**Hydrology and
Earth System
Sciences
Discussions**

Interactive comment on "Dryness/wetness variations in China during the first 50 years of the 21st century" *by* J. Q. Zhai et al.

Anonymous Referee #1

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re-review of 'Dryness/wetness variations in China during the first 50 years of the 21st century' by J.Q. Zhai, B. Liu, H. Hartmann, B.D. Su, and K. Fraedrich (MS No.: hess-2008-1006)

The authors have addressed most of the point satisfactorily and improved the ms. significantly.

However, there are a few remaining issues. I would like to advise the editor that these issues need to be addressed before acceptance of the paper.

- original remark: *The SPI is based solely on precipitation. In a study on drought, evaporation should be discussed as well - or the title and focus of the study should*

  be changed to "Precipitation variations in China...."
  The authors motivate the choice of leaving evaporation out of this study by saying that they focus on 'meteorological drought', rather than 'drought'. That is a valid standpoint, but must be made clear. Explain that you mean 'meteorological drought' when you use the terms 'dryness/wetness' or 'drought'.

- original remark: *The ECHAM5/MPI-OM model is one of the best models available for this type of work .... This point directly relates to Fig. 5 and its discussion. This figure shows the results for the three greenhouse gas scenarios and there is considerable variation between the simulations. These variations concerns the amplitude of the trend, but - more worryingly - the sign as well! The authors must make clear what part of this signal is robust and what part of the signal is likely to be related to climate variability.*
  In my view, you need to use the information of the spread in the ensemble to estimate the robustness of the results. The current approach, to study the ensemble mean only, fails to take advantage of this information. It is insufficient to ignore this issue with the motivation that this is a topic for a next paper. Additionally, it was not clear to me that you used the average of the three simulations.

- original remark: *§2.2: In my opinion, the use of a spatial interpolation method after a selection of areas with a high number of dry month to produce a fully covered grid is plain wrong. There is no need to use this technique at all: you can do the analysis directly. So, for each gridbox, you count the number of months with a SPI value < -1, and on this grid you calculate trends. The method followed in the paper leads to erroneous results.*
  If I understand the method right, you make a selection of gridboxes that have more than 100 months with SPI values < -1. I do not see why you need this selection, this is the point you have to explain. However, if my understanding of the method is incorrect, then this indicates that you need to spend more effort in a clear explanation.

Note that I don not object against some kind of spatial smoothing, as long as this does not distort the overall picture too much.

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