Dear Editor,

We'd like to thank the reviewer for their time, carefully reading our paper and providing their valuable feedback. Below we address their feedback and list the changes we've made to the paper in response.

The reviewer's feedback is shown in blue.

Our response to the feedback will be included in black.

The changed sections in our paper will then be included below in green **with additions presented in boldface** *and removed sections presented with a strike through them.*

The end of the 2nd paragraph says that this work constrains the possibility of PMFs "or parity-violating physics". As far as I can tell, the analysis uses only parity-even spectra, such as TT, TE, EE and BB. The authors should either clarify what they mean or remove the claim.

In addition to producing a non-zero EB and TB power spectrum, PMFs and other parity-violating physics are also known to convert EE power into BB power. By looking for excess power in the BB power spectrum we can find the strength of PMFs or equate the effects of parity-violating physics to an equivalent PMF field strength.

CMB B-mode measurements can also be used to constrain more exotic models, such as the possible existence of cosmic birefringence (carroll98,lue99) or primordial magnetic fields (PMFs) (kosowsky96, seshadri01). Both effects lead to the rotation of E-modes into B-modes **which leads to an increase in power in the BB spectrum.** *By equating the magnitude of the resulting B-modes, parity-violating processes can be translated into an equivalent PMF strength.*

The work assumes a fixed PMF template based on a model with a fixed value of the PMF generation time: a_ν/a_{PMF}. This parameter is entirely arbitrary, and directly determines the amplitude of the tensor mode, which is responsible for the tight bound the authors get from BICEP2/Keck. I think it is misleading to present bounds based on this assumption as bounds on PMF. The authors should, at the very least, add a discussion of how the bound changes if one allows the PMF generation time to be a free parameter that is marginalized over. In such a case, the bound will be dominated by the vector mode contribution, with SPTPol likely dominating the constraints.

As per the advice of the reviewer we have removed any mention of the CMBACT code.

~~We have compared the calculated templates to the outputs of the earlier CMBACT-derived code. We find excellent agreement for the vector modes as would be hoped, however a roughly factor of two difference on the tensor mode power. The origin of the difference is unclear. We have chosen to use the more recent (Zucca16) code instead of the CMBACT-derived code, largely due to our desire to extend the calculation out to $\ell = 10000$ for the Fisher matrix forecasts.~~

**We use the code from zucca16 which allows us to extend the calcula-
tion out to $\ell = 10000$ for the Fisher matrix forecasts.**

We find a 95% CL upper limit of $<$ nG for alone, this is somewhat better
than the limit found by planck15-19 of < 2.0 nG with $n_B = -2.9$ held fixed as
we have done. ~~The improvement is due to a difference in the calculated ten-
sor PMF power (for the same parameters) between the CMBACT-derived and
zucca16~~ **The improvement comes from our choice of value for the PMF
generation time versus the decision in planck15-19 to marginalise over
PMF generation time altogether.** ~~Using the CMBACT-derived code that
planck15-19 used, we find a consistent upper limit of < 1.9 nG. Returning to~~
Using the zucca16 code and adding the ground-based polarization measure-
ments, we find the upper limit is reduced only slightly to $<$ nG.

The authors report a factor of 2 difference in the normalization of the tensor
mode between the public code provided by Zucca et al and a "CMBACT-derived
code", saying that "the origin of the difference is unclear". The public version
of CMBACT is a code for CMB anisotropies from cosmic strings and does not
concern PMFs. Any code that the authors might have used would be a private
modification. Either the authors should clarify what code they are referring to,
or avoid mentioning it.

In the results section, the authors claim that "The improvement [compared
to Planck] is due to a difference in the calculated tensor PMF power (for the same
parameters) between the CMBACT-derived and Zucca et al. (2016) codes". The
also claim that Planck used "the CMBACT-derived" code. This is a false claim.
Firstly, see the point 3 above. Secondly, as far as I know, Planck used a private
code written primarily by Daniela Paoletti.

Instead, the "improvement" comes from the fact that the analysis by Planck
collaboration (as well as Zucca et al) marginalized over the PMF generation
time, which negated the contribution of the PMF tensor modes to the con-
straints.

In summary, I think the paper contains some claims that need revisiting. I
suggest repeating the analysis with the PMF generation time set free and report
the bounds in that case. Only then they can be seriously compared to previous
results in the literature.