nature portfolio

Peer Review File

Measuring the magnetic fields in the chromospheres of low-mass stars

Corresponding Author: Dr Tianqi Cang

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I was asked by the editor to review the spectropolarimetric aspect of the paper. Since I do not have expertise in stellar physics, but only in spectropolarimetry, I refrain from commenting on the stellar physics aspects of this paper.

That being said, I have the following comments to the spectropolarimetric observations presented in the paper.

The authors use Stokes I and V data from two high-res spectropolarimeters, ESPaDOnS and NARVAL, for three active, mid to late M-dwarfs. The data come from the PolarBase data base, described in [54]. In this paper, the usage of PolarBase data for M-dwarfs is explicitly mentioned in Section 8, where the biases are discussed, which result from the selection of line masks for the used LSD models.
The LSD method works especially well when many lines can contribute to the signal. For the photospheric line, the authors gained a factor of about 10 using the LSD method. For the lower chromosphere, only three spectral lines could be used, and a S/N increase by a factor of 2 could be achieved (although I would have expected at most an increase by a factor sqrt(3), and not 2). For the middle chromosphere, the LSD method obviously could not be applied, although not mentioned explicitly in the paper.

Although the LSD method is useful to increase the S/N ratio, it is not efficiently reducing instrumental effects when used with very few spectral lines only.
The small signals in Stokes V for the chromospheric lines make a careful consideration of the cross-talk from Stokes I to V mandatory. Semel et al. [52] describe this problem, and the approach of removing spurious polarimetric signals. In their Sect. 2 they state that one "... should be careful about crosstalk from intensity to circular polarization." This careful consideration is not present in the paper.

I lack in the paper a clear description of how the authors dealt with the issue of possible crosstalk from I to V. The shape of the Stokes V signals presented in Figure 1 for the lower and middle chromosphere could also be a result of this crosstalk. This suspicion is supported by the fact that the magnetic field strength derived for the middle chromosphere for all three stars is almost indistinguishable.

Before a discussion about the magnetic field configuration on the three M dwarfs is possible, the reliability of the underlying data must be confirmed.
The method section needs to be expanded to include a description of the crosstalk. An analysis similar to Sect 2.4. in [52] needs to be presented with the result clearly demonstrating that the presented chromospheric Stokes V profiles are not affected by the crosstalk from I to V.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

The magnetic activity of M-dwarfs is an important astronomical question for two major reasons. Firstly, it aids the understanding of the stellar activity, as these stars cover a different region in the stellar parameter space than the Sun. Any general dynamo theory should be able to explain the activity of both the Sun and other late-type stars. Secondly, most potentially habitable exoplanets are expected to orbit late-type dwarfs. Stellar magnetic activity is known to have a great impact on habitability. This is especially for M-type dwarfs, as the habitable zones are much nearer to the host star, than in the case of the Sun

The manuscript "Measuring the magnetic fields in the upper atmospheres of low-mass stars" is a study of the changes in the large-scale magnetic field of three M-type dwarfs from the photosphere to the mid chromosphere. The authors apply a novel approach, where they study the V-spectropolarimetric signal of lines originating in the photosphere, lower and mid-chromosphere. This approach reveals how the large-scale magnetic field changes through the atmosphere of these stars. In this respect, the study is truly novel, and represents a major step forward in understanding the magnetic activity of M-type dwarfs. The reviewers are not aware of any previous studies, where such methods have been applied on these kinds of stars. The analysis is largely based on studies of the solar chromosphere. This is, of course, the only reasonable approach, although there are indications that the solar analogy cannot always be applied on stellar magnetic activity of more active stars (e.g., Kochukhov et al., 2023, A&A 680, L17).

We do not find any flaws in the analysis, other than the missing significances of the correlations (see below). The fact that the photosphere contributes to the Ca II IRT lines is challenging, but this seems to be properly addressed. However, we suggest changes in some of the conclusions.
Our recommendation is that this study should be published in Nature Communications, but after some significant revision detailed below.

I. Major issues:

A major critic on the manuscript, is that the authors do not enough address the small-scale magnetic field in the objects. The small-scale field is actually only mentioned once in the manuscript (line 108). This is, of course, partly outside the scope of the study, since measuring the small-scale field vector is way beyond the achievable resolution. Still, it is a well-known fact, that most of the magnetic energy will reside in the small-scale field (see, e.g. Kochukhov et al., 2020, A&A 635, A142). Furthermore, estimates of the percentages of the large-scale vs. total magnetic field are available for the three targets in this study (see Kochukhov, 2021). The small-scale field of the Sun is particularly important in understanding the formation of sunspots, active regions, flares and CME:s. The same would be expected for M-type dwarfs. Addressing this question could easily be done by reformulating relevant sections in the manuscript. Furthermore, the ratio between the large-scale and total magnetic field for the targets could be listed in a relevant table.

In many places in the manuscript, the authors write about polarity reversal and changing magnetic topology with height. On the other hand, at least some of the observations can be explained by reasonably simple models (S8) involving large-scale magnetic field loops viewed from different angles. Thus, the different field topologies would rather be an apparent phenomenon depending on geometry and viewing angle. Usually in the context of active late-type stars, polarity reversals refer to possibly cyclic changes in the large-scale radial field, and changing topology is used to describe, e.g., the variations in the percentages of poloidal vs. toroidal and axisymmetric vs. non-axisymmetric field. Thus, the authors should clarify this difference and consider using other wording.

Concerning the correlation between the magnetic fields in different atmospheric layers, the situation is not as clear, as the text in the manuscript seems to indicate. For YZ CMi the case looks quite clear, and the strong correlation can be seen in the plot S6. For AD Leo, the correlations between the chromosphere and photosphere (S5 panels a and b) are rather mixed, and it is hard to visually judge how significant the correlations within the different epochs are. The case is the same for EV Lac (S7, panels b and c). Especially in cases where the number of points is relatively small, correlations should always be given with estimates of significance. Such estimates should definitely be included in this study.

In most cases, the data would be sufficient for Zeeman-Doppler imaging (ZDI). At least for some of this data, ZDI maps have been calculated (e.g., Morin et al. 2008). We acknowledge the point (line 90), that extrapolating the magnetic fields from ZDI maps to the chromosphere will probably not give valid result. This would be best demonstrated by using a ZDI map of one of the targets, extrapolating the field and comparing this with the current measurements. Did the authors consider doing this?

The question of differential rotation is not discussed at all in the manuscript. This is apparently because in previous studies, none of these stars show indications of any strong differential rotation (Morin et al., 2008). However, on scales of months, even a very small differential rotation may have a significant effect. Did the authors consider differential rotation? The possible shear could easily be tested using, e.g., the estimates (with error bars) derived by Morin et al. (2008). In the case of YZ CMi the measurements follow quite well a double sinusoidal fit (S3). Here the stability (against, e.g., differential rotation) of the large-scale magnetic field could be tested by checking whether there is a correlation between the deviations of the points and time difference from the mean HJD.

A small, but principally important question is whether the dynamos of fully convective stars differ that much from the solar dynamo (lines 107-8). Here, there are competing paradigms, which lead to different claims. In the flux transport dynamo paradigm, the tachocline plays an important role, which would mean that fully convective stars would have a different type of dynamo. However, the distributed dynamo paradigm does not rely on a tachocline. There are, as far as the reviewers know, no observations indicating a significant change in dynamos between fully convective stars, and stars with a convective envelope and radiative core. This would support the latter paradigm. Here, we do not demand the authors to choose a side, but rather request that this sentence should be reformulated to accommodate both possibilities.

II. Other comments and suggestions for the main paper:

Age estimates for the three stars are not given anywhere in the manuscript. If estimates exist, we suggest they should be included in the manuscript. As for the stellar parameters, these are taken from the previous study by Morin et al. (2008). We note that there are some discrepancies in the values of v sin i, P_rot and i for EV Lac and YZ CMi: It seems that the inclination for these two targets should be larger than 60 degrees. Did the authors consider this? On the other hand, we do not expect that this would alter the results.

The exposure times are not given for the observations. We urge the authors to report these, or at least their range. It would also be good to give a range for the S/N, rather than the typical noise level (line 588) for all measurements, i.e. the photospheric LSD lines, the CaII IRT and Hα.
It is often a custom to present null profiles of Stokes V-spectropolarimetry. We would encourage to include examples of these for each star in a similar way as in Fig. 1.
In the abstract (line 53) and elsewhere in the manuscript, the concept of “direct” measurement is used. We would refrain from using this wording here, as the methods used involve modelling. Still, this would not reduce the importance of this study, as any estimate of the magnetic field above the photosphere for these targets is novel.

On lines 72-73 we suggest replacing “through the photosphere” with “to the photosphere”.

On lines 78-80: “… measuring the magnetic field of the upper atmosphere, including the chromosphere, is essential to understanding the physical mechanisms responsible for stellar coronal heating and eruptions.” This is certainly true, but in the Sun the small-scale field is crucial for understanding these phenomena. One can expect that the same holds for other late-type stars.

On line 91 there are three references. Comparisons between extrapolation and measurements are included in Yang et al. (2020). But are the other references also relevant here?

On line 96: In “M-dwarfs generally have a lifetime longer”, the word “generally” seems unnecessary, since M-dwarfs always have orders of magnitude longer lifetimes than the Sun.

On lines 98-99: “These conditions …” should be rephrased. This is not only a question of the number and ages of M-dwarfs, as each M-dwarf in the solar neighbourhood seem to have more planets than earlier type stars.

On line 107: “believed to be fully convective” should be rephrased, as this is an established fact.
Paragraph starting on line 111: We suggest considering to first explain the theory, i.e. the formation of the spectral lines in the different layers, and then the observations.

On line 140, reference [41] should probably be [40].

On line 153, the wording “the stars were nearly stable within an epoch, with a timescale of at least one month” raises the question on what ground this epoch length was chosen. Maybe this could be described with a sentence?

On lines 164-167 the wording “the variation in polarity and strength at different heights can be attributed to the drastic variation of corresponding large-scale magnetic field structures with height” should be rephrased. Do the observations really support “drastic variations”, or can they be explained by magnetic field loops of different heights and viewed from different angles?

On lines 215-218, the sentence “This scenario also indicates … intense UV and X-ray radiation” is, of course, true. However, again most of the magnetic energy is known to be contained in the small-scale field, which is also probably contributing much more to flares and CMEs.
We suggest adding “LSD profile”, “Hα”, and “Ca II IRT” to the plot titles in Fig. 1.

On lines 571-572, it says “rotational periods have been estimated from the modulation of the photospheric Zeeman signatures”. However, in the paper by Morin et al. (2008) P_rot was determined in the ZDI inversions.

On lines 617-618. The g_eff for the chromospheric lines are given. References for these values would be useful.

III. Comments and suggestions for the supplement:

On line 15, the sentence “exhibited a small projected rotational velocity, resulting in low v sin i values” is a tautology and should be rephrased.

On line 40-43, the claim “generally form one repeating pattern in the phase-folded diagram” is only half the truth. For YZ CMi, this is clearly the case. But for AD Leo, there does not seem to be any clear pattern. For EV Lac, the pattern is quite vague especially for the lower chromosphere. So, this claim should be rephrased.

On lines 49-50, the statement “large-scale photospheric structure appears more axisymmetric (axisymmetric modes > 90%)

in AD Leo than the other two stars" may have to be rephrased. At least in Morin et al. (2008), AD Leo and YZ CMi have nearly the same axisymmetry.

On lines 88-89, the sentences with "with m giving the order of the spherical harmonics mode and describe. In practice, αℓm is complex coefficients corresponding to the radial poloidal field" seem to have some words missing. Correct also the "m" to the right format.

On line 95, correct the format of B_l.

On lines 116-118, there is a clear need for references. With what method were the flares detected and their energies determined?

On lines 123-124, the sentence "It is still unclear why there are fewer low-energy flares in the situation of more complex fields", should be rephrased to take into account the small-scale field.

In Fig. S1 axis titles and units are missing in the plot.

In Fig. S3 the units (d) are missing from the scale on the right side.

In the extended data table S1, references for the stellar masses would be useful. Also, the parameter ß could be explained also here.

Several corrections should be made in Tables S2-S4. Firstly, the HJD and phase-values should be given with consistent accuracy. Here the possible effect of the exposure times should be considered. Furthermore, using the value "-999.0" when the number of measurements is insufficient is not a good choice, better to use a blank or "N/A" in these cases.

(Remarks on code availability)
The codes worked fine when downloading. However, some comments are written with Chinese letters, making them challenging to comprehend.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

(Remarks on code availability)

Reviewer #4

(Remarks to the Author)
(1) The authors report measurements of intensity profiles and circular polarization profiles for spectral lines which are formed in principle at three different altitudes in the atmosphere of a flare star. They interpret those line profiles in terms of magnetic field strength (including algebraic sign of the longitudinal component) at the three different altitudes. The reported field strengths are probably to be interpreted as "mean" fields at the various altitudes, although the word "mean" can hardly be interpreted as a simple arithmetic mean over the visible disk of the star at the time the data were obtained: with 3 different vector components of the field present at all locations of the stellar surface, the "mean" must involve a complicated process.

(2) The authors claim that, because they have used three lines which are formed at different altitudes, their results allow them to state confidently how the field strength varies as a function of height. But this seems improbable: the 3 lines are formed at heights which are separated by no more than (perhaps) 0.3% of the stellar radius: given that the fields in the star are generated by a global dynamo, it is highly unlikely that any significant changes in "physically real" field strength can be expected to occur over such a tiny range of radial distance.

(3) The authors report (with great confidence) in Tables S2-S4 that the field actually changes sign between photosphere and low chromosphere, or between lower and mid chromosphere. But I find this hard to believe: their confidence, it seems to me, is not warranted when I see the serious "noise" level in the V profiles which they show in Figure 1 in the text (for Halpha and also for CaIRT) and also in Figure S2 (for Halpha).

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I thank the authors for addressing my criticism about the reliability of the Stokes signals. The Null-spectra analysis is indeed a good method to confirm that the measurements are not affected by crosstalk. I agree with the authors and find my point (1.2 Reviewer #1) sufficiently addressed.

However, I was surprised to see the big changes in Figure 1 between the original and the revised submission. The authors say that the "apparent similarity in magnetic field strength in the middle chromosphere of the original Fig. 1 was coincidental", and present "observations of higher S/N and different field strength measurements".
Where do these higher S/N observations come from? How is it possible that the new values for the field strengths are far outside the one-sigma values given in the original version?
I did not find a mention in the paper about these new observations. The magnetic field values in the new version are vastly different from the old version. This is not raising confidence in the results, and I expect a clear explanation for this discrepancy.

In Figure 2 the authors added a Fourier series fit to the data, "showing the strong periodical signal of the magnetic field variation". I see at mose one period, therefore one cannot make a statement about how 'strong' the periodicity is.

As in the previous report, I refrain from commenting on the stellar physics aspects of this paper.

(Remarks on code availability)
I tried to run the code in Docker Desktop for MacOS (M2, 15.5 (24F74)). Unfortunately, the code did not produce any output and stopped with many error messages. I attach the logfile of my run (output.log).

Reviewer #2

(Remarks to the Author)
As stated in the first review, the manuscript presents a novel approach to estimate the magnetic field and its polarity in different atmosphere layers of three M-type dwarfs: The photosphere, the lower and the mid chromosphere. This represents a significant contribution to our knowledge of stellar magnetic activity in M-type dwarfs.

We thank the authors for taking into account the main critics from our previous report and improving the manuscript accordingly. We thus recommend that the manuscript would be accepted for publication in Nature Communications with minor revisions. The suggested revisions are listed below.

1. Line 111-112:
"… whether their dynamos rely on a tachocline…"
This should be rephrased, as fully convective stars do not have a tachocline. It could, e.g., be "… whether the lack of a tachocline will result in a dynamo different from the solar one."

2. Line 219-221:

"We notice that, compared to the other two stars, AD Leo has fewer low-energy flares and more high-energy flares (especially superflares), but not a general weakening of all kinds of flares."
This should be rephrased to clarify what the authors mean with "weakening".

3. Lines 700-703:
" …we take g_eff = 1 and λ0 = 656.28nm for the Hα line. For the LSD profiles of Ca IRT formed in the lower chromosphere. We have typical values of g_eff = 0.968 and λ_0 = 858.56nm, taking from the LSD process, and the g of each line is taken from the VALD."
The text "chromosphere. We" should be "chromosphere, we". The last "g" should be changed to "g_eff". The λ_0 and g_eff for the photospheric LSD lines should be given here. Were all the g_eff values taken from VALD? (If not then give the references.)

4. SI line 015:
"…exhibit a small…"
Maybe change to "… have small …"

5. SI line 081:
"r" and "p" should be in math font.

6. SI lines 107, 108 and 111:
The term "polarity reversal" is still used here. The authors should consider using another terminology.

7. Author's response, page 4:
"We did a preliminary test on the ZDI map of AD Leo for Ca IRT (middle chromosphere), showing that the main variation between photospheric and chromospheric fields is azimuthal and meridional fields."
The authors show a ZDI map of the lower chromosphere using only the three Ca IRT lines. The small number of lines,

compared to the number used for the photosphere, raises the question of how reliable this map is. Furthermore, a well-known fact is that while the radial field can be reasonably well reconstructed using just Stokes V-profiles, especially the meridional field is not well reconstructed. But since this is outside the scope, there is no need to further discuss this.

(Remarks on code availability)
We reviewed the code when the first version was submitted, and did not see it necessary to repeat.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

(Remarks on code availability)

Reviewer #4

(Remarks to the Author)
The authors of the paper have considered in detail the objections which I noted in my original report. The authors have provided cogent replies to my objections.

I therefore now consider the paper acceptable for publication.

(Remarks on code availability)

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I thank the authors for answering my questions. I agree with the publication of the manuscript in its current form.
I was still not able to run the software without errors, but this may have to do with my specific configuration on my laptop, and is not relevant to my positive evaluation of the manuscript.

(Remarks on code availability)

- Reviewer #1's comments -

1.1 Reviewer #1: The authors use Stokes I and V data from two high-res spectropolarimeters, ESPaDOnS and NARVAL, for three active, mid to late M-dwarfs. The data come from the PolarBase data base, described in [54]. In this paper, the usage of PolarBase data for M-dwarfs is explicitly mentioned in Section 8, where the biases are discussed, which result from the selection of line masks for the used LSD models.
The LSD method works especially well when many lines can contribute to the signal. For the photospheric line, the authors gained a factor of about 10 using the LSD method. For the lower chromosphere, only three spectral lines could be used, and a S/N increase by a factor of 2 could be achieved (although I would have expected at most an increase by a factor sqrt(3), and not 2). For the middle chromosphere, the LSD method obviously could not be applied, although not mentioned explicitly in the paper.

Response:
We thank the referee for carefully examining the spectropolarimetric part of our work. We appreciate the thoughtful concerns about potential cross-talk in polarimetry detection.

In the Method section, we added more explanation to tell the difference between the lines we used for the measurements of the three layers. We used a more accurate value of ~1.6 for the signal-to-noise ratio improvement for Ca IRT. We also added a sentence explaining that we use H-alpha individually.

In SI, we added the S/N of all three lines (LSD, H-alpha, Ca IRT) for the magnetic field measurement for each observation in the observation journal Table S2-S4.

1.2 Reviewer #1: Although the LSD method is useful to increase the S/N ratio, it is not efficiently reducing instrumental effects when used with very few spectral lines only.
The small signals in Stokes V for the chromospheric lines make a careful consideration of the cross-talk from Stokes I to V mandatory. Semel et al. [52] describe this problem, and the approach of removing spurious polarimetric signals. In their Sect. 2 they state that one "... should be careful about crosstalk from intensity to circular polarization." This careful consideration is not present in the paper.

I lack in the paper a clear description of how the authors dealt with the issue of possible crosstalk from I to V. The shape of the Stokes V signals presented in Figure 1 for the lower and middle chromosphere could also be a result of this crosstalk. This suspicion is supported by the fact that the magnetic field strength derived for the middle chromosphere for all three stars is almost indistinguishable.

Before a discussion about the magnetic field configuration on the three M dwarfs is possible, the reliability of the underlying data must be confirmed.
The method section needs to be expanded to include a description of the crosstalk. An analysis similar to Sect 2.4. in [52] needs to be presented with the result clearly demonstrating that the presented chromospheric Stokes V profiles are not affected by the crosstalk from I to V.

Response:
We agree that the crosstalk should be carefully considered and presented in the paper. For brevity, we omitted these details from our initial manuscript. We now give the "Null" profiles of each *V* profile in Fig 1. They are comparison profiles that should include no real signal but only noise and crosstalk. We believe this new information can address the concerns about the reliability of the signal.

The apparent similarity in magnetic field strength in the middle chromosphere of the original Fig. 1 was coincidental. Subsequent analysis (see Fig. 2) confirms that the field strength varies significantly across epochs. In the revised manuscript, we replace Fig.1 with the observations of higher S/N and different field strength measurements.

Below, we provide more detailed theoretical explanations and observational evidence to show why the antisymmetric *V* profiles detected in H-alpha and Ca IRT are unlikely to be attributed solely to crosstalk.

- Theoretical aspect:

The crosstalk from I-to-V should maintain the symmetry of the I profile, and it cannot produce the anti-symmetric or more complex shape of the V spectra generated by the Zeeman effect of the magnetic field from the star.

Sect. 2.1. in Semel et al.[58] gives the expression of the error in V profile as a function of I profile:

$$\delta V_{obs} = M_{41} * I_{in}$$

The chromospheric lines H-alpha and Ca IRT can be seen as a single line within a short wavelength range, and M_{41} can be treated as a constant. Therefore, the crosstalk from I-to-V should be a linear superposition of I profile on V. In this scenario, if the V signal includes I-to-V crosstalk, the V profile should closely resemble the I profile, which can only be in a symmetric shape in our observation.

Additionally, Sect. 3.4 of Donati et al. 1997 [40] gives the optimal extraction of polarization spectra of spectropolarimeters ESPaDOnS and NARVAL, including the analysis and solution to address the concern of the reviewer. According to [40], one should check the "Null" profile to see if any potential signature in the polarization profile is indeed real. Details are described in the "Observational aspect" below.

- Observational aspect:

In the standard procedures to extract the spectropolarimetric data of ESPaDOnS and NARVAL, "Null" spectra are "obtained by pair-processing sub-exposures corresponding to identical azimuths of the retarding plate or Cassegrain bonnette" (Donati et al. 1997, Sect. 3.4), in parallel with polarized spectra. These "Null" spectra are not expected to contain any signal and should be pure noise if there is no spurious polarized signature in the V profile (PolarBase database, Petit et al. 2014 [60]). In practice, we can check if any significant signal can be seen in the "Null" profile and whether it is similar to the V profile in order to exclude spurious signatures. We checked all "Null" spectra of H-alpha, Ca IRT LSD, and photospheric LSD in our work, finding no significant spurious signal, and that is why we use these three lines to measure the magnetic field. We added the Null profile in Fig. 1 of the manuscript, and added sentences in the Method part.

In the left panels of Fig.A1 below, we provide the "Null" profiles (N hereafter) of the AD Leo data in Fig.1 in the manuscript. Additionally, there are many other chromospheric lines in the spectra, and some of them can be strong in I. We didn't use them to measure the magnetic field because their V profiles are affected by I-to-V crosstalk. As shown in the right panels of Fig. A1, we illustrate an example of how crosstalk appears in the V profile of Ca II K 393.37 nm (note that it is not Ca IRT), and an unexpected signal can be clearly seen in the Null profile.

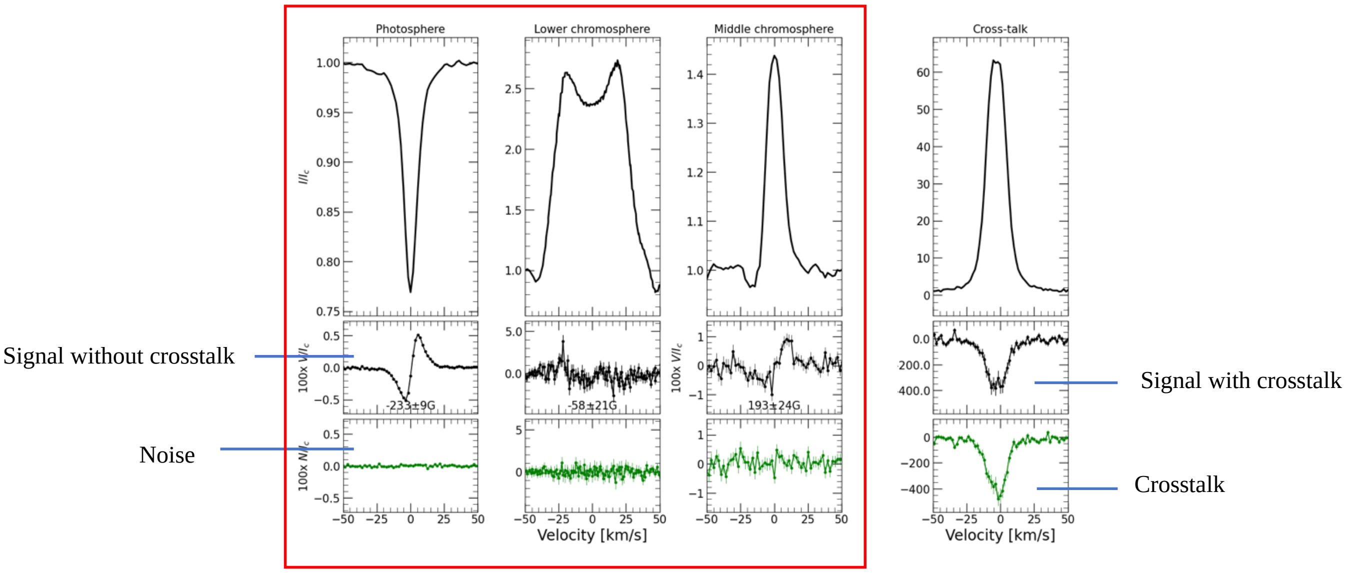


Figure A1. **(Left panels)** The I, V, and N profiles (green lines, N/Ic) of three layers (data identical to AD Leo in Fig. 1 of the manuscript). The N profiles should show random noise without any significant signal, and anti-symmetric shapes in the V profile can be detected even when the data is noisy. Therefore, the inferred uncertainty of the magnetic field can reflect the noise level. **(Right panels)** The I, V, and N profiles of the Ca II K 393.37 nm line from the same observation. A significant signal can be detected in the N profiles, indicating the presence of cross-talk in this line. A similar symmetric profile can also be observed in the V profile, which confirms that the V profile results from spurious signatures (including crosstalk).

2. Reviewer #2 (Remarks to the Author):

The magnetic activity of M-dwarfs is an important astronomical question for two major reasons. Firstly, it aids the understanding of the stellar activity, as these stars cover a different region in the stellar parameter space than the Sun. Any general dynamo theory should be able to explain the activity of both the Sun and other late-type stars. Secondly, most potentially habitable exoplanets are expected to orbit late-type dwarfs. Stellar magnetic activity is known to have a great impact on habitability. This is especially for M-type dwarfs, as the habitable zones are much nearer to the host star, than in the case of the Sun

The manuscript "Measuring the magnetic fields in the upper atmospheres of low-mass stars" is a study of the changes in the large-scale magnetic field of three M-type dwarfs from the photosphere to the mid chromosphere. The authors apply a novel approach, where they study the V-spectropolarimetric signal of lines originating in the photosphere, lower and mid-chromosphere. This approach reveals how the large-scale magnetic field changes through the atmosphere of these stars. In this respect, the study is truly novel, and represents a major step forward in understanding the magnetic activity of M-type dwarfs. The reviewers are not aware of any previous studies, where such methods have been applied on these kinds of stars. The analysis is largely based on studies of the solar chromosphere. This is, of course, the only reasonable approach, although there are indications that the solar analogy cannot always be applied on stellar magnetic activity of more active stars (e.g., Kochukhov et al., 2023, A&A 680, L17).

We do not find any flaws in the analysis, other than the missing significances of the correlations (see below). The fact that the photosphere contributes to the Ca II IRT lines is challenging, but this seems to be properly addressed. However, we suggest changes in some of the conclusions.
Our recommendation is that this study should be published in Nature Communications, but after some significant revision detailed below.

Response:
We thank Reviewer #2 for the thoughtful and encouraging comments. We have carefully revised the manuscript accordingly: (1) small-scale fields are carefully considered in the explanation of the results; (2) a PFSS extrapolation based on an existing ZDI map has been added to illustrate why simple extrapolations cannot account for our chromospheric measurements; and (3) wording and minor issues have been clarified throughout. We believe these changes fully address the reviewer′s concerns and strengthen the paper.

I. Major issues:

2.1.1 Reviewer #2 : A major critic on the manuscript, is that the authors do not enough address the small-scale magnetic field in the objects. The small-scale field is actually only mentioned once in the manuscript (line 108). This is, of course, partly outside the scope of the study, since measuring the small-scale field vector is way beyond the achievable resolution. Still, it is a well-known fact, that most of the magnetic energy will reside in the small-scale field (see, e.g. Kochukhov et al., 2020, A&A 635, A142). Furthermore, estimates of the percentages of the large-scale vs. total magnetic field are available for the three targets in this study (see Kochukhov, 2021). The small-scale field of the Sun is particularly important in understanding the formation of sunspots, active regions, flares and CME:s. The same would be expected for M-type dwarfs. Addressing this question could easily be done by reformulating relevant sections in the manuscript. Furthermore, the ratio between the large-scale and total magnetic field for the targets could be listed in a relevant table.

Response:
We thank the reviewer for highlighting the importance of the small-scale magnetic field. In the revised manuscript, we include the small-scale magnetic field as a potential explanation of magnetic field topology at different heights.

We expand the introduction (l. 88-90) and discussion (l.222-225, 236) to explicitly highlight the importance of small-scale magnetic fields in stellar magnetic activity. In the SI, we include the percentages of the large-scale vs. total magnetic field from Kochukhov (2021) in Table S1.

These additions clarify the role of the small-scale magnetic field and its connection to our chromospheric results.

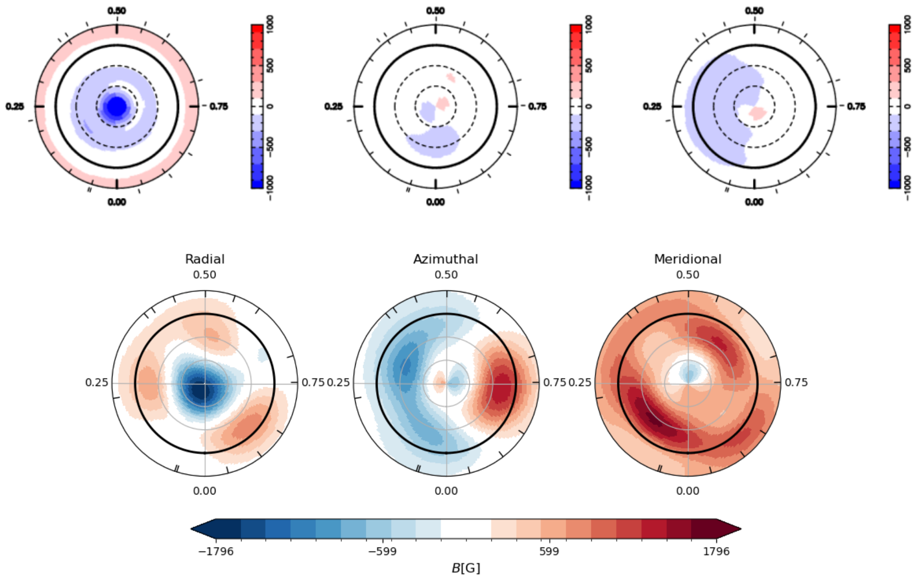
2.1.2 Reviewer #2: In many places in the manuscript, the authors write about polarity reversal and changing magnetic topology with height. On the other hand, at least some of the observations can be explained by reasonably simple models (S8) involving large-scale magnetic field loops viewed from different angles. Thus, the different field topologies would rather be an apparent phenomenon depending on geometry and viewing angle. Usually in the context of active late-type stars, polarity reversals refer to possibly cyclic changes in the large-scale radial field, and changing topology is used to describe, e.g., the variations in the percentages of poloidal vs. toroidal and axisymmetric vs. non-axisymmetric field. Thus, the authors should clarify this difference and consider using other wording.

Response:
We appreciate the suggestions for using more precise terminology to describe our observations and interpretations. We agree that clarifying our use of terms like "polarity reversal" and "changing magnetic topology" will enhance the clarity and accuracy of our discussion.

We replaced instances of "polarity reversal" with "opposite sign" to more accurately describe the observed changes in the direction of the magnetic field. This term better conveys the idea that the magnetic field switches from one polarity to another without implying a dynamic or time-dependent reversal.

As the reviewer mentioned, "changing magnetic topology" should relate to the change of energy distribution of the magnetic map. We did a preliminary test on the ZDI map of AD Leo for Ca IRT (middle chromosphere), showing that the main variation between photospheric and chromospheric fields is azimuthal and meridional fields, with a lower percentage of poloidal and axisymmetric fields. Since the magnetic field should be continuous from the photosphere to the chromosphere, we believed that it is reasonable to use 'topology change' to describe different patterns of magnetic field variation at various heights.

We won't include this ZDI result because it is outside the scope of this work, and the different magnetic field



variation patterns can lead to a similar conclusion. We think these results can address this concern.

2.1.3 Reviewer #2: Concerning the correlation between the magnetic fields in different atmospheric layers, the situation is not as clear, as the text in the manuscript seems to indicate. For YZ CMi the case looks quite clear, and the strong correlation can be seen in the plot S6. For AD Leo, the correlations between the chromosphere and photosphere (S5 panels a and b) are rather mixed, and it is hard to visually judge how significant the correlations within the different epochs are. The case is the same for EV Lac (S7, panels b and c). Especially in cases where the number of points is relatively small, correlations should always be given with estimates of significance. Such estimates should definitely be included in this study.

Response:
We replace the less clear description with a more neutral formulation (l. 189-191). We added a new table (Table S5) about the correlation's performance with *p* and *r*, where the *p*-value gives the significance of correlations and *r* is the correlation coefficient. The *p*-values of all data indicate a high significance (0-0.05) of the correlation for EV Lac and YZ CMi and moderate significance for AD Leo (0.05-0.17).

2.1.4 Reviewer #2: In most cases, the data would be sufficient for Zeeman-Doppler imaging (ZDI). At least for some of this data, ZDI maps have been calculated (e.g., Morin et al. 2008). We acknowledge the point (line 90) that extrapolating the magnetic fields from ZDI maps to the chromosphere will probably not give a valid result. This would be best demonstrated by using a ZDI map of one of the targets, extrapolating the field, and comparing this with the current measurements. Did the authors consider doing this?

Response:
We thank the referee for the suggestion. Since the chromosphere is very close to the photosphere (0.3% R*), the extrapolated field with the PFSS method is almost the same as the ZDI map of the photosphere. Below, we provide a comparison between the photospheric radial field of AD Leo and PFSS at ~1.003R*, and this

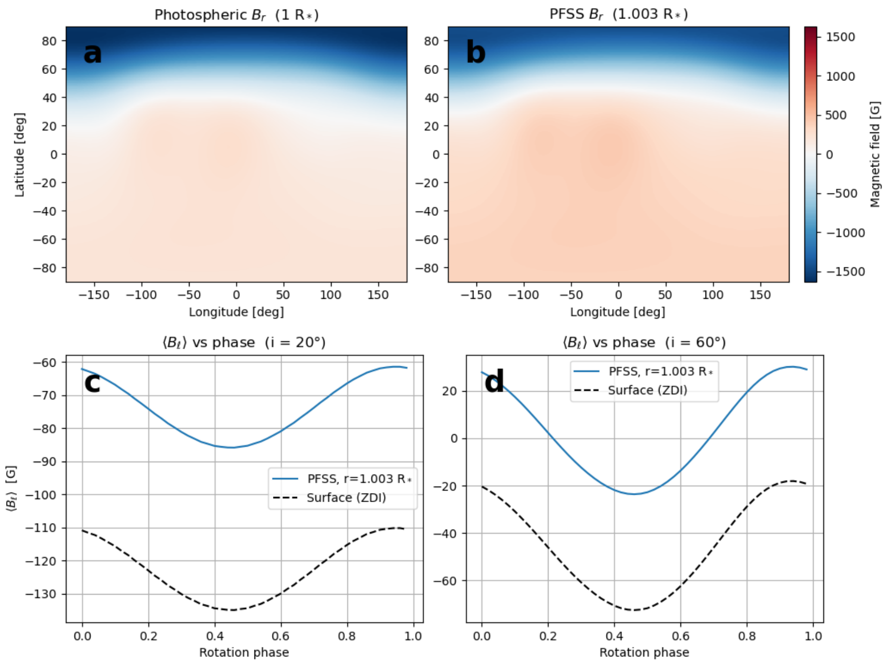


figure is included in the revised manuscript as Figure S5.
We can see a significant difference between $\langle \boldsymbol{B_l} \rangle$ at different heights in (c), and the opposite sign can be reproduced with a larger inclination in (d). However, this extrapolation shows almost no variation in the pattern, indicating a correlation coefficient close to 1.

Here is an estimation of the change of three components of the magnetic field from the photosphere to the chromosphere for AD Leo. Consider the PFSS equations from Lang et al. 2014 :

$$\boldsymbol{B_r = -\sum_{l=1}^{N}\sum_{m=1}^{l}[l\alpha_{lm}r^{l-1} - (l+1)\beta_{lm}r^{-(l+2)}]P_{lm}(cos\theta)e^{im\phi}},$$

$$\boldsymbol{B_\theta = -\sum_{l=1}^{N}\sum_{m=1}^{l}[\alpha_{lm}r^{l-1} + \beta_{lm}r^{-(l+2)}]\frac{d}{d\theta}P_{lm}(cos\theta)e^{im\phi}},$$

$$\boldsymbol{B_\phi = -\sum_{l=1}^{N}\sum_{m=1}^{l}[\alpha_{lm}r^{l-1} + \beta_{lm}r^{-(l+2)}]P_{lm}(cos\theta)\frac{im}{sin\theta}e^{im\phi}}.$$

Taking the ZDI result of Morin et al. (2008), ~60% energies in $\boldsymbol{\ell = 1}$, ~10% energies in $\boldsymbol{\ell = 2}$, and ignoring higher orders, we can roughly estimate the field change from r=1 to r=1.003 by less than 1% for each component. Since the variation is more or less the same for each $(\boldsymbol{\theta, \phi})$, meaning that we cannot have a significant change in the topology by extrapolating the field, especially the radial field. As shown in the ZDI of chromosphere result shown in the preview comment, we should expect a substantial change of $\boldsymbol{\beta_{lm}}$ and $\boldsymbol{\gamma_{lm}}$, which is not considered in the extrapolation.

2.1.5 Reviewer #2: The question of differential rotation is not discussed at all in the manuscript. This is apparently because in previous studies, none of these stars show indications of any strong differential rotation (Morin et al., 2008). However, on scales of months, even a very small differential rotation may have a significant effect. Did the authors consider differential rotation? The possible shear could easily be tested using, e.g., the estimates (with error bars) derived by Morin et al. (2008). In the case of YZ CMi the measurements follow quite well a double sinusoidal fit (S3). Here the stability (against, e.g., differential rotation) of the large-scale magnetic field could be tested by checking whether there is a correlation between the deviations of the points and time difference from the mean HJD.

Response:
As suggested by the reviewer, we tested the differential rotation of YZ CMi in our work with the uncertainty list in Morin et al. (2008) of $\boldsymbol{d\Omega = 0 \pm 1.4 mradd^{-1}}$ and $\boldsymbol{P = 2.779 \pm 0.004d}$. By setting different HJD0 from 2453950 (we used) to 2454800 for the 2008 epoch (around 2454500), $\boldsymbol{d\Omega}$ at $\boldsymbol{\chi^2}$ minimum slightly increase with HJD0, with a maximum $\boldsymbol{d\Omega < 0.7 mradd^{-1}}$, which is smaller than the uncertainty. Applying this value is not significant enough to impact the modeled Stokes V profile. Furthermore, the double sinusoidal fit also works well for AD Leo and EV Lac, suggesting that this fitting is unlikely to be caused by the sheared map (we add a double sinusoidal fitting for all three stars in Fig. 2). As the differential rotation of the photospheric field is not in the scope of this work, and exploring its potential effect on the chromospheric field would be complex, we tend to neglect the impact of differential rotation, but add a sentence to mention it in the revised manuscript.

2.1.6 Reviewer #2: A small, but principally important question is whether the dynamos of fully convective stars differ that much from the solar dynamo (lines 107-8). Here, there are competing paradigms, which lead to different claims. In the flux transport dynamo paradigm, the tachocline plays an important role, which would mean that fully convective stars would have a different type of dynamo. However, the distributed dynamo paradigm does not rely on a tachocline. There are, as far as the reviewers know, no observations indicating a significant change in dynamos between fully convective stars, and stars with a convective envelope and radiative core. This would support the latter paradigm. Here, we do not demand the authors to choose a side, but rather request that this sentence should be reformulated to accommodate both possibilities.

Response:
Thank you for pointing out the debate about whether the dynamo of M-dwarfs is similar to the Sun. We have reformulated the sentence to mention both possibilities (l.108-110): "many active M-dwarfs are fully convective stars, but it is still debated whether their dynamos rely on a tachocline, which is different from the solar dynamo."

II. Other comments and suggestions for the main paper:

2.2.1 Reviewer #2: Age estimates for the three stars are not given anywhere in the manuscript. If estimates exist, we suggest they should be included in the manuscript. As for the stellar parameters, these are taken from the previous study by Morin et al. (2008). We note that there are some discrepancies in the values of v sin i, P_rot and i for EV Lac and YZ CMi: It seems that the inclination for these two targets should be larger than 60 degrees. Did the authors consider this? On the other hand, we do not expect that this would alter the results.

Response:
We add the age estimation listed in Engle 2024 (ApJ, 960, 1, id.62) in Table S1.

Other parameters can calculate inclination larger than the given value in Morin et al. 2008 since they used 60 degrees to prevent mirroring effects in ZDI. We did a Chi2 minimum test to check the inclination of EV Lac and YZ CMi. The exact value of inclination is not sensitive to the results between 50 and 80 degrees.

On the other hand, the uncertainty of vsin *i* measurements in Morin et al. (2008) is about 1 km/s, which would result in a large uncertainty estimation of inclination. The vsin *i* value of YZ CMi and EV Lac is >12% smaller than that of Morin et al. (2008) with CARMENES observations (Tal-Or et al. 2018), which results in a smaller estimation of inclination.

As commented by the reviewer, the inclination value would not alter the results. Considering the larger uncertainty of inclination estimation, we tend to keep the value from Morin et al. (2008) to have a consistent stellar parameter source.

2.2.2 Reviewer #2: The exposure times are not given for the observations. We urge the authors to report these, or at least their range. It would also be good to give a range for the S/N, rather than the typical noise level (line 588) for all measurements, i.e. the photospheric LSD lines, the CaII IRT and Hα.
It is often a custom to present null profiles of Stokes V-spectropolarimetry. We would encourage to include examples of these for each star in a similar way as in Fig. 1.

Response:

We updated the observational log Table S2-S4 of three stars, including information on exposure times and signal-to-noise ratio of Stokes V observations. We included the Null profile in Fig.1 and some explanations of it in the Method section.

2.2.3 Reviewer #2: In the abstract (line 53) and elsewhere in the manuscript, the concept of ″direct" measurement is used. We would refrain from using this wording here, as the methods used involve modelling. Still, this would not reduce the importance of this study, as any estimate of the magnetic field above the photosphere for these targets is novel.

Response:

We used "direct" since the polarimetric signal is the most direct method to detect the magnetic field in solar and stellar magnetic fields. We acknowledge that it may not be precise for the measured values. We rephrase the sentences and remove "direct".

2.2.4 Reviewer #2: On lines 72-73 we suggest replacing ″through the photosphere" with ″to the photosphere".

Response:

Done

2.2.5 Reviewer #2: On lines 78-80: "… measuring the magnetic field of the upper atmosphere, including the chromosphere, is essential to understanding the physical mechanisms responsible for stellar coronal heating and eruptions." This is certainly true, but in the Sun the small-scale field is crucial for understanding these phenomena. One can expect that the same holds for other late-type stars.

Response:

We added more discussion about small-scale fields in the revised manuscript as mentioned in Comment 2.1.1.

2.2.6 Reviewer #2: On line 91 there are three references. Comparisons between extrapolation and measurements are included in Yang et al. (2020). But are the other references also relevant here?

Response:

The other two references, Ishikawa et al. (2021) and Solanki et al. (2003) do not confirm this result directly but provide indirect results to support the variation of magnetic loops. We change the reference here for more accuracy.

2.2.7 Reviewer #2: On line 96: In ″M-dwarfs generally have a lifetime longer", the word ″generally" seems unnecessary, since M-dwarfs always have orders of magnitude longer lifetimes than the Sun.

Response:

We remove "generally" here.

2.2.8 Reviewer #2: On lines 98-99: ″These conditions …" should be rephrased. This is not only a question of the number and ages of M-dwarfs, as each M-dwarf in the solar neighbourhood seem to have more planets than earlier type stars.

Response:

We remove "These conditions imply that", and just state the fact that "There is a higher chance of finding habitable planets around M-dwarfs than earlier type stars."

2.2.9 Reviewer #2: On line 107: "believed to be fully convective" should be rephrased, as this is an established fact.
Paragraph starting on line 111: We suggest considering to first explain the theory, i.e. the formation of the spectral lines in the different layers, and then the observations.
Response:
We followed the suggestions, rephrased the sentences (l.108-110), and added information on line formation (l. 113-116).

2.2.10 Reviewer #2: On line 140, reference [41] should probably be [40].
Response:
It is right, we have corrected this.

2.2.11 Reviewer #2: On line 153, the wording "the stars were nearly stable within an epoch, with a timescale of at least one month" raises the question on what ground this epoch length was chosen. Maybe this could be described with a sentence?
Response:
We modified l.151-152 as suggested.

2.2.12 Reviewer #2: On lines 164-167 the wording "the variation in polarity and strength at different heights can be attributed to the drastic variation of corresponding large-scale magnetic field structures with height" should be rephrased. Do the observations really support "drastic variations", or can they be explained by magnetic field loops of different heights and viewed from different angles?
Response:
We acknowledge that, according to the simulations of PFSS results, even a small variation can result in our observation. We have now reformulated the sentences.

2.2.13 Reviewer #2: On lines 215-218, the sentence "This scenario also indicates … intense UV and X-ray radiation" is, of course, true. However, again most of the magnetic energy is known to be contained in the small-scale field, which is also probably contributing much more to flares and CMEs.
Response:
We add the possibility of small-scale fields in l. 218-221.

2.2.14 Reviewer #2: We suggest adding "LSD profile", "Hα", and "Ca II IRT" to the plot titles in Fig. 1.
Response:
We followed the suggestion and modified the plot titles in Fig.1

2.2.15 Reviewer #2: On lines 571-572, it says "rotational periods have been estimated from the modulation of the photospheric Zeeman signatures". However, in the paper by Morin et al. (2008) P_rot was determined in the ZDI inversions.
Response:
Thanks for pointing out this unclear expression. We tried to avoid the word "ZDI" in the original manuscript for potential readers who are not familiar with this method. Now that we add an explanation about this

2.2.16 Reviewer #2: On lines 617-618. The g_eff for the chromospheric lines are given. References for these values would be useful.
Response:
We added explanations of the g_eff of Ca ITR at l. 651. More detailed information can be found in the corresponding .lsdout of each observation in the data code.

III. Comments and suggestions for the supplement:

2.3.1 Reviewer #2: On line 15, the sentence "exhibited a small projected rotational velocity, resulting in low v sin i values" is a tautology and should be rephrased.

Response:
We have removed "projected rotational velocity, resulting in low" to simplify the expression.

2.3.2 Reviewer #2: On line 40-43, the claim "generally form one repeating pattern in the phase-folded diagram" is only half the truth. For YZ CMi, this is clearly the case. But for AD Leo, there does not seem to be any clear pattern. For EV Lac, the pattern is quite vague especially for the lower chromosphere. So, this claim should be rephrased.
Response:
We have rephrased the expression by adding 'For AD Leo EV Lac, their patterns are less clearer than YZ CMi, but they can be fitted by double sine function as shown in Figure 2'.

2.3.3 Reviewer #2: On lines 49-50, the statement "large-scale photospheric structure appears more axisymmetric (axisymmetric modes > 90%) in AD Leo than the other two stars" may have to be rephrased. At least in Morin et al. (2008), AD Leo and YZ CMi have nearly the same axisymmetry.
Response:
We agree the comment of reviewer. We have removed this paragraph since it is not helpful to support the explanation of the large-scale magnetic field with height. New Figure S5 and l.57-62 have provided related information.

2.3.4 Reviewer #2: On lines 88-89, the sentences with "with m giving the order of the spherical harmonics mode and describe. In practice, $\alpha\ell m$ is complex coefficients corresponding to the radial poloidal field" seem to have some words missing. Correct also the "m" to the right format.
Response:
Thanks for pointing out the typo, and we have added more details about $\alpha\ell m$.

2.3.5 Reviewer #2: On line 95, correct the format of B_l.
Response:
We now use uniform symbols to represent the values of B_l.

2.3.6 Reviewer #2: On lines 116-118, there is a clear need for references. With what method were the flares detected and their energies determined?
Response:
We detected the flares from TESS data using the method described by Xing et al. 2024. We added the references and provided more information on these flare detections.

2.3.7 Reviewer #2: On lines 123-124, the sentence "It is still unclear why there are fewer low-energy flares in the situation of more complex fields", should be rephrased to take into account the small-scale field.
Response:
We remove old expressions and add the possibility that small-scale fields can convert the energy of low-energy flares to magnetic dissipation (l. 124-127).

2.3.8 Reviewer #2: In Fig. S1 axis titles and units are missing in the plot.
Response:
We added the missed titles and units.

2.3.9 Reviewer #2: In Fig. S3 the units (d) are missing from the scale on the right side.
Response:
Fixed.

2.3.10 Reviewer #2: In the extended data table S1, references for the stellar masses would be useful. Also, the parameter ß could be explained also here.
Response:
We have reformed Table S1, and these values have been included.

2.3.11 Reviewer #2: Several corrections should be made in Tables S2-S4. Firstly, the HJD and phase-values should be given with consistent accuracy. Here the possible effect of the exposure times should be considered.

Furthermore, using the value ″-999.0” when the number of measurements is insufficient is not a good choice, better to use a blank or ″N/A” in these cases.

Response:

We used '-' to represent an invalid value. The statistical information has been moved to a new table, Table S5.

2.4 Reviewer #2 (Remarks on code availability):

Reviewer #2: The codes worked fine when downloading. However, some comments are written with Chinese letters, making them challenging to comprehend.

Response:

We now use English everywhere in the code and have added more comments. The new version of the code, including the scripts for new Fig.1 and Fig.2, has been uploaded.

3. Reviewer #3 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

4. Reviewer #4 (Remarks to the Author):

4.1 Reviewer #4: The authors report measurements of intensity profiles and circular polarization profiles for spectral lines which are formed in principle at three different altitudes in the atmosphere of a flare star. They interpret those line profiles in terms of magnetic field strength (including algebraic sign of the longitudinal component) at the three different altitudes. The reported field strengths are probably to be interpreted as "mean" fields at the various altitudes, although the word "mean" can hardly be interpreted as a simple arithmetic mean over the visible disk of the star at the time the data were obtained: with 3 different vector components of the field present at all locations of the stellar surface, the "mean" must involve a complicated process.

Response:

We appreciate the reviewer's thoughtful comments about the interpretation of "mean" fields in our measurements. We would like to clarify the established nature of the "mean longitudinal magnetic field" concept in stellar magnetic field studies.

The term "mean longitudinal magnetic field" (Bl in stellar physics), Solar Mean Magnetic Field (SMMF), and General Magnetic Field (GMF) have been standard tools for characterizing stellar magnetic fields (Babcock 1949) and solar magnetic fields (Hale 1913) for over a century. This approach remains widely used in studying magnetic fields of stars (Kochukhov 2021) and the Sun (Rescigno et al. 2024).

Our measurements follow established procedures in stellar polarimetry (Donati & Collier Cameron 1997, detailed in the Method section). The mean longitudinal field represents a "disk-integrated" quantity (as discussed in Kochukhov 2021, Sect. 2.3). For stars with minimal spatial resolution in spectral observations, this is often the only accessible magnetic field parameter. While cancellation effects are inherent to this approach, it effectively characterizes stellar (include Sun-as-star) magnetic strength levels (Saar 1996, Donati & Landstreet 2009, Reiners 2012, Bose & Nagaraju 2018), and interplanetary magnetic fields (Scherrer et al. 1972, Bilenko 2020, Antonucci 2020).Multiple measurements at different rotational phases further allow characterization of the star's large-scale magnetic topology (Donati & Landstreet 2009, Reiners 2012, Kochukhov 2021).

We have clarified the measurement methodology and physical interpretation of this quantity in the revised manuscript (lines 622-630).

4.2 Reviewer #4: The authors claim that, because they have used three lines which are formed at different altitudes, their results allow them to state confidently how the field strength varies as a function of height. But this seems improbable: the 3 lines are formed at heights which are separated by no more than (perhaps) 0.3% of the stellar radius: given that the fields in the star are generated by a global dynamo, it is highly unlikely that any significant changes in "physically real" field strength can be expected to occur over such a tiny range of radial distance.

Response:

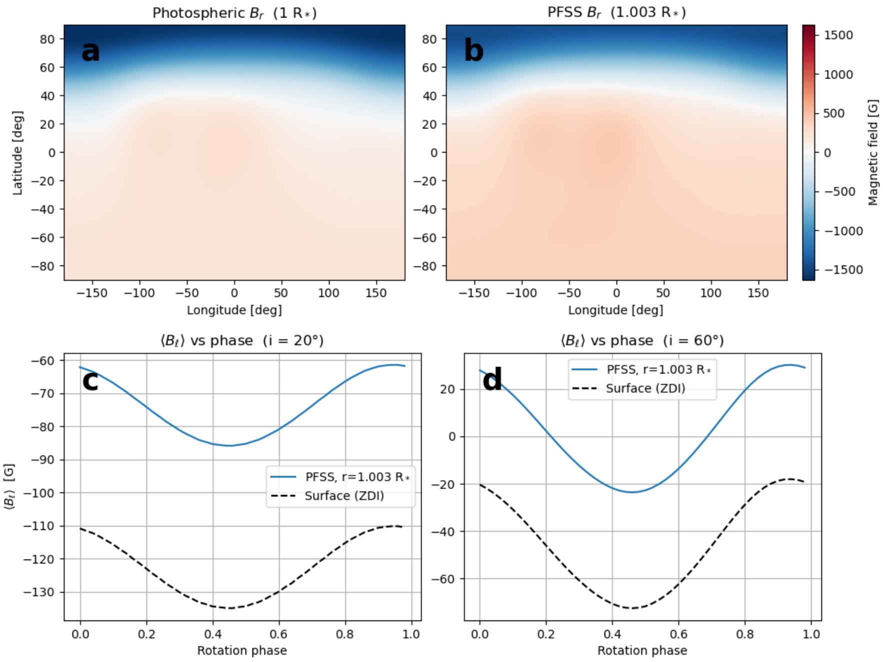
There might be a misinterpretation of the physical aspect of the term ʺmean longitudinal measured field," as a continuing concern regarding comment 4.1. To our knowledge, although the magnetic field is generated by stellar dynamo, it can present a complex structure close to the surface, even for a small height (complexity of chromospheric field can refer Lagg et al. 2017, Sect. 5.2)**.** Below, we would like to emphasize the observational evidence from the Sun that this quantity can always vary significantly from the photosphere to the chromosphere.

For instance, Ishikawa et al. (2021) used observations from the space spectropolarimeter CLASP2 to map the variation of the magnetic field from the photosphere to the upper chromosphere of the Sun. This observation reveals significant variations in the longitudinal magnetic field at different altitudes, similar to our work. Moreover, the SMMF of the chromosphere is, on average, around 0.6 SMMF of the photosphere, and it also changes significantly with solar rotation (Vishnu et al. 2023). The polarity reversal of the longitudinal magnetic field between the photosphere and the chromosphere is also revealed by observations of the solar active region (Mathur et al. 2023).

Explanations of these phenomena in the solar context typically relate to the rapid spread of magnetic field lines in the chromosphere (Solanki 1992), which does not conflict with the concept of ʺglobal dynamo.ʺ This phenomenon can be modeled through a high-resolution 3D radiative MHD simulation (Martínez-Sykora et al.

2019) and is in line with the Fontenla-Avrett-Loeser (FAL) atmospheric model (Pietrow et al. 2020). And small-scale magnetic field simulation in fully convective stars (Yadav et al. 2021).

To further clarify this problem, we provided the PFSS modeling of the AD Leo observation in the selected epoch, as new information in SI 1B and Figure S5 (also shown below). The radial field of the photosphere and the PFSS field at (1.003R*) can be similar (as commented by the reviewer), but the measured LOS field can have an opposite sign. The performance of this result is similar to what we observe with YZ CMi, meaning that even if we consider a "global dynamo", we can still observe the significant change of the mean longitudinal field.



4.3 Reviewer #4: The authors report (with great confidence) in Tables S2-S4 that the field actually changes sign between photosphere and low chromosphere, or between lower and mid chromosphere. But I find this hard to believe: their confidence, it seems to me, is not warranted when I see the serious "noise" level in the V profiles which they show in Figure 1 in the text (for Halpha and also for CaIRT) and also in Figure S2 (for Halpha).

Response:

We appreciate Reviewer #4′s concern regarding the apparent noise in the Stokes V profiles and its impact on the accuracy of our longitudinal field measurements. We acknowledge that Figure 1 and S2 do not necessarily represent our highest signal-to-noise (S/N) data, and we have used figures with above-mean S/N in the new Figure 1, as well as the corresponding comparison profile (Null profile).

We want to stress that our measurements of field strength apply the center-of-gravity (COG) method, and random noise in individual data points is minimized through an integration across the entire line profile. The uncertainty of the measured longitudinal field is then derived using standard error propagation principles. Therefore, the uncertainty we present with the measurements correctly reflects the noise level.

For further confirmation of the observational evidence of opposite sign of fields, even in cases where the S/N is weak, the antisymmetric shape of the Stokes V profile provides a robust indicator of the field′s polarity via the Zeeman effect (Degl′Innocenti & Landolfi 2004). Practically, we assign a value of a=1 emission line in Stokes I (and a=-1 for absorption); while b=1 denotes a sine-like shape in the V profile (blue side low, red side high) and b=-1 indicates its inverse. The product a*b then determines the sign of the magnetic field (+1 for positive polarity and -1 for negative). Figure 4.3.1 illustrates this reliable yet straightforward procedure, underlining how we can accurately identify polarity—if we can recognize the antisymmetric shape in V—and thereby establish confidence in any observed ״opposite sign”.

Below, we provide two spectra in Figure 4.3.2 with lower apparent noise, which could better address the concern of sign reversal between the photosphere and lower chromosphere (AD Leo - 2007-06-25); between the lower chromosphere and middle chromosphere (EV Lac - 2005-09-18). These examples provide additional confirmation of the polarity reversals inferred by our measurements.

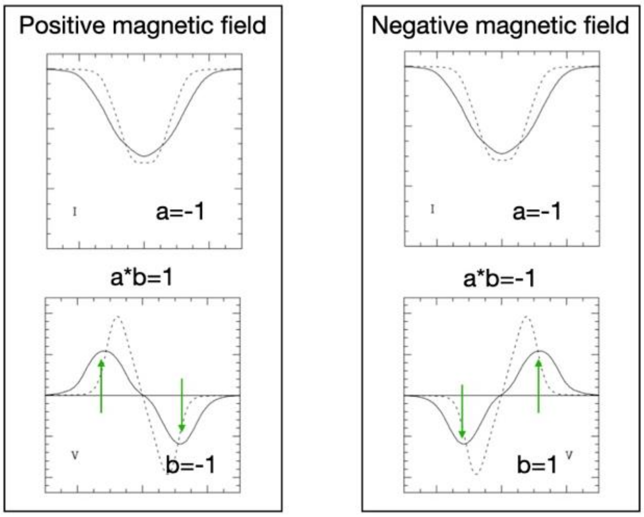


Figure 4.3.1. Sketch for the simple method to determine the sign of the magnetic field. The samples are modified from Degl'Innocenti & Landolfi (2004) Fig 9.18. The upper panels show Stokes I profiles, while the lower panels display the corresponding Stokes V profiles with the Zeeman effect. Solid and dashed lines represent different magnetic field situations, which do not affect the sign determination. Green arrows highlight the antisymmetric shape of V profiles. By taking a = -1 for the absorption line of Stokes I and b = -1 for the negative sine-like V profile (left), we can determine a positive sign for the magnetic field using a * b = 1. Conversely, we obtain a negative sign with a = -1 and b = 1 in the right panels.

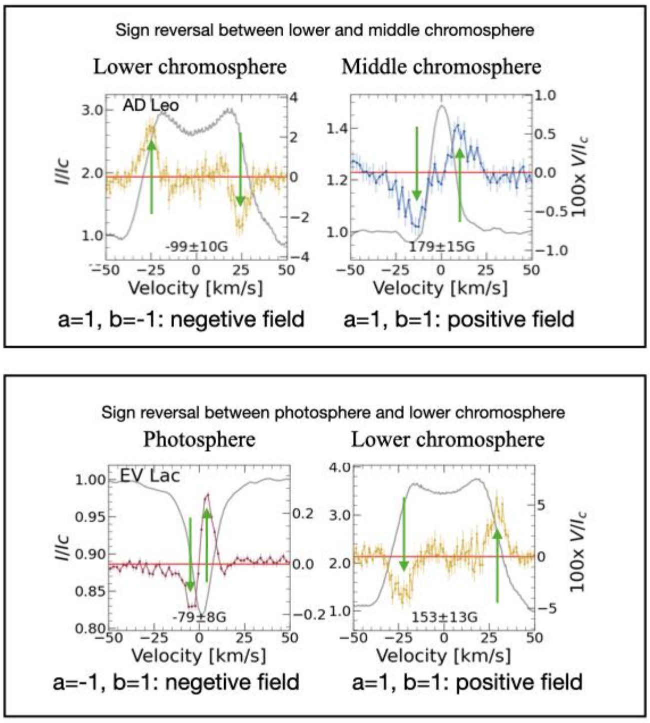


Figure 4.3.2. Evidence of sign reversals between layers, determined by the simple method shown in Fig. A2. Grey lines are Stokes I profiles, colored lines are V profiles, red horizontal lines mark the zero flux of V profiles. Green arrows highlight the antisymmetric shape of V profiles. (Upper panels) Polarity reversal between the lower chromosphere (negative field) and the middle chromosphere (positive field). (Lower panels) Polarity reversal between the photosphere (negative field) and lower chromosphere (positive field).

We thank all reviewers for their careful evaluations and constructive feedback. We are pleased that several of the major concerns have been addressed, and the manuscript is now recommended for publication pending minor revisions.

REVIEWER COMMENTS

Reviewer #1:
1.1 I thank the authors for addressing my criticism about the reliability of the Stokes signals. The Null-spectra analysis is indeed a good method to confirm that the measurements are not affected by crosstalk. I agree with the authors and find my point (1.2 Reviewer #1) sufficiently addressed.

However, I was surprised to see the big changes in Figure 1 between the original and the revised submission. The authors say that the "apparent similarity in magnetic field strength in the middle chromosphere of the original Fig. 1 was coincidental", and present "observations of higher S/N and different field strength measurements".
Where do these higher S/N observations come from? How is it possible that the new values for the field strengths are far outside the one-sigma values given in the original version?
I did not find a mention in the paper about these new observations. The magnetic field values in the new version are vastly different from the old version. This is not raising confidence in the results, and I expect a clear explanation for this discrepancy.

In Figure 2 the authors added a Fourier series fit to the data, "showing the strong periodical signal of the magnetic field variation". I see at most one period, therefore one cannot make a statement about how 'strong' the periodicity is.

As in the previous report, I refrain from commenting on the stellar physics aspects of this paper.

Response:
Thank you for the evaluation of Figures 1&2 and for raising the questions.

In this work, we have used observations of these three stars from 2005 to 2019. In the Supplement Materials, Tables S2-S4 give the Journal of observations, including the signal-to-noise ratio (S/N) of line profiles of each observation and corresponding magnetic field measurements. For each star at each atmosphere layer, we simply show one line profile as an example in Figure 1.

In the original version of the manuscript, we set mainly four criteria on selecting the profiles shown in Figure 1, in order to include as much information as possible for readers:

1) **The profile should be observed in the selected epochs (2008 for AD Leo and YZ CMi, 2007 for EV Lac).**
2) **The polarity signals should be simple enough to recognize the positive and negative polarities.**
3) **The photospheric field and the lower chromospheric field should have the same polarity.**
4) **The lower and middle chromospheric fields should have opposite polarities.**

Under these conditions, we finally chose the line profiles of AD Leo observed on 2008-02-10, YZ CMi on 2008-02-11, and EV Lac on 2007-07-29 to show in Fig.1. The S/N of these profiles is moderate due to the exposure time and weather conditions. Across all of our measurements, only a few observations reveal a middle chromospheric field close to 195G, but we have coincidentally selected three of them.

In the current version, we think that there is no need to meet the above four criteria when selecting example profiles for Fig. 1. So we decide to show line profiles with higher S/N in Figure 1 (AD Leo on 2007-06-25, YZ CMi on 2007-01-27, and EV Lac on 2006-08-09), for better illustration of the observed polarity signals in the chromosphere. The dates of observations are mentioned in the caption of Figure 1.

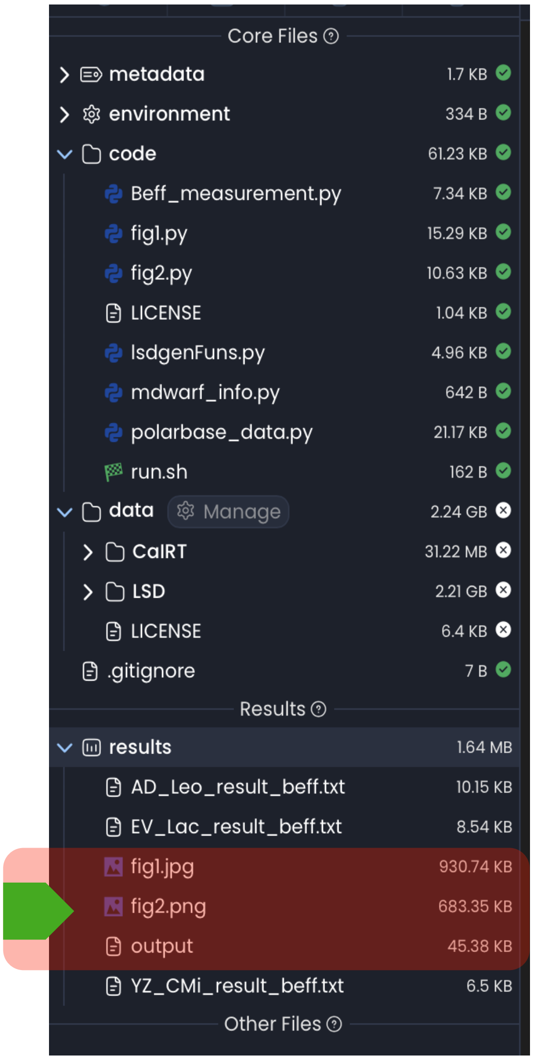
We agree that "strong" may not be suitable in the sentence, so we have removed it.

Reviewer #1 (Remarks on code availability):

1.2 I tried to run the code in Docker Desktop for MacOS (M2, 15.5 (24F74)). Unfortunately, the code did not produce any output and stopped with many error messages. I attached the logfile of my run (output.log).

Response:
Thanks for evaluating the reproducibility of the code. The code works on Visual Studio Code and MacOS (M1Max, 15.6), which should not create much difference between (M2, 15.5). We tested the code with Python 11.

We reviewed the output.log provided by the reviewer, and it is identical to the 'results/output' file in the CodeOcean framework (as shown in the figure on the right). The messages are all normal output information and warnings (mostly related to missing fonts). By default, the result figures should be generated under the 'result' folder (/fig1.jpg and /fig2.png), as shown in the right figure. Please let us know if these files are generated on your computer.



<u>Reviewer #2:</u>
As stated in the first review, the manuscript presents a novel approach to estimate the magnetic field and its polarity in different atmosphere layers of three M-type dwarfs: The photosphere, the lower and the mid chromosphere. This represents a significant contribution to our knowledge of stellar magnetic activity in M-type dwarfs.

We thank the authors for taking into account the main critics from our previous report and improving the manuscript accordingly. We thus recommend that the manuscript would be accepted for publication in Nature Communications with minor revisions. The suggested revisions are listed below.

Response:
We are very grateful to the reviewers for their positive scientific assessment of our work. Their careful review of the manuscript has greatly helped us improve its quality.

2.1. Line 111-112:
"… whether their dynamos rely on a tachocline…"
This should be rephrased, as fully convective stars do not have a tachocline. It could, e.g., be "… whether the lack of a tachocline will result in a dynamo different from the solar one."

Response:
We agree that a fully convective star does not have a tachocline, and we rephrased the sentence as suggested.

2.2. Line 219-221:
"We notice that, compared to the other two stars, AD Leo has fewer low-energy flares and more high-energy flares (especially superflares), but not a general weakening of all kinds of flares."
This should be rephrased to clarify what the authors mean with "weakening".

Response:
The 'weakening' in our manuscript means a decrease in the frequency of all flares across the entire energy spectrum. We now think this half-sentence is redundant and have removed it from the manuscript.

2.3. Lines 700-703:
" …we take g_eff = 1 and $\lambda 0$ = 656.28nm for the Hα line. For the LSD profiles of Ca IRT formed in the lower chromosphere. We have typical values of g_eff = 0.968 and λ_0 = 858.56nm, taking from the LSD process, and the g of each line is taken from the VALD."
The text "chromosphere. We" should be "chromosphere, we". The last "g" should be changed to "g_eff". The λ_0 and g_eff for the photospheric LSD lines should be given here. Were all the g_eff values taken from VALD? (If not then give the references.)

Response:

Thanks for the suggestions. We have included the typical g_eff=1.24 and λ_0 =650 nm for photospheric lines. All values are taken from the VALD. Other corrections have been done as suggested.

2.4. SI line 015:
"…exhibit a small…"
Maybe change to "… have small …"

Response: Done as suggested.

2.5. SI line 081:
"r" and "p" should be in math font.

Response: Done as suggested.

2.6. SI lines 107, 108 and 111:
The term "polarity reversal" is still used here. The authors should consider using another terminology.

Response:
Thank you for pointing out this. We have now replaced 'polarity reversal' by 'opposite polarity'.

2.7. Author's response, page 4:
"We did a preliminary test on the ZDI map of AD Leo for Ca IRT (middle chromosphere), showing that the main variation between photospheric and chromospheric fields is azimuthal and meridional fields."
The authors show a ZDI map of the lower chromosphere using only the three Ca IRT lines. The small number of lines, compared to the number used for the photosphere, raises the question of how reliable this map is. Furthermore, a well-known fact is that while the radial field can be reasonably well reconstructed using just Stokes V-profiles, especially the meridional field is not well reconstructed. But since this is outside the scope, there is no need to further discuss this.

Response:
Thank you for sharing ideas about the ZDI map for Ca IRT. We agree that the meridional field in our ZDI reconstruction cannot provide useful information. At this stage, the preliminary ZDI can only offer a rough conclusion that the magnetic structure in the chromosphere should have a visible difference from the photosphere, which can be directly obtained from the B_l pattern variation. Due to the small number of available chromospheric lines, we agree that the ZDI reconstruction of the chromospheric field requires more careful consideration and testing. We have briefly mentioned this limitation in l.85-89 of the supplementary materials, "It is unclear whether the small number of available chromospheric lines can be used to create a reliable map. Nevertheless, some ideas from ZDI can still be useful for testing the field complexity."

Reviewer #2 (Remarks on code availability):
We reviewed the code when the first version was submitted, and did not see it necessary to repeat.

Response:
Thanks for evaluating the reproducibility of the code.

<u>Reviewer #3:</u>
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Response:
Thanks for evaluating the manuscript.

<u>Reviewer #4:</u>
The authors of the paper have considered in detail the objections which I noted in my original report. The authors have provided cogent replies to my objections.

I therefore now consider the paper acceptable for publication.

Response:
We are very grateful to the reviewer for recommending publication of our paper. We would like to thank the reviewer again for the previous comments, which helped us improve the quality of the manuscript.

Reviewer #1:

I thank the authors for answering my questions. I agree with the publication of the manuscript in its current form.

I was still not able to run the software without errors, but this may have to do with my specific configuration on my laptop, and is not relevant to my positive evaluation of the manuscript.

Response:

We sincerely thank Reviewer #1 for the feedback and positive assessment of our manuscript. Regarding the difficulty in running the software, we appreciate the reviewer's feedback and patience. We have rechecked our code and suspect that the errors may stem from differences in Python and SciPy versions across environments. To facilitate reproducibility, we have added a comprehensive README to the Code Ocean repository detailing the exact environment (Python, SciPy, and related dependencies) used in our tests. We hope this will help mitigate configuration issues, and we remain happy to assist further if any problems persist