**Reviewer 1**

We thank Reviewer 1 for the careful reading and constructive comments, which have helped us improve the clarity of the manuscript. Below we address each comment individually.

**The authors use a Bayesian inversion to recover estimates of Earth parameters from VLBI observations of the Earth's nutation. The main innovations include the use of a better sampler for the Bayesian inversion, a better ocean tide model and a more flexible recovery of motion to the free core nutation. Comparisons of the revised Earth parameters with previous estimates offer new insights into the structure and dynamics of the Earth's interior. The results are very interesting, although the manuscript is aimed at an expert audience. I had trouble following parts of the manuscript. These parts could benefit from clarification or more precise descriptions. Most of my comment deal with points of clarification.**

**Specific Comments**

1. **The introduction has no references before line 47. Statements are often made without documentation or support. For example, "… the precision (of VLBI) had a major improvement in the late 1980s". What is "a conventional model" in the definition of the Celestial Pole Offsets?**

The conventional model is the IAU2006/2000A model of precession nutation, the nutation part is also called the MHB2000. It has been clarified in the revised manuscript. Relevant references have been added in the introduction.

2. **line 59: lager -> larger**

It is corrected in the revised manuscript.

3. **line 60: "We now have almost 25 years of data with better quality…". The abstract suggests that 45 years of data are used in the inversion. It appears that "25 years" refers to the additional data available since the MHB 2000 model. This point is clarified in the next paragraph. It might be helpful to move this clarification earlier in the manuscript.**

The clarification has been moved forward in the revised manuscript.

4. **The sampling algorithm is updated from a single Metropolis-Hastings sampler to an ensemble Markov Chain Monte Carlo method. Are there references for the original and updated methods? Who is the author of the software package "emcee"?**

The reference of the emcee package has been added in the revised manuscript.

5. **line 100: repeated or nested use of "thanks" makes for an odd sentence.**

The sentence has been modified in the revised manuscript.

6. **line 118: "the MCMC has more sampling capabilities than the Metropolis-Hastings method". MCMC is often viewed as a general class of methods, whereas the Metropolis-Hastings is a specific algorithm within that class. A comparison between MCMC and the Metropolis-Hastings methods is confusing.**

We thank the reviewer to point out this confusion of phrasing. Indeed, Metropolis-Hastings is one of the MCMC algorithm. The difference in our implementation for MCMC sampling compared to that of Koot et al. is the number of walkers. In addition, the increased sampling capability comes from the increased amount of data, not the number of walkers. These points have been clarified in the revised manuscript.

7. **Point of clarification on line 146 - "This suggests that the 0.7 scaling factor applied in the MHB model to reduce ocean tidal effects may have been unnecessary". Is the idea that updated ocean**

**tidal models have smaller amplitudes, so there is no need to apply a scaling factor to the new models? Presumably some reduction of the ocean tidal model used in MHB 2000 was "necessary"? Could the authors clarify?**

The matter with this scaling factor was a bit of a detective's work. When calculating ocean tidal effects on nutation, the angular momentum of only several "principal waves" get calculated from the tidal maps, the rest of the tidal terms are interpolated by means of "admittance". In the paper of MHB2000 they applied a constant scaling factor of 0.7, determined by test fittings, to account for the inaccuracy of the motion terms. What we found in Cheng & Bizouard (2025) was that for these terms, the residual amplitudes estimated by least-squares from the CPO series are consistently about 0.3 of the theoretical values, which we suspect were what remained from the "incomplete subtraction" because of the 0.7 scaling factor. So in this work we decided not to apply it. This realization could be attribute to the improvement of both ocean tide modelling and VLBI data quality since the early 2000.

8. **Table 3: "matter" and "motion" terms refer to changes in moment of inertia and local momentum due to the ocean?**

The "matter" term arises from the pressure effect associated with the ocean height, and the "motion" from the ocean current. This clarification is added to the text.

9. **Several solutions are used as "observations" in the Bayesian inversion (see Table 3). Readers are given the names of the solutions and the processing software, but few other details are stated. For example, usn2024b.eoxy and gsf2023a.eoxy use the same processing software. What is the difference between these solutions? The authors note that these different solutions give similar results in the inversion. How should readers assess this statement without information about the input solutions?**

EOP products are produced following the same guidelines given by IERS or IVS, meaning the models used in the data processing are the same, but they can differ in terms of implementation in different softwares used by different analysis centers. For solutions produced with the same software, the processing strategy and products can still differ (outlier detections, a priori values (e.g. in house EOP series instead of the C04 etc). These operational differences could cause small differences in the results of our BEP estimates, and could be a way to assess more realistic uncertainties apart from pure formal errors.

10. **line 186: Does the calculation of "beta" from PREM include the anelastic contribution?**

Yes, the calculation of beta from PREM included the anelastic contribution. This has been clarified in the revised manuscript.

11. **line 191: What value of electrical conductivity is assumed in the calculation of the rms radial field (=0.75 mT)? How is a "realistic" conductivity assessed? How is a "realistic" rms radial field assessed?**

The calculation of the rms radial field is done by following the figure 1 of Mathew et al (2002), assuming a conductive layer of 200 kilometers with a high conductivity of 5 × 105 S m−1 (equal to that of the core) and a

large skin depth (penetration of the time-varying magnetic field into the lower mantle). Such a field strength would require unrealistically high and uniform conductivity throughout the lower mantle (Lobanov

et al., 2021), which is likely too optimistic given that the skin depth is of the order of a kilometer. This point has been clarified in the revised manuscript.

12. **line 276: “The halved Re(K^ICB) values,…..” . The earlier text makes it clear that the “halved values” refer to the MHB 2000 model. Why not be specific in the “Concluding remarks”?**

It has been clarified in the revised manuscript.

**Reviewer 2**

We thank Reviewer 2 for the positive assessment and the helpful suggestions.

**This paper is well written and clearly presented. It is a comprehensive update of estimates of Earth parameters from VLBI nutation time series. The work builds on and compares to earlier studies. It presents results with uncertainties that seem realistic. One small comment is that there are no errors in the cubic-spline estimates of the FCN amplitudes. The rapid increase in the FCN amplitude after 2000 is not seen in all analyses. Would error bars show that the differences are insignificant?**

We made new plots for FCN amplitude variations based on percentile calculation of the posterior ensembles, now the two figures have shades representing an equivalence of 3 sigma.

**I also wonder if the numering of Figures 2 and 3 should be reversed? Figure 3 seems to be the logical next figure after Figure 1. (Alternatively, changing Figure 2 to Figure 1 would put the two FCN figures next to each other.**

We changed the order of figures.

**I also wonder about the availability of software used for this analysis?**

The program can be available on request, but we don't have plans to make it publicly accessible in the near future.