

Interactive comment on “Identifying water deficit and vegetation response during the 2009/10 drought over North China: Implications for the South-to-North Water Diversion project” by B. Zhu et al.

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General comment:

The focus on drought alleviation in relation to the SNWD seems inappropriate. Rather the SNWD was developed to deal with the high water demand in North China which is a problem irrespective of whether drought conditions occur. Further, over-extraction of groundwater from many areas of North China, especially the North China Plain, has a much greater and more permanent impact on groundwater storage than droughts. To ascribe North China's problems with water as being caused by droughts is an extreme

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simplification of the water resources situation there.

The introduction is rather divorced from the content of the body of the paper which focuses on the use of GRACE in analysing water storage in the NCP region. The introduction should focus on the main topic of the paper with a review of key papers from which it can be shown that the work reported in this paper is a worthwhile development on what has been already done in this field and then go on to show how this will be achieved.

The Data and Methods Section (Section 2) lacks a clear description of the methodology of this study. It begins with a rather misleading description of the field area and describes the GRACE system and data sources, proceeding to say that the hydrological modelling described is taken from some other source and is not part of this research. The modelling is reported to have been "evaluated in North China with acceptable uncertainties" (lines 147-148) but with no description of what this means.

Water resources are heavily used in this region, especially in the southern part on the North China Plain, so the focus on drought and failure to take more account of resources use is strange. There is some discussion of this in the section from line 305 to 312, beginning: " One may wonder the role of human over-use of the water resources." Indeed.

Specific comments:

This paper begins with an incorrect statement by claiming that a paper by Palmer 2002 is the source for a statement that " The global climate system has significantly changed in recent years, leading to an increased frequency of extreme weather and other disaster events". The paper is actually by Palmer and Räisänen and it does not say that there has been a change in recent years leading to increased frequency of extreme events but rather it discusses the probability of this occurring in the future. This paper cannot be used to support such a claim for north China. Also, the paper was written 14 years ago and a lot more has been published on this topic since. I wonder how many

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other references used in this paper would stand up to scrutiny?

Lines 49-50: "Drought frequently occurs in most areas of China and accounts for 35% of all economic losses from disasters." No source is given for this specific piece of information.

Line 78: 'SPI' not defined until line 167.

North China, as defined in Fig 1b, is not the area with the most severe water shortage in China (line 50); only the western part of that area is semi-arid; and based on the authors' own map of precipitation distribution (Fig 1a) it receives a lot more than 500mm/yr.

Line 95: "the average per capita water resource is only 23% of the Chinese average." Where does this information come from?

I cannot agree that the spatial distribution of the LAI reduction in Fig 11b is consistent with the area of water storage deficit in Fig 6.

The authors consistently refer to the probability of precipitation in the drought being 84%. I don't understand what they mean by this.

Technical corrections:

Many, perhaps most, of the references are improperly or inadequately cited both in the text and in the reference list.

Section 2.2.3: Precipitation data from the Chinese Met. Admin. have been gridded using a 1984 SYMAP system and "extensively verified for runoff, evapotranspiration, and soil moisture (Zhang et al., 2014)." It is not at all clear what this reference to Zhang et al means. Their work is a dataset of hydrological fluxes and states at 3 hr interval for China for the period 1952-2012 developed using gridded data and the VIC model already referred to earlier in this paper. Zhang et al report that the data set is available on the web. So have the authors of this paper used the Zhang et al data, and if so, why

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don't they just say so? If they don't use the Zhang et al data, what do they use, and why do they refer to Zhang et al in this way?

Section 2.3.1. Given the focus in this paper on drought, it is somewhat surprising that the SPI is simply selected as the drought measure without discussion or reason. What about the more widely used Palmer Index, or even the drought classification of the Chinese Meteorological Association. The description of the SPI given in this section appears to have little relation to what McKee et al wrote. McKee et al say that: "A drought event for time scale i is defined here as a period in which the SPI is continuously negative and the SPI reaches a value of -1.0 or less. The drought begins when the SPI first falls below zero and ends with the positive value of SPI following a value of -1.0 or less." (no page number available). In this paper the authors state (lines 178-179): "When the time periods are small (1 or 6 months), the SPI frequently fluctuates above and below zero (McKee, 1993)." McKee et al do actually say this but it is in reference to their example of the SPI for Fort Collins. It appears to have no relevance to the way the SPI should be used in this study. Note also that the text above is a direct quote from McKee et al though not identified as such. After all that, Section 2.3.1 doesn't actually tell us how they dealt with the SPI, though there is more on that in the results.

Section 2.3.3. Given the spatial distribution of groundwater gauging stations shown in Fig 1b, they do not provide a reasonable cover of the whole area of interest. This matter is not considered at all here in using the first method for estimating GWC. Large parts of the study area have no data.

Line 235: The usual definition of the hydrological year is that it begins at the month of lowest flow/precipitation. In that case the hydrological year here would start in February. So why has it been arbitrarily started in May?

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