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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Observing three dimensional water vapour using a surface network of GPS receivers" *by* S. de Haan and H. van der Marel

**Anonymous Referee #1**

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**General comments**

This manuscript discusses the use of a very specific methodology to retrieve the three dimensional distribution of atmospheric water vapour through ground-based remote sensing. This methodology is an application of GPS meteorology, and it relies on the use of so-called Slant Water Vapour (SWV) observations that are post-processed from the measurements of a network of GPS receivers. The use of ground-based GPS observations for nowcasting and short-term numerical weather prediction is an actively studied area of research and it is within the scope of Atmospheric Chemistry and Physics.

It is disappointing to find several fatal issues in the experiment design. These issues are of substantial nature and in my opinion they are too severe to be fixed as a part of a major revision. Therefore, my recommendation is that the paper should not be accepted to be published in Atmospheric Chemistry and Physics.

Purely geometrical aspects suggest that the information content of SWV observations becomes increasingly relevant as the horizontal scale of the atmospheric phenomena of interest is decreased. The more traditional approach of GPS meteorology, i.e., the use of vertically Integrated Water Vapour (IWV) only, provides a remarkable potential in analysing and forecasting of atmospheric water vapour in the synoptic scales, and also in mesoscale, depending mostly on the horizontal resolution of the receiver network. What the SWV observations can provide on top of the IWV observations is the specific ability to contain information on azimuthally asymmetric features of atmospheric water vapour. This property allows tomographic retrieval of three-dimensional structures of water vapour, given that both the receiver network and the analysis grid are sufficiently dense in horizontal. However, two aspects make this specific property irrelevant in synoptic scale analysis. First, atmospheric water vapour to appears mostly in the lowest parts of the atmosphere. Second, GPS measurements are affected by a number of processes that make the use of low elevation angles very difficult (see Remark 44 in the Specific comments below). The experiments that are conducted in this manuscript would thus be reasonable, if they were designed to focus on mesoscale analysis with horizontal grid spacings definitely not larger than 10-15 km. Such a grid spacing would easily be achievable in the framework of modern limited are models. Unfortunately, the experiments described in this manuscript are performed in far too coarse grids.

Moreover, the determination of background error covariances for the experiments is not acceptable. The applied method (called observational method, designed by Hollingsworth and Lonnberg in 1986) itself is a satisfactory one, but the details of application in this study cannot be justified in terms of science. The background error



ACPD

8, S7379–S7393, 2008

Interactive Comment

covariances are derived using operational 24-hour forecasts of the ECMWF global model. However, the background fields in the experiments are taken from a 6-hour forecasts of a limited area model. In order to provide an optimal analysis in a statistical sense, the background error covariance matrix, that is given as input to the analysis system, should represent the errors of the background field. In this study, the authors make an implicit assumption that the 6-hour forecast errors of a limited area model can be represented by 24-hour forecast errors derived from a global model. No attempt is given in order to justify this assumption.

Finally, sufficient attention has not been paid on the manuscript style. For instance, several symbols appearing in the Equations are not explained at all (see Remark 9 below). My impression is that the authors assume all of the potential readers to be experts of GPS meteorology. Some concepts need to be explained in more detail in order to be comprehensive to those readers who have not such a long experience in this field; please see the list of Specific comments below. Also, statistical measures like bias, error standard deviation, variance, covariance, and correlation are used in a very confusing manner throughout the manuscript, in particular in Sections 3.2 and 3.3.

## Specific comments

The specific comments are listed as follows:

1. Page 17193, Title: The manuscript does not provide any new aspects on atmospheric observing methods. The new aspects discussed in the manuscript are more related to application of already existing analysis methods to an already existing observing system. I think the Title should be more faithful to the ingredients of the manuscript, i.e., to the application of variational methods in the analysis of atmospheric water vapour.



ACPD

8, S7379–S7393, 2008

2. Page 17194, lines 2–3: The individual sentence here is incorrect in its present form. Vertical information of water vapour is obtained by a number of satellite instruments, that are made use of by several global NWP systems in operational sense. It is true that these satellite instruments are, in most cases, not used in operational regional NWP systems. Please reformulate this sentence.

3. Page 17194, lines 5–6: I guess it would be appropriate to formulate this sentence such that nowcasting is not explicitly mentioned, since the manuscript does not really focus on nowcasting. Please consider writing something like "For purposes of numerical weather prediction beyond the synoptic scales, these observations are . . . ".

4. Page 17194, lines 16–23: Results of the experiment run with real SWV data are discussed in an imbalanced way. It seems that only biases are discussed. Given that the manuscript pays attention to both bias and standard deviation of analysis difference, I think the standard deviations should be discussed in the Abstract as well. Moreover, the manuscript text does not provide sufficient evidence in order to justify mentioning the conclusion "the used network is too sparse to detect water vapour inversions" in the abstract (see Remark 56 below). Please remove the last sentence of the abstract.

5. Page 17195, lines 27–28: I guess standard deviation here refers to the error standard deviation, not the standard deviation of IWV itself? Please be more specific. Moreover, it would be more appropriate to write "timeseries of integrated water vapour". What it comes to the convention applied in the manuscript, the tropospheric delay (with the dimension of length) seems to be treated as an intermediate parameter only.

6. Page 17195, line 29: Is the WMO requirement that the error standard deviation should be equal or less than 5% of IWV? In that case the requirement is met if IWV is greater or equal to 40 $kgm^{-2}$. It would be nice to know how often such



ACPD

8, S7379–S7393, 2008

Interactive Comment

high values of IWV are observed at the regions relevant to this study. Probably more often in the summer than in the winter?

7. Page 17197, line 2: “measures the delay”. Wouldn’t it be better to use the concept of “carrier phase shift” or “pseudorange” instead of “delay” here? Since it is not the tropospheric delay that is actually measured by the GPS receiver, it is confusing to use the word “delay” in this context.

8. Page 17197, line 6: The ionospheric delay is eliminated to the first approximation only. Second- and higher-order terms of the Taylor series expansion are not eliminated. Of course, this is usually a very good approximation and the ionospheric delay thus is not likely to significantly alter the error budget of GPS meteorology. However, it would be nice to see this aspect mentioned in the manuscript, since not all of the potential readers of the paper are experts of GPS meteorology.

9. Page 17197, Equations (1)–(4): All symbols appearing in the equations should be explained. I cannot easily find explanation for the following symbols: $s$, $R_d$, $R_v$, and $z$. Moreover, $\beta$ is defined one page later. For those readers who are familiar with these concepts this is not so crucial, but not all readers are.

10. Page 17197, line 14: Definition given to $G$ as the geometric distance is inaccurate (unless $s$ is the geometric ray path and not the real refracted ray path; in this case Eq. (1) is inaccurate). The most commonly used notation implies that $s$ is the curved (real) ray path, the geometric ray path is denoted by $g$ and the geometric distance is $G=\int_g dg$.

11. Page 17197, Eq. (2): What is the accuracy of this approximation? Moreover, is this approximation of refractivity valid at all parts of the electromagnetic spectrum? At what parts of the electromagnetic spectrum does the GPS operate?

12. Page 17197, Eq. (4): this definition of STD differs from Eq. (6) given one page later. I suggest replacing the STD in either one of these equations by some other



ACPD

8, S7379–S7393, 2008

Interactive Comment

acronym, since obviously the STD of Eq. (4) refers to something different than the STD of Eq. (6).

13. Page 17198, line 11: How big is the error that arises due to approximating the $T_m$ as a linear function of surface temperature?

14. Page 17198, line 14: Bernese software is used with “final orbits”. What do you mean by the concept of final orbits? Do you, perhaps, mean the IGS product that provides post-processed estimates for the GPS satellite orbits with a latency of the order of ten days? If this is the case, please include discussion on the expected impact of using orbit information that is less accurate than the final orbits. I wonder if the WMO requirement on the IWV observation accuracy (see Remark 6) would still be met as often as in the case of using these most accurate orbits?

15. Page 17198, lines 20–21: Please be more specific here. What kind of elevation dependency do you use in the weighting? What do you mean by the proper modelling of the correlations?

16. Page 17198, line 25: On which basis has the decision to use the Niell mapping functions been made?

17. Page 17200, lines 1–3: It is stated that the effect of the spreading of offsets, that is described by Elosegui and Davis (2003), will be reduced when the STD is averaged over time intervals of several minutes. I wonder if there is some evidence available to support this statement? Intuitively, I would not expect this kind of reduction to take place, unless the time scales of the offsets themselves are smaller than a few minutes.

18. Page 17200, lines 15–16: It is probably a valid assumption that the synoptic observation errors at different stations are not correlated with each other. However,



ACPD

8, S7379–S7393, 2008

SWV observations corresponding to the same receiver station make use of exactly the same synoptic observations. Therefore, it is likely that the observation errors of SWV at single receiver stations are correlated. Is this correlation taken into account in the study? If not, the grounds for neglecting them need to be discussed.

19. Page 17200, line 20: I would suggest to replace "parameter" by "coordinate".

20. Pages 17200–17201, Eq. (9) and below: please provide an explanation to $N$. Earlier in the manuscript $N$ was used to denote the refractivity, but I guess this is not the case here.

21. Page 17201, line 14: Do you mean "... $p_n$ is the horizontal position"?

22. Page 17201, line 16: Is $\beta(p_n)$ different from $\beta$ that appears earlier in the manuscript, e.g. in Eq.(4)? If yes, how do you determine $\beta(p_n)$?

23. Page 17201, line 21: Do you mean "... the vertical profile $x_{ijn}$"?

24. Page 17201, the last paragraph on the page: How often do you need to apply the algorithm that ensures non-negative elements in the analysis?

    The applied algorithm is not exactly satisfactory, since it forces the analysis away from the observations and thus violates the optimal tomographic solution that is provided by the 3D-Var analysis. I would prefer applying the penalty function approach, even though this would destroy the linearity. The drawback of this is that $J$ cannot be minimized analytically, but there are efficient numerical methods that would still allow the minimization.

25. Page 17202, line 2: I would suggest to replace "vectors" by "elements", if this is in line with what you want to say here.



**ACPD**

8, S7379–S7393, 2008

26. Page 17202, lines 7–11: I agree with the authors that it is in certain circumstances possible to determine the background error covariances on the basis of the differences between the background state and observations; this is exactly what is done in the method of Hollingsworth and Lonnberg (1986). However, I cannot see how observation error covariances could be determined based on the difference between the observations and the model, unless there is some additional data available. This aspect needs to be clarified.

27. Page 17202, line 22: I would say "$f$ decreases with increasing difference between $h_1$ and $h_2$".

28. Page 17203, line 1: Do you mean "...the vertical background error covariance ...".

29. Page 17203, lines 8–10: The reasoning why the observation error covariances are assumed small compared to background error covariances does not really convince. How does the integration over a height of 1 km affect the fact that each profile is measured with the same equipment? Observation errors at a height of, say, 4.5 km are still correlated with those at a height of, say, 1.5 km. Could you please expand the logic behind this assumption?

30. Page 17203, line 24: Would it be possible to include a few references to works that use exponential decay of covariance in NWP? If not, please explain why you have ended up in this conclusion.

31. Pages 17203–17204: Please give more details of the applied fitting procedure. I would guess it was a least-squares fit with a uniform weighting, but it shouldn't be left as a reader's job to guess these kind of details.

32. Page 17204. line 2: Do you mean "the obtained coefficients are given ..."? Moreover, in which units are the coefficients determined? In meters or kilometers perhaps?



ACPD

8, S7379–S7393, 2008

Interactive Comment

33. Page 17204, line 3: This subsection title is not really descriptive of the text, since slant observation error covariances are not discussed at all. The least you can do for this is to replace “covariances” by “statistics” in the subsection title. However, I would consider it even better to say, e.g., “SWV observation error distribution”.

34. Page 17204, lines 11–12: I would suggest writing “...EUSK have a skew distribution due to antenna problems that have been pointed out by van der Marel and Gündlich (2006).”

35. Page 17204, line 13: Do you mean “...the mean difference between ...”?

36. Page 17204, line 20: What do you mean by “systematic observation error correlations”? How does this concept differ from the usual observation error correlations?

37. Page 17205, line 3: What is the lead time of those ECMWF forecasts that are used here?

38. Page 17205, lines 6–7: The statement that ZTD observation error correlations are independent of location conflicts with what is currently known based on earlier studies. See, for instance, the works of Jarlemark et al. (2001), in Phys. Chem. Earth, vol. 26(6-8), pp. 451–456, or Eresmaa and Järvinen (2005), in Tellus A, vol. 57(2), pp. 194–203. Do you have some evidence to support this statement? This is a major issue, since the validity of this hypothesis affects the relevances of both the background and the observation error matrices. 3D-Var is an analysis method that takes statistical properties of different information sources into account, and if these statistical properties are incorrectly tuned, the analysis will not be the optimal one.

39. Section 3, in general: Do you apply some kind of screening or data quality control to the SWV observations, such that those observations that are affected by



**ACPD**

8, S7379–S7393, 2008

Interactive Comment

gross errors could be left out from the analysis? Such a quality control is usually implemented as a part of the analysis system. On the basis of Fig. 5 it would make sense to apply one in this study also.

40. Page 17206, line 21: The nature run originates from a relatively old run with the operational ECMWF NWP model. Why have you chosen to use this specific model run as the truth? Newer NWP model runs would allow considerably denser model grids. This would, in turn, allow drawing more meaningful conclusions in the context of SWV observations. I consider the poor horizontal NWP model resolution as the most severe weakness in this study.

    Moreover, it turns out later (in Conclusions) that the nature run corresponds to winter in the northern hemisphere. Have you considered to use a nature run that corresponds to summer? In summer, the amount of water vapour in the atmosphere is significantly higher, which means that the SWV observations are relatively more accurate than in winter, given that the absolute accuracy is the same.

41. Page 17206, line 24: What do you mean by “realistic errors”? Do you include the realistic errors also in SWV observations, and are these realistic errors correlated with each other?

42. Page 17207, lines 3–11: Did you include artificial errors in the boundary fields as well? I think it would be reasonable to do so, since lateral boundaries constitute a significant error source in real limited area NWP systems.

43. Page 17207, line 19: Is it a correct interpretation, that you perform the simulated data experiment in an analysis grid of 12 grid boxes only? I wonder what size of atmospheric circulations could be explicitly modelled with this kind of a small grid. I think the best you can obtain with this grid is a second- and third-order polynomials in zonal and meridional dimensions, respectively. I am afraid this is

far from being sufficient in order to provide a reasonable analysis of any atmospheric quantity. You should consider increasing the horizontal resolution of your grid, and perhaps also expanding your receiver network towards UK, Germany and France.

44. Page 17207, lines 24–26: I don't get the point of this sentence. In what sense are the ray paths solitary? Let us consider two ray paths that point towards exactly opposite azimuth angles and have elevation angles of 10°. At heights of 1.5, 2.5 and 4.5 km, for instance, these ray paths are separated from each other by 17, 28 and 51 km, respectively. Comparison of these separations with the analysis grid spacing (ca. 110 km) does not really give the impression of solitary ray paths, in my opinion. It is likely that ray paths corresponding to one receiver station sense nearly the same volume elements of your analysis grid. This is in particular true in the lower troposphere, where the most of the water vapour is.

45. Page 17208, lines 6–9: At the lowest model level, the analyzed water vapour seems to be biased with respect to the truth (see Fig. 8; there are more population below the dashed line than above). It seems that there is less water vapour in the analysis than in the truth. I would like to see this mentioned in the text, together with discussion on what is the ultimate reason for this behaviour.

46. Page 17208, lines 24–25: It is true that the standard deviation of SIM-GPS (SWV) is smaller than that of SIM-GPS (IWV). However, it is not a definite signature of the potential of the SWV observations; this can be due to the observation counts (and improper weighting) as well. I wonder if the same effect would result even if the weight of IWV observations was increased by a factor of ten and the SWV observations were excluded from the analysis. Have you tried to exclude the effect of observation counts?

47. Page 17209, line 2: On the basis of what is written earlier on page 17204, I assume the time period that is considered is in 2003. Please confirm.



ACPD

8, S7379–S7393, 2008

Interactive Comment

48. Page 17209, line 3: I guess replacing the word “schemes” by “experiments” would be appropriate here, since the two experiments differ only in terms of updating cycle and background information. “Two different assimilation schemes” would be a relevant expression if one experiment was run with the 3D-Var and the other with the 4D-Var or the Ensemble Kalman Filter, for instance.

49. Page 17210, line 14: What do you mean with the observed bias? How is it possible to observe the bias?

50. Page 17210, line 17: Most often this experiment is called “OBS-GPS-F”, but at this occasion, as well as in the caption to Fig. 11, name “GPS-OBS-F” is used. Please harmonize these notations throughout the manuscript.

51. Page 17211, line 6: It is confusing to use word “bias” in this manner. In statistical mathematics, bias is defined as the expectation of the difference between a data point and the (unknown) true value corresponding to it. Please consider writing “The difference between the bias curves of SWV and IWV is small . . . ”. Please modify the text in a similar fashion on line 9 of page 17211.

52. Page 17212, lines 4–6: It is noted that the standard deviation of NWP analysis is smaller than that of the other fields at the lowest two levels, and this is explained by the fact that the radiosonde observations are made use of in the NWP analysis. This might be a valid explanation, but I wonder why this does not improve the statistics of the NWP analysis on the remaining model levels, i.e., above 1.5 km? On these levels the NWP analysis and NWP background seem to provide equally good statistics.

53. Page 17213, line 5: In order to be precise, “the lowest four levels” is not the correct expression in this context, is it? It seems to me that Fig. 12 includes time series for the first, second, third and fifth model level, if the counting is started from the ground.



ACPD

8, S7379–S7393, 2008

Interactive Comment

54. Page 17213, line 7: Levels 4.5 and 5.5 km are referenced in the text, but Fig. 12 does not include the time series for the level 5.5 km. Please check and correct.

55. Page 17213, lines 18–22: It is found that the standard deviation of OBS-GPS IWV is larger when an hourly persistence is used as a background instead of the NWP forecast. This is explained by the lack of vertical information in the IWV observations. However, exactly the same observations are used in both experiments! Therefore, the offered explanation cannot be accepted. Wouldn't it be more reasonable to explain this difference by the poorer quality of the persistence background as compared with the NWP forecast?

56. Page 17214, lines 15–17. It is stated that water vapour inversions are not determined correctly with the current distribution of GPS sites. This might be the right interpretation, but I am not convinced by the given evidence. Only one case of water vapour inversions is discussed, and that case is discussed very concisely. Moreover, how can you make sure that it is primarily due to the properties of the receiver network that the inversion is not detected? How can you exclude the role of analysis resolution, both in horizontal and vertical, for instance? There should be more discussion on this, and preferably the discussion should be based on more than only one case, if this kind of conclusions are to be drawn.

57. Page 17218, reference MacDonald et al. (2000): For the sake of popularity, availability and the benefits provided by a proper peer-review process, I would suggest replacing this reference by the following: MacDonald, Yuanfu, and Ware, 2002: Diagnosis of three-dimensional water vapor using a GPS network. Mon. Wea. Rev. 130, 386–397.

58. Page 17223: There is a discrepancy between the caption to Fig. 3 and text in Section 3.1 considering the lead time of the ECMWF forecasts that are used for determination of the background error covariance.



**ACPD**

8, S7379–S7393, 2008

Interactive Comment

59. Page 17224, line 3 of the caption to Fig. 4: Do you mean $\rho_w$ error variance, not covariance?

60. Page 17233, caption to Fig. 13: four curves are plotted in each panel of these figures. However, only three curves are explained in the caption.

**Technical corrections**

The technical corrections are listed as follows:

61. Page 17194, line 13: is –> are

62. Page 17195, line 2: posses –> possess

63. Page 17199, line 25: this are –> these are

64. Page 17200, line 13: maybe –> may be

65. Page 17202, line 7: forecasts –> forecast

66. Page 17203, line 6: coarse –> course

67. Page 17203, line 28: is defined –> as defined

68. Page 17207, line 18: left –> right

69. Page 17213, line 3: stars –> triangles

70. Page 17216, lines 7–8: instead –> in addition to

71. Page 17219, reference Undén et al. (2002). Please check that all authors are included in the reference list. Please also check the correct spelling of Norrköping.



ACPD

8, S7379–S7393, 2008

Interactive Comment

72. Page 17224, bottom line of the caption to Fig. 4: is –> are

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17193, 2008.



**ACPD**

8, S7379–S7393, 2008