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Interactive Comment

Interactive comment on "OESbathy version 1.0: a method for reconstructing ocean bathymetry with realistic continental shelf-slope-rise structures" *by* A. Goswami et al.

Anonymous Referee #2

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This paper describes a method for reconstructing the bathymetry of the seafloor, using only the age of the seafloor and its proximity to a continental margin (which can be estimate from a tectonic reconstruction model). A method like this could be useful for estimating the bathymetry of the seafloor at past times, which can be useful for developing the boundary conditions for paleo-climate or paleo-oceanographic models. The authors give several examples (in the introduction), where the topography of the seafloor affects ocean circulation, and is therefore of interest to paleoclimate studies.

The method presented is extremely simple, and consists of two parts. In the first part, they estimate the depth of the open ocean from the age of the seafloor. In the second part, they estimate the bathymetric variations along the edge of the continental shelf.

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They then apply their methods to the present-day seafloor to evaluate the success of their models.

Although the model does a reasonable job of reproducing the topography of the present-day seafloor, I am not convinced that this study represents a significant advancement in "geoscientific model development", for two primary reasons, which I describe briefly below and in more detail later:

First, the "models" developed are merely simple equations – this is not so much an advancement in modeling but instead an application of current understanding of bathymetric variations on the seafloor. For example, the equation they develop for the bathymetry of the open ocean (equation 2) is taken directly from the Turcotte & Schubert 2002 Geodynamics textbook. The sediment correction (section 3.2) is expressed merely as a function of seafloor age, whereas previous authors have developed more sophisticated expressions that take into account latitude or basin-specific variations (see below). The expression for the shelf-slope rise (equation 8) is simply an empirical exercise in slope-fitting for the present-day shelf structures. This is seemingly the newest part of the work, but the authors do not present anything particularly sophisticated – it is just an empirical analysis.

This brings up a second concern: A study like this is only useful if it can be usefully applied to other studies. I don't think that is the case here. In a sense, the models are so simple that they could be developed "on they fly" as part of a larger study on an application of a model like this. Indeed this is already true for the open ocean – there is a long history of various authors developing expressions for seafloor bathymetry as a function of age, and Muller et al 2008 and Conrad 2013 have already applied such expressions for the geologic past. For the continental shelf, I am not sure that the model presented here is particularly useful, but because the model (equation 8) does not take into account any local knowledge of the continental shelf (e.g., presence of a sediment source, whether it is a passive or active margin). This part of the study would mostly only be useful for estimating the shelf topography of a particular margin (instead

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of an average over all margins) – and thus some information about local geology would be available and useful for any practical uses. It would be better to develop a region-specific analysis for different types of margins (sediment-rich vs. sediment starved, active vs. passive), and then apply the correct one as necessary when it is necessary to reconstruct the bathymetry of a margin. In short, I don't see enough here that couldn't be done better on a case-by-case basis in individual regional studies.

I have more specific comments below. I am recommending reject for the paper because I don't see enough "model development" to merit a new publication. Possibly if the authors did much new work to make their sediment and shelf-slope models much more sophisticated (for example, by taking into account sedimentation processes), then they might consider resubmission – but this would be a different type of study than what is presented here. I would be much more excited to see these expressions developed as part of a paper about the eventual application of these models to a geologic problem.

Specific Comments:

page 6, line 12 – the authors are citing an outdated version of Turcotte and Schubert (2002). It would be better to cite equation 4.211 of the 2014 version of this book.

Page 6, line 22 – the authors choose omega_e = 5875 m as the midpoint of the range of the oldest part of the Pacific. This seems rather arbitrary - why did they choose this? Why exclude the oldest Atlantic, which is also about 180 Myr old? It would be even better to find the best-fitting value for this parameter from empirical fits to the seafloor. Also, shouldn't the authors subtract the sediments from this old seafloor before estimating a value for omega_e – because they are handling the sediment contribution separately.

Page 6, line 8 - Similarly, the choice of omega_0=2639.8 m is taken unquestioned from Crosby et al 2006 – is this the best value? Again, it seems that it would be better to invert for these parameters, rather than just assign them – but there have been many studies over the years that discuss this problem.

Page 7, line 14 – sometimes the authors use Whittaker 2013 and sometimes they use Divins 2003 (e.g., page 8, line 10) for the sediment thickness constraints. As I understand it, Whittaker 2013 is an update to Divins 2003 – so shouldn't they just use Whittaker 2013?

Figure 3 – the authors fit a cubic equation to the sediment thickness vs. seafloor age data. The terms for the cubic and quadratic parts of this equation are pretty small, so really this is mostly a linear fit – and there is still significant scatter of the actual data about this relation: many of the data points deviate from the expression by 1-2 km! However, other authors have used a more sophisticated expression that encapsulates some of the spatial variations in sediment deposition: Muller et al (Science, 319, 1357-1362, 2008 – note that this is a different Muller et al 2008 paper than is cited in this references) used both seafloor age and latitude as an expression for sediment thickness – this takes into account latitudinal variations in bioproductivity that contribute to sediment thickness variations. Conrad (GSA Bulletin, 125, 1027-1052, 2013) showed that a different expression for sediment thickness as a function of age applies for the Pacific basin than for the Atlantic and Indian basins. Both latitude and basin-type differentiations are easy to make, and would give a better estimate for sediment thickness in any particular part of the ocean.

Page 8, line 25 – both M and P are measured from the coastline. But the position of the coastline is extremely sensitive to sea level – so I would think that in some locations the values for M and P could change significantly with time even if the slope itself didn't change – if there was a significant rise or fall of sea level. Indeed, I would think that if the authors performed this same analysis only 20,000 years ago, when sea level was 120 m lower, they would find a very different answer! So why should we expect an expression that is calibrated for this particular moment in time to also apply for the geologic past?

Page 10, line 20 – why do the mid-ocean ridge systems have a different average depth (2675 m) than omega_0=2639.8 m, which is assigned on page 6?

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Page 11, section 4.3.1 – the authors describe how their method reproduces the current seafloor better than EB08. Is this due to a better expression for sediment thickness or a better expression for seafloor depth?

Page 12, line 19 – The authors use the rather subjective term of "a good fit" to describe the comparison between predicted and observed sediment thicknesses. It would be better to use more quantitative terminology (average deviation of xx m). Some of the fits are indeed not so "good", as noted (some areas off by more than 1 km).

Section 4.3.3 – I wonder how useful these shelf-slope profiles actually are in practice? The authors show a comparison of observed and predicted slope profiles in Fig. 11, and many of the comparisons are very different - several km different in ocean depth in any one location. How useful is it to make an automated prediction of a shelf topography that is so different from the actual one, even for the present day? For the past, I would think it would be more useful to make an educated estimate that is based on any local geological observations that can be made about that margin. For example, simply including whether the margin is passive or active would give a better prediction of its topography.

Figure 11 – among the comparisons of bathymetry and model predictions for the different continental slopes, only passive margins are shown in the figure. It would be good to include some active margin comparisons in the figure.

Section 5 – one question for the discussion: Sea level is thought to have been higher in the past – this would make the seafloor even deeper than the values in the equations presented here. It seems to me that this contribution (the offset of average sea level from present-day sea level) should be included in the full expression for seafloor depth.

Page 14, line 27 – the authors say that rifting of continental margins is not easy to parameterize. In addition to those mentioned here I would add Kirschner et al. [Tectonics, vol 29, TC4006, 2010] as another study that has developed a modeling approach to looking at the time history of passive margins.

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Figures: It seems to me that there are too many figures given the amount of material presented. Figures 3, 4, 5, and 9 are somewhat redundant – I don't know how useful it is to show all four. Similarly, Figures 6 and 7 seem somewhat redundant.

Figure 10 – The colors in this figure mask many of the deviations between the model and the observed bathymetry. Much of the map is green/yellow – but it is impossible to tell whether these deviations represent only $\sim$100 m deviations or 1000 m deviations. I recommend scaling the colormap so that more detail is visible in the +/- hundreds of m range, and less in the +/- thousands of m range.

Figures 12– This shows observed bathymetry with the sediment thickness model subtracted (in a) and the age-depth model subtracted (in b). I don't think this figure is necessary – it seems to me that part (b) is showing something very similar to Figure 10 (except for continental margins, these two figures should be the same I think)

Figure 13 – This figure is also seemingly not useful. It shows the modeled bathymetry with the components of the model subtracted from it (sediments removed in part a and also age-depth relation removed from part b). Because the only two components of the model have been removed from the model for the open ocean in part (b) – then I would expect the remaining variations to be zero. They seem close to zero but not exactly – why not?

Interactive comment on Geosci. Model Dev. Discuss., 8, 3079, 2015.