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Comment on os-2022-12

Anonymous Referee #2

Referee comment on "Gravity disturbance driven ocean circulation" by Peter C. Chu, Ocean Sci. Discuss., https://doi.org/10.5194/os-2022-12-RC3, 2022

I have read the manuscript by Peter Chu, and while I found it quite thought-provoking, I am forced to conclude that it is actually quite misleading and do not recommend publication in its present form. It seems to me that the mistake the author is making is to formulate the equations of motion in spherical coordinates from the beginning. This is not my understanding of how the equations of motion used by models of the atmosphere and ocean are formulated. Rather, these models use a coordinate system in which the vertical direction is defined as being perpendicular to geopotential surfaces so that gravity always points along the vertical direction with no horizontal component. The resulting coordinate system is orthogonal and curvilinear with the horizontal surfaces varying in distance from the centre of earth in response to variations in the geopotential. The usual practice in the modelling community is to approximate this curvilinear coordinate system by spherical coordinates. It seems to me that the onus is on the author to show that the terms that are neglected when this approximation is made are important and should not be neglected. It should be noted that starting from an orthogonal, curvilinear coordinate system in which the horizontal surfaces correspond to geopotential surfaces, and then approximating the resulting system of equations using spherical coordinates, is not the same as formulating the governing equations in spherical coordinates from the beginning, as the author insists on doing.

It is worth noting that I have no argument with equation (12) in the manuscript which is written in vector form. One consequence of this equation is that the equilibrium state is the one in which isopycnal surfaces coincide with geopotential surfaces, as implied by equation (12) when the pressure gradient term is balanced by the term involving gravity, corresponding to hydrostatic balance. In the coordinate system used by ocean modellers, the equilibrium state corresponds to horizontally uniform stratification.

Another issue I have with the manuscript is the way in which the author evaluates the Jacobian term in his equation (19) using data directly from the World Ocean Atlas without comment. At the very least, one needs to ask what coordinate system is being used by the World Ocean Atlas and whether this is the same as the coordinate system being used in equation (19). Indeed, is it appropriate to simply insert data from the World Ocean Atlas directly into the Jacobian operator? I also feel that the author is being too relaxed in his treatment of the hydrostatic approximation since this strictly applies only in the coordinate system in which gravity acts in the vertical direction. However, these are minor points compared to what I have written in the first paragraph of my review.

In summary, I cannot recommend publication of this manuscript in its present form and I believe the argument being put forward by the author is flawed. At the very least, the author needs to formulate the governing equations in the orthogonal, curvilinear coordinate system in which gravity always acts in the vertical (z) direction. He then needs to consider the terms that are neglected when this coordinate system is approximated by spherical coordinates. Since these terms will, at best, involve accelerations terms arising from the curved surfaces that correspond to geopotential surfaces, I cannot see that these terms can be important or that models, as currently formulated, are fundamentally wrong. If the author believes otherwise, the onus is on him to show readers what these terms are, and why they are important.