Response to reviewer #1

General comment:

Drought monitoring and early warning systems are valuable tools to enhance our understanding and better inform relevant authorities to act early and effectively to mitigate adverse effects that drought may bring to food security systems. Implementing strategies aimed at improving the reliability of Drought monitoring and early warning systems remains key. Authors of this manuscript have implemented an approach of assimilating remotely sensed data (LAI and soil moisture) to a land surface model (Noah-MP) to improve drought monitoring in the MENA region. The study is interesting and valuable not only for the MENA region but could also be applied in other regions experiencing drought challenges. Thus, the manuscript could contribute to valuable knowledge that this journal aims for. However, I have noted some scientific concerns (approaches and discussions) in the manuscript that could lower the quality of this manuscript at the current state.

Response: We thank the reviewer for the positive feedback to our study and please see our responses in more detail below.

Specific scientific comment:

1. Application of SMAP-DA must have failed due to assumptions taken in the model which were, highly likely, not representative of actual position on the ground. Model failure, especially from SSM-DA, was therefore almost certain. For this reason, the authors then focused on quantifying how data assimilation differentiates the categorization of drought and reproduces the evolution, duration, and intensity of past drought events as an indirect way to evaluate its impact on root zone soil moisture, which is good. However, the authors should first better highlight more on these assumptions, such as assuming soil is always irrigated to field capacity (lines 168 to 169) and uncertainty in irrigation information (irrigation frequency/timing and amount), as possible reasons behind DA failure. Could better representation of actual ground information in the model lead to improvements in model simulation after implementing SSM-DA? So in my view the authors should explicitly state that the reasons behind model failure was due to missing or limited in-situ data and local information and the decision to set the model with conditions that may not be the actual position on the ground.

Response: We thank the reviewer for the helpful suggestions. We now have added to our discussion regarding 1) uncertainties in the irrigation scheduling based on the soil moisture deficit approach in section 2.3 lines 180-182; and 2) possible reasons contributing to the failure of utilizing soil moisture data assimilation in improving modeling performance for this case study in section 4 lines 532-534. We agree with the reviewer that the accessibility to in situ observational data with respect to irrigation scheduling and/or remote sensing soil moisture product at a resolution that can detect irrigation signal may all contribute to a better model configuration for the study region. Moreover, the information utilization in the current SSM-DA setup is suboptimal, which may also affect the efficiency of data assimilation (Nearing et al., 2018). For instance, the CDF-matching scaling approach prior to DA may have removed most of the signals. In areas where irrigation and agricultural practices dominate, significant revisions to SSM-DA strategy are required. We are working on a project investigating the potential of utilizing disaggregated SMAP products to detect irrigation scheduling and improving the modeling of irrigation, but this work is beyond the scope of the current study, and we look forward to applying the approach to this data limited region in the future, if positive results can be found for our more data intensive testbed region.

2. Authors should demonstrate or reference other studies on accuracy and uncertainty of the RS datasets, such as MODIS, used in this study.

Response: We have now revised both sections 2.2.1 and 2.2.2 by citing references that have evaluated the accuracy and uncertainties of the SMAP L3_E product and MODIS LAI product.

3. In line 149 to 152, the authors relied on irrigation maps from global datasets. The study area is relatively small. Why couldn't they generate more accurate irrigation maps? Did they attempt to validate the irrigation map from global dataset?

Response: We acknowledge that there are uncertainties in irrigation mapping for our study region by relying on global datasets, which may be drawn from coarse report and satellite-based data (e.g., MODIS available at 500 m resolution), though GIA also incorporates sub-1km satellite information, and may not have been originally validated for this region. We spent much effort searching for possible local irrigation datasets for both model development and evaluation, including reaching out to our MENA partners, but we were unable to obtain such local maps at this time.

When developing our composited irrigation map from the GRIPC, GIA and GIAM datasets, we did check and verify the individual datasets and the composited map against Google Earth's imagery and available irrigation map plots published in the literature, including the following references:

Fig. 3.2 from Molle et al. (2019), which is based on irrigation information (circa 2010) from Morocco's MAPM (Ministère de l'Agriculture et de la Pêche Maritime). (2012). Place de l'eau dans le Plan Maroc Vert. Powerpoint

Fig. 1 from Kharrou et al. (2021) was later used for additional regional verification.

We have updated the text in the revised manuscript in lines 162-164 to now include the following statement:

“The final composite irrigation fraction map was verified against other imagery (such as Google Earth) and published Morocco irrigation maps (e.g., Figure 3.2 from Molle et al., 2019), and it is shown in Figure 1(b).”

4. In lines 306 to 310, the authors noted spatial variability of improvements in results obtained but failed to discuss the reasons behind these? Could they discuss this?

Response: We thank the reviewer for bringing up this question. The impact of data assimilation on total ET is largely determined by their relative impact on E and T component respectively as well as the ratio of E/T for the area. For instance, for grid cells that are classified as croplands, the magnitude of T is much larger than that of E. As LAI-DA leads to great improvement in T and T is the dominant component of ET for croplands, the overall performance for ET is improved. The dominance of transpiration in terrestrial ET especially for croplands are also documented in other studies (Jasechko et al., 2013; Kumar et al., 2020). We have added text in lines 321-323 to reflect this response.

5. Authors have stated in lines 351 to 352 that overestimation of transpiration during summertime, especially for croplands, likely due to misrepresentation of the vegetation seasonality. Why were there no attempts to better represent the vegetation seasonality in the model?

Response: There are indeed several efforts that have been made to address the weakness in the prognostic phenology module of Noah-MP from different perspectives (Li et al., 2021; Niu et al., 2020). In general, one factor might be contributing to this weakness is the oversimplification of parameterizing the effect of soil water stress on transpiration and carbon assimilation. Noah-MP, as well as many other earth system models, uses a β factor to control stomatal resistance and photosynthesis, which is a function of either soil moisture or matric potential depending on the options (Niu et al., 2011). However, this assumption has been shown to have resulted in large uncertainties (Trugman et al., 2018) and can systematically overestimate the effect of soil moisture drought on evaporative fluxes (Bonan, 1996). Another factor might be due to the limitation of static root profiles, which disconnects the interactions between changes in belowground water and nutrient resources and above ground plant carbon assimilation (Niu et al., 2020). The penalty of this assumption is even more obvious in dryland ecosystems or ecosystems during droughts as in reality plants under these conditions show much stronger resiliency to tolerate water stress (Ahlström et al., 2015; Fensholt et al., 2012). For instance, using the default prognostic phenology module, Ma et al. (2017) reported that Noah-MP simulates lower-than-observer GPP, LAI and ET during droughts for Central America and Niu et al. (2020) reported that Noah-MP lacks the ability to simulate the plant resilience to stress under the Texas drought in 2011.

Realizing the importance of the role of vegetation in regulating the hydrological and carbon fluxes especially under stressed conditions, emerging studies started to improve the parameterization of soil-vegetation-atmospheric interactions in Earth System Models. For Noah-MP, for instance, efforts have been made such as introducing plant hydraulics scheme (Li et al., 2021), modifying the β factor (Niu et al., 2020), including crop modules and dynamic rooting depth (Liu et al., 2020; 2016). However, parameter calibration and retrieval for these implementations are heavily relied on in situ observational datasets, which is challenging for the MENA region, where there is limited or inaccessible observational data for supporting this research. Perhaps using satellite observations to aid the derivation of plant related parameters, such as plant hydraulic traits would be one of the future directions to improve the model representation for data scarce regions. The improvement of the modeling structure would in turn provide extended benefits to data assimilation. However, these are beyond the scope of the current study.

Besides the weakness in the prognostic phenology module, human management is another factor that may shift the seasonality of vegetation condition, which is often hard to capture with models alone, in such cases, integrating models with improved representation of both natural and human regulated processes and satellite observed measurements may all contribute to a better representation of the fluxes and states.

6. Authors have stated in line 431 that limited in-situ soil data was available. what were the sources of soil moisture data? Can they also include these data in the manuscript? what about using rainfall data as proxy for soil moisture, standardised precipitation index? did they attempt to use this?

Response: We apologize for any confusion. We meant that there are no in situ observational soil moisture measurements available for this domain. We thank the reviewer for bringing up the idea. We agree that SPI could be a proxy for soil moisture and could be a good candidate index to compare with in terms of characterizing drought. However, as the cross comparison for LAI-DA and SSM-DA resulted soil moisture drought indicator for this study is only based on the period of 2002-2019, which might be too short of a period as SPI is normally computed based on a long-term data record. Besides, we have tried to request data from MENA partners and local stakeholders regarding evaluating the drought characterization, but such data were not provided. So, rather than finding and relying on a qualified reference to evaluate the data assimilation impact on drought characterization, here the goal is more towards comparing the difference in characterizing soil moisture-based drought between LAI-DA and SSM-DA. The evaluation for the data assimilation performance is more focused on ET, GPP, and NPP, which are fluxes that can be evaluated with relatively reliable remote sensing reference datasets.

7. Why are all figures placed at the end of the manuscript rather than at their rightful positions? This approach makes it difficult for the reader to follow smoothly.

Response: We apologize for the inconvenience it has brought for the review process. We will submit our revised version with figures and tables inserted in the main text near the location of the first mention.

8. Line 411, authors are referring to the figure 2(b), representing the difference of correlation (R) – computed as DA minus OL for the period of 2015-2019 for LAI-DA in terms of transpiration? why specifically stratify this figure?

Response: We apologize for the typo. It should be figure 1 (b), which indicates the irrigation fraction intensity and the distribution of the lightly, moderately, and heavily irrigated area. We now corrected this typo in the revised manuscript.

Technical corrections:

1. Line 65, separate DA and reference using separate brackets.

Response: Modified.

2. Line 191, state SSM-DA and SSM-DA_{irr} separately and not as SSM-DA (SSM-DA_{irr}). line 197 it is well stated LAI-DA and LAI-DA_{irr}.

Response: Modified.

3. Line 214, correct the superscript of degree in 0.05o

Response: Modified.

4. Line 256, SIF is not defined but then defined later in line 260. it should be defined at this point

Response: Modified. SIF is now defined in the first paragraph of section 2.5 when it appears for the first time.

5. Line 392, place irr as a subscript in $LAI\text{-}DA_{irr}$

Response: Modified.

References

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