

## ***Interactive comment on “An oceanographer’s guide to GOCE and the geoid” by C. W. Hughes and R. J. Bingham***

### **Anonymous Referee #4**

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The manuscript describes the working knowledge about the geoid and models of the geoid that is required for any oceanography who needs to interpret absolute dynamic sea surface topographies derived from satellite altimetry and a geoid model. Necessarily, there is no new information included, and the manuscript takes the form of a short review paper which conveniently summarizes the relevant information, that one needs to extract from various sources otherwise. The manuscript’s style is mostly clear and concise. As far as I can see all ideas and concepts of this manuscript have been published before or are text book knowledge (e.g., the properties of Legendre Functions and spherical harmonic functions). In that respect (if one regards the manuscript as a review) I feel that there are a few references missing, for example, Smith (1998), who describes most of the recipe for computing geoid heights.

(Dru A. Smith, "There is no such thing as 'The' EGM96 geoid: Subtle points on the use of a global geopotential model", IGeS Bulletin No. 8, International Geoid Service, Milan, Italy, p. 17-28, 1998. [http://www.ngs.noaa.gov/PUBS\\_LIB/EGM96\\_GEOID\\_PAPER/egm96\\_geoid\\_paper.html](http://www.ngs.noaa.gov/PUBS_LIB/EGM96_GEOID_PAPER/egm96_geoid_paper.html))

There are a few inaccuracies that need to be corrected, see below.

At our lab we have have written/assembled a similar manuscript on this topic (geoid models for oceanographers). We never submitted this manuscript to a journal, because we found the scope of such a "guide" not suited for a full journal publication. From this point of view it is a matter of the scope of the journal Ocean Science, whether the manuscript is suitable for publication.

Specific comments:

1.Introduction:

I do not understand why the geoid is not introduced as equi-potential surface (which is as surface of constant potential  $W$ ). The discussion here and in Section 2 is a bit misleading.

2.Definition of the geoid:

p1546, l19, in this paragraph the latest, a reference to Smith (1998) and others seems in place.

eq.7 Minor issue: Choice of symbols is maybe a bit unfortunate:  $\theta$  is often used for co-latitude and  $\phi$  for latitude.

2.1 Permanent tide system: p1551, top, with all of these different number, a reference of where they come from seems in place. To satisfy my personal curiosity: I have seen different values for the conversion factor between mean and zero tide (unfortunately I cannot remember where), eg.  $19.8 \cdot (1/2 - 3/2 \cdot \sin \text{Lat})$ , which would result in 26.7cm (as opposed to 26.6cm). How important is the accuracy of this number?

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### 3. Spherical Harmonics

Minor issue: I find the description of spherical harmonics a bit too much, culminating in the explanation of “orthogonal”. Is that really necessary? (spherical harmonics, Legendre functions, etc. were the subject of a first or second year course of my physics degree, do we need to see it again in this form?)

#### 3.1 Complications with SH

p1557, l121: there is no Gibbs effect in the geoid model itself. The authors probably mean the Gibbs fringes that stem from taking the difference between altimetric SSH and a spectrally truncated geoid model. These two fields are both large in amplitude (compared to the amplitude of their difference, namely, the dynamic topography) and contain different scales, so that their difference appear to include Gibbs fringes.

p1557/1558: I recommend that the authors perform a decomposition of any (eg. numerical model) dynamic topography into spherical harmonics and compare the corresponding spectrum of the dynamic topography (e.g., the per degree variance) with that of the land-sea mask. The spectra are strikingly similar, implying that the representation of any field that is not defined on the entire sphere (such as the dynamic topography or the sea surface) in spherical harmonic functions is awkward if not inappropriate. The method that the authors propose in paragraph l2-l12 is insufficient to overcome this problem.

p1559: the issues with the omission error are not settled, as far as I know. Again, references are missing.

p1560, last paragraph of 3.1: why not include references to local geoid solutions, e.g. GOCINA (and I am sure that there are others, too)?

#### 4. The GOCE measurement system

p1562, l13: “two, six-month period”, Is the comma a typo?

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## 5. Reference ellipsoid

eq.18, there is a factor  $(GM/GM_g)(R/R_g)^n$  for the case that the reference ellipsoid parameters  $GM$  and  $R$  for the geoid ( $_g$ ) are not the same as for the altimetric sea surface height (see eg. Smith, 1998).

It is not immediately clear that eq.18 follows from eq.2.92 of Heiskanen and Moritz.

## 6. Recipe

It's probably helpful if  $R$  is identified with  $a$  from section 5. The text makes this connection, but it would be clearer if the symbols with reflect that too).

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