Editor
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Estimation of the total magnetization direction of approximately spherical bodies
V. C. Oliveira Jr., D. P. Sales, V. C. F. Barbosa, and L. Uieda

First Contact: Dr. Vanderlei Oliveira Jr., vandscoelho@gmail.com
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Handling Editor: Richard Gloaguen, gloaguen@geo.tu-freiberg.de
Handling Executive Editor: Dr. Olivier Talagrand, talagrand@lmd.ens.fr
Manuscript Type: Research Article

Status: Referee Reports (NPG) **Iteration: Revised Submission**

Revised Submission

Editor Decision: Publish subject to minor revisions (further review by Editor) (10 Mar 2015) by Richard Gloaguen
Comments to the Author:
The paper has been improved but need further amendments.

1- Please include the additional tests performed, maybe as supplementary meterial. The additional tests are extremely helpful to judge the potential of applying the presented method.
2 The introduction on magnetic inverse methods should be shortened.
3- Clearly indicate the novel aspects of your study and make sure to differentiate known techniques (e.g. L. R. Pasion)
4- Clearly describe the frame of usability of your approach (e.g. the cases for which there is a use, the cases for which your approach is not adequate).
5- Please implement the critics raised by both reviewers during the last round.

Non-public comments to the Author:
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General comments to the authors:
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I feel I must begin with a brief apology to the authors. I was under the impression that this would be a more open back-and-forth discussion of your work, with more chance for short responses to allow clarification. As such, my original review comments were brief and intended to spur collegial discussion rather than more heated discourse. My comments below will be more detailed to avoid any misunderstandings.

To summarize, I think this is a well thought through manuscript, with good synthetic work to validate the methods and assess their advantages and limitations. While the synthetic tests are well thought through and provide much information on the behaviour of the proposed method, I do not think they are sufficient. Unfortunately there are some similarities with other works that need to be addressed. My other major concern is with respect to application to real data examples. My comments below will speak to these major concerns.

However, I should first clarify some of my previous points as I do not think you have understood my meanings or intent entirely. My comments regarding the similarity of your methods to those of Lelievre & Oldenburg (2009) and Ellis et al. (2012) were with respect to the actual numerical problem being solved. Of course the intended practical application of both approaches are wholly different are you have done a good job explaining this to me. You are solving a numerical problem that is essentially identical to what Lelievre & Oldenburg (2009) and Ellis et al. (2012) would end up with were they to remove many cells from their meshes, and thereby obtain a very small full sensitivity matrix with three rows for each remaining cell (equivalent to your separated dipole sources) and also remove all regularization from the problem. This comment is perhaps too academic and abstract to be completely fair. What I was trying to get at was that it seems out-of-place for you to be using similar numerical solution algorithms that are also used for much larger numerical problems of a similar numerical nature. You are dealing with much smaller problems so why not apply global optimization methods that can provide likelihood information? Please address this in your manuscript.

It is important that you mention the work below and other work in the field of UXO detection:
L. R. Pasion. Inversion of time-domain electromagnetic data for the detection of unexploded ordnance. PhD thesis, University of British Columbia, 2007.
In that work, Pasion inverts magnetic data for a best fitting dipole, the parameters being the location (x,y,z), the magnetization (mx,my,mz) and a dc offset for the data. This makes it a more complicated problem than what you are attempting to solve, and makes your problem a simplification of his. Therefore, what you need to do in your manuscript is compare your methods to his, or other work like his, and explicitly indicate what you are doing that is new or different. What I see as being different in your work is that you are supplying the location information as prior information to the inversion. I think it is critical that you compare against the already published work of Pasion.

I still must hold fast to my comments regarding the applicability of your methods. Yes, they are more computationally feasible than a fine mesh-based inversion for a discretization of magnetization. However, there are clearly assumptions that must be made about the subsurface in order to apply your methods. You say I am confusing assumed premise and prior information. No, they are closely related. Any assumed premise in your inversion methods means they can only be safely applied to problems in which that assumed premis holds. For example, a linear magnetic inversion that assumes low susceptibilities is not applicable to a magnetic problem in which one knows there are significant high susceptibilities and non-linear self-demagnetization effects. If you are going to apply any inversion methods to a particular problem then you must have sufficient a priori information to allow application of those methods. However, I do take your arguments into consideration and allow that it is sometimes acceptable to apply an inversion method where the assumed premise fails, but one must be asking appropriate exploration questions. For example, you mention that you can apply your methods to any shape of body provided you upward continue the data enough such that the response becomes more dipole-like. However, that approach can only provide a single magnetization direction within that body, so this is really only appropriate if the exploration question is to determine the average magnetization direction within a body. I think you should mention that in your conclusions.

You must also have a reliable estimate of the number of sources and their locations (lateral and depth). There is little in your manuscript that attends to the former problem (how many sources exist). Other researchers have investigated problems in which they attempt to determine the number of dipole sources or fit several dipole bodies to overlapping data responses. Here are two relevant references:

S. D. Billings and F. Herrmann. Automatic detection of position and depth of potential UXO using continuous wavelet transforms. In Proceedings of SPIE, Detection and Remediation Technologies for Mines and Minelike Targets VII, 2003.

Song, L., L. R. Pasion, S. D. Billings, and D. W. Oldenburg, 2011, Nonlinear inversion for multiple objects in transient electromagnetic induction sensing of unexploded ordnance: Techniques and applications: IEEE Trans. on Geoscience and Remote Sensing, 49, no. 10, 4007-4020.

I don't think it is enough to simply apply your methods to a real data set. To clarify your arguments for the applicability of your methods, you will need to provide more details of this case study. Specifically:
1) include the exploration question that you are trying to answer using your methods;
2) include the a priori information that allows your methods to be applied, or validate the applicability of your methods here by performing a more thorough investigation of how high you should be upward continuing this real data to safely apply your methods.

Specific comments to the authors:

Line 115:
"This author stresses that ..."
- It is unclear who you are talking about. Please use "We stress that ..." or "Phillips stresses that ...".

Line 134:
You may want to point out that the approach taken by Lelievre et al. and Ellis et al. involves a highly nonunique inverse problem and it is critically important to constrain such inversions to reduce the number of acceptable solutions and obtain usable results. This is an important drawback of using such a flexible approach. In contrast, your approach makes heavy assumptions about the underlying sources and is able to reduce the nonuniqueness of the problem to a point that regularization or constraints are not required.

Line 204:
"In general, the total-field anomaly is produced by a magnetized susceptibility distribution which is anomalous with respect to the mean susceptibility of the crust."
- This statement could be construed to ignore remanent magnetization. It would be more accurate to say something like this: "In general, the total-field anomaly is produced by a distribution of magnetization which is anomalous with respect to the mean induced magnetization of the crust".

The forward problem described in section 2.1 is numerically similar to a mesh-based discretization but with the mesh cells (prisms, tetrahedra, etc) replaced with spherical (dipole) sources. What I mean to say is that you have a simple linear multiplication of a full matrix by a vector. Hence, this material could be reduced making use of citations to similar work by other authors.

Line 297:
It would be helpful if you explicitly stated, both here and in the abstract, that you are solving an overdetermined inverse problem, i.e. there are fewer dipole sources than there are data observations, 3L<
Line 361:
You may want to reference the work of Colin Farquharson on general norms here, because he also applied an L1-type measure to the data misfit term and solved iteratively using IRLS.
Farquharson, C.G., and D.W. Oldenburg, 1998. Nonlinear inversion using general measures of data misfit and model structure, Geophysical Journal International, 134, 213-227.
http://webmail2.eos.ubc.ca/sites/default/files/Farquharson_1998.pdf

Line 365:
"The magnetization vectors are represented in Cartesian coordinates, however they are commonly represented in terms of its intensity, declination and inclination."
- Please provide references in which the magnetization vector is represented by parameters in spherical coordinate systems, and indicate the advantages or disadvantages of such an approach versus using a Cartesian representation.
- Using a spherical framework introduces additional nonlinearity into the problem. How does this affect convergence of the iterative inverse solution? Is convergence guaranteed? Are multiple minima introduced and, if so, are they problematic? Does this suggest that a global optimization strategy should be preferred? Please address these questions in your manuscript.

Section 3.2 Robustness against interfering anomalies.
- I would like to see a test where the assumption of dipole source is still honoured but the two (or more) dipole responses are significantly overlapping. This is what I was expecting when you refer to "interfering anomalies". I think some further research needs to be made into the behaviour of your methods under such a situation.

Section 3.4 Robustness against errors in the centre location.
- This is a nice test. However, I'd like to see how well your proposed procedure works here: applying the Euler deconvolution technique to assess the sphere location. Yes, it is of course important to perform the simpler tests with more controls on the variables, but you should also be using the synthetics to demonstrate the behaviour of your proposed procedure for

various data characteristics. What is the location of the sphere calculated through the Euler deconvolution technique and how far is it away from the true location? Please include this information in your manuscript.

Technical corrections:

Please fix the following grammatical errors. There may be others.

Line 25:
"even for other region of the GAP"

Line 27:
"the non-outcropping sources near from the alkaline complex"

Line 28:
"the same magnetization direction of that ones in the alkaline complex"

Line 70:
"and then better defining exploration targets"

Line 105:
"Although this method does not strongly constraint the source's shape"

Line 384:
"are equal to that ones of the magnetization vectors"

Referee Nomination & Report Request started (03 Feb 2015) by Richard Gloaguen
Minimum Number of Reports Required: 3

Referee #2: Ebbing, Jörg jebbing@geophysik.uni-kiel.de
nominated 03 Feb 2015, accepted 09 Feb 2015, report 22 Feb 2015

Referee #1: Lelièvre, Peter plelievre@mun.ca
nominated 03 Feb 2015, accepted 03 Feb 2015, report 03 Feb 2015

Nominated Referee
nominated 03 Feb 2015, missed nomination deadline

Accepted Referee
nominated 18 Feb 2015, accepted 18 Feb 2015

Editor Initial Decision: Reconsider after major revisions (further review by Editor and Referees) (05 Jan 2015) by Richard Gloaguen
Comments to the Author:
I praise the authors for their thorough corrections and I am sure the referees will appreciate your efforts. On the other hands the critics were fundamental and numerous and I require a new round of discussion. Additionally, one referee requested the opportunity to clarify some of his comments

Editor Initial Decision: Reconsider after major revisions (further review by Editor and Referees) (18 Dec 2014) by Richard Gloaguen
Comments to the Author:
I praise the authors for their thorough corrections and I am sure the referees will appreciate your efforts. On the other hands the critics were fundamental and numerous and I require a

new round of discussion. Additionally, one referee requested the opportunity to clarify some of his comments.

Uploaded Files validated (12 Dec 2014) by Anna Wenzel

File Upload (12 Dec 2014) by Dr. Vanderlei Oliveira Jr. Abstract Manuscript Author's Response

Interactive Discussion

Final Response (01 Nov 2014) waiting for final Author Comment

Invoice paid (19 Sep 2014)

Discussion started (05 Sep 2014), expected end 31 Oct 2014 Interactive Discussion
Minimum number of Referee Reports required: 2

Anonymous Referee #3
nominated 16 Sep 2014, accepted 24 Sep 2014, report 31 Oct 2014 **[Report #3]**

Referee #2: Ebbing, Jörg jebbing@geophysik.uni-kiel.de
nominated 05 Sep 2014, accepted 08 Sep 2014, report 01 Oct 2014 **[Report #2]**

Referee #1: Lelièvre, Peter plelievre@mun.ca
nominated 22 Sep 2014, accepted 24 Sep 2014, report 30 Sep 2014 **[Report #1]**

Nominated Referee
nominated 14 Sep 2014, declined 15 Sep 2014

Nominated Referee
nominated 05 Sep 2014, missed nomination deadline

Nominated Referee
nominated 14 Sep 2014, declined 14 Sep 2014

Nominated Referee
nominated 16 Sep 2014, declined 22 Sep 2014

Nominated Referee
nominated 05 Sep 2014, missed nomination deadline

Nominated Referee
nominated 14 Sep 2014, missed nomination deadline

Nominated Referee
nominated 05 Sep 2014, missed nomination deadline

Initial Submission

Published (05 Sep 2014) NPGD Library

Invoice sent (03 Sep 2014) to Dr. Vanderlei Oliveira Jr.

Final Typesetting Files uploaded (03 Sep 2014) by Dagmar Eikenroth Manuscript Source

Proof-Reading Files uploaded (22 Aug 2014) by Dagmar Eikenroth Manuscript Source

Proof-Reading Files uploaded (18 Aug 2014) by Dagmar Eikenroth Manuscript Source

File Upload (09 Aug 2014) by Dr. Vanderlei Oliveira Jr. Manuscript Text References Figures

Editor Initial Decision: Publish as is (09 Aug 2014) by Richard Gloaguen

Editor found (09 Aug 2014) Richard Gloaguen agreed to serve as Editor

Editor Nomination - Reminder (09 Aug 2014) a reminder was sent to nominated Editors

Editor Nomination (07 Aug 2014) by Executive Editor Dr. Olivier Talagrand

Uploaded Files validated (06 Aug 2014) by Anna Wenzel

iThenticate.com Similarity Report completed (06 Aug 2014) similarities negligible / not found

File Upload (05 Aug 2014) by Dr. Vanderlei Oliveira Jr. Abstract Manuscript

Registered (05 Aug 2014)

Cover Letter (Information for the Editor):
We have developed a fast total-field anomaly inversion to estimate the magnetization direction of multiple sources with approximately spherical shapes and known centres. Applications to synthetic data show the good performance of our method. Applications to real data show that our method is able to estimate geological meaningful magnetization directions.

Suggested Referees: Maurizio Fedi, fedi@unina.it
Walter Medeiros, walter@geofisica.ufrn.br
Afif Saad, afifhsaad@netscape.net
Wladimir Shukowsky, wladimir@iag.usp.br
Fabio Tontini, f.caratori.tontini@gns.cri.nz

First Choice Index Terms: Subject: Predictability, Data Assimilation
Topic: Solid Earth, Continental Surface, Biogeochemistry

Second Choice Index Terms: not specified