Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-79-RC1, 2018

Solid Earth
Discussions
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Interactive comment on "Integration of geological uncertainty into geophysical inversion by means of local gradient regularization" *by* Jeremie Giraud et al.

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Received and published: 22 September 2018

Dear Jérémie & Co-Authors,

I think the method that you present in this manuscript will be useful, and is correct. However, I'm not sure I agree with the reasoning behind the method and the justification you give for the method. More on this below.

But first, this is a well-written, well-organized (except for one structural issue that I'll mention later) and well-illustrated manuscript. The particular combination of geological modelling and weighted gradient term in a minimum-structure gravity/magnetic inversion is novel, as far as I'm aware.

Okay, my thoughts on the justification of the method and the explanation of why it's working ... I really like the synthetic example given in Appendix A, especially the illustrations in Figures A4 & A5. Putting aside for a moment how W_H (shown in Figure A4b) was generated, it is exactly the case that if one weights the gradient (roughness) term in a minimum-structure term with the spatially varying weights shown in Figure A4b, then the gravity (or magnetic) inversion will construct a model for which the gradient is concentrated in the locations where W_H is small. The gravity (or magnetic) inversion is sufficiently non-unique that the data are quite happy for the gradients in the model to be put where W_H is small: the data will essentially never have a strong enough influence to overcome this effect of W_H.

This weighting of the gradient term is a bit like the weighting (well, iterative re-weighting) of the gradient term in an IRLS approach to minimize an L1 measure of roughness rather than an L2 measure of roughness. And the whole point of using an L1 measure of roughness is to get sharp interfaces between mostly uniform regions. The method you present here is kind of like asking for a sharp, L1-type interface (gradient of the model), and that this interface is located where W_H is small, i.e., you're specifying where you want this sharp interface.

I don't have any problem with this process as such. However, I'm uneasy with the connection between the regions of low W_H and your quantification of geological uncertainty. Okay, the process you create, which uses geological uncertainty to locate the low values of W_H, works. But this is because these areas of geological uncertainty (happen to?) correspond to where the boundaries between the geological units are: it's not the geological uncertainty that's the true, fundamental piece of information, it's that this uncertainty in the geological modelling is indicative of a boundary between units, and it's this estimated location of the boundary between units that becomes the key information to provide to the gravity (or magnetic) inversion via the low values of W_H.

What if you were to consider a synthetic example in which there is essentially zero uncertainty in the location of the interfaces. And make a W_H that's pretty much 1 everywhere except zero for the cells straddling the interfaces. I'd expect the gravity inversion would give a nice density model that pretty much has sharp interfaces right where the geological model has it's interfaces. If you then broaden up the zones of low values in W_H, I'd expect the boundaries in the density model to pretty much stay in the same location but now start to be smeared out and more like an inversion result for constant W_H. If you have a broad region of constant low W_H, the constructed density will be smeared out and smoothly varying through here, it won'd be sharp at one end or the other. And if you have the same true synthetic model but try putting the low values of W_H in the incorrect locations (i.e., not where the interfaces in the true model are) then the constructed density model is going to have it's interfaces (sharp or diffuse depending on whether W_H is sharp or diffuse) pretty much where the lows in W_H are, not where the boundaries are in the true model. (Have you tried such a suite of examples?)

So, yes, using spatially variable weights in the roughness term results in the interfaces in the density (or susceptibility) models occurring where you'd like them to occur. However, I don't agree with the thoughts that the gravity (magnetic) inversion is helping out, or overcoming, the geological uncertainty; rather, the uncertainty is mapping out parts of the subsurface on and close to the interfaces, but that it's simply this (fuzzy) location of the interfaces that you're using to tell the gravity (or magnetic) inversion where to put the boundaries in the density (or susceptibility) model.

The above is my main issue with this manuscript: the justification and motivation, not the mechanics of the workflow itself.

Some further comments ...

I think there has to be a typo in equation 3: You've got "max H - max H" on the denominator, i.e., a difference between identical things.

In equation 1, what are you using the prior model for? This isn't a "starting" model, is it? (You say the starting and prior are the same thing on page 7, and use "starting model", without a mention of the prior model, in the caption to Figure 2.) The m_p in equation 1 is important, as the inversion is going to try to construct a model that is close as possible to m_p (which is the whole point of that second term on the right-hand side of equation 1). This is a linear inverse problem, so it won't matter at all if one uses a starting model and then solves for the model update (or just solves directly for the model from the observed data). So a "starting" model should have no influence in the inversion. I'd definitely not use the term "starting" at all, and be careful to always use "prior" when thinking about the m_p in equation 1.

Do you now use a trade-off parameter for the "model term" in equation 1? Maybe you do in the code but it's simply been omitted from this equation when writing this manuscript?

Figure 2 and Figures 3 a & b are fine to show the whole model. But it's hard to make out the details in the parts of the model around features A, B & C. It's therefore hard to assess how much of a difference the "locally conditioned regularization" has made. I think it would be good and important to also show zoomed-in sections through the parts of the model around A, B & C.

Finally, the structural comment: I really like the example in Appendix A. I think that should come in the body of the manuscript, between sections 2 & 3 (perhaps with a description of the geological modelling process, and the process of determining the "geological uncertainty").

Best wishes, Colin Farquharson.

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