion in the TRS". Your phrase "motion in the CRS" was probably intended to mean "motion due to causes of extra- terrestrial origin"; but even the dynamical causes are not always clear-cut: some might consider ocean tide effects on Earth orientation to be of terrestrial origin, while I am inclined to treat them as the indirect effect of the gravitational tidal perturbations and I do compute them as such. I feel, for these reasons, that A2 is not a good statement.)

(iii) In regard to Question 3, I do not favour any of the options options C2, C3, or C4. It appears that none of them would correspond to the procedure envisaged in the Note below. I would class it as :

C.5: to process the observations to extract, in one step, all the unmodelled motions of the CEP from the estimated coordinates of the pole in the TRS.

I feel that the introduction of the instantaneous rotation axis (IRA) as envisaged in C4 is an avoidable complication. The IRA would be a poor choice for estimating the residual nutational motion (i.e., the error in the conventional model used) because the amplitude of a component of period T sidereal days in the CRS would appear reduced by a factor (1/T) in the motion of the IRA.

Irrespective of case A. or case B. above, my answers to your other questions are :

Q. 1. Definitely yes.

Q. 2. Linked to question 3; see above. The classification into dynamic and frequency approaches is not clear-cut. My approach, for instance, does consider different frequency intervals separately, but within each interval, the quantities to be estimated are functions of time, not amplitudes of spectral components (which would be estimated only in a second step, just as amplitudes of circular nutations are being estimated now). I feel that dynamics enters here only in respect of the question whether it can be adequately modelled or not. If there are two unmodelled effects, one of extraterrestrial orgin and another of terrestrial origin, and if both are within the same frequency interval, the two can be estimated separately in accordance with the "dynamic" approach (the first in the CRS and the other in the TRS) only if one does the estimation in the frequency domain, i.e., only if one knows the actual frequencies involved and estimates the amplitudes at the respective frequencies. So here is an unavoidable "mixing" of the dynamic and frequency domain pictures.

Q. 4. Yes. I do not believe that the length of the interval between observations is relevant to the assignment of any part of the motion as celestial or terrestrial, or to the question whether some part of the motion can be modelled or not, or to the possibility of estimating the various parts that correspond to different regions of the frequency spectrum. If I am overlooking something I would be very grateful if somebody could enlighten me on the problem.

Q. 5. I don't see any reason why not; certainly, I see no difficulty in evaluating the partials. But I have no first hand experience, of course.

Q. 6. No

Note :

1.1. The capabilities of VLBI to make accurate measurements of the Earth's orientation in space is not shared by other techniques – at least, not to the same degree. So, for uniform applicability of a new definition of the CEP, I think it would be necessary to specify the celestial motion of the CEP by convention.

1.2. In that case, any deviations from the model adopted by convention will have to be included, along with all unmodelled effects, in the terrestrial motion of the CEP. In particular, imperfections in the nutation model used to define the motion of the CEP in space would appear as retrograde diurnal signals in the terrestrial motion of the CEP. Atmospheric effects on nutation would be among these.

2. With the above scenario, the proposal that I had made (Journees 1998) for the representation of the celestial and terrestrial motions of the CEP (including the high frequency terms) should be modified along the lines suggested by Christian in his email of the 30th July 99.

3. Conventional Model :

3.1. The nutation series that I have now is based on geophysical modelling, and it provides a pretty good fit to observations. It includes not only the direct effect of the tidal gravitational perturbations on the solid Earth, but also the indirect effect due to the ocean tides produced by the same gravitational perturbations. Any better nutation series, if available, would necessarily have to include such indirect effects (as was the case with the KSV series employed in IERS 1996).

3.2. In other words, if such a series were to be used, by convention, to define the celestial motion of the CEP, it would automati- cally include the effects of the retrograde diurnal ocean tides.

3.3. I believe it would be logical then to include also the effects of ocean tides in other parts of the tidal spectrum in the conventional model – if these effects can be theoretically predicted. I believe that this can be done on the same lines as for the retrograde diurnal tides, and I intend to try to do this in the near future.

3.4. In any case, I am of the firm opinion that the diurnal and semidiurnal nutations (in space) which are indeed predictable, should be part of the conventional model. It would be illogical not to include them.

4. The conventional model envisaged in Sec. 3 above would imply that the series proposed for $\delta\psi$ and $\delta\epsilon$ in my Journées 1998 paper would be modified to include also terms with negative n : $n = -1$ for the prograde diurnal nutations as well as the long period ocean tide terms (assuming that the latter can be reasonably well modelled); $n = -2$ for the prograde semidiurnal nutations and for the prograde diurnal ocean tide terms; $n = -3$ for the prograde semidiurnal ocean tide terms; $n = 1$ for the retrograde semidiurnal ocean tide terms. All these are, of course, in addition to the long period nutations comprised under $n = 0$, and like the latter, will be fixed be determined from theory.

5. The motion of the CEP in the TRS would be described by the series given for $x_p(t)$ and $y_p(t)$ in the paper referred to, but with n no longer nonnegative. The free core nutation, for instance would appear under $n = -1$.

6. It is envisaged that any significant periodic signals in the spectra of $x_p^{(n)}(t)$ and $y_p^{(n)}(t)$ would be identified and their amplitudes estimated. In particular, those pertaining to n=-1 (with sign reversed) would provide estimated corrections to the amplitudes of forced nutations as well as the amplitude of the free core nutation. These corrections would then be the focus of future efforts at further improvements in the modelling of nutations.

7. The position of the J2000 pole does not coincide with the z-axis of the

CRF: there exists a constant offset between the two (see discussion between Eubanks, Ma, ... , sent by Veronique). This fixed offset needs to be taken into account in the CEP.

ANNEX : e-mail from CH. BIZOUARD to S. MATHEWS, 30th July

Concerning your astrometric modelling :

I see one big inconvenient : to split high frequency polar motion of geophysical origin in "terrestrial" terms (p) and "celestial terms" (P). In order to make interpretation, we would have to proceed two steps :

1- to demodulate parameters associated to prograde/retrograde diurnal, semidiurnal, ... frequency bands

2- to combine terrestrial and celestial parts, in order to reconstitute for instance the polar motion motion caused by diurnal tides.

Therefore I propose a slightly different procedure by estimating everything "in" the Earth, exept the long periodic nutations (even this point can be discussed). Of course this would break the symetry between the celestial point of view ands the terrestrial one. Only two parameters would be estimated "in space" (the classical pole offsets) and the remaining ones "in" the Earth (two for the long periodic polar motion, 4 for the diurnal band, 4 for the semidiurnal band etc...). This way would allow us to remove the step 2.

I would be even more extremist : I propose to estimate also the long periodic nutation as a retrograde polar motion, but by keeping the principle of your astrometric modeling : thus it would be estimated globally as a retrograde diurnal band, then demodulated by the frequency Ω for reconsituting the "nutational" effect.

The spirit of my CEP is the following : its spatial motion contains what can be modelled in the spatial motion of the geographic axis or the figure axis. Thus the spatial motion of the CEP is fixed by a conventional model, wheras its polar motion contains the complementary unknown shift between the figure axis (or the geographic axis) and the celestial frame. Only the polar motion would be subject to astrometric determinations.

FROM S. LOYER, 19 August 1999

1. Do you agree that a new definition of the CEP is necessary ?

Yes , a new or more precise definition is necessary in order to take into account the observed high frequency motions in the conventional definition. I will not say that a "new definition" is necessary but a "more precise definition" or an "extended definiton" based on the existing one.

2. Which option do you support for a new conceptual definition : dynamic approach (A1, A2 or A3) ? or frequency approach (B1 or B2) ?

I am in favour of the proposition A3 for the conventional definition for the following reasons :

a. It is not difficult to define.

b. What happens to high frequency motion is clear :
$\rightarrow$ it is considered as "polar motion" what ever could be the origin of this motion. This aspect of the definition will help to avoid confusion between concept and physical causes of the motion because concept and physical causes are no more related.

c. It corresponds also to a clear frequency separation : absolute value of spatial frequency lower than 1/2 $\rightarrow$ celestial part; all other motion $\rightarrow$ terrestrial part.

d. It is compatible with "old" observational strategies (estimation of five parameters at 1per/day rate (or less).

e. It is compatible with intensive or subdaily estimations of EOP. (the A2 (actual one) is confusing because it splits identical motions (to the observational point of view) into spatial and terrestrial part of the conceptual pole.)

3. Which option do you support for a new realization of the CEP (C1, C2, C3 or C4) ? C3 is compatible with the A2 concept. This option can be precised : polar (or terrestrial motion) can be estimated according to the following ways depending on the type and/or density of the observations :

a.five parameters per day as usual,

b.the two celestial pole offset per day + terrestrial pole offsets as numerous as possible/or necessary,

c.the two celestial pole offset per day + diurnal and sub-diurnal tidal waves for terrestrial motion,

d.the two celestial pole offset per day + terrestrial waves + (if possible) additional stochastic pole offsets e.long periodic waves for nutation-precession and any possible combination for the terrestrial motion. ... any other possibility keeping the rule that no high frequency motion appears in the terrestrial part.

4. Do you think that the use of one of these options can resolve the overlapping between terrestrial and celestial motions in the case of few hours estimates of the EOP ? Yes, for the observational point of view. But overlapping problem exists in this case as in other cases. We can propose some convenient procedures depending on the point of view : Observational point of view : see answer 3. Theoretical point of view (the building of models) :

- One should compute astronomical torques and should express the results following the above rules : low frequency motion in space into celestial part and high frequency motion expressed in term of polar motion.

- One should compute "geophysical" torques or excitations and express the results in term of polar motion unless for retrograde diurnal part in Earth that can be expressed in terms of corrections to nutation (this stands also for FCN resonant terms).

Comparisons between observations and models : - The residuals in the overlapping bands can be interpreted either in term of nutation or polar motion ... The Earth rotation observations alone cannot help to know which causes are involved in the observed overlapping motions ! They can just help to detect the quality of the models (including all causes, astronomical and geophysical together).

5. Do you think that such option can be implemented easily in the soft-

ware for processing the data ? Yes.

6. Have you another suggestion which can be discussed among the subgroup T5 ? As mentioned in the answer 4. the way of publishing the theoretical models should be precised.

FROM J LIESKE, 25 August 1999

1. Do you agree that a new definition of the CEP is necessary ? Yes, although I prefer to think that it is necessary to define a conventional interpretation of the meaning of Celestial Ephemeris Pole parameters

2. Which option do you support for a new conceptual definition : dynamic approach (A1, A2 or A3) ? or frequency approach (B1 or B2) ? I prefer A3, that the long periodic part of the predictable motion of the CRS is considered as the celestial motion of the CEP, the other part of the motion of the pole is considered as polar motion of the CEP.

3. Which option do you support for a new realization of the CEP (C1, C2, = C3 or C4) ? I support option C2 which puts all the diurnal and sub-diurnal motions both in the CRS and the TRS into estimated polar motion of the CEP which can be analyzed in a second step for providing the high-frequency signal.

4. Do you think that the use of one of these options can resolve the overlapping between terrestrial and celestial motions in the case of few hours estimates of the EOP ? Yes. We have a "simple" model and everything else (which might be of great interest to specialists) is included in the correction terms.

5. Do you think that such option can be implemented easily in the software for processing the data ? Yes. And the reference model will be as simple as possible for those who are not vitally concerned with all the nuances of the various frequencies.

6. Have you another suggestion which can be discussed among the subgroup = T5 ? No. I think you've given an adequate number of options.

FROM A. BRZEZINSKI, 2 September 1999

1. About the instantaneous rotation pole (IRP).

The IRP split up Earth rotation uniquely into the terrestrial component (polar motion) and the celestial component (nutation), and this statement remain valid independently on which the frequency range is taken into account. (By the way, in a view of this fact the word "arbitrary" used in the fourth sentence of the section "The choice of the CEP", taken from our Journées 98 paper, can be a little bit misleading, when not properly understood). The relationship between these two components is simple in the frequency domain, as illustrated by the well-known geometrical interpretation of Poinssot, but is difficult to be accomplished in the time domain where the continuity of periodicities are mixed together. And coming to your questionnaire - I am strongly against turning back to the old idea of using the IRP as a pole of reference, as long as monitoring of Earth rotation is based on the measurements of space geodesy. The reason is that the space geodetic techniques do not observe the motion of the IRP (at any frequencies, including diurnal and subdiurnal ones), as the arguments of Jeffreys (1963) and Atkinson (1973, 1975) remain valid in this case. Only such instruments as superconducting gravimeter, ring laser gyroscope or superfluid helium gyroscope, are capable of measuring the motion of the IRP, but they are still far from the operational stage.

2. About the realization of the CEP.

Let me briefly discuss two possible ways of monitoring subdiurnal variations in Earth rotation.

2.1. Determination of the parameters with a short sampling interval, say of the order of 1 hour. In this case we can use only 2 parameters for describing the direction of the CEP, say the terrestrial coordinates [x,-y]. Its celestial component should be defined by the a priori model. This model can include the celestial offsets determined in a standard way, or not. In the case of VLBI observations the last 2 options are equivalent, because when applying the second one the celestial offsets can easily be recovered numerically from the hourly time series [x(t),-y(t)]. But this is not the case for other techniques in which there is degeneracy between the diurnal retrograde component of polar motion and other parameters, such as the orbital ones.

Better consistency with the VLBI measurements of polar motion is obtained in this case when adding the VLBI empirical values of the CEP offsets to the conventional precession/nutation model in the estimation procedure.

2.2. Determination of the parameters with a sampling interval of 1 day (or longer) and including subdiurnal components in a form of the model proposed by Matthews (1998). Note that this kind of parameterization, though originally devised for the author's conceptual definition B1 (see ibid. or section 3.V in this Newsletter), can easily be extended for any other conceptual definition because the parameters of the n-th celestial component (i.e. $sin\varepsilon_0 d\psi(n)$ and $d\varepsilon(n)$) are completely equivalent to the terrestrial component with the negative index -(n+1). An extreme case, but still equivalent from the point of view of parameterization of the transformation TRS$\longleftrightarrow$CRS, as can be deduced from the paper (Brzezinski and Capitaine, 1993), is such that we move all the celestial terms in the model 3.V to the terrestrial counterpart, which extends the summation to the range from -N-1 to N, where N is a certain integer. (N=0 corresponds to the standard VLBI determinations, for N=2 the model would cover all diurnal/semidiurnal components both with respect to the TRS and to the CRS. In the last case the number of parameters is 12 at each epoch t). From the point of view of time series analysis, each component of this model is the complex demodulate of polar motion at frequency $n\Omega$. In other words, the variations with frequencies near $n\Omega$ are expressed as slow modulations of the complex sinusoid with frequency equal exactly to $n\Omega$, which do not require short sampling interval. Such model follows closely what is done in the real world when observing nearly diurnal retrograde variations in polar motion in the celestial frame.

Detailed comparison of the options 2.1 and 2.2 is not possible here, let me only make a few remarks. The second option, though not completely equivalent to the first one, offer several advantages which make it very attractive.

- The choice of the option 2.2 makes the issue of the subdiurnal EOP estimates (question 4 of your inquiry) out of the context.

- If a certain technique cannot estimate nearly diurnal retrograde variations in polar motion because they are correlated with other parameters relevant to this technique (e.g. the orbital ones), it is enough to remove the corresponding term (i.e. the one with $n = -1$ in the terrestrial representation) from the model.

- This kind of parameterization of Earth orientation is also convenient for geophysical interpretation, because the subdiurnal estimates of the excitation function (e.g. AAM) can easily be decomposed numerically in the similar manner (Bizouard *et al.*, 1998; Petrov, 1998, Ph.d. thesis; Petrov et al. in Proc. Journees'98).

- As mentioned already above, with N=2 this model includes all diurnal/semidiurnal components both with respect to the TRS and to the CRS, which have been recently taken into account.

- It can be shown that if the row measurements used to estimate the EOP enable hourly time resolution in the option 2.1, these measurements are also far sufficient to resolve the model 2.2 with N=2 and sufficient for N=3. Moreover, I am deeply convinced that such a model can be easily implemented in the software for processing the data.

In conclusion, at least as far as the regular subdiurnal (that means without gaps) monitoring of Earth orientation cannot be guaranteed, the option 2.2 of the realization of the CEP, supplemented by the a priori precession/nutation model, seems to be a good solution.

3. About the conceptual definition. I discuss this point intentionally as the last one because it seems to be much less important than the issue of practical realization. Personally, I would support option A1 (which in fact does not differ significantly from the idea of Prof. Yatskiv, as expressed by option A3; see also recent discussion by Christian Bizouard). My arguments are the following :

- This option is consistent with the current definition of the CEP with respect to the nutation (to the accuracy of diurnal/semidiurnal terms in nutation, which are of little importance even at the microarcsecond level), and to the word "ephemeris" (see point 2 of the comments of Dennis McCarthy).

- All geophysical effects (that is due to the angular momentum exchanges between the solid Earth and geophysical fluids) are referred to the Earth-fixed frame, which is consistent with the fact that global geophysical processes perturbing Earth rotation are also observed in the terrestrial frame.

This is the “polar motion gauge” of the parameterization of Earth rotation, strongly preferred by Eubanks (1993).

- The terrestrial motion (polar motion) of such a conventional pole has a clear physical interpretation: this is polar motion of the angular momentum axis of the whole Earth (including outer fluids), from which the lunisolar effects have been removed (Brzezinski, 1992, Sec. 2.3.1). An final remark is that in the formulation of the options A1 to A3 I would add the phrase “caused by external gravitational forces” to “the motion in the CRS”.

FROM D.D MCCARTHY, 7 September 1999

1. I certainly agree that a new definition of the CEP is required.

2. While what you call a dynamic approach is desirable, I think that the frequency approach is the only one which will be unambiguous for the user. Therefore, my vote is for the frequency approach.

3. I would favor your C1 option.

4. The actual procedures used in observing and treating the observations will determine how well the motions are separated, but I believe the C1 option has the best chance to make the situation less confusing.

FROM Ch. BIZOUARD, 7 September 1999

The astrometric modeling proposed by Pr. Mathews constitutes a generalization of the current and practical definition of the CEP. It consists in introducing astrometric parameters for each frequency bands in the spatial oscillation and terrestrial oscillations of the CEP.

Actually it should be well understood that the CEP is a practical way in order to account for the spatial oscillations of the Earth's geographic axis and the diurnal rotation around the instantaneous rotation axis. Whereas it keeps a geometric meaning, it is enough closed to the instantaneous rotation axis for reckoning the universal time.

The classical definition of the CEP restricts the spatial oscillation of the geographic axis to long period terms (the so-called precession-nutation) and to a prograde diurnal band, which is represented in the terrestrial frame as a long period polar motion. The CEP itself consitutes the border between the precession-nutation and the long polar motion. Any determination of the CEP, according to this definition, involves long period corrections to the precession-nutation, the so-called celestial pole offsets, and long period polar motion.

The modeling of Pr. Mathews extents this determination to any prominent frequency bands of the spatial oscillations of the geographic axis. In turn it involves the following frequency bands in space :

- band 0 : long period (classical CEP)
- band 1+ : prograde diurnal (classical CEP)
- band 1- : retrograde diurnal
- band 2+ : prograde semi-diurnal
- band 2- : retrograde semi-diurnal
- band 3+ : prograde ter-diurnal
- band 3- : retrograde ter-diurnal.

Moreover the modeling of Mathews is based upon two requirements :

1) Any frequency band is represented by a parameter varying slowly (with respect to 24 hours)

2) The celestial pole offset involves only long period terms, and retrograde n-diurnal oscillations; the polar motion involves only long period terms, and prograde n-diurnal oscillations

These two requirements are already followed by the classical CEP for which only band 0 and 1+ are concerned. Indeed, band 1+ is estimated as a long period pole motion of the CEP.

The other n-diurnal bands have to be estimated as it follows :

1- : retrograde diurnal celestial pole offset
2+ : prograde diurnal polar motion
2- : retrograde semi-diurnal celestial pole offset
3+ : prograde semi-diurnal polar motion
3- : retrograde ter-diurnal celestial pole offset

Moreover they are mapped into long period oscillations by representing it as a pure circular n-diurnal signal times a parameter depending on time. This parameter provides us with the whole signal of the frequency band. We deal totally with 6 parameters since we consider 6 frequency bands.

These parameters can be estimated easily and together from a set of 24-hours VLBI sessions. This is certainly the most interesting feature of the Mathews modeling. Classicaly the bands 1-, 2+, 2-, 3+, 3- are estimated after the adjustement of the classical EOP parameters (band 0 and 1+). By applying the Mathews modeling, we could obtain the whole information in one step.

Therefore this proposal constitutes a very astute generalization of the classical definition of the CEP.

However I have to point out that :

- The requirement (2) is not obligatory and could appear as artificial. It is only justified from an esthetic point of view : the symetry between "polar motion" and "celestial pole offsets" is not broken. But we could very well estimate the retrograde n-diurnal bands as a polar motion. As the corresponding oscillations are mainly caused by geophysical processes, it may be better to estimate it directly as a polar motion. The modeling of Mathews can be modified accordingly without destroying its main interest, already mentionned. This remark raises the problem how we have to represent conventionaly these n-diurnal bands (except 1+ because it is already stated as a long period pole motion).

- The estimated parameters, except for the band 0 and 1+, have to be "demodulated" in order to interpret the information they contain. This need a supplementary computation after the estimation.

CONCLUSION : the astrometric model of Mathews for the CEP leads to an simultaneous estimation of the spatial oscillations of the geographic axis from a few hours to several days from 24 hours VLBI sessions. The splitting between celestial component and terrestrial ones is only motivated by mathematical esthetism. It can be discussed, and the Mathews modeling can be modified accordingly without removing its main philosophy.

FROM J. KOVALEVSKY, 21 September 1999

Answer to "Computational consequences"

1. Basics

In the problem related to the transformation from the TRS to the CRS, The following statements should, in my opinion, be the basis of new definitions and procedures.

A) The ideal and correct transformation is an Eulerian 3 angle transformation from the true (instantaneous) Earth equator as projected on the celestial sphere, perpendicular to the instantaneous pole of rotation IP, to the CRS defined by its fixed system of coordinates Oxyz with an origin called ? on the principal plane.

B) Because IP is not easily and readily accessible, it seems unavoidable to have another system of eqautorial celestial coordinates OXYZ with its OZ axis close to the IP. This was the role played by the CEP.

C) The definition of such an axis must be such that it is accessible by some treatment of observations with, however, a clear physical meaning. Therefore, I think that it **must not** be based upon an ephemeris in order to be independent of any further change in the theory. The latter can be used to predict a position, as in all other cases in ths solar system, but not to define it. For this reason, not only do I reject the present CEP, but also any other definition involving a theory.

2. Consequences

The difficulty in the realization of the IP is due to the presence of very short periodic terms, whether they are of geophysical origin or part of the nutation. Since the predictability is most difficult for polar motion, only a global numerical treatment of observations is possible. So, my proposals are :

A) To define a "mean" celestial equator with the corresponding Mean Celestial Pole (MCP, or Celestial Reference Pole, CRP) obtained from the motion of the observed directions of the pole in the CRS after filtering out all terms shorter that 1.5 days (for instance). These are taken out and assigned to polar motion. Note that the corresponding precession-nutation theory is the theory without the short period terms, but with possible resonances between them. This goes along with the proposal by Mathews.

B) To the departure point σ on the mean equator will correspond a mean stellar angle S. Then, if N is the ascending node of the mean equator to the CRS, the transformation from the mean equatorial system to the CRS is defined by the Eulerian (3-1-3) matrix M with :

$\psi = \gamma N$
$\theta = inclination$
$\phi = N\sigma + S.$

With, as usual, $N\sigma = \gamma N + Q$, where Q is an integral over time that involves only ψ and ϕ.

C) The best observed position of the IP should be referred to the CMP and the mean equatorial system and transferred to the CRS by the matrix M (unless it is obtained directly in the CRS). This produces increments $\Delta\psi$, $\Delta\theta$, $\Delta\phi$. Put :

$\psi' = \psi + \Delta\psi$
$\theta' = \theta + \Delta\theta$
$\phi' = \phi + \Delta\phi$

One gets a new N' and a new σ'. The new departure point is defined by

$$N\sigma' = \gamma N' + Q'(\psi', \theta').$$

Q' departs from Q only by the effects of short period terms. However resonances may produce long period effects. For this reason, I would prefer keeping $Q' = Q$ (what would it mean?). Part of the $\Delta\phi$ corresponds to the change from $N\sigma$ to $N'\sigma'$ and can be computed. The remaining is a correction to S and corresponds to the irregularities of the Earth rotation.

This procedure may seem complex. It has the advantage to have only 3 parameters defining unambiguously the CMP and separate completely the problems linked with the short period terms that are treated and analysed independently for a separation between nutation and polar motion terms.

At this level, the nutation theory will provide the short period terms, so that the determination of the polar motion terms will follow immediately.

FROM J. VONDRAK, 24 September 1999

1. Yes, I agree that a new definition of CEP is necessary.

2. I support the dynamic approach A3 (that however I feel is not in contradiction with A1, since I believe that only the external torques are really predictable).

3. I support option C4 for a new realization of CEP.

4. I think that the overlapping of celestial ad terrestrial motions can be resolved.

5. I think that the option can be implemented in the software.

6. I have no further suggestions.

FROM P. BRETAGNON, 24 September 1999

Comments about the transformation between the CRS and the TRS

The VLBI observations give the position of the figure equator and axis (TRF) with respect to the ICRF. Therefore, it seems better not to introduce an intermediate reference frame and to analytically represent the transformation between the TRF and ICRF with only three parameters. The comparison between observations and analytical solutions allows us to improve the geophysical models.

Besides,

- any separation between diurnal terms and long period terms is arbitrary;
- any separation between predictable and non predictable does not give a perennial definition;
- there is no sense to separate precession and nutation;

- we have to give up any quantity defined from a rotating origin (equinox of date).

Consequences

TRF has to be defined with the three Euler's angles ψ, ω and ϕ. These angles are analytically singular with respect to the ICRF. Therefore, as ICRF is defined close to the barycentric equator J2000.0, we have to define an ECRF (Ecliptic Celestial Reference Frame) close to the ecliptic J2000.0 by a rotation about the x-axis of, for example

$$\varepsilon_0 = +23°26'21.409000'' \text{ strictly}$$

Then, from the three precession-nutation Euler's angles ψ, ω and ϕ reckoned positively in positive rotation

- the figure axis is completely defined with respect to the ECRF (Ecliptic Celestial Reference Frame) by

$$\begin{array}{rl} & \sin\psi\sin\omega \\ - & \cos\psi\sin\omega \\ & \cos\omega \end{array}$$

- the figure axis is completely defined with respect to the ICRF by

$$\begin{array}{rl} & \sin\psi\sin\omega \\ - & \cos\psi\sin\omega\cos\varepsilon_0 - \cos\omega\sin\varepsilon_0 \\ - & \cos\psi\sin\omega\sin\varepsilon_0 + \cos\omega\cos\varepsilon_0 \end{array}$$

- the instantaneous angular velocity vector (p, q, r) is completely defined with respect to the TRF by

$$\begin{aligned} p &= \dot\psi\sin\omega\sin\phi + \dot\omega\cos\phi \\ q &= \dot\psi\sin\omega\cos\phi - \dot\omega\sin\phi \\ r &= \dot\phi + \dot\psi\cos\omega \end{aligned}$$

Note that the ICRF and ECRF (Ecliptic Celestial Reference Frame) do not define a dynamical equinox. Therefore, the integration constant ψ_0 at $t = 0$ (J2000.0) is not strictly equal to zero and the integration constant

ω_0 at $t = 0$ (J2000.0) is not strictly equal to $-\varepsilon_0$. Moreover, let us recall that to analytically locate the Earth, we have to solve a system of three second degree differential equations and thus to determine, by comparison with observations, six integration constants : ψ_0, ω_0, ϕ_0 and ϕ_1 which form the linear part of the Earth's rotation angle ϕ ($\phi = \phi_0 + \phi_1 t + \Delta\phi$) and lastly two integration constants which are the amplitude and the phase of the general solution of the Euler's reduced system.

FROM H. SCHUH, 4 October 1999

Before coming to a conclusion which is the best new conceptual definition of the CEP I thought about the requirements which should be satisfied by the new definition. Of course, one of the requirements is clarity but there are also several practical reasons from a space geodesist's point of view :

1. The conceptual changes should be such, that no or only very little efforts are needed if old space geodetic data are treated. If possible, a reprocessing should be avoided. There should be no visible differences (i.e. above the error level) in the long-term series of polar motion and nutation after the new concept was introduced.

2. If we want to measure diurnal and sub-diurnal variations of polar motion which are due to geophysical causes this should be possible by using a limited set of parameters (the bigger the variations of polar motion are, the more parameters xp, yp are needed). At present, in GPS and in most VLBI solutions the highest achievable time resolution is 1h-2h. (That's why the "Eulerian approach" is not feasable; it would require a high number of additional parameters which had to be determined to describe the big diurnal variations as mentioned correctly by A. Brzezinski in his remark 3).

On the other side we would also like to "see" the diurnal and sub-diurnal variations already from a single data set (24h in VLBI). There might also exist irregular short-period or quasi-periodic variations of polar motion with periods of a few hours, e.g. triggered by a strong Earthquake or by a typhoon, which we would like to observe just after it happened. Thus, solving for only two parameters per 24h-session is not enough.

3. In case we do not solve in the least-squares parameter estimation for the diurnal and sub- diurnal variations it must be easy to model these vari-

ations. If the diurnal variations are completely neglected, i.e. no correction model is used, (as done in many of the current GPS analyses) the errors should be small. This means we do not want to have very large diurnal variations.

4. We want also to determine corrections to the a priori nutation model (IAU 1980 or another one) but want to use as few parameters as possible for that (e.g. one $\delta(\varepsilon)$ and one $\delta(\psi)$ per 24 h as in the present approach). That's why having diurnal and sub-diurnal variations in polar motion and (!) in nutation would not be very nice and cause a lot of practical problems (too many parameters, correlations between them).

Summarizing the requirements given above one could say that we always have to consider the price what has to be paid when a new definition was adopted and whether we really gain something by the new definition.

Concerning the new concept (question 2 in Newsletter 2) I am clearly in favour of the dynamic approach A3 because it corresponds to all the requirements defined above. Then, short-period variations do only exist in the terrestrial reference frame, i.e. in polar motion. Correspondingly, I favor option C2 for the new realization of the CEP.

If this (A3 + C2) would be adopted it just has to be kept in mind that - a (very small) part of the observed long-period and secular polar motion (in the past and in the future) is in fact due to what has been called so far 'prograde diurnal nutation', - a (very small) part of the observed prograde diurnal polar motion is in fact due to what has been called so far 'prograde semi-diurnal nutation', - a (very small) part of the observed retrograde semi-diurnal polar motion is in fact due to what has been called so far 'retrograde diurnal nutation', - a (very small) part of the observed retrograde ter-diurnal polar motion is in fact due to retrograde semi-diurnal nutation (if exists?), - a (very small) part of long-period nutation is in fact due to retrograde diurnal polar motion.

This has to be considered, e.g. when new empirical models for the influence of ocean tides on polar motion are going to be derived from VLBI or from GPS.

Both, questions 4 and 5 can be answered 'yes' if the approach A3 is followed.

It has to be kept in mind that the FCN (and FICN, ...) will remain in the nutation series which will be determined by VLBI (as in the past).

Finally, I'd like to repeat that the so-called 'Eulerian approach' (with 3 parameters instead of 5) will not help us at all in processing space geodetic data because we then get much bigger diurnal variations due to errors of the a-priori nutation model and due to the FCN which cannot be predicted. Then we need a high number of parameters to be estimated by VLBI or to be entered in a correction model for GPS. Even if the a-priori diurnal variations could be predicted very precisely (and thus less parameters had to be estimated in the least-squares fit) the two effects, polar motion and nutation, had to be separated in a second step according to their different causes. The problem would not be solved but just transfered to a later and probably even more problematic investigation.

I hope these remarks will help for the discussion and I'd be glad to learn if I am wrong.

FROM Chopo MA, 5 October 1999

Subject : In answer to the specific questions of the Newletter 2

1. A new definition is needed as the nutation models include high frequencies not considered by the current definition.

2. I prefer pole concept A2 since it appears to be most physical with all motions in the space frame considered as nutation. However, I am not completely clear about the the analysis consequences since you indicated that some of the high frequency nutation terms would appear as constant(?) offsets in polar motion.

3. I would lean to a realization similar to C2 in the sense that high frequency and transient effects would be extracted from polar motion estimates, either harmonic terms or as a time series with short (subdiurnal) intervals. However, I am not clear how this interacts with the pole concept A2.

4. It is not clear to me that any definition will allow the separation of errors in modeling subdiurnal nutation and polar motion, i.e., saying that the observed differences from the models are all nutation or all polar motion or some specific proportion of the two motions.

5. The implementation in our VLBI software would be substitution of new series for nutation and high frequency EOP and a new precession constant. The structure of the program based on the current, complicated set of rotations would not be changed.

FROM Juergen MUELLER, 5 October 1999

(Lunar Laser Ranging (LLR) data analyses at the Technical University Munich)

Harald Schuh from DGFI (also in Munich) has drawn my attention to your discussion in the Web about a new definition of the CEP. I have read your homepages carefully and want to answer your questions raised at the end of your web page.

ad 1) The most important fact is that you clearly say, what your new definition of the CEP contains. Which parts are nutation? Which parts belong to polar motion? Moreover one should consider how most analysis centers work. That means, any changes should be done in a way that it can be easily performed by everyone too.

ad 2) and 3) Therefore I prefer the options A3 and C2, where the distinction between nutation and polar motion is made in the frequency domain. And simultaneously, many of the traditional procedures can be kept.

ad 4) I think it is difficult to resolve the overlapping.

ad 5) It should cause no problems to implement these option in the analysis software. (For LLR the situation is still more comfortable. The set of LLR observations is small enough that one can reprocess all the data one has. Therefore many of the options discussed on your web page could be used. But one has to consider that many other techniques like VLBI are not able to reprocess all their data!)

FROM Ya. YATSKIV, 14 October 1999

I answer your questions to the sub-group T5 :

1.Yes

2.A3 and B1

3.C2

4.Yes

5.Yes

6.A1 and A3 could be considered as complementary(or combined).