

Review of revised paper by Pham et. al. *Estimating the refractivity bias of Formosat-7/COSMIC-II GNSS Radio Occultation in the deep troposphere.*

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The authors have responded carefully to my review and except for some minor edits and clarifications, the paper is acceptable for publication.

### Comments on revised paper

1. The response to my comment 8 (shown below) was not clear. There are two versions of the 1D-Var retrievals of temperature and water vapor from RO data. The original one, developed more than 20 years ago, is designated wetPrf. The newer, and improved version (Wee et al. 2022) is designated wetPf2. I think the authors used the original version, wetPrf, but it is not clear from their response because they use the term wetPrf2, which does not exist (I know, it can be confusing!). In the revised paper (lines 140-141) they say they use wetPf2, but they reference Wee (2018). If they use wetPf2, they should reference Wee et al. (2022). Please say clearly in the revised version whether they used the original version or the new version.

My original comment and the response:

8. Line 194—*what 1D-Var algorithm was used? The original CDAAC wetPrf or the new CDAAC wetPf2 (Wee et al. 2023)? Or some other one?*

*The wet product we used in the study is the 1D-var wetPrf2. The wetPf2 refractivity is derived from the*

*bending angle profile of atmPrf and through a variational regularization of Abel transform (Wee, 2018),*

*instead of the traditional Abel inversion. We have clarified that the T and Q obtained from the 1D-Var analysis of the RO wet products in line180.*

2. The Rocken et al. (1997) paper was inadvertently omitted in the text of the revised paper. The negative N bias was first noted by Rocken et al. (1997), although the reasons for it were not discovered until a few years later. The reference should be mentioned in the text. Perhaps in line 60: “It is known since 1997 that negative biases in refractivity exist in the lower troposphere, especially in the tropics (Rocken et al. 1997)”
3. The recent paper on detection of superrefraction by Sokolovskiy et al. (2024) should be added to the references in line 53.
4. Line 26—delete “tend to”
5. Line 56-replace “are” with “is” and delete “and regression coefficients”
6. Line 68-replace “choose” with “choosing”
7. Lines 297-298-“drier conditions”
8. Line 362-“investigates”
9. I don’t understand the sentence in lines 461-462. Factors such as temporal variations, topography and meteorological effects are implicitly considered in this study, which is

based on statistics of data that include these factors. Possible biases in the ERA5 analysis is one limitation. Fewer data in high latitudes is another. A third is that the statistical results are not perfect, as indicated in the RMSE and other parts of the paper (e.g. Fig. 10). So the results may be useful in a statistical sense and using the polynomials to determine the negative N bias for individual profiles may have errors.

10. Line 471-I think there was only one anonymous reviewer.

#### Reference

Sokolovskiy, S., Z. Zeng, D. Hunt, J.-P. Weiss, J. Braun, W. Schreiner, R. Anthes, Y.-H. Kuo, H. Zhang, D. Lenschow, and T. VanHove, 2024: Detection of super-refraction at the top of the atmospheric boundary layer from COSMIC-2 radio occultations. *J. Atmos. and Ocean Tech.*, **40**, 65-78. <https://doi.org/10.1175/JTECH-D-22-0100.1>