Response to the reviewer comments for the manuscript "Post-processing scheme for modeling the lithospheric magnetic field" Lesur et al., submitted for publication in SE.

We would like to thank the reviewers for their constructive comments that helped us in improving the manuscript. We address all remarks and comments for each reviewer below, where the original reviewer comments are partly reported in **bold** fonts, the reply is following in normal format.

Although the reviewers did not require this, we have added an appendix describing how the variances of the random variable χ_m^k can be analytically derived. We hope that this is acceptable.

Reply to reviewer~1 comments:

In his general comments the reviewer~1 raised three points:

1- The results presented cannot be reproduced.
2- The equation and workflow should be better described.
3- Synthetic tests should be performed and results compared to other techniques.

We would like first to point out that the processing techniques of satellite magnetic data are not all as robust and well established as are those applied to airborne data in applied geophysics. Techniques for estimating lithospheric field models from satellite data remain more or less experimental. The method we propose here is far from being perfect but nothing equivalent has been proposed so far and it opens new possibilities that are, in our view, worth investigating. We are not at a stage of developments where method performances can be compared.

The first reviewer point is true but we cannot see how to set up interesting tests that can be reproduced. The problem is that the models derived in the first part of the paper require random variables to be estimated. Clearly we cannot do better than give the distribution, average value and variance of the random variables. In the two examples presented the first one is the most important and all possible information to reproduce equivalent results is available. In the second part of the paper, we give now more information about the processing workflow and we believe that a reader used to magnetic satellite data processing can reproduce similar results. An exact reproduction of the results is practically impossible because of the data volume and the complexity of the processing involved.

The second point has been addressed mainly through the detailed remarks. We tried to describe better the workflow, but did not add equations because, as pointed by the second reviewer, mathematics are already "dense".

We have tested all aspects of our processing but decided not to report on synthetic test cases because they are of limited interest: There are no approximations in the equation flow of section 2.1 and appendix A, therefore synthetic test case outputs are just perfectly in agreement (to numerical accuracy) with the theoretical formulae. This remark is now included in the conclusion. The only approximations are on the experimental setup (data sampling rate, orbit altitude and trajectory) and we now comment on these points at several place of the manuscript. There is one major difficulty with the post-processing proposed and that is the impossibility to fully separate the noise model from the lithospheric field model. This point is now commented in details in the conclusion.

Regarding the comparison with other techniques, in the framework of magnetic satellite data processing the only

existing alternative to our post-processing is the "along track" pre-processing technique. This technique can be interpreted in different ways (it is not perfectly clear to us how it has been implemented e.g. in MF7) and suffer the same limitations as the post-processing technique we describe (i.e. the lithospheric magnetic field cannot be fully separated from the noise). Further, because it is directly applied on real data that are selected, the pre-processing is sometimes applied on incomplete orbits (or even small segments of orbits) resulting in instabilities and amplification of errors. If our post-processing does not avoid all leakage between noise and true signal, it is at least free from these difficulties. Because of all these points, comparing the two methods is actually a complex work that would be valid only for the specific setup of the implemented test. The test would then be of limited interest. Further, the amount of information needed to describe such test would be enough for a full paper. We decided not to perform or present this test here and hope that this is acceptable for the reviewer.

Detailed remarks are addressed below.

Pag. 1346 line 5: "where the scaling factor has a specified variance". I would eliminate this sentence because, reading the abstract, the reader has no idea about scaling factor and about its statistical behavior used in the paper.

We have modified the abstract and just describe the scaling factor as a "random variable, normally distributed with zero mean"

Pag. 1350 line 4: "Models of the core field….". In my opinion, the first …

We recognize that the paper will be easier to understand if the information suggested by reviewer is provided. The text has been modified.

Pag. 1350 line 16: "We will also often use the index l for the degree and m for the order". I would delete this …

The sentence has been deleted.

Pag. 1351 line 2: "We simply need to minimize". Why there is the need to minimize? I think that authors …

We introduce the problem in this way to make clear that it is because of the data distribution that the separation of the external/internal field is not possible. That the fit of the internal field to the external field is accurate or not, does not make any difference. The important point is that by making a least-squares fit we find a (non-zero) solution for the internal field. We modified the sentence, added a reference, and hope that the text is clearer.

Pag. 1351 line 5: "…and wi are weights that are defined below". The weights are never clearly defined in the paper. The next reference to weights in at line 10 of page 1351 ….

We did not insist on the value of the weight because this is not really important. What matters is that the Legendre functions of different degrees and same order are orthogonal regarding the scalar product defined by:

$$\langle P_l^{|m|}, P_{l'}^{|m|} \rangle$$

We modified the text to clarify this point.

Pag. 1353 line 9: "However an adaptive Gaussian quadrature….". In order to reproduce the experiments, more information about this method are necessary…

Adaptive integration methods are well known numerical methods for integration. Fortran subroutines are available in numerous free libraries. Two references have been added.

Pag. 1354 line 26: "We do not aim at a precise description of the disturbance….". In order to reproduce the…

This example is given just for the reader to have a feeling on what type of noise the polar electrojet may produce. Our objective is here more qualitative then quantitative. Furthermore, these results cannot be exactly reproduced because they involve a series of random values. Therefore, we are reluctant in providing too many details here. We have nonetheless added some information on the way the model was built.

Pag. 1355 lines 19-25: "The process we applied….". I could not understand exactly the steps. Until now, ….

We realized that, as presented, the general flow of information and data towards the final lithospheric field model is difficult to understand. We have modified the text to help the reader.

Pag. 1357 lines 9-10: "the associated weight…..looks acceptable". What does it mean "acceptable"? In order to reproduce this ….

We understand the concern of the reviewer and modified the text. What we describe is precisely what we did. The data we have added are purely fictitious and therefore no proper criteria can be set to defined how closely they should be fitted. On the other hand, for a relatively large range of weights the difference in fit to the CHAMP data are negligible. The only criterion we can use is the reduction of the spurious oscillations of the lithospheric field model due to the data gaps, and that has however to be set arbitrarily because the field model is not known a priori.

We introduced this full paragraph on the way to reduce the effect of the data gap in field modeling because this is a difficulty that is not easily resolved and that is generally not described in published field models. We recognize that it has some shortcomings but it is the one that works the best.

We rephrase the sentence such as to avoid "acceptable".

Pag. 1357 lines 16: "the data weights used depend only on the data density". Can the authors explain better the dependency of the weights by the data density?

In data sets obtained from polar orbiting satellites, the density of data sampling point is much higher at high latitude, simply because all orbits are crossing over the same area. This high data density close to the poles may affect the model quality and it is a usual process to weight the data to mimic a homogeneous data repartition. Often a simple $\sin \theta$ weight is applied. Here, we calculate the data density in equal area triangles and weight the data accordingly. The text has been modified.

Pag. 1360 line 14: "…an acceptable power spectrum". . See comment about page 1357 lines 9-10.

The important parameter here is the λ and its value is given. We have modified the text.