Thank you for the opportunity to review this manuscript again. While it would have been nice for the authors to address several points more explicitly, which would have helped to increase the scope of the paper, the corrections helped to make the text more readable and make the purpose of the paper more clear.

I have a few additional comments:

**1.**
Regarding how they can substantiate that the model successfully captures the dynamics, the authors reply:
*We will add the posterior spread to show the goodness of fit. To explore and compare with other low-dimensional models than the Langevin model is beyond the scope of this paper*

Here I am not sure what has been added. The densities shown in Fig. 7 were already present in the previous version, and do not really help here, partly because the distributions have been inflated by a factor nu/2. What is interesting are differences of the best fit relative to the data. Maybe the authors could at least quantify the "goodness of fit" of the Langevin model to the different data sets and compare them, in order to point out where the model has issues capturing the EMIC behavior (apart from the resurgence of the AMOC).

**2.**
Regarding the best fit parameters, the authors have responded:
*The most likely value is the mean of the posterior distribution. This does assume that the posterior distributions are uni-modal. We will discuss this in the text.*

The most likely value is not the mean, but the mode of the distribution. This makes an especially large difference for skewed distributions and bi-modal distributions. Can the authors show that the posterior distributions are uni-modal? This needs to be better clarified in the manuscript. Furthermore, I assume they use the marginal posterior distributions of the individual parameters.
The corresponding segment in the revised manuscript reads:
*"The parameter values of these distributions are the means of the posterior distributions."*
This should be rewritten at least: "As best fit parameters, we choose the mean values of the marginal posterior distributions."

**3.**
Regarding the normalization factor C, the authors responded:
*The reviewer is correct, we added. " but can be computed numerically (and therefore used as a likelihood function in the next section)"*

Now it reads as if C can be used as likelihood function, rather than P. Please correct.

**4.**
P16L272: "These six parameters are sufficient to describe the abrupt collapse of the AMOC that leads to a hysteresis loop under varying freshwater forcing."

This statement should be made more precise. It is not the collapse that leads to a hysteresis loop, both are a result of the same underlying mechanisms. Maybe the authors can say that

the parameters are sufficient to describe an abrupt collapse of the AMOC, as part of a hysteresis loop under varying freshwater forcing.

**5.**
P16L272: "… and we did not attempt to obtain an accurate fit of that part of the hysteresis loop."

I think it would be fair to say that with the model at hand it is simply not possible to fit the data, since while asymmetric branches of the hysteresis loop may be obtained, the noise model used is symmetric. Also, it is not clear what is meant by "the resurgence of the AMOC is not the same as the collapse". In a way, when invoking just one positive feedback, it is.
This explanation could be merged with the two very short paragraphs that follow.

Technical corrections:

Abstract: "Steady-state collapse trajectories of the AMOC…."
Maybe better terminology can be found? I don't think that "steady-state trajectories" are a very intuitive concept, and it is not clear what is meant here. Maybe simply say "hysteresis diagrams" or the like.
I also suggest to rewrite: "Differences in between the climate models studied here are mainly due to..."

P4L74: Remove "studied mainly qualitatively in connection with the Langevin equation".

P4L77: More precisely, the third order term and the fourth order coefficient can be eliminated.

P11L198: "Conceptually, this is what an MCMC…"

P13L249: " Our main goal is to model the transition from the on-branch to the off-branch…"

P13L249: "Also, because we assume that other dynamics govern the lower branch and, our simple model has would need to be extended to account for those dynamics

P18L338: … but at present in it is unknown ….