Review of "Effects of Coupling a Stochastic Convective Parameterization with Zhang-McFarlane Scheme on Precipitation Simulation in the DOE E3SMv1 Atmosphere Model" by Wang et al.

This paper by Wang et al. demonstrates the effects of incorporating a stochastic convective scheme, primarily on model precipitation statistics. The Zhang-McFarlane (ZM) convective scheme in the DOE E3SM is modified such that the subgrid-scale convection responds stochastically to grid-scale forcings. Simulations with the stochastic scheme are compared to simulations with a deterministic ZM scheme. The stochastic scheme compares more favorably to observations in several measures of tropical precipitation distribution. Most notably, the frequency of light rain is reduced while the frequency of heavy rain is increased. This increased precipitation variability does not appear at the expense of model mean state degradations. The behavior of the mean and extreme precipitation under warming is nearly unchanged with the stochastic scheme. The authors also highlight a resolution dependence of the stochastic scheme. A lower vertical resolution model displays greater increases in the tails of precipitation distribution when compared to higher resolution models.

Overall, the paper is well-written, and contains results that are interesting and worthy of dissemination. I do have a short stack of issues that must be addressed prior to publication. The introductory text on the technical details of the convective scheme is too opaque. One or two figures and related arguments appear suspect, and need some thinking through. Please see specific comments below.

Major comments

1. **Section 2.1**. For someone with no background knowledge about the Plant and Craig scheme (like me), the text in this subsection is inadequate to explain how it works. I found myself repeatedly referring back to PC08 and Wang et al. 2016. I understand that explanations have appeared in these predecessor papers, but a little organization or perhaps a schematic would help the reader.

   (a) In **eq. 1, Line 110**: it would be helpful to point out at the outset, which of the variables $\langle m \rangle$, $\langle M \rangle$ and $\langle N \rangle$ are coming from the deterministic portion of the ZM scheme. One has to read until **Line 132** or refer to other papers to know that $\langle m \rangle$ is fixed.

   (b) The integral over the 'probability' in eq. 1 does not equal 1, i.e.,

$$\frac{\langle N \rangle}{\langle m \rangle} \int_0^\infty e^{-\frac{m}{\langle m \rangle}} dm = \langle N \rangle.$$

   Presumably $\langle N \rangle > 1$, so this is not a true probability, but actually denotes the mean number of clouds with mass flux between m and $m + dm$. However, this measure is being compared against a random number between 0 and 1 to generate the subgrid-scale spectrum of clouds. If this is indeed a true probability, then the right normalization is perhaps $\langle M \rangle$. At this point it is unclear if this is a typographical error or a case of inadequate information or a structural error in the implementation of the convective scheme. Please resolve this apparent inconsistency.

(c) **Line 122**: 'quasi-equilibrium' suddenly appears with no context. This is an important concept that deserves a little more attention. Move this closer to eq. 1 and at least mention that the quasi-equilibrium assumption yields the mass-flux $\langle M \rangle$ from the closure in the ZM scheme.

(d) Please include the details of how the convective tendencies are produced. In PC08, a plume model is used to compute the temperature and moisture tendencies for random number of clouds with a different cloud base mass-fluxes. These tendencies are then combined to generate ensemble-mean tendencies. Is a similar plume model used here? Is it different from the plume model within the ZM scheme, which—somewhat confusingly—generates its own spectrum of entraining plumes?

2. The parameter $\langle m \rangle$ in the PC08 scheme appears to control the subgrid-scale mass-flux distribution, but is held fixed in this study. It would be useful if the authors could state whether and how the results are sensitive to perturbations in $\langle m \rangle$ (or any other parameter that controls the subgrid-scale variance). One would expects the rain-rate pdfs, particularly the tails of the distribution, to be impacted. A large sensitivity should be quantified and documented in this paper, a small sensitivity can simply be mentioned in text.

3. Figure 10 and interpretation: The reasoning in this analysis is not clear. The fact that precipitation is related to the low-level vertical moisture transport is not surprising because those two terms nearly equal each other in heavily raining situations. Can one really jump to a causality argument and claim that $-\omega q/g$ 'explains' the large-scale precipitation pdfs in Fig. 8? Moreover, if one insists on using $-\omega q/g$, it remains to explained why the low-level q flux pdfs in Fig. 10 do not exactly correspond to the large-scale precipitation pdfs in Fig. 8. For instance, in Fig. 10, the tails (>70 mm/d) of the STOCH and STOCH-30L q flux pdfs are nearly coincident, which is quite different from the divergent tails of the large-scale precipitation PDFs in Fig. 8. I suggest that the authors remove this figure and interpretation, as it muddies more than clarifies.

Minor comments

1. Line 365: Mention if this the dilute CAPE or the non-entraining CAPE.

2. Line 381: Remove the 'an' in 'no longer an approximately linear relations'

3. Figures 1 and 2: there is excessive precipitation variance over a few land regions: central Africa, the Himalayas, the Maritime Continent and near the Colombian coast. If not explained, this should at least be be pointed out.

4. Figure 9 and interpretation. This figure does not explain why the convective precipitation pdfs are different between EAMv1 and EAMv1-30L. All we know is that EAMv1-30L generates tends to generate higher precipitation values for a given CAPE values (but why?). However, it is nice to see how the stochasticity affects the CAPE-precipitation relationship, so I suggest that the authors keep this figure, but change the interpretation.

5. Figures 14c and d: There are some quantitative differences between STOCH and EAMv1 that are harder to gauge with the coefficient a from equation 8. I suggest that the authors also show the fractional change in r_x such that the units are in %/K. In addition, it would also be useful to show the tropical precipitation pdfs like in Figure 3 to verify that the pdfs get stretched by nearly the same factor with and without the stochastic parameterization.