Dear Professor Marshall,

Thank you very much for reviewing my manuscript.

Major Responses

First, your negative review is on the base of the concept that "*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*," which I disagree. The departure of a z-surface (i.e., spherical/ellipsoidal surface) from an equipotential (or more accurately say, the departure of an equipotential surface from a z-surface) represents the gravity disturbance, and is mainly caused by the nonuniform mass density inside the solid Earth rather than by the tidal forces. There are near 3 orders of magnitude difference between the two. You may find the definition of the gravity disturbance from the cited reference (Hackney & Featherstone, 2003) [or Equation (10) in the manuscript] and the quantification of the time-independent gravity disturbance by static gravity models with observations in geodetic community (see website http://icgem.gfz-potsdam.de/home). From a popular static gravity model EIGEN-6C4, the globally averaged absolute value of the gravity disturbance at z = 0 is estimated around 20 mGal (1 mGal $=10^{-5}$m s^{-2}). However, the sun and moon create time-dependent tidal forces that affect the measured value of gravity by about 0.3 mGal (from the Wikipedia website https://en.wikipedia.org/wiki/Gravity_anomaly). Such evidently large gravity disturbance (additional body force) exists no matter with or without tides and is totally neglected in ocean dynamics.

Second, your negative review is due to your interpretation of the geoid surface, "*I am pretty sure the community does not have in mind that following a z surface* (I think that you mean the geoid surface) *from boundary, to the centre, of the Indian Ocean requires one to climb roughly 100m against gravity,*" which I disagree. The geoid surface is an equipotential surface of the true gravity (normal gravity plus gravity disturbance). Following the geoid surface from boundary, to the center, of the Indian Ocean requires one to climb roughly 100m but NOT against the TRUE GRAVITY. Because the value of the true geopotential keeps the same on the geoid surface. You may find detailed information about the geoid in the Introduction Section of my manuscript or a geodesy textbook.

Third, your negative review is due to your treatment of the geoid surface as the sea surface elevation, "*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*," which I disagree. The geoid surface and the sea surface elevation are independent and different. The geoid surface is an equipotential surface of the true gravity, and nothing to do with the sea surface elevation. The famous Brun's formula in geodesy shows that the gravity disturbance at z = 0 is the multiplication of the standard gravity (9.81 m/s^2) and the horizontal gradient of the geoid. See Equation (A1) of Sandwell and Walter (1997): Marine gravity anomaly from Geosat and ERS 1 satellite altimetry, JGR 102, 10,039-10,054 (see website: Marine gravity anomaly from Geosat and ERS 1 satellite altimetry (ucsd.edu)).

Fourth, your negative review is based on the statement, "*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,*" which I disagree. The z surface represents vertical location in the spherical (or local) coordinate system. The equipotential surface is defined by the true gravity vector field. The z surface can never be defined as an equipotential surface. Even in geodesy, the gravity and equipotential are the major subjects. The equipotential is represented in the spherical coordinate system with z as the independent variable representing the vertical location.

Responses to Your Assessment

"*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*."

Yes. This manuscript shows that the gravity disturbance driven ocean circulation is comparable to the wind driven ocean circulation.

"*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?*"

Yes. A fundamentally important forcing term, the gravity disturbance, is missing in ocean dynamics.

The Ekman layer dynamics and the Sverdrup/Stommel/Munk dynamics are related, but not the same. A paper published on the Ekman dynamics cannot be the reason to reject a paper on the Sverdrup/Stommel/Munk dynamics. I would like to know if any one has studied the effect of the gravity disturbance on the Sverdrup/Stommel/Munk dynamics, and if any paper has included the gravity disturbance in numerous publications on the Sverdrup/Stommel/Munk dynamics such as the Sverdrup transport.

Response to Your Specific Comment

"*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 vortictiy onto the vertical component of the vorticity equation*."

Eq(19) is the curl of depth-integrated circulation from Eq.(18), i.e., the horizontal momentum equation for steady-state low Rossby number with friction. Eq(18) shows the balance among the Coriolis force, horizontal pressure gradient force, friction, and the additional gravity disturbance. Due to the Boussinesq approximation, all the terms are independent on the density or density anomaly (i.e., density minus constant reference density) except the gravity disturbance term, which is the gravity disturbance multiplied by the density. That is the physical reason of the additional source of the vertical vorticity arises through the projection of the baroclinic production of vortictiy onto the vertical component of the vorticity equation, i.e., the vertically integrated Jacobian of the density and gravity disturbance.

Response to the Recommendation

"*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*."

I hope you may change your recommendation after reading my responses. Also, I can't get my head around your critics on the choice of coordinate system. I just use the common z coordinate system like most of oceanographers do.

"*Finally, I note that there is quite a lot of overlapl 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*."

I will revise my manuscript before publication to reduce redundancy.