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Ocean Science Discussions

Interactive comment on "An oceanographer's guide to GOCE and the geoid" *by* C. W. Hughes and R. J. Bingham

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Reply to referees' and other comments on "An oceanographer's guide to GOCE and the geoid"

We thank the referees for their thoughtful comments, and these will be accounted for in the next version of the paper. Here, we will address first a number of points which have been raised by several people, followed by a more detailed discussion of points raised by individual referees and commenters, to indicate how the next version will be improved.

A point raised by several people is the question of suitability of what is essentially a short review article containing no new information for publication in OS. We believe the comment by David Webb indicates that the article is suitable, but would like to

make some additional comments. There is currently no obvious place in the refereed oceanographic literature for articles which take an unusual approach to a subject, or summarise known information from a particular perspective, without adding any substantial new results. There are journals dedicated to reviews, but not to less substantial works simply promoting new perspectives. We think this is a shame, as such articles can be very stimulating and can provoke new thinking on problems, but currently have no obvious outlet. We hope that the new publishing model embraced by Ocean Science will change that, and can act as a catalyst for more dynamic discussion within the oceanographic community. In this particular case, our aim is to distil information which is well known to geodesists, but actually widely spread throughout books, a few journal publications, and a lot of 'grey' literature, into a form readily accessible to oceanographers. We feel that this is a worthwhile exercise, but wish to avoid contributing more to the hard-to-find literature of bulletins and reports by publishing in a truly accessible, refereed journal.

A second point which several people have raised is a suggestion that there are a number of missing references. We will do our best to rectify this by including the Smith (1988) reference mentioned and some GOCINA references. However, it was precisely this lack of readily-available references which led to the writing of this article. Many of the geodetic references appear (like the Smith reference) in the "grey" literature of Bulletins, internal reports, and conference proceedings, making it difficult for an oceanographer to find them. Indeed, it is particularly difficult to determine which are considered to be authoritative by the geodetic community given the apparently ephemeral nature of many of the publicactions. We would appreciate any advice on which reports etc. to include as relevant references.

A third point concerns Gibbs' phenomenon. It became clear from the referees' comments, and from conversations with a number of people, that there is a wide range of interpretations of what is meant by "Gibbs' phenomenon". Some people believe it only refers to artificial oscillations induced at the boundaries of a non-periodic domain by

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attempting to represent a non-periodic function as an infinite sum of smooth, periodic basis functions. We have used the term in its more general sense as referring to the same behaviour when it results from attempting to represent a step discontinuity as an infinite sum of smooth, periodic basis functions, and even more generally, when it results from attempting to represent small scale but finite gradients by a finite sum of basis functions which do not include short enough length scales to accurately represent the gradient. This is how the term is commonly applied in both meteorology and in medical imaging. It has also become apparent that quite a number of oceanographers are unfamiliar with any version of Gibbs' phenomenon. Accordingly, we have decided to expand this section to remove any possibility of ambiguity and to give explicit examples of the phenomenon.

The final point which several referees picked up on is one of notation: we have failed to conform to the geodesists' convention that theta represents colatitude and phi represents latitude. We will switch notation to conform to this convention.

Referee No. 6:

Page 1563, line 12, "the ellipsoid is hardly ever within a metre of the sea surface, so what is meant?" This was a mistake. The point is referring to the earlier discussion in section 2 about the near-equivalence between geopotential at the sea surface and height of the sea surface above a reference geopotential surface, which becomes untenable if the reference surface is far from the sea surface. What we should have said was "As long as the chosen reference geopotential is one which is, for the real earth, within about a metre of the sea surface...". This will be changed for the next version.

Page 1559, line 3, "in practice,...", the referee asks for a reference to be provided. This concerns the practice of changing between permanent tide systems by (artificially) adding a small correction to the C20 term. We are not aware of any reference in which this is explicitly stated, the observation comes from our reading of various programmes in which this form of correction is implemented. Any advice would be welcome.

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Referee No. 4:

"I do not understand why the geoid is not introduced as equipotential surface...". This is because, to many oceanographers, the need to actively consider the shape of equipotential surfaces is not evident. There is a need first to explain what the geoid is in the more familar language of "level" surfaces and the direction of gravity before moving on to the definition as an equipotential, which can seem quite abstract.

References to Smith (1998) and to sources for the numbers in permanent tide systems will be added.

Spherical harmonics - these are not familiar to many oceanographers (if oceanographers have any familiarity at all with spherical harmonics it is usually a distant, unused memory from undergraduate days). It is therefore felt worthwhile to provide a brief description together with pointers to the main ways in which they differ from the more familiar Fourier series.

Gibbs effect: dealt with above

p1557-8: Concerning representation of the sea surface height in spherical harmonics. We agree that this does not solve the problem completely, although we have a submitted paper which clearly shows that it is preferable to the more intuitive method of producing a gridded geoid, subtracting from the sea surface height, and then smoothing the resulting dynamic topography. The best way to overcome this problem is still a subject of research. We will add comments to this effect, together with references, in the revised version.

"issues with the omission error are not settled..." We agree, and hope that we gave that impression.

"Again, references are missing"... such as? We can try to supply them but help would be appreciated.

p1560 "include references to local geoid solutions" - we will add some appropriate

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references.

p1562 "two, six-month period" - comma will be removed.

eq. 18 (accounting for different values of GM and R in geoid and ellipsoid) - the referee's suggestion will be implemented.

"It is not immediately clear that eq. 18 follows from eq. 2.92 of Heiskanen and Moritz". We will expand the description slightly (also needed is eq. 1.73). There is also a typo in the equation (5J2 should be 5nJ2). Actually, to describe completely how to obtain this equation, or how to interpret H&M's 2.92 would require references to equations throughout the earlier section of the book. It is to save readers this kind of trouble that we wrote this article.

Identify R with a from section 5. We plan to merge and clarify sections 5 and 6, and will ensure that this suggestion is implemented.

C. Tscherning

Gibbs' effects: see above

About the gravity gradient tensor: we will rephrase this more carefully. The tensor actually measured involves 9 independent numbers, with the asymmetric component containing information about rate of change of rotation, and the symmetric component (six numbers) relating to gravity. In fact, since the trace is zero (ignoring the mass of the gradiometer itself), it could be argued that there are only 5 numbers representing the gravitational field. We would defend the term "tensor" though, as it conveys a lot of meaning to those familiar with the term.

We disagree that we should avoid the calculation of normal gravity. It is, indeed, possible to simply calculate the gradients of the potential directly from the spherical harmonic coefficients but, as the quoted manuscript demonstrates, the formulae can be rather involved, making it is easy to make mistakes. We will mention this alternative, but for the sake of having a self-contained description with the minimum of unnecessary detail,

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we prefer to include the normal gravity formula.

Equation 22, start the summation from l=2: agreed. We will change this.

Referee No. 2

This referee writes particularly from a geodesist's perspective, and suggests that the current text be shortened with much of the current text replaced by references, while expanding the description of oceanography for geodesists.

Although we are happy to expand slightly the description of oceanography, we do not think it appropriate to shift the focus as strongly as the referee suggests. It is, after all, supposed to be a guide for oceanographers, not an oceanography guide for geodesists. As such, it is important that all the necessary information be found in one place.

"Important references are missing": Again, a plea for some help with identifying which references in the geodetic literature are considered "important".

We will include some examples from available models, as suggested.

Detailed suggestions:

1) Definition as an equipotential surface. As in our reply to referee No. 4, we disagree. This may be appropriate from a geodesist's perspective, but not for an oceanographer.

2) Use phi for latitude: agreed.

3) 21 or 21.4 km bulge: this will be unified.

4) Degree variances: This will be added.

5) Gibbs' effect: see above.

6) The sentence ... is unclear ... the dimension of the altimeter footprint ... is smaller than 7 km. We will rewrite this more clearly. The altimeter footprint depends on satellite and on surface wave conditions, but is about 5 km for typical satellite altitudes and moderate significant wave heights of 3 m, scaling linearly with wave height (Chelton et

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al, J. Atm. Oceanic Tech, June 1989, pp 407-438), so 7 km was not unreasonable, but we will change this to 5 km and qualify it by pointing out the variability.

7) What is "Omission error in coastal region"?: It is the omission error near to the coast, it's hard to think of another way to say it. It is particularly problematic because it is impossible to average sea level over an area surrounding a coastal point, for comparison with a smoothed geoid estimate. We will endeavour to clarify this point.

8) Two, six-month: comma removed.

9) We will delete the longitude dependence of gamma.

10) We will add references to recent geoid models.

11) "not used to work" No. The original is correct.

Referee No. 3

The referee suggests some reorganisation of the paper and splitting into subheadings. This seems sensible, and we will do this where appropriate. In addition, we will add some discussion of error covariances as suggested.

We disagree that much of section 5 should be discarded, but agree that it is currently rather clumsily interpolated. We intend to merge sections 5 and 6, and incorporate the numerical values of GRS80 more smoothly throughout, in the form of example values (valuable for checking code and getting an order of magnitude feel for things). We feel that it is important to summarise the procedure in a clear, step by step manner.

Interactive comment on Ocean Sci. Discuss., 3, 1543, 2006.