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Interactive Comment

Interactive comment on "Variability of summer precipitation over eastern China during the lastmillennium" *by* C. Shen et al.

Anonymous Referee #2

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Review of the paper " Variability of summer precipitation over eastern China during the last millennium" by C. Shen, W.-C. Wang and Y. Peng, Y. Xu and J. Zheng.

The authors present interesting and new information about the precipitation changes over eastern China during the last millennium. They show that the dominant period of variability was different for different periods and try to understand the cause of this variability using simulations performed with a climate model. I thus consider that the paper may be published in Climate of the Past but only after major revisions. Two points need a very careful attention as detailed below: (1) more precisions are required for several points and (2) the authors should not overemphasize conclusions that are not clearly supported by their results.

1. Several times, the manuscript is not precise enough and the reader is not able to understand clearly the method used.

1.1 It is not clear to me which datasets used in this study are new and which ones are coming from source not widely used up to now. If I understand well Table 1, the CNCC_dataset has not been published previously. For the D/W index a reference to CNMA 1987 is given but it is not in the reference list. Should it be CNMA 1981 as in the main text? In any case, a few sentences describing those datasets and the method used to construct them would be necessary.

1.2 The experimental design for the model simulations is clearly too brief. The name of the model is given but no description of the model is provided. Even the resolution, which is an important element when analyzing regional features as proposed in the manuscript, is not given. The authors say that they use a forcing similar to the one of other simulations with EBMs and GCMs but different models have used a wide range of forcings. They must thus specify the ones that are used.

1.3. Apparently, the three simulations proposed are new. However, no general information about those simulations is presented and no reference describing those simulations is given. Is the spin up procedure adequate to avoid long term drift of the climate? Is the large scale climate stable or are they shifts that could influence the evolution of precipitation over China? Other simulations have been performed with the NCAR model over the last millennium. How the present model results compared with those previous simulations?

1.4. At several occasions, it is hard to determine if the analysis is performed using reconstructions or model results. This should be clearly specified (e.g. top of page 625, 2nd paragraph of page 626)

1.5. The wording "summer precipitation&" is used many times without a clear definition of the months considered as part of "summer".

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2 Some results are not enough supported by the results and alternative interpretations are probably as reasonable and sometimes more reasonable than the ones proposed. The authors should thus be more careful in the discussion of their results.

2.1 One of my main concerns is about the significance of the various peaks, particularly the ones on Figure 4 and figure 8. On figure 4, nearly all the peaks are significant in the band 15-120 yr and the power in the band between 10 to 15 years is well below the mean. I do not doubt that the peaks are significant on a purely statistical point of view. However, such a high number of significant peaks rather implies that the statistical model used to estimate the confidence (which is not precisely discussed or justified in the manuscript) is not able to reproduce the mean behavior of the time series. As this mean behavior have much less variance that the time series at low frequency and more at high frequencies, the peaks at low frequencies appear significant but, from the information available, this do not indicate that variability in the band 21-23 years on the top panel of figure 4a is clearly different from variability in the band 24-25 years. I could admit that the authors decompose the variability in different bands such as proposed on figure 3, to investigate the variability at different time scales but no clear band with a higher variability stands out from the analysis. The only clear information is that the power increases for lower frequencies. The authors implicitly admit that point as the 3 bands investigated on Fig. 4 (15-35 yr, 40-60yr, 65-170yr) nearly cover the whole domain. It appears thus much more justified from the analysis presented in the manuscript to state that the variability has been decomposed in various bands for the purpose of the analysis rather than stating that those band clearly stands out as period of clearly enhanced variability compared to other ones. The discussion of individual peaks should then be suppressed.

2.2 From the results presented, I do not agree with the interpretation of the causes of variability in the centennial band obtained from model results. It is true that the run with solar variability has enhanced variability in this band compared to the two other runs but the differences are not that large (see point 2.1). Furthermore, the fact that the variability is also lower in the run with the full forcing (i.e. the one that should be the most realistic) do not provide arguments in favor of the interpretation proposed by the authors that the variability in this band in due to solar forcing. At this stage, there are just three simulations, one with a slightly higher variability than the other two in the band 65-170yr. This could be due just by chance. Additional simulations would certainly be required to estimate a potential role of the solar forcing. It is thus not possible to make a difference between the 40-60 yr band, for which the authors argue that the variability is due to internal processes, and the 65-170 yr band. From the results shown, variability in all the bands appears consistent with internal processes. If the authors consider that it is not the case, additional analyses are required. The authors appear not really convinced themselves by the role of solar forcing as they write "peaks in the centennial band oscillation revealed by proxy do not match those in the two simulations with solar forcing and full forcing" (page 62), line 18). The role of solar forcing should thus not be emphasized in the abstract or conclusions.

Additional points.

1. The references IPCC 2001, IPCC 2007 are used several times, in particular in the introduction. Please cite the chapter corresponding to the references for an easier access of the reader to the precise material cited.

2. Page 613, line 21. It is mentioned that " ENSO event and Quasi-biennial Oscillation (QBO) have been the primary source of precipitation over East Asia". Are they the main source of precipitation or the main source of variability?

3. Page 613, line 26. It is mentioned that "A 1500-yr cycle in Holocene monsoon dynamics has been driven by solar activities". This interpretation is controversial and the authors should indicate that it is still a hypothesis that requires to be validated or not.

4. Page 614, line 12. The authors should be more precise when mentioning the MWP as a period as warm as the last century. A lot of work has been devoted to that subject

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and the authors must specify the temporal and spatial scale they consider because conclusion could be different for different time/region.

5. Page 617, line 19. The snow cover in which region has an impact on precipitation?

6. Page 618, line 2. "more suitable" is a weak term. How can you study interannual variability if a 10-yr moving average has been performed to the time series?

7. Page 621. The authors are a bit optimistic when they mention summer precipitation patterns similar in the model and observations. The model has a large maximum around 105 E, 30N which is not present in the observations. This affect the zonal gradients of precipitation in the zone investigated. Line 13. Does the author mean "zonal gradient in summer precipitation" or "meridional gradient in summer precipitation"

8. Page 621 lines 24-29. The authors mentioned that the means of summer precipitation in the two regions are significantly different at the 99.99% level. They consider then that the model could be used to determine the variability in the two regions. However, two regions with different means could have exactly the same variability. It is thus necessary to test also if they have a different variability before considering that model results could be used to investigate the contrasted behavior of those two regions.

9. Page 624-625. The description of the different modes of variability is very general. Many processes or regions mentioned have no clear link with the precipitation in China and the discussion is thus not convincing. Would it be possible to use the model to try to understand the mechanisms responsible for the low frequency changes in China?

10 Page 627 line 10. Change "are visibly apparent", for instance, by "can be seen&". 11 Page 627 line 26. "corresponds to those episodes with different temperature conditions very well". This sentence is much too strong and should be modified.

Fig. 1. The color legend is hard to see. It would be better to use more than one color (green). The caption is not explicit enough to understand clearly the meaning of the top panel of figure 1.

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