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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Preliminary signs of the initiation of deep convection by GNSS" *by* H. Brenot et al.

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Summary

The paper "Preliminary signs of the initiation of deep convection by GNSS" by Brenot et al. examines the inclusion of gradients in total zenith delay (ZTD) from a GNSS network over Belgium with the intent of improving near real-time forecasting of convective initiation. Most GNSS/GPS derived precipitable water vapor studies do not consider the effects of anisotropy and assume azimuthal homogeneity in water vapor fields. This work considers the inclusion of anisotropy when observing water vapor fields (ZTD gradients, in this case). I find this study interesting and definitely worth pursuing and I encourage the authors to do so. However, the paper in its present form needs to be entirely reworked. The paper in its present form lacks quantification and error analysis,

contains odd and vague/imprecise language, it requires more explanation with respect to the gradient calculation and needs clearer, more enlightening figures. Finally, given that the ultimate motivation of this study is to improve "real-time" forecasting (nowcasting), it is not clear to me why the authors did not compare (in this study, not a future study) ZTD and gradients calculated with final orbits to those calculated using real-time predicted orbits. If ZTD estimates degrade radically with use of real-time predicted orbits (I suspect they don't degrade radically), then the point of this study is moot. This question should be addressed in order to assess the viability of the approach used in the study.

Major Comments to the Authors

Specifically, there is a lack of quantification throughout the paper and the reader has to depend on vaguely expressed qualitative assessments (see below in "Specific Comments and Technical Issues"). For example, there is no assessment of error with respect to decreasing the observation time interval for calculating ZTD or gradients, that is, how much error is introduced in ZTD and gradient values with 15 minute data intervals as opposed to longer intervals (or smaller intervals, e.g., 5 minutes)? This, I suppose, will become even more important when using real time predicted orbits. And, as mentioned above, the error associated with real time predicted orbits needs to be estimated.

Atmospheric instability is mentioned many times throughout the paper, but it is not quantified in any respect. Can you quantify this, for example, with CAPE or with some other measure of instability? Or, can you quantify the observed gradients in terms of meteorological variables (e.g., temperature, dew point, etc.) so that the reader can better assess the meteorological conditions of your case study. This would give the reader a feel for the strength of the water vapor/temperature gradients along which deep convection is developing (independent of the GNSS-derived ZTD gradients).

Throughout the paper, there is a great deal of odd, imprecise and unclear uses of language that makes the paper difficult to read. Some of the terminology and the variables used are not really common in the atmospheric sciences literature, for example, “digital counts (DC)” and “Neutrosphere”. There are many uses of the word “humidity” that need to be specified; that is, relative, absolute, integral measures, etc. Furthermore, many statements, such as, “But finally clusters were stretched by high altitude air currents (associated with a depression coming from France) and by the impact of the vertical shear of the wind” do not convey much information and are confusing. More examples will be cited below in the section “Specific Comments and Technical Issues”.

The methodology is not entirely transparent, in particular, with respect to the actual calculation of anisotropy in the ZTD. There is a reference to Chen and Herring (1997) and a few others, but how they exactly apply here is not absolutely clear. There should be a bit more detail given, particularly considering that ground-based GNSS/GPS meteorology comprises a minuscule percentage of ACP articles or that the reader who is more interested in the “prediction of convective initiation” aspect of your study may have limited knowledge of GNSS ground-based meteorology. Some of the references meant to provide more/clearer explanations of the meteorology and/or the gradient calculation are written in French or are not in easily accessible references. This necessitates more explanation directly in the text.

The figures with images are too small, in particular, 1, 6 and 10 are difficult to interpret. Perhaps, Figure 5 could include a “difference” figure between the observed ZTD fields.

With regards to the basic science, including anisotropy in ZTD fields appears to be sound. However, I believe a simpler way to relate ZTD measures at individual stations in a network to convective activity exists, without necessarily calculating ZTD gradients. For example, it is not gradients in water vapor, per se, that are responsible for convection or its signal in the data. It is water vapor convergence above the receiver antenna. In this respect, I would suggest just looking at local d (PWV)/dt (d(ZTD)/dt in your case) for each station in the network over the same 15-minute intervals, then, observe the spatial distribution of this variable and how it changes with time over the

entire network. Large, rapid increases in PWV (or ZTD), that is, d (PWV)/dt > 0 indicate water vapor convergence over the antenna while rapid decreases in d (PWV)/dt is indicative of precipitation which decreases column water vapor. This behavior is revealed in your Figure 4, without consideration of the gradients. You could attempt to derive your “H20” criteria, based solely on d(ZTD)/dt. The advantage here is that most scientists, researchers, forecasters who work with GNSS meteorological data, have easier access to ZTD (PWV) than to its gradients.

It may be interesting to do two tests for assessing the value of including the ZTD gradients in predicting convective initiation: 1) analyze the evolution of d (ZTD)/dt at each site and its spatial distribution; and 2) assess the evolution of d (ZTD)/dt including the gradients; this way you can determine the contribution of each variable to the prediction of convective initiation.

Specific Comments and Technical Issues.

Pg. 20352

Line 20. What do you mean by “mean” meteorological observation?

Line 21. What do you mean by “first order” and “second order” in this context?

Pg. 20353

Line 5. “forerunners” could probably be better expressed as “precursors” or “antecedent conditions”

Figure 1. What is the color code for? What are the geographical regions? Are they relevant to the issue at hand? This figure is really difficult to see.

Pg. 20354

Figure 3. Put numbers on the values for cloud top temperature.

Line 9 through 13. This is very vague, meteorologically speaking. What do you mean?

Line 15. Nemeghaire and Brenot (2010) is written in French, so it may be of very limited value to the ACP readership.

Line 17. What is ALADIN? Do you have any citations for a description of this model?

Line 21. This is Ground-Based GNSS (GPS) and it essentially gives the column integrated value of water vapor (i.e., precipitable water vapor). Humidity fields is a bit misleading.

Pg. 20355

Line 15. Brenot (2006). If you have this in English, cite that version first. Line 21. Typically, the atmosphere is assumed azimuthally homogeneous.

Pg. 20356

Line 10 -15. Again, the behavior in d (ZTD)/dt in the figure is essentially d (PWV)/dt associated with the atmospheric component of the hydrological cycle. Prior to precipitation, d (PWV)/dt is positive, water vapor convergence into the column. After precipitation, column water vapor decreases and d (PWV)/dt decreases. This is not real clear, "high values of gradient components (amplitude 2 times over mean amplitude)"

Pg. 20357 Line 7. I don't see a huge improvement in the fields in Figure 5. Maybe a difference figure might help emphasize this improvement. Line 9 (and throughout the paper). Probably "blobs" is a better word than "bubbles". Line 13. What is of interest is the column of water vapor, not the total tropospheric column. That is, you're looking for the "cone of influence" of water vapor above the receiver. In this case, we use a water vapor scale height of around 2.5 km and if we assume an average satellite elevation angle of around 30 degrees, then the cone radius would be about 10km or so.

Pg. 20358

Line 5. ZTD are not "equivalent" to the humidity fields. They are not even equivalent to the PWV fields, but can be used to estimate them.

Pg. 20359

Line 9 and 21. "Digital Counts" is not something that is used extensively in the atmospheric science literature, use "cloud-top temperature" in Kelvins. Line 22. Probably to say a "negative" correlation would be more typical.

Pg. 20360

Line 21. Technically speaking, cloud formation is a "sink" or a loss term for PWV, which is what you observe with ZTD. However, convergence may continue to bring water vapor into the column and hence PWV (or ZTD) continues to increase with cloud formation, or even rain, which is a much larger "sink" term.

Pg. 20362

Line 4. Again, how are you measuring "instability"?

Pg. 20363

Line 21. This is a bit unclear. Humidity can vary, but GNSS/GPS is really providing an "integral value" in all cases.

Pg. 20364

Line 7. The more common variable to consider would be cloud-top temperature. Line 17. Again, it is not contrasts or gradients that are precursors to convection, but water vapor convergence, which, of course, can be associated with fronts and gradients in the water vapor fields. Line 26. This should be verified for this study. Am I mistaken, or are the predicted orbits not available for this event period? Or is there something I am missing? If the predicted orbits are available, then how come you didn't attack this issue?

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