

## **Annex to Newsletter 5 of the subgroup T5**

### **Individual answers and comments to the Newsletter 3 and Newsletter 4**

(21 March 2000)

#### **1 Answers and comments to Newsletter 3**

FROM B. GUINOT, 10 February 2000

My reactions to the proposals of Newsletter 3 are as follows.

I strongly support proposals 1 to 4 (espacially 2).

For proposal 5, a change of name is preferable. It could be mentioned that the CEP was a realization of the new pole. "Celestial Intermediale Pole" is a better description of what is meant ; however, it would lead to "Celestial (and Terrestrial) Intermediate Origin on the equator (if the concept of non-rotating origin is adopted), which seems a bit strange. For this reason, I have a slight preference for "Celestial Reference Pole".

No comment on 6.

In the wording of 7, I prefer "by the external forces acting on the Earth" and "the motion is considered after filtering out terms of period shorter than 2 days".

No comments on 8, 9, 10.

Concerning 11, the definition of a MCRP, I am reluctant to such a definition. It might generate difficulties similar to those due to the separation of precession and nutation, with the introduction of cross terms in the processing of observation, at least in principle.

In 12, the reference to C3 and C2 should be explicited.

FROM S. MATHEWS, 16 February 2000

It does not matter at all whether signals in particular frequency ranges are estimated in the CRS or TRS. All that is required is that everything in one continuous band of width 1 cpsd must be estimated in the same frame. For example, the Free core nutation must be estimated along with other

nutations (of frequencies between -0.5 and +0.5 cpsd in space) in the CRS though the physical origins of the two are different. Don't you agree?

FROM S. MATHEWS, 9 March 2000

You say (answer to N. Capitaine):

"Concerning the diurnal and subdiurnal nutations, my opinion is that they do have to be taken into account in the a priori model, but in the a-priori model for polar motion (in the TRS) for the reasons explained in the Newsletter 3. The difference between the proposal of the Newsletter 3 and yours (Journees 1998) is that all the motions, except those which are long-period ones in the CRS, are considered to be in the TRS."

It was natural that the nutations which have periods  $> 2$  days were thought of and estimated as motions in the CRS and polar motion (essentially the parts with periods  $> 2$  days in the TRF) to be thought of and estimated as motions in the TRF, since the estimation was being done from VLBI data at intervals of a few days.

Now we have a means of estimating also the higher frequency components of the motion in either frame from the same kind of data.

The essential content of my Journees 1998 proposal was the method for estimation for the higher frequency components, whether in the CRS or TRS. The suggestion for distribution of the different frequency components between the CRS and TRS was incidental, and was purely a matter of aesthetics. As I have said many a time since then, it does not really matter what part of the higher frequency spectrum is estimated in one frame and what part in the other, or whether all of it is estimated in just one of the two frames.

I regard to the place of the diurnal and semidiurnal nutations in the a priori model, what you propose would involve:

- (1) Transferring the computed diurnal and semidiurnal nutation terms for the nonrigid Earth from the CRS to the TRS for inclusion in the a priori model of motion in the TRS;
- (2) Estimation of corrections to these from observational data, and adding them to the a prioris; and
- (3) Transferring the corrected terms back to the CRS.

If the diurnal and semidiurnal nutations are included in the a priori model for motion in the CRS, then what is involved is

- (1) Estimation, in the TRS, of amplitudes of signals (due to any deficiencies in the model) at the corresponding positions in the TRS; and

(2) Transferring these to the CRS, where they will be applied as corrections to the a prioris.

That is one step less; but what is more important in my view is that the a priori model places these terms where they really ought to be: as nutational motions in the CRS.

In practical terms, both the procedures are absolutely equivalent; I would only plead and hope that the more natural and the (slightly) simpler course that I am suggesting would find favour!

FROM S. MATHEWS, 11 March 2000

The conceptual definition is indeed dependent on the choice made.

I must admit that I hadn't given thought to the fact that the diurnal and semidiurnal nutations, when viewed in the TRS, would appear in the same bands as long-periodic and diurnal polar motions of terrestrial origin. So it is unlikely that residuals of the these nutations can be seen against that kind of background.

Having said that, let me see if I can persuade you that for the same reasons that you say, it would be best to leave the diurnal and semidiurnal nutations in the CRS itself.

(1) The transfer function (rigid  $\rightarrow$  nonrigid) is the same for the celestial motion (nutations) as for the corresponding terrestrial motion (wobble). So step (1) is not unavoidable: it is unnecessary: the transfer function can be applied in the CRS itself.

(2) Transforming these nutations to the TRS would have served some purpose if the intention was to make estimation of corrections to the a priori model possible; but we have agreed that these signals are already very small and that the corrections, being even smaller, are much too small to be estimated. So why bother to bring something which is clearly of celestial origin to the TRS?

In short, there is no step to be taken at all if these nutations are just left as part of the a priori model for motions in the CRS.

I am not clear about the trend of thought in your last para:

"An other reason is that the estimation of the high frequency terms in a second step can be applied to all the techniques, whereas your method can only be applied to VLBI. Perhaps in that case it can be more efficient."

I have been able to figure out why my method is applicable only to VLBI. It permits high frequency signals to be extracted even from data with points once every few days, but I would think that the method would

be even more effective if the data are much more dense in time. Or did you mean something else altogether?

I am also not sure whether you are suggesting that corrections to the a priori model for high frequency nutations may possibly be estimated in a second step through techniques other than VLBI. How would the dominance of the phenomena of terrestrial origin be avoided in those techniques? I must be missing something.

FROM D.D. MCCARTHY, 22 February 2000

#### Question 1

Proposal 1. Yes, they are not adequate for modern observations.

Proposal 2. Yes, a clear concept of the reference pole is the ideal.

Proposal 3. Yes, at least as accurately as to be used with the most accurate observations.

Proposal 4. Yes, the improvement in the definition of the CEP should have minimal impact to users. However, we should not rule out the possibility of some discontinuity.

Proposal 5. No. I see nothing wrong with an improvement in the definition but still retaining the old name. The original concept of the "ephemeris" pole was that the definition or the means of realizing it could be changed without having to introduce more names. We are refining the definition of the CEP - not defining a new pole. My sense of the situation is that most users will be less concerned with a refined definition of a familiar pole than they would be with a new name and a new definition.

Proposal 6. Yes.

Proposal 7. Yes, but this need some discussion

Proposal 8. Yes.

Proposal 9. Yes.

Proposal 10. Yes.

Proposal 11. No. I don't see the necessity for adding even more poles with hazy definitions. We are already confusing most users by refining the definition of the CEP.

Proposal 12. Yes.

FROM A. BRZEZINSKI, 8 March 2000

## General comment

The intermediate celestial pole has to be uniquely defined with respect to one of the fundamental reference systems involved, either to the CRS or to the TRS. An obvious choice is the CRS, not because of the "celestial" in the description of the pole but because only its celestial motion (in a classical sense of this description, that is meaning a slow, as compared to the diurnal cycle, motion in space) is well predictable. Then its description with respect to the TRS is just a consequence, because the combination of the celestial and the terrestrial motions must give the motion of the terrestrial z-axis in space, as expressed by eq.(40) of Brzezinski and Capitaine (1993), also reproduced as eq.(1) on p.3 of Newsletter 2. This basic fact is not sufficiently clear from your proposals. Any attempt to constrain simultaneously the celestial and the terrestrial motion of the intermediate pole can lead to internal inconsistency, as in point 11).

## Comments to the specific points.

Ad 1) Definitely yes.

Ad 2) Yes.

Ad 3) This is a strong, and may be not necessary constraint on the conceptual definition. I agree with Jan Vondrak (Annex to Newsletter 3) that only the external torques are really predictable. Therefore, when adopting this constraint, the definition can only refer to the lunisolar and planetary torques while rejecting all geophysical effects such as variations excited by the oceanic and atmospheric tides, which are not so well predictable (that is in fact a tentative conceptual definition proposed in point 7)).

Ad 4) Yes.

Ad 5) Generally yes. Abandoning the word "ephemeris" is in the spirit of point 2) stating that the new pole should be defined by a clear concept but not realization. The word "celestial" expresses in my feeling a general fact that only the celestial motion of the pole is well predictable while the terrestrial motion is largely unpredictable and has to be determined from observations. Since the "Celestial Reference Pole" can be associated with the "Celestial Reference Frame" and understood as z-axis of the CRF, I am in favour of the second description with a small reordering: "Intermediate Celestial Pole" with the acronym ICP.

Ad 6) What about the variations driven by diurnal atmospheric tides and the irregular free core nutation oscillation, which are both within this range

of frequencies? Does this statement not contradict a tentative conceptual definition given in point 7)? See also comment to point 3).

Ad 7) Concerning the first part, I would prefer the statement: "the pole of the intermediate equator of which motion with respect to the CRS is produced by the lunisolar and planetary torques" plus additional description of the pole/equator position at a specific epoch, e.g. J2000. In the second part I would say "including all terms with periods longer than 2 days".

Ad 8) Yes, but this is a consequence of adopting point 7).

Ad 9) According to "General comment", all components of the motion of the intermediate pole which are not included in the celestial motion defined in 7), are in the terrestrial motion. The terrestrial motion should be further split up into the predictable part (diurnal and semidiurnal nutation terms, diurnal and semidiurnal variations excited by oceanic/atmospheric tides) expressed by a model and unpredictable part (including Chandler wobble, annual wobble, FCN oscillation, etc.) which should be monitored; note, however, that this decomposition is already the problem of practical realization but not a part of definition.

Ad 10) It would be very good, nevertheless fulfilling both these requirements is probably possible only in the case of the VLBI observations. The other techniques can use the celestial pole offsets from VLBI as a priori values and estimate only the polar motion corrections.

Ad 11) As discussed in "General comment" above, you should not constrain simultaneously the motion of the pole in the CRS and in the TRS, because these motions are fully correlated to each other. One can of course define the "mean" pole which moves slowly in the TRS but this is another intermediate pole which differs from that defined in 7) by diurnal and sub-diurnal terms with respect to the TRS. In my opinion, the concept of "mean pole" is an unnecessary complication which can be even confusing, therefore I suggest to not introduce it.

Ad 12) I am deeply convinced that the option C3 with the procedure proposed by Mathews and discussed in my paper at the IAU Colloquium 178, can easily be implemented in practice and used to monitor the high frequency components of polar motion with daily averaging. It was demonstrated in the paper by Christian Bizouard and others, presented at the IAU Colloquium 178, and will be a subject of the paper by Mathews and Herring at the IAU Colloquium 180.

A final remark is that when discussing the realization of the new intermediate celestial pole, hence the composition of the transformation matrix between the TRS and CRS, we should not forget the axial component of

rotation, in particular the problem of modeling and monitoring the high frequency variations in LOD/UT1.

FROM A.-M. GONTIER, 9 March 2000

1. Do you support the proposals above (which ones from 1 to 12) ? YES  
FROM 1 TO 12

2. Which complementary comments do you consider as necessary ? /

3. If you do not support one or several proposals, which alternative proposals are preferable ? /

FROM L. PETROV, 13 March 2000

Are you agree that

Q1) The IAU 1980 conceptual and conventional definition of the CEP must be abandoned,

A1) Yes

Q2) the reference pole has not to be defined by its realization but by a clear concept not dependent on further improvements in the model,

A2) Yes

Q3) this pole must be defined such that it can be realizable by a model as accurately as possible,

A3) No. A definition of the EOP and an implementation are quite different things. Attempts to make a definition of the notions closed to the notions used in the current technique of observations and data analysis leads to contradictions when the technique is changed.

Q4) the change from the CEP to the new definition has to be as less as possible in its practical realization,

A4) I think it doesn't matter.

Q5) a change of name could be considered as the "Celestial Reference Pole" (CRP), or the "Celestial Intermediate Pole" (CIP),

A5) I propose not to use at all a word "pole".

Q6) concerning the motion with respect to the CRS, the choice should be to specify this motion by a model including only the components with periods longer than 2 days (such a model will automatically include all the geophysical perturbations such as the retrograde diurnal motion due to ocean tides),

A6) No. I believe that the definition should not specify frequency range. It is difficult to separate these constituents in theory and it is impossible to separate them in analyzing observations.

Q7) a tentative conceptual definition is “the Pole of the intermediate equator of which motion with respect to the CRS is produced by the luni-solar and planetary torque” (or ... “by the external gravitational forces acting on the Earth”),

as the high frequency motion is not considered in the nutation model, it may be possible to add to the conceptual definition that “the motion of the equator with respect to the CRS is considered for an Earth with an equatorial symmetry”, or that “the motion is considered after filtering out terms of period shorter than 2 days”,

A7) No.

Q8) the prograde diurnal terms in nutation should be considered as long periodic terms of the polar motion and the prograde semi-diurnal terms in nutation should be considered as prograde diurnal terms of the polar motion,

A8) No.

Q9) concerning the motion with respect to the TRS, the choice should be to sharpen the definition of the pole of reference by taking into account the prograde diurnal variations as well as the prograde and retrograde semi-diurnal variations as a predictable part of the polar motion which can be realized by a model,

A9) No.

Q10) the processing of the observations should include the estimation of the celestial pole offsets wrt the model for precession-nutation as well as the corrections to an empirical model for polar motion,

A10) NO, NO, NO! I consider the phrase “The processing of the observations should include” as a threat to a freedom of scientific work. An analyst should decide him/herself how to make an analysis of the observation.

Q11) a “Mean CRP” (MCRP) could be defined including only the long periodic part of the motion both in the TRS and the CRS, and then a “true CRP” as obtained by adding the high frequency components to the polar motion of the MCRP.

A11) NO. Let’s keep in mind an Occam principle.

Q12) a conventional procedure must be given for estimating the high frequency components in polar motion in order to provide the best realization of the pole in the processing of observations : C3 is proposed when possible or C2 in the more general case.



A12) NO, NO, NO! I consider it as a threat to a freedom of scientific work. Any attempt to establish a "conventional procedure" should be declared as illegal. I know at least 6 different ways to estimate EOP, and I think we should encourage appearance of new alternative schemes instead of lobbying some algorithm which seems to somebody today the best.

FROM J. VONDRAK, 14 March 2000

1. I am supporting all 12 proposals of Section 4
2. With no additional complementary comments. I think everything is clearly explained and defined
3. No alternative proposals are necessary.

FROM O. SOVERS, 14 March 2000

I'll just give you some random thoughts.

As far as fitting data to theoretical models is concerned, I think the only thing that matters is that the model should include all the important frequencies, both in TRS and CRS. Ideally it doesn't matter whether these frequencies come from well-understood physics or not, though it's unlikely that some totally new frequency will be discovered empirically.

If the above is accepted as true, then the separation into celestial and terrestrial parts is "only" a matter of conventions for the model, and book-keeping for the data analysis. The latter is not a serious problem, and thus it seems to me that the job of this subgroup is to arrive at a conventional theoretical separation of motions in the CRS and TRS. This separation should be somewhat immune to a burning need to revise it in a few years.

In my opinion, the 'preliminary proposals' in Newsletter 3 are a reasonable way of solving the dilemma. I look forward to learning more during discussions at IAU 180.

FROM V. DEHANT, 15 March 2000

- (1) The proposition of Christian Bizouard based on Sonny Mathews' first proposition.
- (2) The only problem I have is related to the fact that it is difficult to predict the FCN free nutation; this is 0.1 to 0.2 mas and may change; so a

residuals will still remain in space. The evaluation in the terrestrial frame does not contain any retrograde diurnal part. I did not see any proposition which could incorporate this, but I might have overlooked something.

FROM H. SCHUH, 15 March 2000

Concerning the Preliminary Proposals (paragraph 4 of Newsletter 3) my answer is: 1) to 4) I agree 5) I also agree and prefer 'CIP' 6) I agree 8) to 10) I agree 11) is not quite clear for me 12) I agree

I am in favour of option (2) which is definitely the only one which can be realized without causing too much trouble for those who work process space geodetic data and those who analyse EOP time series.

I absolutely support the proposed realization because it seems to be the only one which is feasible in all space geodetic techniques without too big efforts.

FROM B. KOLACZEK, 15 March 2000

I agree with all 12 preliminary proposals and I support the C3 procedure of realisation of the CEP.

It seems for me that in definition of the MCRP "Long periodic part of motion" ought to be described exactly. It means the periods ought to be given.

The summary of the main point.

I support the option 2.

FROM S. LOYER, 15 March 2000

list of proposals 1-12 which I support :

1 - 2 - 3 - 4 - 6 - 8 - 9 - 11 - 12

list of proposals that I comment :

5 - 7 - 10

5 ) for proposal 5 , I prefer the name CIP

7 ) I do not like very much the first part of the conceptual definition. As we saw during the discussions it is dangerous to associate the conceptual definition of the Intermediate Axis with any part of the torque...due to the overlapping between motions due to different physical causes. To my opinion this intermediate axis is a "mathematical concept" as it appears from proposal 8 and is no more related to the a physical concept and this should be clearly said in the conceptual definition.

10 ) Yes, If possible...

FROM C. BIZOUARD, 15 March 2000

1. Do you support the proposals above (which ones from 1 to 12) ?  
Not all. Here below my points of contention.

1) The IAU 1980 conceptual and conventional definition of the CEP must be abandoned,

YES

2) the reference pole has not to be defined by its realization but by a clear concept not dependent on further improvements in the model,

NO

I would simply define the reference pole as the geographic pole minus the spatial constituents which are not included in the conventional precession-nutation model. I would thus consider that the reference pole should be defined by its realization (the conventional model of precession-nutation), because its "existence" can not be justified except by the astrometric determination of the Earth orientation and the associated representation by 5 parameters of the Earth's orientation.

Any other definition I have seen is confusing. There is no clear concept for an axis which does not exist from the physical point of view but only for astrometric strategy purposes!

What should be clearly stated is not the definition of the CEP, because it seems to me already clear, but what frequency components should be put in the 5 five parameters giving the complete Earth orientation from the observations.

3) this pole must be defined such that it can be realizable by a model as accurately as possible,

See point (2).

4) the change from the CEP to the new definition has to be as less as possible in its practical realization,

YES

5) a change of name could be considered as the “Celestial Reference Pole” (CRP), or the “Celestial Intermediate Pole” (CIP),

CIP seems to me O.K. But it may better to let the present name CEP unchanged, in order to avoid confusing in the future.

6) concerning the motion with respect to the CRS, the choice should be to specify this motion by a model including only the components with periods longer than 2 days (such a model will automatically include all the geophysical perturbations such as the retrograde diurnal motion due to ocean tides),

YES, but taking a conventional precession-nutation model which satisfy also this frequency condition.

7) a tentative conceptual definition is “the Pole of the intermediate equator of which motion with respect to the CRS is produced by the luni-solar and planetary torque” (or ... “by the external gravitational forces acting on the Earth”),

as the high frequency motion is not considered in the nutation model, it may be possible to add to the conceptual definition that “the motion of the equator with respect to the CRS is considered for an Earth with an equatorial symmetry”, or that “the motion is considered after filtering out terms of period shorter than 2 days”,

Again I think it is an error to attempt to define physically the CEP. I repeat, it has only the astrometric “cooking” for justification.

8) the prograde diurnal terms in nutation should be considered as long periodic terms of the polar motion and the prograde semi-diurnal terms in nutation should be considered as prograde diurnal terms of the polar motion,

I would prefer, that the gravitational induced constituents of the geographic pole be considered at the level of the precession-nutation model.

My philosophy is to reserve the polar motion parameters “x,y” for the modelled constituents of the spatial motion of the geographic axis, and not to include in these parameters some well-modelled gravitational effects. (x,y) should remain the “garbage” of our bad knowledge of the Earth rotation.

As the next conventional precession-nutation will be very accurate, the corrections on the precession-nutation model for period greater than 2 days will be no more than 0.5 mas. Therefore the spatial EOP parameters would take much less significance than today. And with the increasing temporal

resolution, they could be little by little not needed at all for representing the Earth orientation.

9) concerning the motion with respect to the TRS, the choice should be to sharpen the definition of the pole of reference by taking into account the prograde diurnal variations as well as the prograde and retrograde semi-diurnal variations as a predictable part of the polar motion which can be realized by a model,

See below.

10) the processing of the observations should include the estimation of the celestial pole offsets wrt the model for precession-nutation as well as the corrections to an empirical model for polar motion,

As said below, with an update precession-nutation model, the celestial pole offsets will become very small and will take much less interest than today. If the temporal resolution of the observations increases, we can think to suppress them, because they would be then estimated as retrograde diurnal polar motion.

11) a “Mean CRP” (MCRP) could be defined including only the long periodic part of the motion both in the TRS and the CRS, and then a “true CRP” as obtained by adding the high frequency components to the polar motion of the MCRP.

No. The less definition there are, the best it is.

12) a conventional procedure must be given for estimating the high frequency components in polar motion in order to provide the best realization of the pole in the processing of observations : C3 is proposed when possible or C2 in the more general case.

No. Nothing can be imposed for estimation procedure.

2. Which complementary comments do you consider as necessary ?

3. If you do not support one or several proposals, which alternative proposals are preferable ?

See below my comments and suggestions.

FROM C. MA, 16 March 2000

I have not had time to study carefully the two newsletters but I generally agree with the questions posed at the end. I think option 2 will be better for actual implementation, assuming that nutation offsets averaged over a 24-hr session will be estimated from VLBI observations.

## 2 Answers and comments to Newsletter 4

FROM D.D. MCCARTHY, 22 February 2000

- Question 1. Yes
- Question 2. Yes
- Question 3. Yes
- Question 4. Yes
- Question 5. I favor what is referred to in your Newsletter as alternative (v)
- Question 6. Non-rotating origin
- Question 7. I don't think it is necessary (strictly) but it is certainly to be preferred.

FROM P. BRETAGNON, 6 March 2000

- (1) yes
- (2) yes
- (3) yes
- (4) yes
- (5) The equator of date is referred to the ICRS by

$$[TRS] = R_3(\phi).R_2(\omega).R_3(\psi).R_1(\epsilon_0) [CRS]$$

where  $R_1(\epsilon_0)$ .  $[CRS]$  define one "conventional fixed ecliptic" with  $\epsilon_0 = 23^\circ 26' 21.409''$  for instance.

It is not possible to directly refer  $[TRS]$  to  $[CRS]$  with three Euler's angles (see comments).

- (6) the intersection of the moving equator with the fixed ecliptic.
- (7) ?

Comments about the newsletter 4 of the subgroup T5

- in 2 (ii), it is written : "the definition 2) is on the fixed equator of the TRS, ..." No, the origin of the definition 2) is on the moving equator and the fixed ecliptic.

- in 2 (iii) Euler's angles The formula (2) has no sense. When we refer the moving equator to the CRS, the precession-nutation variables  $\psi$  and  $\theta$  are singular and there are no analytical representations of such variables.

It is why we have to use (as Woolard) Euler's angles referred to the fixed ecliptic (see answer 5 above).

- 2 (ix) It seems to me it is no necessary to have an Earth's angle of rotation which includes only the "intrinsic Earth rotation". It is easier to use the third Euler's angle reckoned from the intersection of the moving equator with the fixed ecliptic and this is better than the introduction of new developments ( $s$  and  $s'$ ) computed with some approximations.

FROM A. BRZEZINSKI, 8 March 2000

General comment

A basic idea is to make the parameterization of Earth rotation, understood as time dependent transformation between the terrestrial and the celestial reference systems, as simple as possible by removing all the elements which are no more necessary after adoption of the new ICRS. It seems for me that the idea of using the coordinates of the CEP as the transformation parameters (point v of your presentation, or point viii which is a special case of point v - am I right?) together with the use of the "non-rotating origin", is very attractive, particularly from the point of view of interpretation of the observed variations. However, there is one issue which has already been mentioned in the context of adoption the new intermediate celestial pole (point 4 of the preliminary proposals presented in Newsletter 3) but not here, namely about the practical consequences of the change such as continuity of the procedures applied so far by the people deriving the Earth orientation parameters or those using the EOP in reduction of the observations. So my answers given below are from the point of view of somebody who tries to interpret the observed EOP's in terms of different perturbations, but who takes less care of practical aspects.

Answers to your questions

Q.(1) Yes

Q.(2) Yes.

Q.(3) Yes.

Q.(4) Yes.

Q.(5) Celestial and terrestrial coordinates of the intermediate pole.

Q.(6) "Non-rotating origin".

Q.(7) Yes.

(1) Do you agree that the current parameters in the FK5 system must be abandoned for being consistent with the newly adopted ICRS ? YES

(2) Do you agree that, for consistency with ICRS, the current formulation combining the motions of the equator and of the ecliptic wrt the CRS has to be abandoned ? YES

(3) Do you agree that the angle of Earth rotation must no more be reckoned from the true equinox which is moving due to precession and nutation and which is referred to the ecliptic of date ? YES

(4) Do you agree that new parameters for the orientation of the Earth's axis in the CRS must include both precession and nutation ? YES

(5) Which parameters (either among the parameters presented previously or new ones) do you propose to use for the EOP referred to the ICRS in place of the current parameters referred to the FK5 ? THE CELESTIAL POLE COORDINATES X,Y OF THE CEP IN THE CRS

(6) Which origin on the moving equator do you prefer ? THE NON ROTATING ORIGIN (OR "DEPARTURE POINT") ON THE MOVING EQUATOR (8)

(7) Do you agree that in order to provide Earth rotation from the orientation angle around the axis of the CEP, it is necessary to use an origin without any instantaneous rotation wrt the CRS around this axis ? YES

FROM B. GUINOT, 11 March 2000

(1) to (4) Yes.

(5) Direction cosines of the axis of rotation (for the new definition of the Celestial Ephemeris Pole) in the CRS and in the TRS. Notes : 1. UT1 should be proportional to the stellar angle, as defined in Newsletter 4. 2. Question. Should we consider a re-definition of the coordinates of the pole in the TRS so that they correspond to the usual direct trirectangular coordinates in use ?

(6) The non-rotating origin (NRO). Note. It is recalled that, in case of change of model of precession/nutation, quantity s providing the NRO changes by an amount which is much smaller than that of the change of direction of the pole at changeover. In addition, the net effect of such a model change on the stellar angle (and on UT1 defined as being proportional to the stellar angle, with an invariable relationship) is negligible at the microarcsecond level. For these reasons, I do not see the advantage of a purely geometrical definition, since anyway the sidereal rotation of the Earth, which requires the concept of NRO, is needed. However, if a geomet-



rical origin is required, my preference would be the equality of arcs reckoned from the node, as proposed in (6) of 2(ii) of Newsletter 4.

(7). Yes (see above in 6).

FROM L. PETROV, 13 March 2000

Q1) Do you agree that the current parameters in the FK5 system must be abandoned for being consistent with the newly adopted ICRS ?

A1) Yes.

Q2) Do you agree that, for consistency with ICRS, the current formulation combining the motions of the equator and of the ecliptic wrt the CRS has to be abandoned ?

A2) Yes.

Q3) Do you agree that the angle of Earth rotation must no more be reckoned from the true equinox which is moving due to precession and nutation and which is referred to the ecliptic of date ?

A3) No answer. The question has internal logical contradictions.

Q4) Do you agree that new parameters for the orientation of the Earth's axis in the CRS must include both precession and nutation ?

A4) No answer. The question has internal logical contradictions.

Q5) Which parameters (either among the parameters presented previously or new ones) do you propose to use for the EOP referred to the ICRS in place of the current parameters referred to the FK5 ?

A5) Euler angles for definition, any angles for theory or series of results from observations provided authors presented expressions between the set of parameters which they used and Euler angles. Refer to [http://giub.geod.uni-bonn.de/Mitarbeiter/petrov\\_docs/discussion/eop.html](http://giub.geod.uni-bonn.de/Mitarbeiter/petrov_docs/discussion/eop.html) for argumentation.

Q6) Which origin on the moving equator do you prefer ?

A6) I prefer to abandon a notion of moving equator at all.

Q7) Do you agree that in order to provide Earth rotation from the orientation angle around the axis of the CEP, it is necessary to use an origin without any instantaneous rotation wrt the CRS around this axis ?

A7) In general this statement is not correct.

FROM J. VONDRAK, 14 March 2000

(1) yes

(2) yes

(3) yes

(4) yes

(5) I propose option (v), i.e. the coordinates of the CEP in the CRS and TRS  $(X, Y, x_p, y_p)$ .

(6) I prefer the non-rotating origin (small sigma)

(7) yes

FROM V. DEHANT, 15 March 2000

(1) Yes

(2) Yes

(3) Yes

(4) Yes

(5) (6) (7) up to people reducing data

FROM B. KOLACZEK, 15 March 2000

I agree with questions 1-4 of this Newsletter.

In the question 5 I propose to use previous EOP parameters referred to the ICRS. Introduction of the Euler's angles instead of the present EOP parameters needs deep consideration. We have to remember about analysis of long series of EOP parameters and separate analysis of polar motion and nutation corrections.

No opinion about the questions 6 and 7.

FROM S. LOYER, 15 March 2000

Concerning the questions in newsletter 4 concerning the new parameters, I can only give general remarks. (I have no time to investigate the consequences of the different proposals).

Remarks :

a) The number of parameters should be reduced with the suppression of all the non-necessary intermediate axis. The reduction up to 3 parameters only seems very difficult for observational techniques that cannot estimate these parameters at a high rate. But it could be possible in principle as soon as some techniques can provide high rate information.

b) After a rapid look I answer yes to questions 1 and 4.

c) the general problem of how we represent the link between terrestrial and celestial frame is polluted by the historical constraints on Time definition. Nowadays the "physical time" is no longer related to Earth rotation and we should definitively abandon the constraint to have GST appearing in the matrix representation we use for orientation.

This is (may be) necessary to have a way of define the "human time" from the motion of the Earth relatively to the Sun.

But this is not necessary at all to have the "human time" (or something that looks like human time, like GST) appearing explicitly in the orientation representation.

Any proposition that suppress the intermediate axis that were historically introduced for the time definition is thus interesting...

FROM C. BIZOUARD, 15 March 2000

(1) Do you agree that the current parameters in the FK5 system must be abandoned for being consistent with the newly adopted ICRS ?

YES

(2) Do you agree that, for consistency with ICRS, the current formulation combining the motions of the equator and of the ecliptic wrt the CRS has to be abandoned ?

YES

(3) Do you agree that the angle of Earth rotation must no more be reckoned from the true equinox which is moving due to precession and nutation and which is referred to the ecliptic of date ?

YES

(4) Do you agree that new parameters for the orientation of the Earth's axis in the CRS must include both precession and nutation ?

YES

(5) Which parameters (either among the parameters presented previously or new ones) do you propose to use for the EOP referred to the ICRS in place of the current parameters referred to the FK5 ?

The combined parameters of Aoki and Kinoshita referred to the fixed ecliptic of the epoch J2000 (iv) or the (X,Y) coordinates (v)

(6) Which origin on the moving equator do you prefer ?

the intersection, gamma' of the moving equator with the fixed ecliptic or the non rotating origin.

(7) Do you agree that in order to provide Earth rotation from the orientation angle around the axis of the CEP, it is necessary to use an origin without any instantaneous rotation wrt the CRS around this axis ?

YES

FROM H. SCHUH, 20 March 2000

Your questions:

(1) yes

(2) yes

(3) yes

(4) yes

(5) as I already wrote in my response to your former questionnaire I do not see any benefit of the 3-parameter approach from a practitioner's point of view. Thus, the options (iii) and (viii) should be discarded. From the other options number (v) is probably preferable but I have to think about it again.

(6) I haven't come to a final conclusion about the definition of the origin but probably the 'non rotating origin' is the best choice.

(7) yes