

Interactive comment on “Completeness of radiosonde humidity observations based on the IGRA” by António P. Ferreira et al.

Anonymous Referee #1

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The manuscript by Ferreira et al. presents an analytical description and a rationalized statistical analysis of the radiosounding data archive available through the NOAA-IGRA initiative. The authors report an extensive and precious description of a huge number of information about the changes occurred in the number, geographical density and type of radiosoundings and payloads used around the globe since the beginning of last century to present for the measurement of atmospheric humidity. The authors focus they study on humidity measurements and they investigate the datasets in a critical way to show if, in term of continuity and coverage, the available data may support climate studies, though they do not assess this aspect on a scientific sound basis. The paper is well written and curated and the provided analysis and it update expected on a biannual basis may support many activities currently ongoing, not ultimately the

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Copernicus Climate Change Service (C3S). I am in favor of the manuscript publication. Nevertheless, I have to provide the authors with major revisions, because to my opinion a few concepts must be clarified and modified in the text of the manuscript, though this does not affect my positive view on the manuscript itself. Below I enclose my general comment and a bunch of minor issues to solve.

General comments. 1. In the manuscript, starting from the abstract, it is mentioned that the IGRA V2 datasets is investigated according to various completeness criteria in order to show if the length of the available data records is sufficient for the purposed to estimate climate variability trends. The authors mainly refer to the temporal continuity of the observation and not the spatial gaps which are anyhow investigated in the statistics. Despite this purpose, the authors clarify in the conclusion that their work can only be supportive of climate studies or works related to the climate homogenization of the time series, and nor they do not discuss on a quantitative basis what is the length of data records required for climate studies neither they refer to past literature to asses this aspect. Assuming that the authors are not interested in the going ahead in the assessment of the effect of gaps in the data records on the estimation of climate signals (which is well beyond the scope of the paper), I'd ask them to change the tone of the manuscript, where needed, to better clarify the scope of the manuscript. 2. The authors make use of IGRA data V2, which is the most updated version of IGRA which embed several improvements compared to the previous data version (V1). Nevertheless, in my personal researches I had a chance to find many bugs in the IGRA V2 where many data present in the V1 are missing and this is not dependent on the extended quality check applied within the IGRA V2. I had also a chance to report this bug to Imke Durré (PI of IGRA). I got similar feedbacks from other EU and US colleagues during discussion at various workshops. The station of Lindenberg in Germany (WMO index=10393), so accurately described in its past history in the manuscript, is one those affected by this issue (at least until a few weeks ago, my last access to IGRA). Therefore, I am wondering whether a comparison between IGRA V1 and V2 has been carried out and if gaps have been found and fixed somehow in the datasets investigated in the manuscript. 3. The

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description of statistical analysis of IGRA V2 data is very extensive way, providing several details and a long description of each figure and table. When reading, I have been very interested by the content, though sometimes the reader may get tired by the way the manuscript is written. In a way similar to the conclusions, I'd suggest to the authors to change the style of their writing and privilege a description in "bullets" to describe the results whenever possible. this will help also to clarify the text itself. For example, for the relative humidity observations, At pag.5, it is reported that the average number of non-standard levels in weather-balloon sounding reports increased from about zero by 1945 to about 30 by 2000, but later in the text it is said that RH observation were already available since the 1930, and again later on (pag. 18) it is stated that RH are becomes more abundant since 1949. Though all of this information are exact the reader may get confused and their comparability and usefulness could be limited if the statical analysis is not described in a more schematic way. 4. The use of the metric present in the Eq.1, "zonal coverage index" at pag 11 is not clear to me. Have been it used in the past and its adde value with respect to other metrics was shown? What's the added value with respect to a station density per 1000 km, for example? Has the ocean surface been excluded from the global surface, given that this can be calculate more clearly for fixed stations? There are many concerns to me on the use of this index, which requires clarification from the authors. Personally, I think that the user can make use of a much simpler index or statistics (a few of these are used later on in the manuscript by the authors themselves) to show if a zonal belt are under and over- represented. This is also depending on the different atmospheric circulation occurring in the different zonal belts, so I am wondering what's the usefulness of adopting this metric. I ask the authors to clarify in the manuscript of the added value due to the use of this metric. 5. The average vertical resolution could be useful information, but to my opinion, thinking about different applications it could be useful to have a statistic of the available level for the different regions of the atmosphere: Planetary Boundary Layer, Free Troposphere, Upper Troposphere/Lower Stratosphere (UT/LS), Upper Stratosphere. This classification may have a stronger

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impact to orienting users' application in the selection of the available data, e.g for trends calculation. 6. Please check the use of the term "error" throughout the manuscript and replace it with "uncertainties" where more appropriated.

Specific comments Pag.2, lines 10-12: the authors could mention that recent reanalysis products, for example ERA5 from ECWMF, will improve the 4 times daily frequency of the products up to hourly.

Pag.2, line 13: replace "not to mention" with "with not negligible"

Pag.2, lines 17-19: uncertainties due to balloon drifting and observation time are considered negligible citing the publication by Kitchen (1989) as well as the radiosonde profile accuracy. To my opinion these cannot be considered minor, and anyhow if minor or not this is depending on the considered application. First of all there more recent papers by Seidel et al. (2011) dealing with radiosonde balloon drifting. Estimates of elapsed time from balloon launch to various pressure levels, due to vertical balloon rise, have median values increasing from about 5 min at 850 hPa to about 1.7 h at 10 hPa, with ranges of about 20% of median values. Observed elapsed times always exceed those estimated using assumed 5 or 6 m/s rise rates. Regarding the data data accuracy, if we are referring to the ensemble of effect which may alter the sensors' optimal measurement conditions, like the solar radiation effect to the effect of a sensor time-lag, these have been better quantified for the more recent radiosonde types and are quite relevant for any kind of climate application (see for example the GRUAN quantification by Dirksen et al. 2014). This is also the reason why many scientific groups have developed homogenized dataset of radiosonde data for climate application. For there reason above, I'd reformulate these line in the manuscript and I'd provide more updated references.

Pag.2, line 26: please replace "errors" with uncertainties and then add also that the radiosonde sensors may have a limited sensitive to ppm water vapor concentrations in the UT/LS as one of the main reason because humidity data above 300hPa are

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unreliable.

Pag.2, line 32: please change “lag” time “time-lag”.

Pag.3, lines 19: “the remainder of this section”.

Pag.3, lines 21: put “available” in between of “levels” and “in radiosonde”

Pag3, line 26: “. . .relevant for the study”. Study of what?

Pag3, line 26: “ please change “indicated” with “reported”.

Pag4, lines 7-10: Please put a reference related to the importance of data continuity for climate studies (trends, annual cycles).

Pag4, Line 10-12: I tend to disagree with the introductory sentence while I like the authors approach in the manuscript; therefore, I think in this sentence you must report which is needed to investigate the simultaneous spatial and time sub-sampling on the data whatever challenging this might be in the practice.

Pag. line 24: pressure is not measured anymore in the most recent radiosonde types (e.g. RS-41 operation since a couple of years); please for completeness you may mention this.

Pag.5, line 17-19: this sentence could be a good opportunity to claim for the importance of having high resolution measurements and, therefore, more levels available in the radiosounding report. This is in line with the high resolution BUFR files already flowing to the Met services from more than 100 stations worldwide.

Pag. 7, line 13: “anything but uniform” can be modified “quite heterogeneous”.

Pag. 7, lines 22-24: this sentence is the “official” IGRA description, but going through the data the authors may realize that among the 2761 stations, many of them are “near-surface” stations and not radiosounding stations. All the reports are empty (-9999) for many stations. These must be clarified and the reported number estimated in a more

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precise way.

Pag. 7, line 16: Is the reported typical average accuracy in the troposphere only related to the most recent radiosonde types? Please clarify.

Pag.11, line 1: please put a descriptive reference for GRUAN, I suggest Bodeker et al., 2016 BAMS.

Pag.11 line 3: It is not true that all of the GRUAN sites are transmitting data to the GTS. A few sites are still working to establish this data flow. This is also connected to the sentence at line 9, reporting the fact that not all GRUAN stations are present within IGRA.

Pag.11, line 6: The added value of GRUAN is not only to provide data the quality of which should be “above the average”, but to provide traceable uncertainties and a fully disclosed data processing described in peer-reviewed literature. Please add more details to show the real added value coming from GRUAN

Pag.11, eq.1: see my general comments above.

Pag.12, line 18: what’s the meaning of the “relevant” in this sentence?

Pag.12, line 19: How did you choose the value of the constant temperature T_0 , please clarify in the text.

Section 2.3.3: this section refers to the quantification of the number of soundings which can be qualified to estimate precipitable water vapour. I am not sure to what extent this section may really confuse the reader. From one side, I think this is redundant with assessment of other indicators in the manuscript and would add value if then the selected radiosoundings according to the criteria reported in this action may really represent soundings for which an accurate estimation of the water vapor is feasible. I think the authors should clarify that the radiosoundings selected according to the presented criteria allow to calculate an estimation of water vapor content which is the closest possible to the true one given the small number of vertical levels available.

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The accuracy of the calculation of precipitable water vapour for these radiosoundings is anyhow affected by many other aspects: presence of clouds affecting the measurement sensors, homogeneity the water vapor field close to the surface, non-linearity of the water vapor variability along the vertical profile and so on.

Pag.16, line 26: Please provide more details to explain the differences in the maximum range covered by the measurement of these parameters. For example, the way wind and humidity have been measured in the past compared to temperature?

Pag. 21, line 1: Did the metadata adhere to any international standards like ISO19115 or WIGOS? Please clarify this aspect.

Pag.22, line 32: "exhibits" instead of the plural.

Finally, a general comment about the plots in the different Figure. They are good and clear but the quality of the figures must be improved for the printing. Supplementary material is quite useful for the reader.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2018-95>, 2018.