**Comments by Referee #3** (Christian Bizouard):
(Responses by the authors are highlighted in blue; note that the line breaks in the original comment have been changed for clarity)

The main interest of that paper is to show that the "intrinsic" circulation in the ocean excites significantly the polar motion at both inter-seasonal and interannual time scales. Stemming from meso-scales eddies, this intrinsic circulation is chaotic, and till now is not fully captured by oceanographic observations. However, it can be simulated, as done by the authors. In many respects, the authors are reshuffling the deck when it comes to modelling polar motion. They show that the inter-annual polar motion could result from the oceanic circulation, without the need of the core-mantle interaction as advocated by many recent papers.
Thank you for assessing and appreciating our work, Christian.

I am not sure whether the authors realized that their study could also modify the current understanding of the Chandler wobble. Indeed, according to Fig. 2b, the mass term of the intrinsic excitation at the level of 1 mas could contribute to Chandler wobble in a very significant way.
Yes, this implication was and is well on our mind. We have quite compelling results on the Chandler wobble excitation by intrinsic oceanic signals but decided not to squeeze them into the current manuscript but reserve them for another paper. The relatively short analysis window (1995–2015) is a bit of an issue, though.

In the forced part of the ocean angular momentum, I wonder whether the authors considered the pole tide as a source of forcing. Could the authors address that important question in the revised version?
Any pole tide signal, which at interannual periods is dominated by an oscillation at the Chandler frequency, is removed from the observations by means of the deconvolution operator (Eq. 1). As a static and thus dynamically irrelevant phenomenon, the pole tide is also omitted from OCCIPUT, just as in any other ocean model. Hence, the treatment of the pole tide in our study is consistent among the geodetic and oceanic excitation, i.e., it is absent from both series. We will insert a corresponding note in the revised manuscript (e.g., in Sect. 2.2 “Oceanic excitation”).

The paper is well written, the approach is well presented. Only legends in Fig. 5 and 6 and captions of tables 1/2 deserve some light improvements: the legends for excitation functions have to be well split, in tables write "Percentage of Explained Variation (PVE)".
Thank you for the hints, we will consider them in the revised manuscript.