Clim. Past Discuss., 4, S360–S364, 2008
www.clim-past-discuss.net/4/S360/2008/

Climate of the Past Discussions

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

Anonymous Referee #1

Received and published: 4 August 2008

This paper offers a combined analysis of instrumental measurements in the recent decades, 1500-yr proxy data, and millennium model simulation for understanding of the variability of precipitation over eastern China. They found, with the proxy data, that centennial (65-175yr) oscillation was weak (strong) in warm (cold)condition, while the pentadecadal oscillation is strong during both warm and cold conditions. Their model results suggest that centennial oscillation may be linked to Gleissberg solar cycle, while pentadecadal and bidecadal oscillations are results of the internal variability. They also concluded that the recent increased frequency of drought in north and flood in south is unusual over the past five century. The strategy and motivation are well laid out and the results are summarized well.

However, figure captions and explanation of the procedure are incomplete in many

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

places, which make it hard to read. Some of conclusions are unsupported by enough evidence and thus not convincing. Some analysis results has flaws in couple of places.

The manuscript needs major revisions and improvement.

Major comments

1. Analysis of the modern record (Section 3.1)

2. 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.

3. The PC 2 has a dipole pattern, but the division between NC and MLYRV is around 32N, not as the red line indicated.

4. 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?

5. 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.

6. Fig. 2: The prominent peaks, in my view, are 2 yr for MLYRV and 3-yr for NC. Use of 90% significant level is too low. In addition, the caption is incomplete. For instance, what does the term "summer" mean here, and what precipitation units were used?

7. 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

CPD

4, S360–S364, 2008

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.

8. Analysis of the filtered proxy data (section 3.2)

   (a) 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?

   (b) 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 "common" 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.

   (c) 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 12^{th} and 13^{th} century?

9. Analysis of model simulation (section 3.3)

   (a) 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

CPD

4, S360–S364, 2008

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 models’ caveats and how that would impact the results should be given. Otherwise, readers like me would have no confidence in the model results.

(b) 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 “structure of temporal pattern is similar to...” given that they do not have any phase relationship. This type of statement needs to be quantified.

(c) 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.

(d) 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 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 forc-

CPD

4, S360–S364, 2008

ing, 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?

(e) 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.

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