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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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H. F. Goessling (Referee)

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

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respect to trends, 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.

We really appreciate the careful reading, the number of comments raised by Dr. Goessling and the general positive assessment of the manuscript. Please find below the answers to each comment and if you have any further concerns, please feel free to raise these new comments.

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 ...

This has been addressed in the revised manuscript as follows:

“The aim was to account for the possible role of changes in air temperature over land, in comparison to sea surface temperature (SST), but also the role of land evapotranspiration and the ocean evaporation on RH variability.”

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.

We have addressed the suggested analysis in the revised manuscript. In the methodology section we have included an explanation about this:

“Also from FLEXPART simulations, we obtained the fractions of moisture from the continental and oceanic sources annually and for each cold and warm season. The purpose was to compare with the results obtained on the role of the land evapotranspiration and ocean evaporation of RH variability and trends.”

In the results section (section 3.3) we have included the analysis suggested:

“In any case, attributing causality to the observed RH changes is quite complex given divergences found at the global scale. We have computed the fraction of continentally-originated humidity for each region and season and related this fraction to the strength of the agreement between RH and Land evapotranspiration at the annual and seasonal scales. Supplementary Table 1 shows the percentage of contribution of continental areas to the total moisture in each one of the fourteen analyzed regions, which oscillate between 31.6% for West Europe and 64% in Northeast Asia. There is not a significant relationship between these percentages of contribution and the strength of the agreement between RH and land evapotranspiration obtained in each region (Supplementary Figure 47). This reinforces the complexity of attributing changes of RH to a single factor. In any case, in some of the regions that show significant changes in RH

have been identified, there are also changes in the total contribution from continental areas at the seasonal and annual scales (Supplementary Figures 48-50). Both West Sahel and East Sahel show increased contribution of continental areas. On the contrary, La Plata region, in which there is also a strong agreement between RH and land evapotranspiration and that shows a significant negative trend in both variables, there is a decrease of the continental contribution. This stresses the complexity of giving a unique attribution to the observed RH changes."

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.)

Replaced here and throughout the entire manuscript.

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?

The cited studies show several findings of declines in RH over coastal areas. We do not state that these results invalidate the hypothesis but they add reasonable uncertainties as stated in the manuscript. Thus, under warmer conditions in regions close to water bodies it could be reasonably assumed a stationary RH instead a general RH decrease.

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? We fully agree with your comment, and the term "global

warming" has been removed from the sentence.

P4L88-96 and P4L100-103: In this context (continental recycling ratios, evaporation-precipitation feedback, and the role of circulation changes), I cannot 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.

Yes, really a perfect citation for this statement. Thanks.

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

Replaced

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

We have detailed this in the revised manuscript:

"In order to avoid biases in the filling due to differences in the distribution parameters (mean and variance) between the candidate and the objective data series, a bias correction was performed on the candidate data. Thus, normal distribution was used for bias correction of RH. The data of the candidate series were re-scaled to match the statistical distribution of the observed series to be filled, based on the overlapping period between them."

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).

Modified in the revised manuscript.

P6Eq1-8: Most of the equations need some corrections with respect to units. Note thatT and P have units which implies that some of the constants used need to have the same units.

We have included the units in the revised manuscript.

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

Time periods were addressed in the revised manuscript.

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".

Replaced in the revised manuscript.

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

Replaced in the revised manuscript

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?

Of course, the vertical distribution is coincident with the specific humidity profile in the way that the ERA-Interim reproduces it. The FLEXPART model uses the whole levels of ERA-Interim to compute the specific humidity and the particles over a specific area take the value of q at the corresponding level. The model also ensures the existence of particles at all levels.

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 âĹij10 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.

We are aware of the great discussion that in recent times exists about the residence time of water vapor in the atmosphere. Depending on the approach you use to estimate it the average residence time can vary from 3-5 to 8-10 days or even more (see Läderach, A. and Sodemann, 2016 and its discussion supporting for shorter periods or van der Ent and Tuinenburtg, 2017 supporting for longer periods).

It has been usual to consider 10 days (as average), and this is the time used in most of the studies to compute moisture transport (including most of the previous studies by the authors). 10 days is not a magical number in the sense that there is a great variability depending on the season and the latitude considered. So in this work we preferred to compute an "optimal" time more than a real residence time, approach already used in our recent studies (e.g. Miralles et al. 2016). To do this we computed for each analyzed region "the most adjusted time" comparing the moisture transport for precipitation with precipitation data taken from a reanalysis. So first, the sources of moisture for each target region where calculated in a backward mode using 15 days of transport. Once the sources regions where delimited, we calculated in a forward mode the balance of E-P again during 15 days (we move all the particles departing each source and reaching the target region). Then we checked over the target region which was the "most adjusted time" of Flexpart result for E-P<0 ("precipitation") for each grid point, and finally we calculated the mean value of this "optimal adjusted time" for the whole target area. This is the time that we used in this study.

An explanation of this approach is included in the new version of the manuscript

Läderach, A. and Sodemann, H.: A revised picture of the atmospheric moisture residence time, Geophys. Res. Lett., 43, 924– 933, doi:10.1002/2015GL067449, 2016.

D.G. Miralles, R. Nieto, N.G. McDowell, W.A. Dorigo, N.E.C Verhoest, Y.Y. Liu, A.J. Teuling, A.J. Dolman, S.P. Good, L. Gimeno (2016) Contribution of water-limited ecoregions to their own supply of rainfall, Environmental Research Letters, vol 11, doi:

doi:10.1088/1748-9326/11/12/124007

van der Ent, R. J. and Tuinenburg, O. A.: The residence time of water in the atmosphere revisited, Hydrol. Earth Syst. Sci., 21, 779-790, https://doi.org/10.5194/hess-21-779-2017, 2017.

P9L226: "as better as" - bad grammar.

Replaced in the revised manuscript.

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

Replaced by:

“iii) the optimal lifetime selected for each region was chosen according to the minimum absolute difference between the FLEXPART simulated precipitation. . .”

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?

We have used the same units (°C). We think it is not relevant if the ratio or the difference is used to assess long term trends in the evolution of SST and land temperature.

P10L260: "signification" - Should be "significance", right? (Occurs many times throughout the manuscript.)

Replaced here and throughout the entire 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.

This has been included in the discussion section.

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

Replaced

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

West Sahel is correct here. East Sahel shows a clear RH decrease.

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?

This is analyzed in depth in section 3.2

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)?

This is true. The sentence has been rewritten as follows:

“Given this high consistency between the HadISDH and the ERA-Interim datasets in terms of both the magnitude and sign of change in RH (Figures 1 and 2) and also in interannual variations (Supplementary Figures 2 and 3),. . .”

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.

No, it means that in these regions q has dominantly decreased (left), so to maintain a RH constant in these areas, according to the observed warming rate, there is a deficit of absolute humidity quantified in more than 2 g/kg-1. Negative value (in red) represent a deficit of moisture and positive values (in blue) represent an increase of q higher than that necessary to maintain Rh constant according to warming rates.

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

We followed this suggestion in the revised manuscript.

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)?

See above. This has been addressed in depth in the revised manuscript.

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.

The sentence has been rewritten to avoid attributing causal links:

“As illustrated, the interannual variability of RH in the region is correlated to changes in the total annual precipitation and the total annual land evapotranspiration in the continental source region.”

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.

The sentence has been removed and the paragraph simplified.

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.

Sentence has been removed.

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

Replaced

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

The sentence has been removed in the revised manuscript.

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?

It could be a possible explanation. In any case, we have included it as possible hypothesis for this pattern.

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.

This issue has been stressed in the revised manuscript.

P19L474: "Island" -> "Continent"

Replaced

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 Ì ËŻAs 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).

Yes, it is consistent and stated in the revised manuscript.

Sect.4: In my view, a lot of the material presented here belongs rather into the introduc-

tion (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.

Following the suggestions raised by reviewer #1, the revised manuscript includes a new section of "Conclusion" to highlight the main findings of the research.

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

Removed statement in the revised manuscript.

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).

Cited in the revised manuscript.

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.

See above. This has been addressed in depth in the revised manuscript. In any case, although the FLEXPART methodology could overestimate continental contribution as the reviewer suggests, we would like to stress that major analysis are not based on the total moisture provided by different sources obtained by means of the FLEXPART scheme but using the GLEAM data set, which is independent. FLEXPART was mostly used to identify the surface area corresponding to the main oceanic and continental moisture sources and we consider that the possible surface overestimation or underestimation has a minor impact here.

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

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.

Thanks.

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?

Really this figure does not provide a different angle in comparison to the analysis in the specific regions but we think that it is relevant to summarize the findings obtained in the different regions to provide if there is a possible spatial relationship.

References ### Goessling, H.F. and Reick, C.H., 2011. What do moisture recycling estimates tell us? Exploring the extreme case of non-evaporating continents. Hydrology and Earth System Sciences, 15, pp.3217-3235. doi:10.5194/hess-15-3217-2011

Goessling, H.F. and Reick, C.H., 2013. On the" well-mixed" assumption and numerical 2-D tracing of atmospheric moisture. Atmospheric Chemistry and Physics, 13, pp.5567-5585. doi:10.5194/acp-13-5567-2013

Held, I.M. and Soden, B.J., 2006. Robust responses of the hydrological cycle to global warming. Journal of Climate, 19(21), pp.5686-5699. doi:10.1175/JCLI3990.1

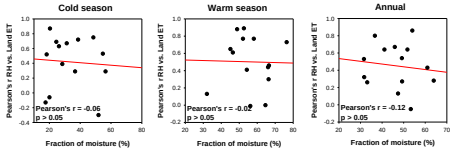
Finally, we would like to thank Dr. Goessling for his effort on reviewing our manuscript and the constructive inputs suggested to improve it.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2017-43, 2017.

Region	Cold season	Warm season	Annual
West Europe (region 1)	18.3	45.1	31.6
Scandinavia (region 2)	17.5	46.5	33.1
Central-East Europe (region 3)	24.5	56.2	41.1
South-East Europe and Turkey (region 4)	26.2	66.5	49.2
West Sahel (region 5)	48.6	58.1	54.0
India (region 6)	56.5	48.8	51.8
East China (region 7)	51.9	54.2	53.3
North East Asia (region 8)	36.7	76.4	64.0
La Plata (region 9)	38.7	52.1	45.9
Canada (region 10)	20.1	66.3	47.4
Central USA (region 11)	28.4	64.6	49.3
West North America (region 12)	20.4	52.6	36.7
Amazonian (region 13)	31.3	32.0	31.7
East Sahel (region 14)	54.7	66.1	61.0

Supplementary Table 1: Percentage of moisture coming from the continental source in each one of the fourteen analyzed regions obtained from the FLEXPART model

Fig. 1.



Suppl. Figure 47: Relationship between the average fraction of moisture coming from continental areas and the strength of the agreement between RH and Land Evapotranspiration obtained in each region (by means of the Pearson's r coefficient between RH and Land Evapotranspiration).

Fig. 2.

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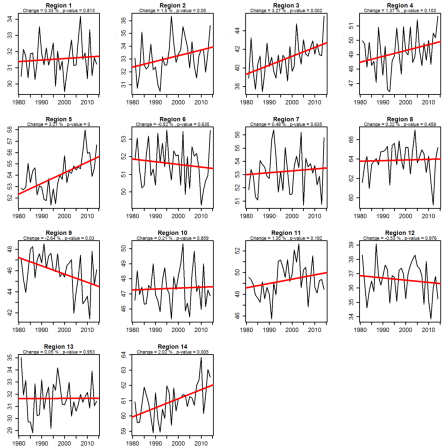

Suppl. Figure 48: Evolution of the percentage of continentally-originated humidity for each of the fourteen regions at the annual scale. The magnitude of change (in %) and statistical significance of the trend is indicated.

Fig. 3.

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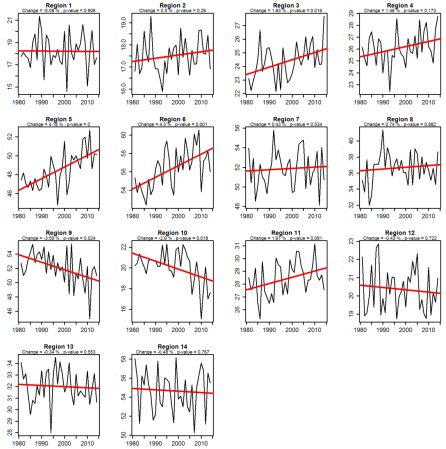

Suppl. Figure 49: As in Suppl. Figure 48, but during the cold season.

Fig. 4.

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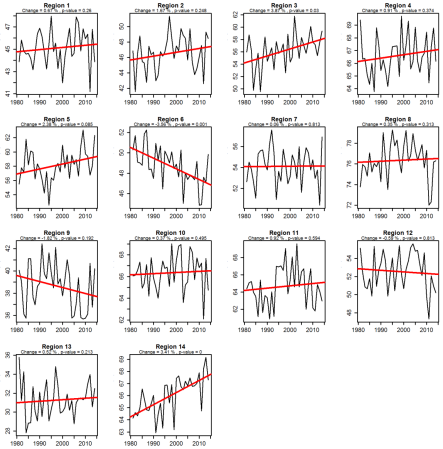

Suppl. Figure 49: As in Suppl. Figure 48, but during the warm season.

Fig. 5.