

Example reviews

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This document contains three reviews: first, an exemplary review rated 5 out of 5, second a good review rated 3 out of 5, and finally a review that was rated 1 out of 5.

An outstanding review – this review was rated 5 out of 5 by the editor. Some details have been redacted.

In their manuscript [manuscript title], the authors use a time series of surface wind speed observations in [geographic region] to evaluate trends in surface wind speeds in [time period]. The authors show that surface wind speeds generally slowed down over this time period, until around [year], when they claim the wind speeds started increasing again. They refer to this switch in the trend of the surface wind speed as the “turning point”. The authors then reconstruct surface wind speeds accounting for the influence of various large-scale atmosphere/ocean circulations. While they don’t find any individual circulation to have a strong impact on wind speeds in [geographic region], they do show that the combined effect of 6 climate patterns can be used to explain 86% of the change in surface wind speeds. They then use a single CMIP-class model

If the turning point detected in this study is robust, it is certainly an interesting find. The fact that no single circulation pattern explains this change is also interesting, since the combined “interactive” effect of multiple climate patterns appears to be generating a detectable surface wind signal.

Ultimately this work could be suitable for publication in [journal name], following major revisions relating to the robustness of the TP calculation and the physical mechanisms connecting large-scale circulations to surface wind speeds.

Major comments:

1) More details on the robustness of the TP calculation are necessary. To clarify, I don’t mean details on HOW the TP was calculated, but rather how the TP calculated here would be sensitive to slight changes in the length of the time series.

Specifically from the description of the methods, and from looking at figure 2, I'm left with a lot of questions about the robustness of the approach to determine the TP. Perhaps the authors could consider including a discussion of how sensitive their results are to the length of the time series they use.

- For example, if they started in [year] (especially for winter), would a turning point show up at all?
- Would it be in the same place? What if the data started in [year]? How does the upwards slope (post turning point) compare to the magnitude and duration of other upward-sloping sections of the times series?
- E.g. if the time series went from [time period], we would think there was a big turning point around [year] - but such a turning point doesn't have any impact on this analysis.

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- So, how do we know this is a real signal and not just a few recent years of increased wind speed that is well within the range of multi-year segments of increased wind speed that have happened in the past?

2) Particularly since the title explicitly says [redacted], it would greatly benefit the paper to dedicate more space/analysis to this topic.

Specifically:

- Currently, the manuscript looks at correlations between various atmospheric/oceanic modes (e.g. the Arctic Oscillation and the Pacific Decadal Oscillation). However, the authors do not present any compelling arguments for why any individual mode ought to produce changes in wind speed over [geographic region]. Including a discussion of how each mode would be expected to impact surface winds (e.g. by looking at how winds historically vary with each mode) and discussing what mechanism would drive the change (e.g. Mode A sets up a low pressure system over location B which generates C change in wind speeds in place D...)

- The authors don't show any particular mode has a strong impact on [geographic region] wind speeds, but rather instead show that the combined effect of several modes is required to generate a significant response in their reconstructed wind speeds.

3) The use of only a single CMIP model for the future projections because it happens to match the observations during the current period better than the other models is slightly concerning to me. Perhaps consider discussing why the other CMIP models do not capture the historical wind trend? Do they capture the correct pattern of LAOCs? For that matter, does the model you select capture the correct historical pattern of LAOCs and thus reproduce the surface winds, or is the historical pattern of LAOCs incorrect in the model, and the surface winds just happen to match observations? If that latter, it seems like little weight should be put on what the model projects under future scenarios.

Minor, but still important comments that should be addressed:

- RWS: please provide more details on how this is calculated. I know [journal name][journal format], but in the present form of the manuscript I don't follow how the RWS were determined (other than "using FSRA").

- Abstract: it would be much easier on the reader to avoid such heavy use of acronyms in the abstract, as it makes it hard to read. TP gets used a lot in the abstract, which is fine, but FSRA, LOAC, and RWS are only used a few times (only once for RWS), and are not sufficiently common acronyms to read easily – that is, each time the acronym comes up, the reader has to go and remind themselves what it is. This is fine for the main body of the paper where the acronyms get used a lot and have space to be properly introduced, but just makes the abstract hard to read.

- Page 1, line 53 and line 58: this use of brackets is confusing. I know it seems to be used more and more in scientific papers, but it greatly confuses the meaning, as the reader has to (1) read the sentence ignoring everything in the brackets then (2) re-read the sentence only focusing on the bits in the brackets. It saves space, but loses meaning. This bracket use is repeated quite often throughout the paper, and while the authors may think this comment is stylistic, but I am not alone in finding this way of writing hard to read! See for example this opinion piece in EOS 10 years ago:

<https://eos.org/opinions/parentheses-are-not-for-references-and-clarification-saving-space>

This disrupts the natural flow of reading through the paper leading to unnecessary reader frustration.

The first sentence in question reads: "Mean annual and seasonal SWS exhibited decreases (increases) before (after) the TP, and the most significant decreases (increases) were found in spring (autumn)."

This could be rewritten to be only slightly longer but much more clear as: "Mean annual seasonal SWS exhibited decreases after the TP, with the largest decreases in spring, while SWS increased after the TP, most strongly in autumn."

The following sentence does the same thing with the brackets: "The decreases (increases) in SWS before (after) the TP were caused by the decreasing (increasing) frequency..." and could be corrected in a similar way.

- Page 3, lines 53-59: Would the authors also like to include (3) "to identify the turning point"? That seems like a neat part of this study, and one that a significant portion of the paper was devoted to, but isn't listed here as "the point".

- Page 5, Line 35: "difference of trend... determined as TP". Is there also a requirement that the regression is positive (increasing) then negative? Otherwise, wouldn't this approach as described capture a situation like:

Years 1-10: wind speed increases slowly, but at the same rate each year

Years 10-15: wind speed increases quickly, but at the same rate each year

Year 10 would be identified as the turning point, even though it ISN'T a TP in the way the authors imply in their intro (i.e. that winds were slowing down, and now they're speeding up - the sign on the rate of change flips). I expect the authors account for this, but I suggest they clarify the text in the methods to make it obvious to the reader what was done here.

- Page 5, Line 37: the authors define the acronyms here, so I wouldn't bother doing it in the abstract - it was confusing in the abstract

- Page 6, Line 6: is this the statistical test used for all the figures, or specific to the FSRA calculations? Please clarify this, and explicitly state what statistical test was used for the figures (e.g. a t-test?)

- Page 7, Lines 3-7: consider moving this data to a table

- Page 7, line 9: what statistical test is used here?

- Page 7, line 9: it is important to mention here that, though the TPs are significant through whatever test the authors are using to determine that here, the slopes afterwards are not (except in Ann/autumn). I.e. the TP maybe be less of a true reversal of the trend in changing wind speeds and more of a flattening of the trend in surface wind speeds.

- Page 7, Line 40-56: Move to a table, then use a few sentences to tell us what is important about this list of numbers.

- Page 7, Line 40: "SWS reversal was an explicit phenomenon" – what does this mean? Overall the manuscript is very clearly written, but this phrase does not make sense to me.

- Page 8 line 4-7: see earlier comment about confusing brackets. In the first sentence, I think the "strong winds before (after) the TP" could just be replaced with "strong winds before and after the TP...". The next sentence could be replaced with "...trend for all wind categories was decreasing before the TP, and increasing after".

- Page 8 Line 6-9: "Before the TP, all trends passed the 0.001 significance level". Related to one of the major comments above re: the methods for calculating the TP, if you took the trend over the whole time series, not just up to the TP, would you also have a significant decreasing trend? More explanation is needed for why this method is robust.

- Page 8 Line 14: again, what significance test is used?

- Page 8 Line 17-22: "the weakest increase observed in strong winds during autumn and the stronger increase observed in strong winds during winter" - This doesn't seem consistent with figure 2, where the trend isn't significant in winter (or summer, or spring). The only significant trend is in autumn, which also (visually) looks like the strongest increase. Perhaps check the accuracy of this sentence and add values to the slopes on the bar-graph

insets to the left column of figure 2. (I mean annotate the bars - hard to tell the value from only 2 points on the y-axis). Or perhaps I'm misinterpreting something...

- Page 8, Line 25: it would be good, here or in a discussion section, to talk about why you might expect summer vs winter vs spring vs autumn wind trends to have different trends over the time period... e.g. if winter winds are dominated by one mechanism, and summer winds by another mechanism, stating how each mechanism might be expected to change over the time period and the implications for the observed SWS would be a helpful discussion.

- Page 8, line 27-35: bracket use in both sentences. Could correct the first e.g. as "Before the TP, the maximum trend difference among the four winds was found in the annual mean, with a standard deviation of 0.10 m/s/decade, while after the TP the peak difference occurred in autumn (standard deviation 0.13 m s⁻¹ decade⁻¹).", and a similar thing for the second sentence. The brackets used this way disrupt the narrative, even if it saves a few words in the final count.

- Page 8 Line 48 – Page 9 Line 12: the brackets here require the reader to read the whole paragraph twice to understand what is being said.

- Page 9 Line 27: Would the correct interpretation here be that no single CI has a large explanatory power, but only put together to they generate a large amount of explanatory power?

- Page 9, Line 37: "WP" – WPI? Otherwise define.

- Page 10, Line 54: Please clarify - it reads like the LAOC's are contributing more than 100% of the change in SWS, which doesn't make sense!

- Switch the order of tables 1 and 2 in the text? Table 2 is discussed first.

- Page 11, Line 40-48: Or (3) missing model physics or (4) a result of model resolution

- Page 11, Line 53-56: "For the near-term, mid-term, and lon-term forecasts, the SWS only showed an increase in the near-term under the SSP245." - Does this suggest that the model hasn't yet reached the TP? Or that the present TP is only expected to last for a short amount of time before winds start decreasing again? (i.e. it is a short-term uptick in winds)?

- Page 12 line 12: brackets

- Page 12 line 19-22: "Therefore, the increasing trends of the light and gentle winds were associated with the strengthening of the emissions." I'm not sure I follow this reasoning. Rather, perhaps the authors mean "That is, associated with increased emissions, light and gentle winds increased while strong winds decreased. Overall, the total wind trend was YYYY."

- Page 12 line 35-38: "Therefore, the frequency trends of the different categories of windy days can reveal the causes of long-term changes in SWS." Again, I don't follow this jump. I read this as "the trend shows the trend" which I suspect isn't what the authors mean.

- Page 12 line 43: In observations! However, most CMIP models do not reproduce this phenomenon. One model which does show the present day trend shows...

- Page 12 Line 56: Just a quick note to say that I'm not opposed to all brackets! The use of brackets here is just fine and doesn't interrupt the flow of reading!

- Page 13, Line 17: It is important to note here that the majority of the CMIP6 models don't show this behavior at all

- Page 13, Line 27-30: That is, EC-Earth3 isn't showing a robust, long term turning point, but rather just a short increase in the near term that goes away... how does this compare to EC-Earth decadal variability?

- Table 2 (Page 20, Line 6): "...power of the predictor (Rank)" – What does "(Rank)" mean here?
- Table 2 (Page 20, Line 9-12). I can't find this being explained in the methods (the 300-repetition test). Please include in methods.
- Figure 7 (Page 27, Lines 1-17). Add titles to subplots. Something like:
 - (a) Delta SWS (observed, modelled)
 - (b) Projections by 20 yr period (or something like that - somehow make it obviously visually different from c/d/e)
 - (c) 2021-2041
 - (d) 2041-2080
 - (e) 2081-2100

Typos:

- Page 5, line 32: "firstly" -> "first" ? Not sure what is meant here.
- Page 11, line 38-40: typo/grammar? Orphan from previous sentence? "Noting that all decreasing trends in the models were weaker than observation."
- Page 13, Line 45: "revealed" -> "determined"?
- Figure 6 (page 26, line 16): "Reconstruct" -> "reconstruction"

A review rated 3 out of 5. Around 65% of the reviews that we receive were rated 3 out of 5 in 2020. This review is helpful in informing the editor's decision, but some of the statements are slightly vague and difficult for the authors to address. The review also doesn't contain enough detail or insight to warrant a score of 4 or 5.

Thank you for the opportunity to review. This manuscript investigates the climate intensity of renewable natural gas (RNG) and concludes that because of methane leakage and modes of methane production, renewable natural gas can be climate intensive. Although I found the work to be generally clear, I have several major concerns with the manuscript:

- 1) The article concludes quite forcefully that RNG is incompatible with climate goals. This is not appropriately justified.
- 2) Throughout the manuscript, the author uses many long sentences that can make the meaning difficult to follow. I recommend revising to improve this issue.
- 3) The author relies heavily on hypotheticals throughout the piece. These should be more explicitly justified.

Other comments:

- 1) I think the figures could be more effective
- 2) I was unable to access / review the supplementary data but I appreciate that it was included
- 3) Intro — what is a drop-in fuel?
- 4) power-to-gas: do you mean methane only, or are other gases included in this analysis?
- 5) clarify "excess" variable electricity
- 6) did you consider using alternatives to GWP in the analysis?
- 7) is your downstream leakage assumption specific to a particular region, or do you expect to see better data emerging soon?
- 8) how significant is the assumption that "inputs to RNG production like electricity, hydrogen, and support infrastructure are assumed to be climate neutral"? could those conditions exist?

Reviews rated 1 out of 5. These reviews do not help the editor to make a decision and the recommendation is not justified. Reviews rated 1 may also contain rude or biased comments against the authors.

This manuscript is poor and should not be published.

This manuscript is fine – I recommend acceptance.