Comments on the ERA-2005 numerical theory of Earth rotation

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Abstract

Two papers recently published in Celestial Mechanics (Krasinsky 2006, and Krasinsky and Vasilyev 2006) have presented a model for Earth-rotation variations, called ERA-2005, based on numerical integration of a new set of equations for the rotation of a deformable Earth followed by a fit of the results of the integration to VLBI data. These papers claimed that this model was superior to any other existing model. The purpose of this Note is to bring to light fundamental errors in the derivation of the basic equations of the new theory, compounded by serious deficiencies in the process of fitting to the data; they make ERA-2005 unsuitable for consideration as a geophysics-based model of nutation and precession.

Keywords: Earth, reference systems

1 Introduction.

Two papers recently published in Celestial Mechanics have presented a new model for the variations in rotation of a deformable Earth, based on numerical integration of a set of rotation equations and its fit to VLBI data.

In the first of these papers, Krasinsky (2006) presented a revised version of the Sasao-Okubo-Saito (SOS) formulation of the equations governing the variations in rotation of a two-layer Earth (Sasao et al., 1980), and used the Poincaré
formalism to generalize it to the case of an Earth consisting of a mantle enclosing a fluid core with a hypothetical inner fluid core within it but without any solid inner core. Numerical integration of more general equations that take account of the inner fluid core layer, ocean tides, dissipative effects, etc., was done, after transformation to a celestial frame, for determining the temporal variation of the Earth rotation variables. Krasinsky and Vasilyev (2006) presented, in an accompanying paper, the results obtained by optimization of the fit of the numerical solutions to the time series constructed from a number of Very Long Baseline Interferometry (VLBI) data sets. (These two papers will be referred to hereunder as Papers 1 and 2.) The new numerical model of Earth rotation variations, thus constructed, goes under the name ERA-2005.

The ERA-2005 papers express a number of criticisms of the MHB theory (Mathews et al. 2002, Buffett et al. 2002, Herring et al. 2002) on which the current IAU 2000 nutation model is based, and assert that ERA-2005 provides a superior and more accurate geophysical model. It is clearly necessary to examine the validity of these claims and criticisms. This is the chief motivation for this Note.

2 Criticisms and claims made in the ERA papers

Krasinsky holds that the SOS equations are incorrect and need to be revised because of errors in the original derivation, and that these errors are reflected in the IAU 2000A nutation model since this model is based on the MHB theory of Mathews et al. (2002) which, in turn, is a generalization (in a number of ways) of the SOS theory. (For brevity, we shall refer to this paper simply as MHB.) The principal points of the contentions in Paper 1 are (a) that the expressions employed by SOS for the increments to the inertia tensors of the whole Earth and of its core regions due to the effects of the centrifugal potentials associated with the Earth’s variable rotation are incorrect, and (b) that the effects of dissipative phenomena on the responses of the Earth to tidal forcing are not adequately modeled in the MHB theory where they appear through complex increments to compliance parameters, since that theory fails to reveal a secular variation of the obliquity.

The ERA papers go on to claim a smaller weighted root mean square (wrms) value for the residuals of the numerical output from ERA-2005 (relative to observational data up to 2005) than the wrms of the IAU 2000 residuals relative to the same data, and even predict that the latter residuals in ecliptic longitude will worsen to the 2 mas level by 2009. On the basis of the claimed improvement in the residuals as well as the supposed errors in the MHB theory, they assert that the ERA-2005 model is the superior one.

It is also claimed that thanks to more rigorous equations, the ERA-2005 theory makes it possible to detect the geodesic precession from the analysis of VLBI data, while the IAU 2000 theory would not.

We show however in the next section that the arguments for revision of the SOS theory are merely a reflection of fundamental misconceptions about the centrifugal perturbations associated with wobble motions, and about the
deformations produced by them, and that it is Krasinsky’s revised version of the SOS theory rather than the original SOS theory that is in error. We point out a couple of major consequences of this error, including in particular, a wrong expression for the scale factor relating the precession rate in longitude to the ellipticity parameter $e$. In succeeding sections we comment on other points of interest: the different ways in which dissipative phenomena are modeled in the ERA and MHB theories; certain strange and unphysical aspects of the Earth model employed; and the values reported for some of the parameters of ERA-2005 as estimated from fits to data, that are inconsistent with information from seismological and astronomical studies.

We also comment on problems with the claimed detection of the geodesic precession.

3 Comments on centrifugal perturbations and application of the Love number formalism

Consider first the centrifugal potential associated with the variable angular velocity $\omega$ of Earth rotation ($\Omega$ of MHB) which consists of the constant angular velocity $\omega_0$ of rotation of the unperturbed Earth ($\Omega_0$ of MHB) and a very small perturbation $d\omega = \omega - \omega_0$ resulting from the torquing action of the tidal potential. It has been a well accepted concept, which is the basis of Clairaut’s theory of the hydrostatic equilibrium Earth, that the entire ellipticity $e$ of the unperturbed Earth results from the incessant action of the time independent centrifugal potential due to the steady rotation $\omega_0$ which has nothing to do with the luni-solar perturbation. It is the small incremental centrifugal potential associated with the perturbation $d\omega$ due to the luni-solar potential (along with additional increments resulting from the differential rotations of the core regions which also result from the tidal perturbation) that produces the time dependent deformations and the attendant increments to the inertia tensor of the Earth, which react on the Earth rotation itself.

On the other hand, it is insisted in Paper 1 that the rotational increment $dI_r$ to the inertia tensor should be that arising from the total deformation produced by the centrifugal potential associated with the full angular velocity $\omega$.

Treatment of the constant part of the centrifugal potential as if it were part of the tidal perturbation is a grave conceptual error, which leaves the off-diagonal elements $c_{13}$ and $c_{23}$ that appear in the SOS theory unaltered but makes the diagonal elements of $dI_r$ many orders of magnitude larger than otherwise. This error is propagated further through the use of reciprocity relations connecting the incremental inertia tensors $dI_r^c$ and $dI_r^i$ of the full core and inner core to $dI_r$ (see equation (77) of Paper 1).

This error is compounded by a misstep in the application of the Love number formalism to express the increments to the Earth’s gravitational potential that result from the above-mentioned deformations. The incremental gravitational potential at the Earth’s surface is the appropriate $k$ Love number times the potential causing the deformation, which is the centrifugal potential here. The relevant Love number is $k_2$ in the case of time dependent degree 2 potentials;
but it is the secular Love number \( k_s \) that is appropriate when the potential is time independent. It has long been accepted that the behaviour of the Earth under a force which is sustained over a very long period is akin to that of a fluid body yielding to shear forces, rather than that of a purely elastic solid body. For example, it is mantle convection, rather than elastic deformation of the mantle, that results from the stresses associated with persistent thermal gradients. It is because of the Earth’s fluid-like behavior under forcing over very long periods that the need arises to use the fluid/secular Love number \( k_f \) or \( k_s \) in going over from the degree 2 part of the constant centrifugal potential of \( \Omega_0 \) to the deformational contribution that it makes to the geopotential. The expression which relates \( k_f \) or \( k_s \) to the ellipticity \( e \) of the Earth may be written down in the form

\[
e = k_s \frac{M_E R^2}{3 A} \frac{\Omega_0^2 R}{GM_E/R^2},
\]

which brings out the fact that the Earth’s ellipticity \( e \) is determined by the balance between the constant centrifugal force and the gravitational force at the Earth’s surface, as is expected in the hydrostatic equilibrium state wherein shear resistance plays no part. It may be noted that the need to use the fluid/secular Love number (with a value \( \approx 0.94 \)) has been pointed out in other contexts: see, for example, the third paragraph of Section 1.1 entitled “Treatment of the permanent tide” in Chapter 1 of the IERS Conventions 2003 (IERS Technical Notes 32) and more particularly, Fig. 1.2 of the same section, concerning the contribution from the time independent part of the degree 2 zonal tidal potential to the geopotential.

The ERA theory fails to make the distinction between the Earth’s short-period and long-term responses to forcing, and applies \( k_2 \) to the full potential associated with the rotation \( \omega \), though only a tiny part of it arises from the time dependent perturbation part \( \omega - \omega_0 \) of \( \omega \). As a consequence, one finds at the end of Sec. 3.5 of Paper 1 the strange claim that the flattening parameter \( J_2 \) of the Earth is made up of a part \( \sigma J_2 = (k_2/k_s)J_2 \approx J_2/3 \) which arises from the deformation caused by the centrifugal potential associated with the total angular velocity \( \omega \), and an unforced part \( J_2^{(0)} = (1-\sigma)J_2 \) which would be present even if the Earth were non-rotating. (Basic physics tells us that in the absence of the rotation-driven potential, the balance between the gravitational force in the Earth and the elastic force resisting compression, without any persistent shear resistance, would cause the shape to be spherical.) If the Love number \( k_s \) appropriate to the effectively constant potential had been used, it would have been found that there was no scope at all for an unforced part \( J_2^{(0)} \) (or for the equivalent \( e_0 \) referred to at the beginning of Sec. 4.1 of Paper 1) since \( k_2/k_s \), and hence \( \sigma \), would be replaced by unity; the deformation contribution to \( J_2 \) would then be all of \( J_2 \) (or equivalently, the ellipticity \( e \) would be entirely due to the constant rotation), which is in accordance with the hydrostatic equilibrium theory that is universally acknowledged.

The consequences of the erroneous concept concerning the centrifugal potential, and of the use of the wrong Love number referred to in the last paragraph, pervade and vitiate essentially all of the rest of the ERA theory and are re-
sponsible for the differences between the revised SOS equations developed in Paper 1 and the original equations (other than the dissipation terms). To give an example of the consequences: The luni-solar precession rate becomes proportional to the scale factor $e/(1 + e - e\sigma/3)$ dependent on the deformability parameter $e\sigma = ek_2/k_s$ which is $\kappa$ in the SOS notation. (An incorrect factor $e/(1 + e + 2e\sigma/3)$ is shown in the numbered paragraph 4 of Sec. 1 of Paper 1, apparently by mistake.) However, the scale factor which follows from all earlier treatments is $e/(1 + e)$, consistent with the Poincaré concept of hydrostatic rigidity; it has remained unquestioned so far. (Though the Poincaré treatment of nutation employed an Earth model made up of a rigid shell enclosing a uniform fluid core, the very same scale factor emerges transparently from the SOS or MHB equations which take full account of the Earth’s deformability as well as the stratification of the fluid core. It is stated mistakenly in the above-cited paragraph of Paper 1 that the SOS theory would lead to the scale factor $e/(1 + e + e\sigma)$ in the case of a deformable Earth.) The dependence of the ERA scale factor on deformability is simply a consequence of the conceptual errors in the revised SOS equations that have been pointed out above.

Another consequence of the revision is the new expression for the frequency of the Free Core Nutation (FCN) mode given in Appendix A.1 of Paper 1. When translated to the SOS notation, it is $(A/A_m)\Omega_0(e_c - \beta - \gamma/3)$. Ignoring the choice of the positive sign for $f_{FCN}$ as noted below equation (150), which makes no sense, we observe that the presence of the new term $\gamma/3$ with a value $\approx e_c/4$ represents a significant deviation from the original SOS formula. The FCN frequency is critical in determining the magnitude of the nutations, and the results of past estimates of its value from fits to data have been quite robust. So it is very surprising that it has had to be taken as an external input into the ERA fitting process. As stated in the paragraph numbered 2. in Appendix A.1 of Paper 1, the 431 day period that “is reliably estimated from the analysis of VLBI observations” in the earlier literature was used as a “a strong constraint”, because the value calculated from the ERA parameter estimates turned out otherwise to be 415 days as stated in the last paragraph of Sec. 3.3 of Paper 2–and this is unacceptably far from the reliable estimates.

4 Comments on the modeling of the dissipative phenomena

In regard to the effects of dissipative phenomena, they are lumped, in Paper 1, into a time delay $\tau$ between the action of the tidal potential and the deformational response of the Earth as a whole, which is converted into an equivalent phase delay $\delta = \omega\tau$. (Different phase delays $\delta_c, \delta_i$ are assigned to the core regions.) The inspiration for this comes from the explanation for the observed slow secular decrease of the Earth’s axial rotation rate and the slow increase of the Moon-Earth distance, both ascribed to a continuous transfer of the Earth’s rotational energy into the energy of orbital motion of the Moon because of the time delay between the tidal forcing by the Moon and the deformational response.
In the MHB paper, on the other hand, specific phenomena which cause the responses to tidal forcing to become out of phase are individually modeled, based on concrete information available from observations: from seismic normal modes and propagation of seismic waves, for the modeling of anelasticity effects; and from space geodetic observations of the perturbations of geopotential coefficients, for the incremental inertia tensor elements due to the ocean tides. It is true that MHB did not consider the possibility of any contribution to the obliquity rate. The intent of their paper was only to construct a theoretical model for the precession in longitude and the nutations; comparison of the theoretical predictions with observations was also for these quantities only, though the obliquity rate correction to the IAU 1976 model was estimated empirically by Herring et al. (2002) from VLBI observations along with the other quantities. The computation of the second order contributions to nutation and precession from the torques produced by the action of the tidal potential on the deformations produced by this potential (in Appendix A of MHB) was incomplete. Lambert and Mathews (2006) made a complete calculation of these effects (including the effect on the obliquity rate) through a treatment which automatically takes account of the phase shifts arising from anelasticity, ocean tide effects and boundary couplings of the core. Their result for the tidal contribution to the obliquity rate (0.00127 mas/yr) is much smaller than the 0.024 mas/yr found by Williams (1994) by a method based on the transfer of angular momentum between the rotational motion of the Earth and the orbital motion of the Moon keeping the total of the two constant. The sources of this discrepancy have not yet been identified.

Though we have shown above that the major criticisms of the MHB theory made in the ERA papers are based on misconceptions which vitiate the basic equations of the ERA-2005 theory, it seems desirable still to examine other relevant aspects of the physical modeling and the optimization of the fit of the output from the theory to the VLBI nutation-precession time series, especially in view of the claim made that the w.rms of residuals of its fit to VLBI-based series is lower than that of IAU 2000, and hence that ERA-2005 provides a superior model for nutation and precession.

5 Comments on the Earth model: Inner fluid core; Ocean tides

The Earth model that is employed in ERA has a fluid core containing an inner fluid core within, while the solid inner core, with dimensions and properties that are pretty well determined from seismological observations, is neglected. The so-called fluid inner core is totally nebulous in nature. With no basis in any Earth model, the “preliminary” values of its parameters as shown in Sec. 2.1 are arbitrary choices. The ratio of its moment of inertia to that of the whole Earth, as well as its ellipticity, seem to be chosen to be just about the same as those of the conventional solid inner core. Its existence is postulated for the sole purpose of providing an explanation for a free wobble mode with a period of about 420 days (in addition to the well known FCN mode) that is claimed to have been
found in the residuals of the VLBI nutation series relative to IAU 2000. The proposed new mode (which has been named as FICN though it has nothing to do with the FICN mode in the earlier literature which is associated with the solid inner core) is supposed to explain the apparent “beats” with the 431-day FCN mode in the residuals (Fig. 3 of Paper 2). But the beat frequency, which is half the difference between the individual mode frequencies, would correspond to a period of over 90 years. It would take observations over at least half that period, theoretically, to separate the two modes. Even then, the amplitude of the free FICN would have to be of comparable magnitude to that of the FCN, and both the amplitudes would have to remain essentially constant over a long time span of the above order if the beats are to be recognizable as such—and neither of these conditions is physically reasonable, since the inner core is visualized as being very much smaller than the whole core, and the geophysical mechanisms available for the excitation of the free modes are highly variable in time. In these circumstances, the postulated fluid inner core merely serves as a device for the introduction of a couple of adjustable parameters ($T_{FICN}$ and $\nu_{ur}$ in Table 3 of Paper 2) into the fitting process.

Another aspect of the modeling that is bizarre and physically unacceptable is the representation of “the combined action of oceanic tides and non-uniform structure of the Earth’s interior” by an increment $\delta k_2 = k_2^{(1)} \cos \theta_B + k_2^{(2)} \cos^2 \theta_B$ to the Love number parameter $k_2$ (Sec. 3.3 of Paper 1), where $(\pi/2 - \theta_B)$ is the geographical latitude of the position of the perturbing celestial body $B$ (Moon/Sun). It is incomprehensible how the perturbation of the Earth’s gravitational potential by ocean tidal and other mass distributions which have a complicated spatial variation could be represented by a spatially uniform increment $\delta k_2$, and how the incremental potential everywhere outside the Earth could be determined by a scale factor which has one value, $k_2 + k_2^{(1)} \sin \beta + k_2^{(2)} \sin^2 \beta$, when the body is over a northern latitude $\beta = \pi/2 - \theta_B$ and a different value $k_2 - k_2^{(1)} \sin \beta + k_2^{(2)} \sin^2 \beta$, when the body is over the corresponding southern latitude $-\beta$, with no reference to the nature of the non-uniform mass distributions. With no physical justification, the only role that the constant parameters $k_2^{(1)}$ and $k_2^{(2)}$ seem to have is to serve as convenient additional adjustable parameters for the fitting.

It may be remarked that a superficially similar expression for the effect of the ocean tides is found in Kaula (1969). However, the dependence of $k_2$ in that work is not on the angular position of the celestial body $B$ in the terrestrial frame, but on that of the location $r$ at which the incremental geopotential is to be evaluated. The latter dependence is unobjectionable: even for an oceanless Earth, the latitude dependence of the Love numbers on account of the equatorial bulge is well known.

6 Comments on the estimated parameters

The fitting of the output from the ERA-2005 theory to the precession-nutation time series constructed from VLBI observations is done by varying the values of a large number of parameters that are treated as adjustable; they include
14 geophysical parameters (among which the ad hoc parameters $k^{(1)}$ and $k^{(2)}$ appear), 6 empirical parameters (with admittedly unclear physical significance), and the initial values of the three Earth orientation parameters in space. Among the empirical parameters, four are adjustments to the 4 coefficients pertaining to the retrograde and prograde annual nutations (see Table 4 of Paper 2). (Adjustments to just the 2 prograde annual nutation coefficients were made in MHB. Other empirical parameters used were in the modeling of the frequency dependence of ocean tide admittances; no free parameters were used in the theoretical computation of the effect of the ocean tidal components on nutations. One other parameter that was introduced was for minor fine-tuning of the anelasticity model.)

Returning to the other empirical parameters in the ERA theory, we note that $E_1$ is intended to “correct” the ratio of the amplitudes of the retrograde and prograde 18.6 year nutations, and $E_2$ to “correct” the out of phase nutations. These are ad hoc corrections to bring down the rms of residuals below what the geophysical parameters alone could accomplish. When comparing the plots of the residuals calculated with ERA-2005 and IAU 2000, the authors recognize (first paragraph of Sec. 3.3 of Paper 2) that accounting for the free oscillations in ERA-2005 is the main cause why the rms errors for ERA-2005 are less than those for IAU 2000, the free nutations being not included in the latter theory.

One finds among the estimated geophysical parameters both $e$ and $k_2 = k_s \sigma$. Since the ERA scale factor for the precession rate is a function of $e$ and $\sigma$, it is evident that estimates of these two parameters must be highly correlated. Again, both $e_c$ and $\kappa_{el}$ are among the estimated parameters, where $\kappa_{el}$ is the counterpart of Re $K^{CMB}$ of MHB. Since the inertial coupling with $e_c$ as coefficient and the CMB coupling represented by $\kappa_{el}$ are both proportional to the differential wobble between the mantle and the core, these two parameters should be 100% correlated, and it is not at all clear as to how they can both be estimated parameters. In the MHB work, the combination ($e_f + \text{Re } K^{CMB}$) and Im $K^{CMB}$ were among the estimated parameters. A separate estimate for Re $K^{CMB}$ could however be obtained, thanks to the relation between it and Im $K^{CMB}$ that the theory of the electromagnetic coupling at the CMB called for; and the estimate for $e_f$ followed then from the estimate for the sum.

A list of estimated values obtained from the fit for 14 parameters is found in Table 3 of Paper 2. The authors do not make any effort to justify these values in the light of what is known otherwise about the main parameters; they say simply: “In the present work, we do not discuss physical meaning of the geophysical parameters …”. Such a discussion is essential, however, to assess how geophysical the theory is. The following remarks are offered in that spirit.

1. The estimate shown for $e$ in the Table is $3.283410 \times 10^{-3}$, which is not consistent with any other estimates from astronomical observations. It is lower, by 0.0346%, than the MHB estimate $e = 3.284548 \times 10^{-3}$. Knowing also that the first order lunisolar precession rate of MHB is about 5040.7″/cy, one can easily infer the corresponding ERA value by multiplying the above rate by the ratio of the ERA scale factor $e/(1 + e - e \sigma/3)$ (evaluated using the values from Paper 2 for $e$ and $\sigma$) to the MHB scale factor $H_d$ taken with the MHB value for $e$. This ratio turns out to be less than unity by $2.84 \times 10^{-5}$. Consequently
the ERA precession rate must be less than the MHB value by about 0.14″/cy. This difference should have been easily discernible as a rather steep slope in the difference (ERA-2005 − IAU 2000) in the top curve in Fig. 3 of Paper 2; why it does not, remains incomprehensible.

Unfortunately, the ERA-2005 estimate for the precession rate is not given explicitly anywhere. The available information about it is from the penultimate sentence of the first paragraph of Sec. 3.3 of Paper 2 which says that the difference between ERA 2005 and IAU 2000 in the secular trend in the longitude variable (i.e., in the precession rate in longitude) is (−0.82±0.22) mas/cy. This very small value is grossly inconsistent with the difference of about −0.14″/cy = −140 mas/cy mentioned above.

2. In regard to other parameters: it is observed that the ERA-2005 estimate for $\alpha$ (which stands for $A_c/A$) is about 4% less than the value computed from recent Earth models like PREM. To our understanding, seismological Earth models do not provide so much of a leeway in the value of this ratio. The ERA estimate for $k_2$ is quite unrealistic, being about 10% less than the accepted value of about 0.3 that recent Earth models lead to on integration of their deformation equations; and the estimated value of $e_c$ ($e_c = 3.3761 \times 10^{-3}$), which is seen to be even higher than that of $e$, is definitely unphysical: the hydrostatic equilibrium structure requires $e_c$ to be only about three-fourths of $e$, and there is no conceivable mechanism that could bring about such a gross deviation from this value as the ERA fit requires. The authors of the ERA papers seem to be quite disdainful of seismologically constructed Earth models and to consider themselves unconstrained by parameter values based on such models.

All in all, it should be abundantly clear that the ERA-2005 nutation-precession model cannot be considered to be based on sound geophysics, and that there is a strong internal inconsistency (noted in paragraph 1. above) between the precession rate dictated by the estimated value of $e$ taken together with the scale factor, and the claim of a very small wrms of the residuals for ERA-2005.

7 Comments on astronomical quantities used or estimated

Besides the problems mentioned in the last two sections in regard to the geophysical and empirical parameters estimated and the estimates obtained, there are also problems with some of the astronomical quantities involved and with the estimates obtained in ERA 2005 for some of them.

1. It is stated in Paper 1 (paragraph 4. of Sec. 1) that the ERA-2005 theory has made possible, for the first time, a direct determination of the geodesic precession from the analysis of VLBI data, due to the superiority of the equations on which the model is based over other theories.

The claim of superiority of the ERA theory has been shown in earlier sections to be not valid.

It may be thought possible, in principle, to estimate $H_d$ (or $e$) and the
geodesic precession $p_g$ simultaneously from observational data on nutation and precession since the observed precession rate involves both the predominant part due to $H_d$ (about $5040.7''$/cy) and the relatively small $p_g$ (about $-1.92''$/cy), while nutation amplitudes depend on $H_d$ and are independent of $p_g$. But, in reality, due to a number of practical reasons, separating the geodesic precession from the other contributions to the precession rate from observations alone is very difficult. That is why precession-nutation models hitherto have used theoretical values for $p_g$, while $H_d$ (or $e$) has been estimated from precession-nutation observations.

Nevertheless, Paper 2 (Sec. 3.4) claims to have actually confirmed the effect of the geodesic precession from a fit of the ERA-2005 theory to VLBI data. The validity of that claim is seriously questionable for the following reasons.

- The discrepancy (of about $-0.15''$/cy, i.e. $\approx 10\%$) of the estimated value for $p_g$ with respect to the theoretical value is reported (Sec. 3.4) to be compensated by corrections to other parameters (mainly to $e$), which clearly indicates a significant correlation between $p_g$ and other estimated parameters. Unfortunately, nothing is said in the ERA papers about the degree of correlation or about the magnitude of the changes in the estimated geophysical parameters when $p_g$ is estimated.

- The fact that the estimate shown for $e$ in Table 2 of Paper 2 is not consistent with any other estimates from astronomical observations makes doubtful the possibility of a reliable estimation of both $e$ and $p_g$.

2. Another problem is related to the celestial reference system to which the Euler angles considered in the ERA model are referred. Paper 2 actually leaves it unclear whether the reported values for the Euler angles (e.g. Table 2) are referred to the inertial mean ecliptic frame at J2000.0 (e.g. Sec. 2, paragraphs 1 and 2, or Sec. 3.2), or to the geocentric ecliptic reference system derived from the GCRS (Geocentric Celestial Reference System) through a rotation $\theta_0$ for the J2000 mean obliquity around the x-axis (cf. Sec. 2, paragraph 1). Moreover, Paper 2 does not make clear (eg. Sec. 3.1, 2nd paragraph) how the obliquity value $\theta_0$ is determined.

8 Concluding remarks

The assertion in the ERA-2005 papers about higher accuracy is based solely on a claim (a) that numerical integration of the set of equations of the ERA theory leads to lower residuals relative to the VLBI-based observational data than the residuals of IAU 2000A which is based on the MHB theory, and (b) that especially for the last couple of years of the data set used, the predictions from the MHB theory for the nutation-precession in longitude, in particular, show increasing deviations from the data. No other information that would have been of value in making comparisons between ERA and other theories has been made available. The ERA estimates for the precession rate in longitude or for the coefficients of any of the spectral components of nutation, which are quantities of the greatest interest, are not shown. No information is given on the
magnitudes of the contributions to nutation from ocean tides or from the core mantle boundary coupling. Nothing has been said about the amplitudes and phases of the FCN or FICN oscillations, whether they are constant or variable, or whether excitation of these free modes by geophysical mechanisms are allowed for, though these oscillations are stated to be included in the ERA model unlike in MHB and other theories. And finally, there is no discussion or attempt at justification of the values of the geophysical parameters involved in the theory (estimated from fits to data or assumed a priori), most of which differ to an unreasonably large extent from numbers computed from seismological Earth models or estimated from earlier determinations.

To sum up, the comparisons of the results of the ERA theory with those of other theories consist of nothing more than graphs, as functions of time, of the residuals of the Euler angles computed from the ERA theory relative to VLBI data and to the IAU model; and, of course, the rms of the residuals. We have brought to light, in this Note, fundamental errors in the derivation of the basic equations of the ERA-2005 theory and unphysical aspects of the geophysical modeling, compounded by serious deficiencies in the process of fitting to the data and in the estimates obtained for various parameters. It should be clear therefore that ERA-2005 does not constitute a sound geophysics-based model of nutation and precession.

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