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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical dynamics of equatorial waves in tropical radiosonde wind data" *by* T.-Y. Koh et al.

Anonymous Referee #1

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Summary:

The paper analyses long-term records of raw radiosonde wind data for selected stations on the Malay Peninsula. The authors find a good fit of the wind speed to to Weibull distributions with particular values of the shape parameter k, the maximal wind speed v_max and the scale parameter c. The authors determine vmax as the threshold velocity where at most 1 station report finds winds above vmax + 2knots. Given observed values of k, c is computed from the pdf of the observed winds, which results in a good indication of the climatological winds. The authors give bounds to k, ranging from essentially normally distributed Gaussian behaviour of the vector wind components with equal variance (in the paper Rayleigh distribution with k\approx 2), to the lower bound k=5/3, where the authors attempt a link to the observed energy dissipation rate in the

inertial scale range 10-1000km. A new measure (Shannon entropy) is introduced to further highlight the differences of behaviour in wind speed records (Gaussian vs. non-Gaussian) in the vertical. Particular regions of interest identified in the paper are the boundary layer, the tropical tropopause layer, and the "free" atmosphere. The paper suggests applications to the wider tropics but finds substantial differences except for selected stations in the easterly mean wind zone (called EMZ in the paper). Moreover, the authors make the point that the understanding gained from the dynamical interpretation of the statistics may help regional quality control measures.

Main Comments:

The paper is well written and I like the paper for the analysis of the raw radiosonde data, the statistical measures applied to it and the way the fitted distribution and it's parameters distinguishes different regimes both spatially throughout the tropics and in the vertical (albeit i would have wished that the authors comment more on the significant differences in figure 7).

The application to quality control is less clear. In modern data assimilation systems there are several levels of quality control. I give the example of ECMWF's 4-dvar system. First, data sources may be explicitly blacklisted for their consistently bad performance, i.e. mostly measured by their departure from the first guess background (fg) model. These data sources are still monitored on a monthly basis and may re-enter the analysis system if performance improves. The 4-dvar system routinely checks for innovation departures (obs-fg) larger than a fixed threshold (9-15m/s increasing with height) and subsequently rejects the data. There is a further level of quality check ("a buddy check"), where departures from neighbouring observations are compared and data is rejected if departures are larger than 9-10 m/s. To get a feel, in ERA40 about 5 percent of radiosondes were blacklisted for wind observations, about 1-2 percent failed the (obs-fg) check and about 0.1 percent failed the buddy check. It should be noted that radiosonde wind observations from the stations Phuket and Songkhia were (and still are) blacklisted due to regular occurrences of innovation differences (obs-fg) of more than 10-15 m/s in the troposphere. I am not sure how this has influenced the obtained statistics. Moreover, it is not clear that additional regional quality control as suggested by the authors would eliminate or even detect this problem. Also wouldn't a climatological threshold eliminate extreme events that a data assimilation system would allow as long as the fg model equally represents such event ? Further, for direct comparability ECMWF compares the individual wind components and not wind speed. The global assumption used is that the distribution for each wind component is Gaussian at all vertical levels, and equal variance is assumed for u,v. Indeed the paper may contribute to a better understanding of the non-Gaussian behaviour at specific levels/regions and implies the importance of other influences (extra-tropical/surface/stratosphere), but to me it also confirms by means of the maximum Shannon entropy measure, that Gaussianity of the wind components in the EMZ upper troposphere is a good assumption.

Regarding Section 5:

I don't really see the need for the interpretation regarding the different shallow water layers as this may be seen as controversial and potentially wrong. Instead 5.1 and 5.2 could simply be replaced with a discussion of the results in relation to (Zagar et al., 2005, Balanced tropical data assimilation based on a study of equatorial waves in ECMWF short-range forecast errors), as there it is found that (linear) equatorial waves as described by shallow water theory explain 60-70% of the error variance in the free tropical atmosphere. Zagar et al also find a dominance of equatorial Rossby waves on the variance statistics, as is found in this study. This would also allow to delete appendix A ?!

However, note also that the dynamics of the tropics has somewhat evolved from the Gill-Matsuno pattern and in part based on scale-analysis recent models have arisen that incorporate these, a selection of references i would suggest are

Scale-dependent models for atmospheric flows, R. Klein, Ann. Rev. Fluid Mech. 2010. 42:249-74. Scale analysis for the large-scale tropical atmospheric dynamics, J.-I. Yano

and M. Bonazzola, J. Atmos. Sci., 2009. 66:159-172. Madden-Julian Oscillation, C. Zhang, 2005, Rev. Geophys. 43, RG2003. A nonlinear perspective on the dynamics of the MJO: idealized large-eddy simulations, N. P. Wedi and P. K. Smolarkiewicz, J. Atmos. Sci., 2010. 67:1202-1217.

Section 5.3 is interesting but could be perhaps explained better, such as the meaning of Shannon entropy in relation to the deviation from Gaussianity. I think this section is also timely and justifies the title. For example, Sardeshmukh and Sura, J. Climate 2009 find an intriguing relationship by means of statistical dynamics between higher order moments (skewness and (excess) kurtosis) which essentially all complex non-linear dynamical models appear to follow. Interestingly, this relationship arises from linear models forced by different Gaussian white noises, such that if these are correlated, non-Gaussian probability distributions arise. More statistical analysis of this kind with a focus on tropical regions could help to further elucidate dynamical behaviour. I would like to encourage the authors to include variables such as temperature and moisture in the future.

Regarding section 7:

Perhaps I missed it but i didn't find any mention on the applicability and the appropriateness of the Weibull distribution in the wider tropics. This must be stated, not only the shape factor k (Fig. 7).

Regarding the summary in section 8:

I would suggest to substantially tone down the statistical theory of equatorial waves and concentrate on the to me undisputed merits of this paper, the presentation of raw radiosonde data with a novel statistical representation that potentially allows some inferences on the dynamics of the tropics.

minor comments:

- spelling of author Artstein throughout the document.

- p. 16346, line 23 unclear,what is meant with ensemble of physical systems ?

- p. 16350, line 21, "in this work, it «is» found ...

- as commented above I suggest to remove/substantially revise sections 5.1/5.2

- as commented above I suggest to better clarify the meaning of the formulas and symbols in section 5.3.

- as commented above I suggest to revise section 6 to include references to quality control in modern data assimilation systems.

- as commented above I suggest to show the applicability and the appropriateness of the Weibull distribution on wind speed data in the wider tropics.

- section 8 should be revised accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16345, 2010.