

Interactive comment on “On the nonlinear and Solar-forced nature of the Chandler wobble in the Earth’s pole motion” by Dmitry M. Sonechkin et al.

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As is known from conventional tidal analysis used for predicting ocean sea-level heights, the forced responses must be (and are) numerically exact in terms of matching periodicities. Any of the lunar cycle combinations that do not match can be eliminated. My point is that there is only one lunar cycle possibility that works precisely for the Chandler wobble cycle, and since it is exact, it must be considered as the null hypothesis for any alternative model (such as a natural resonant response).

There is a practical analogy for this: consider an electrical circuit that exhibits a characteristic noise. If when that noise is measured it matches precisely the 60 Hz mains frequency, then that noise is most likely the result of inadequate filtering of the mains,

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and other alternative models should be dismissed as low-likelihood possibilities.

So the original hypothesis of the Chandler wobble derived from Euler's original prediction of 305 days was actually a good characterization of the system's band-pass behavior, with the (exact) 365 day annual and (exact) 433 day lunar nodal period manifested as a forced response passed through the filter response window. The Q-factor of that window does not necessarily have to be high.

Overall, I am in agreement with Professor Sidorenkov's idea (which we have cited in the book) but only desire that the precision must be stressed, as that will provide a benchmark to compare against any alternative models of greater complexity. This seems to be a fundamental geophysics model that has been overlooked in the research and I want to see the strongest supporting argument backed with evidence presented in the paper. At some point this model should be as well accepted as the lunisolar model for ocean tides.

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