Review of Törnros and Menzel

Characterizing droughts under current and future climates in the Jordan River region

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General Comments

The authors calculate the SPI at several time scales using observed data for the Jordan River region and determine that the SPI-6 is most strongly correlated with NVDI, a measure of vegetation cover and vigor. Using three GCM simulations, SPI-6 is then calculated for current (1961-1990) and future conditions (2031-2060), and subsequently used to identify the most extreme drought events during each period. These drought events are used within a hydrological model to compute irrigation water demand for the region. The authors conclude that droughts in the eastern Mediterranean region are projected to be prolonged and more severe over the next century, resulting in an increased total water demand for the region.

The content and conclusions of this paper have merit and deserve publication in HESS, but the paper requires significant rewriting prior to publication.

The primary weakness of this paper stems from lack of detail in explaining the work that was performed, but in the Introduction and Methods sections. Because the Introduction does not clearly explain how the various sections fit together, it is difficult to follow the linkage between (1) SPI/NVDI correlations with observed data, (2) drought durations estimated using GCM simulations, and (3) future estimates of irrigation water demand. This problem is further exacerbated by a lack of specifics in the Methods section. The results appear to be interesting, but without details on precisely how SPI and drought durations were calculated and how these values were aggregated, these results do not have context.

I agree with Reviewer 1 that several statements in this paper are very strong, without significant support from the literature. When SPI is used in arid regions and to model climate change, there should be significant qualifications because this index has poor performance near zero precipitation and does not include the effects of temperature on evapotranspiration.

Specific Comments

Drought indices and their usage

The introduction lists several drought indices. I agree with Reviewer 1 that drought types (meteorological, soil moisture, or agricultural) should be introduced along with types of drought indices. It is important to note that the Palmer Drought Index is based on a soil water balance equation, incorporating an estimate of potential evapotranspiration, while the SPI does not. This has implications

when discussing soil moisture/agricultural droughts, particularly when climate change projections show a significant increase in temperature and duration of warm/dry spells. The author may want to mention the SPEI (Vicente-Serrano 2010), which is calculated similarly to SPI, but incorporates evapotranspiration, thereby including the effect of temperature increases.

SPI – Definition and Methods

SPI should be better defined within the introduction and significantly more detail should be provided on methodology.

Page 5877, Lines 14-15: The definition of SPI is poorly worded. Please use a clearer SPI definition, as in (Guttman 1999, Agnew 2000, Lloyd-Hughes and Saunders 2002, or Tsakiris and Vangelis 2004).

Page 5877, Lines 19-20: Using SPI < -1 to define drought is an arbitrary definition proposed by McKee et al (1993) because it is convenient and is easily understood statistically. Although this definition has been used regularly in the literature, there is little physical basis and no consensus that this is the only valid definition of drought. Therefore, please soften the language in lines 22 and 23. Also, when citing this SPI<-1 definition, it is useful to explain its statistical meaning – that accumulated precipitation less than this magnitude is expected to occur in 15.9% (1 std dev) of the month in question.

Page 5878, Line 13: The paper claims that a drought index based on precipitation alone is appropriate for the eastern Mediterranean, citing Törnros (2010). Many papers have concluded that SPI is less appropriate in arid or semi-arid regions because of difficulties fitting distributions near zero and the importance of evapotranspiration (Edwards and McKee 1997, Wu 2007, Lloyd-Hughes and Saunders 2002). Tornros (2010) finds a correlation between precipitation and NDVI, but does not compare this with any other indices or climate variables, such as evapotranspiration. So, while precipitation may be correlated with NDVI, this does not imply that a more thorough water balance index is not better suited to describe agricultural droughts in the eastern Mediterranean.

Page 5878, Lines 22-24: When trying to quantify the effects of climate change using a drought index, it would be useful to use an index such as the SPEI, which includes increases in evapotranspiration losses due to temperature increases. This is not necessary for this paper, but a note should be made to this effect.

Explanation of Methods

SPI methods should be highlighted in greater detail. Page 5881, Line 19-21 makes it seem as though 1961-2001 is used as the reference period (or long-term time series) for calculating SPI using observations. However, within the climate change section, it appears that the entire time series (1961-2060) is used to normalize accumulated precipitation (Page 5882, lines 25-27), making it impossible to compare observed SPI with GCM simulations of current conditions. Is this correct? If you were to select a single reference period (1961-2001), you could verify GCM current condition simulations with observed data, as suggested by Reviewer 1.

The SPI notoriously has difficulties in fitting precipitation at or close to zero (Wu 2007). For much of the region, the method for handling zero precipitation becomes important at SPI-1 or SPI-2, particularly in April or May. How is this handled in your work? Could this explain the potentially anomalous result (negative correlation) for SPI in Figure 3?

Spatial aggregation methods are not well explained in the Methods section. It is unclear whether SPI is calculated for all 96,000 cells separately and aggregated using the land cover classes or if you average precipitation based on these land cover classes and then calculate SPI. Additionally, the paper highlights the need for climate sub-regions because climate in the region is heterogenous (Page 5880, lines 14-15), but it appears these regions are not used in the correlation analysis (Figure 3). Ji and Peters (2003) do make use of climate regions in their similar work.

The method for calculating drought duration and unique drought events should be explained in greater detail. There is a short explanation in the results section (Page 5886, Lines 21-23) which should be moved to the Methods section and expanded upon. Similar to SPI, the paper should be very specific whether (1) mean drought duration was calculated for all cells and averaged, (2) SPI values were averaged by region and drought duration was calculated, or (3) precipitation was averaged by region followed by SPI and drought duration calculations.

Results

Page 5884, Lines 23-25: The paper states "no significant correlation is obtained between NDVI and the 1-month SPI". However, Figure 3 shows p-values of 0.03 (Mosiac/May), 0.06 (Cereals/Apr), and 0.08 (Cereals/May). This statement should be qualified that "little significant correlation is detected …".

The link between SPI and IWD analysis is a bit unclear. It appears that SPI is used only to identify a single, most extreme drought event from the current and future GCM simulations. Once identified, the full GCM dataset is used to simulate water demand for these two periods using the TRAIN model. If this is true, please provide characteristics of these drought events (duration, mean temperature, precipitation, wind speed, radiation, humidity). This will help the reader understand the projected differences between the current and future conditions.

Technical Corrections

- Page 5879, Lines 13-16 This statement provides good rationale for the research, but seems out of place following your main research statement. Perhaps it can be moved earlier in the Introduction.

- Spatial interpolation of precipitation is extremely fine for climate variables (1 km^2). I assume this level of downscaling is performed because the land use cover database uses this resolution. This should be mentioned during the downscaling discussion (Page 5881, Line 1-2).

- Page 5887,Lines 1-5: I recommend presenting mean drought duration as decimal months (8.95 months rather than 8 months and 29 days) to make it clear that the original data is at the monthly

scale. Showing the number of days implies that you are calculating drought duration at the daily scale.

- Page 5888, Lines 11-13: Please remove the semicolon and make this sentence into a list: “100-150 mm during the reference drought, between 150-200 mm during the future drought period, and in excess of 200 mm for some land uses in extreme cases”.

References

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