Dear Editor,

We would like to thank the referee for their careful assessment of our work, and for considering our paper suitable for publication after the points raised in their report are addressed.

In the following, we proceed to address each point and outline the corresponding changes to our paper. The textual changes to our paper are highlighted with colour in the revised version.

1. I found the title to be somewhat misleading. Based on the title, I had assumed that the paper would be about WIMP annihilations in the early universe causing their abundance to deplete below the observed level. However, the paper turned out to be about annihilations happening at late times, which are probed by observations of the Virgo cluster. I won't insist that the authors change their title if they like it, but I will point out that it could be changed to better characterize the work.

   We understand the potential for confusion. Our intention with the title was to highlight that a magnetised early Universe would seed dense structures which survive until today and enhance late–time WIMP annihilations. Although our constraints are derived from present–day signals (Virgo cluster), they directly probe conditions imprinted in the early Universe. To make this clearer we are happy to adopt the slightly modified title *"Do WIMPs Survive the Legacy of a Magnetized Early Universe?"* which preserves the spirit of the original but makes more explicitly that we are studying present–day annihilation seeded by early–Universe magnetogenesis.

2. In the captions of Figs. 1 and 4, will the authors specify what they have assumed about the IGMF coherence length?

   We thank the referee for this comment. We agree that the previous draft lacked sufficient detail on the evolution and characterisation of the magnetic coherence length and field strength. In the revised manuscript, we have made three changes to address this point:

   – **Section II (pages 2-3):** We have substantially expanded the description of the magnetic–field evolution. We now explicitly state how the comoving coherence length and field strength evolve in the turbulent ($Re > 1$) and viscous ($Re < 1$) regimes. This section now fully specifies our assumptions about the coherence length at each epoch, which we apply to all the benchmarks presented in the draft.

   – **Section V (page 6):** We have updated the discussion to refer to the description in Section II. We added clarifying sentences that the type of evolution shown in Fig. 3 is illustrative of the same prescription used for all benchmarks and constraints shown in Figs. 1 and 4. We have also improved **Fig. 3** to better illustrate the evolution of the magnetic field, which is relevant to Question 6.

   – **Figure captions (Figs. 1, 4):** We have modified the captions to explicitly point to Section II for the assumptions on the evolution of the magnetic field strength and coherence length.

   These changes together ensure that the assumptions about the IGMF coherence length are now clearly spelled out in the text and explicitly referenced in the captions.

3. At Figs. 1 and 2, what do the authors mean by "LCDM baseline"? For me personally, the EW and QCD phase transitions are a part of LCDM cosmology, but self-annihilating WIMP dark matter is not. I think it may benefit the readers if the authors could clarify that the curve labeled LCDM assumes ~ 100 GeV WIMP dark matter forming prompt cusps, and the other curves account for additional prompt cusps thanks to the magnetic field. Would it also be interesting to compare with the predictions for a model of structure formation that is free of prompt cusps?

   We thank the referee for pointing out this ambiguity. In the revised manuscript we now explicitly state that prompt cusps are a generic and theoretically motivated feature of standard ΛCDM cosmology (see **Introduction**, page 2): they arise naturally as the first gravitationally bound dark–matter structures whenever the dark–matter free–streaming length suppresses the growth of smaller perturbations and prevents hierarchical merging at earlier times. Prompt cusps therefore provide a natural minimal building block in standard ΛCDM, and in the presence of primordial magnetic fields this assumption is even more motivated, since the enhanced small–scale power generated by primordial magnetic fields fixes the first scales to become non-linear and collapse, naturally seeding such cusps. If, in addition, the dark matter is self–annihilating, these structures substantially enhance the annihilation rate and the resulting gamma–ray signal, particularly in environments such as galaxy clusters.

   Throughout the paper we have accordingly replaced "ΛCDM" with "ΛCDM with self–annihilating dark matter" wherever relevant, to make this distinction explicit. We have also added a clarifying sentence in the caption of Fig. 1.

We also clarify that, for the ΛCDM baseline, we adopt a kinetic–decoupling temperature of $T_{\rm kd} = 30\,\mathrm{GeV}$ (last paragraph of Sec. II but also Sec. V (page 6)), but we do not fix the dark–matter mass to 100 GeV in any of the figures. A discussion of how the results depend on $T_{\rm kd}$ was provided in the last paragraph of Sec. II.

Finally, we note that if one were to assume shallower inner density profiles for the first halos—such as NFW–like profiles instead of prompt cusps—the resulting gamma–ray limits would remain stronger than in the standard ΛCDM case (also with NFW profiles) without primordial magnetic fields (since PMFs still enhance small–scale structure), but weaker than the limits obtained in the scenario with prompt–cusp profiles considered here.

4. At Fig. 1, in the caption the authors refer to their model as "BΛCDM" but in the text it is either "bΛCDM" or "bΛCDM." Should these be the same?

   We thank the referee for pointing this out. The text has now been corrected to BΛCDM.

5. This is my primary concern. Regarding the QCD-PT and EWPT-PT benchmark scenarios and the discussion surrounding Fig. 2, the authors state that they're taking the magnetic energy density to be $\rho_B = \rho_{\rm SM}$ and the comoving coherence length to be $\xi = (aH)^{-1}$ at the time of the phase transition. They characterize this choice as "an optimistic upper bound" saying that "more realistic magnetogenesis scenarios are expected to yield smaller field strengths." I think this is a fair characterization. I agree that it would be hard to imagine a model of magnetogenesis that so efficiently moves energy from the plasma into the magnetic field. However, if I have understood the calculation correctly, the predictions for WIMP annihilation depends sensitivity (presumably as a power law?) on the assumed IGMF field strength and coherence length. Therefore, I feel that the authors are making overly-bold claims in their abstract and conclusion. For example, the abstract first mischaracterizes their own assumption by saying "incorporating benchmark values for the magnetic field strength motivated by cosmological phase transitions such as the electroweak and QCD phase transitions," and then claims "We find that large portions of the WIMP parameter space are excluded." I think the abstract and conclusions should be revised to clarify that (1) the assumed field strength and coherence length are motivated by the maximal values that could be achieved at an EW or QCD phase transition and that (2) that the predicted WIMP signals would be weaker if a smaller field strength and coherence length were assumed. I'd also like the authors to determine how their signal (e.g., the J factor) scales with the assumed field strength and coherence length, and I'd like them to present this result around the discussion of Fig. 4 and in the conclusions section. I feel that this addition will help the reader to have a clearer sense of how the conclusions are impacted by relaxing the "optimistic" assumption.

   We thank the referee for this comment. We have substantially revised the **Abstract**, the **Conclusions** to clarify the physical meaning of our benchmark scenarios. We now state explicitly that the electroweak- and QCD-phase-transition benchmarks correspond to upper-limit, idealised cases in which the magnetic energy density equals that of the Standard Model plasma ($\rho_B = \rho_{\rm SM}$) and the coherence length equals the horizon size at generation. We emphasise that these assumptions are not realised in specific magnetogenesis models but instead bracket the maximal conceivable impact of phase-transition magnetogenesis on small-scale structure. Conversely, the DESI–Planck case is described as a data-driven, observationally motivated scenario—not an optimistic one—representing the present-day PMF amplitude inferred from cosmological observations.

   We have also added explicit statements in the **Conclusions** noting that weaker primordial fields or shorter coherence lengths would proportionally reduce the predicted annihilation signal, thereby shifting the exclusion curve in Fig. 4 toward smaller dark-matter masses and weakening the quantitative bounds. Nevertheless, the qualitative implication—that PMFs enhance early structure formation and thus strengthen indirect-detection limits relative to standard ΛCDM with self-annihilating dark matter—remains robust.

   To illustrate this scaling explicitly, we have added Fig.5, which shows how the predicted annihilation J–factor varies with the present–day magnetic field strength B_0 and the comoving coherence length ξ_0 and the corresponding discussion in the last paragraph of Sec. V.

6. Regarding the magnetic field evolution,

   (a) In Fig. 3a the caption should specify whether the authors are showing the evolution of the comoving magnetic field strength or the physical field strength.
   This is now indicated in the caption's figure.

   (b) It would be interesting to compare their B-field evolution model with a simple power-law relation, assuming that the inverse cascade scaling continues all the way through till today. My understanding from astro-ph/0410032 (Ref. [76] in the manuscript) is that despite the intermittent periods of viscous damping, the overall behavior can be approximated by extrapolating the inverse cascade power-law scaling from production until recombination. The authors may even want to add this as a dashed line to Fig. 3a, but it's not necessary.

Thanks, in the revised manuscript (Fig. 3) we now show the cosmological evolution of the comoving magnetic field and relevant length scales, along with a dashed power–law guide corresponding to the inverse–cascade scaling $B_{\rm com} \propto a^{-10/17}$ and $\xi \propto a^{4/9}$.

We have also expanded the accompanying discussion in Sec. V (pages 3-4) to describe the different dynamical regimes — turbulent, neutrino–dominated viscous, and photon–dominated viscous — and to clarify how the evolution departs from the simple power–law extrapolation once viscous damping becomes important.

(c) The authors state that they're assuming turbulent evolution that conserves the Hosking integral $B^4\xi^5 = \text{const}$. However, I think it's fair to say that there's an open question regarding how primordial magnetic fields would evolve. Earlier studies (discussed in 1303.7121) indicated that helical magnetic fields would evolve according to an inverse cascade with $B^2\xi^1 = \text{const}$, while nonhelical fields would evolve according to a direct cascade. It seems to me that the authors would predict a larger IGMF strength and coherence length if they had assumed a helical field evolving under the inverse cascade. They may want to remark on this fact, perhaps as motivation for further study.

We have added a brief comment in Sec. V (page 6).

7. At Eq. 9 the authors discuss the fraction of prompt cusps that survive tidal disruptions and thereby contribute the the dark matter indirect detection signal. If I have understood the calculation correctly, the survival of these prompt cusps is important for getting the strong indirect detection limits. The authors state "$f_{\rm surv} \sim 0.5$" and provide a reference, but writing ~ 0.5 suggests a large uncertainty. If that's the case, the text should specify the size and source of this uncertainty, and it should state whether the authors using an optimistic value or a conservative value. In fact, the provided reference argues that the uncertainity is small 0.5 ± 0.1, and the authors should state whether they think this estimate applies to their dark matter model, and clarify how the value of $f_{\rm surv}$ is expected to impact their results (e.g., how does the J factor depend on it).

We thank the referee for this insightful comment. In Ref.[64] the survival fraction of prompt cusps was found to be $f_{\rm surv} \simeq 0.5 \pm 0.1$ with disruptions dominated by mergers of prompt halos with each other. In our case, the population of prompt cusps follows a similar formation history, but our CMB and QCD benchmarks yield more massive halos and therefore a lower number density, implying fewer mergers and tidal encounters. Consequently, adopting $f_{\rm surv} = 0.5$ is a conservative choice for these scenarios. For benchmarks such as the electroweak–scale transition, where the halos are lighter and potentially more numerous, the survival probability could differ and would need to be reassessed in future dedicated studies. We have added a comment about this on page 5.

Finally, we also changed the author ordering.

We thank the referee again for their valuable feedback, which has led to substantial improvement and clarity of the manuscript, and hope that the paper can now be published in its current form.

Kind Regards,
María Olalla Olea Romacho, Malcolm Fairbairn, and Pranjal Ralegankar.