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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.

Anonymous Referee #2

Received and published: 8 October 2018

Dear authors,

I think your paper is generally suitable for Solid Earth, and it is already very well written. However, there are several points where the ms needs to be improved. In particular the title and most of the rest of the ms seems to indicate that you add geological information only in the uncertainty guided inversion. Clearly, this would give additional information only for the surface structures. However, in the field example, you have used information from various geoscientific disciplines, which also add information at depth. This should be corrected throughout the ms. Furthermore, the synthetic example in the appendix is too difficult to understand with the limited information given.

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You find more detailed comments below!

Best wishes,
Anonymous Reviewer

Specific comments:

- Section 2.2: Geological models have natural limits. Unless boreholes are available, geological observations are limited to mapping at the surface. Even though dip angles of layer interfaces measured at the surface may lead to assumptions about the depth of the interface at a given lateral offset, there is pretty poor control on this. The layer interface may not have linear depth variation, but be undulating. I recommend a general discussion of the shortcomings of geological models in terms of their uncertainties at depth.

- The synthetic example in Appendix A1 raises a number of questions and does not seem to work along the lines reported earlier on in the ms. Is the reference model in Fig. 4a your true model? Is it also used as the prior model m_P in eq. 1? I guess not but in an inversion context reference and prior models are basically the same. The reader would have assumed a synthetic gravity model and independent geological information (mostly at surface cells and not so much at depth, see above). Instead the matrix W_H is derived from the reference model itself, also at depth. I agree that this is helpful in showing the basic functionality of the method, bu this does not really helpful in showing the limits of the method.

- The gravity data set is limited to the NE by a fault, meaning there may be a significant density contrast right at the border of the measurement area. A comment on possible improvements in model constraints by extending the measurement area to the NE seems advisable.

- Section 3.1, p.6, l.10: Your data set is not a geological one, but a more general geoscientific one, as it includes geophysical and spectometry data. At least, I guess that the Landsat 8 and ASTER data are spectometry data and this should be mentioned clearly. So, it becomes clear here that you put much more into the matrix W_H and into the reference (prior) model than what would ever be available just from geological data. Hence, the title is directly misleading. It indicates more limited scope and applicability than what you present in the paper. Please, replace “geological uncertainty” by “geoscientific uncertainty” in the title! I think this will also generate a larger exposition for your paper

- Since you include information from various geoscientific disciplines, it would be meaningful to add a larger paragraph and figures that describe what contribution the various methods make to W_H.

- Fig. 2b: There is structure in the W_H matrix in volumes where the density contrast is zero, e.g. in the SW corner of the model and $7 * 10^5$ m E and $7.12 * 10^5$ m W (small green blob). Please explain where these anomalies in W_H come from.

- Conclusions, p. 13, l.5-10: Please add a careful discussion as to whether the fact that you see predominantly the shallower part of structures A and C is a result of too little depth weighting in the inversion (e.g. Li and Oldenburg, 1996; Kamm et al., 2015).

Technical corrections:

- p.3, l.1: “In the methodology section,”

- eq. 1: Wouldn’t you usually want to have another scalar factor on the model term to test different weighting of the various terms in eq. 1? Also, I wonder whether the model would not be very rough, if a diagonal W_m was used and W_H was set to zero in large parts of the mesh.

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- eq. 2: Please provide more reasoning for this equation, in particular the log transform.
- eq. 3: Please correct this equation. As it is reproduced now, the denominator would always be zero and the argumentation in the paragraph containing eq. 3 cannot be understood.
- Fig. 1: Labels A, B and C as well as the dashed lies are supposed to indicated three greenstone belts. However, there are only two dashed lines, leading to confusion on where A and B are. This needs to be corrected.
- p.5, l.27: Please, remove “Inverted”.
- p.7, l.2: “the PGM is displayed”.
- p.9, l.:19-21: Kalscheuer et al. (2015) would be another suitable reference.
- p.9, l.30: “the two compared cases”
- Fig. 3: Consider replacing $\delta\|\Delta m\|^2$ by $\|\delta m\|^2$
- p.12, l.19: “The interpretation of the inversion results also reveals that greenstone B”
- p.12, l.27: “mitigates the non-uniqueness of the inversion”
- p.13, l.31: “proof of concept”
- p.14, l.13: “is shown in”
- Please define the various RMSEs based on eq. 1.
- p.15, l.15: “the general features”
- p.16, l.20: “(i.e., first line) and (e)-(h) (i.e., second line)”

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References

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J. Kamm, I. A. Lundin, M. Bastani, M. Sadeghi, and L. B. Pedersen. Joint inversion of gravity, magnetic, and petrophysical data – A case study from a gabbro intrusion in Boden, Sweden. *Geophysics*, 80(5):B131–B152, 2015.

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