Hydrol. Earth Syst. Sci. Discuss., 11, C2130–C2134, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C2130/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Design droughts as planning tool for ecosystem establishment in post-mining landscapes" *by* D. Halwatura et al.

Anonymous Referee #2

Received and published: 30 June 2014

General comments

The paper addresses an interesting topic that has potential for implication beyond post-mining landscapes and may be of interest to ecohydrological assessment of disturbed systems. It assesses drought through various drought indices within a statistical framework that has conceptual similarities to design rainfall as done for infrastructure planning. In the following I will focus the review on the context of ecosystems restoration and not on the details of the methods being employed.

Overall, the approach is pointing in the right direction to develop a quantitative framework for "ecological engineering" of restoring terrestrial ecosystems. However, the paper does not deliver on the ambitious title, namely to introduce a novel method. The

paper uses established statistical analyses of weather data and it actually stops right there. It does not carry through a methodology that is actually applicable. If a paper claims to introduce a new method, than I expect that the method is first introduced and then followed by a clear presentation of at least one case study. In this case the statistical analysis has been developed to move towards a risk assessment procedure, not design criteria. In fact, it is not clear what is meant by "design" in the context of ecosystems restoration. It needs to be defined with clear criteria. This is not as straight forward as building a reservoir lake, for which we need to design a flood release structure and apply safety factors for flood scenarios. In fact, rehabilitation measures will be carried out irrespective of any kind of seasonal weather forecast and is at best optimized for local condition of the mine site through field trials. So how will the drought assessment alter the rehabilitation measures for a post-mining landscape? The authors make the point in the paper that their approach contributes to the risk assessment of rehabilitation success. The discussion on the design remains vague. I suggest that the title of the paper should reflect this by eliminating the term "design drought" and by introducing the term "risk assessment" or "risk framework", or alternatively, rework the paper to quantitatively define the design issue and within this context the term "design drought" .

I believe that the paper will have a wider readership if the description of the methodology includes a clear statement of the assumption made as well as a clear statement of its limitations. It is in particular important to focus on the temporal structure of the drought indices. The authors mention certain temporal pattern such as seasonality, but they fall short to clearly indicate the implications.

For example, can temporal clustering of wet or dry spells be explicitly be accounted for – this is very important for the risk of rehabilitation success. Furthermore, the restoration of arid ecosystems may have quite different requirements compared to ecosystems in the subtropics or seasonal Mediterranean climate zones. The authors have identified these differences with the application of different drought indices, but they

remain vague in their description of how to apply their findings in the context of mine site rehabilitation.

So what the method really does is to provide us with a climatic fingerprint of a region. If we assume that an established native (undisturbed) ecosystem is stable, implying that it is in tune with the climatic conditions, the fingerprint on its own doesn′t tell us very much. The question is how this can actually be used in a risk context. It requires us to define critical water deficit thresholds as a function of the rehabilitation stage. This is of course specific to ecosystems ranging from the overall landscape scale to even the spatial and temporal scales of species establishment. So the linkage between the fingerprint and rehabilitation (water) requirements is crucial and essential. If you can provide one example of that linkage, the manuscript would be completed and could provide a compelling case for others to follow. Currently it only points into this direction, but it doesn′t deliver. The paper by Audet et al (2013, Biogeoscience) has provided us with a linkage between weather/climate and vegetation link. A clear statement how the drought indices relate to work of Audet et al. would be most helpful. Another point to be made, is that rehabilitation success is most of the time influenced by a combination of substrate constraints along with the climatic situation. As I said above, a functional native ecosystem should be fully in tune with the climatic conditions. Based on this, I would expect that the system will respond to natural disturbances with resilience. So biophysical as well as biogeochemical constraints of the post-mining substrate will have to be assessed against the drought indices and their recurrence time scales.

Alternatively, you could actually focus on the method alone, but it needs to be shown that something novel has been generated, irrespective of the application to ecological restoration. Either way, I suggest the title and contextual focus to be changed accordingly.

Detailed Comments (page number/line numbers)

Abstract 4810/4 Is water the stressor or the lack of water. Clarify.

4810/17 Not clear why evaporation plays a minor role

4810/24 Vague statement, specific mentioning of what those “environmental barriers” are.

Introduction

4811/18. . . The relevance water stress during rehabilitation is reviewed here. Can this be resolved in more detailed perhaps in the discussion section such that quantitative criteria could be derived?

Materials and Methods

4816/22-24 Notation of eq. 1 not clear. Should ‘t it be . . . for I_i < 0 without the negative sign and the absolute value within the summation?

4818/15-20 Table 3 doesn’t exist.

4819/2 Fig. 7 is mentioned before Fig. 5 and 6. Correct sequence.

Implication. . .

4822/26-28 Good example of vague statement: Why can’t you be specific and provide an example with mentioning the species, the duration values and then use your method to make a well-informed assessment of the risk of rehabilitation failure. If we don’t have that type of information available, than the method proposed is useless.

4823/16-29 This section is a good example of the use of the indices. The issue of simplification and the use of surrogate information should be presented earlier in the paper to justify why you go through the statistical analysis of correlating your different indices.

Furture . . .

4824/23-26 The statement that the analysis in not predictive should be presented earlier in the paper (introduction). As mentioned above, list all assumption of the method

(for example assumption on independenc etc) before you introduce the method and then clearly indicated limitations based on that.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4809, 2014.