GENERAL

I did enjoy reading this interesting manuscript. It treats an important topic, controls on diffuse groundwater recharge, which is a complex physical process, and thus, difficult to assess. Groundwater as a fresh water supply resource is getting more prominent throughout the world, as surface water becomes less available. Therefore, more knowledge about groundwater recharge, and thus seeing the potential locations of future fresh water resources, is necessary. As such, it is getting to be an important issue, now and in the future. However, while reading the manuscript, I started to develop some comments. Nevertheless, the results (chapter 4) are very interesting, and if possible (see major concern 1.), this Discussion paper should be transformed to a regular HESS peer-reviewed paper.

Major concerns

1. Based on a quick internet search, I deduce that this article is a reduced copy of a report in 2011, including quite a few figures. Basically, my major point is: some/major parts of the HESS-2012-9 are (directly) copied and pasted from the following report: Barron, O. V., Crosbie, R. S., Charles, S. P., Dawes, W. R., Ali, R., Evans, W. R., Cresswell, R. G., Pollock, D., Hodgson, G., Currie, D., Mpelasoka, F. S., Pickett, T., Aryal, S., Donn, M., and Wurcker, B.: *Climate change impact on groundwater resources in Australia*, Waterlines Report No 67, National Water Commission, Canberra, 2011.
So: how new is this manuscript? Or is it just a summary of the above mentioned report? I could not find the last part of manuscript in previous reports; if this is new, this should be emphasized more early in the introduction of the manuscript. It is up to The Editor how to handle my concern.

2. In this article, a recharge analysis was conducted using the WAVES model (page 6029, line 4; page 6029, line 8). From my point of view, not enough information is given about the WAVES model (a conceptual description with a schematization would have been clarifying). How good is this model; how does it perform? The quality of the model is not considered at all: no description, no comments on applicability and performance. It is mentioned that more details are described in Crosbie et al. (2012b), but this manuscript is just submitted to Climate Change, so it is not accessible for me (maybe parts of the executive summary of L. Zhang and W.R. Dawes can be used).
E.g.:
-on page 6025, line 14: *Considering the difficulties in designing a field based experiment to investigate the controls that climate characteristics have on recharge, modeling is the preferred method for investigation.* But then you have to be sure that all relevant parameters and mechanisms must be implemented. As the goal is to detect these mechanisms, this path/approach/procedure is questionable. Making a model of a system you do not fully understand is tricky. How should you interpret conclusions?
-on page 6029, line 3 and page 6029, line 20-21: Elaborate why a 4m (0.5m, 3.5m) depth soil profile was modelled and why free-draining lower BC is chosen. Is this good enough? Does it affect the result
-on page 6030, lines 23: *only partially validated'*. What does this mean? Not all relevant mechanisms included? Is a verification with WAVES with historical data performed? What does it say about the reliability of the modelling results?

So: how good is WAVES, how good is it in relation to other models, how is the calibration process, and what about validation and verification? The principle issue is: how do you know that the WAVES model is capable to model the controlling factors, when you not completely know all these mechanisms. To 'accept' the results, I have to get an idea of the applicability of this model to this

specific problem. How do other codes perform? Is a sensitivity analysis executed? Please elaborate and/or refer properly.

Recommendations

As a major revision, I suggest that:
1. the authors clearly specify what is new in this manuscript in comparison to the old report(s) and the new one in Climate Change.
2. identify the possibilities and limitations of the WAVES model: make it for reader acceptable to trust the results. Explain the choices of number of layers, effect of model parameters on overall results, etc.

Comments and remarks:

- page 6026, line 27: *for development of an adequate climate adaptation strategy*. But also for fresh water resources management now and in the future
- page 6027, line 4: *Australia has a highly varied and variable climate*. Okay clear. Can you also tell us how representative these results are worldwide? Upscaling possibilities? Elaborate on that.
- page 6027, line 15: this study aims to investigate: *at a point scale and at a continental scale*. So, on two completely different scales. How is the upscaling process? Can they be interlinked?
- page 6027, line 19-20: *examine the influence of other climate variables such as vapour pressure deficit, temperature and solar radiation*: I do not think these climate variables are really examined in this manuscript. E.g., these terms are lumped in section 4.1 (*Relative importance of climate characteristics in recharge estimation*) and relative importance to annual rainfall is considered, but that is about it. A rephrase of the aim of the paper in this enumeration on page 6027 would be adequate.
- p. 6028: line 2: *shown in shown*
- p. 6028: line 2: Fig. 1. In this figure it is definitely not clear (for me) that there are 15 zones. Say 8 are clear/visible but the other 7 (especially in the northeast) are too small to recognize. Mention that in the caption or text. Why are these 7 zones so small? Are they really different ones?
- page 6029, line 24: how is the group *perennials* determined? Literature?
- page 6029, line 25: *User Manual*: sensitivity analysis is implemented in the User Manual?
- page 6034, line 24: *Figure 4 shows*: no: I cannot detect this in figure 4.
- page 6035, line 4-5: *(note that ... climate zones).*: why not?
- page 6035, line 17: $R^2_P > 0.7$: is this strong? Note that the symbol R^2_P is explained later in page 6037, line 19-20… Earlier is better.
- page 6036, line 2: *Under similar annual rainfall R^2_P is greater in the climate types with winter dominated rainfall (Cs)* **for all combinations of soil and vegetation**.: but Csa and Csb are not shown in K=0.01 md^{-1}… Consider this.
- page 6036, line 23-26: why only chosen for Cs an Aw (perennial and K=1 md^{-1})? What can be said about other zones, locations and K's?
- page 6037, line 19-25: very long sentence. Make it readable..
- page 6038, line 15: K=0.001 md^{-1}?
- page 6041, line 14: Section *5.3 Implications for climate change studies*. Should this section not be positioned earlier in the paper? As context of the research?
- page 6042, line 14-16: *However, for the majority of the considered climate types the total annual rainfall had a weaker correlation with recharge than the rainfall parameters reflecting rainfall intensity.*: and what about the other climate types?
- page 6050: *Adapted from Barron et al. (2011).*: what is new here? (see my major concern 1).
- page 6061: *(a) K=1 md^{-1} and (b) K=1 md^{-1}*. The same?
- Can the author also elaborate about the other 7 Climate zones? Upscaling the results for the considered 8 ones? If not, why not?
- Throughout the paper, punctuations (.) are sometimes forgotten, e.g. on page 6030, lines 21 and 22.