

Goswami et.al: OESbathy version 1.0

Review of manuscript submitted to Geoscientific Model Development (gmd-2015-32)

Benjamin Hell, expert bathymetry, Swedish Maritime Administration

2015-04-15

General comments

The manuscript submitted by Goswami et.al. introduces a method to model world ocean bathymetry based on today's observed relationships between the age of oceanic crust, the sediment cover of oceanic crust and the geometry of passive continental margins. If these relationships are known for the geologic past, or can be extrapolated from today, the method can reconstruct paleo bathymetry for the World ocean configurations in the geologic past.

Considering the importance of ocean bathymetry for e.g. climate modelling, the question of reconstructing paleo bathymetry is very relevant. The presented approach is comparably easy to apply, because it does not rely much upon observed geologic data other than crustal age and ocean basin geometry.

The authors test their method against the present world ocean bathymetry. The OESbathy model appears to work well for the open ocean, i.e. mid-ocean ridges and abyssal plains. It has limitations when it comes to passive continental margins, especially those heavily shaped by terrestrial sediments. The method does not deal with active continental margins, large igneous provinces, seamounts and other regional "anomalies" such as hotspots.

When applying the model for a particular situation in the geologic past, calibration with any existing additional paleo bathymetry data probably increases the significance of the results. It would be good if the authors could test or comment this option in the final version of the article.

The manuscript and supplementary material are well-structured and reasonably complete, including the model code. However, a discussion of and references to prior work on the topic of paleo bathymetry modeling are still missing. Also the figures could be improved in order to make it easier for the reader to follow the argumentation in the text.

Apart from a number of suggestions for less significant changes (see below), I believe that the manuscript with these improvements will be mature for publication.

Specific comments

The following more specific comments on the manuscript are structured according to the GMD review criteria.

Scientific significance

Does the manuscript represent a substantial contribution to modelling science within the scope of Geoscientific Model Development (substantial new concepts, ideas, or methods)?

- *Does the paper present novel concepts, ideas, tools, or data?*
- *Does the paper represent a sufficiently substantial advance in modelling science?*

Not being an expert in paleo bathymetry modeling myself, I believe that the manuscript introduces a significant advance in at least two ways: (1) The method can be applied to the entire World ocean, and (2) it takes passive continental margins into account.

However, the approach seems not to be fundamentally new, and similar or related reconstructions have been made by e.g. Hayes, Zhang and Weissel (2009; EOS Transactions vol 90/19) or Celerier (1988; Palaios vol 3). The lack of references to such prior work is a major issue of the manuscript that should be fixed in the final article.

Scientific quality

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Do the models, technical advances, and/or experiments described have the potential to perform calculations leading to significant scientific results?

- *Are the methods and assumptions valid and clearly outlined?*
- *Are the results sufficient to support the interpretations and conclusions?*
- *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?*
- *Are the number and quality of references appropriate?*

According to the authors' approach, bathymetry is a superposition of three factors: (1) the underlying oceanic crust, (2) the sediment layers on top of it, and (3) typical passive continental margins comprising shelf-slope-rise structures. Although this of course is a simplification, at least the first two factors are a valid and common assumption. The presented method, however, seems to be limited with regard to the third factor. Passive continental margins vary a lot in comparison to each other, and it seems to be difficult (if not impossible) to derive some kind of "typical geometry" for them. Some specific comments about this can be found below under the technical corrections. But the authors are very honest and quite clear in discussing this shortcoming of their method.

The model is verified by applying it to present day world ocean bathymetry. The authors compare the modeled results carefully with the actual present day data, and describe them accurately.

In section 4.2 the reconstructed open ocean regions are described. According to Fig. 10, most areas are modeled to within ± 1000 m of the actual, present day bathymetry. According to Fig. 3, these areas also feature generally less than 1000 m of sediment. The model relies upon a linear regression of sediment thickness data with a lot of variation (Fig. 2), indicating a more complex reality than what the model is capable to simulate. From the figure it is not clear why and how the chosen regression line fits the data best. The question how much the model really is improved by adding the modeled sediment layer is unfortunately not answered.

One can argue about the validity of the reconstructed shelf-slope-rise structures (section 4.1). Also here, the validation is heavily based on a regression line fit to data with large variation (Fig. 8). The authors mention an "anomalous" outlier originating in the Gulf of St. Lawrence. If this point was not

taken into account, a simple linear relationship would likely fit the data equally well. However, to me the variation of the points in Fig. 8 indicates that one should not try to find any simple relationship “explaining” the difference between continental slope and rise regimes. Maybe not distinguishing between slope and rise would have yielded equally good (or better) modeling results.

The lack of references to prior work has already been mentioned above. Otherwise, the references generally appear to be appropriate and complete.

Scientific reproducibility

To what extent is the modelling science reproducible? Is the description sufficiently complete and precise to allow reproduction of the science by fellow scientists (traceability of results)?

5. *Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development papers should be similarly reproducible. For MIP and benchmarking papers, it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible.*

The methodology is described very thoroughly. This includes a discussion of the data the model is derived from, the formulae for subsidence due to plate cooling, and the sediment model. The supplementary material also includes the program code. To run the programs, proprietary standard software is needed (Matlab, ArcGIS). All input data is open.

Presentation quality

Are the methods, results, and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?

7. *Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model.*
8. *Does the abstract provide a concise and complete summary?*
9. *Is the overall presentation well structured and clear?*
10. *Is the language fluent and precise?*
11. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?*
12. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?*
14. *Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical, and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged.*

The title of the manuscript clearly reflects its contents. One could argue about the word “realistic”, as the quality of the shelf-slope-rise structures is the major shortcoming of the model.

The abstract is easy to read, appears complete, summarizes methods and findings well, and also includes shortcomings of the model.

The manuscript is structured in the classic way (introduction, methodology, results, discussion, conclusions). I would swap sections 4.1 and 4.2 in order to follow the same structure as under section 3, but that is a minor comment.

Apart from minor issues (see below), the language is clear and the manuscript is very readable. This is also true for (most) formulae, abbreviations etc.

The manuscript in its current form is very comprehensive, and the authors should consider shortening it carefully in order to better support the main findings. One example is section 5.2, that is not strictly related to the OEMbathy model at all, apart from its last paragraph. Also the amount of figures can be overwhelming, especially if one includes the supplementary material. Where appropriate, figures should be combined. In figure series, insignificant figures could be left out. The supplementary material is complete and appropriate, but one could maybe leave out plain reproductions of other scientists' datasets that are easily accessible through the internet.

Although not explicitly mentioned under this section in the review guidelines, I would like to spend a couple of words on the quality of the figures, which I think should be improved in order to make it easier for the reader to follow the authors' argumentation. I believe that the importance of high quality figures cannot be underestimated for the perceived quality of an article.

Many of the figures are too small to be most useful (e.g. the maps, Fig. 8 and Fig. 11). Also, often the color scales chosen are not the most appropriate for its purpose. One problem is very low contrast (e.g. Fig. 3). In other occasions, a color scale (with the same colors) differs in values between figures that are sometimes even being directly compared in the text (e.g. Figs. 12a and 13a). A very common problem (and certainly not only in this manuscript!) is the inappropriate use of the rainbow color scale to express a "positive-neutral-negative" relationship, as e.g. in Fig. 10 (and many more). Such figures are much easier to interpret if the zero or neutral value is plotted in a neutral color (white or grey), and positive and negative values get a specific color of their own. A standard color scale for such cases is e.g. blue-white-red. The rainbow colors with a "floating zero color" (between different figures) are rather confusing. If the authors even stick to a consistent color for the land areas (e.g. grey), the eyes of the readers will be pleased even more...

Technical corrections

Please take these corrections as suggestions. Some of them are more significant than others (e.g. the ones regarding section 3), and many are certainly a matter of taste (or nit-picking).

Section 1

Page 3081 Line 27: Name the model "OESbathy", and do not abbreviate it. As the authors state, the abbreviation "OES" is already being used for "Open Earth Systems".

P 3082 L 7: Are the confidence levels really "quantitative", or only qualitative? Standard deviation works strictly only for Gauss distributions, and ocean hypsometry is not Gauss distributed.

Section 2

P 3082 L 12, P 3083 L 1: Use subsection headings for ocean crust age and sediment thickness data.

P 3082 L 12 to L 26: The entire section could be shortened significantly.

P 3082 L 12 and L 14: Use the same term, either “ocean crust age” or “crustal age”.

P 3082 L 13: Remove the web address from the citation, and put the complete web address to the data into the reference instead (<http://www.earthbyte.org/Resources/agegrid2008.html>).

P 3082 L 15 and L 18: I would call “reconstruction age” rather “reconstruction time”.

P 3082 L 20: Leave out the (self-evident) definition of bathymetry.

P 3082 L 24: Which exact version of EB08 was used? The download page lists several versions.

P 3082 L 24: It is irrelevant that the data comes in decimal degree coordinates.

P 3082 L 26: “0 Ma” instead of “000 Ma”

P 3083 L 1: Divins (2003) has been outdated since an update was published 2013.

P 3083 L 6 and L 12: How was the “resampling” (instead of “re-gridding”) done?

Section 3

P 3084 Eq 2: The use of m as counting index is odd. Use i instead.

P 3084 L 15 to L 18: Spell out the values of the mentioned constants.

P 3085 L 10 to L 14: These two sentences are hard to understand. This is partly because the figure references could be put in better places. I also wonder what “multicomponent” refers to here. Here is a suggestion for re-phrasing this section (which would eventually also require a re-ordering of Figs. 2-5):

On top of the depth-to-basement $w\tau$ (Fig. 4), a parameterized sediment layer was isostatically added to complete the open ocean bathymetry. The OESbathy sediment thickness was parameterized based on a third degree polynomial fit (Fig. 2) between area corrected global sediment thickness data (Whittaker, 2013) and age of the underlying oceanic crust τ . The resulting global sediment thickness is shown in Fig. 3, and Fig. 5 shows the result of adding this sediment layer isostatically to the basement depth.

P 3085 L 13 and Fig. 2: It remains unclear why this polynomial line fits the data best. From the figure, a linear regression seems to fit the data points equally well (or badly). The data also suggests that the regression line should not (almost) pass through the origin, and that it heavily underestimates sediment thickness for $\tau > 160$ Ma. The modeled sediment layer is a factor for the quality of the overall model, so the assumptions it is based on and its limitations should be explained very clearly.

P 3086 L 15 to 16 and Eq 8d: Please explain why geometric relationships between l_{sh} and $l_{sl} + l_r$ as well as between l_{sl} and l_r should be assumed overall. The scattered data in Figs. 8B and 8C does not imply a strong correlation between these lengths.

P 3087 L 1: To my eye the curves fitted to the data in Figs. 8B and 8C are very speculative. As the results show that the model’s shortcomings are mostly in the reconstructed shelf-slope-rise structures, I believe that the data in Figs. 8B and 8C primarily shows that the observed reality is much more complex than a simple geometric relationship. Maybe one could argue that there is a (linear?)

relationship in Fig. 8B. But the point cloud in Fig. 8C probably shows that one at least should not distinguish between continental slope and rise when modeling passive margins in the here presented way (i.e. rather assume that $P = M$ everywhere).

P 3087 L 8: The statement that “the methodology works well...” is adventurous, considering the fact that even the passive margins that the authors believe are modeled correctly often feature errors of 1000 m or more. Unfortunately I_{sh} and I_{sl} is not plotted on any of the axes in Fig. 8, so the author’s reasoning about “anomalously” wide or narrow shelves cannot be verified. I believe that these problems are a clear sign that nature is more complex than what can be modeled with these simple relationships, or what could be classed as “normal” and “abnormal”.

Section 4

P 3087 L 17 and P3088 L 13: Swapping the order of subsections 4.1 and 4.2 should be considered.

P 3088 L 4: The number “-0.003” should be “-0.004” according to Fig. S5.

P 3088 L 10: There is no Fig. S4c. Probably the authors refer to Fig. S4 set 3.

P 3088 L 8-12: If the “anomalous” point originating from the Newfoundland shelf was removed in Fig. 8b, a much less steep linear regression would fit the data better than the than the polynomial.

P 3089 L 10: It would be interesting to know if the authors have made any (unsuccessful) attempts to also model active margins, and why they chose not to reconstruct trenches in the presented model.

P 3089 L 12: I believe that standard deviation is used incorrectly in this context. Strictly, standard deviation is a measure of the width of a normal distribution of samples (Gauss curve). Global bathymetry is not normally distributed and a hypsogram is definitely not a Gauss curve. Therefore, a calculated standard deviation for global ocean bathymetry mathematically does not have a meaning. The authors should use a more appropriate statistical measure for the spread of the data.

P 3090 L 5: Refer to “Sect. 3.3” instead of “Sect. 3”.

P 3090 L 26: Maybe “profiles” would be a better word than “lines”.

P 3091 L 1: Figures are labeled with upper-case letters, while references are lower-case.

P 3091 L 20: Is “hyper-extended shelf” a commonly used phrase? Otherwise a neutral formulation such as “because our parameterization fails to model this extremely wide shelf” would be more suitable.

P 3091 L 25: “enormous layers” instead of “an enormous pile”

Section 5

P 3092 L 21-22: Please explain further why extrapolation back in time produces narrowing of the shelf-slope-rise structures.

P 3092 L 25: It is unclear what “far field” means in this context.

Section 5.2: Apart from the last paragraph, nothing in this long section is about OESbathy.

P 3093 L 2: “reconstructions far back in time” instead of “deep time reconstructions”

P 3093 L 12: It is unclear which map the authors refer to, Fig. 12b or Mueller et.al. Fig. 11. Neither of them has an unsaturated color scale.

Section 6

P 3095 L 8: Is the shelf-slope-rise reconstruction method really “well established”?

P 3095 L 13-14: See comment above regarding the use of standard deviation.

References

P 3097 L 1 and L 20: Resources from the internet should have a complete web address and access date. Also, the web address in Fig. S3 should point at the original source, not a download site where the data set is mirrored (<http://www.ngdc.gov/mgg/sedthick/sedthick.html> and <http://www.ngdc.gov/mgg/sedthick/sedthick.html>, respectively).

Figures

Fig. 7: Label the sub-figures in the same way as in Fig. S4 (i.e. Set 4, Set 15 and Set 17).

Figs. 7 and S4: The many and colorful gridlines are disturbing. What is the color scale for the background data in the middle panels?

Fig. 8A: Could be included in or merged with Fig. 1

Fig. 11: Highlight the labels in the map (e.g. with bold font) for the profiles shown in the lower part of the figure. Otherwise they are very hard to find. I also wonder how the profiles are ordered; maybe there is a more intuitive order that would make it easier to jump between the profiles and the map.

Fig. 12: Color scale says “Distance”, should be “Depth”.

General use of units: Mathematically correct is to write e.g. “Depth / m”, so that the plotted number becomes dimensionless. “Depth, m” or “Depth (m)” is unfortunately common, but not quite correct.

See also the general comments about the figures under presentation quality.

Supplementary material

Table S1: The SI unit for density is “kg m⁻³”, not gram-meter per light speed squared ;-)

Fig. S3: Use the same color scale as in Fig. 5. Have the color scale start at zero.

Fig. S6: Unclear which data set was subtracted from which one. What about positive values on the color scale? They seem to exist at least in the Pacific Ocean. A positive-neutral-negative color scale would be easier to interpret (see above).

Fig. S8: The color scales should be white for values between 0 and 1000 or 2500, respectively. Fig. S8B does not add any information above what is shown in Fig. S8A.

Fig. S9: Typo “.diagram”. The figure is not explained anywhere in the text, and it is rather complex. Therefore it should be explained better in the caption, e.g. summarizing the general workflow and stating that the numbers refer to the numbers of the scripts in the supplementary material.

Overall evaluation

Principal criteria	Excellent (1)	Good (2)	Fair (3)	Poor (4)
Scientific significance		X		
Scientific quality		x		
Scientific reproducibility	x			
Presentation quality		Text	Figures	