# Referee report on MS#M172754

**Title: Topology-preserving discretization for the magneto-frictional equations arising in the Parker conjecture**

**Authors: Mingdong He, Patrick Farrell, Kaibo Hu, and Boris Andrews**

This paper presents a novel numerical approach to ideal magnetic relaxation in astrophysics, where conservation of magnetic helicity – a topological conservation law – is known to control the dynamics. The underlying problem of whether or not tangential discontinuities form is somewhat analogous to the formation of finite-time singularities in the Euler equations, so is of broad interest. The authors do not try to address this problem, but they do make an important advance by presenting a finite-element discretization that preserves both helicity and energy. This is a significant advance on previous numerical schemes (which have usually been either finite-volume or Lagrangian type).

The paper is well written and I anticipate being able to recommend it for publication if the authors can address the following queries.

1. Definition 3.5 (L236): The generalized helicity reminds me of the "Bevir-Gray" helicity known for many years in fusion plasmas. See, e.g., D. MacTaggart and A. Valli, *J. Plasma Phys.* *85*, 775850501, 2019. In the original version for a toroidal domain, one defines this helicity as

$$H^{\mathrm{BV}} = (\boldsymbol{A}, \boldsymbol{B}) - \oint_{\gamma_1} \boldsymbol{A} \cdot \mathrm{d}\boldsymbol{l} \oint_{\gamma_2} \boldsymbol{A} \cdot \mathrm{d}\boldsymbol{l},$$

   where $\gamma_1$ and $\gamma_2$ are loops on the boundary of the domain, the long and short way respectively. This quantity $H^{\mathrm{BV}}$ is then gauge invariant. The extra term is the product of the toroidal magnetic flux in the domain and the (effective) magnetic flux "through the hole of the torus", thus giving it a nice physical interpretation of subtracting any topological linking with this external flux. Therefore, I'm wondering whether the authors' generalized helicity can somehow be viewed a generalization of the $H^{\mathrm{BV}}$ formula? That might give it a physical interpretation.

2. Proof of Theorem 3.8: It seems to me that the last inequality is tacitly throwing away what could be a significant fraction of the energy in practical applications where there is a strong harmonic "guide field". For example, refs [37-38] in the paper. If I understand correctly, one could strengthen (3.15) to

$$|\tilde{\mathcal{H}}| \leq C^{-1}(\|\boldsymbol{B}\|^2 - \|\boldsymbol{B}_H\|^2).$$

   In other words, the presence of generalised helicity requires a minimum energy in the non-harmonic part of the field alone.

3. L365: Reading that the finer mesh has resolution only $32 \times 32 \times 80$ set alarm bells ringing. To me, this appears still very coarse compared to typical finite-volume calculations where it is usual to have hundreds or nowadays even thousands of grid cells in each direction. Could the authors comment on how the accuracy compares? In other words, is this SP finite-element method significantly more expensive than previous solvers? I do also worry that such a coarse mesh will be limited in resolving the tangential discontinuities that we expect to form during the relaxation. So it would be good to know whether it would be practical (in future work) for the resolution to be increased.

4. I commend the authors for making their source code available. However, I have so far failed to install this successfully. The Zenodo page [21] recommends using `firedrake-install`, but this seems to be obsolete and indeed failed on my machine. I did succeed in installing Firedrake according to its own instructions, and installed the required `avfet_modules`

from `https://github.com/BorisAndrews/andrews_farrell_2024`. But there is still an apparent conflict between the two and e.g. `bejh.py` fails to run. I don't know if this is something that can/should be resolved through the anonymous referee process, but I feel I should flag it up in case the installation instructions can be updated.

5. A related question. The use of `avfet_modules` in the source code for making the figures suggests to me that a "finite-element in time" discretisation is being used, whereas in L300 of the paper it says that the implicit midpoint method was used for the computations. Could the authors clarify?

The following suggestions are all smaller things that I hope would improve the paper:

6. L24: for a more recent mathematical work on the Parker problem, consider citing "Obstructions to topological relaxation for generic magnetic fields", A. Enciso and D. Peralta-Salas, *Arch. Rational Mech. Anal.* **249**, 6, 2025.

7. L25: suggest "fluid diffusion" $\to$ "fluid viscosity"

8. Eq (1.1): Firstly, the domain of the integral should be specified (to make clear that it is a volume integral). Secondly, I think the authors should clarify already here that conservation of $\mathcal{H}$ assumes $\boldsymbol{B} \cdot \boldsymbol{n} = 0$ on the boundary. Similarly it should be clarified that this is required also in (1.2).

9. L48: here one could also cite "On the limitations of magneto-frictional relaxation", A. Yeates, *Geophs. Astrophys. Fluid. Dyn.* **116**, 305, 2022 who gives a review of magneto-friction in their introduction.

10. L60: suggest to reword this sentence, because the magnetic energy is *not* non-increasing in the original MHD system, only the total energy. (In particular, magnetic energy can be converted to and from kinetic or internal energy.)

11. Eq (1.4): Again, there is an assumption of boundary conditions here that would be better clarified now rather than later.

12. L70: "The existence of tangential discontinuities of the stationary solutions of (1.3) is therefore equivalent to the Parker conjecture." The Parker conjecture does not have a universally-agreed statement, and this is only true for the "force-free" version of the conjecture, as opposed to allowing more general magnetohydrostatic equilibria with gas pressure gradients. This should probably be clarified.

13. L84: suggest to insert "method" after "mimetic Lagrangian"

14. L113: spurious word "the"

15. L128: I think $\|u\|_{0,2} := \|u\|$ should be the other way around?

16. Footnote 1 (p4), eq (3.1): I think there are too many curls on the right-hand side of the first equation.

17. L234-5: Probably my stupidity, but I don't understand why it follows that $\boldsymbol{B}_H$ remains constant?

18. Eq (3.15): Is there a reason for not using $\mathcal{E}$ to denote the energy here, for consistency with earlier?

19. L301: More explanation is required for the reader to understand the difference between the "trivial" and "nontrivial" experimental setups. In particular, only one form of $\boldsymbol{B}_0$ in (4.1) is given, but comparing Figs. 6 and 7 suggest that some sort of vertical (harmonic) field is added in the non-trivial case? It would be good to clarify this explicitly. Also, for the trivial case, do we have $\boldsymbol{B} \cdot \boldsymbol{n} = 0$ *exactly* on the $z$-boundaries, or does this rely just on the radial decay factor in $\boldsymbol{B}_0$?

20. L302: Am I right that the magnitude of $\tau$ does nothing in this model other than rescale the time variable? In which case, is there a particular reason for using 100 rather than 1? As a side remark, note that in some implementations of magneto-friction in Solar Physics, the $\tau$ parameter is made a function either of $\|\boldsymbol{B}\|^2$ or of space, which can have a significant effect on the evolution.

21. L307: This sentence is extremely brief (for a non-specialist in finite elements) and would benefit from at least a reference about how this projection is done.
22. L320: Do we know the actual *value* of the Poincaré constant $C$ for simple rectangular domains? This could give an idea of the tightness of the Arnol'd inequality in each case. (Relates to my point 2 above.)
23. Fig. 2: Can the authors explain why the helicity error increases at early times, before becoming completely flat? (Not greatly important as these are very small errors.)