

Atmos. Meas. Tech. Discuss., referee comment RC2  
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## Comment on amt-2021-55

Anonymous Referee #1

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Referee comment on "Atmospheric Optical Turbulence Profile Measurement and Model Improvement over Arid and Semi-arid regions" by Hao Yang et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-55-RC2>, 2021

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The paper presents measurements performed with a self-developed radiosonde during a month-long field campaign to observe high-altitude atmospheric optical turbulence at three sites in northwestern China. Based on these data an improved model for the estimate of the atmosphere refractive index structure constant was developed. The paper is potentially interesting, but there are some aspects, both regarding the analysis of the data and the fitting of the empirical model that should be clarified, before the paper can be suitable for publication.

### Major remarks

1) In the analysis of the data there are some points with which I do not completely agree:

- a) In several parts it is written that the upper part of the troposphere is influenced by the presence of a subtropical jet. Given the latitude of the sites and the season (winter), I think that the high wind speed at these heights is connected with the polar jet stream and not with the subtropical jet stream.
- b) In several parts it is written that the observations highlight the presence of penetrating convection into the stratosphere during wintertime. This is not clear from the data presented and in particular I am honestly very skeptical about the presence of penetrating convection in winter in that part of China. This part should be discussed in detail and comparisons with similar cases should be reported.
- c) Data should be reported with respect to the height above sea level and not with respect to the height above ground level. In this way data can be directly compared.

2) In the analysis of data there are some points that are rather obvious and do not need to be highlighted:

- a) on page 7, line 192 it is written that “the atmospheric pressure was gradually decreasing”. If this sentence refers to the fact that the atmospheric pressure decreases with height (with an exponential profile), this is of course obvious.
- b) it is also obvious that the stratosphere is more stable than the troposphere (page 8, line 242).

3) It is not completely clear to me the significance of the results presented in Fig. 5. In particular, I do not understand why, if I have understood correctly, results presented in Fig. 4 refer to a total of 56 profiles, while the results presented in Fig. 5 refer to 6 profiles only (2 for each site). I would like to see some more details about the procedure followed to fit the data presented in these two figures, to better understand what is the added value of the new formulation proposed and in particular if this can be valid also for other situations both in the same location or in other (at least similar) locations.

4) In Figure 4 it is evident an enhanced vertical variability of  $C_n^2$  in Golmud with respect to the other two sites. On the one hand, this high vertical variability is suspicious since this is an average profile from 19 profiles. On the other hand, if this is a real feature, it should be commented.

### **Minor remarks**

Page 2, line 46: space is missing between “al.” and “(1964)”.

Page 2, line 75: I would not define “long-term” a month-long field campaign.

Page 4, equation 3: it should be  $10^{-6}$  and not  $10^{-5}$ .

Page 4, line 138: space between “h” and “is” is missing.

Page 5, line 147: Hufnagel-Valley model.

Page 5, equation 5: check this formula.

Page 5, lines 169-170: this formula is not for the estimated  $C_n^2$  value.

Page 7, lines 192-193: "the pressure was stable without obvious diurnal changes": what do you mean with diurnal changes? From measurements performed twice a day you cannot appreciate diurnal changes.