Review of essd-2023-85, "Global marine gravity gradient tensor inverted from altimetry-derived deflection of the vertical: CUGB2023GRAD", by Walter H. F. Smith (ORCID, ResearcherID)

General Comments

In my opinion, this manuscript does not meet the standard of enabling the user to evaluate whether the data product CUGB2023GRAD will be useful, whether it is an improvement on existing products, or even whether or how the authors have taken steps to avoid circular reasoning. Critical issues in data editing, filtering, and computation are not adequately described. I also believe that the input data sets may not be the best ones for this kind of analysis.

The work described seems to rely heavily on the previous works of the Danish group led by Ole Andersen and the Scripps group led by David Sandwell. While a few papers from these groups are cited, often the most relevant ones are not cited. It would be helpful to have reviews from Drs. Andersen and Sandwell.

Specific Comments

The strong coherence of the data with the GEBCO bathymetry (manuscript Figure 9 and its discussion) should not be taken as a measure of the quality or value of CUGB2023GRAD, because depths predicted from satellite altimetry are in the GEBCO product. In order that the reasoning not be circular here, one would need to demonstrate that the coherence had been computed from a portion of the GEBCO product that contained almost entirely in situ measured depths, and not depths estimated from altimetry. The GEBCO Source ID grid could be a help here.

As the manuscript makes clear, all six gradient tensor elements arise from differentiation of one scalar quantity (the disturbing potential), and so these elements are six different views or characterizations of one set of information. The manuscript does not demonstrate the utility of having all these different views of the same information.

The manuscript presents the trace (sum of the diagonal elements) of the gradient tensor and suggests that the quantitative value of this sum is an evaluation of the product. But since the equations used all derive from the assumption that the gravity field obeys Laplace's equation, the trace ought to be zero by definition. The manuscript does not present any way for the reader to undestand quantitatively the significance of a non-zero trace: how does it compare to the noise in the data, noise in the model components, limitations on the resolution of each quantity, etc.?

The manuscript has too many equations presenting the general theory, as this could have been summarized with a citation to a standard textbook. Some of these equations are given in spherical coordinates and some in Cartesian coordinates, and it is not clear which coordinates are used for the calculations done to produce CUGB2023GRAD. What the manuscript needs to do is to explain how the calculations in the 2 degree by 2 degree patches of Earth surface area were carried out. I presume they used Fourier transforms on Cartesian coordinates after a remove-restore procedure.

In my opinion 10.1016/j.asr.2019.09.011 does a very good job of demonstrating quantitatively the contribution of each satellite altimeter mission to the overall marine gravity field model, treating the east-west and north-south components separately and treating each as functions of latitude, and showing what weight should be given to each. The present manuscript does not do this very well. Perhaps the present study's analysis of what weight to give each satellite mission in deriving each of the tensor quantities in each of the 2x2 squares might furnish some interesting information, but that information is not presented here.

The filtering of the data is an important detail, but the equation describing the filter (Equation 1) is wrong: if tau is the filter width parameter then tau-squared should appear somewhere in the argument to the exponential in Equation 1. Another minor point: I assume that the data to be filtered are very closely spaced, and in that case computing the filter using Equation 2 followed by the inverse cosine will be quite inaccurate.

The input data used are available only at the "1 Hz" nominal sampling rate, for many of the altimeters included in this study. As many papers by Sandwell and his colleagues for over 30 years have shown, "1 Hz" data are down-sampled from boxcar averages of the original data, which have nominal sampling rates of 10, 20 or 40 Hz, depending on the satellite; the boxcar averaging has bad side-lobes; and the 1-Hz downsampling aliases sidelobe energy into long along-track wavelengths, spoiling the accuracy of the resulting along-track deflections of the vertical. For this reason, Sandwell and colleagues have taken great pains to design specialized filters and downsampling rates. Therefore I believe that the accuracy and utility of CUGB2023GRAD may be limited by the fact that it starts from "1 Hz" data. (The along-track filter design description in 10.1029/95JB01308 pre-dates the development of two-pass retracking [10.1093/gji/ggt469] and so the filters now used have different pass- and stop-band specification than what is described in 95JB01308.)

An important detail is the removal of the non-geoidal signals from the sea surface height. One of these, the Mean Dynamic Topography, is mentioned in Equation 3. But others (tides, transient dynamic signals, etc., as well as errors in radar path delays, sea state bias, etc.) are not mentioned. One wonders how this was done. It will have an important impact on the quality of CUGB2023GRAD.

Equation (4) correctly shows that the north and east components of deflection are the coresponding partial derivatives of the geoid height anomaly, but in the Introduction these deflection components are incorrectly described as derivatives of the disturbing potential. The correct relationship requires relating the geoid height to the disturbing potential, such as via Bruns formula.

It is not correct to say there has been little prior work on the vertical gravity gradient; in addition to 10.1126/science.1258213 , 10.1029/2020JB020017 should also be cited.

The Generic Mapping Tools should be cited for the program *surface* at line 111; which citation depends on which version was used: https://www.generic-mapping-tools.org/cite/

If the authors, or anyone reading this review, needs to download PDF reprints of David Sandwell's papers, they are available for free download (no paywall) here.