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Interactive comment on "Technical Note: Two types of absolute dynamic ocean topography" *by* Peter C. Chu

Anonymous Referee #1

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I found this paper difficult to understand. After reading it several times, I think that it isn't really about two different oceanic dynamic topographies. It's really about two different ways to estimate a geoid, and thus the ocean sea surface height relative to that geoid. Is it not true that were data perfect, the two geoid estimates would necessarily agree? (The question is posed on line 119, but the answer uses two different data sets with differing error budgets and hence would never be identical. What does theory say?) There are also a number of confusing statements. Going through, roughly in page order,

I don't understand the first sentence of the Introduction. What is a spherical harmonic model in a flat-Earth approximation?

Eq. (1) is also applicable if defined relative to the center of the Earth..

V appears not to be the gravitational potential, but the anomaly of the gravitational potential.

If g is really fixed in Eq. 2, it should be written as g_0 as e.g., in Eq. (6) \Delta g appears on the left of the equation and also on the right. Are we meant to interpret g as g=g+\Delta g?? Brun's formula requires a reference.

The paragraph starting on line 60 seems to be the crux of the issue, or of my misunderstanding. H_r is called a "reference depth". Is this different from what is usually called the "level of no motion"? Is it not true that if the actual geostrophic velocity were known at this depth, it would be a "level of *known* motion" and the two definitions of D would coincide? The paper states that the differences between the two definitions of D is beyond the scope of the paper. But isn't this just the problem of classical oceanography of figuring out whether there is a deep reference level where the geostrophic velocity actually vanishes? Or at least getting an error estimate on it? If my reading of this paragraph is incorrect, then I do not understand at all what the paper is trying to say.

Going on, doesn't the traditional marine geoid really require that the water density should be constant? If it is constant, then it follows from the geostrophic relationship that no flow exists. But if the density is a function of depth, there is (to first approximation) no flow, but the integrated water density and hence absolute height would depend upon the vertical distribution.

The ocean circulation is forced by buoyancy and wind. Does the minimum energy argument apply to a forced system where energy put in must also be dissipated (line 284)? Maybe the corrections are negligible here?

Perhaps I have completely misunderstood this paper, but if so, it needs to be completely rewritten to make it less obscure, and to explain what it actually means.

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