Recommendations from reviewers about the submitted manuscript ‘Estimating the magnetization distribuition within rock samples’

Reviewer #1 :

This manuscript deals with a hot topic in rock magnetism: techniques to sense the distribution of magnetization in a non-destructive way. These techniques are very necessary to get information on how materials acquire and store their magnetization, as many current techniques alter the sample before or during the sample preparation. This work is therefore very timely and it is pleasantly written and deserves to be published in G-cubed after some minor revisions are made. Figures and tables are properly made and illustrative to the story. A general remark is that the paper in its current form is very technical. Although the introduction frames the work nicely, the authors could add some discussion on the implications and potential for the rock magnetic community and maybe even differing/neighboring subjects to enhance the impact of their work.

I have to be honest that I did my best to follow along and check the mathematical derivations and did not find any mistakes or typos while doing so, but I always find it hard to check these kind of things...

Major comment: sensitivity of the method
In the set-up as described in the manuscript the sensitivity and the resolution of the technique will depend on the interplay between (1) the size and magnetization of the magnetic markers in the sample, (2) the distance between the markers and the scanner (e.g. the size of the sample and the scan height), (3) the sensitivity and geometry of the scanner, and (4) the number and pattern of the scan points on the surfaces. What I miss in this manuscript is an explicit discussion on how these proxies are connected: if the magnetic markers become very small, can they still be properly resolved? What happens if multiple markers obscure each other because of their position or difference in magnetic moment? Can the results of the technique be improved (e.g. the sensitivity increased) if the number of measurement points is increased and the distance between them is lowered? Etc. etc. For the audience these questions are important to answer because they give some bearing on how big the technological step presented here actually is. In fact, this topic relates to my more general remark on the emphasizing the implications of this work more. Furthermore a more explicit discussion on this topic also gives more information on the rudimentary results of the real sample.

Minor comments / Typos
Line 25: add 'our' or 'the' between 'in' and 'laboratory'
Line 26: add 'the' before 'magnetization distribution'
Line 61: to me 'produce' should be 'producing', but I am a non-native English speaker
Line 75: SMM -> SSM
Line 95: 'develop' -> 'developed'

Line 260: 'observed' is a bit confusing 'initial' or 'starting' would be better
Line 298: 'ultra-fine' for 1 mm might be confusing as most of the real markers in rock magnetism are << tens of microns.
Line 358-359: please rephrase 'then it is important...these data'
Line 398: please rephrase 'we considered...zones'

Reviewer #2 :

The paper proposes an interesting new method, which might have potential for the future non-destructive paleomagnetic measurement using a rectangular block. However, there are several important points to be reinvestigated as below. I must say that the authors need to redesign the model and the manuscript significantly before it can be accepted in G-cubed for publication.

1. In the introduction, the authors reviewed the paleomagnetic methods including scanning magnetic microscopy, inversions etc. However, the important aspect in paleomagnetism using pass-through magnetometer and deconvolution is missing. These method deals with the model based on rectangular prisms with a series of uniformly magnetized blocks, which is exactly the same as the model used in this paper. Dodson et al. (1974), Constable and Parker (1991), Jackson et al. (2010) and Oda and Xuan (2014) are the examples on deconvolution. The authors need to review, investigate and describe the advantages of their method over these.

2. The authors need to show clearly the advantages of their measurement and modeling over the existing methods such as scanning SQUID microscopy on thin sections, pass-through measurements using u-channel samples, and discrete measurements on sliced/cut small samples.

3. The authors have investigated the effect of noise and positioning error with the model data. It is valuable to investigate these. However, it is not enough to demonstrate the advantage of the new method over the existing other methods. It is recommended to investigate the effect (residuals or discrepancies from the given model data) of the following conditions. (1) Number of measurement points or density of the data, (2) Number of measurement planes (out of four planes) used for the analyses. By reducing plane one by one, the authors can demonstrate the reduction in reliability. (3) Use of different block length (or thickness) which is used for the model. For example, if 4mm x 3mm x 3mm block was used for the model, assume 2mm x 3mm x 3mm block or 6mm x 3mm x 3mm block for uniformly magnetized ones for inversion. The authors may investigate the difference. The authors need to give an idea how to estimate the best thickness of uniformly magnetized unit blocks for inversion with
unknown measurement data on a continuous prism of for example 16mm x 3mm x 3mm size.

4. The authors show the measurement data by a new magnetometer constructed with a Hall probe. They also describe that the circular effective area is ~300 μm. This means that the magnetic field (magnetic induction with the

term used by the authors) is integrated over the circular area. The effect of integration needs to be considered for the inversion. I also would like to encourage the authors to provide more technical details on the new Hall probe magnetometer together with this new measurement/analyses method.

5. The authors take the conclusions of Oda et al. (2011) erroneously. They conducted scanning SQUID microscopy and the results are the core of their paper. However, the authors take the results of measurements on thin sliced blocks and discuss the advantages over this measurement. The authors also use the measurements of thin slice blocks taken by Oda et al. (2011) in order to model with the framework that the authors propose. Actually, based on the scanning SQUID microscopy, Oda et al. (2011) have shown that the magnetization within each block is not uniform and some of them may contain strong dipoles magnetized in various directions. I would say that the use of data presented by Oda et al. (2011) for the model with uniform magnetization proposed by the authors is inappropriate. It is strongly recommended that the authors discuss the new method without using the data by Oda et al. (2011) and use only the synthetic data or the data they have produced with their own magnetometer.

Minor points
Lines 345-346: The authors say "... magnetized anhysteretically by and inducing field of ~ 1T". This might be isothermal remanent magnetization (IRM) instead of anhysteretic remanent magnetization (ARM)?

Lines 348-349 and Table 3: The authors describe Inclination and Declination of the magnetizing directions? However, there are no actual data of magnetization directions and intensities corresponding to each block sample. Also, it is recommended to provide more details on the sample preparation with the amount of magnetic material used for the blocks. Rock magnetic properties of the materials used should also be provided.

[References]

Dodson RE, Fuller MD, Pilant W (1974) Geophys Res Lett1:185-188. doi:10.1029/GL001i004p00185.

Constable C, Parker R (1991) Geophys J Int 104:453-468. doi:10.1111/j.1365-246X.1991.tb05693.x.

Jackson M, Bowles JA, Lascu I, Solheid P (2010) Geochem Geophys Geosyst 11, Q07Y10. doi:10.1029/2009GC002991.

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