

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

C. Shen et al.

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(referee comments in italics, responses in bold)

Referee #1

Major comments

I. Analysis of the modern record (Section 3.1) 1. The authors claim PC1 has a three belt mode of spatial pattern. But, I don't see that from Fig.1. I think the PC1 has a structure of dipole pattern with MLYRV positive and SEC negative with NC has little loading in PC1. 2. The PC 2 has a dipole pattern, but the division between NC and MLYRV is around 32N, not as the red line indicated. 3. The first two modes

are statistically inseparable if North (1982) test is applied. (Please apply this test). In that case, examination of their PCs is necessary. What are their corresponding PCs?

In the revised manuscript, we use 80-stations (instead of 37-stations in original version) network to conduct the principal component analysis. The results show that the first four PCs can be separated according to North et al. (1982)'s rule of thumb (see table). We interpret the spatial patterns based on the new PCA result.

PC	λ	Explained variance(%)	$\Delta\lambda$	$\delta\lambda$
1	14.6	18.3	3.6	2.3
2	11.0	13.7	4.3	1.7
3	6.7	8.4	1.3	1.1
4	5.4	6.8	1.3	0.9
5	4.1	5.2	0.6	0.7

$\Delta\lambda$: difference between two consecutive PCs

$\delta\lambda$: sampling error estimated by the formula of North et al (1982)

4. *This analysis serves as a basis for division of NC, MLYRV, and SEC. But the red lines in Fig. 1 do not match the division of the actual rainfall pattern. Drawing the boundaries in Fig. 1 is thus confusing. Since the NC and MLYRV have been used many times later, their geographic locations must be clearly defined.*

We define North China (34-41N; 107-120E), and the middle-lower Yangtze River Valley (26-34N; 109-122E).

5. *Fig. 2: The prominent peaks, in my view, are 2 yr for MLYRV and 3-yr for NC. The authors claim that the 5-7 yr peak is related with the ENSO period. I think this is incorrect. It has been well recognized in the ENSO community that the ENSO has a broad peak of 2-7 yr with a biennial (2-3 yr) and a low frequency (4-5 yr) component*

(Barnett 1990). While the ENSO has two components, the monsoon variability tends to be more biennial (Lau and Shen 1988). The 2 and 3 yr peak often seen in the Asian monsoon region is largely associated with ENSO turnaround (Wang et al. 2003, J. Climate). I would interpret the 2 and 3 yr peaks are associated with ENSO.

We rewrite the section, following the reviewer's suggestion.

II. Analysis of the filtered proxy data (section 3.2) 1. Fig. 3 shows 10-yr running mean DWI time series. It is not clear whether the spectra shown in Fig 4 are made using this running mean or yearly time series? This must be clarified. Without the information one cannot comment on the results. Also, what is the advantage to use MTM? How different the MTM results compared with other spectral analyses?

The spectra shown in Fig 4 are made using this 10-yr running mean DWI. MTM is chosen because this method provides useful tool for the spectral estimation of a time series whose spectrum may contain both broadband and line component. The comparison of the MTM results with other spectral analyses (e.g. Blackman and Tukey method) shows that spectral patterns are similar to each other, although there are somewhat differences in the width of spectral bands and significant levels between them.

2. Determination of the ranges for the centennial and bidecadal peaks in Fig. 4 is somewhat subjective. Clarifications are needed, because subsequent analyses are based on the subjective definition of the time scale for centennial and decadal variations. The authors seem trying to identify spectral peaks in Fig.4, but in general, why should one think the two time series should have the same preferred spectral peaks? The spectral peaks in MLYRV and NC seem not coherent on a range of time scales. A cross-spectrum analysis may help to pick up the coherent spectral peak if that is the purpose of the authors.

We conduct coherency analysis on two time series from NC and MLYRV. The result shows that coherent spectral peaks significant at 95% or 90% confidence

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level include 15-yr, 19-yr, 25-yr, 33-34-yr, 47-50-yr, 68-yr, and 149-yr. These coherent spectral peaks only cover parts of individual peaks revealed by the MTM, so we combine those individual peaks into relatively broad bands based on the result of coherency analysis and MTM. The result of coherency analysis is included in the revised manuscript.

3. *Page 620 line 2, Why do authors think the transition in the phase relationship of centennial oscillation between NC and MLYRV could have been caused by major shift of climate over China in 12th and 13th century?*

It is speculative, based on the closely matched timing between the transition and the major shift.

III. Analysis of model simulation (section 3.3)

1. *Fig.5 compares model simulated and observed MJJAS precipitation. Overall, I would say the model did poor job over the EA region. If you calculate the map correlation coefficient and root mean square error and compare to other models you would see how poor this model is. Yet, authors stated on P 621 line 11, nevertheless, the summer precipitation is well simulated in our study region. To support this statement, I suggest authors make a comparison of the climatological seasonal cycle of NC and MLYRV time series with observation. That would help to say how good the model is in reproducing the climatology for the two key regions. Also, I don't feel confident to examine a specific region if the large scale pattern surrounding the specific region is no good. Some objective assessment of the model caveats and how that would impact the results should be given. Otherwise, readers like me would have no confidence in the model results.*

We agree with the comment that the model did not do a good job in simulating summer precipitation over the East Asia region. Nevertheless, comparison of the climatological seasonal cycle of NC and MLYRV between model and observation, as suggested by the reviewer, indicates a good agreement. We add this

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comparison to figure 6.

2. *How good is the model reproducing temperature variation in general? Can the model reproduce the relationship between NC and MLYRV as well? P 622 line14. What do you mean by 8220;structure of temporal pattern is similar to8230;8221; given that they do not have any phase relationship. This type of statement needs to be quantified.*

The model reproduces observed temperature well, but we did not include it mainly because the present manuscript focuses on precipitation. In any case, we include statements in the revised manuscript. We use correlation analysis to quantify the statement in the revised manuscript.

3. *The author claim, This (centennial) oscillation is clearly visible in the solar forcing and full forcing runs, especially in the solar forcing run (P623 line 14). But Fig. 8 shows that the full forcing run does not produce significant centennial peaks (Only the solar forcing run does.). In addition, why in the full run, which includes the solar forcing, the centennial peak becomes insignificant? This question is important for claiming the centennial oscillation being forced by solar cycle.*

Indeed, the centennial oscillation in the full forcing run is not as significant as in the solar forcing run. We rewrite the statement as "This (centennial) oscillation is clearly visible in the solar forcing run, however, the full forcing run does not produce significant centennial peaks as well as the solar forcing run". We rewrite other part of this paragraph following this statement. The possible reason why the centennial peak becomes insignificant in the full forcing run is that the change amplitude of solar forcing is smaller than that of other forcings, the response of summer precipitation to solar forcing may be overwhelmed by other forcings. The centennial oscillation being forced by solar cycle is not emphasized in the revised manuscript.

4. *P623 line 17-22. The authors find that the peaks in the model centennial oscillation do not match those of proxy data. They argue that due to chaotic components of*

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internal variability in models and the uncertainty in forcing reconstruction, it is unrealistic to anticipate this type of matching. I disagree. The internal variability can destroy the phase relationship on higher frequency time scale but, if the centennial variability is due to external forcing, its phase should have a clear relationship with that of solar forcing. Can the author show this relationship? If not, how can you claim the model response is due to external forcing? We cannot take proxy and the reconstructed forcing as exact truths, but if they have no phase relationship in their evolutions, how can we see anything about response and forcing or cause and effects?

We agree with that the internal variability in models is not a factor causing this mismatched. The centennial oscillations are significant in both proxy data and solar forcing run, however, their phase relationship with solar forcing is not fixed through time. Nevertheless, the role of solar forcing for centennial oscillation is not emphasized in the revised manuscript.

5. P624 line 28 to P625line 3. Why do you expect a global forcing (solar forcing) have a regional footprint (in the eastern China)? I find no logic here.

We do not fully understand the reviewer's question, because the few lines (P624line 28 to P625line 3) discuss the pentadecadal oscillation, which is not involved with solar forcing.

Referee #2

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

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the main text? In any case, a few sentences describing those datasets and the method used to construct them would be necessary.

In the revised version, we have added a few sentences describing these datasets. CNCC-dataset was published and its source is added. For the D/W index, the reference is CNMA 1981.

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.

Yes, descriptions of the model simulations and forcings are included.

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?

The experiments were conducted using the NCAR model, which is well-documented; nevertheless, more descriptions about the model and simulations are added. To address the specific questions raised by the reviewer, yes, 300-years spinoff period was used and now explicitly indicated; and a small climate drift was identified, which will not affect the results. We did compare with published temperature of similar (but not identical) NCAR model simulation, and the results are consistent, however, no precipitation information is available for comparison.

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)*

Yes, it is explicitly indicated.

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

The term refers to precipitation from May to September, which is explicitly defined.

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 than 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.

Agree. This is why we combine those pecks into three broader bands instead of discussing the individual peaks.

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 is 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.

Yes, indeed, and we rewrite the section following reviewer's suggestion.

Additional points

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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.*

Done.

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?*

They are the main sources 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.*

Yes, it is done.

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

Yes, it is more explicit now.

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

It is over the Tibetan Plateau, as more explicitly indicated.

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?*

We rewrite the sentence as "therefore, they are used to examine decadal to centennial variability".

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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"

We mean "zonal 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.

We consider only the "mean" (not the variability) to distinguish the difference between the two regions for comparison of precipitation characteristics between model and observation.

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?

It certainly is possible to use the model to explore the mechanisms, but the research is beyond the scope of the present study.

10. Page 627 line 10. Change "are visibly apparent", for instance, by "can be seen".

It is changed

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.

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It is 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.

We redraw the figure 1 and delete the top panel.

Interactive comment on Clim. Past Discuss., 4, 611, 2008.

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