Referee report on the manuscript

Magnetic Prandtl number dependence of turbulence generated by chiral MHD dynamos

submitted for publication in GAFD special issue on "Recent Developments in Natural Dynamos"

by Schrober, Brandenburg, Rogachevskii & Kleeorin

This paper presents a numerical study on the chiral MHD dynamo and its dependence on the magnetic Prandtl number (Pr_M). The paper also summarises many previous results by the same group on the same topic. This scenario has, during the recent years, increased in popularity to explain the generation
of primordial magnetic fields in the early universe. The existence of such primordial fields is implied by observational evidence of the intergalactic medium (IGM) hosting a weak magnetic field coherent over Mega parsec scales. Their origin is not yet understood, and is currently a field under intense study.

The main results concern with the energy ratio resulting from the chiral dynamo instability in a system where the instability powers turbulence through the Lorentz force, that in turn drives a large-scale dynamo instability, which then quenches the chemical potential through its conservation law. The energy ratio is found to be independent of the initial chemical potential that drives the small-scale dynamo, but decreasing as function of Pr_M. These results shed new light to the properties of the chiral MHD dynamo, and are definitely worth publishing in the GAFD special issue.

I would urge the authors to pay attention to the following issues, however:

1. The implications of the two major findings are not properly discussed.

- The chiral alpha effect is proportional to the initial chemical potential, mu_0, and that determines how much magnetic energy will be available to be transformed into kinetic one by the Lorentz force. In that sense, instead of the ratio of kinetic to magnetic energy (Upsilon), a more relevant quantity to define and discuss would be an energy transfer rate that takes into account this inevitable dependence on mu_0. The energy ratio being independent on mu_0 does not imply that the amount of energy that ends up in turbulent form would not. This is the impression that one gets now.

- The Prandtl number dependence and the estimated values of Pr_M in the early universe imply that the energy ratio would drop to values of the order of 0.01. The implications of such small energy ratios for the chiral dynamo scenario are not clearly discussed, but are hidden in Section 4.2, where the authors have decided to estimate a magnetic Reynolds number for the early universe instead, also introducing a free parameter vartheta to "explore different initial conditions". It is totally unclear which parameter choices resulted in the final estimate of Re_M being O(3), nor are all the parameter values given in 4.2 to enable the computation of the numbers (e.g. value of g_100?),
if the reader would be interested in repeating the exercise.
Also, one remains wondering, whether Eq. (31) can be correct at all.
Moreover, Eq. (32) gives, if one estimates the coherence scale of the IGM mgf to be about a Megapc, value of 10(-41) for vartheta.

I strongly urge the authors to check, correct, and clarify Sect 4.2, and more clearly discuss the relevance of the chiral dynamo scenario given the small energy ratios obtained with the expected, large Pr_M.

- The models have different values of lambda_mu (hence different k_lambda) and are, therefore, not directly comparable (different phases last for a different time, are not reached at all, and so on). Now this heterogeneity issue is dealt with computing averages over different tresholds of Upsilon. A better way would definitely have been to divide each simulation by their phases (defined in section 2.3, and appear easily distinguishable from the simulations), and compute the energy ratios separately for each. This would have, in addition, given much more information about the system than collapsing the data into a tresholded time average smearing the phases.

2. The paper is really hard to read.

- One reason is the overwhelming amount of different parameters given, not properly defined, or defined much later in the text than when used for the first time, and their meanings not properly explained. I strongly recommend that the authors make a table of all parameters and their meanings and typical values. Even more, I would also suggest that the authors check whether all these parameters are needed. If their number can be reduced, all the better.

- The other reason for the poor readibility are illegible axis labels (Fig 3) or heterogeneous axis definitions (e.g. the usage different time axis in between Fig 2., 3, 6-8). The authors make an attempt to clarify their complicated plots by indicating some parameter values to mark some key transition points (t_box, lambda_mu, k_mu, k_lambda, k_1, Bsat, B1->2, C_lambda mu_0/lambda ....), but this does not work taken that the parameters are not explained clearly enough.

- The caption of Table 1 is confusing, if not completely wrong. The tabulated values are hard to relate to the actual model parameters.
Is the purpose of this table to list only input quantities? It is not completely
clear whether k_mu and k_lambda are measured as outputs from the simulations, or are they computed from the analytical/empirical formulae (11)/(12)? This should
be clarified. What does it mean if k_lambda/k_1 is smaller than one - the large-scale
dynamo instability is not properly captured? It would have been a good idea to list some output quantities as well. urms? brms? Upsilon? Bsat? B_1->2? ....

- Please avoid repetition of the definitions of what is plotted in the figures in their caption and the text (e.g. Fig 1 caption and the text on page 6, last full
paragraph). The paper is exhaustively long in any case, and extending the text this way does not help.

- Please consider to what extent Section 2.3 material has been represented in your earlier papers on the topic, and whether such an extensive review is really necessary here. This section should, at the least, be made much more compact.

3. The text is not comprehensible/is misleading/appears irrelevant at places.

Abstract:

Here you only talk about the small-scale chiral dynamo, but then refer to the large-scale dynamo by talking about "MHD dynamos". This is confusing.

"we estimate the properties of chiral magnetically driven turbulence..." is an overstatement and vague. Values of magnetic Prandtl and Reynolds number are estimated.

Intro: please reformulate the following sentences to contain a clear, sensible message:

1st para
"MHD dynamos are often caused by ..., so for example in the cases of the small-scale ...."

2nd para

"In recent years, the nature .... has been more and more constrained."

"The lower limits on the strength .... might be due to possible remains..."

The chiral instability is by no means the only proposed scenario for primordial mgfs, as the authors now claim. Please modify to better reflect the reality:

"Cosmological seed fields, however,..., and have been connected to a microphysical effect....

Section 2.2 is overall quite confusing. This section, indeed, is comprehensible to only those readers that are familiar with MFE, and as such, it is difficult to see the rationale to include it in this form. Please restrict the discussion to the relevant points for the chiral dynamo here, do not dwell into the delicacies of turbulent dynamos.
The text starting from "In MFE, it implies that ... " until the end of the section seems all dispensable.

4. Please check the language carefully. At least correct

"\Upsilon^2" in the abstract, "analytical analysis" and " conversation law" in the Intro, and "size of the inertial range" in 4.1