

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2022-12

Anonymous Referee #1

Referee comment on "Gravity disturbance driven ocean circulation" by Peter C. Chu,
Ocean Sci. Discuss., <https://doi.org/10.5194/os-2022-12-RC1>, 2022

This paper makes the claim that the neglect of spatial variations in the Earth's gravity field due to the inhomogeneous composition of the Earth leads to substantial revisions to the classical solution for the Sverdrup/Stommel/Munk solution for the depth-integrated circulation of the oceans. If true, this would certainly be a noteworthy result. However, having read the paper several times, and also the recent papers by the same author on the modifications to the equations of motion and oceanic/atmospheric Ekman layers due to the neglect of the same effect, I'm afraid that I am left scratching my head and wondering whether I'm missing something fundamental?

The key question is what one adopts as the vertical coordinate in the equations of motion? Perhaps I was fortunate as a graduate student to have sat through the lectures of Carl Wunsch, but I have always understood a constant "z surface" to represent a time-mean equipotential that accounts for the gravitational and centrifugal forces, that surface being a "bumpy spheroid" due to inhomogeneities in the Earth's gravity field. Under this convention, the only departures of a z-surface from an equipotential are due to the temporal variations in the gravitational field, i.e., the tidal forces.

I accept that the above assumption may not be spelt out explicitly in many text books, and that these variations in the gravity field are neglected from the vast majority of (if not all) computational climate models. However, it strikes me as rather odd to define a z surface as either a spherical or spheroidal reference surface and then incorporate additional horizontal gravitational forces, as is done here and in the author's other recent papers. I am pretty sure the community does not have in mind that following a z surface from boundary, to the centre, of the Indian Ocean requires one to climb roughly 100m against gravity.

I have not worked through the details, but related to the final point above is that the assumption of a rigid lid at $z=0$ in the derivation of the Sverdrup/Stommel/Munk equation (19) is unjustified given the order 100m variations in sea surface elevation in the authors coordinate system.

I am also missing a good physical explanation in the paper of how the additional torques arise to drive the additional depth-integrated circulation? In equation (19), the additional source of vertical vorticity arises through the projection of the baroclinic production of vorticity onto the vertical component of the vorticity equation. However, if a z surface is defined as an equipotential, then this term vanishes identically, calling into question the statements made in the abstract - the solution should not depend fundamentally on the choice of coordinate system.

So, in summary, I'm afraid that I cannot recommend this manuscript for publication as I feel that the results rely on a particular and, in my honest opinion, rather odd choice of coordinate system. If I have missed something fundamental, then I apologise in advance and am happy to stand corrected.

Finally, I note that there is quite a lot of overlap between this and the author's three previous papers on a similar topic, especially in the preliminary material. If the manuscript is published, then I would suggest pruning the material down to focus on that which is novel to this contribution.