Response to comments from Referee#2:

This paper introduces a new index (SVVAI) that quantifies the tropospheric vertical velocity, with the application of this index as a meteorological drought diagnostic. The SVVAI is compared to the 3-month Standardised Precipitation Index (SPI3) for six case-study drought events in China. The potential for the SVVAI in drought prediction is also explored for these drought events. The SVVAI is a novel approach to diagnosing drought, with most drought indices based on surface variables rather than atmospheric. Unfortunately, I found the execution of the article to be poor, and recommend rejection.

Response:

Thank you for highlighting the novelty of this paper. It is a new attempt but not perfect indeed.

Regarding the poor execution of the article mentioned by Referee#2, further studies have been conducted and some preliminary results have been achieved. However, it is another specified research that I can say too much herein.

Most of the detailed comments are constructive, and we appreciate it. Some of them surely facilitate the further modifications in the next version, while those issues that we are unable to solve in one single paper will be displayed in the discussion part.

Besides, some comments indicate that some misunderstandings happen mainly due to different academic backgrounds (e.g., concerns about the roles of dynamical climate diagnose in Figure 2 and Figure S2). Also, relevant illustrations (referee#2's concerns) may be easily ignored possibly due to unnoticeable locations. For example, it is noted that simulation results shown herein are concluded based on eighteen cases covering severe regional drought processes over all the climatic regions over mainland China during 1981-2016, most of which are displayed in the supplement files. Anyhow, we will try to illustrate them and make it as clear as possible.

Last but important, results in the paper generally consists of three parts: 1) relevant physical backgrounds in the form of routine climate diagnose on evolutions of vertical motion/horizontal divergence; 2) a newly proposed SVVAI and its application in simulating drought processes; 3) new attempt of drought prediction using the forecasted SVVAI. The first two parts are our major goals, and the third one is preliminary and displays the potential of being applied to drought prediction. It is a little difficult to solve all concerns in one single paper.

Major comments

1. The English is not of the required standard. It is frequently a significant challenge to interpret the text, requiring very careful reading. This makes it very difficult to focus on the intended presentation and interpretation of results. I suggest the authors improve the grammar before resubmission.

Response:

As commonly pointed out by Referee#1 and #3, English expression herein needs to be improved before resubmission. We will surely self-check, ask help from native English speakers to improve it.

2. The conclusions for both drought simulation and prediction are hampered by sample size. By selecting only a few case studies, it is difficult to draw overall conclusions about the relative benefits of SPI3 versus SVVAI. For example, how do the two indices compare during (a) other droughts, and (b) on all days (not just droughts)?

Furthermore, the authors' conclusions about the SVVAI being equal or superior to the SPI3 in predictive performance (L316) to be overstated. With only a small sample of events and no use of forecast verification metrics, it is impossible to draw this kind of conclusion.

Response:

As climate extreme events, severe drought processes rarely happen, leading to small sample sizes in nature. Even so, the severe drought events without seasonality consideration during 1981-2016 over all the climatic regions in China are research targets herein, which tend to result in relative general conclusions. In this situation, sample sizes herein are not so small.

However, we agree on the comment of making comparisons on all days (not just droughts). It is an important test whether the simulated SVVAI systematically corresponds to SPI, especially two tails (severe droughts and pluvial). We will add relevant illustration in the discussion part and try to implement it in our further researches.

We disagreed with the comment concerning the conclusions near L316. Preliminary comparisons of drought prediction between the SVVAI and SPI were conducted based on RAW forecast products indeed. We understand post-process procedures of hydrometeorological variables are important for operational forecasting. However, comparisons made based on RAW products are also necessary especially for a preliminary exploration of drought prediction using SVVAI. Actually, that further and specified post-process procedures are necessary were mentioned in LINEs 423-426 (in the discussion part).

Another concern from Referee#2 seems to be no use of forecast verification. Quantitative assessments (Fig.8 and Fig. S10) based on the mean and standard deviation of sample bias (see LINE 316-334) is one form of forecast verification metrics. This is a little different from routine forecast validations because process-based forecasting is our concern.

3. I do not think the authors adequately describe the reasoning behind using the hor-izontal divergence and vertical velocity as diagnostics of drought. Dynamically, how do we expect these variables to change during drought? Is that seen in the results presented here (e.g. Fig 2). The paper could do more to link the results to dynamical processes. For me this is a “major comment” because it underpins the whole point of the paper.

Response:

Dynamical subsidence is one important drought-inducing factor based on general knowledge of atmospheric science. To explain it as clearly as possible, we have a detailed introduction in LINEs 66-85, a very specified analysis of case studies in section 3. Dynamical process diagnoses during the drought (Figure 2 and Figure S2) and associated vertical profiles as a function of different periods (Figure 3 and Figure s3) are important backgrounds of the newly proposed SVVAI indeed.

Referee#2’s comments say the paper could do more to link results to dynamical processes. What we illustrated above is what we can do. To tell the truth, we have no better ideas regarding this comment, but we will think about it deeply.

4. There should be greater discussion of the relative benefits of SVVAI and SPI3. Why would you use SVVAI when you could just use SPI? In what circumstances is it preferable to use SVVAI? Can you draw those conclusions from the results presented here?

For example, Fig 6 implies that SVVAI is only comparable to SPI3 for eastern China, but there is no discussion of this. Does this mean SVVAI is not a useful indicator of meteorological drought for the rest of China?

Response:

Discussion about relative benefits and preferable application of SVVAI can be found in LINEs 396—401 (the third issue in the discussion part). We treat SVVAI as a complementary index compared to SPI, and we agree to confirm circumstances in which SVVAI is preferable.

Regarding the discussion about why SVVAI is comparable to SPI3 for eastern China shown in Fig.6, detailed climate-related illustration can be found in LINEs 292—299.
Performance of SVVAI in Figure 6 help understand why drought simulation in eastern China perform better (section 4.1 and 4.2). However, it did not mean it is not useful for other droughts for the rest of China, please see performance regarding droughts in regions except eastern China in section 2 and 3.

5. There is too much information presented across all the figures. I think some of the information should be synthesised. For example, is it really necessary to have 3 similar figures, each with 11 panels (Figs 9, 10, 11)? Surely there are ways of presenting this information more concisely. I also do not advocate simply moving some figures to the supporting information, as that already contains many results.
Response:
Figs 9-11 correspond to the 2011 mega-drought with a spatially huge coverage (illustrated in LINEs 340—343), which is a good case to verify the application of the SVVAI at the grid-scale nationwide. However, we think this comment is also reasonable, and we will further move one or two figures into the supplement files for simplification.

6. I find the conclusions of the forecasting section to not match the results. As mentioned in point 2, the statements about predictive performance are severely hampered by the sample size of events and lack of forecast verification metrics.
Response:
The corresponding response can be found in point 2.

Greater synthesis of the forecast results is needed. For example, for Figs 9, 10 and 11, the authors draw conclusions about model performance by visual inspection of spatial fields. While it is useful to visualise it this way, I do not think it needs to be done 3 times over (this is related to point 5). In fact, the authors already use a measure of pattern correlation (PCC). Could PCC not be used to quantify the similarity of forecast fields to the observation, or between the two models' forecast fields, as a way of presenting results more generally?
Response:
Pattern correlation coefficients (PCC), which is so-called Anomaly Correlations (AC), can surely be used to quantify the similarity of forecast fields to observation. Details about PCC (i.e., Anomaly Correlation) in section 8.6.4 of the book (Wilks, 2011). Anyway, we will move the key illustration and references from supplement files to section 2.6 to make it clear.
References:
Wilks, D. S. (2011). Statistical Methods in the Atmospheric Sciences (3 ed. Vol. 100): Academic Press.

Furthermore, I am concerned by the lack of forecast calibration. To be fair, the authors note that the CFSv2 forecasts used in SVVAI prediction are raw (L423-426), and could benefit from calibrating to the ERA-Interim vertical velocity and horizontal divergence data. However, it is not mentioned that the precipitation forecasts are also raw, and hence the SPI3 also require post-processing to indicate operational forecast skill. It is entirely possible (probable, even?) that the biases in the precipitation forecasts are larger than the biases in the atmospheric variables (in fact I think this is what the authors are saying in L83-85). Therefore, the forecast results are presented with the models on an unequal footing, making comparisons very difficult.
Response:

To tell the truth, this comment might be reasonable but a little bit confusing. We derived precipitation and vertical motions featuring omega from raw CFSv2 forecast products and treat them on an equal footing. Since forecast precipitation might have experienced post-processing procedures before release (means that precipitation products have better qualities than omega products), the promising performance of SVVAI in drought prediction may be more convictive.

Minor comments

1. Figure captions are not detailed enough. For example, what is Figure 7 showing?Why the many different orange and black lines? I assume they are different initialisation dates but please describe what's shown in the figures.

Response:

Thank you for the comment, as also pointed out by Referee#1. We will surely check figure captions and make them detailed and clear.

2. L153: What does the "critical proportion" mean? Please explain. Is this where you define "extreme" and "severe" drought processes?

Response:

Yes, it is related to the way we define grades of drought processes. We will make the description in section 2.3 clear. Thank you for the comment.

3. Figure 2 caption: "shadow" should be "shading".

Response:

We will modify it.

4. Figure 4, 7: The y-axis label should surely be something like "Standardized anomaly" rather than SPI3?

Response:

Some of them will be revised as SVVAI.

5. Section 6: Remind readers which forecast model is used (CFSv2) at the beginning of the section.

Response:

We described it in section 2.6. Anyhow, we will surely mention it again at the beginning of section 6.

6. L316: How do the authors come to the conclusion that SVVAI_ave is equal or superior to SPI3? Please walk the reader through the figure so that they can draw the same conclusions.

Response:

We agree with the comment and will revise it. Originally conclusions that SVVAI_ave is equal or even superior to SPI were drawn based on Figure 8 and Figure S10. We think it reasonable but will weaken/limit the conclusion because raw products are employed without hydrometeorological post-processing procedures indeed.