

Short comment 1 from Conradie

The paper uses a novel approach to study a topical question from a mechanistic perspective and the approach in this context could yield valuable physical insight and understanding into potential future changes over the south-western Cape region of South Africa. However, the paper suffers from a few major shortcomings; namely:

1. Long-term internal rainfall variability over the region is substantial and work by others (e.g., Wolski, 2018) suggests that for the entire available record (since the late 19th Century), variability overwhelms trend. However, this paper makes no attempt to distinguish between variability and trend and does not acknowledge the possibility that some apparent trends may be products of internal modes of variability. The period between the 1980s and 2010s has mostly coincided with a drying period of the lowest frequency internal mode in the observational record. **The paper shows that drying trends are intensifying and that multi-year variability is present amongst the trends due to climate change. The Wolski 2018 paper uses a rain gauge to establish regional trends, which emphasizes the inherently noisy behavior.**

2. The datasets which are compared have different periods of records over which patterns of internal variability and trend vary considerably. However, the potential biases thus introduced are neither acknowledged, nor corrected for in any apparent way.

3. Although rainfall trends are shown separately for different months, the argument around which the article is developed does not consider seasons separately. Changes in wind and humidity during summer, when easterlies appear to be strengthening, will have little overall impact on rainfall over most of the southwestern Cape, where winter rainfall concentration is quite large. The rainfall seasonality and mechanisms (frontal systems, mainly) are also never mentioned. Consequently there is no discussion of the impact of the observed trends on rain-bearing synoptic systems. Without this, the argument made would appear to focus mainly on the secondary matter of evaporation in the dry season, rather than the matter of primary concern, namely wet season precipitation. This contribution is certainly valuable, but the author appears to suggest that the work explains the primary causes of drying in the area, which does not appear to be the case. **Given that the rainfall trends are analyzed per month, and that May and Sept show significant drying trends, it is inferred that the wet winter season is shortening over time. This paper does not cover all the causes, but instead focuses on the mesoscale structure of drought, and the processes underlying an expanding dry summer season.**

4. The “vegetation fraction trend” analysis has numerous severe shortcomings, in that it fails to account for (or, at least it is not explicitly stated how any of this is done, if it is): (a) land use changes or urban expansion; (b) the distribution of agricultural land, plantations and wild vegetation; (c) the presence of natural or alien vegetation; (d) time since last fire (which recent work by Wilson, Slingsby and others has shown to be the most important determinant of NDVI in fynbos). **The green/brown colour ratio is an objective fraction that provides useful insights, despite land use and vegetation type. When vegetation fraction is reduced by a drier climate and urbanization, surface temperatures are more susceptible to heating, and moisture recycling is suppressed.**

Additionally:

1. Formatting of mathematical content, formulae and quantitative data is utterly appalling, to the extent where certain quantitatively dense sections are barely readable and difficult to follow. Basic conventions regarding typesetting of variables and numbers are apparently entirely ignored. At the very least, some in-line formulae should be displayed separately. **The equations are embedded in text to highlight the key processes underlying the drying effect of easterly winds. In every case, all variables and units are defined.**

2. The suggestion that the hottest period over the region occurred during January 2011 is suspect, given that previous investigations have generally found March 4, 2015 to be the warmest day recorded at most stations over the considered area, while the hottest extended period occurred during January 2016. These assessments admittedly consider air-temperature, but further substantiation of the methodology used to select the case study in 3.3 should be provided. The SAWS airport data used for this case study appear to have been sourced through Weather Underground, but this is not stated explicitly enough; in my experience, these data are not always consistent with those published subsequently by SAWS in daily weather bulletins. **As stated in text, the study area-averaged MODIS land surface temperature data were ranked to find the hottest period.**

3. Cape Town does probably have around 4 million people, but this should be substantiated from somewhere. **It is interpolated between censuses; a reference is given.**

4. The collective storage in the dams supplying Cape Town did not at any stage drop to 13% of capacity, as the author appears to suggest. Perhaps by including other regional dams not used by Cape Town, this quantity could be obtained, but then this is a little misleading. **The DWA reports in 2018 suggest < 20% of storage capacity, so that was changed.**

5. Many abbreviations are not defined and the style is often terse, such that the writing seems somewhat rushed. **Table 1 was expanded, acronyms are now defined.**

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Initial reply by author: **Subsequent feedback here in bold blue...**

[Conradie's SC] feedback offers guidance to improve the paper and also reveals a different research approach than taken by the author.

1. The drying trend is embedded in seasonal and year-to-year variability. Fig 3b shows that the down-trend accounts for 17% of variance in PDSI (P-E) anomalies, while Fig 3c shows that the down-trend accounts for 4% of variance in measured streamflow. **With seasonal cycle retained.** These points can be emphasized in the revision. Hence the author agrees that variability is high and trends are small but important. **The analysis of DWA station data was revised so that streamflow and potential evaporation trends are calculated, after filtering out the seasonal cycle. The outcome is that 31-41 % of variance is covered by the drying trend since 1956.**

2. The author uses a variety of datasets that are constrained by advances in science. The P-E gauge dataset (PDSI) provides a longer context and helps identify the late 1970s as another dry era similar to recent years. Using high resolution satellite data on soil temperature and vegetation helps identify locations around the mountainous catchments

that show desiccation. These datasets have been previously corrected for bias, as indicated in their respective references.

3. Fig 3a shows the rainfall trends per month and covers the issue of seasonality. It is clear that 'drying' is greatest at the beginning and end of winter, (May and September), hence interpretations of a 'shorter' winter rainy season. The remainder of the comment highlights the different approach this author takes to the study of drought. Whereas winter rainfall adds to the water budget, only summer evaporation can subtract. Therefore this author focuses on all seasons, and on the factors contributing to surface water losses. One of these is the subsidence of easterly winds that is compounded by coastal upwelling.

4. The vegetation trend aligns with the surface temperature trend, eg. warming and drying reduce the green / brown ratio. Urbanization, agriculture, alien invasion and fires all impact on vegetation and this can be revised accordingly. Notwithstanding human direct / local effects, the indirect effects of global warming that shifts the rain-bearing westerlies poleward, tends to prevail. **A caveat is added in 2. Data and Methods**

Additional:

1. an improved presentation of formulae can be done, **yet the author prefers to retain the text-embedded format, and is careful to define all variables and units.**
2. the author uses the MODIS 8-day land surface temperature averaged over the Hottentots Holland mountain area to rank the hottest (and driest) case, **this is re-iterated in text**
3. minor references can be added, **many new references were found and included.**
4. the Dept of Water Affairs values were used to indicate total capacity, which may differ from Municipal data, **this was changed to reflect < 20% of capacity according to DWA data in 2018**
5. the writing can be extended in revision. **Numerous text additions were made, and a new analysis of PDSI wavelet spectra and trend/variance - cropped at different start dates - is given in section 3.2.**

A final overall point is that this author believes that there is a need to study desiccation up in the mountain catchments via streamflow and area-averaged satellite-blended datasets, as opposed to a rain gauge in the Cape Flats.

End of initial author feedback

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Editor comment:

Title: The title reads "The climate of desiccation in the SW Cape". Since desiccation means drying, what is author implying by "The climate of drying".

In the Abstract, the author states that Cape Town's reservoirs are drying up and there is evidence of increase in temperatures over these reservoirs. Is the author suggesting that the drying of the reservoirs is due to an increase in the surface temperature? The low water levels in the reservoirs are mainly due to climate variability which is never considered in the whole paper. **That was analyzed and interpreted for the PDSI record.**

Under “2 Data and Methods”, the variables to be analysed are not clearly stated. What the author has done is to explain the data sources used. The author begins by stating that the study area has a dense network of rainfall, streamflow and potential evaporation stations. However, the author does not explain the reason for using satellite-derived data when there is a dense network of stations. The study area has major spatial variations of elevation (from sea level to over 1100 m), and land uses (urban residential, industrial, vineyards, orchards) and land cover types (grasslands, fynbos, forests, bare rocks). How accurate are satellite derived estimates of surface temperature, rainfall, evaporation in view of these spatial variations? **At the end of the introduction, the focus of the study is said to be ‘the mesoscale structure of drought and processes underlying...’**

**Although the network of observations is dense, nothing can compare with satellite products with 1-5 km resolution (eg. MODIS, CHIRPS2).**

Several studies have demonstrated that rainfall estimates based on satellite data are not accurate due to orographic effects and frontal rainfall systems prevalent in the study area. The author is not explicit about which variables and their characteristics will be subjected to trend analysis. Linear regression method is known not to be the best method for trend analysis. Why were other methods not considered? A major weakness of this paper is that the period of data investigated is never clearly presented with justification. Results are presented for 2010-2017, 1980-2017, 2000- 2017, 1981-2014, 1956-2017, 1901-2017? Why are these different periods being used? **This is now made clear in text, captions, and by new information given in Table 1. A new analysis was added on trends in PDSI in different intervals: 1901+, 1980+ and 2000+ to consider how multi-year variability affects the result.**

The construction of the Berg River Dam was completed in 2009. How is it possible that the decline in water levels will have influenced land surface temperatures during the 2000-2017 period even before the construction of the dam? **Yes, this was added, and is an interesting feature. Does dam construction exacerbate climate change? The results here suggest so...**

In 3.2 Temporal Characteristics, which river flow measuring station was used to determine a downward trend of river flows for the Upper Berg River? The author does not always explicitly state what is presented in some of the Figures. A reader struggles to understand most of the figures. Table 1 is not referred to in the text. The effect on interannual and multidecadal climate variability on rainfall have not been considered. Several studies have demonstrated trends may be a reflection of multidecadal variability. The starting and ending period of a trend analysis can influence the results obtained. There is a very rich literature on climate variability of the study area, and this is not reflected in the Introduction, and Methods used. **Yes, an additional analysis of how different starting dates affect trend and variance was added, using PDSI (Appendix 2).**

Reviewer 1

Title: What do the authors refer to as climate of desiccation? It’s not clear what exactly they intend to do? **The title was modified.**

Abstract Overall, it’s too general, it lacks details. It’s more of a descriptive section. What was the period covered by the study, in which they report that reservoirs are drying up and vegetation browning? **Details were added in the abstract and table 1.**

Materials and methods What are you referring to as high resolution NOAA MODIS satellite data since the data used was at 5-100km spatial resolution? Was there any validation to assess remotely derived results? **The MODIS infra-red data have been validated by NASA, and are currently in 5<sup>th</sup> generation, with a horizontal resolution of 1 km, as now stated in Table 1.**

Results The overall presentation of results is poor, making it very difficult for a reader to comprehend. How reliable were the remotely-sensed results derived at such a low spatial resolution? **Table 1 now stipulates the data set resolution and starting date. Satellite data is typically high resolution, but in some cases (eg. net OLR) is gridded to lower resolution.**

Figures Overall, the presentation of Figures is not clear what exactly they present for example, Figures 2c and 2d, Authors indicate that they show MODIS surface temperature trend (2000-2017), (d) vegetation fraction trend (2000-2017), BUT there is only one map? This does not show the variation or trends for those particular periods. No in-text reference to Figure 2b?

**There are three distinct panels to Fig 2 c,d,e which show linear trend maps in land surface temperature and vegetation fraction since 2000, and rainfall since 1981, respectively. The Fig 2 caption and text in 2. Data and Methods and Results section 3.1 were expanded accordingly.**