

Interactive comment on “Adaptation tipping points of urban wetlands under a drying climate”

by Amar V. V. Nanda et al.

Anonymous Referee #1

Received and published: 20 June 2017

Overview

This paper uses adaptation tipping points (ATPs) as a method for measuring the effectiveness of wetland management strategies which can be used to monitor indicators as the climate changes. The research is undertaken using largely qualitative methods through desktop literature review, document analysis and interviews supplemented by measured indicators of exceedance of dryness indicators and objectives in management plans. The research objective is to develop an approach that can be used in data sparse situations and potentially under conditions of future climate uncertainties. The idea of the paper is a good one which builds on Kwadijk et al 2010 which shifts the sequence of inquiry from a top down approach to one where conditions of failure of policy objectives drives the monitoring system. The following revisions are necessary

[Printer-friendly version](#)

[Discussion paper](#)



Interactive
comment

for publication. 1) Tighten the argument with particular attention to the English writing style which is quite unclear in places and variable throughout. 2) The problem statement in the introduction is poorly written. The abstract is better and could be used in the text in the same sequence. The introduction is a sequence of literature rather than a discussion and leaves the reader unclear why certain issues are raise. For example, the terms 'thresholds' and 'tipping points' and then 'turning points' are used without any definitions as to how they differ. At line 94 the authors settle on ATPs but don't actually discuss the distinction from the other terms. One is left with the impression that all the terms are the same, but used in different contexts. My knowledge of the same literature is that these terms are in fact distinctly different, as discussed in the Werners et al 2013 paper. 3) Between lines 112 and 113 there is a jump in logic. What goes before does not in itself support ATPs as a suitable methodology for measure management strategy effectiveness. The lines 109 through 114 need to be recast to make sense. Later on at line 156 the statement is made that 'these observed impacts of climate change on the hydrology make the wetland a suitable study area to apply the ATP method.' Yet nowhere is this justified. It seems to be assumed that because there are climatic influences ipso facto ATP is a suitable methodology. The paper needs to be more specific about why this is so. What is it about ATPs that make it so? 4) At line 118 /119 the sentence suggests that there are three ecosystem functions and three aims. This is confusing. The sentence could say something like "effectiveness is defined using three ecosystem functions –hydrological response and variation/ temporal scale ecosystem responses/ recovery rate or alternative stable state of ecological processes. These were defined using literature review and interviews; hydrological time series data for each socio ecological objective from the management strategy; and minimum and maximum water level thresholds compared with mandated management objectives and policies respectively" 5) Similarly lines 130- 39 are quite confused. 6) Line 147 using Mediterranean climate as a descriptor for Western Australia seems to be written for a European audience. Could be deleted. 7) Line 154 refers to 'despite high resilience...' which is not supported by any evidence. Delete. 8) At line 202 the

[Printer-friendly version](#)[Discussion paper](#)

Interactive comment

term 'acceptable thresholds' is introduced with no mention of to whom and why they are or have to be acceptable. What follows at line 209 should be in the section 2 items. 9) Why the two time periods are created at 1995 is not mentioned? 10) The discussion section 331 -332 seems to restate the objective of the research in a new way. First you have set out that you are testing ATP as a method for monitoring the effectiveness of the management strategy. Now it is redefined to assess whether a baseline ecosystem management strategy was sufficient to sustain the ecological resilience of the ecosystem. This is a fundamental problem with paper because by the time the ATP is reached it is in all likelihood too late to act because there is a lead required to undertake the action and have it operate to maintain the ecosystem. So this means the ATP as a measure of management strategy effectiveness cannot operate except as an ex post description of what has happened, rather than a measure for prospective management. Fundamentally the authors need to address whether a different strategy could have halted the decline by running a stress test of different strategies for their effectiveness in maintaining the plans objectives. This is what the dynamic adaptive policy pathways approach (Haasnoot et al 2013) does. 11) At 372-374 there is a claim of the criticality of scale and level of policy and legislation and its relationship with shifting the social system, but there is no discussion of why this should be so. This could be elaborated and with reference to literature on scale (Oran Young could be useful here). The ideas are picked up again at line 391. Given the initial claim of linking the ATPs to the strategy this section is somewhat under done. What were the connections between the ecosystem function and the strategy- use examples of how the plans did this and whether the plans were monitored. 12) The sentence at 412-414 does not say how this could be done, i.e. 'by capturing the combined measures to adapt the current strategy.' What is meant by this? 13) Line 419 raises 'time periods' for when the policies expire. If this is prospective then putting time periods on would not be possible due to uncertainties about the rates and scale of the changes. All you can do is describe under what conditions the policies would fail. While you go on to raise the use of scenarios it is unclear whether the two sentences are connected. 14) Line

[Printer-friendly version](#)[Discussion paper](#)

427-430 makes unsupported claims. These need to be discussed and supported. 15) The table with the timeline should be redrawn to extend the period when the thresholds were exceeded. Also there is a mix up of dates in the two lower right boxes as they relate to the timeline. In summary, the study is an interesting idea that potentially could build the body of knowledge but the paper needs significant rewriting to be clear and to fully discuss the link with the management strategy. It is also a post fact research inquiry, which begs the question as to how it can be applied prospectively. This could be further explored and clearly reported. The strength of the paper as conceived is its link with monitoring a management strategy, but no data is presented for the reader to see what the strategy sets out to do and whether the indicators were present in it and how the ATPs could be applied for uncertain future conditions before the ecosystem declines beyond the point of no return.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-307>, 2017.

[Printer-friendly version](#)

[Discussion paper](#)

