

Author's response to the final comments on the manuscript:

Estimation of the total magnetization direction of approximately spherical bodies

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We would like to thank the Editor and the referees for their constructive review.

The last version of the manuscript has been revised. We have incorporated most of corrections recommended by reviewers. Below we comment in detail on the recommendations and questions raised by reviewers.

General comments to the authors:

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.

We will not continue debating the differences between our method and the methods proposed by Lelièvre & Oldenburg (2009) and Ellis et al. (2012). In the last review, we presented an in depth discussion about these differences.

We focus on your question: *“why not apply global optimization methods that can provide likelihood information?”*

Response: Yes, we can use a global optimization method as well as other methods too. We have chosen to follow the optimization methods shown in our manuscript because they work very well. Only in this manuscript we present the results of 202 inversions confirming the efficiency of the used optimization methods.

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.

We disagree with you. In our manuscript, we are inverting total-field anomaly. Rather, in the work in this thesis the author inverts TEM data and magnetic induction. Hence, a comparison of our work with the mentioned Phd Thesis is outside the scope our manuscript.

Although we do not add this reference, we have added a recent paper that presents an outstanding and comprehensive review of methods that determine the remanent and total magnetizations of magnetic sources. This paper is

Clark D. D. (2014) Methods for determining remanent and total magnetisations of magnetic sources – a review, *Exploration Geophysics*, 2014, 45, 271–304, doi: 10.1071/EG14013

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 premise 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.

We fully agree with you. Our approach assumes sources with constant magnetization direction. In our manuscript, this aspect is written in the methodology, tests and in the conclusion sections. In this new version of the manuscript, we also added this information in the abstract section and a new sentence in the conclusion section.

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.

No action was taken. According to the *Manuscript preparation guidelines for authors* (http://www.nonlinear-processes-in-geophysics.net/for_authors/manuscript_preparation.html), *"Papers should make proper and sufficient reference to the relevant formal literature. Informal or so-called 'grey' literature may only be referred to if there is no alternative from the formal literature"*. In our opinion, this reference is not part of *"the relevant formal literature"* and is not accessible to the reader.

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.

Actually, we need to yield the number of sources and their locations. The number of sources is a subjective topic. In our work, the locations of the sources are provided by Euler deconvolution. However, this is not a restriction and the interpreter may use any other method. No action was taken.

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;

All geologists want to answer a geologic question. However, many geologic questions are not related to a petroleum exploration question or mining exploration question.

In our manuscript, we want to know the following geological questions. 1) What are the magnetization directions of the alkaline bodies in the study area? 2) Do these bodies belong to the same alkaline province? 3) Were the non-outcropping alkaline bodies emplaced in the subsurface at the same geological time interval?

We have already answered these questions in the manuscript.

- 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.

We are sorry, but we do not know if we understood your comment.

To validate the result of our Application to field data on the Goiás Alkaline Province, Brazil, we performed a reduction to the pole of the real data. The reduction to the pole was calculated by using the magnetization direction estimated by our method. Notice

that the reduced-to-the-pole anomaly (Figure 16 in the new version) is predominantly positive. This fact strongly suggests that estimated magnetization direction is close to the true one.

Although this estimated magnetization direction was obtained from the inversion of a simple dipolar total-field anomaly (Figure 15 in the new version), this estimate was successfully used to reduce to the pole a complex non-dipolar total-field anomaly on the Goiás Alkaline Province as shown in Figures 17 and 18 in the new version of our manuscript.

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 ...".

Thank you. We have rephrased this sentence.

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.

We fully agree with you. We clarified this point in our manuscript.

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".

Thank you. We have modified this sentence according to your suggestion.

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.

Thank you for your suggestion. We have decided to maintain the description of the forward problem in section 2.1. Although it is similar to other discretizations presented by other authors, in our opinion, the details described in section 2.1 are extremely important to make our work reproducible.

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, $3L <$

Thank you. We have explicitly stated that our method is an overdetermined inverse problem.

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

We agree with you. We added this reference.

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.

We are sorry, but we do not know if we understood your comment.

To our knowledge, almost all the classical references in the geophysical literature (and also that ones in our manuscript) represent the magnetization vector in terms of its intensity, declination and inclination.

- 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.

Thank you for reviewing. The use of a spherical framework would introduce nonlinearity into the problem if the intensity, inclination and declination were the parameters to be estimated. In our method, we estimate the Cartesian coordinates of the vectors \mathbf{h}^i (Eq. 12), resulting in a linear inverse problem. We do not solve an iterative nonlinear inverse problem to estimate the intensity, inclination and declination of the magnetization vectors of the sources. We just represent the least-squares and robust estimates in spherical coordinates for convenience, as pointed out in our manuscript. We have introduced some sentences in order to clarify this point.

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.

Thank you for reviewing. We partially agree with your comment. The section 3.2 of our manuscript shows the results obtained by our method in the presence of an interfering anomaly (Fig. 3b) which mostly affect the positive signals of the original total-field anomaly (Fig. 3a). This interference disturbs the dipolar pattern of the original total-field anomaly.

Besides, the total-field anomaly without interference (Fig. 3a) varies from ~ -1550 nT to ~ 750 nT. The total-field anomaly with interference (Fig. 3b) varies from ~ -1550 nT to ~ 1000 nT, showing that the interference represents $\sim 33\%$ of the positive amplitude of the original total-field anomaly (Fig. 3a). Therefore, the original total-field anomaly and the interference are significantly overlapping.

However, we recognize that it is possible to formulate a more sophisticated test. Taking this into consideration, we included a new synthetic test showing the results obtained by our method in recovering the magnetization direction of synthetic bodies which simulate a more significantly overlapping anomaly.

See Figure 9 and Table 3 in the new version of the manuscript.

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.

Thank you for your suggestion. In light of this, we included a new synthetic test illustrating the performance of our method in recovering the magnetization direction of a more complex source. In this test, we simulated an igneous intrusion formed by a sill which is fed by a vertical pipe. This intrusion is embedded in weakly magnetized sediments that are overlaying a basement which is magnetized by induction, generating a regional anomaly.

See Figures 10-13 and Table 4 in the new version of the manuscript.

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.

Thank you for reviewing. In this subsection, we analyse how the errors in the coordinates of the centre of the source affect the results obtained with our method. The estimated magnetization directions as well as the errors in the location of the source are shown in Figure 7. We did not estimate the location of the sphere through the Euler deconvolution technique because the performance of the Euler deconvolution technique is out of the scope of our work.

Technical corrections:

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

Thank you for reviewing. We have fixed all these errors.

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"

On behalf of all authors