The authors have improved the manuscript and the logic is clearer. In general, the interpretation in the result section is still weak. There are still some issues that the authors may consider addressing or clarifying in the manuscript before I can recommend it for publication on ESD.

1. The novelty of this research, as highlighted by the authors, is the usage of multiple methods and models to investigate the impact of precipitation and temperatures on drought trends in EA. According to tables 2 and 3, observational/reanalysis and model data have different spatial resolutions, have they been resampled to the same resolution or used directly in the analysis? Also, the resolution of model data, e.g. GFDL (2.02°*2.5°), the extent of one region NK (2°N-4.5°N, and 34°E-41°E), it means probably only 6 grids are used to study the trends in variables in this region, whether the global-scale model simulation data is applicable to detect changes over small regions divided in table 1 / Figure 1. For trends derived from datasets with different resolutions, how were they eventually synthesized?

2. About datasets in section 2.2, the authors selected 35yrs or longer, multiple datasets spanning different lengths of years were used, e.g. observation Berkeley from 1750-2019, CenTrends from 1900-2014, GCM MIROC from 1850-2018, for trend analysis, 5-10 years more or less may not largely influence the final trend, however, if it were a 50-100-year difference, could the trend be biased simply because of the different temporal coverage?

3. GCM simulated precipitation data have poor accuracy compared to temperature. Apparently, in Figure 5, observations (CenTrends) suggested an increasing trend in precipitation over region SS, and four GCM models suggested declining trends, in the end, the synthesized trend was declining. Similar in Figure S1 for EE and S4 for NK. This seems that the synthesized trend is largely influenced by less reliable model simulation instead of the observed trend. The authors may consider justifying this in the discussion section.

4. As mentioned earlier, the results section seems weak, the authors did point out the regional differences of trends in four variables, additional interpretations may need to be added regarding the regional differences, for example, from the perspective of regional climate etc.

5. The authors selected soil moisture because it is a better indicator of crop health than precipitation to study agricultural drought, in conclusion, it's concluded that soil moisture can not be relied upon due to the large uncertainties in both observations and simulations and precipitation should be included given more reliable simulations and observations. This can be confusing and contradictory. As the authors concluded, previous studies using precipitation and this study using soil moisture all detected no consistent trends on droughts. This implies that drought in the study area is not getting worse with increasing temperature and precipitation deficit from the perspective of both meteorology and agriculture. The authors may need to rewrite this properly.

6. The discussion section is hard to read, please consider revising.

Some minor revisions are suggested as follows:

1. Page 2 Line 3, particularly "thorough" or "through" threats to food security?

2. The author did mention that the study period was from the pre-industrial era to 2018 in abstract, but it's hard to tell the study period from the datasets or introduction sections.

3. Some one-sentence paragraphs can be considered to combine with the others based on the logic.

4. Tables 2, 3, and 4 seem outside of the right sections.

5. In section 3.2, more convincing references should be included to justify those assumptions and decisions, for example, the authors assumed that using RefET doesn't influence the overall conclusion, does this mean that the crop coefficients are the same across different regions, if not, PET may vary stronger than RefET.

6. Line 24-28 in the conclusion section should be placed in discussion sections.

7. Units of trends in four variables should be added in Figures 5 and 6 and S1-DS6 in the Supplement figures.

8. Please consider revising some wordy interpretation， e.g. page 13, We therefore assume for ... We therefore..... and the discussion section is so lengthy that readers can easily get lost.