Atmos. Chem. Phys. Discuss., 10, C13020–C13022, 2011
www.atmos-chem-phys-discuss.net/10/C13020/2011/

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 #2

Received and published: 31 January 2011

This is a very interesting paper discussing the statistical properties of wind date from radiosonde measurement in the tropics. The authors are here very ambitious in that they not only analyzes radiosonde data over the Malay Peninsula, but also offers a theoretical discussion of the statistical properties of the wind and proposes a framework for data monitoring.

My primary concern here is that the authors were here too ambitious. Given the limited amount of space, the authors are not able to provided sufficient detail on their work. In particular, while the theoretical section could potentially be very enlightening, lack sufficient depth to allow the reader (or at least this reviewer) to follow the full line of reasoning. As a consequence, it appears more speculative that it should. Given this, my recommendation to the authors would be to split their paper into two separate

manuscripts, one focusing on the data analysis, and the other offering the theoretical analysis. This would give them sufficient room to fully discuss their framework.

Specific issues:

Sections 3 and 4:

The use of the Weibull distribution for the windspeed is sufficiently justified by the fact that it seems to be the standard in the literature. Nevertheless, given the authors interest in establishing a theoretical basis for the use of such distribution, it would be interesting to further evaluate whether the Weibull distribution in indeed the better choice for the representation of the windspeed. As a non-expert reader, a few questions come to mind:

- while the authors have tested the statistical significance of the Weibull distribution, one wonders whether the simpler Rayleigh distribution could also be used, and still results in statistical significance. If this were the case, one could argue that the Rayleigh distribution (and its gaussian underpinning) could not be rejected as a proper distribution.

- The Rayleigh distribution is the distribution of the velocity associated with a gaussian distribution of zero-mean for the wind component. One could assume that the veolocity field (u,v) is actually a multivariate gaussian distribution with a non-zero mean. Such distribution would better reflect the climatological mean distribution. Would such a distribution do a better job at capturing the wind speed distribution than the Weibull?

-It would also be useful to have a discussion of which aspects of the distributions need to be captured. The Weibull and Rayleigh distributions differ mostly in their 'tail', but the authors appear to recommend rejecting extreme wind measurement as dubious. If indeed one cannot trust the extreme windspeed measurement, why should one bother with the distinction between Weibull and Rayleigh distributions?

Section 5:

This section is both very interesting, and very frustrating, as the authors have not provided their readers with enough information to follow their line of arguments.

Section 5.1 and 5.2

- The discussion here alternates between ideas from geostrophic turbulence and tropical wave dynamic. A key problem here lies that most 'geostrophic turbulence' arguments usually applied in the context of midlatitude dynamics (such as the Gage and Nastrom data) involve non-linear wave interactions, while the tropical literature (such as Wheeler and Kiladis) usually considers linear or quasi linear dynamics. As the authors ave themselves pointed out, there is no evidence of a turbulent cascade at the synoptic or larger scale in the tropics.

- Equation (9) is a key part of the argument. I was not able to rederive it given the information provided by the authors. It does seem surprising that the Boltzmann distribution (eqn. 6), which implies a gaussian distribution for the wind vector at each wave number, would results in anything but a gaussian distribution. More information on the derivation of eqn. 9 is needed.

Section 5.3

- The point of this section is unclear. It does seem that the authors rederive the central limit theorem based on information theory. Is there anything else?

- There seems to be an inherent contradiction between section 5.2, in which the Weibull distribution emerge as the sum of the wind over a all horizontal wave number, and section 5.3 in which the Rayleigh distribution arises from summing random wind distribution. In particular, why do the arguments of section 5.3 do not apply to section 5.2?

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