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Reply on CC1

Nele Tim et al.

Author comment on "The impact of the Agulhas Current system on precipitation in southern Africa in regional climate simulations covering the recent past and future" by Nele Tim et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-47-AC3, 2022

We thank Stefaan Conradie for his constructive comment. In the following, we list the comments (in italics) and reply to them (in normal font).

1. I would argue that the claims regarding recent WRZ rainfall trends made in the introduction (lines 31–33) are unclear, misleading and not well substantiated. Since this relates to relatively central conclusions of the study, a correction is required. More specifically

(a) Roffe et al. (2021) is cited to suggest a trend towards decreasing rainfall in recent decades over the WRZ using station data, whereas Roffe et al. (2021) do not conduct rainfall trend analysis at all, as far as I can tell. Many recent WRZ station rainfall trend assessments have been conducted, such as in Burls et al. (2019), Roffe et al. (2022) and Mahlalela et al. (2019).

Roffe, SJ, Fitchett, JM, Curtis, CJ. (Investigating changes in rainfall seasonality across South Africa: 1987–2016. Int J Climatol. 2021; 41 (Suppl. 1): E2031– E2050. https://doi.org/10.1002/joc.6830), found that 'trends for most locations across the southwestern Cape and western coast are indicative of a decrease in the strength of seasonality with a reduction in winter wet-season rainfall….’.

(b) MacKellar et al. (2014) is cited to support the assertion that “gridded observational data sets and reanalysis data indicate an increase ... [in] precipitation in the WRZ”, as opposed to the decrease found by Wolski et al. (2020) with station data. However, MacKellar et al. (2014) also use station-based rainfall data and not, as far as I can tell, gridded observations. Furthermore, for the period 1960-2010 considered by MacKellar et al. (2014), significant rainfall trends are found at only one station in the WRZ; trends at all other stations are insignificant and small in magnitude. This is entirely consistent with 50-year SW cluster trends centred on 1985 depicted in Wolski et al. (2020, Fig 4).

The reviewer is correct. MacKellar et al. (2014) show a statistically significant increase in precipitation over the WRZ in one station data. However, the trends at all stations of the WRZ are positive in the annual mean and this homogenous pattern is very unlikely to arise by chance, even considering that the station records are not totally independent of one another. Thus, although the statistical significance is individually low, taken all together it is very high. We would reformulate this paragraph.

(c) My reading of the maps in Onyutha (2018) also indicate negative rainfall trends over the WRZ over 1990–2015.

Figure 2n in Onyutha (2018) shows an increase in precipitation.

(d) Considering all studies I'm aware of that analyse recent rainfall trends over the WRZ (in addition to those previously listed: Ndebele et al., 2019; Du Plessis and Schloms, 2017; Hoffman et al., 2011; Kruger and Nxumalo, 2017; Otto et al., 2018; Seager et al., 2019; Pascale et al., 2020; Deitch et al., 2017; Conradie et al., 2022b; Zscheischler and Lehner, 2022), both station data and gridded gauge-based datasets indicate satellite-era drying, particularly since the 1990s. The primary disagreements between studies are because of either:

- *differences in the time period or spatial domain considered (as noted in the conclusion); or*
- *because the Climatic Research Unit (CRU) gridded rainfall dataset exhibits a growing negative rainfall bias after 2000, associated with a loss of data availability from wet reporting gauges Wolski et al. (2020).*

Wolski et al (2020) found that the remaining stations in CRU after 2000 are in a dryer region. This does not consequently imply a too strong drying but a spatially limited representation of the precipitation trend in the WRZ. Regarding the precipitation trend in the rainfall season of this region, austral winter, Onyutha (2018) found an increased in precipitation using CRU. Thus, there seems to be uncertainties regarding the different studies.

(e) Hence, any statement about existing evidence on rainfall trends over the WRZ should rather focus on the general agreement of data sources regarding drying in recent decades, but perhaps note that the results are sensitive to spatial and temporal bounds used.

We would include some of the articles suggested in the comment above to get more comprehensive impression on found precipitation trends. We would not only focus on the agreements, but also on the disagreements to avoid conveying the message that the historical precipitation trends are a robust result.

2. As noted by the first reviewer, the GPCP data (the version and a download link should be provided) used in this study covers too short a period to conduct robust trend analysis, especially given the well- established low-frequency variability in the WRZ, YRZ and SRZ (e.g. Dieppois et al., 2016). The short period of data coverage appears to be the result of the authors choosing to use daily rainfall data for these analyses, but it is not clear why daily data are required. Monthly GPCP data admittedly have a very coarse resolution, but the higher resolution daily data presented do not appear to accurately capture regions with large climatological rainfall gradients (see point 3 below). In addition, as far as I am aware, GPCP has not previously been used to study rainfall variability or change over the southern or western parts of southern Africa, where, as the authors note, the mechanisms driving rainfall occurrence and variability are different to the rest of southern Africa. Given that the authors pay considerable attention to rainfall changes over the WRZ during winter, validation should include comparison with a dataset that has previously been used over this region (and not CRU, given the biases pointed out previously), or whose regional veracity is demonstrated. GPCC data are available over a long period, at a much higher resolution and have been demonstrated to exhibit climatology, seasonality and variability characteristics deviating little from gauge-based observations (Wolski et al., 2020), so would seem an obvious choice. However, there may be reasons why other datasets are preferable in other parts of the domain, so the authors should of course use their discretion in selecting the most appropriate observations for their purposes.

Reviewer 1 suggested to use CHIRPS or ERA5. But as CHIRPS covers a relative short period too (1981 onwards), we would add ERA5 (as suggested by the reviewer) and additionally GPCC (available 1891 onwards) and CRU (available 1901-2020) for the validation purposes. By using CRU anyway, we can compare it to GPCC and check the effect of the bias on the precipitation trends for WRZ and SRZ.

3. The WRZ is mischaracterised in section 3, specifically in lines 126–129 and 138–140 and Figures 2 and 3. Specifically:
As far as I can tell, the bounds used for the WRZ domain in figures and analysis are not stated anywhere, nor is justification provided for the domain choice. It appears that an area that extends too far to the north-east has been selected, thus including places that receive very little rainfall, with all-year or late summer rainfall seasonality (cf. Roffe et al., 2019; Conradie et al., 2022a). This is reflected in the WRZ rainfall seasonality plots in Fig. 3, where the JRA-55 WRZ curve shows a secondary rainfall peak in March, suggestive of a late summer rainfall influence, whereas the GPCP data have an absolute minimum in September, suggestive of an interior YRZ seasonality. Furthermore, the amplitude of the seasonality in all datasets are much lower and the total annual rainfall much lower than most studies of the WRZ tend to find.

The WRZ region in this study covers the domain 15°E - 20°E and 28°S - 35°S. This WRZ spans the region west of Cape Agulhas (20°E and 35°S) up to the South-African - Namibian border at the west coast (as indicated in the blue line in Figure1 Roffe et al. (2021)). The choice of a rectangular domain lead to the inclusion of a small part of the YRZ and, yes, the WRZ includes regions of very little rainfall. The WRZ region is, of course, the same for all data sets. We would add the geographic coordinates to the manuscript.

In the text the WRZ is equated to the "Western Cape region", despite the WRZ covering less than half the Western Cape provincial area and extending beyond its borders. Without a specific definition, the "Western Cape region" is likely to be assumed by readers to correspond approximately to the South African province by that name. Furthermore, the WRZ is described as a "dryer region". Whereas some of the driest recording gauges in South Africa are located in the WRZ, the wettest recording gauge in the country is also situated in the WRZ and there are multiple gauges in the domain recording in excess of 1500mm per annum on average (Lynch, 2004; Slingsby et al., 2021). The South-Western Cape, as generally conceived, is certainly not a uniformly dry area, as appears to be suggested.

We would change the wording in a revised version.

Furthermore, the relatively coarse resolution GPCP data do not adequately capture the regions of high (> 3mm/day from other datasets) rainfall in the south-west, the Drakensberg or eastern Madagascar (Fig. 2 and 3). CCLM appears to do better over the Maloti-Drakensberg of Lesotho than the reference data.

We would add other observational data sets as mentioned above.

The fact that absolute scales are used in all panels of Fig. 2 and that the area on which most of the paper focuses on is dry relative to the Congo Basin and northern Madagascar means that the relative amplitude of the CCLM biases relative to GPCP are difficult to ascertain. In understanding trend and variability influence, relative bias is at least as important to assess as absolute biases.

We would include the bias in percent of the total precipitation in an appendix. Comparing to other observational data sets will improve the validation section.

No mention appears to be made of the fact that the amplitude of the WRZ seasonal cycle is about 2x larger in the hindcast simulation than the historical simulation. I therefore cannot concur with the authors that they demonstrate that "CCLM represents well the annual cycle of both rainfall regions SRZ and WRZ"; that "precipitation is generally realistically represented in the southern part of [their] domain"; or that "larger biases are mainly out of [their] focus region".

The amplitude of the WRZ seasonal cycle in the historical simulation is larger than the hindcast simulation. The underestimation of precipitation in the hindcast simulation is mentioned in the manuscript.

4. Whereas it is noted in the work that the YRZ occurs along the South Coast, it is not clear why in the analysis only the WRZ is distinguished from the rest of the domain, which appears to be collectively considered as the SRZ. In addition, very wet areas near the Equator apparently included in the SRZ have a very different precipitation seasonality to those areas south of roughly 15◦S. Whereas the text notes that these areas are not the focus of the study, why are these regions then used as part of the aggregation domain in Fig. 3? Or if they aren't, what domain bounds are applied for Fig. 3?

Figure 3 is applied over the whole domain. As the YRZ is the transition zone between WRZ and SRZ and represents a small region, it is not separated from the large SRZ. Trends are given here anyway as spatially resolved plots and as a spatially averaged timeseries. We would state the differences in precipitation trends in the YRZ-part from the rest of the SRZ in a revised version.

5. The purpose of section 4 in the study is not clear. The questions posed at the beginning of the section are not clearly answered, nor are they clearly related to the overall aims of the study. Figures 4 and 5 compare trends over very different time periods, meaning that results are not really directly comparable. This is noted in the text, but the comparisons are interpreted nonetheless. In addition, these figures do not indicate statistical significance or compare the magnitude of trends to interannual variability or climatology for the seasons, making assessment of their practical significance difficult. The JRA-55 trends also seems suspiciously large over the centre of the domain; there isn't perhaps a scaling error here? The statements in the text regarding the significance of JJA WRZ rainfall trends are not clear and do not appear consistent with the figures; Figure 5(d) for the historical simulation strongly suggests negative trends.

The main objective of the study is to quantify the impact of the Agulhas Current (variability and trends) on precipitation. To do so we need first to determine the precipitation trends per se. This is the rationale for this part of section 4, which follows trough later with the estimation of impact of the Current on those trends.
As mentioned in the response to a previous comment, and also to a suggestion by reviewer 1, we would add other observational data sets covering the hindcast period.

6. Regarding section 5:

How can SST trends be directly and separately attributed to Agulhas Current or Agulhas Leakage volumes using a simple univariate linear least-squares regression model when the two variables appear to co-vary significantly and apparently are both associated significantly with anthropogenic warming? Surely the method may merely be detecting changes or variability associated with a covariate? This is indirectly acknowledged in lines 249–250: "the SST trends in the North Benguela Upwelling System are probably not directly impacted by the Agulhas Current, but rather climate change impacts both the Agulhas Current and the SSTs". But it is not clear then how the conclusion can be drawn in line 251 that "the Agulhas Current and the Agulhas leakage contribute to the warming trend of SSTs adjacent to southern Africa." Perhaps the framing of this section needs to be *looked at?*

The parameters of the linear regression model are estimated using the historical interannual variability and this estimation is only minimally affected by the trend caused by climate change, since the interannual variability contributes much more strongly than the trend to the correlations between predictor and predictand. Yes, both variables co-vary at the interannual timescales, but to capture this covariability is exactly the objective of the statistical model. Once the regression parameters are estimated, the contribution of the Agulhas Current to the trend is calculated by multiplying the regression parameter by the trend in the Agulhas Current. This is a standard statistical procedure.
The linear regression model enables us to detect the proportion of the SST and precipitation trend that can be attributed to the strength of the Agulhas leakage and Agulhas Current. The correlation coefficient shows additionally the statistical connection between both variables. Surely, both variables, Agulhas strength and SST, can be impacted externally by the same forcing, like GHGs.

Again the significance of correlations are not mentioned or displayed in most cases, making assessment of the robustness of results difficult.

The statistical significance is more useful when calculating the correlation between two time series. However, when calculating the correlation between one time series on one side and multiple time series on the other side, the individual statistical significance is not that helpful, and it is the spatial pattern of correlations that gains in importance. This can be explained by a counter example: for instance, the correlation between the Agulhas Current and multiple grid-cells may be statistically insignificant individually, but if this correlation has the same sign over all grid cells, the spatial pattern can be highly significant, since this result is very unlikely to arise by chance. Here, the data independence between grid cells needs also to be taken into account to formally establish the statistical significance. We could add the level of formal individual statistical significance to the figure captions, but this can be also misleading. More informative in this case is indeed the magnitude of the trend compared to the interannual variability.

Furthermore, the statement in lines 247–248 that "the strong SST trend in the Cape basin is possibly more linked to the Agulhas leakage" needs to be substantiated.

This conclusion is plausible, as the climate change signal would be of larger spatial scale, but the reviewer is right that it is not completely substantiated by our results. We would rewrite this sentence.

In Fig. 9, are annual totals being considered here? And if so, why, when previously seasonal precipitation had been assessed?

As southern Africa has a winter and a summer rainfall zone, we separated the validation of CCLM and the analysis of the precipitation trend seasonally. For the analysis of the impact of the Agulhas Current System, we use annual totals as the strength of the Agulhas Current and the Agulhas leakage are only available at this temporal resolution. We will analyze the contribution for the winter season separately and will include it if it differs remarkably from the contribution of the whole system.

Lines 272–273: "The change in the dependency of Agulhas leakage strength on precipitation from the past to the future requires further investigation." Surely this is the incorrect direction of implied dependence?

We of course meant the change of dependency of precipitation on the Agulhas leakage. We would rephrase the sentence.

7. Parts of the discussion in the conclusion seem like they would be better suited to the introduction or results sections. The statement: "Dosio et al. (2021b) found a good agreement of JRA-55 to other observational data sets for precipitation (1979-2018) and show generally comparable precipitation seasonal means in observational data sets for southern Africa" should not be in the conclusion, but rather should inform assessment in section 3 of the implications of the comparisons conducted. At the scale that results are reported and conclusions drawn in this work (e.g. the south-east coast of South Africa, the South African South Coast or the WRZ), the cited paper does not appear to provide support for this claim, however.

Section 6 is titled Conclusions *and* Discussion, but we agree with the reviewer that this section needs a better structure, separating more clearly the discussion and the conclusion parts. We would do so by including a short Conclusions section without reference to previous results.

More minor concerns are:

1. The statement on line 1–2; "It defines the seasons and it directly impacts one of the principal sources of income, agriculture" should be substantiated with a citation. It is also not clear what is meant by rainfall "defin[ing] the seasons".

We meant that precipitation is in this region a critical 'defining' variable, which furthermore spatially divides the region in subregions with a winter and summer precipitation regimes.

2. It should be clarified in lines 16–18 that the far south-west (the WRZ) is unique in that most of the annual precipitation here occurs during the winter season; the all-year or year-round rainfall zone (YRZ), as noted in line 130, also receives significant rainfall contributions during winter, as do some other parts of the east coast (see, for example, papers reviewed by Roffe et al. (2019)). Also, while frontal rainfall contributes ≈ 90% of winter rainfall in the core of the WRZ (Burls et al., 2019), it is not accurate to imply this is the only source of rainfall in the WRZ (see, e.g., Abba Omar and Abiodun (2020)).

We would clarify this in a revised version.

3. As noted by the first anonymous reviewer, Jury (2020) and Jury (2015) are relevant to the discussion of rainfall and the Agulhas Current.

Both references would be included in a revised version.

4. The motivating paragraph, lines 41–43, makes claims that probably require further substantiation and clarification.

Changes in the winds, ocean currents and its link to anthropogenic change are the subject of the previous paragraph This is just a summarizing sentence. To be more clear, we would merge it to that paragraph.

5. Line 65: "Therefore, there is probably a large-scale common mechanism related to the increased radiative forcing that is behind this large-scale spatial pattern of precipitation reduction, and which is independent of changes in the Agulhas Current System." This mechanism has been widely studied by, for example, Seager et al. (2019) and Polade et al. (2017).

We would include cite Seager et al. (2019) and Polade et al. (2017). The mechanisms that these authors suggest for the Southern Hemisphere is essentially the expansion of the Hadley cell caused by increased atmospheric greenhouse gases. Surely, the Agulhas

Current System is only one of the drivers of precipitation changes. Our results show that 1/10 of the drying can be attributed to the Agulhas Current System.

6. Line 81: The "broader scope" of this study should be more precisely detailed here.

The scope is defined in lines 81-83.

7. Lines 128-131: The division of South Africa into 8 rainfall zones by the South African Weather Service in 1972 (Rouault and Richard, 2004) is only one of a range of subdivisions that have been proposed. The sentence should be rephrased to indicate this.

We would do this in a revised version.

8. Variables in equations should be denoted by a single character or symbol (with sub- and/or superscripts as necessary), not multi-character strings, to avoid ambiguity (e.g. is AL(t) a single function or is it A(t) × L(t)?), as is standard practise in all physical sciences (see, e.g., IUPAC guidelines linked to on the Copernicus manuscript preparation page). Where words are used in subscripts they should not be italicised.

We would do it as requested by the journal in a revised version.

9. The "wetting" referred to in line 163 appears to be focussed in the western interior, not west coast.

Figures 4b, 4c do show positive trends along the western coast as well. In Figure 4d these coastal trends are smaller but they are positive.

10. Line 169: "The hindcast simulation and JRA-55 agree that precipitation in the WRZ has increased in the past." The figure depicts this for the JJA season only; it probably does not apply in the annual total due to reductions in the shoulder seasons seen in most datasets.

Yes, we analyze in this section precipitation in austral winter (JJA) in the winter rainfall zone. Trends over the whole year are calculated in section 5.

11. Line 183: "is linked to" should perhaps be "may be linked to", unless the authors demonstrate the nature of the link.

We would change that in a revised version.

12. Line 288–289: Citation suggested.

It would be added in a revised version.