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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "A comprehensive model for the kyr and Myr time scales of Earth's axial magnetic dipole field" *by* Matthias Morzfeld and Bruce A. Buffett

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The geomagnetic field varies on time scales from about a year to several tens of million years. The vastly different time scales have been revealed by various data sources, all having their specific inherent problems and limitations. In their pioneering work from 2001, Hoyng and Schmitt suggest that at least the axial dipole variations on time scales from millennia to some millions of years can be describe by the Langevin equation, a simple stochastic differential equation used, for example, to model Brownian motion. In recent years, the respective model has been refined to include the effects of correlated noise, random errors, or the limited time resolution of sedimentary data. This

manuscript introduces another refinement, a Bayesian approach that allows to incorporate different types of 'data' in a probabilistic manor to constrain the (five) parameters of the stochastic model. The paper is interesting and well written, but requires a few additional clarifications here and there (see below). The authors already mention that their analysis reveals difficulties and/or inconsistencies, but the paper remains too vague at this point. There problem could result from the different (and inconsistent) treatment of time-domain and frequency domain data, but this is hard to judge from the manuscript. A generally more critical discussion of the approach also seems in order. In addition, it remains unclear whether the results reveal anything new about geomagnetic field variations. A few respective additional sentences, for example in the conclusion, would certainly strengthen the paper.

Mayor points:

1) Data: the stochastic model is constraint based on paleomagnetic and archeomagnetic data, which both have many problems. The dating uncertainties and the smoothing due to the lock-in-time in sediments are mentioned in the text, but there are more. While there is little the authors can do about this, some more critical discussion and application seems in order. For example, the fact that the two paleomagnetic models show sizable differences suggests using a large uncertainty when modelling the respective data. The reversal rate is another example. How well can one determine the reversal rate based on a 30 Myr record when the underlying process is Poissonian?

2) Geomagnetic power spectra: The power spectra and the respective models are not discussed in any detail. Also missing is an explanation how the spectra are incorporated in the Bayesian approach. Combining hundreds of 'frequency data points' with only three 'time domain data points' may not be the best way to proceed and certainly required some care. A parametrization or identification of the most important spectral features seems the way to go here. See Baerenzung et al. (2018) for such an alternative approach. Figure 3 and 7 suggest that neither the spectra from the stochastic model runs nor the theoretical spectra do a very convincing job in replicating the spec-

tra from the data, at least when it comes to the location of 'knees' or of typical slopes. Some additional text seems in order here. How much can we trust the spectra and in which frequency range? What are the limitation of the stochastic model when it comes to the spectra? How much agreement can we reasonably expect? Please also remind the reader why the Sint-2000/PADM2M and CALS10K.2 spectra are so different where they overlap in frequencies. Why does figure 7 show a different range than figure 3? Do you really think that the stochastic model can capture the high frequency part shown in figure 7?

3) Critical discussion: The authors already point out some problems or 'inconsistencies' in the sense that their model cannot capture all 'data' convincingly. This should be discussed in more detail. Is this a problem in the data or is the model too simple? The deficiencies in describing the spectra seems to imply the latter. It seems to me that the model is doing OK for describing the long-term variations where any complexity due to the convection and the internal dynamo process may not matter so much. Implementing a archeomagnetic (or even historic model) seem then overambitious. The authors should also discuss whether we can we learn anything about the geodynamo from this approach?

Minor points:

1) Please check the way you cite. You seem to mix up citep and citet.

2) Abstract, last sentence: Bayesian reasoning is frequently used for combining different data. What exactly is new in your approach?

3) Same sentence: ... data sets, which is particularly ...

4) Page 1, line 23: What do you mean by: ... even basic analytical calculations are often intractable?

5) Page 2, line 5 and following: To my knowledge, the basic concept has been introduced by Hoyng, Schmitt and Ossendrijver in a series of papers in 2001 and 2002

(see below). Please give them credit. What exactly are the new ingredient in the B13 model? Note that Meduri & Wicht (2016) claim that the linearization used later in the paper (and which may also be a component of model B13) may only be of limited use.

6) Page 2, line 54: Define SDE.

7) Page 2, line 19: Buffett and Puranam (2017) try to mimic the effects of sedimentation ...

8) Page 3, line 29: ... to reduce the influence of non-dipole components and various error sources ...

9) Page 4, line 3: CALSK10k.2 sampled at an interval of 1 year? Is this really the model's resolution?

10) Page 5, line 14: At least set "Brownian motion" in quotation marks.

11) Page 5, line 18: Concerning a constant D, see Meduri & Wicht (2016).

12) Page 5, line 22: ... well potential potential ...

13) Caption of fig. 2: ... and potential U(x), with U'(x)=- v(x) ... Or use an integral formulation.

14) Page 6, line 4: Could you explain iid in a few words for the non-experts?

15) Page 6, line 9: ... are affected by affected by ...

16) Caption of fig. 3: There is something wrong with the sentences.

17) Page 8: Because an SDE is noisy ... Well, no surprise there. Could you be more specific? Wouldn't this also depend on the noise parameters? How expensive is an SDE integration? How long do you have to integrate?

18) Page 8, line 10: The comparison does not look too good. Please discuss.

19) Page 8, line 14: ... the time averaged value of the absolute value of x(t) ...

20) Page 8, line 23: approximating -> approximate

21) Page 8, line 27: The SD is not very close to the observation. Please discuss.

22) Page 9, line 4: Sampled once per year . . . (see above).

23) Page 9, line 18: Discuss the comparison.

24) Page 9, line 30: . . . the bound may be overly pessimistic . . . Well, the problem is not solved yet, but it looks like electron-electron interaction could at best only have a mild effect. Anyway, there is no need to dive into this topic in the paper and I would simply drop this sentence.

25) Page 10, top: Note that the stochastic model represents longer statistical time scales. Arguing with flow velocities is of limited use here.

26) Sections 4.2.2 and 4.2.3: Please provide more details here. (See comment above)

27) Figure 6: Provide colorbar. Discuss a bit more. How well are the parameters constrained, for example compared to the priors?

28) Page 17, line 2: The “however” seems out of place.

29) Page 17, line 5: . . . the impact of each . . .

30) Page 21, line 1: . . . in the context of our simplistic model CALS10k.2 mostly constraints . . .

31) Page 21, line 30: Every geomagnetic data point is indeed the result of hard work, but why is this a challenge for the model described here?

32) Page 22, top: Incorporating different type of ‘data’ in a Bayesian approach is a standard application. Please point out the specific novelty in your approach.

33) Page 22, line 4: “We use the full paleomagnetic record”? The years of hard work have resulted in more than just Sint-2000 and PADM2M.

Baerenzung, J., Holschneider, M., Wicht, J. Sanchez, S. and Lesur, V., Modeling and Predicting the Short-Term Evolution of the Geomagnetic Field, J. Geophys Res. (solid Earth), 123, pp.4539-4560, 2018.

Hoyng, P., Ossendrijver, M.A.J.H. and Schmitt, D., The geodynamo as a bistable oscillator,Geosphys. Astrophys. Fluid. Dyn., 94, 2001.

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