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Earth System Dynamics
Discussions

Interactive comment on "Recent changes of relative humidity: regional connection with land and ocean processes" *by* Sergio M. Vicente-Serrano et al.

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Summary

The authors analyse changes in relative humidity (RH) over continental regions during the period 1979-2014. For a number of regions with clear positive or negative trends in RH, they relate the RH changes to possible trends in local air temperature and local precipitation as well as continental evapotranspiration (ET), SST, and ocean evaporation. The authors determine relevant source regions for which to compute the latter three quantities based on an existing Lagrangian vapour tracking algorithm. They analyse relationships between these quantities and RH not only with respect to trends,

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but also with respect to interannual variations. Generally, the clearest relations they find are positive correlations of RH with precipitation and ET, and they conclude that continental ET plays an important role as driver of continental RH changes.

The results shown in the paper are interesting and well presented (although the text needs quite a number of corrections in terms of language/grammar), and I think they merit publication. However, I am not convinced that the causality put forward in the interpretation is sufficiently evidenced, so I would recommend to phrase some conclusions more cautiously. Specifically, I would argue that the positive correlation between RH and continental ET does not prove the suggested causality. There are a number of other points, of which some are minor but some are not so minor, detailed below, that deserve clarification and/or revision.

Overall, I recommend the manuscript should be reconsidered after major revisions.

Specific comments

P1L20-22: "The aim was to account for the possible role of changes in air temperature over land, in comparison to sea surface temperature (SST), on RH variability." - This sentence seems to suggest that this was the only aim of the paper, but the role of land and ocean evapo(transpi)ration changes is obvisouly also accounted for ...

P1L22-25: "Results demonstrate a strong agreement between the interannual variability of RH and the interannual variability of precipitation and land evapotranspiration in regions with continentally-originated humidity." - After having read the paper, I wonder if the last part of this sentence is supported by the content: It appears that the authors have not systematically assessed how the fraction of "continentally-originated humidity" is related to the strength of this agreement. However, this would be quite interesting. I suggest to compute the fraction of continentally-originated humidity (sometimes called the continental recycling ratio) for each of the regions (and season), and to relate this fraction to the strength of the agreement to verify (or not) this statement.

P3L83: "Nonetheless, there are unavailable empirical studies that support ..." - In my view it would be more logical (and elegant) to rephrase this to something like the following: "However, there are no empirical studies available that support ...". (It seems that the term "nonetheless" is used in the sense of "however", which I think is not correct, also elsewhere in the paper.)

P3L84-86: "One of these hypotheses is related to the slower warming of oceans in comparison to continental areas. In particular, specific humidity of air advected from oceans to continents increases more slowly than saturation specific humidity over land. This would decrease RH over continental areas [...]. The observed decrease in RH over some coastal areas, which are adjacent to their sources of moisture, adds further uncertainty to this hypothesis." - Is the observed decrease in RH, be it over coastal areas or further inland, not rather supporting the hypothesis?

P4L87-96: As explained here, the land-atmosphere feedback seems to be "only" a positive feedback rather than one that could explain why RH is altered under global warming in the first place, no?

P4L88-96 and P4L100-103: In this context (continental recycling ratios, evaporation-precipitation feedback, and the role of circulation changes), I can not resist to recommend that our paper on exactly these aspects (Goessling and Reick 2011), where we have combined moisture tracing with an idealised perturbation experiment in a climate model, revealing that circulation changes play a major role, should also be considered; see also other related comments below.

P4L107: "advections that were" -> "advection that was"

P5L125-128: Could the authors clarify how exactly the gap-filling was done?

Sect2.1: I suggest to use consistent headers for subsubsections 2.1.1-2.1.5, that is, either referring always to the dataset described in that section or to the variable(s).

P6Eq1-8: Most of the equations need some corrections with respect to units. Note that

T and P have units which implies that some of the constants used need to have the same units.

Sect2.1.5: While in the other sections the time period is always explicitly mentioned, that's not the case here; please clarify.

P8L190: "The statistical significance of the time series was tested at the 95% confidence interval" - I suggest it should be "trend" instead of "time series", and "level" instead of "interval".

P8L195: "with uneven number of stations" - I suggest this should be something like "with low station density".

P9L213-235: First, it would be good to clarify in what way the particles are distributed in the vertical initially: Does their vertical distribution correspond to the specific humidity profile? Second, I am wondering what explains the relatively short "optimal lifetimes" of 4-7 days found (as reported later in Sect. 3.2), and in particular what this implies. Given that even with the global-mean residence time of $\sim$10 days only the closest (1-1/e)*100% of a (typical) source region is captured, it appears that only half or even less is captured on average with shorter backward tracking times. It appears that contributions from adjacent sources (in particular from nearby land) are thereby overestimated compared to more remote contributions (in particular ocean). I suggest to clarify this.

P9L226: "as better as" - bad grammar.

P9L232-233: "the optimal lifetime selected for each region was that fulfills the minimum absolute difference between" - bad grammar.

P10L253-254: "the ratio between air temperature in the target region and SST in the source region" - If I am not mistaken, this is a quantity that depends on the units used for temperature (Kelvin or Degrees Centigrade). Also, wouldn't it be more straightworward to use simply the temperature difference instead of the ratio?

P10L260: "signification" - Should be "significance", right? (Occurs many times through-

out the manuscript.)

P11L263-267: "While a pixel-to-pixel comparison does not produce a reliable assessment of the possible contribution of land evapotranspiration to RH changes, given that the source of moisture can apparently be far from the target region, we still believe that this association can give insights on the global influence of land evapotranspiration on RH changes." - Here and generally, I have the impression that the suggested causality is not sufficiently attested and discussed. I would argue that increased land ET tends to be caused primarily by increased precipitation (except in very humid regions), and that anomalous precipitation (caused, e.g., by circulation anomalies) is simply accompanied by corresponding RH anomalies. And I think this is still largely valid for a non-local comparison, where land ET is determined for the "source region", because (i) the source region tends to overlap strongly with the target region and (ii) also most of the non-overlapping part is rather close, where spatial correlations of synoptic-scale anomalies are still high. I suggest this could be the main explanation for the positive correlations between RH, precipitation, and land ET.

P12L290: "uneven distribution" -> "low density"?

P12L302: "West Sahel" - Should be "East Sahel", right?

P13L315-317: "On the contrary, areas of complex topography in the Northern Hemisphere, Australia, India, Northern South America and Africa showed positive trends." - Can the authors comment why this should be so?

P13L324-326: "high consistency between the HadISDH and the ERA-Interim datasets in terms of both the magnitude and sign of change of in RH (Supplementary Figures 2 and 3)" - Are these figures not showing the correlation of interannual variations instead of the "magnitude and sign of change" (where the latter sounds as if the long-term trend is referred to)?

P13L328-P14L337: If I am not mistaken, according to Figure 4, q has decreased less

than it would have had to decrease in order to maintain RH constant (apparently due to a cooling trend?) in the mentioned regions (Southwest North America, the Amazonian region, Southern South America, and the (eastern!) Sahel). It appears that this corresponds to an INCREASE in RH in those regions, instead of the decrease shown in Fig. 1. Please clarify this contradiction.

Sect.3.2: I suggest to structure this subsection with subsubsections corresponding to the regions discussed.

P14L353-354: "the atmospheric moisture is mostly coming from the western Sahel region itself, in addition to some oceanic sources located in the central eastern Atlantic Ocean." Here and generally, I would find it helpful if the fractions of moisture from the different source regions could be quantified, e.g., through tables that list which percentage stems from the continental source region, which percentage from the oceanic source region, and which percentage from elsewhere. Regarding the western Sahel specifically, such numbers could be compared with numbers reported in Goessling and Reick (2013), according to which "only" 40% of the precipitation in the western Sahel stems from the African continent (consistently between different tracing methods, see Table 2 therein). Is it possible that this discrepancy is due to the short "lifetimes" used for the backward tracing, which leads to an overestimation of nearby contributions (as argued above)?

P15L363-365 and: "the interannual variability of RH in the region is strongly controlled by changes in the total annual precipitation and the total annual land evapotranspiration in the continental source region." - As detailed above, I think that the causal links are not sufficiently evidenced.

P15L374-375: "These relationships together would explain the observed trend in RH" - I find this paragraph confusing. In particular, the connection between correlations of interannual variations on the one hand and long-term trends on the other hand is not clear.

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P15L378-379: "These results would suggest that RH has mostly changed over the West Sahel region, as a consequence of changes in the continental humidity sources" - Again, I think that the causal links are not sufficiently evidenced.

P15L380: "the same results" -> "the corresponding results"

P16L396-397: "Given the high control of these variables on the interannual variability of RH" - see my above comments on the causality issue.

P17L417-418: "other regions showed a weak correlation between the temporal variability of RH and land evapotranspiration in the moisture source region. A representative example is China" - I am wondering whether this might be explained largely by the fact that relative interannual ET variations are just much weaker in China (around 10% of the mean value) compared to other regions (20-30% of the mean value) so that the signal-to-noise ratio is worse in China?

P18L438-439: "Figure 10 depicts the relationship between RH and land evapotranspiration seasonally and annually at the global scale" - these are local ("pixel-by-pixel") correlations again, right? I recommend to clarify this, and in what way the interpretation differs from the previous analysis where RH in target regions is correlated with ET in corresponding source regions.

P19L474: "Island" -> "Continent"

P20L492-494: "although some regions showed positive changes in the oceanic evaporation, the amount of increase was much lower than that found for SST, suggesting a general positive trend in most of the world ÌĄs oceans" - I appears that this is consistent with the finding that, in contrast to q, "the global-mean precipitation or evaporation, commonly referred to as the strength of the hydrological cycle, does not scale with Clausius–Clapeyron" (Held and Soden 2006, in particular Fig. 2 therein).

Sect.4: In my view, a lot of the material presented here belongs rather into the introduction (e.g., L539-564; L575-584; L609-616; L629-644). I think that the main conclusions

from this paper could be worked out much more clearly if the references to other work were repeated here in a much more condensed form, so that they can be better linked with the authors' conclusions.

P22L534-536: "This finding highlights the importance of land evapotranspiration processes in defining RH variability over large world areas." - The causality again ...

P22L545-547: "Numerous model-based studies have supported the strong influence of land evaporation processes on air humidity and precipitation over land surfaces" - a very strong link has also been shown in Goessling and Reick (2011).

P23L565-567: "results indicate that humidity in the analyzed regions is largely originated over continental rather than oceanic areas." - I'd like to repeat (i) my suggestion to report percentages telling how much of the moisture arriving in the target regions stems from the different sources, and (ii) that the method used here might overestimate continental contributions.

P25L522: "since" - this word does not seem to make sense here; I suggest to rephrase the sentence.

P25L624-632: I fully agree that these other factors, in particular circulation variability and trends, introduce considerable uncertainties into analyses such as the one undertaken here.

Fig.9: It is not entirely clear to me in how far the "inter-regional" correlations shown here provide a different angle on the matter compared to the "intra-regional" correlations of interannual variations. Could the authors comment?

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