Response to Reviewers Comments

Reply to the comments by Referee #2

We thank the reviewer for the valuable comments and suggestions on improving the manuscript. Below is our point-by-point response to these comments. The reviewer's comments are in italic and our responses are in normal font.

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.

Reply: We thank the reviewer for the positive remarks.

Major Concerns

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.*

Reply: We will revise the manuscript to make this clearer by expanding the description of the stochastic convection. For the reviewer's information here, **<*m*>** is the mean mass flux of a cloud and is a prescribed parameter, set to 1×10^{7} kg s^{-1}. <*M*> as referred to in line 114 in the original manuscript is the ensemble cloud mass flux, obtained from the closure of the deterministic ZM scheme. <*N*> is the mean number of clouds, equal to <*M*>/<*m*>.

(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.*

Reply: We thank the reviewer for pointing this out. The implementation is correct. It is just that we did not describe it accurately in the text. The probability we compare the random number with is

$$\frac{1}{<m>}e^{-\frac{m}{<m>}}dm$$

Then the sum of mass fluxes generated this way is multiplied by the factor <*N*> to rescale it to the mass flux of all clouds.

(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.*

Reply: Done.

(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?

Reply: Yes, a similar plume model is used here; it is the bulk plume model of the ZM scheme, which gives the vertical profiles of heating and drying per unit cloud base mass flux. After summing up cloud base mass fluxes from all convective clouds successfully launched by comparing individual probabilities with random numbers, we obtain the total cloud base mass flux.

The product of this total mass flux and the temperature and moisture tendencies form the bulk plume model gives the final temperature and moisture tendencies by the subgrid convective clouds.

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.

Reply: Following the reviewer's suggestion, Figure 10 and related interpretation have been removed in the revised manuscript.

Minor comments

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

Reply: Dilute CAPE

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

Reply: Done.

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 pointed out.

Reply: We will point out these degradations in the revision.

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.

Reply: A coarser vertical resolution means stronger vertical mixing resulting in higher precipitation. The interpretation is added in the revision.

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.*

Reply: Following the reviewer's suggestions, we show the fractional change in r_x normalized by the global-mean surface air warming in Fig. R1. Consistent with Fig. 14, the spatial pattern and magnitude in the two simulations resemble each other. It also tells that the resemblance of the coefficient a from Eq. (8) between the two simulations as shown in Fig. 14c&d results from the similar response of the fractional change in r_x to global warming. Taking the tropical western Pacific (5ºN-20ºN; 130ºE-170ºE) where the maximum fractional increase in r_x emerges for example, as climate warms, the pdfs in EAMv1_4k and STOCH_4k also get stretched by about the same amount. Fig. R1 has been merged into Fig. 14 as Fig. 14e and f in the revision.

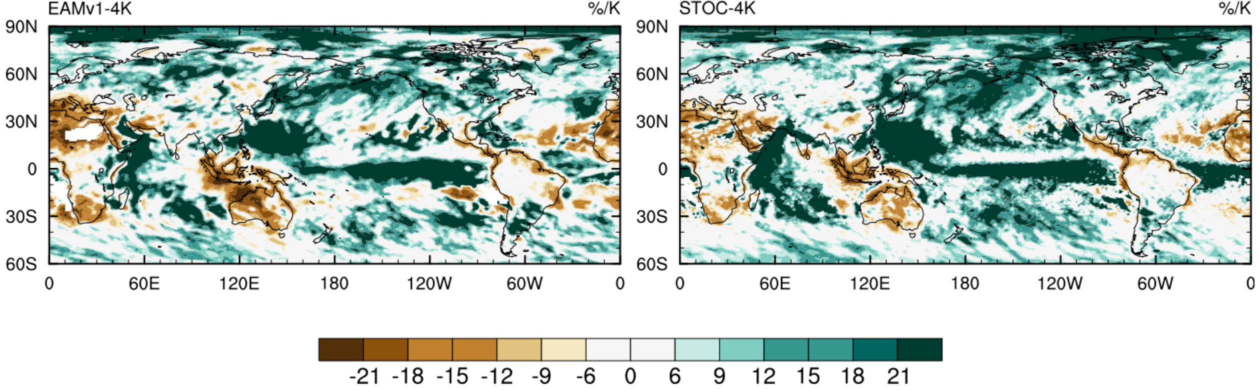

Fig. R1. Geographical distributions of the fractional change in r_x normalized by the global-mean surface air warming from +4K experiments.

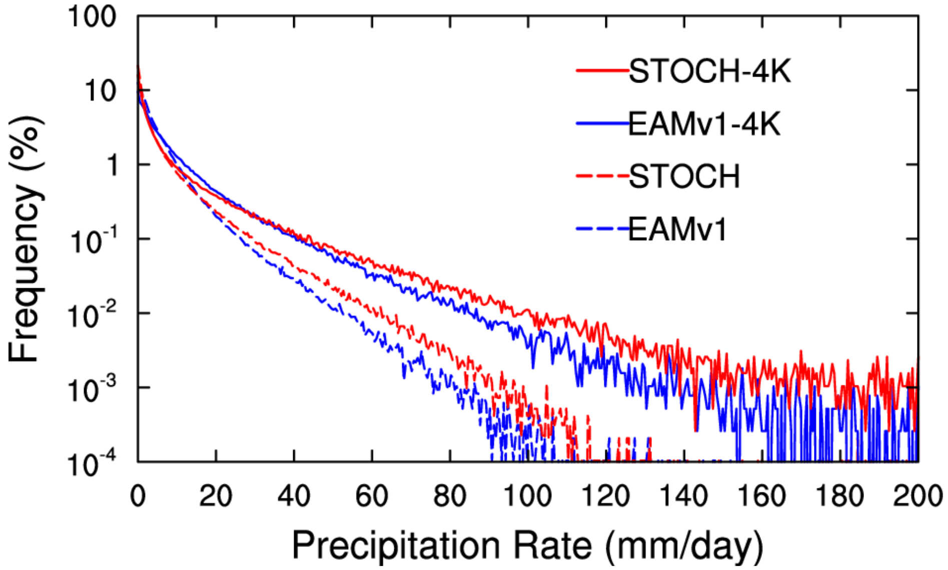

Fig. R2. Frequency distributions of total precipitation intensity over the tropical western Pacific (5ºN-20ºN; 130ºE-170ºE) for EAMv1, STOCH, EAMv1-4K, and STOCH-4K.