The authors are interested in the interplay between primordial magnetism and dark matter. They argue that the presence of a primordial magnetic field (PMF) would lead to the formation of dark matter minihalos with prompt central cusps, assuming that the dark matter is a weakly interacting massive particle (WIMP). Further assuming that the WIMP's interactions allow for a self-annihilation channel into visible-sector particles, they derive constraints on the interaction cross section from the non-observation of the ultra-high energy cosmic rays that result from dark matter annihilations in the cusps.

Overall this is an interesting study that addresses important outstanding questions – namely the signatures of primordial magnetism and nature of dark matter. The work is relevant insofar as it draws upon recent developments in several fields (e.g., prompt cusps in DM simulations, Hosking integral in B-field evolution). The manuscript is well written, and I expect that it would be easy to read, even for a non-expert. My primary criticism is that the reported constraints seem to be overblown, considering the authors have made some generous assumptions regarding the magnetic field. See #5 below.

I have several mostly-minor questions and comments, which I would like the authors to address before I can recommend the manuscript for publication.

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.

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

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?

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

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.

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.
   (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.
   (c) The authors state that they're assuming turbulent evolution that conserves the Hosking integral $B^4\xi^5 = \mathrm{const}$. 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 $B^2\xi^1 = \mathrm{const}$, 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.

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).