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Discussions

Interactive comment on "Groundwater-dependent ecosystems: recent insights, new techniques and an ecosystem-scale threshold response" *by* D. Eamus et al.

D. Eamus et al.

derek.eamus@uts.edu.au

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Dear Referee's

I would like to thank all three referees for their insight and extremely valuable comments on our Ms "Groundwater-dependent ecosystems: recent insights. New techniques and an ecosystem-scale threshold response"

In the following pages I provide our responses (in tan colour) to these reviewers' comments.

Anonymous Referee #1 Received and published: 27 May 2015 The Authors presents a

review on groundwater dependent ecosystems (GDEs) with the focus on the definition of their location, the quantification of their groundwater (GW) use, and their response to GW extraction. Although the review is in general well written and likely to be of interest to the readers of HESS, I have few issues that I would suggest the Authors to consider, as listed in the following points.

We thank the referee for his comment about the review being generally well written.

- The review is excessively long and touches on many topics that have been already reviewed in the recent literature. Recent review papers on GDEs are: Naumburg et al. (Environ. Manage., 35(6), 726–740, 2005), Lubczynski (Hydrogeol. J., 17(1), 247–259, 2009), Klove, B., et al. (Environ. Sci. Policy, 14(7), 770–781 and 782-793, 2011), and Orellana et al. (Rev. Geophys., 50, RG3003, 2012). I think the Authors should put their work in the context of what is already available in the literature and focus on what is currently missing from these existing review papers.

We have reduced the length of the review and have included mention of the Naumberg et al (2005) and Orellana et al (2012) references in the introduction, as suggested by this referee.

- Some sections of the paper are largely available in the existing literature and do not need to be repeated. For example, section 2 could be considerably shortened if not removed completely.

We agree that section 2 can be considerably shortened and we have now greatly reduced this section in the revised Ms.

Likewise, the parts on groundwater fluctuations and isotopes have been extensively reviewed in other recent papers.

We have reduced the text on GW fluctuations and isotopes, as suggested and this has significantly tightened the Ms.

The case studies are disconnected from the other sections of the review; I would con-

HESSD
12, C2700–C2716, 2015

sider to remove them.

We disagree with the suggestion to remove the case studies as we are not aware of any attempt to provide two contrasting case studies in a review of GDEs. We feel this does have value in the current review.

It seems to me that the new topics addressed here are the use of remote sensing (RS) technologies in GDE studies and the response to GW levels. Maybe, the Authors could focus their review on these issues. Accordingly to the points above, I would suggest to re-organize the review as:

1. Introduction: contextualize the review and focus on RS and ecosystem response to GW levels. 2. Identify GDEs: I would just present the sections on RS and maybe touch briefly on GW fluctuations and isotopes. 3. A primer on remote sensing...: I would include here current sections 4 and 5. I think the section on GRACE, which is a RS technology as well, should be here. 4. Current section 8 5. Current section 9 6. Conclusions

Two of the referee's suggest a significant restructure of the review. Having given this considerable thought and played around with a couple of new structures, we have come to the conclusion that a restructure does indeed improve the readability and flow of the Ms. Consequently we have restructured the entire review in light of the two (similar) suggestions presented by two reviewers.

SPECIFIC COMMENTS: - P4678, L6; ...GDEs, and (3)... We have made the change suggested.

P4691, L14: ...water table results...

We have made the change suggested.

- P4695, L7-8: it seems to me that these two paragraphs are disconnected. There is a logic jump.

HESSD

12, C2700–C2716, 2015

We have reworded these two paragraphs to make the connection clearer.

- P4695, L16: ...applications, downscaling...

We have made the change suggested.

-P4697, L22: Fig 2 should be Fig. 3. The references to figure numbers is not correct throughout the manuscript.

We apologise for the errors in Fig numbering. We have corrected these throughout.

- Section 6.3: as far as I know, MODFLOW models GW flow; the modeling of flow in the unsaturated zone is very simplified and does not use the Richards equation. I also believe that Ajami et al. (2011 and 2012) did not model the unsaturated zone, but included direct root water uptake from GW in MODFLOW.

The referee is correct that MODFLOW is used to model groundwater flow and does not use Richard's equation. In contrast, HYDRUS is a variable saturation model that solves Richard's equation. The section title has been revised to clarify the distinction while still implying that both types of hydrological modelling are important to fully understand the ecohydrology of GDEs. The issue of root water uptake is a particularly good example, thus we restricted our discussion in the second paragraph of this section to those studies that used HYDRUS for evaluating interactions between groundwater, soil water in the vadose zone, and root water uptake. As the referee argues, the two references by Ajami et al. (2011 and 2012) are not relevant as they refer to direct root water uptake from GW in MODFLOW.

- P4704, L22 25: what are 'end-member analyses'?

We have now explained this term in the text.

- P4708, L16-17: I would not say that ET rates exceeded radiation. I would use the term latent heat in relation to radiation.

We have amended the text as suggested.

HESSD

12, C2700–C2716, 2015

- Table 5: I would not use a table to explain terms in a figure. I would include this table in the figure or figure caption.

We disagree with this suggestion as putting all of this information into the figure or figure caption would make it extremely unwieldy. A table is the optimum place for this set of information.

- Figs. 3, 4, and 5: I would remove these figures; they are not very informative.

We believe these figures assist the reader and have value in the review.

- Fig. 8: this figure carries a lot of information and is very difficult to understand. What is the meaning of the different types of arrows?

We are sorry that the referee found this difficult to understand. We have tried to make this figure simpler and added some words of explanation to the figure legend.

- Fig. 9: what is the variable on the vertical axis? I understand that this figure is from a PhD thesis and the Authors might want to keep details for other publications; however, it is very difficult to understand what this figure refers to. Further, I would not fit a curve across the points. Any curve that goes from about 0.9 to about 0.7 when GW is around 9 m would fit the data well; as such, there is no point to fit a curve and report the R2. I would rather show the experimental points and have a vertical line or a colored vertical bar when GW is between 8-10 to show that there is a threshold effect.

We have added some text to the figure legend to explain what the normalised value refers to (ie how it was derived from the data). The referee is correct to note that the cut-off cannot be identified with precision and we have added text to the Ms to acknowledge this point. However, the statistical fit is valuable as it shows that there is a break within the data, but we can't identify with high precision the exact location of this break. However, we note that Dr O'Grady found this approximate location of the break to concur with other published studies.

The Authors might also want to link these results to the work by Benyon and Doody

HESSD

12, C2700–C2716, 2015

(2004) on plantations, where the suggested value of GW level for possible root water uptake was above 6 m.

We thank the referee for this observation and have made reference to two reviews and one field study where a threshold is discussed. We prefer to cite the larger, Benyon et al (2006) review here.

Interactive comment on "Groundwater-dependent ecosystems: recent insights, new techniques and an ecosystem-scale threshold response" by D. Eamus et al. A. O'Grady anthony.ogrady@csiro.au Received and published: 28 May 2015

Eamus et al. present an interesting and timely review of the current state of knowledge and approaches for addressing issues in relation to GDE's. The manuscript is clearly relevant to the readership of HESS. While one anonymous commenter on the review argues that the parts of the review cover pre-existing reviews (e.g. around remote sensing) I believe that there is still value in having a review that brings much of this previous work together under the umbrella of groundwater dependent ecosystems.

We thank Dr O'Grady for these supportive comments.

Potentially the primer on remote sensing could be reduced, but I don't see it as a serious issue. The review didn't really cover traditional water balance approaches (eg the Doody and Benyon paper suggested by the other reviewer). In my mind these present a point of truth, against which the remote sensing techniques can be validated. Indeed many of the insights into the O'Grady et al 2011 paper cited throughout this manuscript were based on a review of existing albeit limited number of water balance studies that have quantified groundwater discharge.

We agree with Dr O'Grady and have made reference to more traditional water balance approaches in the text.

I found the discussion on remote sensing pretty interesting, but in my experience I find the way that remote sensing is applied in practice somewhat frustrating, and so

HESSD

12, C2700–C2716, 2015

a discussion on the limitations of remotes sensing in relation to identifying GDEs is I think really warranted. In itself that may be a separate review, but I think it would be good to recognise some of these limitation here. In reality remote is somewhat blunt instrument that often has very little validation, it is not unusual to have three GDE remote sensing products that give a different answer to the same problem. Further more remote sensing on its own can provide very little information on the source of the water in the signal, thus the "groundwater signal" may not accurately reflect the groundwater system the water manger is concerned about, e.g. a regional aquifer v a perched aquifer. I see remote sensing as a valuable way of focussing limited resources into areas of most concern or high risk, so that more detailed assessments can be preformed. The underlying assumption that systems with access to groundwater have an unlimited water supply (top paragraph of 4692, 'it is assumed that actual et rates are equivalent the et of a reference crop' is a flawed assumption. For example the salinity of groundwater may vary from fresh to saline, thus the plant available water is somewhat less.

We agree with this suggestion and have added additional discussion of the limits of RS in the study of GDEs. We have noted that saline GW may invalidate the assumption and have added words to this effect in the Ms.

With respect to the discussion on ecological response functions, I thought the approach to analysing the co-ordination of traits presented in figure 9 was really nice.

We thank Dr O'Grady for this supportive comment.

There is remarkably close agreement in terms of thresholds identified to that identified by Kath et al 2014 Global Ecology and Conservation, 2, 148-160, which is a nice approach at coming at this problem using remote sensing. It may be worthwhile recognising though that these approaches are correlative in nature, in that they correlate state with state, but are not in themselves ecological response functions, rather a prediction of what that response function might look like.

HESSD

12, C2700–C2716, 2015

We thank Dr O'Grady for this comment and have added reference to the Kath et al paper.

This is a good review that should be published in HESS

We thank Dr O'Grady for this highly supportive final comment.

Reviewer comments to the manuscript HESS-2015-90 "Groundwater-dependent ecosystems: recent insights, new techniques and an ecosystem-scale threshold response" by Eamus et al. GENERAL COMMENTS

In this paper, Eamus et al. review the last advances accounted for providing a better understanding of Groundwater Dependent Ecosystems. The review rests over three main pillars: (1) Identification of GDEs; (2) Quantification of their water requirements, and (3) Definition of response functions to water table changes. Authors refer to a relevant number of recent studies that cover a wide range of techniques based on remote sensing, hydrodynamics, and ecophysiological and dendroecological measurements. Among all these techniques, a more emphasis has been given to satellite-based or remote sensing techniques developed recently to answer the two first pillars described above.

In general, it is a good and well-written paper which addresses a relevant scientific issue within the scope of HESS.

We thank the referee for these supportive comments.

Several items refereed inside the manuscript seem to be "out of the blue" (e.g. section 5 regarding the GRACE mission). In this regard, more space could be saved in an attempt to simplify the text or, if it is preferred, to go in deep in other interesting sections, e.g.: a) adapt the text in sections 3.2.3 and 4 to the different methods implicitly suggested in table 3; b) improve the conclusions maybe suggesting a a potential roadmap of activities or items that should be addressed in the next future, and how water management boards or agencies should address this topic.

HESSD

12, C2700–C2716, 2015

With the new structure to the Ms we think the links between sections is much improved. We thank the referees for the suggestion to alter the structure.

Does the paper present novel concepts, ideas, tools, or data?

Because its nature, this paper review concepts and methods previously published in scientific literature. Most of the references are appropriate and relatively new. However, the list lacks of other key references that should be recognized here.

We thank the referee for the detailed bibliography (s)he provided in their review. These are indeed very useful additions and we have included 90 % of the literature the referee provided in the amended text.

Are the scientific methods and assumptions valid and clearly outlined?

Paper structure is improvable. A new structure is suggested to get the concordance required between the objectives depicted in Introduction and the rest of sections.

As noted earlier, we have significantly restructured the Ms in line with the detailed suggestions given by two of the referees.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Some figures and tables should be better credited. Please, put more attention to this item.

We have provided all the acknowledgement information in the legends, as requested.

Does the title clearly reflect the contents of the paper?

Yes, but could be shortened. Maybe “Groundwater-dependent ecosystems: Recent insights and, satellite and field-based studies”

We have amended the title of the Ms to make it shorter and more “punchy”.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced,

HESSD

12, C2700–C2716, 2015

combined, or eliminated?

Several changes are suggested in the following notes.

MAJOR COMMENTS

Structure article In order to make easier the comprehension of the topics covered within the manuscript, several changes in the structure are suggested (sections should be in concordance with the three pillars depicted at the end of the Introduction). For example, 1. Introduction 2. Identifying GDEs 2.1. Indirect methods 2.2. Direct methods 2.2.1. Satellite-based approaches 2.2.2. Water table depth fluctuations 2.2.3. Isotopic analyses 3. Quantifying water requirements of GDEs 3.1. Satellite-based approaches (now section 4 and 4.2.) 3.1.1. Scaling issues (now section 4.1.) 3.2. Hydrological modelling 3.2.1. Conceptual water balance approaches (now 6.1) 3.2.2. Physically-based water balance approaches (now 6.3) 3.3. Field-based measurements 3.3.1. Daily fluctuations of water table (now section 6.2.) 3.3.2. Isotopic techniques (now 6.4.) 4. Functional responses of GDEs to changes in water table depths 4.1. Evidences from dendrochronology and plant growth traits (now section 8) 4.2. Two case studies in semiarid regions (now section 7) 4.2.1. The Gnangara Mound (SW Australia) 4.2.2. Riparian forests in southwestern USA 4.3. Integrating multiple-scale responses (now section 9) 5. Concluding remarks

As noted above, we have significantly changed the structure of the Ms in accordance with these detailed suggestions.

In this review, section 5 focusing on GRACE measurements (and all the references inside) must be eliminated because the spatial and time resolutions of the outputs provided by this mission are not appropriate at all to infer data useful for improving our knowledge on GDEs. In the following, major items organized according the sections suggested in this review are highlighted

We strongly disagree with this because GRACE data can, for the first time ever, provide

HESSD

12, C2700–C2716, 2015

10-day data on aquifer trends across the duration of multiple-year droughts (such as are frequently evident in Australia and elsewhere), which is important information for managers of GDEs and aquifers. We provide two examples of where GRACE data have been used in this manner.

Introduction First paragraph is suggested to be reduced. Please go directly to the focus of the paper, i.e. GDEs, trying to highlight what has been done until now in order to identify them and understand their functioning. Regarding this there are similar review essays reported in scientific literature focusing on GDEs (e.g. (Barron et al., 2014; Naumburg et al., 2005; Orellana et al., 2012)). Within this framework, authors are encouraged to highlight the reasons why a new revision is required.

We have greatly reduced the introduction.

Regarding the potential drivers that are threating the health and good ecological status of GDEs, authors may refer other excellent reviews recently written (see e.g. (Danielopol et al., 2003; Kløve et al., 2011a, 2011b) .

This section has been deleted.

The simplified classification scheme with 3 classes described in section 2 is suggested to be moved to Introduction. Authors could delete the description of the detailed classification scheme without affecting the quality of the paper (a reference to a previous work would be sufficient for the purposes of this paper)

We have amended the text as requested.

Table 1 does not provide useful and relevant information to the topic discussed here. It is suggested to be deleted

Table 1 provides a data set to support the discussion on the use of stable isotopes.

Identifying GDEs Indirect methods Two interesting applications are described by Brown et al. (2011) and Howard and Merrifield (2010).

HESSD

12, C2700–C2716, 2015

Direct Methods – Satellite-based approaches (now section 3.2.3)

Mapping GDEs based on the "green island method" or the concept of "spatial anomaly of vegetation" has been also tested by Contreras et al. (2011) in remote regions of central Argentina. Contreras et al.'s use positive anomalies of a vegetation index (VI) as surrogates of groundwater (or lateral inflow) reliance. Anomalies are spatially computed from the observed VI and a local rainfall-based expected value resulting from a regional Mean Annual Precipitation-VI function previously calibrated for a set of reference (non-disturbed) sites. In Contreras et al. (2013) the usefulness of the spatial vegetation anomaly is complemented with other seasonal phenometrics or greenness traits in order to get more accurate information on groundwater reliance patterns.

We thank the reviewer for this and now cite the Contreras et al reference.

Direct Methods – Stable isotope analysis (now section 3.2.2)

Here, key references are Jobbagy et al. (2011) and Aranibar et al (2014) who use water stable and C/N isotopes to explore the reliance and dynamics of Prosopis woodlands in the Monte desert.

We thank the reviewer for this. We have cited the Jobbagy et al reference. The Aranibar et al reference is, we think, less valuable to the current text, which focuses on use of 18O and deuterium rather than 13C and 15N.

Quantifying groundwater consumption rates O'Grady et al. spreadsheet tool (now section 6.1) - For the "groundwater risk model", it is stated that "groundwater uptake by vegetation is assumed to occur when ET exceeds rainfall". Authors should question this assumption or justify better its validity. This could be assumed at the annual scale, but not at the monthly scale in which soil moisture storage may play an important role in providing water to vegetation. If this statement is not right (probably I am missing something), please explain briefly the reasons.

We thank the reviewer for this insight and have modified the text to account for the soil

HESSD

12, C2700–C2716, 2015

moisture storage issue raised.

Sub-daily fluctuation in groundwater depth - “White method” refers to White (1932). Please cite it.

White 1932 is cited in the reference list.

Figure 4 is not self-explanatory and is difficult to understand from who is not familiar with the method. Please improve the figure and its caption to avoid jumping to the text.

We have added text to the legend to improve readability.

Ecological responses to groundwater table changes

Page 4715, L12-15. It is suggested that the water table depth threshold is around 9-10. However the abrupt breakpoint suggested may range between 6-10 m (no measurements exist in between).

As noted in our response to another reviewer, we have acknowledged in the text that we cannot precisely identify the breakpoint.

Tables Table 3. A lot of references inside have been not cited in the “References” section. Include studies of Contreras et al. (2011, 2013) as a “Green island method”.

We apologise for omitting some of the references cited in the tables from the reference list. We have now included these in the reference list.

MINOR COMMENTS:

- Page 4682, Line 23. Where says “Identifying the location of GDEs is the vital first step to managing them”, change by “Identifying the location of GDEs is the first requisite step to manage them”.

We have amended the text as suggested.

- P4685, L9-10. Where says “Remote sensing (RS) provides rapid and spatially extensive techniques to assess [. . .]”, change by “Remote sensing (RS) provides a robust

HESSD
12, C2700–C2716, 2015

and spatially-explicit mean to assess [. . .]

We have amended the text as suggested.

- P4685, L11. Delete “This is now discussed” (vague sentence)

We have amended the text as suggested.

- P4686, L28 – P4687, L1-2. The relationship found by Jin et al. (2011) is not surprising at all. The two-side effect of groundwater table depth in vegetation productivity has been described widely, also in forests (Bogino and Jobbágy, 2011) and crops (Nosetto et al., 2009). Shallow groundwaters (<2 m) usually promote negative effects on growth vegetation because waterlogging or root anoxia, or salinization as Jin et al. described in his paper

We completely agree with this point and have changed the text to reflect this.

P4687, L8&9&10. “EVI” instead of “eVI”

We have amended the text as suggested.

P4687, L24. Maybe “alternative” instead of “alternate”?

We have amended the text as suggested.

P4688, L10. At the end of the sentence, change “drought” by “droughts” or “drought periods”

We have amended the text as suggested.

P4689, L14. “Scaling-up” instead of “Moving”

We have amended the text as suggested.

P4697, L22. “Fig. 3” instead “Fig. 2”. From here, all the references in the text to figures are wrong. Please check them.

Apologies for the errors in Fig numbering. We have corrected these errors.

HESSD

12, C2700–C2716, 2015

P4698, L10-11. “Local” instead of “existing”? Another key reference regarding the hydrological equilibrium hypothesis is given by Nemani and Running (1989).

We have amended the text as suggested and included this citation.

P4698, L13. Explicit which means foliar[N]

We have explained the meaning of foliar [N].

P4700, L6. “The White method tends to over-estimate ET”. Do you mean ETg instead of ET?

We thank the referee for noting this error, which we have now corrected.

P4700, L9. “because” instead of “although”?

We have replaced the word “although” with the word “furthermore”.

P4701, L26. “HYDRUS” instead of “HYRDUS”

Apologies for this typo, which we have now corrected.

P4703, L12-15. Regarding this, interesting studies have been recently published by Guevara et al. (2009) and Giordano et al. (2011)

We thank the reviewer for noting these studies and we have cited them in the text.

Section 7. Is it necessary to introduce each case study describing a “problem”. I think these sentences do not add relevant information, so I suggest to delete them in both sub-sections.

We have deleted the statement of “the problem”.

P4706, L27. “Gnangara” instead of “Gnangarra”

Apologies for this: we have amended the text as suggested.

P4708, L7. Maybe “reliance” better than “dependency”

HESSD

12, C2700–C2716, 2015

We have amended the text as suggested.

Section 8. Maybe rename as “Effects of groundwater on growth and dendrochronological traits”

We have amended the text as suggested.

- Regarding dendroecological approaches, Giantomasi et al. (2012) provide a very interesting study in the Prosopis woodlands of the Monte desert.

We thank the referee for this observation and have amended the text to cite this reference.

P4714, L11. “observed” instead of “resultant”

We have amended the text as suggested.

P4714, L12. “Refer to. . .”. Move this last sentence as part of the figure caption.

We disagree with this suggestion and prefer to leave the text as it currently stands.

P4716, L6. “Main means for” instead of “principle means of”.

We have amended the text as suggested.

P4716, L8 “methodologies which include the use. . .” instead of “methodologies, including use. . .”

We have amended the text as suggested.

P4716, L10. Delete “putatively” (not relevant)

We have amended the text as suggested.

P4716, L11. “the location of GDEs but also. . . features of their functional behaviour” instead of “the location but also. . . features of the functional behaviour of GDEs”

We have amended the text as suggested.

HESSD

12, C2700–C2716, 2015

P4716, L12-16. This sentence is too long. Please make shorter or rephrase.

We have amended the text to make this shorter.

P4716, L17. “providing data on” instead of “pertaining to both”

We have amended the text as suggested.

Fig. 7 (caption). “Eamus (2006b)” instead of “Eamus (2006)”

We have amended the text as suggested.

Figure 8 (caption). “Table 5” instead of “Table four”

We have amended the text as suggested.

“References” Section

P4720L31. First author is “Doody” instead of “Doodym”

We apologise for this error and have corrected the citation.

Is it possible make shorter the reference of Kattge et al. (2011)?

Not really, no.

Derek Eamus Professor Environmental Sciences UTS

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4677, 2015.