EGU Sphere 2024-2716 Paper – Review

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Summary of the Study

The paper titled *"Atmospheric horizontal gradients measured with eight co-located GNSS stations and a microwave radiometer"* by Tong Ning and Gunnar Elgered presents an investigation of atmospheric horizontal gradients measured by eight co-located GNSS (Global Navigation Satellite System) stations and a microwave radiometer (WVR) at the Onsala Space Observatory in Sweden.

The study includes a comprehensive dataset composed of eight GNSS stations and a WVR, all co-located, and covers a 22-month period. This provides a robust dataset for evaluating atmospheric gradients, including all typical weather conditions that may happen during a year and observed across different GNSS station designs and processing configurations.

The authors investigate the agreement between GNSS-derived atmospheric gradients and those obtained from the WVR, the impact of the setup of constraints on GNSS tropospheric gradient estimation, and the added value of fusing gradients obtained from the 8 GNSS stations to improve correlation with the WVR results by reducing random errors.

The authors also nuanced their finding considering the impact that the Liquid Water Content (LWC) can have on the WVR-based gradient retrieval accuracy and mentioned the WVR data cleaning after a rainy period (rain drop drying), which affect the WVR capability to retrieve accurate Zenith Wet Delay (ZWD) and horizontal gradients, hence the inter-comparison.

The methodology used for the paper is usually quite clearly stated but the paper somehow misses to identify/mention clearly the overall goal(s) targeted by the study (beyond inter-comparing horizontal gradients of two techniques, what do the authors expect to demonstrate? For which application(s)? How results can be useful/(re)used by potential readers? Etc.) and the novelties the authors bring with respect to previous studies or existing literature.

The discussion and interpretation parts are also somehow limited. As an example, the seasonal break down of the results via monthly means is a good start but the authors somehow stopped half-way without valorising their presented results: Indeed, one naturally expect that with (physical) gradients of larger magnitude the correlation between techniques will also rise (i.e., increasing the S/N will reduce the (relative) impact of random errors on the inter-comparisons) but a reader would certainly like to read more on this. E.g., at which threshold of amplitude an estimated gradient will be dominated by a real physical signal/information content, hence if the level of random errors can be "neglected" or if a user can cope with it?

The authors may also have elaborated more on the impact of the two different observation schemes of the WVR (see Figure 3) and the impact of the scheme differences on the gradient inter-comparison. Does it explain part of the bias? Do the authors see no difference because of the time averaging (monthly means)? It hard to believe that on short timespan (let's say 5 to 15

min) there is no impact of the scheme differences on the gradient inter-comparison, hence an impact on some possible applications (e.g., authors mention NWP data assimilation in their conclusion). Unless, the GNSS processing also applies a quite high cut-off angle.

Also, the monthly aggregation underlines the hypothesis that the amplitude of gradients is seasonally dependent, but it is not supported by any literature reference in the paper. While the amplitude of the gradients might have seasonality's, their amplitudes are surely also largely influenced on a weather-event based. Hence, it might have been better to aggregate the results, not by month, but in different bins of the amplitude of the gradients, which would have been physically closer to some classification such as stable/…/moderate/…/severe gradient conditions, independently if these events happen in a certain month or another one. This would enhance and strengthen the author's findings.

Concerning the literature, the paper would also be strengthened if the values obtained by the authors could be compared with other manuscripts. How do these inter-comparison values compare? The use of ERA5 data to estimate the hourly gradients could also add value to the manuscript with a 3rd independent dataset.

To conclude, the paper makes an interesting contribution to understanding atmospheric gradients by examining GNSS and WVR tropospheric gradient datasets. The methodology is sound, and the findings quite straightforward. Addressing the limitations mentioned above would further strengthen the study's implications and relevance and would enhance clarity and interpretability. It is recommended to address these limitations prior to publication. Below, authors will find some additional specific remarks.

Specific remarks:

- Introduction:
  - The sentence "The investigation demonstrates […] general data assimilation enhancements" is not clear. What do you mean? That assimilating also gradients in NWP brings an added value? Rewording would bring clarity.
  - Some additional references can surely be added into the list at line 29. E.g., the COST Action GNSS4SWEC final dissemination report can be used for summarising several of them.
  - Line 29: the "the satellite geometries of GNSS measurements […]" expression is not clear/correct and can be reworded with e.g., something like "the remote sensing of the atmosphere at a given station is improved as more GNSS constellations is added, by increasing the number of simultaneous measurements and their spreading in all directions, hence benefiting to the gradient estimation".
  - Line 31: "Glonass" is usually written "GLONASS" in the literature.
- Datasets section:
  - Line 66: "The input to the data processing" is not correct. The input is raw GNSS observation. You can probably reword with something like "The GNSS data processing uses ionospheric free linear combinations".

    - Line 67: “atmospheric parameters”. Be more specific (ZTD+gradients).
    - Line 71: “Equal weighting of the observations was applied”. Do you mean that no elevation dependent weighting is applied during the data processing?
    - Line 82: You mention that the VMF data server (2024) is using ERA-Interim. Isn’t it not ERA5? The ECMWF web site mention that “ERA-Interim production stopped on 31st August 2019”. If it is ERA5, then you must change ERA-Interim everywhere necessary in the text.
    - Line 90: You mention that part of the WVR time series has been recorded with an elevation angle above 25°. Won’t you expect that it has an influence on the GNSS-WVR gradient bias (unless the cut-off angle in the GNSS data processing is also high, but usually we would use 3°)?
    - Line 110: Not clear to me what you mean by “synchronisation of the datasets”.
    - Line 111: “In addition the pure GNSS datasets” → “In addition to the GNSS-based datasets, we use”.
- Comparison section:
    - Lines 135-136: “The rms differences are [...] standard deviations.” You state it but you don’t interpret. What is your actual finding(s) with that?
    - Title of section 3.2: you can remove the part “for LWC [...] 0.7 mm” for readability.
    - Line 147: missing the symbol “<” before “0.05” and “0.7 mm”.
    - Lines 152-153: “The explanation [...] of 0.1°”. This is not clear to me: do you mean that the recorded elevation angle of a given WVR data has an uncertainty of 0.1° compared to the actual elevation angle? If yes, do you really think it can explain the bias between WVR and GNSS? This is also related to my previous point mentioning that part of the WVR data was recorded with an elevation angle set above 25°. I think this is worth some discussion in the paper.
    - Line 166: “what does “the total gradient from the WVR” means? Doesn’t the WVR provide only the wet gradients? And don’t you remove the hydrostatic gradients from the GNSS total gradients?
    - Lines 173 and following: the fusion procedure is not that easy to understand. It can be clearer.
    - Figure 8: Dates below the graphs can be smaller to enhance readability. Even, they can be at 90° of the horizontal axes.
- Conclusion:
    - Line 203: “for applications with higher [...] errors”. Which applications?
    - Lines 204-206: I can understand that you tried to reference to the Product Requirement Document (PDR) of E-GVAP but it doesn’t include requirements for gradients, only for ZTD/ZWD/IWV. The mentioned 15mm threshold is for ZTD and your paper focus only on gradients.
- Everywhere in the text: Don’t use the expression “GNSS data” or “GNSS measurements” when you refer to e.g., gradients as it is confusing with raw GNSS observations. Please use the exact wording instead. Similar remark can be done with e.g., WVR data.
- How acronyms are cited can be harmonized. E.g., the text mention “GIA (glacial isostatic adjustment)” and “Zenith Total Delays (ZTD)”. Please, choice one way of citing acronyms

and ensure that the first letter of the words is capitalized or not everywhere (according to journal's rule).

- Kierulf et al. Is mentioned in the text with the date 2019 while in the reference list it is 2021.