

Vacuum birefringence and the polarized X-ray emission from a radio magnetar

Corresponding Author: Ms Rachael Stewart

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

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)

One of the more puzzling properties of empty space, or “vacuum”, is that it can affect the propagation of electromagnetic radiation when a strong magnetic field is present. This “vacuum birefringence” has been a long-standing prediction of quantum electrodynamics that so far has not yet been observed in nature. The reason is that the ultra strong magnetic fields of more than 10^{12} Gauss which are necessary to observe these effects are many orders of magnitude stronger than the strongest magnetic fields that can be produced in laboratories on Earth. Nature’s strongest magnets, the subclass of neutron stars called “magnetars”, on the other hand, provide natural laboratories to study the effects of strong magnetic fields on the propagation of light. Vacuum birefringence is predicted to be mainly observable in the X-ray band. With the launch of NASA’s IXPE satellite, which is the first X-ray sensitive satellite solely devoted to measurements of X-ray polarization, astronomers now have the observational capabilities to search for observable signatures of birefringence.

In their paper “Vacuum birefringence in the polarized X-ray emission of a radio magnetar” by Stewart et al., the authors present their analysis of joint observations with radio telescopes and IXPE of 1E 1547.0–5408, a well known magnetar. They find interesting polarization behavior that in their interpretation appears to be consistent with predictions of neutron star models that include vacuum birefringence. As the potentially first direct discovery of birefringence (indirect evidence already exists, as also acknowledged in the manuscript) the work addresses an interdisciplinary topic between astrophysics and physics that tests one of the fundamental theories of Nature. The topic is therefore in principle well suited for publication in Nature.

Given the strong claims in the manuscript, however, a very careful analysis and interpretation is required. At the current state of the manuscript the presentation in the paper still requires some improvements, especially also in the discussion of the limitations of the analysis and modeling done, as well as a general “toning down” of the claims in the paper. These reviewers agree that the potential of the dataset is to provide the most compelling evidence of vacuum birefringence to date, but there are limitations in the modeling that support a claim of this strength, at least as presented in the submitted manuscript. In the following, we will concentrate on addressing these points in more detail. We emphasize, however, that even though the following paragraphs may sound very critical, they are intended to help the authors in improving the paper to make it publishable in Nature and should be seen in light of our overall assessment that the results do indeed in principle deserve publication in the journal.

We believe that the following points need to be addressed in more detail in a revised version of the manuscript:

* The version of MAGTHOMSCAT used in this work does not account for radiative transfer in the neutron star atmosphere, including the phenomenon of vacuum resonance. The authors are open about this limitation and have carefully selected only the softest energy band (2–3 keV) for modeling, with the intention of avoiding the effects of vacuum resonance.

However, a comprehensive confirmation of the QED vacuum in action is not possible without a consistent treatment of both the atmospheric emission and its propagation through the magnetosphere, and without testing the resulting predictions against observational data. Consequently, the observed energy dependence of the PD and PA remains unexplained within the framework of the employed model.

* A qualitative explanation of the model-independent PD behavior with energy is suggested, based on mode conversion above ~ 1.5 keV (presumably the “adiabatic” energy), with the vacuum resonance placed around 6 keV - where the extrapolated PD in the spectro-polarimetric analysis approaches zero. However, the authors appear to find no evidence of changes in PA or PD above 5 keV, and Fig. 1 shows a rather constant PD trend above 4 keV. Therefore, the inferred location of the vacuum resonance appears to be an overinterpretation of the data.

* At the same time, changes in the electron density in the atmosphere mean that the vacuum resonance energy varies throughout the atmosphere, including the outer layers. The vacuum resonance can produce global depressions or deviations from a pure blackbody spectrum, particularly at energies near the adiabatic mode conversion energy and the vacuum resonance itself, depending on the assumed atmospheric conditions and photon propagation regime (Ho & Lai 2003, MNRAS, 338, 233–252; van Adelsberg & Lai 2006, MNRAS, 373, 1495–1522). The fact that the NICER spectra are well described by a smooth blackbody, and that no corresponding physical modeling has been performed to test the consistency of the spectral energy distribution, PD behavior with energy, and known source parameters (e.g., previous magnetic-field estimates), indicates that definitive statements about the QED vacuum in action are not adequately supported.

* It is unclear what is used as input for MAGTHOMSCAT, specifically what assumptions are made about the intrinsic atmospheric emission that subsequently experiences the vacuum birefringence effect in the magnetosphere. Is the input always assumed to be a 100% polarized component, both with and without QED vacuum effects? This point is particularly important, as the predicted energy-dependent intrinsic PD should differ for atmospheres with and without vacuum birefringence. In the latter case, in our understanding the atmospheric emission can retain a higher polarization degree in a considered energy range, as it is unaltered by mode conversion. Switching on and off the QED effect only affects the vacuum birefringence component during magnetospheric propagation - the same, in our understanding, as in the reference used to support the statements at line 176–178. Comparing the results under the assumption that the internal (atmospheric) emission has identical spectral and polarization properties with and without vacuum birefringence is a useful exercise, but it cannot be used to fully discriminate between the vacuum birefringence and no-vacuum birefringence scenarios.

* It appears that alternative hypotheses are not explored in sufficient depth, ruling out which would significantly strengthen the argument for the necessity of vacuum birefringence. For example, if vacuum birefringence were absent, could the observed energy dependence of the PD be explained by other effects (e.g., the proximity of the proton cyclotron resonance)?

* Another issue concerning the input to MAGTHOMSCATT is that the energy and angular distribution of the atmospheric emission is not specified. The statement at lines 640–642 could be interpreted as implying isotropic emission with a constant PD across energy, which would not be correct. For example, strong anisotropy in the emission from highly magnetized atmospheres that include vacuum birefringence effects is clearly illustrated in ref. [6] (Taverna et al. 2020, §2.1.1, Fig. 3). This information is crucial for pulse profile modeling, as the underlying beam patterns can significantly alter the phase variability of the emission.

* The discussion assumes familiarity with the source geometry, which is only partly resolved through Fig. 8. Inclusion of a figure defining all relevant angles and the coordinate system, at an earlier stage would help readers to follow the discussion.

Smaller comments:

* From L251–254, it seems that the authors imply that the optimal emission regions - a wedge or a hot spot offset from the magnetic axis - do not contradict a twisted magnetosphere. While this might be true, it is unclear how the twist of the magnetic field lines in the magnetosphere would affect the reasoning in Sect. 3.7, especially since it should significantly impact the results of the vacuum birefringence modeling in the magnetosphere.

* Give units for coordinates in lines 426 and 432

* line 443: Responses are not uploaded to Xspec, they are assigned to a data set

* It is unclear why it makes sense to interpret the PA phase dependence using the rotating vector model, which naturally assumes a dipolar configuration (lines 710–715), when the modeling earlier in this section already indicated a non-dipolar configuration.

* Sect. 3.3 and 3.4: explicitly state the ephemeris used for barycenterization (this is mentioned for IXPE, but not for NICER and Parkes, and readers should not have to indirectly infer that the same ephemeris was used for barycenterization).

* Sect. 3.5: Since the whole X-ray analysis only deals with continuum parameters, it is not clear why the data are only rebinned to 5 or 30 counts per bin and why c-stat is used. The resulting narrow bins obfuscate the variation shown in Fig 4 and also make deviations between the different continuum models less obvious. Stronger rebinning (at least a factor of 4) would help in this analysis, and also reveal possible deviations between the applied continuum model and the data. Figure 4 should also include the NICER spectra. If the authors are afraid that this confuses the figure, using separate y-axes for both instruments and separate residual panels would help.

* Line 663: why is the emitted area $4\pi R^2$? The area itself should be available directly from NICER, and even if this is intended as a cross check, shouldn't the area of the emitting spot be πR^2 , since this is not a sphere? The total extent is very important, as a smaller emitting area reduces the contribution from different magnetic field orientations across the surface.

Referee #2

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #3

(Remarks to the Author)

The authors present coordinated IXPE, NICER and Parkes/Murriyang observations of the radio magnetar 1E 1547.0–5408, reporting an unusually high soft X-ray linear polarization. They combine phase-resolved data with radio geometry constraints and Monte-Carlo atmospheric plus magnetospheric modeling (MAGTHOMSCATT) and argue that these data are best explained by the action of vacuum birefringence (VB) in the magnetosphere — i.e., a direct, astrophysical signature of the vacuum polarization predicted by quantum electrodynamics. While evidence for VB has been presented before, this would be the strongest to date.

Generally, an important strength of this paper is the presence of an unusually high-quality multi-instrument dataset and contemporaneous coverage. The reported X-ray polarization is very high and statistically significant.

However, the key argument underlying their results, first emphasized in Refs. 20 and 21, relies on the fact that, in the absence of VB, for emission originating near the magnetic axis of a neutron star, where the radiation is polarized along the local magnetic field direction, the net observed polarization from a line of sight directly above the region is low, generally below the 15-20% level. It increases at larger viewing angles but still remains below ~50% at viewing angles roughly twice the extent of the emitting region. In contrast, when VB is present, the net polarization is expected to retain its intrinsic value at emission (potentially reaching 100%), and its polarization angle will follow the rotating vector model (RVM). Consequently, the conclusions of the authors crucially hinge on the location of the magnetic axis of the neutron star relative to the line of sight as the star rotates.

The authors derive the magnetic field geometry in the radio emission region, located approximately 300 km from the stellar centre, using the RVM. According to their analysis, the spin axis is oriented within a few degrees of the line of sight, while the magnetic pole lies even closer to the spin axis than to the observer's direction. As a result, the magnetic pole remains near, but does not cross, the line of sight during the star's rotation.

The observations in the X-rays establish that the polarization degree is significantly larger than can be achieved given the geometry established for the spin and magnetic field without VB. Furthermore, the X-ray polarization can be fit by an RVM with a similar angle

between the line of sight and the rotation axis and also a similar position angle of the rotation axis on the sky. This latter point is interesting (although the authors do not mention this) and implies that the polarization mode of the X-rays and the radio are the same.

While the above findings provide compelling evidence for the presence of VB, several aspects of the paper require substantial strengthening and clarification to make the conclusions more solid and convincing.

In particular:

- * X-ray polarization measurements indicate that the longitude of the magnetic axis—corresponding to the point at which the magnetic axis is closest to the line of sight—differs between the X-ray and radio observations (see Figure 2). More specifically, Figure 6 shows that the parameter ϕ_0 from the rotating vector model (RVM) is 36.84 degrees for the radio data, while Figure 10 shows a value of 80 degrees for the X-ray data. Given that, under VB, the X-ray polarization direction is established at a considerable distance from the neutron star—comparable to the region where radio emission originates—one would expect the phase values of ϕ_0 to be more closely aligned. Why is it not the case?

- * The X-ray modeling is based on the new code MAGTHOMSCATT. This predicts that the emission should be fully in the O-mode, which is at odds with what long discussed in the literature of highly magnetized atmospheres (e.g., Ho & Lai 2001, 2003; van Adelsberg & Lai 2006): because in a strongly magnetized plasma the X-mode photons encounter a much smaller opacity than the O-mode photons, they can escape from deeper, hotter layers of the atmosphere. As a result, the emergent radiation from the atmospheres of magnetars is strongly dominated by the X-mode. It remains therefore unclear whether their atmospheric model is ready for use to draw strong and fundamental physical conclusions. This aspect would be much strengthened by performing an independent fit also with another atmosphere code specifically designed for magnetars.

- * Connected to the above: the size of the hot spots is found to match the inferred emission region inferred from the blackbody (BB) fits. However, in the atmosphere literature it is known that atmosphere models typically yield larger emitting areas (related to the lower inferred effective temperatures) when compared to pure BB fits. Thus repeating the suggestion made above: to strengthen their conclusions, it would be advisable for the authors to repeat the fits to the X-ray data using an alternative magnetized atmosphere model. This would help assess the robustness of their results and mitigate model-dependent biases.

- * Polarization is very sensitive to the configuration of the magnetic field in the radiation region, and so is the inference of the emission geometry. The authors assume a dipolar field. But magnetars are believed to have complex and multipolar, twisted field configurations. It is important that the authors make a deep assessment of the influence of their input assumption on the robustness of the conclusions which they draw.

- * While the authors claim to employ a sophisticated and modern statistical approach, the presentation lacks rigor in several places. Specifically, the use of reduced chi-squared values is not informative without explicitly stating the degrees of freedom (dof). All instances of reduced chi-squared should be replaced with the full chi-squared/dof notation, which allows the reader to independently assess both the goodness-of-fit and the significance of the results. Additionally, the authors should report the chi-squared/dof values for the best-fitting model *without* VB, as this serves as the null hypothesis. They should also provide the corresponding chi-squared/dof values for the intensity of the Stokes parameters Q and U individually. Furthermore, what is the odds ratio

comparing fits using only the X-ray data versus those that also include the radio data? These aspects must be quantitatively addressed to support the statistical validity of the analysis.

* Given that much of the authors' argument relies on the RVM fit to the radio data, it is important that they justify their choice of prior distributions, particularly the use of uniform priors.

Physically, the angle ζ (between the line of sight and the rotation axis) should be sampled uniformly over the sphere. This implies a prior of $P(\zeta) \propto \sin(\zeta)$ or $\cos(\zeta)$. Once this is established, one can consider various arguments for the distribution of α (the angle between the magnetic and rotation axes). For instance, assuming α and the spin axis are uncorrelated, a uniform prior in α may be acceptable, but this must be explicitly justified. Otherwise, a more physical choice would be a uniform prior in $\cos(\alpha)$, again corresponding to an isotropic distribution.

It is also worth noting that directly fitting for $\beta = \zeta - \alpha$ introduces a non-trivial prior, since the distribution of β will depend on the joint priors of α and ζ . If the authors choose to fit β directly, they must account for this induced prior structure. The authors could additionally argue that, for the object to be visible as a radio pulsar, β should fall within a plausible range (e.g., -40° to $+40^\circ$), and that within this range a uniform prior is reasonable. However, this does not negate the need for the α prior to follow a physically motivated form, such as uniform in $\cos(\alpha)$.

Ultimately, it is crucial to ensure that the fitting procedure does not artificially favor small values of ζ . Since the derived conclusions depend sensitively on the geometry, the choice and justification of priors is of paramount importance, as well as the statistical demonstration that their conclusions are robust with respect to the choices made.

Referee #4

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #5

(Remarks to the Author)

This manuscript presents X-ray and radio observations of the magnetar 1E 1547.0–5408, reporting strong evidence for quantum vacuum birefringence. The authors base their claim on several key pieces of evidence: 1. A high (65%) phased-averaged polarization degree at 2 keV; 2. The absence of significant depolarization during the radio pulse; 3. A decrease in polarization degree between 2–4 keV; 4. Consistency between the radio and X-ray position angles (PAs), as fitted with a Rotating Vector Model (RVM). If confirmed, the finding of the manuscript would be of profound significance, with impact extending beyond astrophysics to fundamental physics domains like quantum electrodynamics, thus meeting the high-impact criteria for publication in Nature.

My comments primarily address the robustness of the evidence provided by the work, with the most significant concern relating to point 4.

The assertion that the X-ray and radio PAs are consistent via a joint RVM fit is not sufficiently supported. The X-ray data provide only seven PA points to constrain an RVM model with four highly correlated parameters. Therefore, caution must be taken to evaluate the quality of the fit. The manuscript only vaguely mentioned that the most favourable geometry corresponds to a small inclination and viewing angle, but did not provide any quantitative description, and did not provide any details of the fit (e.g., prior ranges of the RVM parameters as in the radio analysis). Later, it referred to one representative case, but did not mention anything about the measurement uncertainties, and did not discuss any other possible solutions of the fit. In fact, not much details have been provided to evaluate the goodness of the fit. Additionally, from Figure 2, it is hard to see any consistency between the X-ray and the radio PA curve, despite a generally similar trend. Since the consistency of X-ray and radio PA serves as a complementary piece of evidence supporting the presence of VB, more detailed and quantitative arguments need to be provided.

In Figure 3, while the model without birefringence is clearly inconsistent with the data, the model *including* birefringence does not provide a fully convincing match to the observations, particularly for the Stokes Q parameter. The authors should explain these residual discrepancies in greater detail.

In the posterior distribution shown in Figure 5, the parameter for the PA slope appears to be truncated by the plot boundary. It is unclear if this is a physical constraint or an artifact of the prior range imposed during sampling. This needs to be clarified,

as it could affect the interpretation of the model constraints.

The manuscript reports a PD of $59\pm 5\%$ (2–3 keV), $37\pm 5\%$ (3–4 keV), and $40\pm 11\%$ (4–8 keV), then fits a simple linear decrease with energy. This model seems inadequate, as the data suggest the PD saturates or flattens beyond 3 keV. The authors should discuss other plausible trends and the potential impact of their chosen model on the conclusions.

Additionally, can the analysis provide an estimate for the resonant energy in 1E 1547.0–5408?

Figure 2 presents aligned X-ray and radio pulse profiles, but the method for aligning the X-ray data in time with the radio data collection site is not explained. A more detailed description of this alignment procedure is necessary.

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)

Referee Assessment of the manuscript 2025-08-20266B, "Evidence of magnetospheric vacuum birefringence in the polarized X-rays of a radio magnetar", by Rachael E. Stewart et al.

We thank the authors for their revised version of the manuscript, which took into account our comments and added extended clarifications of the modelling and analysis, as well as the addition of a separate rotation vector model (RVM) fit to the IXPE data. However, even including the added material and the author's discussion of the manuscript by Taverna et al. (2026; arXiv:2601.15452), we find that the authors continue to overstate the implications of their results.

While we remain in principle supportive of publication in Nature, given the uniqueness and peculiarity of the combined radio and X-ray dataset, we do not support the current phrasing that "These observational findings, combined with theoretical simulations, provide evidence for quantum vacuum birefringence naturally occurring in magnetar magnetospheres." In our view, this statement goes beyond what is robustly supported by the data.

The central issue is that the conclusions are presented as direct physical evidence, whereas they in fact rely on a specific set of modelling assumptions and the following results. Specifically, the manuscript currently suggests that the combined efforts of observational analysis and modeling provide unambiguous evidence of the presence of vacuum birefringence. A more accurate statement would be that the inclusion of vacuum birefringence is required to reproduce the data within the adopted modelling framework. We emphasize that this is a subtle but very critical point, even though it tends to be often ignored in the literature: Providing unambiguous evidence for a certain physical effect is only possible if alternative models can be ruled out. If a specific model framework that includes a physical effect can be made to describe a data set, but without testing other models, the strongest claim that can be made is that the data do not rule out the existence of that effect, which obviously is a much weaker claim. But it is central to scientific epistemology that science operates by falsifying alternative hypotheses, not by showing that a hypothesis may work.

This is especially true for cases such as the one presented here, where even in the framework presented by the authors some aspects of the data – such as the qualitative behaviour of the Stokes parameters – are not fully reproduced (see below). It is important, therefore, that the distinction between model-dependent inference and observationally robust conclusions is made more explicit throughout the paper: At most, the results can be interpreted as a possible indication of vacuum birefringence in action – given the underlying modelling assumptions – but not as "evidence" in the stronger sense implied by the title, the abstract, and the framing of the introduction.

However, in light of the new discussion for the geometry determination, even such a claim is possible only after several other issues are resolved.

Geometry:

We appreciate that the authors took the time to clarify the comparison of the results with Taverna et al. (2026) and to perform the RVM fit to the radio data using an alternative convention for the observed polarization angle. However, as is clear from these results, as well as from the basic formulas for the transformation of angles between different conventions (Everett & Weisberg 2001), such transformations do not change the absolute value of the impact parameter. Similarly, the absolute value of the cosine of the magnetic obliquity reflects the proximity of an emitting region to the rotation axis (without specifying the up/down orientation). Thus, a near-aligned geometry remains near-aligned even under a change of convention, as does the angular distance between the LOS and the emission region.

According to the presented analysis, the RVM fit to the radio data results in a nearly aligned geometry and a small absolute value of α . However, an independent RVM fit to the X-ray data yields best-fit values similar to those of Taverna et al. (2026) (modulo convention-based angle transformations): a noticeable offset of the emission region from the rotation axis (~ 18 deg) and a large absolute value of the impact parameter, implying a significant minimum angular distance between the emission region and the LOS across rotational phases (Sec. 3.7). This point should be explicitly mentioned in the body text. We emphasize that given the confusion between the different angle conventions pointed out in the author's comments on Taverna et al. stresses again the point that it is important to define such angles in an unambiguous way. We therefore do not

understand why the authors have chosen not to define these angles in Fig. 11 and would like to reiterate our request that they be included. We also strongly recommend that the figure display other relevant axes and angles.

It is also important to stress that the 3-sigma contours cover a large parameter space and overlap with the radio constraints only in a very small region at the edge. While it is fair to say that “the inferred geometric parameters are consistent with those obtained from the radio RVM fit at the 2.5-sigma level” (Sect. 3.7), and that this provides some evidence that the radio and X-ray emission can originate from the same region, other solutions are still possible. The former interpretation is implied – but never clearly stated – in the body of the text, and needs to be made more explicit. From the reply to the referees, as well as Sects. 3.7 and 3.8, it is clear, however, that the authors use this assumption as an established basis for further analysis and conclusions. Section 3.8 suggests that the geometric parameters derived from the radio analysis are compared to those obtained from the physical modelling (lines 864–866). Moreover, the radio solutions are used to disfavour a polar-cap-like shape of the emission region (lines 889–890; 894–895). Such an assumption should be stated explicitly in the body text – as an assumption – along with the fact that it is adopted in the physical modelling of the Stokes parameters.

Stokes parameter behaviour:

The description of the modeled Stokes parameters implies that, for the chosen geometry, the best fit including vacuum birefringence (VB) provides a significantly more accurate description than the fit without VB. Moreover, the phase variation of Q and U in the no-VB case is described as “strong phase-dependent oscillations in Q and U” – a characterization that is repeated multiple times throughout the manuscript and therefore carries considerable weight. However, Fig. 3 suggests that the phase dependence of Q and U in this case simply corresponds to a two-peaked profile rather than a single-peaked profile. Furthermore, the observed data in both panels indicate that the Q/U profiles are also two-peaked, not single-peaked as suggested by the model including VB.

Considering the unclear role of the parameters inferred from the radio analysis, as well as the absence of a systematic exploration of the full parameter space across different geometries in the physical modeling of the Q and U parameters, we find that this result remains limited in scope and does not support strong conclusions.

We note that the question of the correctness of the inferred (or adopted) geometry for the X-ray polarization is crucial for the main message of the paper. In particular, Taverna et al. argue that, for the geometry inferred from the X-ray data analysis, no strong indication of vacuum birefringence is required.

In addition, the authors should recognize that, even if all of the issues outlined above are satisfactorily addressed, the modeling of Q and U remains inherently dependent on the underlying assumptions (e.g., those implemented in *magthomscatt*). While some of these assumptions have been discussed in the revised version (for example, the influence of the dominant polarization mode), others – such as the choice of polarization-dependent emission beaming – have not been critically assessed in terms of their impact on the results. The authors are not required to explore all such aspects in detail, but should clearly state the extent to which their main conclusions depend on the adopted modelling assumptions.

Referee #2

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #3

(Remarks to the Author)

We have reviewed the revised version of the manuscript and appreciate the authors' careful consideration of the comments raised in the first report. However, the revision has reinforced our concern that the paper suffers from a degree of over-modeling that ultimately detracts from the robustness of the conclusions. In particular, the results derived from the *MAGTHOMSCATT* modeling do not appear sufficiently well supported given the current limitations of the code and the way it is applied in the manuscript.

For example, the fact that an atmosphere expected to be dominated by X-mode photons in magnetars yields results broadly consistent with those obtained using *MAGTHOMSCATT*—which is dominated by the O mode—does not inspire confidence in the detailed conclusions that rely on this modeling.

The flexibility of *MAGTHOMSCATT* in exploring complex hotspot shapes (e.g. wedge-shaped or offset circular caps) further introduces significant

degeneracy. The preferred wedge-shaped configuration appears highly tuned, and its physical motivation is not independently established. Alternative geometries yield comparably acceptable intensity fits but poorer polarization fits, yet all of these comparisons are made within a framework that does not adequately describe magnetar atmospheric physics.

The authors do acknowledge a critical limitation: the current implementation of MAGTHOMSCATT does not include vacuum-resonance (VR) mode conversion in the atmosphere, despite VR being explicitly invoked elsewhere in the manuscript to explain the observed energy-dependent depolarization between 2–4 keV. This creates a fundamental inconsistency in their analysis. The modeling is deliberately restricted to the 2–3 keV band to avoid VR effects, while the broader interpretation of the polarization spectrum relies on VR physics. Consequently, the simulations cannot self-consistently connect the observed energy dependence of polarization degree, the phase-resolved Stokes behavior, and the claimed role of vacuum birefringence across the atmosphere–magnetosphere system.

Thus, the statement that “the overall fit quality improves dramatically with the inclusion of VB” risks overstating what is actually demonstrated: namely, that one incomplete and flexible model family fits the data better than another incomplete family.

Also importantly, as appreciated more from the revised version, the analysis relies on statistically fragile phase bins. That is: the discrimination between models appears to rely strongly on a small number of phase bins that lie close to the detection threshold. The statistical leverage of these bins is limited, and detailed conclusions about hotspot morphology or the necessity of specific propagation effects are therefore not secure.

Given all these issues, the MAGTHOMSCATT results should be best viewed as illustrative rather than diagnostic. In our opinion they should not be used to draw quantitative conclusions. Therefore, our recommendation is that of removing all results based on the MAGTHOMSCATT modeling and refocusing the manuscript on the robust, model-independent observational results and the conclusions that can be drawn with minimal modeling assumptions. Eliminating this material would strengthen the paper by avoiding conclusions that currently rely on an incomplete and ill-suited modeling. The paper would benefit from emphasizing what is firmly established observationally and the robust inferences that can be drawn by combining the radio and X-ray polarization data. Rather than claiming the modeling provides decisive evidence for magnetospheric vacuum birefringence, the authors should frame VB as the most plausible physical interpretation consistent with extremely high PDs, suppressed depolarization near the radio pulse, coherent PA swings across wavebands.

Referee #4

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #5

(Remarks to the Author)

The clarity of the manuscript has improved significantly, and it is pleasing to see that the radio and X-ray RVM fits are broadly consistent. I have only one further comment, detailed below.

The prior ranges for Φ_0 and Ψ_0 differ between the radio and X-ray RVM fitting. The manuscript also discusses the inconsistency between the values of Φ_0 and Ψ_0 obtained from the radio and X-ray analyses. While I appreciate that this may not be the case, it would be beneficial to adopt consistent prior choices for both fits. Doing so would allow us to definitively rule out the possibility that the choice of prior is responsible for the observed inconsistency. Specifically, the prior range for Φ_0 is currently restricted to 270–360 deg, yet the fitted value from the X-ray data falls outside this range. It would therefore be advisable to extend the prior range for the radio fit, for example, to completely exclude the possibility that the radio fit could converge to a value closer to the X-ray result.

Additionally, for the X-ray fit, it would be helpful to explicitly state the prior ranges adopted for the alpha and beta parameters.

Version 3:

Reviewer comments:

Referee #1

(Remarks to the Author)

The authors have significantly improved the manuscript with respect to the earlier revised version. Specifically, the revised manuscript now includes the appropriate caveats with respect to the interpretation of the MAGTHOMSCATT model and provides a more careful discussion of the assumptions that enter the modeling.

We also commend the authors on the revised Fig. 10, which now is a good illustration of the assumed geometry. Given that for readers not familiar with the model this figure is required knowledge, we strongly recommend that the figure be put in the main part of the manuscript.

We believe that the manuscript can now be accepted.

Referee #2

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #3

(Remarks to the Author)

We appreciate that in this revised version the authors have downplayed the strength of their claim, and thus the paper feels more balanced in this respect. However, we still do not consider the paper suitable for publication in Nature in its current form for the reasons detailed below.

Section 3.8 relies entirely on the MAGTHOSCATT model. As discussed in our earlier report, this model does not incorporate the relevant physics—most notably free-free opacity—required for accurately modeling magnetar atmospheres. Indeed, it yields predictions that are qualitatively inconsistent with models that do include free-free opacity. Consequently, the conclusions drawn from these fits are not reliable and, in our view, should not be included in the paper.

Furthermore, in a previous version of the manuscript, the authors discussed the best-fitting model without vacuum birefringence (VB), which they ultimately discarded because the intensity fit yielded $\chi^2 = 49.9/30$ for the centered-spot model, compared to $\chi^2 = 33.9/30$ for the wedge model. It is unclear to us how the wedge model can have the same number of degrees of freedom as the spot model. More importantly, a fit with $\chi^2 = 49.9/30$ (or even $44.9/30$) corresponds to a probability of only about 2% under the null hypothesis. Therefore, any claim that the data “require” VB would need to be supported at a confidence level exceeding 98%, which is not demonstrated here.

We therefore recommend removing Section 3.8 entirely and eliminating any conclusions elsewhere in the manuscript that depend on it. In our assessment, the MAGTHOSCATT model is not appropriate for this application, and even if it were, the statistical evidence presented does not justify the strength of the conclusions drawn by the authors.

Referee #4

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Referee #5

(Remarks to the Author)

The authors have successfully addressed my last comment. There is no further remark from my side.

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Referee 1 & 2 Comments

Referee #1 (Remarks to the Author):

One of the more puzzling properties of empty space, or “vacuum”, is that it can affect the propagation of electromagnetic radiation when a strong magnetic field is present. This “vacuum birefringence” has been a long-standing prediction of quantum electrodynamics that so far has not yet been observed in nature. The reason is that the ultra strong magnetic fields of more than 10^{12} Gauss which are necessary to observe these effects are many orders of magnitude stronger than the strongest magnetic fields that can be produced in laboratories on Earth. Nature’s strongest magnets, the subclass of neutron stars called “magnetars”, on the other hand, provide natural laboratories to study the effects of strong magnetic fields on the propagation of light. Vacuum birefringence is predicted to be mainly observable in the X-ray band. With the launch of NASA’s IXPE satellite, which is the first X-ray sensitive satellite solely devoted to measurements of X-ray polarization, astronomers now have the observational capabilities to search for observable signatures of birefringence.

In their paper “Vacuum birefringence in the polarized X-ray emission of a radio magnetar” by Stewart et al., the authors present their analysis of joint observations with radio telescopes and IXPE of 1E 1547.0–5408, a well known magnetar. They find interesting polarization behavior that in their interpretation appears to be consistent with predictions of neutron star models that include vacuum birefringence. As the potentially first direct discovery of birefringence (indirect evidence already exists, as also acknowledged in the manuscript) the work addresses an interdisciplinary topic between astrophysics and physics that tests one of the fundamental theories of Nature. The topic is therefore in principle well suited for publication in Nature.

We thank the referees for this indication of suitability for Nature.

Given the strong claims in the manuscript, however, a very careful analysis and interpretation is required. At the current state of the manuscript the presentation in the paper still requires some improvements, especially also in the discussion of the limitations of the analysis and modeling done, as well as a general “toning down” of the claims in the paper. These reviewers agree that the potential of the dataset is to provide the most compelling evidence of vacuum birefringence to date, but there are limitations in the modeling that support a claim of this strength, at least as presented in the submitted manuscript. In the following, we will concentrate on addressing these points in more detail. We emphasize, however, that even though the following paragraphs may sound very critical, they are intended to help the authors in improving the paper to make it publishable in Nature and should be seen in light of our overall assessment that the results do indeed in principle deserve publication in the journal.

*We thank the referees for this guidance, and we address the concerns raised below. Changes to the manuscript text are in **boldface**.*

We believe that the following points need to be addressed in more detail in a revised version of the manuscript:

* The version of MAGTHOMSCAT used in this work does not account for radiative transfer in the neutron star atmosphere, including the phenomenon of vacuum resonance. The authors are open about this limitation and have carefully selected only the softest energy band (2–3 keV) for modeling, with the intention of avoiding the effects of vacuum resonance. However, a comprehensive confirmation of the QED vacuum in action is not possible without a consistent treatment of both the atmospheric emission and its propagation through the magnetosphere, and without testing the resulting predictions against observational data. Consequently, the observed energy dependence of the PD and PA remains unexplained within the framework of the employed model.

- *We accept the referees' viewpoint that a comprehensive confirmation of the QED vacuum in action would require detailed considerations of the vacuum resonance in the magnetized atmosphere. Assessing signatures of magnetic vacuum birefringence (VB) in the magnetosphere (the territory specifically tackled by our paper) is addressing an important part of the puzzle but does not completely solve the puzzle.*

*The focus of this work is the effects of VB in **magnetospheres** of magnetars. We have made minor modifications to the text in places to hopefully make it clearer that we are probing magnetospheric environs. These also include tweaks on the title and the abstract to help rephrase the tone a little and make sure that the paper claims to present evidence for VB in action rather than a comprehensive proof.*

*What we aim to highlight in this work is the evidence of **magnetospheric** VB effects. In particular, VB in the magnetosphere alters the polarization properties of photons as they propagate from the stellar surface to the observer. With the geometry constrained by the radio data, Q and U traces for QED-on and QED-off cases are distinct, as thoroughly illustrated in the manuscript. To be more specific, without VB, the polarization is determined by a wide variety of magnetic field directions for emission locales on the surface, leading to strong oscillations in Q and U with pulse phase, which are clearly inconsistent with data. This property holds as long as the atmospheric emission is dominated by one polarization mode. Such a dominance is realized in our presented simulations, yet it is also often present in works that incorporate vacuum resonance (VR) modifications within a radiative transfer framework. Previous atmospheric VR explorations in the literature range from the early works of Lai et al. and Ozel to the more recent work by Kelly et al. 2024. If VR modifications still permit high polarization degrees (PDs) emerging from the atmosphere at lower energies within the IXPE band, then while the VR can rotate the polarization angle (PA) by 90 degrees when the X mode dominates surface emission rather than the O mode, we find (discussed below) that **magnetospheric VB still needs to be active** in order to approximately reproduce the observed PD and PA pulsation traces; switching VB off in the magnetosphere still generates strong oscillations in Q and U, which is not what is observed. To highlight further the distinction between the QED-on and QED-off polarization signatures, we have*

added an extra panel in Figure 3 (panel d) displaying the results using Q-U representation. *The associated discussion has been added at lines 262-268.*

We have added a brief mention of the caveats of our radiative transfer treatments not including VR and its implications at lines 228-234.

* A qualitative explanation of the model-independent PD behavior with energy is suggested, based on mode conversion above ~ 1.5 keV (presumably the “adiabatic” energy), with the vacuum resonance placed around 6 keV - where the extrapolated PD in the spectro-polarimetric analysis approaches zero. However, the authors appear to find no evidence of changes in PA or PD above 5 keV, and Fig. 1 shows a rather constant PD trend above 4 keV. Therefore, the inferred location of the vacuum resonance appears to be an overinterpretation of the data.

- *We agree that it is indeed difficult to make strong claims about atmospheric vacuum resonance given the current quality of the data. We note, however, that based on the works of Lai et al. and the more recent Kelly et al. work, the impact of the vacuum resonance should in principle lead to some depolarization of the outgoing X-rays, and perhaps more so at higher energies within the IXPE band. Yet, the polarization state should still be dominated by the same linear polarization mode up to the resonance energy. Hence, the substantial decrease in the narrow 2-4 keV range and the observed constant PA across this band are in principle qualitatively consistent with depolarization due to vacuum resonance. We also now provide an updated analysis that includes a specific focus on the 4-5 keV band to search for the continued trend established in the 2-4 keV range. However, this band accentuates our 4-8 keV result in showing a slight deviation compared to the lower energies. *We substantially updated the text remarking on the vacuum resonance energy to remove all over-interpretation of the data (last paragraph of section 3.8). We have also clarified this element (including the caveats) in the main text at lines 197-201, and in the new Figure 4.**

* At the same time, changes in the electron density in the atmosphere mean that the vacuum resonance energy varies throughout the atmosphere, including the outer layers. The vacuum resonance can produce global depressions or deviations from a pure blackbody spectrum, particularly at energies near the adiabatic mode conversion energy and the vacuum resonance itself, depending on the assumed atmospheric conditions and photon propagation regime (Ho & Lai 2003, MNRAS, 338, 233–252; van Adelsberg & Lai 2006, MNRAS, 373, 1495–1522). The fact that the NICER spectra are well described by a smooth blackbody, and that no corresponding physical modeling has been performed to test the consistency of the spectral energy distribution, PD behavior with energy, and known source parameters (e.g., previous magnetic-field estimates), indicates that definitive statements about the QED vacuum in action are not adequately supported.

- *We agree that definitive statements about the QED vacuum in action inside the atmosphere are not supported. The modifications to the text we have noted above*

(changes in select places including the title and abstract) hopefully help clarify that our analysis directly addresses the action of the VB only in the magnetosphere.

In terms of the X-ray data, we offer the caution that finding deviations from a pure blackbody from the NICER spectrum is a truly complex exercise and will depend on many elements. For instance, this source suffers from a relatively large absorbing column resulting in a reduced S/N below 2 keV. Moreover, the effective area of NICER (and all other soft X-ray instruments) diminishes above 4-5 keV, again resulting in a significantly reduced S/N. With the available statistics, and in the narrow energy range we consider, we find that a single BB is sufficient to describe the spectral curvature. There is also the prospect of contamination of surface signals by additional components emanating from the magnetosphere, as appears to be the case in a number of magnetars. Thus, the current quality of data is not sufficient to distinguish deviations from BB spectra noted in Ho & Lai (2003) and van Adelsberg & Lai (2006). Future, deep broad-band spectral and polarimetric datasets may be able to address the higher energy spectral element in this source.

We added some brief comments pertaining to these elements at lines 234-239 and 995-1001.

* It is unclear what is used as input for MAGTHOMSCAT, specifically what assumptions are made about the intrinsic atmospheric emission that subsequently experiences the vacuum birefringence effect in the magnetosphere. Is the input always assumed to be a 100% polarized component, both with and without QED vacuum effects? This point is particularly important, as the predicted energy-dependent intrinsic PD should differ for atmospheres with and without vacuum birefringence. In the latter case, in our understanding the atmospheric emission can retain a higher polarization degree in a considered energy range, as it is unaltered by mode conversion. Switching on and off the QED effect only affects the vacuum birefringence component during magnetospheric propagation - the same, in our understanding, as in the reference used to support the statements at line 176–178. Comparing the results under the assumption that the internal (atmospheric) emission has identical spectral and polarization properties with and without vacuum birefringence is a useful exercise, but it cannot be used to fully discriminate between the vacuum birefringence and no-vacuum birefringence scenarios.

- *In the magnetar domain, the disparity in the magnetic Thomson differential cross sections of the two linear polarization modes when photons travel obliquely to the magnetic field is substantial. As a result, the intrinsic emission from a surface locale is highly linearly polarized (PD ~ 100%) in directions away from the field, dominated by the ordinary or parallel mode. Along the magnetic field direction, specifically, within the so-called magnetic scattering cone (see Baring et al. ApJ, **982**, 126, 2025), the relative disparity in the differential cross sections of the two modes diminishes. As a result, there is a mixture of the two linear polarization modes within this cone, and the PD is lower (~50%). A comprehensive analysis of the polarization characteristics of emergent*

atmospheric radiation at different magnetic colatitudes can be found in Dinh Thi et al. *ApJ* **992** 188 (2025). This is what is input into the magnetospheric propagation module.

We agree with the referees that, in principle, the emergent PD should be different when including QED vacuum effects in the atmosphere compared to when these effects are not considered. However, in the softest energy range (below 3 keV) that we are considering for the model, the depolarization by vacuum resonance (VR) is not expected to be large. As a result, the intrinsic surface emission with QED effects in the atmosphere is still highly polarized, consistent with the data. This suggests that, when the VR impacts radiative transfer in the atmosphere, the net amplitudes of Q and U are reduced. These reductions are less significant at low energies while they become more significant as energy increases. If VR influences within the atmosphere were in fact significant below 3 keV, the ensuing depolarization of the surface signal would preclude being able to match the high PDs in the data regardless of whether or not VB was present in the magnetosphere. The overall rotational phase variations of Q and U, hence the PA, should remain nearly the same until the VR influences become really significant, likely above 4 keV. In other words, the Stokes vector, $S=(Q,U)$, remains within one quadrant for different energy bands, while its magnitude decreases with energy. We note that there is a modest shift in PA in going from the 3-4 keV band to the 4-8 (or 4-5) keV band, and this may indeed be a signature of a greater influence of the vacuum resonance above 4 keV. To test such a conjecture would require both extensive simulation of the VR at different surface locales and greater count statistics above 4 keV.

The behaviors described above are evident in the phase-resolved PA for different energy bands in Figures 2 & 9 and in the energy-resolved Q and U shown in Figure 1. On the other hand, if VB is not present in the magnetosphere, the electric field vector directions follow the local surface magnetic fields, leading to strong oscillations in the traces of Q and U that are inconsistent with the observed data, regardless of whether VR influences are substantial or small. Thus, we can still discriminate between the scenarios with and without VB, even when the depolarization due to vacuum resonance is not incorporated.

We have added clarifications at lines 1005-1011.

* It appears that alternative hypotheses are not explored in sufficient depth, ruling out which would significantly strengthen the argument for the necessity of vacuum birefringence. For example, if vacuum birefringence were absent, could the observed energy dependence of the PD be explained by other effects (e.g., the proximity of the proton cyclotron resonance)?

- We agree that it is always possible that some other influences might account for the decline of the PD above 3 keV, regardless of evidence for VB at energies below 3 keV. The potential impact of a proton cyclotron resonance is indeed an alternative, and for this magnetar, this resonance arises at 2.8 keV at the surface magnetic pole. This resonance is very narrow for 0.5-5 keV atmospheric plasma temperatures because of the high proton mass. The absorption rates favor the perpendicular polarization (X mode) and so a cyclotron resonance line produces a relative enhancement of the

parallel mode (O) in the continuum spectrum. Photon spawning from higher harmonics (well studied for electron cyclotron resonances in neutron star accretion columns) is likely small because of the high proton mass and so will not re-populate the line. The line therefore appears at the local cyclotron fundamental energy, and when summed over different surface locales with different field strengths, would get smeared out. It would require detailed transfer simulations to determine if it has a significant effect on the polarization degree, akin to those that were conducted in the late 1990s in the context of electron cyclotron lines in accreting X-ray binaries.

*We feel that the prospect for the proton cyclotron resonance in depolarizing the spectrum above around 4 keV is limited, yet we do add brief mention of it at around **lines 1012-1017** to motivate future investigations.*

* Another issue concerning the input to MAGTHOMSCATT is that the energy and angular distribution of the atmospheric emission is not specified. The statement at lines 640–642 could be interpreted as implying isotropic emission with a constant PD across energy, which would not be correct. For example, strong anisotropy in the emission from highly magnetized atmospheres that include vacuum birefringence effects is clearly illustrated in ref. [6] (Taverna et al. 2020, §2.1.1, Fig. 3). This information is crucial for pulse profile modeling, as the underlying beam patterns can significantly alter the phase variability of the emission.

- *The anisotropy, and also polarization, characteristics of the emergent emission obtained in MAGTHOMSCATT are detailed in Dinh Thi et al ApJ **992** 188 (2025). The strong beaming characteristics in the magnetar domain, as well as the dependence of the anisotropy on magnetic colatitude, were fully taken into account in the simulations performed for this paper. They are well documented in the above paper and prior papers on the simulation by our group that were cited in the original manuscript.*

*We have added clarifications at **lines 826-828**.*

* The discussion assumes familiarity with the source geometry, which is only partly resolved through Fig. 8. Inclusion of a figure defining all relevant angles and the coordinate system, at an earlier stage would help readers to follow the discussion.

- *We appreciate this suggestion. The relevant angles for interpreting the Stokes parameters have been described in Section 3.6 on radio polarization, and are very standard. The surface magnetic field colatitude and longitude are also standard angles. The coordinate angles for simulation results are benchmarked through the meridional configuration where the stellar rotation axis, magnetic moment axis and the observer's line of sight are all coplanar. These angles are all well described in Dinh Thi et al, ApJ **992** 188 (2025), Dinh Thi et al. ApJ in press (2026) [arXiv:2512.22978], and other group papers cited in the manuscript. The phasing of model light curves is subject to phase rotation to fit the phasing of the observational data (addressed in the new **Section 3.8** in*

the revised manuscript). We feel that this type of detail in a figure is not suited to the compact Nature forum but rather to longer, detailed technical papers. We have now included a citation to [Figure 1 of Kun Hu et al 2022 ApJ 928 82](#), where such angles are illustrated, in the text at [line 208](#).

Smaller comments:

* From L251–254, it seems that the authors imply that the optimal emission regions - a wedge or a hot spot offset from the magnetic axis - do not contradict a twisted magnetosphere. While this might be true, it is unclear how the twist of the magnetic field lines in the magnetosphere would affect the reasoning in Sect. 3.7, especially since it should significantly impact the results of the vacuum birefringence modeling in the magnetosphere.

- *Twists are unlikely to impact the net polarization degree that is high due to the impact of VB. The principal signature of twists is that they will rotate the PA (i.e., angle ψ_0) due to the presence of a toroidal field component. To strengthen our study, we have now performed a detailed RVM modelling of the X-ray polarization data and presented results in the new Section 3.7 in the Methods. In particular, the new [Figure 10](#) shows that there is a modest difference of 12-20 degrees between the PA values of the X-ray signal and the radio emission that may well emanate from somewhat higher altitudes than the PLR. This could well be a sign of fields that are more twisted at the X-ray PLR than in the radio emission zone; this possibility motivates future detailed investigations of twist models.*

We have included a brief comment on this in the new [Section 3.7](#), along with some details in [Section 3.8](#).

* Give units for coordinates in lines 426 and 432

- *The manuscript has been updated such that the coordinate system is identified as J2000 now in [lines 491-492](#).*

* *line 443*: Responses are not uploaded to Xspec, they are assigned to a data set

- *We have updated the manuscript to clarify this element to say: “For the spectral products, we generated the `arf` and `mrf` for each detector via the HEASoft tool `fixspecalcarf` and utilized the corresponding responses within Xspec during the spectral fitting.” in the now [lines 506-508](#).*

* It is unclear why it makes sense to interpret the PA phase dependence using the rotating vector model, which naturally assumes a dipolar configuration (lines 710–715), when the modeling earlier in this section already indicated a non-dipolar configuration.

- *Please see the comments above. Given that we adopt a dipolar magnetic field configuration for our atmospheric X-ray modeling, the use of the standard RVM fitting for the PA sweep is appropriate for this morphology. This approach is standard practice, and has been employed in several previous IXPE studies of magnetars. The twists that we suggest might be present are generally at low to moderate altitudes, and likely below the altitude of the radio emission, so we expect that the dipolar RVM implementation is accurate overall.*

* Sect. 3.3 and 3.4: explicitly state the ephemeris used for barycenterization (this is mentioned for IXPE, but not for NICER and Parkes, and readers should not have to indirectly infer that the same ephemeris was used for barycenterization).

- *We have added the following sentence in lines 579-580 addressing this point: “All ToAs were barycentered using the JPL planetary ephemeris DE405.”*

* Sect. 3.5: Since the whole X-ray analysis only deals with continuum parameters, it is not clear why the data are only rebinned to 5 or 30 counts per bin and why c-stat is used. The resulting narrow bins obfuscate the variation shown in Fig 4 and also make deviations between the different continuum models less obvious. Stronger rebinning (at least a factor of 4) would help in this analysis, and also reveal possible deviations between the applied continuum model and the data. Figure 4 should also include the NICER spectra. If the authors are afraid that this confuses the figure, using separate y-axes for both instruments and separate residual panels would help.

- *The quoted binning values (5 counts per bin for NICER and 30 counts per bin for IXPE) represent minimum grouping thresholds rather than the typical number of counts per channel. Given the long IXPE exposure and the high effective area of NICER, most energy bins contain substantially more counts than these limits; the grouping primarily affects the lowest and highest energy channels.*

For IXPE, this minimum binning ensures that the edge energy bins are in the Gaussian regime, which is appropriate for Stokes Q and U spectra whose uncertainties are approximately Gaussian. For the NICER case, lighter grouping preserves spectral resolution and provides improved leverage on the background modeling performed with SCORPEON, which is most sensitive at the lowest and highest energies.

We have added some clarification in Section 3.5. We have now also included the NICER spectra and their associated residuals as a separate figure from the IXPE spectra for clarity; these are shown in a new Figure 6 of the manuscript

* Line 663: why is the emitted area $4\pi R^2$? The area itself should be available directly from NICER, and even if this is intended as a cross check, shouldn't the area of the emitting spot be πR^2 , since this is not a sphere? The total extent is very important, as a smaller emitting area reduces the contribution from different magnetic field orientations across the surface.

- The emitting area was obtained directly from the flux, $A = (F * 4 * \pi * d^2) / (\sigma * T^4) = 13 \text{ km}^2$. The notation “ R^2 ” in the paper is a convention commonly used in observational studies of magnetars. This is equated to a physical area of our emission regions, which is just $2 * \pi R_{\text{NS}}^2 * (1 - \cos[\theta_{\text{cap}}])$ for a specified neutron star radius R_{NS} and a cap opening angle of θ_{cap} . [We have clarified the text on this point at lines 871-877.](#)

Referee 3 & 4 Comments

The authors present coordinated IXPE, NICER and Parkes/Murriyang observations of the radio magnetar 1E 1547.0–5408, reporting an unusually high soft X-ray linear polarization. They combine phase-resolved data with radio geometry constraints and Monte-Carlo atmospheric plus magnetospheric modeling (MAGTHOMSCATT) and argue that these data are best explained by the action of vacuum birefringence (VB) in the magnetosphere — i.e., a direct, astrophysical signature of the vacuum polarization predicted by quantum electrodynamics. While evidence for VB has been presented before, this would be the strongest to date.

Generally, an important strength of this paper is the presence of an unusually high-quality multi-instrument dataset and contemporaneous coverage. The reported X-ray polarization is very high and statistically significant.

However, the key argument underlying their results, first emphasized in Refs. 20 and 21, relies on the fact that, in the absence of VB, for emission originating near the magnetic axis of a neutron star, where the radiation is polarized along the local magnetic field direction, the net observed polarization from a line of sight directly above the region is low, generally below the 15-20% level. It increases at larger viewing angles but still remains below ~50% at viewing angles roughly twice the extent of the emitting region. In contrast, when VB is present, the net polarization is expected to retain its intrinsic value at emission (potentially reaching 100%), and its polarization angle will follow the rotating vector model (RVM). Consequently, the conclusions of the authors crucially hinge on the location of the magnetic axis of the neutron star relative to the line of sight as the star rotates.

The authors derive the magnetic field geometry in the radio emission region, located approximately 300 km from the stellar centre, using the RVM. According to their analysis, the spin axis is oriented within a few degrees of the line of sight, while the magnetic pole lies even closer to the spin axis than to the observer's direction. As a result, the magnetic pole remains near, but does not cross, the line of sight during the star's rotation.

The observations in the X-rays establish that the polarization degree is significantly larger than can be achieved given the geometry established for the spin and magnetic field without VB. Furthermore, the X-ray polarization can be fit by an RVM with a similar angle between the line of sight and the rotation axis and also a similar position angle of the rotation axis on the sky. This latter point is interesting (although the authors do not mention this) and implies that the polarization mode of the X-rays and the radio are the same.

- *Yes, this is an interesting nuance that didn't make it into the original draft. We are happy to briefly mention it, and now do so at lines 788-789.*

While the above findings provide compelling evidence for the presence of VB, several aspects of the paper require substantial strengthening and clarification to make the conclusions more solid and convincing.

- *We thank the referees for this positive appraisal, and we address the concerns raised below. Changes to the manuscript text are in **boldface**.*

In particular:

* X-ray polarization measurements indicate that the longitude of the magnetic axis—corresponding to the point at which the magnetic axis is closest to the line of sight—differs between the X-ray and radio observations (see Figure 2). More specifically, Figure 6 shows that the parameter ϕ_0 from the rotating vector model (RVM) is 36.84 degrees for the radio data, while Figure 10 shows a value of 80 degrees for the X-ray data. Given that, under VB, the X-ray polarization direction is established at a considerable distance from the neutron star—comparable to the region where radio emission originates—one would expect the phase values of ϕ_0 to be more closely aligned. Why is it not the case?

- *We thank the referee for raising this point. Our updated RVM analysis (see below) indeed confirms a statistically significant offset between the values of ϕ_0 inferred from the radio and X-ray PA swings, amounting to a difference of approximately 80 degrees; a similar offset was present in the original presentation (see new section 3.7 and Figure 8).*

As discussed in Section 3.6, our estimates place the radio emission height in the range of several tens to several hundreds of stellar radii, with preferred values around 50–100 stellar radii. In contrast, our X-ray modeling indicates that the polarization-limiting radius, where the X-ray polarization vector decouples from the magnetic field, is of order a few tens of stellar radii. This is therefore likely somewhat closer to the neutron star surface than the radio emission region.

*For the (assumed longitudinally-uniform) radio signal, ϕ_0 encodes the rotational phase at which the magnetic axis is approximately co-planar with the line of sight and the spin axis: a convention that defines the so-called meridional plane. The X-ray surface emission for the best-fit wedge case emanates from a restricted range of longitudes ranging from zero to 120 degrees, **as in Figure 10** (old Figure 9). This ideally incurs an effective offset of 60 degrees in magnetic longitude on the surface from a zone centered on the meridional plane. The centroid of the surface X-ray hotspot is, of course, not at the pole, and at around 8-12 degrees magnetic colatitude. The combined influences of these latitude and longitude offsets between the two bands is what contributes most of the difference to the ϕ_0 values, since the narrow radio cone and the X-ray trajectories (spanning a considerably larger solid angle) sample different field directions in the magnetosphere. Yet, an additional offset in the RVM ϕ_0 may arise because of the somewhat different characteristic formation radii of the X-ray and radio polarization signals, especially in cases where fields are twisted at low/moderate altitudes in the magnetosphere.*

*We have expanded this discussion in the revised manuscript in the **new section 3.7**.*

* The X-ray modeling is based on the new code MAGTHOMSCATT. This predicts that the emission should be fully in the O-mode, which is at odds with what long discussed in the literature of highly magnetized atmospheres (e.g., Ho & Lai 2001, 2003; van Adelsberg & Lai 2006): because in a strongly magnetized plasma the X-mode photons encounter a much smaller opacity than the O-mode photons, they can escape from deeper, hotter layers of the atmosphere. As a result, the emergent radiation from the atmospheres of magnetars is strongly dominated by the X-mode. It remains therefore unclear whether their atmospheric model is ready for use to draw strong and fundamental physical conclusions. This aspect would be much strengthened by performing an independent fit also with another atmosphere code specifically designed for magnetars.

- *The referees are correct that the works cited above predict a dominance of X-mode photons emerging from deeper layers in strongly magnetized atmospheres. Yet, our conclusions regarding the necessity of vacuum birefringence (VB) in the magnetosphere do not depend on which polarization mode dominates at the surface, but rather on the presence of a strongly dominant mode capable of producing the high polarization degrees observed. An atmosphere dominated by X-mode photons leads to very similar inferences; the main difference is a 90° rotation of the polarization angle.*

To verify this statement, we performed a set of numerical experiments in which the polarization mode of each photon was switched upon its emergence from the surface, i.e., $X \leftrightarrow O$. This resulted in emergent radiation dominated by X-mode photons. This change in photon polarization mode reverses the signs of the Stokes parameters Q and U, while leaving the beaming pattern unaffected.

For this X-mode-dominated atmosphere, the Q and U traces in the absence of VB also exhibit strong oscillations due to the rotation of the polarization vectors with local surface magnetic field vectors. This behavior is inconsistent with the observed behavior of the polarization vector angle (i.e., PA), which rotates within a single quadrant. Consequently, VB is required to reproduce the observed Stokes parameters. We found that, for the X-mode-dominated atmosphere, the inferred geometric angles are very similar to those obtained for the O-mode-dominated atmosphere. However, the best-fit value for the coordinate rotation angle in this case is $\Delta\alpha_{\text{ref}} = 15$ degrees, which differs by 90 degrees from the O-mode result, as expected when switching the polarization mode.

*We have added this discussion in **Section 3.8** (formerly Section 3.7).*

* Connected to the above: the size of the hot spots is found to match the inferred emission region inferred from the blackbody (BB) fits. However, in the atmosphere literature it is known that atmosphere models typically yield larger emitting areas (related to the lower inferred effective temperatures) when compared to pure BB fits. Thus repeating the suggestion made above: to strengthen their conclusions, it would be advisable for the authors to repeat the

fits to the X-ray data using an alternative magnetized atmosphere model. This would help assess the robustness of their results and mitigate model-dependent biases.

- *We agree with the referees that blackbody emission sizes may not be accurate. In the paper, we display only the cases in which the hotspot size matches the value inferred from the isotropic Stefan-Boltzmann law. However, in our considerations prior to the original submission, we explored a broader set of cases in which the hotspot size does not necessarily match the blackbody estimate. In all hotspot size cases explored, models without vacuum birefringence (VB) failed to reproduce the combination of high polarization degree, stable Stokes Q and U behavior, and large X-ray pulse fraction observed in the data, whereas models including VB consistently provided a satisfactory description.*

*If the hotspot is centered at the magnetic pole, without VB, there is strong X-ray depolarization ($PD \sim 0$) when the radio beam almost crosses the line of sight, due to the cancellation of polarization vectors of photons from different surface locales. Moreover, a polar-cap scenario cannot reproduce the large observed intensity pulse fraction given the geometry inferred from the radio data (a near-aligned rotator and a small viewing angle). For larger hotspots, the intensity modulation is further reduced and depolarization becomes even stronger, both of which are inconsistent with the observed minimum PD of 40% in the X-ray data. We note that the best result among the VB-off cases considered in our work corresponds to a pole-centered circular hot spot with a slightly oblique viewing geometry (**Figure 12**, top row). However, both Q and U magnitudes are underestimated in this case. *To further illustrate this, we have added a new panel in Figure 3 (panel d).* We note that for cases of hotspots centered off the pole, without VB, the polarization vectors are determined by the local magnetic field directions at the emission locales within the hotspot. This naturally leads to strong oscillation in the phase-resolved traces for Q and U, inconsistent with the observations.*

*In summary, our conclusion regarding the presence of VB in the magnetosphere remains unchanged even when considering hotspots of different sizes. Describing all of this in the text does not suit the compact constraints of the Nature format. *Accordingly we have added a brief comment on the insensitivity of the conclusion of the need for VB to the hot spot size at lines 880-883.**

* Polarization is very sensitive to the configuration of the magnetic field in the radiation region, and so is the inference of the emission geometry. The authors assume a dipolar field. But magnetars are believed to have complex and multipolar, twisted field configurations. It is important that the authors make a deep assessment of the influence of their input assumption on the robustness of the conclusions which they draw.

- *We indeed thought about the potential role of twisted field geometries prior to the original submission. In the text, there was a comment that the positioning of the X-ray emission region on the surface may be consistent with twists that move field line footpoints on the surface. Probing such a possibility of twists is not easy with the present data, and also*

due to the broad range of possibilities for twisted field configurations. Twists are unlikely to impact the net PD that is high due to the impact of VB; the co-alignment of photon E-vectors with the field structure will persist, with depolarization only arising when VB is switched off. The principal signature of twists is that they will rotate the PA (i.e., angle ψ_0) due to the presence of a toroidal field component. This will arise whether magnetospheric VB is active or not. Yet in the absence of VB, the net PD will be lower and the strong oscillations of Stokes Q and U will persist due to the different samplings of surface field directions (as discussed above). Accordingly, the presence of twists will still require invoking the action of VB in order to explain the phase-resolved data.

We have added a brief comment on this at lines 288-292.

To strengthen our offering, we have now performed a detailed RVM modelling of the X-ray polarization data and presented results in the new Section 3.7. In particular, the new Figure 9 shows that there is a modest difference of 12-20 degrees between the PA values of the X-ray signal and the radio emission that may emanate from somewhat higher altitudes than the PLR. This could well be a sign of fields that are more twisted at the X-ray PLR than in the radio emission zone that is likely at somewhat higher altitudes. This possibility motivates future detailed investigations of twist models.

* While the authors claim to employ a sophisticated and modern statistical approach, the presentation lacks rigor in several places. Specifically, the use of reduced chi-squared values is not informative without explicitly stating the degrees of freedom (dof). All instances of reduced chi-squared should be replaced with the full chi-squared/dof notation, which allows the reader to independently assess both the goodness-of-fit and the significance of the results. Additionally, the authors should report the chi-squared/dof values for the best-fitting model *without* VB, as this serves as the null hypothesis. They should also provide the corresponding chi-squared/dof values for the intensity of the Stokes parameters Q and U individually. Furthermore, what is the odds ratio comparing fits using only the X-ray data versus those that also include the radio data? These aspects must be quantitatively addressed to support the statistical validity of the analysis.

- *We thank the referees for helpful suggestions. We have replaced the reduced χ^2 by the total-chi-squared / dof for both with and without VB cases. We have also reported these numbers separately for intensity, Q, and U.*

We clarify that we do not perform any joint or simultaneous modeling of the X-ray and radio datasets.

In our modeling of the X-ray data, we explicitly explore the full geometric parameter space in (α, ζ) , as described in Sections 3.7 and 3.8 paragraph 4. The radio-derived geometry serves only as an external, independent reference that allows us to identify which regions of this parameter space are physically plausible and preferred.

We have now clarified this distinction in the revised manuscript to avoid any ambiguity regarding the role of the radio constraints in our analysis (Lines 862-866).

* Given that much of the authors' argument relies on the RVM fit to the radio data, it is important that they justify their choice of prior distributions, particularly the use of uniform priors.

Physically, the angle zeta (between the line of sight and the rotation axis) should be sampled uniformly over the sphere. This implies a prior of $P(\text{zeta}) \propto \sin(\text{zeta})$ or $\cos(\text{zeta})$. Once this is established, one can consider various arguments for the distribution of alpha (the angle between the magnetic and rotation axes). For instance, assuming alpha and the spin axis are uncorrelated, a uniform prior in alpha may be acceptable, but this must be explicitly justified. Otherwise, a more physical choice would be a uniform prior in $\cos(\text{alpha})$, again corresponding to an isotropic distribution. It is also worth noting that directly fitting for $\beta = \text{zeta} - \text{alpha}$ introduces a non-trivial prior, since the distribution of beta will depend on the joint priors of alpha and zeta. If the authors choose to fit beta directly, they must account for this induced prior structure. The authors could additionally argue that, for the object to be visible as a radio pulsar, beta should fall within a plausible range (e.g., -40° to $+40^\circ$), and that within this range a uniform prior is reasonable. However, this does not negate the need for the alpha prior to follow a physically motivated form, such as uniform in $\cos(\text{alpha})$. Ultimately, it is crucial to ensure that the fitting procedure does not artificially favor small values of zeta. Since the derived conclusions depend sensitively on the geometry, the choice and justification of priors is of paramount importance, as well as the statistical demonstration that their conclusions are robust with respect to the choices made.

- *We thank the referee for this important comment regarding the potential biases introduced by prior choices in the RVM fitting. We have re-run the radio RVM fits adopting isotropic priors for the geometric parameters, corresponding to uniform sampling in $\sin(\text{alpha})$ and $\sin(\text{zeta})$. This has led to a slightly revised version of the corner plot in [Figure 8](#).*

With these updated priors, the inferred magnetic inclination and viewing angles shift slightly toward larger values but remain consistent with our original results at the 1-sigma level. Applying the same physically motivated priors to the 2007 radio dataset results in a somewhat larger shift in the recovered posterior distributions for alpha and zeta, although these remain consistent with our updated 2025 measurements within 1–2 sigma. This demonstrates that our geometric conclusions are robust with respect to the specific choice of prior.

For consistency, we have also adopted the same uniform-in-sine priors in our updated X-ray RVM analysis (Section 3.7). [We have revised the main text \(Lines 206–212\) and Sections 3.6 and the new 3.7 to reflect these changes.](#)

Referee 5 Comments

This manuscript presents X-ray and radio observations of the magnetar 1E 1547.0–5408, reporting strong evidence for quantum vacuum birefringence. The authors base their claim on several key pieces of evidence: 1. A high (65%) phased-averaged polarization degree at 2 keV; 2. The absence of significant depolarization during the radio pulse; 3. A decrease in polarization degree between 2–4 keV; 4. Consistency between the radio and X-ray position angles (PAs), as fitted with a Rotating Vector Model (RVM). If confirmed, the finding of the manuscript would be of profound significance, with impact extending beyond astrophysics to fundamental physics domains like quantum electrodynamics, thus meeting the high-impact criteria for publication in Nature. My comments primarily address the robustness of the evidence provided by the work, with the most significant concern relating to point 4.

The assertion that the X-ray and radio PAs are consistent via a joint RVM fit is not sufficiently supported. The X-ray data provide only seven PA points to constrain an RVM model with four highly correlated parameters. Therefore, caution must be taken to evaluate the quality of the fit.

The manuscript only vaguely mentioned that the most favourable geometry corresponds to a small inclination and viewing angle, but did not provide any quantitative description, and did not provide any details of the fit (e.g., prior ranges of the RVM parameters as in the radio analysis). Later, it referred to one representative case, but did not mention anything about the measurement uncertainties, and did not discuss any other possible solutions of the fit. In fact, not much details have been provided to evaluate the goodness of the fit. Additionally, from Figure 2, it is hard to see any consistency between the X-ray and the radio PA curve, despite a generally similar trend. Since the consistency of X-ray and radio PA serves as a complementary piece of evidence supporting the presence of VB, more detailed and quantitative arguments need to be provided.

- *We thank the referee for highlighting the need for a more rigorous and quantitative analysis of the X-ray PA variation. In response, we have now performed a full Bayesian fit of the phase-resolved X-ray PA to the RVM, following the methodology adopted in several previous IXPE studies (e.g., Doroshenko et al. 2022, NatAs, 6, 1433). Specifically, we employed the Naghizadeh-Khouei & Clarke (1993, A&A, 274, 968) likelihood to fit a binned 2–4 keV PA profile (15 phase bins) using Equation (1) in the manuscript.*

The resulting fit provides a statistically adequate description of the data, with $\chi^2 = 12.8$ for 11 degrees of freedom (reduced $\chi^2 = 1.16$). The posterior distributions of the fitted parameters are now shown in the new Figure 8. As expected given the more limited statistics, the X-ray constraints are broader than those obtained from the radio data, particularly for ζ (the angle between the line of sight and the rotation axis). Nevertheless, the inferred geometric parameters (α and ζ) are consistent with those derived from the radio RVM fit at the 2.5σ level. In particular, we recover a small magnetic-to-rotation axis inclination, $\alpha = 18 \pm 4$ degrees, in broad agreement with the radio-based solution.

We adopted uninformative, uniform priors for all model parameters, as now described explicitly in the revised text. We also verified the robustness of our results against the

choice of phase binning, repeating the analysis with bin numbers ranging from 8 to 16; all tests yield consistent posterior distributions. Finally, as an additional validation, we performed an unbinned likelihood analysis using the formalism of González-Caniulef et al. (2023, MNRAS, 519, 5902). The results are fully consistent (almost identical posteriors) with those obtained from the binned approach.

We note that our analysis indicates a small/modest difference in ψ_0 between radio and X-ray, which may suggest some twisting of the magnetospheric field, and a significant difference in ϕ_0 , which is consistent with the hotspot in the best case solution being offset from the magnetic pole and being centered in a magnetic longitude away from the meridional plane. This offset is required to explain the relative phasing of peaks in the radio and X-ray intensity and polarization data.

All of the above is now discussed in a new section 3.7 entitled X-ray Polarization Angle Modeling.

In Figure 3, while the model without birefringence is clearly inconsistent with the data, the model *including* birefringence does not provide a fully convincing match to the observations, particularly for the Stokes Q parameter. The authors should explain these residual discrepancies in greater detail.

- *We thank the referee for this careful observation. Indeed, one phase bin (the 7th) deviates from our best-fit model by more than 3σ , particularly in Stokes Q. At present, it is difficult to unambiguously identify the origin of this discrepancy. We note that this point corresponds to the seventh phase bin, for which the polarization degree lies just above the 99% MDP, implying only marginal statistical significance (see [Figure 9](#), left panel). Improved count statistics could potentially resolve the difference in this bin. We actually explored different data binnings that did improve the statistical fit somewhat, yet we decided not to display these as the differences were not statistically significant.*

A second possible explanation is that our best-fit configuration represents the optimal solution within the explored parameter space. It is conceivable that slightly different hotspot morphologies — particularly more complex or extended surface regions — could yield a marginally improved match to the Q/I profile. Yet, introducing a more complex geometry would also naturally produce more structured Q and U curves in the absence of VB (as observed in [Figure 12](#), orange lines), and would therefore not alter our main conclusions for the requirement of VB. [We have added this clarification in lines 247-250 and 918-935 of the revised manuscript.](#)

In the posterior distribution shown in Figure 5, the parameter for the PA slope appears to be truncated by the plot boundary. It is unclear if this is a physical constraint or an artifact of the prior range imposed during sampling. This needs to be clarified, as it could affect the interpretation of the model constraints.

- *The default XSPEC priors are uniform within the hard parameter limits defined for each model component. To address this, we expanded the prior range for the PA slope to $-10^\circ/\text{keV}$ to $+10^\circ/\text{keV}$. With this broader prior, the best-fit value remains unchanged at $-4^\circ/\text{keV}$ with a 1σ uncertainty of $\pm 4^\circ/\text{keV}$, and the truncation in the posterior distributions is resolved. We have updated the manuscript text and figures accordingly in lines 670-674.*

The manuscript reports a PD of $59\pm 5\%$ (2–3 keV), $37\pm 5\%$ (3–4 keV), and $40\pm 11\%$ (4–8 keV), then fits a simple linear decrease with energy. This model seems inadequate, as the data suggest the PD saturates or flattens beyond 3 keV. The authors should discuss other plausible trends and the potential impact of their chosen model on the conclusions. Additionally, can the analysis provide an estimate for the resonant energy in 1E 1547.0–5408?

- *We thank the referee for this suggestion. A refined analysis of the 4–5 keV and 5–8 keV bands confirms that the polarization behavior above ~ 4 keV becomes more complex, consistent with the referee's assessment. However, only $\sim 10\%$ of the total source counts are detected above 4 keV, with $\sim 90\%$ residing in the 2–4 keV band. As a result, the current dataset does not provide sufficient statistical leverage to robustly characterize curvature or detailed structure in the high-energy polarization behavior. Since our primary conclusions rely on the well-constrained 2–4 keV band, where the linear model provides an adequate description (and better than a constant polarization model with $\Delta\text{Cstat/d.o.f} \sim 40/2$), the main results remain unaffected. A substantially deeper exposure would be required to firmly establish the polarization trend at energies above 4 keV. The complementary 4-5 keV polarization analysis is presented in new Figure 4, and a corresponding discussion has been incorporated in Section 3.5, lines 635-663.*

Figure 2 presents aligned X-ray and radio pulse profiles, but the method for aligning the X-ray data in time with the radio data collection site is not explained. A more detailed description of this alignment procedure is necessary.

- *Provided that both datasets are folded using the same timing ephemeris — including the dispersion measure (DM) and the phase-defining parameters TZRMJD, TZRFRQ, and TZRSITE—the resulting pulse profiles are intrinsically aligned. These parameters are included in our timing solution (Table 2), and we verified that the folding was performed using software packages (Tempo2 and PINT) that fully account for these quantities. We have now added a more explicit description of this alignment procedure in Section 3.4.*

Letter to Referees 1-5

Dear Referees,

We wish to draw attention to a recently submitted manuscript by Taverna et al. (2026; arXiv:2601.15452), which presents an independent IXPE spectropolarimetric analysis of 1E 1547.0–5408 using the same X-ray dataset analyzed in our study, but without the inclusion of simultaneous radio observations.

The two works reach broadly similar conclusions, e.g., regarding emission from a single hotspot within a highly magnetized atmospheric layer. Their phase-resolved X-ray polarimetric analysis demonstrates that the polarization-angle (PA) variation can be described by a rotating-vector model (RVM), suggesting the influence of magnetospheric vacuum birefringence. However, the source geometry inferred in that work differs substantially from the geometry derived in our analysis.

In our study, we find a geometry consistent with an almost aligned rotator and a viewing direction close to the spin axis. This conclusion is independently supported by both our radio PA analysis and our X-ray atmospheric modeling. In particular, the radio data provide very tight constraints on the geometric parameters owing to their high signal-to-noise. The X-ray modeling incorporates not only the intensity but also the full Stokes Q/I and U/I information. In the revised manuscript, we additionally include a separate analysis in which we fit the X-ray PA data alone with an RVM (Section 3.7) as an independent verification of the presence of magnetospheric VB. While the resulting posterior distributions are necessarily broader than those obtained from the radio data, they are nonetheless broadly consistent with the radio-derived geometry.

In contrast, Taverna et al. (2026) constrain the geometry solely by fitting the phase-dependent X-ray PA with an RVM, obtaining $\zeta = 106.6 \pm 30.9^\circ$ and $\alpha = 157.7 \pm 4.0^\circ$, which differ markedly from our results. We find that the discrepancy in the inferred geometry is driven primarily by a difference in sign convention between the PA defined in the RVM formalism and that used in the observer’s reference frame, as discussed by Everett & Weisberg (2001). This implies that the negative slope of the observed PA with rotational phase should result in a positive impact parameter $\beta = (\zeta - \alpha)$. When this distinction is not taken into account, the inferred relation between the PA slope and β changes sign. **A similar issue appears to affect the RVM constraints reported by Camilo et al. (2008; S. Johnston private communication), where a PA convention error likely influenced the reported values, as our reanalysis of those data returns a geometry consistent with the IXPE-era observations.** We illustrate the effect of adopting the “wrong” convention by applying it to our 2025 radio data and to archival radio observations, demonstrating that the inferred values of α and ζ shift significantly and become more closely aligned with those recovered by Taverna et al. (2026), while the qualitative conclusion of a near-aligned geometry remains unchanged. We further note that the RVM admits intrinsic geometric symmetries, with α and ζ defined modulo 180° , such that, once these degeneracies are properly accounted for, the geometry inferred by Taverna et al. (2026) is not intrinsically inconsistent with our radio-derived solution, contrary to the interpretation presented in that work.

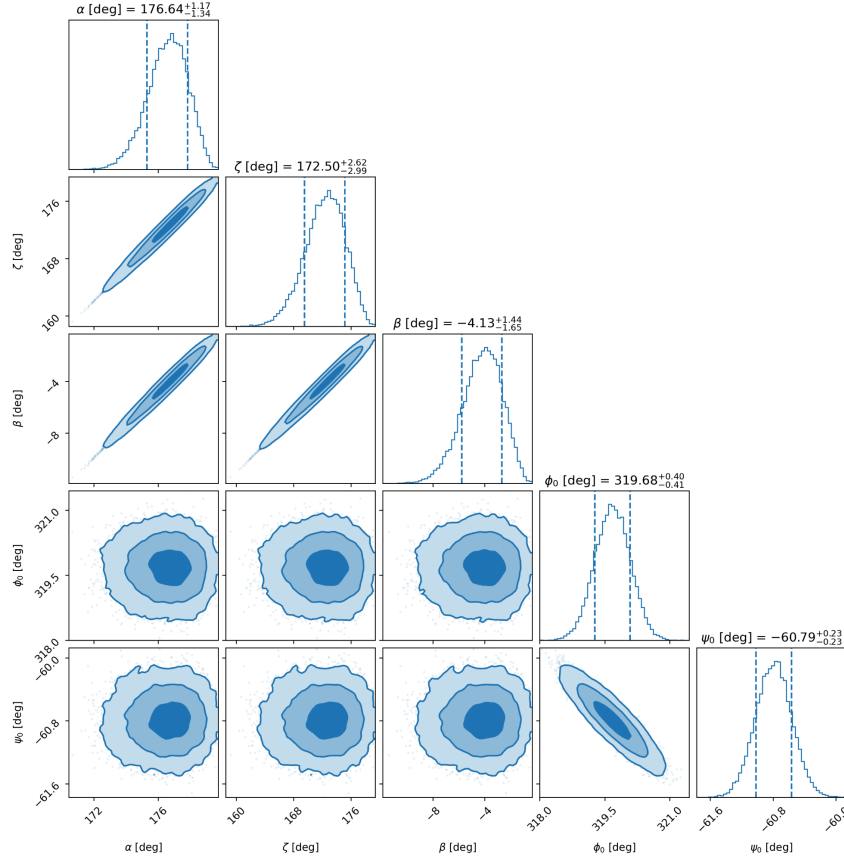


Figure 1. One- and two-dimensional posterior distributions for an RVM fit to the 2025 radio PA swing using the “incorrect” PA convention.

We further note that, in Taverna et al. (2026), the median RVM traces corresponding to low- and high-obliquity geometries produce broadly similar descriptions of the X-ray PA variation, with the high-obliquity solution yielding a somewhat lower reduced χ^2 (0.5 versus 1.7). On this basis, the authors argue against a low-obliquity configuration. However, the radio-derived RVM fits provide constraints on the geometry that are approximately an order of magnitude tighter than those obtainable from the X-ray PA data alone. When the radio and X-ray constraints are compared using a consistent PA convention, they are compatible at the 2–3 σ level, as shown in Figure 9 of the revised manuscript. In this context, we therefore place greater weight on the radio-informed geometry.

From a radio pulsar perspective, the unusually wide pulse profile of 1E 1547.0–5408 further favors a near alignment between the rotational and magnetic axes and a small viewing angle relative to the magnetic axis. Reconciling such a profile with the large impact angles implied by the median X-ray-only RVM solutions would be challenging for a 2.1 s rotator.

Finally, we note that the geometric configuration adopted by Taverna et al. (2026) in their atmospheric modeling lies within the parameter space explored in our work, although no direct model–data comparison is performed in that study. In our analysis, the best-fitting

no-vacuum-birefringence solution corresponds to a similar region of parameter space ($\zeta = 20.5^\circ$, $\alpha = 2^\circ$; lines 890–897), but is disfavored once the full Stokes I, Q, and U datasets are considered and is excluded by the radio constraints at the 4σ level.

This note is intended solely to clarify the relationship between the two studies and to assist in placing our results in the context of the most recent literature.

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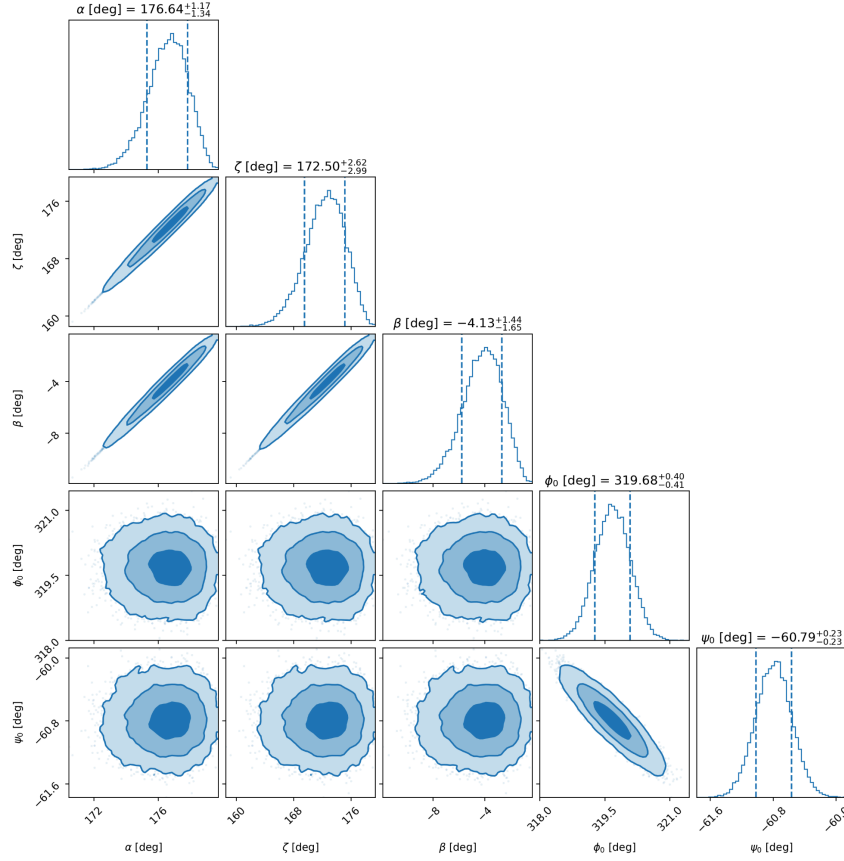


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This note is intended solely to clarify the relationship between the two studies and to assist in placing our results in the context of the most recent literature.

Referee 1 & 2 Comments

Referee #1 (Remarks to the Author):

Referee Assessment of the manuscript 2025-08-20266B, "Evidence of magnetospheric vacuum birefringence in the polarized X-rays of a radio magnetar", by Rachael E. Stewart et al.

We thank the authors for their revised version of the manuscript, which took into account our comments and added extended clarifications of the modelling and analysis, as well as the addition of a separate rotation vector model (RVM) fit to the IXPE data. However, even including the added material and the author's discussion of the manuscript by Taverna et al. (2026; arXiv:2601.15452), we find that the authors continue to overstate the implications of their results.

While we remain in principle supportive of publication in Nature, given the uniqueness and peculiarity of the combined radio and X-ray dataset, we do not support the current phrasing that "These observational findings, combined with theoretical simulations, provide evidence for quantum vacuum birefringence naturally occurring in magnetar magnetospheres." In our view, this statement goes beyond what is robustly supported by the data.

The central issue is that the conclusions are presented as direct physical evidence, whereas they in fact rely on a specific set of modelling assumptions and the following results. Specifically, the manuscript currently suggests that the combined efforts of observational analysis and modeling provide unambiguous evidence of the presence of vacuum birefringence. A more accurate statement would be that the inclusion of vacuum birefringence is required to reproduce the data within the adopted modelling framework. We emphasize that this is a subtle but very critical point, even though it tends to be often ignored in the literature: Providing unambiguous evidence for a certain physical effect is only possible if alternative models can be ruled out. If a specific model framework that includes a physical effect can be made to describe a data set, but without testing other models, the strongest claim that can be made is that the data do not rule out the existence of that effect, which obviously is a much weaker claim. But it is central to scientific epistemology that science operates by falsifying alternative hypotheses, not by showing that a hypothesis may work.

This is especially true for cases such as the one presented here, where even in the framework presented by the authors some aspects of the data – such as the qualitative behaviour of the Stokes parameters – are not fully reproduced (see below). It is important, therefore, that the distinction between model-dependent inference and observationally robust conclusions is made more explicit throughout the paper: At most, the results can be interpreted as a possible indication of vacuum birefringence in action – given the underlying modelling assumptions – but not as "evidence" in the stronger sense implied by the title, the abstract, and the framing of the introduction.

- *We thank the referees for their careful assessment and constructive suggestions. In response, we have substantially revised the manuscript to clarify the presentation and more closely align the interpretation with the observational results.*

In particular, we have refocused the title, abstract, and main text to emphasize the observational findings as the primary contribution, and we have adopted a more cautious framing of the interpretation, presenting magnetospheric vacuum birefringence (VB) as a natural explanation for the observed polarization properties, rather than as an established or uniquely required interpretation. We now explicitly state where conclusions depend on modeling assumptions and consistently frame theoretical results as conditional on the adopted framework.

Following the referees' guidance, we have significantly reduced the prominence of the modeling in the main text. The discussion is now condensed to a brief, illustrative paragraph, with Figure 3 moved to the Methods and the reliance on specific atmospheric models de-emphasized. In addition, Section 3.8 has been substantially shortened (from over four pages to approximately one and a half pages), with extended modeling details removed. We also removed the associated Figures 12 and 13.

We have also clarified the limitations of the geometric constraints from the X-ray RVM analysis, explicitly noting that the inferred parameters are not uniquely determined. This clarification has been incorporated consistently in both the main text and the Methods.

We again thank the referees for their constructive feedback. We hope that these revisions provide a clearer and more balanced presentation of the study, in which the observational results are placed at the forefront and the interpretation reflects both their significance and their current limitations.

However, in light of the new discussion for the geometry determination, even such a claim is possible only after several other issues are resolved.

Geometry:

We appreciate that the authors took the time to clarify the comparison of the results with Taverna et al. (2026) and to perform the RVM fit to the radio data using an alternative convention for the observed polarization angle. However, as is clear from these results, as well as from the basic formulas for the transformation of angles between different conventions (Everett & Weisberg 2001), such transformations do not change the absolute value of the impact parameter. Similarly, the absolute value of the cosine of the magnetic obliquity reflects the proximity of an emitting region to the rotation axis (without specifying the up/down orientation). Thus, a near-aligned geometry remains near-aligned even under a change of convention, as does the angular distance between the LOS and the emission region.

According to the presented analysis, the RVM fit to the radio data results in a nearly aligned geometry and a small absolute value of β . However, an independent RVM fit to the X-ray data yields best-fit values similar to those of Taverna et al. (2026) (modulo convention-based angle transformations): a noticeable offset of the emission region from the rotation axis ($\sim 18^\circ$) and a large absolute value of the impact parameter, implying a significant minimum angular distance between the emission region and the LOS across rotational phases (Sec. 3.7). This point should be explicitly mentioned in the body text. We emphasize that given the confusion between the different angle conventions pointed out in the author's comments on Taverna et al. stresses again the point that it is important to define such angles in an unambiguous way. We therefore do not understand why the authors have chosen not to define these angles in Fig. 11 and would like to reiterate our request that they be included. We also strongly recommend that the figure display other relevant axes and angles.

- *We have now expanded the geometry figure (Figure 10) in the Methods and have displayed the angles for more clarity.*

It is also important to stress that the 3-sigma contours cover a large parameter space and overlap with the radio constraints only in a very small region at the edge. While it is fair to say that “the inferred geometric parameters are consistent with those obtained from the radio RVM fit at the 2.5-sigma level” (Sect. 3.7), and that this provides some evidence that the radio and X-ray emission can originate from the same region, other solutions are still possible. The former interpretation is implied – but never clearly stated – in the body of the text, and needs to be made more explicit. From the reply to the referees, as well as Sects. 3.7 and 3.8, it is clear, however, that the authors use this assumption as an established basis for further analysis and conclusions. Section 3.8 suggests that the geometric parameters derived from the radio analysis are compared to those obtained from the physical modelling (lines 864–866). Moreover, the radio solutions are used to disfavour a polar-cap-like shape of the emission region (lines 889–890; 894–895). Such an assumption should be stated explicitly in the body text – as an assumption – along with the fact that it is adopted in the physical modelling of the Stokes parameters.

- *We have revised the manuscript to explicitly state that the geometric parameters inferred from the X-ray RVM analysis do not uniquely constrain the geometry, and that alternative solutions remain possible. These clarifications have been incorporated in both the main text and Section 3.7. In the new, significantly shortened section 3.8, we acknowledge the discrepancy between the physical modeling and the X-ray RVM results, and defer any detailed discussion on the matter to future work.*

Stokes parameter behaviour:

The description of the modeled Stokes parameters implies that, for the chosen geometry, the best fit including vacuum birefringence (VB) provides a significantly more accurate description

than the fit without VB. Moreover, the phase variation of Q and U in the no-VB case is described as “strong phase-dependent oscillations in Q and U” – a characterization that is repeated multiple times throughout the manuscript and therefore carries considerable weight. However, Fig. 3 suggests that the phase dependence of Q and U in this case simply corresponds to a two-peaked profile rather than a single-peaked profile. Furthermore, the observed data in both panels indicate that the Q/U profiles are also two-peaked, not single-peaked as suggested by the model including VB.

- *We have removed repeated references to “strong oscillations” in the VB-off case and focus on the quantitative comparison between our modeling and data.*

Considering the unclear role of the parameters inferred from the radio analysis, as well as the absence of a systematic exploration of the full parameter space across different geometries in the physical modeling of the Q and U parameters, we find that this result remains limited in scope and does not support strong conclusions.

We note that the question of the correctness of the inferred (or adopted) geometry for the X-ray polarization is crucial for the main message of the paper. In particular, Taverna et al. argue that, for the geometry inferred from the X-ray data analysis, no strong indication of vacuum birefringence is required.

- *In the revised manuscript, we predominantly focus on the observed polarization properties. The modeling results are presented as illustrative and conditional on the adopted framework, and we avoid implying that a particular geometry or configuration is uniquely required for the interpretation.*

In addition, the authors should recognize that, even if all of the issues outlined above are satisfactorily addressed, the modeling of Q and U remains inherently dependent on the underlying assumptions (e.g., those implemented in `magthomscatt`). While some of these assumptions have been discussed in the revised version (for example, the influence of the dominant polarization mode), others – such as the choice of polarization-dependent emission beaming – have not been critically assessed in terms of their impact on the results. The authors are not required to explore all such aspects in detail, but should clearly state the extent to which their main conclusions depend on the adopted modelling assumptions.

- *We again thank the referees for their constructive feedback. We hope that the revisions adopted here result in a clearer and more balanced presentation of the work, in which the dependence of the modeling results on the adopted assumptions is stated explicitly. In particular, we now emphasize that the modeling of the Stokes parameters is inherently framework-dependent and is presented primarily as illustrative, while our main conclusions are driven by the observational properties of the polarization.*

Referee 3 & 4 Comments

Referee #3 (Remarks to the Author):

We have reviewed the revised version of the manuscript and appreciate the authors' careful consideration of the comments raised in the first report. However, the revision has reinforced our concern that the paper suffers from a degree of over-modeling that ultimately detracts from the robustness of the conclusions. In particular, the results derived from the MAGTHOMSCATT modeling do not appear sufficiently well supported given the current limitations of the code and the way it is applied in the manuscript.

For example, the fact that an atmosphere expected to be dominated by X-mode photons in magnetars yields results broadly consistent with those obtained using MAGTHOMSCATT—which is dominated by the O mode—does not inspire confidence in the detailed conclusions that rely on this modeling.

The flexibility of MAGTHOMSCATT in exploring complex hotspot shapes (e.g. wedge-shaped or offset circular caps) further introduces significant degeneracy. The preferred wedge-shaped configuration appears highly tuned, and its physical motivation is not independently established. Alternative geometries yield comparably acceptable intensity fits but poorer polarization fits, yet all of these comparisons are made within a framework that does not adequately describe magnetar atmospheric physics.

The authors do acknowledge a critical limitation: the current implementation of MAGTHOMSCATT does not include vacuum-resonance (VR) mode conversion in the atmosphere, despite VR being explicitly invoked elsewhere in the manuscript to explain the observed energy-dependent depolarization between 2–4 keV. This creates a fundamental inconsistency in their analysis. The modeling is deliberately restricted to the 2–3 keV band to avoid VR effects, while the broader interpretation of the polarization spectrum relies on VR physics. Consequently, the simulations cannot self-consistently connect the observed energy dependence of polarization degree, the phase-resolved Stokes behavior, and the claimed role of vacuum birefringence across the atmosphere–magnetosphere system.

Thus, the statement that “the overall fit quality improves dramatically with the inclusion of VB” risks overstating what is actually demonstrated: namely, that one incomplete and flexible model family fits the data better than another incomplete family.

Also importantly, as appreciated more from the revised version, the analysis relies on statistically fragile phase bins. That is: the discrimination between models appears to rely strongly on a small number of phase bins that lie close to the detection threshold. The statistical leverage of these bins is limited, and detailed conclusions about hotspot morphology or the necessity of specific propagation effects are therefore not secure.

Given all these issues, the MAGTHOMSCATT results should be best viewed as illustrative rather than diagnostic. In our opinion they should not be used to draw quantitative conclusions. Therefore, our recommendation is that of removing all results based on the MAGTHOMSCATT

modeling and refocusing the manuscript on the robust, model-independent observational results and the conclusions that can be drawn with minimal modeling assumptions. Eliminating this material would strengthen the paper by avoiding conclusions that currently rely on an incomplete and ill-suited modeling. The paper would benefit from emphasizing what is firmly established observationally and the robust inferences that can be drawn by combining the radio and X-ray polarization data. Rather than claiming the modeling provides decisive evidence for magnetospheric vacuum birefringence, the authors should frame VB as the most plausible physical interpretation consistent with extremely high PDs, suppressed depolarization near the radio pulse, coherent PA swings across wavebands.

- *We thank the referees for their careful assessment and constructive suggestions. In response, we have substantially revised the manuscript to clarify the presentation and more closely align the interpretation with the observational results.*

In particular, we have refocused the title, abstract, and main text to emphasize the observational findings as the primary contribution, and we have adopted a more cautious framing of the interpretation, presenting magnetospheric vacuum birefringence (VB) as a natural explanation for the observed polarization properties, rather than as an established or uniquely required interpretation. We now explicitly state where conclusions depend on modeling assumptions and consistently frame theoretical results as conditional on the adopted framework.

Following the referees' guidance, we have significantly reduced the prominence of the modeling in the main text. The discussion is now condensed to a brief, illustrative paragraph, with Figure 3 moved to the Methods and the reliance on specific atmospheric models de-emphasized. In addition, Section 3.8 has been substantially shortened (from over four pages to approximately one and a half pages), with extended modeling details removed. We also removed the associated Figures 12 and 13.

We have also clarified the limitations of the geometric constraints from the X-ray RVM analysis, explicitly noting that the inferred parameters are not uniquely determined. This clarification has been incorporated consistently in both the main text and the Methods.

We again thank the referees for their constructive feedback. We hope that these revisions provide a clearer and more balanced presentation of the study, in which the observational results are placed at the forefront and the interpretation reflects both their significance and their current limitations.

Referee 5 Comments

Referee #5 (Remarks to the Author):

The clarity of the manuscript has improved significantly, and it is pleasing to see that the radio and X-ray RVM fits are broadly consistent. I have only one further comment, detailed below.

The prior ranges for Φ_0 and Ψ_0 differ between the radio and X-ray RVM fitting. The manuscript also discusses the inconsistency between the values of Φ_0 and Ψ_0 obtained from the radio and X-ray analyses. While I appreciate that this may not be the case, it would be beneficial to adopt consistent prior choices for both fits. Doing so would allow us to definitively rule out the possibility that the choice of prior is responsible for the observed inconsistency. Specifically, the prior range for Φ_0 is currently restricted to 270–360 deg, yet the fitted value from the X-ray data falls outside this range. It would therefore be advisable to extend the prior range for the radio fit, for example, to completely exclude the possibility that the radio fit could converge to a value closer to the X-ray result. Additionally, for the X-ray fit, it would be helpful to explicitly state the prior ranges adopted for the α and β parameters.

- *An inconsistency between the recovered values of ϕ_0 is expected. As indicated in the bottom panel of Figure 2, there is a physical observed offset between the inflection points of the X-ray and radio PA swings. Extending the prior range on ϕ_0 to 200 - 360 degrees, which encompasses the recovered value of ϕ_0 from the X-ray RVM fit, returns identical posterior distributions to the original, more restricted prior range. Using the exact same 0 - 360 degree prior range for the radio PA fit returns a bi-modal distributions in ϕ_0 , ψ_0 and α . These arise from inherent symmetries in the RVM that result from the unknown spin-direction of the magnetar, which magnetic pole of the magnetar is crossing our line of sight and the comparatively limited phase range covered by the radio PA values. We have clarified these points in the current version of the manuscript in Section 3.6. We have also added the explicit prior ranges used for α and β used in the X-ray RVM fit in Section 3.7.*

Referee #1 & 2:

The authors have significantly improved the manuscript with respect to the earlier revised version. Specifically, the revised manuscript now includes the appropriate caveats with respect to the interpretation of the MAGTHOMSCATT model and provides a more careful discussion of the assumptions that enter the modeling.

We also commend the authors on the revised Fig. 10, which now is a good illustration of the assumed geometry. Given that for readers not familiar with the model this figure is required knowledge, we strongly recommend that the figure be put in the main part of the manuscript.

We believe that the manuscript can now be accepted.

- *We thank the referees for their feedback throughout the review process and for their work in facilitating a stronger manuscript. We now include Figure 3 in the main text to improve clarity.*

Referee #3 & 4:

We appreciate that in this revised version the authors have downplayed the strength of their claim, and thus the paper feels more balanced in this respect. However, we still do not consider the paper suitable for publication in Nature in its current form for the reasons detailed below.

Section 3.8 relies entirely on the MAGTHOSCATT model. As discussed in our earlier report, this model does not incorporate the relevant physics—most notably free-free opacity—required for accurately modeling magnetar atmospheres. Indeed, it yields predictions that are qualitatively inconsistent with models that do include free-free opacity. Consequently, the conclusions drawn from these fits are not reliable and, in our view, should not be included in the paper.

Furthermore, in a previous version of the manuscript, the authors discussed the best-fitting model without vacuum birefringence (VB), which they ultimately discarded because the intensity fit yielded $\chi^2 = 49.9/30$ for the centered-spot model, compared to $\chi^2 = 33.9/30$ for the wedge model. It is unclear to us how the wedge model can have the same number of degrees of freedom as the spot model. More importantly, a fit with $\chi^2 = 49.9/30$ (or even $44.9/30$) corresponds to a probability of only about 2% under the null hypothesis. Therefore, any claim that the data “require” VB would need to be supported at a confidence level exceeding 98%, which is not demonstrated here.

We therefore recommend removing Section 3.8 entirely and eliminating any conclusions elsewhere in the manuscript that depend on it. In our assessment, the MAGTHOSCATT model is not appropriate for this application, and even if it were, the statistical evidence presented does not justify the strength of the conclusions drawn by the authors.

- *We thank the referees for their feedback throughout the review process and for their work in facilitating a stronger manuscript. We note that the current presentation of the modeling discussion is illustrative and should provide impetus for future interpretative*

explorations of the observational signatures presented in the main text, and on which our conclusions now heavily rely. As for the relevant missing physics pieces in the modeling, we have indicated those in the now Section 3.4 of the Methods.

Referee #5 (Remarks to the Author):

The authors have successfully addressed my last comment. There is no further remark from my side.

- *We thank the referees for their feedback throughout the review process and for their work in facilitating a stronger manuscript.*