Review of the *AMT* manuscript:

A feasibility study to Reconstruct Atmospheric Rivers using space and ground-based GNSS observations

Endrit Shehaj, Stephen Leroy, Kerri Cahoy, Juliana Chew, Benedikt Soja

This is an interesting simulation study with useful results. The potential impact is, however, limited due to some choices in the design of the study (see my general comments below). The presentation of the results would merit more attention. I have started to compile a list of specific comments and corrections, but this list is not yet exhaustive.

General comments:

(1) Study design: You correctly state that "Commercial RO companies around the world are now projecting RO sounding densities exceeding 100,000 daily" but nevertheless you only consider up to 60 satellites in your simulations. Already in 2024 the YunYao constellation alone reached 22 RO satellites and the Tianmu constellation 23 satellites. For a feasibility study it would have been desirable to go beyond what is actually already there.

(2) Study design: "we used refractivity profiles in the geopotential height interval 1–10 km above the ground in 1-km intervals to compute and map IWV". Why?
There is very little water vapor at 9 km or at 10 km. But there is a lot of water vapor in the lowest km (which is critically important, when IWV is computed). Why did you not consider this information?

(3) Study design: All results are based on a single AR event in October 2021, therefore the general conclusions should be considered with some care (e.g., the lateral movement of ARs can be quite different from case to case). The proposed combination of space and ground-based observations may work well in the selected study region, with a high density of ground-based GNSS receivers. In other regions this will not be the case.
Furthermore, most of the AR activity is over the ocean, where essentially no ground-based GNSS receivers are available. Why didn't you consider an alternative information source for IWV values, like SSMI/S satellite data?

(4) The study uses "Perfect observations". While this fine as a first approach, the limitations should at least be discussed (in more detail). E.g., when IWV values are computed from actual RO profiles, there is a problem due to (often) missing data in the lowest few hundred meters of the RO profile.

(5) Please follow the AMT guidelines throughout, e.g. "Kursinski, et al., 1997" (line 42) should be written as "Kursinski et al., 1997" (without ","). Use "Leung and Qian" instead of "Leung & Qian" …
Please update the bibliography:
List all author names, "et al." is not appropriate here.
Please use DOIs throughout.
Please check for missing information throughout, e.g. for "Chen, X. et al., 2018.": issue ? page numbers ?

Specific comments and technical corrections:

Line 45: For your study, a specific advantage of the RO method is that it allows to see water vapor below clouds.

Line 46: Since ARs are narrow, you should also discuss the horizontal resolution of RO data.

Line 52: It would be helpful to give approximate numbers for the atmospheric Rossby radius of deformation

Line 60: “ARs can release massive amounts of moisture in the form of precipitation and snowfall” snow is a form of precipitation.

Line 65: “river, (NOAA, 2023).” --> “river (NOAA, 2023).” without “,”

Line 66: “(Zhang, et al., 2018) shows .. ” --> “Zhang, et al. (2018) show “

Line 69: Clicking on this link produces a 404 error (“Bad request”), because the part after “10.1002/” is not transferred.

Line 72: Also a click on this link produces a 404 error.

Line 75: I would suggest to avoid the acronym NWM for “numerical weather model”, but to use NWP model instead.

Line 107: “Present retrieval algorithms for RO fail in the planetary boundary layer” is a bold statement, which is not entirely true. And the formulation “Present retrieval algorithms for RO fail in the planetary boundary layer (PBL) in which ARs evolve due to spherical asymmetry …” is unclear. You don’t mean that “ARs evolve in the PBL due to spherical asymmetry”, right?

Line 109: It is true that super-refraction induces negative biases in retrieved refractivity – if it is present. However, in contrast to what one might assume, RO data can e.g., often be exploited down to the surface right in the center of ARs, where water vapor values are high but gradients are not.

Line 126: “We focus on the Pacific basin”. You just focus on (parts of) the *North* Pacific, right?

Line 134: “at 19:00” Is this local time, or UTC?

Line 142: “the wettest day ever” Could you quantify this, in terms of mm precipitation?

Line 159: “In this section, we our approach” Please add the missing word.

Line 173: you should add “medium Earth orbit” as explanation for “MEO”

Figures 2, 3: “Nb.” Is an uncommon abbreviation for “Number”.

Figure 3: I would suggest to use at least the same font size as in Fig. 2.

Line 262: “This reflects the terrain altitude shown in Figure 6 (bottom subplots).” I don’t see any bottom subplots in Figure 6.

Line 275, equation (2): Do you count the levels from the “top of the atmosphere” downwards, as ECMWF does? Maybe you explain this in a bit more detail. I assume that you used the surface pressure as “pi+1/2” for level 137?

Line 276: “gravitational acceleration” In fact this is the “acceleration due to gravity” (note the different meanings of “gravitation” and “gravity”).

Line 447: “These numbers indicate that information saturates beyond 48 satellites.” This might be true for the IWV field as a whole, bot not when you analyze ARs – which is the main focus of your study. See your own comment: “large residuals seen over the ocean are caused by abrupt horizontal discontinuities in IWV, especially near the AR itself, where large refractivity gradients occur within a few kilometers.” (line 395+).
Therefore, also the conclusion “In this case, a 48 satellites constellation is appropriate to reconstruct the AR structure.” (Line 607) is misleading, since you did not really analyze the structure of the AR.