Anonymous Referee #2

Received and published: 7 August 2012

Your manuscript links long-term (1982-2006) hydroclimatology data with remotely sensed vegetation data for a north-south transect in eastern China. While you have attempted to draw some fundamental relationships, which is to be applauded, by: (i) using a sub-optimal formulation of potential evapotranspiration; (ii) not accounting for water-storage carry-over at the start of the growing seasons or stream flow during the growing season; (iii) providing little discussion of the difference between climate change and climate variability; and (iv) not providing a theoretical background, this means my recommendation is that a major revision is needed. Some new analysis is needed and the structure of the manuscript, in my opinion, needs to be improved.

Answer:

(i): I changed my potential evapotranspiration model to a new formulation, which captures the four main meteorological variables (wind speed, radiation, humidity and air temperature) as suggested. All data related to ETp/ETa had been updated. The model is the FAO-56 (Allen et al., 1998) crop reference evapotranspiration (ET0) model. I calculated ETp in the specific form of ET0).

(ii): the assumption related to water-storage carry-over had been discussed in the section: 2.5 Definition of growing water deficit index (GWDI).

(iii): We did.

(iv): The theoretical background of GWDI had been added. See section 2.5.

Using the same sub-headings for your Methods, Results and Discussions sections asthe scientific objectives / questions that you ask will greatly improve your manuscript. Ilook forward to seeing a much improved next version.1) Introduction. The current introduction is a pedestrian; it needs to be rewritten to be much more compelling. Why should a reader spend the time reading and learning from your manuscript? Currently there are many facts provided, however it is difficult to distil the key scientific question that is going to be asked and how previous papers provide the impetus (or spring-board) for your current study. In my opinion there is a little too much discussion about remote sensing re-visit rates and not enough discussion about the science question(s) being addressed in your manuscript.

2) Mini-review. I would like to see a tabular mini-review of methods, findings and implications
of previous similar papers that have been conducted over the world. Providingthis in the Introduction provides a global context which your results can then be discussed
in the Discussion section.

3) Structure: I would much prefer to the see following used as the basis for your
manuscript. 1. Introduction – finish this section with the 3-4 questions / objectives / hypothesis
that form the backbone of your m/s 2. Materials – have as many sub-headings
as you need in this section to introduce all the datasets that you use 3. Methods – this
has 3-4 sub-headings which directly relate the questions / objectives reported at the

end of your Introduction 4. Results – this has the 3-4 sub-headings which directly relate the questions / objectives reported at the end of your Introduction 5. Discussion – again this has the 3-4 sub-headings which directly relate the questions / objectives reported at the end of your Introduction 6. Conclusion
Re-structuring your manuscript in this manner will improve the accessibility and clarify of your analysis and thinking.

Answer:

Major revision had been made for the whole m/s based on the suggestion.

4) P6651 L2, why must be a change in form or function, why not form and/or function?

Answer:

Yes, changed.

5) P6651 L5, if warming were occurring in isolation then yes in conditions that are not water-limited we would expect actual evapotranspiration (ETa) to increase. HOWEVER, atmospheric demand, as measured by declining rates of pan evaporation (Epan), has been reducing in many locations over the past few decades. Please see McVicar et al., (2012a); their Table 5. You need to discuss the trends in the 4 meteorological variables that influence the evaporative process. The impact of changes in atmospheric evaporative demand varies depending on the limit to ETa: being energylimited (EL), water-limited (WL) or equitant (straddling the EL-WL limit); please see McVicar et al., (2012b and the references therein) for a discussion of this concept.
McVicar, T.R., Roderick, M.L., Donohue, R.J., Li, L.T., Van Niel, T.G., Thomas, A., Grieser, J., Jhajharia, D., Himri, Y., Mahowald, N.M., Mescherskaya, A.V., Kruger, A.C., Rehman, S., and Dinpashoh, Y. (2012a) Global review and synthesis of trends in observed terrestrial near-surface wind speeds: Implications for evaporation. Journal of Hydrology. 416-417, 182-205, doi:10.1016/j.jhydrol.2011.10.024
McVicar, T.R., Roderick, M.L., Donohue, R.J. and Van Niel, T.G. (2012b) Less bluster ahead? Overlooked ecohydrological implications of global trends of terrestrial nearsurface wind speeds. Ecohydrology (Accepted) doi: 10.1002/eco.1298.

Answer:

Literature had been updated. The logic had been concerned in the revision.

6) P6651 L20, in a water-limited case, yes then precipitation (P) trends will have the largest impact on vegetation trends. However, in an energy-limited case, then it would

likely be changes in radiation (and other forms of energy input) that the environment will respond to. Along this vein, I am surprised that the following paper is not referenced herein.

Nemani RR, Keeling CD, Hashimoto H et al. (2003) Climate driven increases in global terrestrial net primary production from 1982 to 1999. Science, 300, 1560–1563.

Answer:

Yes, agree. Literature had been added.

7) P6652 L5, some papers need to be cited at the end of this sentence, there are several to select including

Donohue, R.J., McVicar, T.R. and Roderick, M.L. (2009) Climate-related changes in Australian vegetation cover as inferred from satellite observations for 1981-2006. Global Change Biology. 15(4), 1025-1039.

Goetz SJ, Bunn AG, Fiske GJ, Houghton RA (2005) Satellite observed photosynthetic trends across boreal North America associated with climate and fire disturbance. Proceedings of the National Academy of Sciences of the United States of America, 102, 13521–13525.

Myneni RB, Keeling CD, Tucker CJ, Asrar G, Nemani RR (1997) Increased plant growth in the northern high latitudes from 1981 to 1991. Nature, 386, 698–702.

Tucker CJ, Slayback DA, Pinzon JE, Los SO, Myneni RB, Taylor MG (2001) Higher northern latitude Normalized Difference Vegetation Index and growing season trends from 1982 to 1999. International Journal of Biometeorology, 45, 184–190.

Answer:

Literature had been updated.

8) P6652 L15, does ET here mean actual evapotranspiration (ETa) or potential evapotranspiration (ETp)? It is good to explicitly define these terms and use the abbreviations accordingly.

Answer:

Yes, we defined based on your suggestion.

9) P6653 L27, potential susceptibility to what? Is this to climate change? Is this the case then I am wondering how you isolate climate change (CC) from climate variability (CV) in your analysis. I look forward to seeing how this is performed.

Answer:

Here we discuss potential susceptibility to the rising growing season water deficit, the index captures the variables of atmospheric evaporative demand and vegetation water assumption, including Tair and Precipitation. For climate change, we concern about the consistent long-term trends.

10) P6654 L7, as explained in McVicar et al., (2012b Figure 1 caption), the units should be depth integrated over time (units = mm a-1). This is why trends in P, ETa, ETp and Epan should have the units of mm a-2. For a plot from which the trend can be calculated the abscissa (or X-axis) data are years (units = annum or a) and the ordinate (or Y-axis) data are depth integrated over time (units = mm a-1), so the resultant slope (i.e. dY/dX) has units of mm a-2.

Answer:

Yes. Revised.

11) P6655, L11, the widespread use and theoretical basis of ANUSPLIN are documented in the Supplementary Material of McVicar et al (2010).
McVicar, T.R., Van Niel, T.G., Roderick, M.L., Li, L.T., Mo, X.G., Zimmermann, N.E. and Schmatz, D.R. (2010) Observational evidence from two mountainous regions that near-surface wind speeds are declining more rapidly at higher elevations than lower elevations: 1960-2006. Geophysical Research Letters. 37, L06402, doi:10.1029/2009GL042255

Answer:
Yes. Literature updated.

11a) P6655, L24, there are several long-term AVHRR datasets that are processed in different approaches, hence it would be savvy to defend your decision to use the GIMMS processed AVHRR data based on the findings of Beck et al (2011).
Beck, H.E., McVicar, T.R., Van Dijk, A.I.J.M., Schellekens, J., de Jeu, R.A.M. and Bruijnzeel, L.A. (2011) Global evaluation of four AVHRR-NDVI data-sets: Intercomparison and assessment against Landsat imagery. Remote Sensing of Environment. 115(10), 2547-2563, doi:10.1016/j.rse.2011.05.012

Answer:
Yes. Literature updated.

12) P6655 L15 and Table 1. It seems that the climate surfaces are at a monthly timestep (P6655 L15), yet the current static the start-date and end-date of the growing seasons for the 12 vegetation classes are defined with a daily time-step (Table 1). How is this important difference resolved?

Answer:
We use curve fitting method, specifically, we develop the daily time-step phenological curve based on smooth spline method. We published this method in:

Yu, Z., Sun, P. S., Liu, S. R.: Phenological change of main vegetation types along a North-south transect of eastern China, Chinese Journal of Plant Ecology (In Chinese), 34, 316–329, 2010.

13) P6656 L20 and Table 1, RE Growing season variability and trend. You are using data from 1982 to 2006, yet in Table 1 the start-date and end-date of the growing seasons for the 12 vegetation classes are defined with no measure of variance around them. I would have expected that due to both climate variability and climate change that there would be different start-dates and end-dates throughout the entire series, yet it seems these dates are fixed. Please perform some additional analysis showing the series of the start-date and end-date for each of the 12 vegetation classes, illustrating the variance and the trend.

Answer:
Yes, Now I add statistics to the phenological events. I listed them in Table 1 just to show some how the growing season length had been determined.

14) P6657 L3, I think you mean near-surface air temperature (Tair) which is typically observed at a height of 2 m above the ground-level, as opposed to the surface temperature (Ts).

Answer: Correct, revised.

15) P6657 Section 2.3, based on the results presented in McVicar et al (2012a Table 7 and the references therein) you need to discuss and use a formulation of ETp that explicitly accounts for trends in the 4 meteorological variables that influence the evaporative process. In China, and elsewhere, as seen in the Attribution studies reviewed there (i.e., McVicar et al 2012a Table 7) the clear importance of wind speed trends on the evaporative process is clearly seen; it can not nor should not be implied that wind speed can be assumed to be non-trending. You defend your choice of ETp formulation based on its use in the humid forests of the eastern US, yet you are dealing with 12 vegetation classes that are not all forested (Table 1) and are not all in locations that the

climate could be described as being ‘energy limited’.

Answer:

Good suggestion. The whole section had been changed. I changed the original Harmon method to a ‘fully physically based’ PET model: FAO-56 (Allen et al., 1998) to calculate PET in the specific form of reference ET (ET0). The FAO -56 model is Penman based model considering all four important variables including wind speed.

15a) P6657 Section 2.3, Plus I assume that h is the actual hours of sunshine (what would be lower case n in the Angstrom–Prescott equation) compared to the theoretical total sunshine hours (if there were no cloud – the upper case N in the Angstrom–Prescott equation).

Answer:
Yes, exactly. For the widespread FAO-56 method, I don’t think this is necessary to introduce.

16) P6657 Budyko method (L21-24). As an introductory background to Budyko’s framework, explicitly discussing the spatial and temporal aspects, you should read and consider citing Donohue et al (2007). The paper by Zhang et al. (2001) did not discuss rainfall seasonality. The following papers do discuss rainfall seasonality (Donohue et al 2012 and Zhang et al 2008). Recently Donohue et al (2012) developed the Budyko–Choudhury–Porporato (or BCP, model) to better model ETa than can be done using Choudhury’s formulation of Budyko. The BCP model provides estimates of n (used in Choudhury’s formulation of Budyko, this is a catchment-specific model parameter that alters the partitioning of P between stream flow (Q) and ETa – which is estimated as a function of plant-available soil water holding capacity (j), mean storm depth (a) and effective rooting depth (Ze). It could be worthwhile to use the BCP model to estimate steady state ETa. If you use the Zhang et al. (2001) formulation for modelling steady state ETa then where did you obtain the fractions of landscapes that were non-forest compared to forest for each of the 12 vegetation classes?

Donohue, R.J., Roderick, M.L. and McVicar, T.R. (2012) Roots, storms and soil pores: Incorporating key ecohydrological processes into Budyko’s hydroclimatic framework. Journal of Hydrology 436–437, 35-50. doi: 10.1016/j.jhydrol.2012.02.033
Donohue, R.J., Roderick, M.L. and McVicar, T.R. (2007) On the importance of including vegetation dynamics in Budyko’s hydrological model. Hydrology and Earth System Sciences. 11(2), 983-995.
Zhang L, Potter N, Hickel K, Zhang YQ, Shao QX. 2008. Water balance modeling over variable time scales based on the Budyko frameworkâ˘A˘ Tmodel development and testing. Journal of Hydrology 360(1–4): 117–131.

Answer:

Yes, I updated these literatures in the introduction and theoretical framework sections. The vegetation types that I selected to analyze based on a couple of GIS measures just to ensure its purity, for example, buffering. This had been introduced in section 2.2, *datasets and spatial interpolation.*

16) P6658, Section 2.5 Growing season water deficit index (GWDI). The formulation of GWDI assumes: (1) there is no carry-over of water from the non-growing-season that impacts growth during the growing-season; (2) there is no carry-over of water from the previous year(s) that impacts growth during the growing-season (Richard et al 2006); and (3) there is negligible streamflow generated from the area during the growing season. These are implied assumptions that need to be explicitly stated. For the 12 vegetation classes for each year you need to calculate what proportion (or percentage) of annual precipitation is received during the growing season; I envisage a new 12-part figure with time on the X-axis (1982-2006) and the Y-axis being the proportion (or percentage)of annual precipitation is received during the growing season. Also on this plot you may wish to show the actual annual precipitation as a second (or right hand side) Y-axis so that readers can better appreciate the climate variability experienced by the 12 vegetation classes. While you could use a static growing season as the basis for this proportion of annual precipitation calculation I would prefer using a dynamic growing season definition to account for CV and CC in all your revised calculations. Most likely, being in eastern China, which is dominated by the summer monsoon, this will be in the order of 60-80%. However, if there was a growing season with large rainfall (and hence lots of clouds and low temperatures so therefore a low ETp) this would result in a high GWDI, yet lateral water loss from the area is ignored. It is important for you to note this method would not be suitable for Mediterranean climates (most P in winter and most growth in summer), as the volume of the soil-store that carries over the winter to the following summer is critical for growth. This means that the current formulation of the GWDI is not globally applicable, and to be globally applicable a suitable time-step water balance model would need to be run in the background to define this carry-over.

Richard, Y., N. Martiny, N. Fauchereau, C. Reason, M. Rouault, N. Vigaud, and Y. Tracol (2008), Interannual memory effects for spring NDVI in semi-arid South Africa, Geophysical Research Letters., 35, L13704, doi:10.1029/2008GL034119.

Answer:

Yes, similarly with the widespread aridity index (AI), in the section 2.5, I have stated my assumptions for GWDI as:

We chose not to define ecosystem water storage change (dSW/dt) and stream

flow (Q) in GWDI for the three main hydrologic conditions: (i) arid condition, the soil water storage is usually very low, the interception evaporation, soil evaporation and plant transpiration are highly depend on within-growing season precipitation. (ii) humid condition, the soil water storage reaches maximum, ETa becomes energy limited as described in Mcvicar et al.,(2012b). In the above two cases, the carry-over of water from the previous year(s) that available for plant transpiration, soil evaporation during the whole growing-season is assumed to be negligible (Richard et al, 2006). (iii) semiarid/semihumid condition, similar with 'equitant' climate, as defined by Mcvicar et al.,(2012b) according to dominant limitations (water or energy limitations) of ETa typically vacillates seasonally. In this case, the dSW/dt maybe more influential than in the first two cases, but we still can assume the effect of soil water carry-over is short-term, within the first month of growing season, for the whole growing season is negligible.

Please check the revision section 2.5 for more details.

Regarding the precipitation, I have shown more information in either monthly, annually and growing seasonally in Table 3.

Dynamic growing season length is not feasible for comparison analysis either inter- or intra- vegetation types.

The main advantage of using GWDI is that it captures not only the meteorological variables related to atmospheric evaporative demand (ETp), but also the actual vegetation water assumption (ET0). Unlike the aridity index (AI) defined as the ratio of P/ETp (Budyko, 1974), GWDI explicitly takes vegetation response into account, therefore the index allows for better diagnose of ecosystem water balance which may suggestive of long term vegetation activity change. For mediterranean climates, we may use the annual based water deficit index. Another reason that I choose growing season is to avoid the snow pack influence.

16a) P6658, L8, what does the statement 'sound water status' mean?

Answer:

Revised.

16b) P6658, L10-11, how vegetation responds to a continuing water stress will be community, to species, specific. How a landscape responds to drought depends on the many factors, and you should consider that most of the communities will have experienced similar droughts in the paleo-record and hence I don't agree that you can state that a GWDI > 1 indicates increased susceptibility to environmental change, as you are ignoring the carry-over of water stores into the growing season. These (usually) deeper water resources (in the soil and groundwater systems) are typically what has allowed vegetation communities to withstand previous drought conditions. To successfully argue that GWDI > 1 indicates increased susceptibility to environmental change you need to develop / refer to a conceptual model that has a suitable timeframe for the analysis you undertake and is hydrologically balanced. Considering the temporal extent of your analysis is of paramount importance here, especially for forests, as you are dealing with 25 years of data and the ecohydrological equilibrium concepts you use are relevant for a longer time-frame for deeper rooted forests. It is all a matter of signal-to-noise given longer term trends.

Answer:

1) When I talk about the vegetation response, I mean NDVI greening or browning trends, may also including the change in growing season length, not the specific species succession.

2) They are not landscapes, they are purified vegetation types with dominant species. The vegetation areas have been buffered to remove edge effect.

3) I re-defined the semiarid and semihumid thresholds in the GWDI frame work. I think this maybe helpful to predict vegetation activity change. Instead of arguing vegetation susceptibility to environment (I know it is hard to define a threshold of vegetation modification), I discussed more about the greening or browning trends reversal in the revision.

17) Figure 2. Can you please make this a 6-part figure and show the baseline 1982 surfaces (from which the percentages changes are calculated from) in addition to the 3 percentages changes already provided. It's interesting to me that in the NE part of

the transect precipitation has decreased (Fig 2b) yet Ti-NDVIg has increased (Fig 2c), and this pattern is essentially reversed for the area of large P increase in the southern part transect (i.e., near Hunan Province and Jiangxi Province).

Answer:

Figure 2 exhibits the average change rate during the study period, because of the annual and spatial variations, baseline year is not so desirable for the readers to compare. NE part of the transect Ti-NDVg increased though not significantly, this reflects the counteracting effect of warming and decline P, obviously, warming (Prolong growing season length) may have dominant effect in this region. While in the south, warming is not so much influential to plant growth, compared with other factors, e. g., radiation, wind speed.

18) Fig 3a, why do you expect that vegetation types SEBF and STG have such low correlation coefficients between Ti-NDVIg and MGP? IN Fig 3b what is going on for vegetation type TGS?

Answer:

SEBF and STG are in high precipitation regions, they are not sensitive to MGP. TGS, the temperate grass steppe, had been previously found significant increase trend in NDVI during 1982 – 1998 (Piao, et al., 2006) . While during my study period (1982-2006), TGS increase trend is not significant (see Table 4). The decreasing pace in greening is attributes to progressing water limitation despite of warming continues. Thus TGS is less strong-correlated with temperature than others. It is still significant!

Piao, S., Mohammat, A., Fang, J., Qiang, C., and Feng, J.: NDVI-based increase in growth of temperate grasslands and its responses to climate changes in China, Global environ. Chang., 16, 340-348, 2006.

19) P6662, L20-24, does this mean its climate change or climate variability?

Answer:

I originally mean climate change, since you asked, I think for CTCF is climate change and for the other two, could be climate variation.

20) Fig 5, for many of the vegetation types there appears to be an increasing GWDI as a function of time. It would be to display the equation of the line-of-best-fit on each of these plots. However, this could be an artefact of the use of your formulation of ETp and/or the natural climate variability in eastern China.

Answer:

Yes, Now I have already changed to a more complex ETp formation. In fact, My index in very similar with the aridity index. See *section 2.5.*

21) Fig 6a. This hyperbolic relationship is due to how the Budyko framework models ETa which is then used in your model of GWDI. If you algebraically relate the Budyko

framework with you model of GWDI you will derive an equation for GWDI that follows fits these data. This is an entirely expected result.

Answer:

Now I use a theoretical framework to explain this. The following figure and text is from a revised version ms.

GWDI is based on the following assumptions:

Assuming steady-state water balance, we chose not to define ecosystem water storage change (dSW/dt) and stream flow (Q) in GWDI for the three main hydrologic conditions: (i) arid condition, the soil water storage is usually very low, the interception evaporation, soil evaporation and plant transpiration are highly depend on within-growing season precipitation. (ii) humid condition, the soil water storage reaches maximum, ETa becomes energy limited as described in Mcvicar et al.,(2012b). In the above two cases, the carry-over of water from the previous year(s) that available for plant transpiration, soil evaporation during the whole growing-season is assumed to be negligible (Richard et al., 2008). (iii) semiarid/semihumid condition, similar with 'equitant' climate, as defined by Mcvicar et al.,(2012b) according to dominant limitations (water or energy limitations) of ETa typically vacillates seasonally. In this case, the dSW/dt maybe more influential than in the first two cases, but we still can assume the effect of soil water carry-over is short-term, within the first month of growing season, for the whole growing season is negligible.

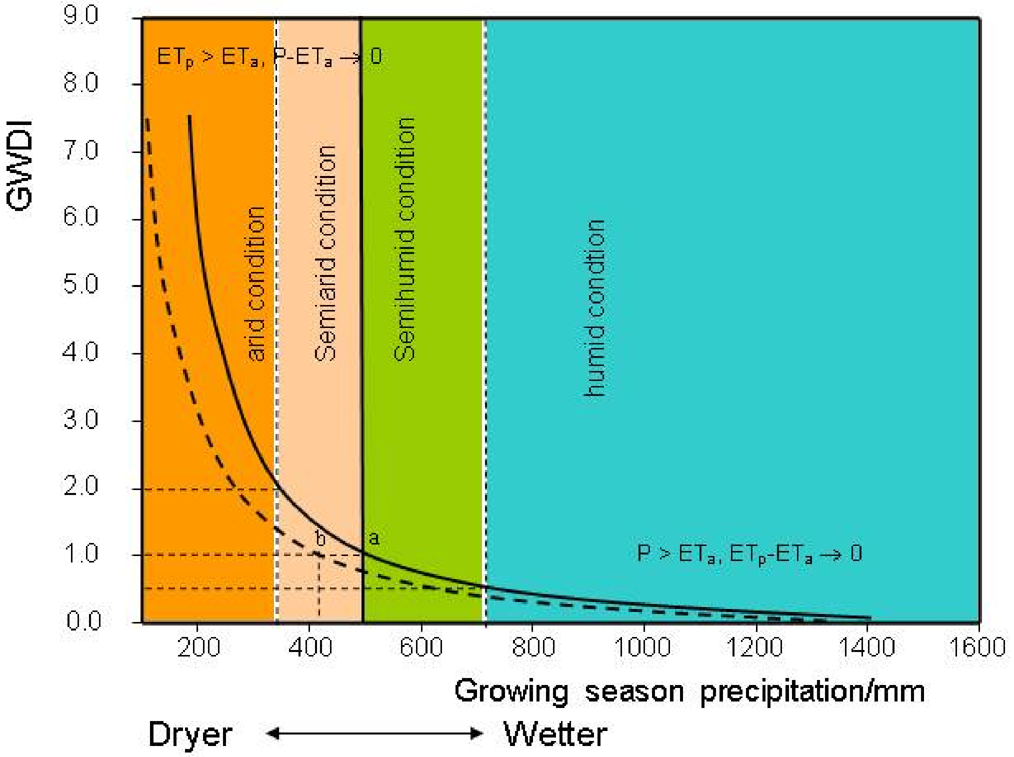

Figure 2 describes the GWDI response curve to precipitation. The vertical solid line represents the balance precipitation that corresponding to GWDI = 1, the same condition with AI = 1 (assuming P = ET0), and is likely the 'equitant' condition defined by Mcvicar et al (2012b) according the dominant limitations of evapotranspiration. The boundaries of the semiarid/semihumid condition are somewhat arbitrary, defined here by +100% (GWDI = 2.0) and -50% (GWDI = 0.5) from the case where GWDI = 1.0. For 'equitant condition', soil water storage may play a role in compensating the influence of short-time water deficit to vegetation ETa. The left-shifted ($a \rightarrow b$) balance line (dash-line curve in figure 2) represents that the potential influences of soil water storage (e.g., 100 mm dSW/dt is equivalent to 100 mm precipitation) to GWDI. In extreme dry condition (GWDI > 2.0), the plant growth and ETa are water-limited ($P - ETa \rightarrow 0$ and $GWDI \rightarrow \infty$). The significant upward trend in GWDI together with a browning trend in vegetation, may indicate a rising vegetation susceptibility to environmental change. In extreme wet condition (GWDI < 0.5), the plant growth and ETa are energy-limited ($ETp - ETa \rightarrow 0$ and $GWDI \rightarrow 0$). The ecosystem retains more than demand water resources which is available for plants use and saturation excess runoff (blue water) (Calder, 2005; Liu et al., 2009).

22) Fig 6b. Why would you expect the slope coefficients between AGDD and GWDI for the temperate regions to be higher than those for the cold temperate and subtropical vegetation types? What is the ecohydrological driver of this result? Is Tair increasing more rapidly in where the temperate vegetation types are located when compared to Tair where the cold temperate and subtropical vegetation types are located? Or it is due to changes in P and Tair, I am also curious about trends in other meteorological variables that govern the evaporative process.

Answer:

This is a good question. The reason I guess in the dry area, Tair increasing results more rapidly evaporative water loss, through interception evaporation, soil evaporation, as "water-limitation (Mcvicar, 2012)" area, the interception evaporation and soil evaporation may occupies a large proportion in ETa.

23) Fig 7.The use of Ti-NDVIg < 3.2 can also be used for long-term averages, as you have done. It can not be used on an annual time-step; you need to explicitly state this.

Answer: yes. I use growing season NDVI just to avoid snow influence in non-growing season.

24) P6664, L3, who made these 'previous assertions'? Or where they findings based on data analysis or assertions based on reasoning?

Answer: Yes, I revised.

25) P6664, L15-16, suggest that part of the dramatic ETp increase reported is due to using a sub-optimal formulation of ETp. By sub-optimal I mean one that does not capture the trends of all the primary meteorological variables influencing the physics of evaporation rates.

Answer: Now it is no longer sub-optimal. I use a new ETp formation as described in question (15).

26) P6664, L16, I can understand what ETg is from Table 5 (noting I think this should be ETa_g (so it can never be confused with ETp_g) yet I do not think this symbol ETg is ever defined in your text. All symbols / abbreviations need to be defined in the body of your paper (the abstract should be considered a separate document).

Answer: I re-defined them now.

27) P6664, L18-27, you seem to be reporting your results (so this text seems more suited to the results sections).

Answer: I changed this text.

28) Discussion – the text in the Discussion needs more work; it seems that the wind has gone out of your sails RE the quality of the English expression. This needs to be improved.

Answer: I have improved the whole discussion.

29) P6665, L1, why obviously only a result of warming? Concentrations of CO_2 are increasing (or enhanced, herein denoted eCO_2) so this could also be a partial cause for this response. There is some literature suggesting that eCO_2 will impact preferentially impact woody species (or species with a C3 photosynthetic pathway compared to those with a C4 photosynthetic pathway). Please see Donohue et al (2009 and the references therein) for more discussion on this issue. As your study did not consider eCO_2 there is no way you can scientifically discount this as a potential (likely partial) cause for this response.

Answer: I changed this wording.

30) P6666, L1-3, this is the end of an important paragraph. Currently, the majority of

this paragraph summaries results and then ends with one sentence, with no citations to previous research, discussing the scientific implications of the findings. This needs to be improved; the broader scientific discussion of your findings needs to be better placed in the scientific framework for understanding how ecohydrological equilibrium may be impacted by a changing climate.

Answer: Yes, I add discussion here based on the scientific framework of GWDI. I discussed the ecological implications of GWDI trends.

C3591

HESSD

31) P6666, L14, what you are saying that Tair has increased steadily in China directly contradicts the results presented in Liu et al (2011 Table S4 – the Supplementary Material Table S4). In that Table for the entire nation, and 8 sub-regions, they show that the rate of Tair increase has accelerated from the early 1990s to 2007 when compared to the period defined by 1960 to the early 1990s. As you are dealing with a shorter time period this could be an explanation for your result of assumed linearity during 19tc82-2006, however, I am unaware that you actually tested for a break-point spatially in the Tair grid time series. It is imperative that you do so.
Liu XM, Luo YZ, Zhang D, Zhang MH, Liu CM. 2011. Recent changes in panevaporation dynamics in China. Geophysical Research Letters 38: L13404. DOI: 10.1029/2011GL047929.

Answer:

I tested the trend based on non parametric Mann-Kendal test. I don't think there is contradiction between mine and Liu's results. I use simulated surfaces and spatially averaged.

The trend test is highly dependent on the time span of data used. During our study period (1982-2006), the over all transect PET upward trend is certain, this is agree with a recent PET study for China by using modified P-M method (The following figure, Liu Changming and Zhang Dan, 2011, Acta Geographica sinica, 579-588), although the PET showed downward trend for the whole study period of 1960 - 2007, but upward trend is significant during 1991- 2007.

Moreover, the upward PET trend can also be proved by pan evaporation (Epan) result, which increased averagely 7.94mm a-1 during 1992-2007 (Liu et al., Geophys. Res. Lett. 38, L13404. doi:10.1029/2011GL047929).

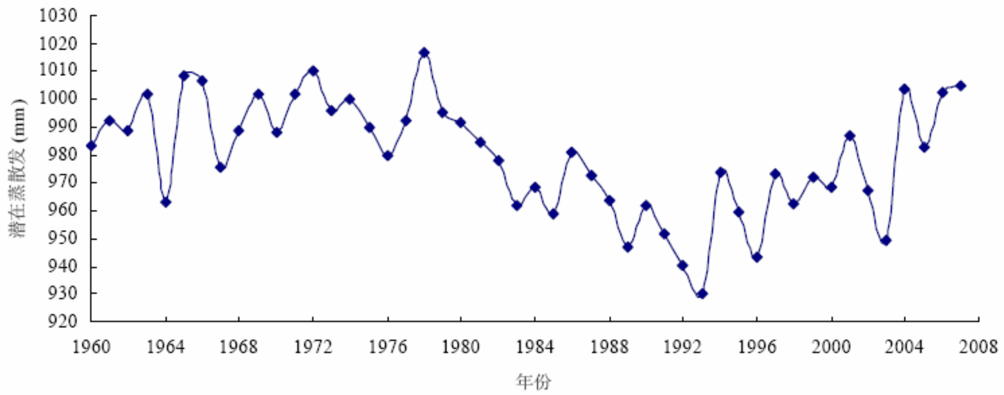

图6 1960-2007年潜在蒸散发变化趋势

Fig. 6 Changing trend of potential evapotranspiration between 1960 and 2007

The Y axis is PET.

32) P6667, L5-9. I suggest the findings of Martiny et al (2006) can be placed in a water-limited (i.e., where annual P is between 200 mm / a and 600 mm /a) where there is a linear increase with increasing annual P amounts, and energy-limited (or actually some other limitation) when P is greater than 600 mm /a.

Answer:

Yes，I defined similar water deficit classes based on these knowledge. Figure 2.

33) P6667, L18, what is the basis for this speculation? You need to expand this suggesting a mechanism (or process) that others could test. This is an ideal opportunity to reveal your process knowledge and you must perform scientific discussion in the Discussion section.

Answer:

Revised.

34) Section 4.4, Due to previous concerns I do agree with the founding basis for your concept of ecosystem susceptibility. In an earlier section(s) of the paper you need to be more emphasis placed on the theoretical or conceptual framework supporting (or underpinning) this concept. Please consider adding a section entitled ‘Background Theory’ directly after your Introduction section where describe the theoretically framework you are using to interpret the data in.

Answer:

Yes, I add theoretical or conceptual framework in the section of 2.5.

35) Also note it is the Conclusion section not the Conclusions section, just as your Introduction section is singular and you introduce more than one idea, so your Conclusion section should be singular and again you can report multiple ideas. I know it's a bit of an 'old-school' comment; however the details are important.

Answer:
Revised

36) Tables 2, 3 and 4. The results for the 12months should be listed under a heading of monthly, not seasonally, as there are usually only 4 seasons used in most (not all) environmental scientific literature. Plus for all these tables you need to say what the units are in the table caption; providing this information will greatly assist readers.

Answer:
Revised

37) Table 5, please also provide units in the caption.

Answer:
Revised

While there is some more work to do improving your m/s so that it meets HESS standards, you have a very interesting and informative paper. Given this, I look forward to seeing the revised manuscript, which I hope will be much improved and therefore likely that I will recommend acceptance into HESS.

Answer:

Thank you very much. Following your suggestions, I have made substantial change on the m/s, including re-calculate the ET0 based on a full physically based model, with consideration of wind speed. I worked out a theoretical frame and further clarify the structure of the article. I redefined the water –deficit threshold based on new literatures and new value for different vegetation types. I think the m/s had been greatly improved. Thanks again for you diligent work on this.