

Reply to the Anonymous Referee #1.

In spite of the apparent negativeness, we thank this colleague for his/her comments which are valuable for us to recognize those points that were not clearly explained in our paper. For convenience, below the referee's main comments will be repeated in *italics*, while our reply will be in normal text, where our proposed additions/changes of the article will be given in red and within inverted commas ("xxxxxxx").

...

The paper in my view presents several weaknesses and therefore is not publishable in a scientific journal. Of course we do not agree and will try to explain why with our point-to-point responses below. However we admit that all points arose by this referee were pertinent and appropriate. However, all points can be considered in the paper inserting proper sentences in the present text.

Two major issues

1- It is not discussed in the paper in what the variation of the surface included in the 32 00 nT isoline, at the surface of the Earth, characterizes the state of the core. This surface area can increase, or decrease, for numerous reasons such as a general decrease of the observed magnetic field strength without an increase of the magnetic field complexity, or by a simple decrease of the dipole field or finally an increase of the field complexity without decrease of the dipole field strength.

(NB Obviously this Referee was speaking about the 32 000 nT isoline and not 32 00 nT as written in the introductive part of this point).

As we say in the manuscript this isoline is representative of the South Atlantic Anomaly (SAA) at the Earth's surface where the geomagnetic field intensity is the lowest than the field over the rest of the globe (see pag. 5118, lines 9-11, pag. 5121, lines 5-7) .

We agree with the fact that SAA increases because of:

- a) a general decrease of the whole field strength
- b) a decrease of the dipole field
- c) an increase of the field complexity.

This is already said in some parts of the paper: for instance, a) is said at pag. 5119, lines 27-28; b) is said at pag. 5119, lines 6-7; c) pag.5120, line 23. Well, all the above points would point to a possible imminent geomagnetic transition: in particular, the first two (a and b) are the typical known ingredients for a possible polarity change. The third means an increase (decrease) of the Shannon Entropy (Information) which has been recently revealed (De Santis et al., 2004) as another important ingredient for a possible polarity change.

However, we recognize that the whole aspect was not sufficiently underlined in the paper so we propose to include the following sentence at pag. 5121, line 11:

“.. surroundings. The origin of the SAA can be either due to a decrease of the whole geomagnetic field strength or of the dipole field, which are two typical ingredients for a possible geomagnetic reversal. Another cause could be an increase of the field complexity, i.e. an increase (decrease) of the corresponding Shannon entropy (information), which has been recently revealed as another important ingredient for a possible polarity change (De Santis et al., 2004; De Santis and Qamili, 2008). Therefore, we could postulate ...”

We think that in this way the paper would be more clear regarding this aspect.

... Numerous other mechanisms can probably be defined that would lead to a change of the surface of the area, however none of these mechanisms give any information on the state of the core, the strength of the magnetic field inside the core, or the vigor of the convection in the liquid outer core. The authors should

therefore demonstrate that the surface of the SAA, as defined by the area included in the 32 000 nT isoline, is a relevant indicator for the state of the core.

We admit that this Earth's surface field isoline is not a perfect proxy of the reverse flux in the outer core, but we expect that its behaviour in time grossly represents the proportion of the reverse flux with respect the normal one in this part of the southern hemisphere. At the present knowledge of the geomagnetic field, we think we could not do it better.

We could insert the above sentence somewhere in the paper where we introduce the SAA (pag. 5121, after line 5) however, we think that this concept can be easily deduced from what already written.

2- One of the important steps in the work presented is fitting functions of times (described in section 2) to the estimated area of the SAA. For this fit to be acceptable a proper estimation of the level of error in these area estimates has to be done. This in turn can lead to an estimation of the accuracy of the extrapolated curves. Data error estimates are not presented in the paper even if this is an essential piece of information that needs to be described in order to assess the validity of the final results.

*In my view, **defining an accurate error budget for the area of the SAA is not possible. Not only one has to find what is the accuracy of the Gauss coefficients (That is, may be, possible for GUFM but probably not for the IGRF/DGRF), but also one has to estimate what are the contributions of the unknown small scales of the magnetic field. One also has to estimate what effect has on the SAA area the regularization process applied for deriving magnetic field models from geomagnetic data.** From Fig 1 it is clear that the fit to the area estimated from the IGRF/DGRF 2005, 2010 is not particularly good, whereas these two models are probably the two most accurate models of the whole IGRF/DGRF series.*

We agree with this referee about the intrinsic difficulty to estimate the error budget: it was for this reason that we considered only statistical errors from the fits. We say something about the errors in extrapolating the Gauss coefficients from the Earth's surface downward the core-mantle boundary (Page 5125, last paragraph of section 2). However, we admit that this point should be better taken into account. Therefore, we propose to insert a sentence in the final version of our paper in order to consider this point with a very simple approach. In particular, to meet the Referee's requirement, we suggest to add the following sentence at pag. 5126, line 8, just after "... from 2014 to 2027"

"...from 2014 to 2027.

In the above analyses we did not consider any error in the SAA area estimates. Defining an accurate error budget for the area of the SAA is not possible. Not only one has to find what is the accuracy of the Gauss coefficients, but also one has to estimate what are the contributions of the unknown small scales of the magnetic field. One also has to estimate what effect has on the SAA area the regularization process applied for deriving magnetic field models from geomagnetic data. Nevertheless we expect that the greatest contribution comes from the Gauss coefficients errors, so we try to take them into account in a simple way. Likely, errors in the Gauss coefficients change with time, say from 10% at the beginning of the time interval and 1% at the more recent times, so we cannot be too wrong in supposing an average crude error budget of 5% to propagate with the same percentage to the SAA area values. When these errors are considered in a weighted log-periodic fit the results (not here shown) are not significantly different from those above (e.g., we find a critical time of 2042). Therefore, in all cases a critical process..."

It is evident that we used most of the arguments (and words: please note in particular those **in bold**) given by the Referee "to fill the gap" that, we admit, was in the paper. We present the results below as Fig.1R. We propose not to include the figure in the paper in order to not put another figure. However, we can do differently if requested.

From Fig 1 it is clear that the fit to the area estimated from the IGRF/DGRF 2005, 2010 is not particularly good, whereas these two models are probably the two most accurate models of the whole IGRF/DGRF series.

We agree that the two most recent data points are among those most deviating from the fit. We say this clearly in the manuscript (page 5128, lines 14-17). It is also for this reason that we add a sentence below about the need to update the fit as other more recent data are available (pag. 5128, lines 20-23).

Frankly, we do not believe that, as this Referee affirms, both the above points preclude the publication of the paper. We understand that our approach is quite original in geomagnetism, however our article is honest on these points, providing, together with the interesting results interpreting the geomagnetic field as a critical system approaching a critical transition, also the corresponding limitations.

The other remarks

- Choice of the fitting functions.

We think the paper already explains why we use these functions and not others (all section 3 from pages 5122-5125, and first paragraph of section 3 from page 5125).

-Stability of the extrapolation process

We underline the fact that the process of extrapolation is not stable far from the transition, but can improve with the approaching of the impending transition. This is the reason we also propose to update the fits as new data are available (Page 5129, lines 19-23).

-Link to GSL

Most of this link was already suggested in a previous paper (De Santis et al., 2012) so in the present paper we just mention a little about this (pag. 5127, lines 11-14).

Detailed comments

1- page 5120 ,Line 15: The sentence is unclear. The magnetic field strength of a dipole is the weakest at the dipole equator. I assume that in the second sentence of the paragraph, the authors refer to a field that is more complex than a pure dipole. This should be stated.

We try to explain this better inserting the **red** part at pag.5120:

“The most recent geomagnetic dipole field is decreasing very rapidly and its temporal linear extrapolation would predict a null field at around 1000 yr from now. In some parts of the Earth’s surface this zero value would be reached even earlier **since this field is more complex than a pure dipolar field**: for instance, in the Polar Regions the field would be zero in around 300 yr (De Santis, 2007).”

2- Page 5121, Line 5: The South Atlantic anomaly is a depression of the geomagnetic field strength at the Earth surface. Please add “strength”.

Ok. This will be done in the final version.

3-Page 5121 Line 10. Sentence starting with “ In practice,...” is unclear and probably need rewriting. What is the link of a magnetic monopole with a vortex in the liquid outer core?

The link between a magnetic monopole and a vortex in the liquid outer core was explained in a previous paper (De Santis and Qamili, 2010). Simply stated, this comes from a strong coupling between electric currents (and motions, under the frozen-flux approximation) and magnetic (e.g. monopolar and dipolar) fields. To better clarify this, we propose to insert **the red part** in pag.5121, line 9:

“In practice, **under the frozen flux approximation in the outer core (strong coupling between material motions and magnetic fields)**, this would represent the magnetic expression of a vortex in the outer core, as a component of a strong magnetic flux with reversed polarities with respect to the surroundings.”

References

- De Santis, A. and Qamili, E.: Are we going towards a global planetary magnetic change? 1st WSEAS International Conference on Environmental and Geological Science and Engineering (EG'08), 149-152, 2008.
- De Santis, A., and Qamili, E.: Equivalent monopole source of the geomagnetic South Atlantic Anomaly, Pure Appl. Geophys., 167, 339-347, 2010b.
- De Santis, A., Tozzi, R., and Gaya-Piqué, L.R.: Information content and K-Entropy of the present geomagnetic field, Earth Planet. Sci. Lett., 218, 269-275, 2004.
- De Santis, A., Qamili, E., Spada, G., and Gasperini, P.: Geomagnetic South Atlantic Anomaly and global sea level rise: a direct connection? J. Atm. Sol. Terr. Phys., 74, 129-135, 2012.

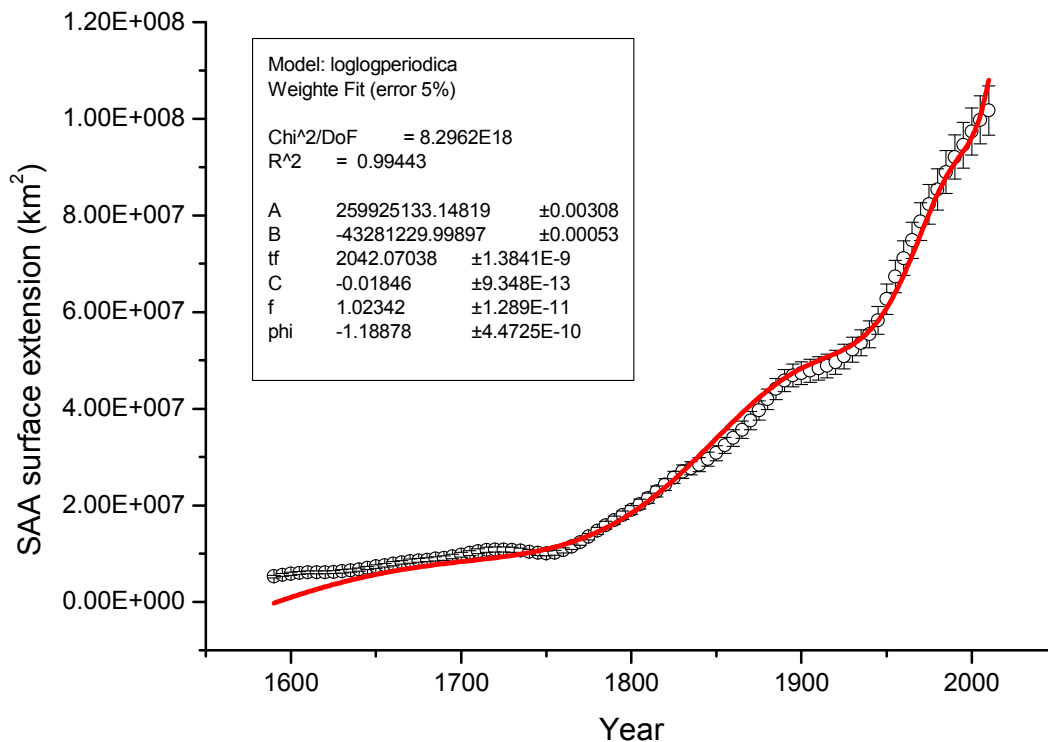


Figure 1R. Log periodic fit to SAA estimates taking into account of their 5% errors.