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

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Referee no. 2 has given lengthy comments on our manuscript. In particular, a number of questions were raised regarding the context of the experiments (a.o. climate scenario used) and utility for the end user. We have provided elaborate replies on all points brought forward and, where applicable, proposals on how to improve the manuscript.

1. If the heat stress indicator or dry spell indicator increase over the years, this does not mean that the seasonal forecast will be more valuable; the question would be whether (1) the variability in the indicators between different seasons increases and/or (2) the seasonal forecast improves. If a heat stress indicator / dry spell indicator increases on average due to climate change is not sufficient to

make seasonal forecasts more important; it will shift agricultural choices to other crops/animal farming;

We do not exactly understand this point. We agree that if the indicator frequency increases, but is in fact every year the same in severity and timing, the climatology would be a perfect forecast. However, the fact that variability between seasons exists is enough rationale for forecasts (in fact it is the whole point of forecasts). It is also not to be expected that variability (although not specifically studied here) will decrease in the future. Forecasting will thus definitely gain importance if indicator thresholds are more often reached, and therefore more often valuable mitigation actions will be taken based upon them. This is regardless of changes in variability in the forecast indicators or possible improvements in the forecast system. If the performance remains the same, more often a mitigation action will be taken (i.e. more often preventing negative impacts). As for the second point, shifting of agricultural choices is an additional possible adaptation measure, but our point is that forecasting could be another! Even where crop changes are considered, if a crop is chosen which is more resistant to dry spells, this only changes the dry-spell threshold, but doesn't change the point that forecasts become more valuable. We will try to stress better what the assumptions are in the description of our approach and propose to keep the analysis as is.

2. Why necessarily combine the heat stress indicator and the dry spell indicator and climate change in one paper? The reviewer does not see any reference yet in two separate papers in which more information is given on whether these indicators seem appropriate for seasonal forecasts. The proof given in this paper on the predictability of the indicators used is not yet convincing – independent of the question whether they would be appropriate for the intended end-users. The expected changes in climate change and what this may do for agriculture in South Africa – without reference to seasonal forecasts – is a paper in itself. (It should be mentioned somewhere that climate change scenarios are very uncertain about

rainfall in general – let alone dry spell prediction).

We have provided many references to paper showing that the indicators used are appropriate and useful. For dry spells, we referred to Barron et al. (2003), Nyakudya et al. (2011) Barron and Okwach (2005), Ochola and Kerkides (2003), Rockström (2000). For the relevance, causes and effects of the heat stress indicator used, we referred to a similar amount of literature, some repeated here (Archer van Garderen, 2011; Dikmen and Hansen, 2009; Ravagnolo and Misztal, 2000) . We believe this sufficiently covers the relevant literature, explaining the relevance of dry spells and heat stress for dryland farming in Southern Africa. We combined heat stress and dry spells in order to demonstrate the rationale of forecasting for farming across 2 different indicators, connecting to 2 different types of farmers and production systems, and their related decisions for mitigation, both highly relevant for Southern Africa. There are many more indicators that could be investigated (e.g. onset of rainy season, length of rainy season, total seasonal rainfall, heat wave duration), but for this paper, we believe 2 are sufficient to demonstrate our point. We will mention this more clearly in the introduction section of the paper. We will also add more on the uncertainty of precipitation in climate change projections.

3. The usefulness for end-users for individual farmers, as the prediction spatial resolution is too low for individual farmers. The grid cells are 50 km by 50 km but as the surrounding 8 cells are used for the probability distribution over the 40 year – the real resolution is coarser, isn't it? Does this still make sense for an individual farmer?

   We agree with the reviewer, we have discussed this point in section 4.3.2. In fact, we agree that the forecasts for dry spells are probably not ready to use by an individual farmer, as this requires that the absolute numbers of the forecast (not values below percentiles) are accurate enough at the local (farmer) scale. We also discuss here that a useful follow-up step would be to investigate medium-

   range forecasts, as these have a higher resolution and suffer less from model drift. The point of the probability distribution, consisting of the cell under consideration and its 8 neighbouring cells was not mentioned yet. We propose to include this point in section 4.3.2.

   As for heat stress, this typically occurs as a consequence of large, synoptic-scale phenomena, easily occurring at the scale of the Limpopo basin. So a skillful forecast of an above-normal frequency of occurrence of heat stress during the coming season may well find impact even at the local farmer scale.

4. It should have been mentioned straight upfront in the abstract, and in the methodology, that the A2 extreme emission scenario is used, which is only emphasized in 4.3.2. *We will mention this in the revised abstract.*

5. I am not sure if the indicator for dry spells frequency is meaningful for end-user decision making. (If the end user is a subsistence farming I am not sure also of the heat stress indicator)

   We hope we have covered this point enough with discussion section 4.3.2, where we will add the point of the referee of the population of the distribution function, consisting of the cell under consideration and its 8 neighbours.

The paper itself is well written, and the reviewer has confidence the authors can write a good paper and know the data handling in these complex models as well as the statistical methods. The section "4.3 Limitations of methods" shows they are well aware of the limitations. The objection the reviewer has is that most of these limitations are known beforehand and therefore not so much a result of the study of the data. (E.g. the choice of the A2 emission scenario)

Listing limitations in a research paper is good scientific practice and we do not dispute or claim that some of the limitations are known beforehand. This paper did not aim to discover 'limitations' of for example the A2 emission scenario - we investigated whether within these limitations, the usage of seasonal forecast *is a potential adaptation strategy. The listed limitations provides the context of the scientific results. The reviewer does not seem to remark that the limitations invalidate any of our results, and hence we believe that section 4.3. contributes to an enhanced discussion.*

HEAT STRESS INDICATOR While the reviewer is not a dairy expert, layman's knowhow is that:

- Not everywhere in the region cattle is held for dairy production. The areas in which the heat stress indicator increases, according to the paper, already have limited dairy production;
- Cattle by subsistence farmers is not held for dairy production mainly; the mitigation measures to be taken are limited as per current technical and financial resources for these mitigation farmers. The reviewer did not look it up, but assumes that most dairy is produced in large scale farmers. This does not deny that the dairy taken from cattle which are held for purposes of meat production as well, does form a dietary supplement for the subsistence farmers considered.
- A cow is a cow, whether it is from a subsistence farmer or from a large scale farmer; it will be affected similarly by the heat stress indicator, while the large scale farmer may be more recipient to the heat stress indicator seasonal predictions and may have more means to act. Why emphasize that this is for subsistence farmers?

  We try to answer to all 3 points mentioned above: first of all, it is true that also large scale farmers can benefit from forecasts. But also smallholder farmers often have options to mitigate drought. We mention some of these options on p. 14750, l. 23-27. Also, smallholder farms are more vulnerable to drought. E.g. a completely failed crop can pull a community into poverty, while large scale

farmers often have safety nets available such as crop insurance. Finally, coming back to the first point of the referee, we showed that across Zimbabwe, heat stress conditions will seriously increase in frequency. In absolute numbers, dairy production in this area may be low (compared to large entrepreneurs), but its importance for smallholder farmers and the agricultural communities around it is enormous, given their vulnerability to failing dairy production. Please have a look at the following youtube movie from the DEWFORA project to gain appreciation of this point. http://www.youtube.com/watch?v=Vgr-OiED338. In addition, in South Africa, Namibia and Zimbabwe, as well as Mozambique, dairy production is frequently recommended as a development strategy for small-holder farmers, on occasion within the context of land reform and restitution.

DRY SPELLS INDICATOR

- The way in which dry spells are analysed is not clear at all. The reviewer is aware of the Markov chain relationships in rainfall occurrence, but does not see how referring to the papers (page 14756, line 4) is sufficient to explain this crucial step in the analysis of dry spells. Such a method would have to be calibrated on daily rainfall records – which are not there for the future, are they? From reading the paper, the reviewer suspects that the method is used to disaggregate monthly rainfall, but to do this it would need to be calibrated on a few daily rainfall records in the region. As far as the reviewer is aware, it has never been used to disaggregate future climate change predicted rainfall. *We will clarify the analysis in the text. We did not calibrate a full Markov chain model, we merely converted the precipitation time series into a two-state time series (0: wet day, 1: dry day) which is the first step in such an analysis. We did not disaggregate monthly rainfall. The time series of both forecast and climate changed rainfall were both (sub)daily. The temporal resolution of the data used will also be clarified in section 2.2.*

- The dry spells indicator seems to take a 10 day dry spell as severe as a two times 5 day dry spell. It does not make sense to the reviewer that both would have similar impact on tilling. *Which length is relevant depends on the crop, that is considered. Some crops are more vulnerable for dry spells than others. We will clarify the relation between dry spell length and crop type in section 2.3.*
- Indeed, as stated in the paper, the seedlings are most vulnerable, and it would make sense to incorporate this in an indicator. *This is true. For the moment we have focused on only 2 indicators as mentioned earlier. Others could be incorporated as well in a future study. We will mention possible other indicators for further study in the manuscript.*
- Maize is mentioned as the particularly vulnerable crop. If this crop has a growing season of 120-140 days then why only study DJF? *During the start of this study, we elaborated whether to choose DJF or also include November in the analysis. The choice for DJF was somewhat practical: this gave us one additional month of lead time to consider in the study. The reviewer is correct that including November would be more logical from the point of view of the growing season length. We will mention the reasoning behind this choice in section 2.1. We should also mention that the predictability of dry spells is not as high as for heat stress index and that more work is needed on the precipitation forecast can be fully used in the region.*

These are the main comments, which would need quite a change in the paper (or a split up in three papers). Further suggestions/comments, while reading through, are added as track changes / comments in the document itself. *We didn't find a supplementary annotated manuscript in the discussion page of our manuscript. Of course, if you send us the annotated manuscript, we will consider your further comments in our revisions. Please note that we consider the novelty of the paper that it does not represent an isolated analysis of seasonal forecast skill or climate change, indeed the strength of this paper is to combine the two and thus breach a gap between the different time*

scales and (often) modeling communities.

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