Review of Törnros and Menzel "Characterizing droughts under current and future climates in the Jordan River region" by A.F. Van Loon

Comment: In this paper the authors calculated the SPI of the Jordan River region for different time scales and correlated this to the NDVI-index. They concluded that the 6-month SPI best describes drought in the region and subsequently used this index to estimate the difference between current and future conditions. They found that drought duration and severity are projected to increase in the region. Model results showed that also the irrigation demand is projected to increase.

General comments

Comment: The research presented in this paper is in my opinion not very novel, but it might be important for the region and in that respect deserves a publication in HESS. My main comment to this paper is that the authors are quite blunt in their statements and interpretation of the results. The manuscript requires major rewriting before it can be published.

Primarily more explanation is needed on the link between meteorological drought and vegetation. For example the relation between SPI and NDVI is not further investigated by looking at time series.

Reply: First of all we would like to thank Dr. Anne Van Loon for carefully reading our manuscript and providing us with many detailed and helpful comments which will help us to improve the manuscript. We are of course glad to hear that the results could be of importance for the study region, we are also grateful that some issues have been highlighted and that we have the opportunity to reply to these.

In a revised version of the manuscript we would like to clarify that SPI is a meteorological drought index that we have used as a proxy for soil moisture drought. In that way we can establish a link between SPI and NDVI. Hence, address the impact of meteorological drought on an agricultural response variable (NDVI). We would also like to discuss the results more in detail and clarify the assumptions and cautions that have to be taken when using SPI in order to address agricultural droughts (see also the specific comments below). In that way we can also deliver clearer interpretations of the results.

In the study, we have addressed the relationship between multiple time-scales of SPI and NDVI based on monthly regression analyses. One of the reasons why we choose scatterplots instead of time-series in order to visualize the results is that the data have a very strong seasonality. This makes it hard to distinguish the inter-annual variations in the monthly data if time-series are used. Furthermore, we are interested in the linear relationship between SPI and NDVI for different months; these relations are harder to communicate with time-series. Although we agree that additional time-series would be useful, we believe that the scatter-plots are even more informative.

Comment: The authors should do more analysis with the data they have, for example, using the modelling results and looking at the correlation between SPI and soil moisture anomalies (by using a soil moisture drought index, the threshold level method or the method described in Sheffield and Wood (2007)) and between soil moisture anomalies and NDVI.

Reply: Thank you for this suggestion. There is a close relation between vegetation and soil moisture in arid and semi-arid regions (e.g. Ji and Peters, 2003), and by only investigating the relation between SPI and NDVI we agree that the link SPI-soil moisture-NDVI is not completed. For the purpose of this study, however, we strongly believe that it is sufficient to apply NDVI as an indicator of soil moisture without first analyzing the relation between 1) SPI and soil moisture anomalies and 2) soil moisture and NDVI. We feel that the required amount of additional analysis would address too many relations and draw attention from the characterization of current and future drought which is another important part of this study. Although we would be prepared to include additional analyses, we strongly believe that this would result in an extensive study in itself. Nonetheless, in a revised manuscript we will clarify that NDVI is used as an indicator of soil moisture and refer to other studies, e.g. Ji and Peters (2003) and Farrar et al. (1994).

Comment: In the climate change analyses the authors compare the future period with the control period, but they fail to do the comparison between the control period and the observations. In the Discussion section a study is mentioned that shows that the precipitation in the GCMs is underestimated, but this only concerns averages. There is no mention about extremes. I advise the authors to include the observations in the comparison in Figure 6.

The results of this research should be substantiated by more references.

Reply: A comparison between the SPI application on GCM reference data and observation data is unfortunately not that straightforward since different normalization periods are used (see a more detailed reply in one of the comments below). Instead we believe that it is more valuable to address the uncertainties of the GCMs. Therefore, we agree that the information regarding extremes should be included in a revised version of this manuscript. Furthermore, more references will be included.

We would once again like to thank Dr. Anne Van Loon for spending time in reading our manuscript and for providing us with useful comments. Below, we have addressed all of specific comments one after the other.

Specific comments

Abstract

Comment: p.5876, l.9: inter-annual variation of NDVI? The SPI was correlated with monthly NDVI

Reply: This will be corrected to "inter-annual variation of the monthly NDVI".

Comment: p.5876, l.10-11: assuming that NDVI is a good proxy for agricultural drought, which is not the case because other influences play a role as well and irrigation is already applied in the region (see p.5889).

Reply: By conducting correlation analyses, we have identified a drought index that partly can explain the inter-annual variation in the monthly NDVI. It is true that SPI is a meteorological drought index, since also other factors like temperature and wind speed contribute to an agricultural drought. Nonetheless, SPI may be used for monitoring agricultural response variables as the NDVI, as shown by among others Vicente-Serrano et al. (2012) and as indicated by the result of the present study.
It is correct that irrigation has an influence on the results, after all irrigation is practiced in order to stimulate vegetation growth, especially during time-periods with rainfall deficits. Nonetheless, there is still a correlation between the amount of rainfall and NDVI

also for farmland (Törnros, 2010). Furthermore, the water available for agriculture is limited and each farmer only receives an annual water quota for irrigation (Fleischer et al., 2008). Therefore, a relation between SPI and NDVI can also be expected for agricultural areas.
In the revised manuscript it will be clarified that SPI is a meteorological drought index and not an agricultural drought index, but that there are studies which have shown that SPI can be used for monitoring drought impacts on agricultural response variables like NDVI (Vicente-Serrano et al., 2012).

Introduction

Comment: p.5876, l.21-22: Reformulate the definition of drought to something like "Drought is an extended period with water deficits, often related to a lack of precipitation". And discuss the different drought types meteorological drought, soil moisture or agricultural drought and hydrological drought already here instead of mentioning them only on page 5877.

Reply: Thank you for this advice. The definition of drought will be re-defined and different drought types will be discussed.

Comment: p.5877, l.4-5: Cite papers that give an overview of drought indices and discuss the pro's and con's of the various indices. Examples are: Mishra and Singh (2010) and Sheffield and Wood (2011).

Reply: The introduction will be extended with a discussion about advantages and disadvantages of some of the most commonly applied drought indices (PDSI, SMI, Deciles, SPI, SPEI). References will also be given to studies which have addressed the differences between the indices more in detail, including Mishra and Singh (2010) and Sheffield and Wood (2011).

Comment: p.5877, l.11-12: The SPI has been recommended by the WMO for characterizing meteorological drought. SPI is not recommended for agricultural drought. You need to be much more careful which drought type you are studying. You should be aware of the discussion about using a meteorological drought index like SPI as proxy for soil moisture and hydrological anomalies. Several studies pointed out that great caution is needed in using these meteorological drought indices for drought related to soil moisture and water resources (see for example Teuling et al. 2013).

Reply: Thank you for this advice. Although SPI is a meteorological drought index, it may also be used to addressing the agricultural response to drought (e.g. Vicente-Serrano et al., 2012). Nevertheless, we will point out that is based on the assumption that SPI is linked to soil moisture drought. We also agree that we used the term agricultural drought in situations when it would be better to use terms like *agricultural response to drought* or *vegetation growth*. This will be considered throughout the manuscript. We will also clarify that WMO recommends SPI for addressing meteorological droughts.

Comment: p.5877, l.20-24: This is a too simple statement. It should be something like: "Some studies have shown that in some cases SPI can be used as proxy for soil moisture drought and hydrological drought, since in the end the hydrological cycle is mainly driven by precipitation and the catchment acts as a low-pass filter." In the Discussion section, you should come back to this issue and discuss that other factors than precipitation play a role in the development of soil moisture drought and hydrological drought as well.

Reply: Thank you for this comment. The linkage between SPI, and the time-lags between precipitation and the water supplies in soil moisture will be explained more in detail. We also agree that we have to discuss other drought related factors including

temperature, wind speed and water holding capacities of the soil etc. This will be done in the revised manuscript. Furthermore, we would like to add the following in the introduction: "… a meteorological drought is related to abnormally low precipitation. Although factors like temperature, wind speed and soil conditions also are of importance, a meteorological drought can trigger both hydrological droughts (abnormally low lake levels or river runoff) and agricultural droughts (abnormally low soil moisture)".

Comment: p.5877, l.29 – p.5878, l.1: Correlation does not show "how" vegetation responds to drought, it shows that there is a certain relationship between the two variables, but it does not explain the causes of this relationship. Correlation therefore does not directly "support the performance of the drought index".

Reply: We agree that the sentence was poorly worded and that it should be rewritten in a revised version of the manuscript.

Comment: p.5878, l.10-13: Again you should refer to the discussion about using indices based on precipitation for agricultural drought. It might well be that in the southeastern Mediterranean region precipitation is the dominant factor and soil moisture droughts and hydrological droughts are only caused by a precipitation deficit, but it should be discussed why other factors can be neglected, e.g. snow accumulation, evapotranspiration, non-linear transformation of the drought signal in the subsurface.

Reply: This is an important point and we agree completely. The region has the characteristic of a desert climate with a fairly continuously variation of temperature and radiation input over the year, whereas precipitation has a strong inter-annual variation (Noy-Meir, 1973). Furthermore, a pre-study has shown a strong correlation between precipitation and NDVI in the region (Törnros, 2010). Therefore, we have chosen to focus this study on multiple time-scales of SPI. Nonetheless, we do not exclude that another drought index like the PDSI would perform just as good (or even better) than the SPI. Such a comparison could be the target of a future study. We still agree that this should be mentioned; factors like snow accumulation, wind speed, soil type, evapotranspiration will also be discussed in the revised manuscript.

Comment: p.5878, l.13: "a drought index based on precipitation alone is appropriate for the southeastern Mediterranean region" > appropriate for what? to characterize agricultural drought? to predict biomass of vegetation?

Reply: We mean that the index is appropriate in order to address inter-annual variations in monthly NDVI (which can be considered as an agricultural response variable to drought). The sentence will be clarified in the revised version of the manuscript.

Comment: p.5879, l.6-13: The objectives of this study are not very clear and not well linked. What is the overall objective? How do the separate parts contribute to that objective? In which way do the focus points that are mentioned contribute to the objective?

Reply: Thank you for this comment. The overall objective will be re-formulated: "The overall objective of this study is to derive current and future drought characteristics and to address the agricultural response to drought in the wider Jordan River region".

Based on this comments, we also agree that a clearer link between the separate parts and focus points are needed. This could be phrased like: "First of all, this requires a drought index that can explain the spatiotemporal variation of vegetation. Secondly, the drought index has to be applied both on current climate data as well as on climate data received from future projections. Thereafter, a hydrological model can be used to simulate the Irrigation Water Demand (IWD) during identified droughts. In that way, the simulated IWD can be used to address the impact of climate change on the agricultural

sector. More in detail, the focus is on: (1) the interannual relationship between multiple timescales of SPI and monthly NDVI in order to identify the time lag between precipitation and drought conditions indicated by abnormally low NDVI; (2) to characterize current and future droughts according to duration and frequency; and (3) to simulate the IWD during the longest current and future drought in order to address the agricultural impacts of a changing climate."

Materials and methods

Comment: p.5879, l.22-23: The assumption that land use has not changed and will not change in the future should be substantiated by literature and discussed in the Discussion section.

Reply: We are sorry for this poorly worded sentence. Indeed, there have been land-use changes taking place and future land use changes are also expected (e.g. Koch et al., 2008). What we wanted to say is that this study addresses the impact of climate change and not those of land use change. The hydrological impacts of land-use change have already been addressed by Menzel et al. (2009). This will be clarified in the revised manuscript.

Comment: p.5879, l.26: The fact that this study focuses on agricultural drought should be mentioned in the Introduction section. And: even though this study focuses on agricultural drought it would be very useful to include natural vegetation in the analysis, especially shrubland, because that allows for a comparison between the response to drought of natural vegetation and crops. Since the arid region is mainly covered with shrubs this is relevant to interpret the results of the effects of climate change in this region (as given in Fig. 6).

Reply: It will already in the introduction be clarified that this study focuses on agricultural drought. We also agree upon the value of including shrub land in the analysis, we just have to make sure that the scatterplots fit in Fig. 3.

Comment: p.5881, l.14: Unclear how the "most appropriate" SPI timescale is identified. This is not straightforward. Explanation is needed on why linear correlation is used.

Reply: This will be clarified: "In order to address the relationship between the SPI and NDVI, a technique applied by Ji and Peters (2003) was used. They showed that the relationship between SPI and NDVI can be explained with a linear relationship as long as seasonality is taken into account. The correlation analyses were conducted separately for different land-uses. First, the average SPI and NDVI value of all pixels in each land use was determined for each month. Thereafter, the relationship between the two aggregated parameters was evaluated for each month of the year according to the correlation coefficient and the p-value. The SPI timescale resulting in the highest correlation with NDVI was seen as the most appropriate one."

Comment: p.5882, l.2: Why was the relation between SPI and NDVI only studied per land use class and not also per climate region as was done for the climate change analyses?

Reply: The correlation analyses were conducted separately for each land-use because the temporal relationship between precipitation and vegetation growth differs between plant species (Rosenthal et al., 1987). Therefore, in our opinion, it would be less appropriate to conduct this analysis ONLY according to climate regions. As you mention, we could have chosen to conduct the correlation analysis for land-uses AND climate regions. However, this would probably result in a redundant amount of correlation

analyses and obstruct the communication of the results. Although we could reconsider this for a revised version of the manuscript, we believe that the disadvantages would be greater than the benefits.

Comment: p.5883, l.11-12: Is this model calibrated? How are the parameters of the model estimated?

Reply: The TRAIN model is physically-based and the need or even the possibility for a physically based calibration is limited. Nonetheless, the model requires high quality input data and the validation is of importance. The hydrological model has been further developed and applied within the GLOWA-Jordan project. First, it was applied with data from a research site located in the Yatir Forest, at the northern fringe of the Negev desert in Israel. The research site has a mean annual precipitation of 285 mm and is operated by the Weizmann Institute of Science (www.weizmann.ac.il/ESER/People/Yakir/YATIR/). In addition to all required model input parameters, evapotranspiration is further measured based on the Eddy Correlation method. The evapotranspiration series was used for the validation of TRAIN at the point scale (Hausinger 2009; Menzel & Törnros, 2012). The model has thereafter been applied on the wider Jordan River region (e.g. Menzel et al., 2009) and validated with both runoff data (not published) and evapotranspiration derived from MODIS thermal data (Schmidt, 2012). The model parameter Leaf Area Index (LAI) was estimated by using typical vegetation phenology's for different land uses and the water holding capacities of the soil were delivered by Schacht et al. (2011). Relevant literature will be provided in the revised version of the manuscript.

Comment: p.5884, l.10-12: From the range in Fig.2 it cannot simply be concluded that "vegetation develops more slowly and reaches a lower maximum" during drought years. The consistency in the temporal information is completely lost. For this conclusion time series of dry and wet years should be studied separately.

Reply: Thank you for this comment. We agree and in a revised version of the manuscript we will either provide time series of dry and wet years or reformulate the sentence in the line of "it can clearly be seen that the NDVI differs between years".

Comment: p.5884-5885: Can you exclude that the good correlation between SPI6 and NDVI is caused by seasonal variation in both precipitation and crop growth independent of drought occurrence?

Reply: Since we conducted the correlation analysis between SPI and NDVI for each month of the year separately, we can exclude that the correlation between the 6-month SPI and NDVI is because of seasonal variations. This will be mentioned in the revised version of the manuscript.

Comment: p.5885, l.5-7: Be careful with the use of the significance. Significant results do not necessarily mean good results. Not-significant results do indicate bad results and the difference between significant and not-significant results can be used as a source of information.

Reply: We agree, at the same time we have defined significant as $p < 0.05$ and we believe that the use of the word significant facilitates the understanding for the reader.

Comment: p.5885, l.15-18: This is a very blunt statement. A negative correlation does not necessarily mean that the "shorter time scales are not capable of addressing agricultural drought" and a positive correlation does not necessarily indicate that "vegetation responds to precipitation accumulated over several months and that response is delayed". To prove this some more analyses are needed, like the correlation

between precipitation anomalies (SPI) and soil moisture anomalies and between soil moisture anomalies and vegetation (NDVI). Additionally, some example time series should be shown.

Reply: We agree that the sentence could be clearer. We will change "addressing agricultural drought" with "monitoring vegetation growth" and reformulated the second sentence: "The 3-month SPI has a higher average correlation (r = 0.18), probably because there is a time lag between precipitation and vegetation growth, and the impact of water deficits on vegetation is cumulative (Ji and Peters, 2003)".
We have not conducted any correlation analyses between SPI and soil moisture, although we would be willing to do that, we strongly feel that it is out of the scope of this study.

Comment: p.5885, l.22: It is not about "performance", only the relationship has more variation.

Reply: In the revised manuscript we would like to clarify what we mean by performance. With a good performance we mean that the drought index can be used to monitor the drought impacts on an agricultural response variable (NDVI; Vincente-Serrano et al., 2012). The sentence will be reformulated.

Comment: p.5885, l.24: What could be the reason that the "6 month SPI best explains the interannual variability of NDVI"? This is a very long timescale, which can surely not only be due to the reaction time of soil moisture to precipitation. Please elaborate on this.

Reply: Thank you for this comment. One of the reasons could e.g. be that the early season (October) precipitation is of high importance for the agriculture in the region (Ben-Gai et al., 1993; Otterman et al., 1990). The rainfall amount falling in October is e.g. not included in the 3-month SPI in January-May (Fig. 3). It will be elaborated upon this in the revised version of the manuscript.

Comment: p.5886, l.9: Convective rainfall events are not well simulated by GCMs. If this is the reason there should also be a clear difference between seasons and months.

Reply: Previous studies have shown an increase in the relative contribution of convective processes in the region when applying the RCM RegCM3 (Samuels et al., 2011; Krichak et al., 2010; Krichak et al., 2011). Therefore, we still hold it for possible that convective precipitation could be a reason why certain local areas show increases in precipitation, although Smiatek et al. (2011) mention difficulties in simulating convective process in the RCM MM5. Regarding the second part of the comment, there is a clear difference between months, although not visualized here it is mentioned on p.5886, l.11-19. Nonetheless, we will reformulated the sentence and include a reference to Samuels et al. (2011).

Comment: p.5886, l.23-24: Why not aggregated per land use type like in the previous analyses? It would increase the consistency if you would do both throughout the research.

Reply: Thank you for this suggestion. Before submitting the manuscript for review, we considered different options for presenting the results. Since the relationship between precipitation and vegetation growth differs between land-uses (see earlier comment) we decided to conduct the correlation analyses separately for each land-use. If one would like, this step could be seen as a validation of the drought index.
When it comes to communicating the results regarding drought characteristics, we believe that the results are more valuable if they are presented for the whole region (or even more valuable if they are presented for sub-regions) and not limited to certain land-uses. Although we would be prepared to change this, we still believe that climatic sub-

regions are appropriate for presenting the drought characteristics, even though it may decrease the consistency.

Comment: p.5886, l.25-28: Also include SPI characteristics for the observations and compare with the modelled control period.

Reply: We agree that it would be interesting to compare the SPI derived for observed data (1961-2001) with the SPI based on the two climate projections (1961-2060). Unfortunately, such a comparison would not be that straightforward since one has to consider that the different SPI applications are standardized according to different time periods (and that changing this would require major changes in the code). Although we agree that such an attempt would be interesting, we feel that it is out of the scope of this study. Furthermore, we believe that the drought characteristics we provide on p.5886 l. 28 – p. 5887 l. 19 and Fig. 6 provide enough information for the reader in order to get an understanding of the changing drought characteristics. See also a related comment by reviewer#2.

Comment: p.5887: Fig.6b does not give delimited drought events, but number of months with negative SPI.

Reply: On p. 5877 l. 19-20 we have defined that "Drought conditions occur when the SPI is negative in an event where the minimum SPI drops below −1 (McKee et al., 1993)". Although we will reformulate this sentence (see comment by reviewer#2), this is the definition we used throughout the study. Hence, Fig. 6b actually shows delimited drought events, not only the number of months with negative SPI.

Comment: p.5887, l.28: The pattern with "more vegetation" in Fig. 7a is not consistent with the NDVI map in Fig.2a.

Reply: Thank you for pointing this out. In our opinion, such a comparison is not that straightforward since the maps show different things and have different resolutions. However, we are aware of that fact that we parameterize LAI on a land use basis and not on a pixel basis. Therefore, the differences between land uses appear sharper in Fig 7a than in Fig. 2a. We have recognized the importance of the vegetation parameterization and another study incorporates a remotely sensed vegetation index in the TRAIN model (Törnros & Menzel, 2013).

Comment: p.5888: Include the correlation between SPI and NDVI and model results.

Reply: The correlation between SPI and NDVI is included in section 3.2. Unfortunately, we are a little bit unsure about the second part of this comment but assume that "model results" refer to simulated IWD. That there is a strong linkage between drought (as indicated by the SPI) and IWD can be seen in Fig. 7 (where the Fig. 7c shows more severe droughts than in Fig. 7b). It is also important to notice that this figure is based on mean values from three climate projections (this will be clarified in the text; see also comment by reviewer#2). Although one could derive correlation coefficient between 1) SPI and IWD and 2) NDVI and IWD this is nothing we have calculated. We would be willing to address this in a revised version of the manuscript.

Discussion

Comment: Include more references.

Reply: Considering that both the introduction and the discussion will be extended, more references will be included in the revised version of the manuscript.

Comment: p.5889, l.25-27: Why can the model be considered plausible? You can get the right results for the wrong reason.

Reply: In a revised version of the manuscript we will discuss some uncertainties of the results (see previous comments) and reformulate this sentence.

Conclusions

Comment: p.5890, l.12: Why are the results also "relevant for arid and sub-humid regions in general" No comparison with literature is done.

Reply: We agree that this sentence is redundant and will be removed in the revised version of the manuscript.

Comment: p.5890, l.23-27: These are non-funded generalisations. I suggest to take these lines out.

Reply: The lines will be removed.

Comment: p.5890, l.27: The arid-region was mainly covered by natural scrubland, which was not taken into account in the analysis.

Reply: We will include shrub land in the analysis (see earlier comments). Alternatively, we will remove the sentence.

Technical corrections

Comment: p.5877 l.2: "characteristics of soil moisture and drought" > "characteristics of soil moisture drought"

Reply: This will be changed according to the suggestion.

Comment: p.5877 l.14: "the calculation realizes the fit..." > choose another verb

Reply: This will be changed according to the suggestion.

Comment: p.5879, l.6: "a drought index, which ..." > "a drought index that ..."

Reply: This will be changed according to the suggestion.

Comment: p.5879, l.16: "preparedness for drought" > "drought preparedness plans"

Reply: This will be changed according to the suggestion and moved (see comment by reviewer 2).

Comment: p.5879, l.25: "remains dry" > choose another verb

Reply: This will be changed according to the suggestion.

Comment: p.5885, l.3: do not use the word "superior" in this context. Just say that the correlation between SPI6 and NDVI is more positive than the shorter timescales.

Reply: This will be changed according to the suggestion.

Comment: p.5885, l.20-21: "the value favors slightly the 6 month SPI" > rephrase

Reply: The sentence will be re-phrased.

Comment: p.5886, l.28: "projections" > "models"

Reply: This will be changed according to the suggestion.

Comment: p.5887, l.24: "in general" > "in most of the area"

Reply: This will be changed according to the suggestion.

Comment: p.5889, l.24: "potential erroneous" > "potential errors"

Reply: This will be changed according to the suggestion.

Comment: Fig.1: indicate countries

Reply: Countries and authorities will be indicated in the revised figure.

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