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Interactive Comment

Interactive comment on “Azimuthal asymmetry in ground-based GPS slant delay observations and their NWP model counterparts” *by* R. Eresmaa et al.

Anonymous Referee #2

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Review of "Azimuthal asymmetry in ground-based GPS slant delay observations and their NWP model counterparts" by R. Eresmaa, H. Jarvinen, S. Niemela, and K. Salonen, submitted for publication to Atmos. Chem. Phys. Discuss., 2007

General comments

The work presented in this paper is of importance for the future of R&D in the field of atmospheric observations using ground-based GPS receivers. This technique already yields observations of the total atmospheric delay at GPS frequencies. These observations are used now in routine operations by at least two European NWP centers. But such observations originating from a given GPS receiving station do not provide any in-

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formation in the horizontal dimension. Yet, it is possible to process ground-based GPS measurements into atmospheric slant delay (SD) observations which are not purely zenithal but possess an azimuth dependence. However, serious doubts have been raised about the usefulness of these observations as regards their actual information content. The present paper attempts to bring an answer to this question. For this purpose, the authors developed ad hoc methodologies and applied them to a dataset of SD observations and their simulated counterparts calculated from a numerical weather prediction (NWP) model.

My general comment is that this paper was very well written, the methodology was seriously thought through, and the work was carefully carried out and so were the resulting interpretations. There is one particular point that I think needs to be mentioned explicitly and discussed: the distributions of asymmetricities computed from NWP appear to be non-Gaussian (while those from observations seem to be close to Gaussian). All views or tentative explanations from the authors on this issue will be welcome.

This paper should be accepted for publication. I have formulated below a few remarks that may help get the points further across to the readers. In some instances I have also asked for clarifications on the method and/or results.

Given its contents, the paper length is appropriate and the number of figures and tables is about right.

Detailed comments

Title and Abstract

Overall, the paper discusses extensively one parameter called 'asymmetricity'. I wonder if this should not be reflected in the paper title and abstract.

If so, the title may be shortened into something like "Asymmetricity in/of ground-based GPS slant delay observations". Or, if the authors want to stick to an accurate title: "Assessment of the asymmetricity in ground-based GPS slant delay observations using

NWP".

Section 1: Introduction

The first sentence mentions the 'potentially beneficial' use of ground-based GPS for NWP. I would say this statement is now out-of-date, since at least two NWP centers in Europe now assimilate ground-based GPS observations in their operational data assimilation systems: Meteo France (since 2006 in their global and limited-area NWP data assimilation systems) and the UK Met Office (since 2007 in their limited-area NWP data assimilation system).

Would it be possible to start by giving out an estimate of the horizontal and time scales the ground-based GPS observations aim at? This is to support the discussion suggesting that these observations may be "an attractive development" to help capture the "strong humidity gradients ... typical fingerprints of severe weather".

Section 2: Methodology and used data

It seems that only a subset of the observations was used "due to computational limitations of the NWP model". What were these limitations? – the model domain ? How large was the original SD dataset?

The discussion mentioning previous criticism of the SD processing is most welcome (lines 3-10 page 3183). However, the authors stop short of saying what to do of it, or how to address the problem. At least one answer should be included.

How can the authors be sure that hydrostatic and non-hydrostatic model runs would have produced only "insignificant" differences? I would take this statement out, unless the authors did carry out the computations and checked for themselves the veracity of this point. Besides, it is sufficient to say that the authors did not use a non-hydrostatic model in order to avoid mixing different model physics, which may have confused the results and the subsequent conclusions.

It is my understanding that the sequence of operations 1-4 in page 3184 was applied to both SD observations and their NWP model counterparts. However, the enumeration only mentions "observations". I would suggest clarifying this by calling observations and model counterparts "observables" and use that term in the procedure description.

Last two lines of page 3184, "the determination of SDa does not make use of separate mapping functions": did the authors mean "SDs" ?

The notion of "asymmetricity" is very interesting. I did come across something similar in GPS radio occultation studies ('spherical asymmetry'), but I did not come across a measure of "asymmetricity" before. Is this really new in GPS studies, or have previous authors used it before? If so, a proper reference is needed. Otherwise, it is definitely one important result of the paper (enhance methodology of atmospheric GPS studies by introducing a new concept and showing its applicability) and it deserves being cited in the abstract, title, and conclusion.

A NWP user may be interested in what SD observations can bring on top of ZTD observations. Consequently, the normalization (the denominator in the definition of ra) could have been SDs instead of SD. Besides leading to very slightly larger values of ra, this would tell what kind of extra variability one may expect to capture if one takes into account SDa instead of SDs. Note that this is just remark, i.e. I am not asking to carry out any new computations, plus ra is small enough that the difference may be negligible (it would only matter for large values of ra, but these are apparently always below 1%).

"As a result, the hydrostatic mapping function of Niell (1996) is selected": did the authors mean that they tried out different mapping function and actually found that Niell's 1996 mapping function was giving the best results ?

Section 3: Asymmetricity in the SD observations

The discussion of asymmetricity in the observations in page 3186 is very interesting as it is. One remark is that the discussion stops short of evaluating the actual asym-

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metric versus symmetric information signal in the SD observations. One could perhaps evaluate it by comparing the variability of observed SDs with the variability of observed SDa, possibly normalizing both of them by the SD observation error for the various zenith angles. This could help actually get a better idea of the intrinsic 'asymmetric information content' in SD observations.

There is in the text only a quick allusion to SD observation error – apparently discussed in a separate paper submitted to QJRMS. My impression is that this allusion could be expanded a bit in order to improve clarity.

Did the authors investigate the same statistics as shown here but for each separate GPS receiving station ? For one, depending on the station location one may see higher asymmetricities related to natural atmospheric phenomena (coastal versus inland stations for example), and for two, the GPS equipment and the station antenna configuration may have an impact on the results.

Section 4: Asymmetricity in the NWP model counterparts

I note that all the distributions of asymmetricities have their peaks at zero, which indicates that most of the time there is no asymmetricity in the observations and NWP counterparts datasets. It does not indicate however that each asymmetric event in one direction is compensated by an asymmetric event of the same magnitude but in the other direction. In fact, SD observations show somehow symmetric distributions, while NWP counterparts do not (this is pointed out by the authors). I assume the authors verified that the number of observations in each azimuthal direction was about equally distributed with respect to azimuth. Would it be possible that the problem of the distributions not being symmetric around their peaks be station-dependent? By lumping all the events together, one only sees the result of a combination of effects. The limited station network considered here, furthermore over a small geographical area, may not be sufficient enough to reproduce complete variability so that considering the whole dataset together may not ensure smoothing out the station-location dependen-

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cies (although the authors did pick stations over flat terrain thus reducing the possibility of terrain masking). Yet, this does not explain why observations show ... symmetric asymmetricities while NWP model counterparts do not.

Equally troubling to me (or probably more) is the fact that the observations present gaussian-looking distributions while the distributions of NWP counterparts are more triangular-looking . This needs to be pointed out as it means some source of information in the NWP model is either missing or misused in order to reproduce properly the SD observations. The existence of an asymmetricity-dependent bias in SD calculations from NWP could also have something to do with it (I would probably suggest looking in that direction first). Overall, the non-gaussian NWP asymmetricity counterparts sheds doubt on the idea that the information contained in SD observations can be readily extracted, because data assimilation usually assumes gaussian distributions, while obviously here something is not quite right. Although I do not expect a definitive answer from the authors on this point, I would like to see a mention of that point in the paper. But ultimately, cracking down on this issue and finding (and fixing) its cause may help any work on the assimilation of SD data that will come downstream of this paper.

Section 5: Intercomparison in highly asymmetric cases

The link between the first paragraph and the rest of the section needs to be strenghtened. It is my impression that the first paragraph in section 5 essentially says that the analysis in observation data assimilation is a filtering process where small-scale information comes from the background – and hence (not said explicitly) that one first needs to focus on the observations where the small-scale signal is the strongest.

In this section the authors attempt to identify whether the presence of an asymmetry event in the observations datasets is indeed related with an asymmetry event found in the forecasts. Basically, the authors evaluate here one of the four possible situations: the observations and one (or several) supporting model forecasts see a concurrent asymmetry event. But there are three more possible situations: neither observations nor model forecasts see an asymmetry event, or the observation (the model forecast) sees an asymmetry event, but the model forecast (the observation) does not. Overall, this resembles very much the quantitative precipitation rainfall forecasts evaluation, where one forms 2x2 contingency tables for various rainfall thresholds (here the threshold would be on the asymmetricity). Note that this is just a remark and does not need to be looked at in detail before final paper acceptance.

Constructing random gaussian distributions requires two statistical parameters (mean and standard deviation, the size of the sample being fixed). Where are these two parameters coming from?

Section 6: Conclusions

In the second bullet, I would replace "real atmospheric asymmetry" by "asymmetry seen in NWP model forecasts" (since one does not have complete access to reality but only to some representation of it, NWP forecasts here).

In the last sentence, one obstacle that can be mentioned is the fact that no SD observations are currently available in near real-time. But this is probably not the blocking point yet.

The caption of table 2 may need to explain that "SMF" indicates "the number of supporting model forecasts".

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3179, 2007.