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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The potential value of seasonal forecasts in a changing climate" *by* H. C. Winsemius et al.

Anonymous Referee #1

Received and published: 14 January 2014

GENERAL COMMENTS

This is a nice paper investigating the potential skill and perhaps utility of seasonal forecasting in southern african farming. However it mixes seasonal forecasting with climate change projections too liberally. This creates two specific issues:

(1) Any forecasting system will assume that the system considered will continue to behave (in some part) in future as it has in the past. System changes related to climate change can render this assumption is invalid, to a degree. It can probably be argued that this specific forecasting system is less sensitive to changing boundary conditions than say a purely statistical method, but the argument is not presented. This needs much more careful discussion along with a more careful analysis and discussion along

the lines of Section 4.3.4 to provide appropriate caveats (for example, what does the forecasting system assume about ocean and land cover among other possible boundary conditions? How important might that be?).

(2) The justification of the study appears to be the argument that (a) climate is likely to change, (b) forecasts with some skill appear possible and "therefore" (c) forecasts are going to be more important in future. The logic of a+b -> c is poorly developed and not immediately obvious to me. For example, perhaps the current farming methods are better geared towards the current climate and therefore might be better able to beneficially use forecasts under an unchanged climate, when compared to a changed climate where they may be maladaptive and a structural change in farming methods would first be required before forecasts can be used beneficially? What if climate change makes livestock farming too high risk or uneconomical altogether? Please present a more careful argument. Both issues are interpretative and do not affect the experiment and results, which are relatively straightforward and worthwhile publishing.

SPECIFIC COMMENTS

Title: include mention of southern Africa?

14751/12-14) This raises the question of stationary within the context of the forecasting method (see main general comment 1). Please briefly review and discuss most tenuous assumptions.

14753/22-23 and 14754/15: for the sake of reproducibility please provide details as to where /how one can obtain (or apply to obtain) each of the data sets used.

14756/8) 3 and even 5 or 10 days seem rather short for a dry spell – they must be very common? Pls discuss and provide some justification as at face value it would strike me as at the low end of being useful... Also, how are carryover spells from one month to the next dealt with?

14757/25-26) see general comment 2.

14762/12-13) Non sequitur, see general comment 2.

14763/1-3) see general comment 2. Also, that forecasts might be available does not necessarily mean that the livestock sector will be able to use the forecasts... In fact the next page (14764/2-3) you give some good examples why it might not.

14765/24-26 and 3 lines next page) Indeed this is a very important point that needs a much more stronger caveat and careful discussion, see general comment 1. There have just been some more high-profile papers published on the apparently changing nature of ENSO and therefore there appears to be plenty of potential for non-stationarity in the assumed boundary conditions of your forecast method. Just to say that "we believe that possible changes .. will not change the conclusions" is too facile.

TECHNICAL CORRECTIONS

14749/27 (page/line) "significantly up to 20%" rephrase avoiding this term with statistical connotations.

14752/13) units of kt (kilotonnes) might be more readable.

14753/3) change to '6 to 12' assuming you don't mean 6/12=0.5 months ahead.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14747, 2013.