Atmos. Chem. Phys. Discuss., 7, S1983–S1994, 2007
www.atmos-chem-phys-discuss.net/7/S1983/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S1983–S1994, 2007

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

R. Eresmaa et al.

Received and published: 22 May 2007

We would like to thank Referee 2 for the suggestions and comments for improving the manuscript. In particular, we are grateful for pointing out the fact that the asymmetric delay components of the SD model counterparts do not follow a Gaussian distribution. Discussion on this issue is included in the revised manuscript, even though no definitive explanations can be provided at this stage. Our answers to the Detailed comments of the Referee are provided below. The extracts from the Referee's comment are shown in *italics*.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

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 . . .*

We agree that the manuscript makes use of the concept of asymmetricity to such extent that it is reasonable to modify the title and abstract by adopting the concept also there. Title of the revised manuscript is "Asymmetricity of ground-based GPS slant delay data".

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 I would say this statement is now out-of-date, since at least two NWP centers in Europe now assimilate ground-based GPS observations in their operational data assimilation systems . . .*

We agree. This sentence is modified in the revised manuscript to account for the recent advance.

> *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 . . .*

Yes, it is possible and reasonable to include such an estimate in the beginning of the text. These observations are assumed to be most useful for short-range NWP, where

ACPD
7, S1983–S1994, 2007

the forecast lead time is typically of the order of 3–18 hours. Correspondingly, the horizontal scale of the phenomena in question is of the order of a few hundreds of kilometers or smaller. A statement is included in the revised manuscript.

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 number of used GPS receiver stations was reduced from 17 in the original data set to 13 because four receiver stations were located outside the smallest NWP model domain (2.8 km grid spacing) used in the study. Extension of the 2.8 km resolution model domain to cover all receiver stations would have increased the number of grid points by a factor of three. Such an increase was considered too expensive.

The original data set consisted of SD observations at time interval of 30 seconds. The use of all observations would have resulted in too large files and too much computing time. Therefore, the data set was thinned in time such that only observations made at every tenth minute were picked. This means that the number of observations in the original data set was approximately 20 times larger than the number of observations in the thinned data set.

> *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.*

We represent the NWP community and are not working with the GPS data processing.

ACPD

7, S1983–S1994, 2007

Although we have some ideas for potential improvements, we feel it uncomfortable to speak about potential improvements on that area in a scientific journal. Therefore, no answers are provided in the revised manuscript.

> *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.*

We are not sure, but we believe that the differences would be insignificant, due to the reasons listed in the original manuscript. We agree that this statement is speculative. In our opinion, nevertheless, this remark is useful enough to be mentioned in the revised manuscript, too.

> *It is my understanding that the sequence of operations 1–4 on 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.*

It is correct, the procedure is applied to both observations and the model counterparts. The suggestion provided by the Referee is applied in the revised manuscript.

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

ACPD
7, S1983–S1994, 2007

Determination of SDa involves determination of SDs, but not vice versa. Therefore, we prefer the original formulation.

> *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.*

As far as we know, the concept of asymmetricity has not been used earlier in the context of GPS delay observations. The concept is brought up in the title, abstract and conclusions of the revised manuscript.

> *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 a remark . . .*

We do not fully understand the point of defining the asymmetricity by normalizing with SDs instead of SD. No action is taken due to this remark.

> *"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?*

ACPD
7, S1983–S1994, 2007

Yes, we tried out four mapping functions: the hydrostatic and wet mapping functions of Niell (1996), and the hydrostatic and wet mapping functions of Boehm et al. (2006) (Global Mapping Functions, GMF). The hydrostatic mapping functions clearly showed better results than the wet mapping functions, but no difference could be seen between the hydrostatic mapping functions of Niell and GMF. Since the elevation cutoff of the SD data in our data set is 10°, we consider it unlikely that some other mapping function would provide further improvement.

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 asymmetric 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 various zenith angles. This could help actually get a better idea of the intrinsic 'asymmetric information content' in SD observations.*

This is an interesting suggestion, which we had not explored before. We have now studied the variabilities of the symmetric and asymmetric delay components using the following procedure:

1. Calculate the symmetric and asymmetric delay components to all SD observations.
2. Normalize the symmetric and asymmetric delay components by the value of the hydrostatic mapping function of Niell, corresponding to the zenith angle of the observation.

ACPD
7, S1983–S1994, 2007

Interactive Comment

3. Calculate the standard deviations of the normalized data for both the symmetric and asymmetric delay components at zenith angle intervals 0°–5°, 5°–15°, . . . , and 75°–80°.

4. Calculate the ratio of the two standard deviations at each zenith angle interval.

The results show that the standard deviation of the asymmetric delay component is between 6–10% of the standard deviation of the symmetric delay component. The shape of the ratio as a function of satellite zenith angle is such that there is a minimum at the zenith angle of 40°, and the largest ratio occurs at the zenith angle of 80°. In conclusion, the variability of the asymmetric delay component is less than one tenth of the variability of the symmetric delay component.

The discussion on the asymmetricity of the SD observations is modified in the revised manuscript in order to include these recent findings.

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.

We agree. The text is modified by including a slightly more detailed description of SD observation error statistics.

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.

ACPD

7, S1983–S1994, 2007

We have investigated the statistics also for separate GPS receiver stations. Unfortunately, the limited number of data makes it difficult to draw conclusions on zenith angle dependencies at separate receiver stations. We have plotted percentage curves (of the form shown by the dotted line in Figs. 2 and 3 of the original manuscript) for each receiver station. In fact, there are considerable differences between the receiver stations. As the Referee suggests, the differences might arise from either topographical effects or the receiver station equipment differences. We are not able to draw any more specific conclusions on this aspect so far. Discussion on the receiver station dependency is included in the revised manuscript.

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 . . .*

We have had looked at the distributions of separate receiver stations, and we can conclude that the problem of asymmetric distributions appears essentially similar at all receiver stations. The distributions of the SD model counterparts are slightly skewed towards positive asymmetric delays at small zenith angles and more strongly towards

ACPD
7, S1983–S1994, 2007

negative asymmetric delays at large zenith angles. Clearly, the skewness of the distributions depends on zenith angle; therefore, we would suggest that such behaviour results from the Niell mapping function being inaccurate for describing the zenith angle dependency of the SD model counterparts. The Niell mapping function is more consistent with the SD observations, because the SD observation processing has made use of the same mapping function. This explains the fact that the SD observations are symmetrically distributed around zero, while the SD model counterparts are not.

Ideally, the existence of a zenith angle dependent bias should be compensated by applying separate, specifically tuned mapping functions for the observations and the NWP model counterparts. The reason for applying the same mapping function for both the observations and the model counterparts is that we wanted to apply an equivalent and identical treatment to both information sources. The revised manuscript is modified as a result of this discussion.

> *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.*

ACPD

7, S1983–S1994, 2007

Interactive Comment

EGU

We agree with the Referee that the model counterparts do not show Gaussian distributions, while the observations do: a Gaussian distribution would appear as a parabola in Figures 4 and 5 of the original manuscript. We are particularly grateful to the Referee for pointing out this fact, since we had not ourselves paid attention to it. At the moment, we have no explanation for this behaviour. It seems that the model counterparts contain too little asymmetry at small asymmetricities. On the other hand, there seems to be too much asymmetricity in the SD model counterparts at highly asymmetric events. This behaviour can be seen also in Figures 2 and 3 of the original manuscript. This issue is brought into the discussion in the revised manuscript.

Section 5: Intercomparison in highly asymmetric cases

The link between the first paragraph and the rest of the section needs to be strengthened. 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.

We agree that the first paragraph is somewhat out of the context determined by the rest of the section. However, the impression of the Referee is not quite right. Instead, the first paragraph aims at explaining the motivation for the statistical approach taken in the previous section. We recognize that it would be more appropriate to put this explanation in the beginning of section 4. This modification takes place in the revised manuscript.

In this section the authors attempt to identify whether the presence of an asymmetry event in the observations datasets is indeed related with an

ACPD

7, S1983–S1994, 2007

Interactive Comment

asymmetry event found in the forecasts. Basically, the authors evaluate here one of the four possible situations . . .

We consider this remark as a clarification of the assessment reported in the original manuscript. Note that the purpose of this assessment is to evaluate whether high asymmetricity in SD observations is due to real atmospheric asymmetry, or is it solely due to other effects present in the GPS measurement. For this purpose, it is considered sufficient to focus on one of the four possible situations. No action is taken due to this point.

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?

We used parameters 0 and 1 for mean and standard deviation, respectively. Note that these parameters have no effect on the resulting conclusions. What is important is the order at which the random number happen to occur in the data set. The statistical parameters of the random numbers are not meaningful for this study.

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).

It is true that we cannot make conclusions on the real asymmetry by using the information extracted from the NWP model. Nevertheless, we would like to stick in the original

ACPD

7, S1983–S1994, 2007

Interactive Comment

EGU

formulation, which is identical to the second question set up in the Introduction. The answer to the question, provided in the Conclusions, is slightly modified to account for this aspect.

> *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.*

We agree. The last sentence is modified to cover this aspect.

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

This is correct. The caption is modified accordingly.

References

Boehm, J., Niell, A., Tregoning, P. and Schuh, H.: Global Mapping Function (GMF): A new empirical mapping function based on numerical weather model data, Geophys. Res. Lett., 33, L07304, doi:10.1029/2005GL025546, 2006.

Niell, A.: Global mapping functions for the atmosphere delay at radio wavelengths, J. Geophys. Res., 101, 3227–3246, 1996.

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