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Atmospheric Measurement Techniques Discussions

Interactive comment on "Adaptive neuro fuzzy inference system for profiling of the atmosphere" *by* K. Ramesh et al.

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GENERAL COMMENTS

The paper outlines an enhancement of adaptive neural network techniques for the retrieval of temperature and humidity profiles from ground-based microwave sounders by means of a fuzzy inference system. The paper is mostly well written and the topic fits well into the scope of AMT.

However, the description of the developed ANFIS method is lacking and it would be very difficult to replicate the system based on the given details. The analysis of the retrieval quality is also lacking compared to literature. For example it is difficult to judge the bias and distribution shape of the values at different altitude levels.

SPECIFIC COMMENTS

page 2717: The whole introduction is devoid of references to literature. This needs to be corrected.

page 2718, lines 3-6: The choice of ANN/ANFIS method over others, such as linear regression, optimal estimation or even 1D-VAR is not motivated. For example, the cited paper of Cimini et al. (2006) shows that a rather straightforward regression scheme (M-REG) following Rodgers (2000) produces less biased values over large altitude ranges.

page 2723, Section 3.3: The choice of fitness quantities is peculiar. I am not sure why not simply the cited paper of Cimini et al. (2006) is followed and the bias and standard deviation are plotted (either in absolute or relative form). Especially the bias is relevant for judging the retrieval quality as this determines the gain achievable by averaging. The definition of rather well known diagnostics takes too much room (certainly compared to the terse ANFIS description, which the typical reader of this journal will be much less familiar with). It would be better to simply follow the approach in Cimini et al. and extend the following discussion. Lastly, the achieved vertical resolution is not discussed at all. Further, I am interested how many degrees of freedom the resulting profile contains, if this approach from optimal estimation is also applicable here (if it isn't this would seem to be a disadvantage of this method).

page 2725, Section 4.1: It is unclear why the radiosonde date is available more often at certain altitudes that differ between temperature and humidity. While radiosonde measurements are often compressed to only indicate significant changes, it seems very important to use high resolution data with at least one measurement per target altitude and radiosonde ascent. See for example Schneider et al. "An empirical study on the importance of a speed-dependent Voigt line shape model for tropospheric water vapor profile remote sensing", jqsrt, 2011, (doi:10.1016/j.jqsrt.2010.09.008) for an example on how to treat and compare against high-resolution radiosonde data. If the unavailability of data at certain altitudes is a necessary technical limitation of the available radiosondes or available dataset, it must be discussed. As shown in Schneider et al., the variability of humidity is also much higher than can be reproduced from ground-based measurements, which they compensate for by folding the radiosonde measurements with the averaging kernel of their retrieval system. While the ANFIS almost certainly cannot be easily quantified with a similar matrix, the retrieval has almost certainly also smoothing qualities due to the nature of the instrument sensitivity. It should be discussed how this affects the training and validation.

page 2725, Section 4.2: The "test" profiles are selected to be interspesed in the training data. This choice should be discussed as well as how the system is envisioned to be used in practice. Shall the training data of one year be used to retrieve profiles of succeeding years, or shall the ANFIS system be constantly trained with 12:00 data to reliably retrieve temperature and humidity in the times in between regular radiosonde ascents?

page 2726, line 13: It remains to be shown that spatial inhomogeniety of water vapour is the underlying cause for disagreement between retrieved and measured humidity profiles. All kind of systematic effects might be responsible, in particular rain. For example, the situation around 2011-07-27 has a bad correlation for several consecutive days, whereby the ANFIS is notably better. Is is unlikely that the ANFIS is capable of compensation for horizontal humidity gradients, so horizontal inhomogeniety cannot be the sole explanation. Can horizontal inhomogeniety be shown to be the real cause for an exemplary days using meteorological (model?) data? For example by showing that the days with bad correlation had especially high wind speeds?

page 2727, Section 4.3: The behaviour of SMAPE in Fig 5d seems to indicate a much larger bias for the ANFIS method, which does not seem to be supported by the Fig 5a. As the temperature value in a certain altitude remain in a rather small interval, a SMAPE of 3 percent should indicate a mean difference of about 2K, which cannot be discerned in Fig. 5a. A plot of the mean difference (bias) would certainly help diagnosing this.

page 2728, Section 4.4: The mean profile of ANFIS shows much less bias than the

ANN one. As the RMSE of both methods is comparable, this would suggest that the variance of ANFIS would be much larger. Lastly the SMAPE plot seemingly contradicts the mean plot, as the ANN model has an obvious and rather significant dry bias at higher altitudes. This needs to be corrected or sufficiently explained.

MINOR/TECHNICAL CORRECTIONS

page 2718, line 21: "The ANN used in this MWR is useful to train vertical profiles..." This sentence doesn't make sense to me. Isn't the ANN trained by vertical profiles?

page 2719, line 10: FIS may incorporate human knowledge, which is one of its main advantages. In what way was human knowledge employed in building rules for the ANFIS?

page 2720, line 9 - 11: I am not sure how to interpret the given vertical resolution figures for the MWR. As I fathomed the MWR to be a passive radiometer, upwards pointing measurements have, per se, no vertical sampling. However, as different channels have different altitude-varying sensitivities to temperature, pressure, and trace gas concentrations, one might deduce vertical profiles of these entities with certain resolutions.

page 2720, line 14 - 15: "These channels were selected based on their sensitivity to the occurrence of thunderstorms over the study site". It is not clear, what makes these channels sensitive to thunderstorms. It is possible to, for example, determine the sensitivity of the measured signal with respect to changes in temperature or humidity in certain altitudes and optimize the channel selection accordingly. Or even better to optimize the information content or degrees of freedom of the retrieved profiles.

page 2720 line 18 - 20: Later on, it is related to missing training data at certain altitudes. You should describe here at what vertical sampling the radiosonde data is given and how that sampling is converted to the sampling (regular 1 km grid) required by the ANFIS model.

page 2721, line 19: “Most of the rule-based prediction models need a few rules to predict”. This sentence seems rather pointless.

page 2721, line 20 - 21: “Since the number of predictors (10) is large, it may produce many dispiriting ANFIS structurs”. The number may be large for ANFIS models, but 10 measurements is not much for many nadir or limb-pointing satellites. Assuming that the measurements are well characterized with respect to accuracy and precision, adding more measurements should usually improve the result.

Further, I do not know what “dispiriting” means in this context.

page 2722, line 14 - 19: This description follows uncomfortably close to Jangs 1993 paper. What is confusing in this context, is that Jangs paper explicitely notes that his example has two input with two membership functions each. This paper has 10 inputs and does not seem to specify how many membership functions are specified for each of the 10 inputs. It is also not clear, if the a_i and b_i factors are defined by humans or optimized by the ANFIS.

page 2722, line 23 (Eq. (3)): Here, the membership function B_i was not defined and it is unclear why the index i runs from 1 to 2, except that it did so in Jangs paper. It would be more helpful here to use the numbers and notions of the concrete ANFIS model described by this paper. As all this may be fully or partly automated by the subtractive fuzzy clustering, at least the initial set of rules should be described concisely. The Fig.2 seems to imply that for each input many (21?) membership functions exists, whereby one membership function of each input is multiplicatively combined with one of all other inputs. It might also be instructive to show or discuss the structure of the reduced models, assuming that the structure of each model is different and somehow interpretable for each altitude level.

page 2723, line 6 (Eq. (5)): The use of f_i here seems to conflict with the use of f_i for observed values in Eq. (7). Also, comparing with Jang (1993) Eq. (22), the w_i on the r.h.s. seems to be lacking a bar.

page 2723, line 10: The meaning of this sentence is unclear. “more” than what? And why are a small number of rules needed?

page 2724, line 8: The definition of e_i should be e_i = f_i - y_i without the absolute value, else the SMAPE could not become negative as depicted later on.

page 2726, line 2: The variable “r” should be introduced in some manner, even though it is frequently used for correlations.

page 2727, line 2: I assume this relates to the profiles retrieved only from the test data.

page 2727, line 10: “MSE” -> “MAE” ?

page 2727, line 24 - 25: A reduction of the RMSE does not necessarily imply a reduction of bias, as the RMSE is the sum of the mean error (bias) squared and the error variance. The SMAPE plot is the closest diagnostic depicting something like a bias and ANFIS seems to be quite bad compared to ANN.

page 2728, line 5: Fig. 6b should show the retreived test data, not the trainings data, which is depicted in Fig. 3.

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