Review of Solid Earth submission doi:10.5194/sed-5-2419-2013 by Steffen, Wu and Wang titled: **"Optimal locations of sea-level indicators in glacial isostatic adjustment investigations"**

General statements: This paper addresses a very important problem: How will we begin to address the difficult question of lateral variability in mantle viscosity as it may be reflected in the various data sets that have been analyzed since the era when radiocarbon and other dating methods allowed the great data sets that constrain paleo-coastal shore lines and other indicators of past positions of the high tide wave action against the coastal land environment. The authors have take a step, albeit, a very small one, toward examining the tradeoffs of realistic rheological lateral heterogeneity model with realistic data that have here-to-fore constrained only the radial variability in mantle viscosity. For attacking this problem head-on, and in a straightforward way, the authors are to be applauded.

Well, that is the good news. Now the bad news is that this paper suffers from being rather terribly written. If it were not for the good organization of the work performed, and its logical steps to take, my over all disposition toward recommending this paper might differ considerably.

I think that the authors simply have not taken enough time to read their own paper. For example, in the Conclusions (section 6): "We have ... data to four parameters which are important in GIA modelling: ice-load history, lateral lithospheric thickness variations, background viscosity, and lateral lithospheric thickness variations."

So I ask the authors: What is wrong with this sentence?

When I read things like this as a reviewer I have to ask myself: How much time did the three authors really put into this work? And then I have to wonder if the writing was this lack-luster, what about the research itself? I could go on, but I not really going to get anywhere by tossing them further vitriol.

I think the paper can go forward, but only with the following attended to.

1. Completely rewrite the Conclusion section – highlighting only the main discoveries from the research.

1.a For example, the 6 items in the bullet list are all known to be important by every investigator in the field of GIA and RSL. There simply is no value, and no new information, delivered to the community here.

1.b The paragraph that follows the bullet list: lines 18-25, page 2435 and continuing in the remainder of the paragraph can (and should be) condensed into two sentences, capturing only the main points.

1.c Now in the remaining parts of the Conclusion section is where the real 'beef' of the results -> conclusions in this paper, at least in this reviewers opinion. The point about what can be revealed in more accurate data is very interesting, and possibly the main reason that this paper should be published (in a shortened form). To obtain this type of accuracy may be quite a tough challenge. With relative sea-level so much lower (higher) the coastal environment where the splash zone exists is different – and this brings in a whole host of new questions that coastal geomorphologists and ocean scientists must tackle. A little more emphasis should be placed on this point.

1.d The remainder of the Conclusions need not be altered, albeit it could be cleaned up some. A suggestion to the 1st author for self-editing: Re-read and try to eliminate unnecessary words/phrases. Such as: "In turn, more than 14,000 …" -> "More than 14,000 …". Another example: consider the following 56-word, 308-character paragraph:

>"In view of our investigation, new RSL data should be searched around the world. It is clear that the focus has always been set on coastal areas. However, our sensitivity maps show that the deep sea and many areas on land with lakes have high sensitivities and any sea-level indicator found there can be of help."

With the 35-word, 230-character paragraph:

>"Our sensitivity study suggests the value of collecting and interpreting RSL data in coastal areas that are surrounded by deeper ocean and that non-marine fresh water lakes also provide valuable new information to constrain models."

Doing this is just a vehicle for getting to more compact and readable scientific writing. It takes iteration. And it does not have to end in "The King's English", so to speak.

2. The Discussion section reads better and is well-organized. However, there is some repetitiveness with respect to the Results section. I ask the authors to consider condensing and examining for redundancy. Try to reduce the total word count from 1,030 to 800.

3. The paleo-environmental community might read this with a touch greater gusto and acceptance if you were to adopt: ka (calendar years before 1950) in place of kaBP. (see Alley et al., Holocene climatic instability: A prominent, widespread event 8200 yr ago, Geology, 25(6), 483-486, 1997. – cited over 1,270 times!)