

## ***Interactive comment on “Noise characteristics in Zenith Total Delay from homogeneously reprocessed GPS time series” by Anna Klos et al.***

### **Anonymous Referee #3**

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Review of manuscript amt-2016-385: “Noise characteristics in Zenith Total Delay from homogeneously reprocessed GPS time series” by A. Klos and co-authors.

This work investigates deterministic and stochastic modelling of reprocessed GPS ZTD time series from a global GPS network composed of 120 stations. The deterministic component of ZTD series has been quite thoroughly analysed in past studies, namely their seasonal and diurnal variations, and this study doesn't bring new light on these features. In contrast, no clear model for the stochastic component of ZTD time series is established yet. A proper modelling of ZTD noise characteristics is however known to be of special importance when subsequent parameters are estimated from the time series such as secular trends. This work is thus timely since long GPS ZTD time series start being used for climate applications. To my opinion, the relevance of this study is to analyse a global GPS network for nearly the full GPS history (1995-2015). However,

in its present form, this work suffers several major methodological deficiencies which make the results highly uncertain and incomplete. The manuscript also lacks clarity and thorough interpretation of the results. A major revision and substantial rewriting are necessary. General comments for the revision are given below while an annotated manuscript is provided with specific comments.

## Major comments

### 1. Inadequate homogenization method

I think there are several problems with the method adopted to detect and correct the offsets in the ZTD time series. First, it is assumed that discontinuities (offsets) detected in coordinate time series are also valid for ZTD time series. Though equipment and processing changes can induce discontinuities in both coordinates and ZTD series, it is not established that they impact them simultaneously in a systematic way. Moreover, the authors also included earthquakes as sources of discontinuity for ZTD. Though this is valid for position, earthquakes have no impact on ZTD estimates unless the station coordinates are tightly constrained during the processing, but this is not the case here. For a proper and unambiguous detection offsets in the ZTD series, it is necessary to analyse the ZTD time series, or more efficiently ZTD difference time series, using e.g. nearby stations or a NWP model as a reference (Vey et al., 2009; Ning et al., 2016).

Second, the estimation of a series of offset parameters simultaneously with an over-all trend parameter (eq. 2) is an ill-posed problem. This is easy to check from the parameter covariance matrix of the deterministic model. Moreover, the estimation of offsets from the absolute ZTD series and not from differences is very likely to amplify the instability and produce unrealistic offset values and trend estimates. The presence of offset values as large as -69 or +50 mm (given in Table S1) or significant changes in the trend estimates when the noise model is changed (as reported in Table 2) confirms this deficiency.

Both major deficiencies need to be corrected before the trend estimates can be given

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any geophysical credit and noise characteristics can be determined accurately.

Note that from their benchmarking of homogenization algorithms for monthly climate data, Venema et al. (2012) concluded on the superiority of relative homogenization algorithms over absolute (without using neighboring stations). Worse, the absolute homogenization algorithm they tested decreased the homogeneity of their simulated data.

Venema et al., Benchmarking homogenization algorithms for monthly data, *Clim. Past*, 8, 89-115, doi:10.5194/cp-8-89-2012, 2012.

## 2. Incomplete deterministic and stochastic models

The authors decide to include 4 periodic components for the seasonal cycle as well as 2 components for the diurnal cycle. This choice is quite arbitrary and poorly justified. Contrary to past studies which focussed only on the interpretation of the periodic parameters or on the overall trends, it is especially important here to include all relevant periodic components that might mix with the noise models. Inspection of the PSD plots provided in the manuscript and in the supplement suggests that higher diurnal harmonics should be included. A more rigorous spectral analysis should be presented to derive the general deterministic model. I would also like to see the RMS residual as an indicator of the amount of signal (or noise) remaining in the series after the deterministic model is fitted. The same remark holds for a preliminary choice of stochastic models. Inspection of the PSD plots would help pre-selecting the proper classes of noise models. For example, one might argue that, contrary to the coordinate time series PSDs, the ZTD time series do not resemble a power law since the spectrum is nearly flat at both high and low frequency ends. On the other hand, although the authors introduce various general stochastic models, such as white noise, power law and ARFIMA, they don't explore sufficient options and combinations from these models. Though the power law model could be excluded a priori, a more exhaustive number of combinations of ARFIMA( $p,d,q$ ) and white noise models should be tested. The de-

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cision of where to stop can be inferred from the inspection of AIC, or BIC, or another criterion. In regard to the objective of determining the optimal noise model, the present work considered too few variants to achieve this goal.

### 3. Relevance and interpretation of results

Section 3.1 present results about seasonal and diurnal cycle of ZTD. Though these are significant components in the temporal variation of ZTD series, the results presented in this study don't add new information compared to past studies using ZTD, ZWD, or IWV (e.g. several publications by Jin et al., Bock et al., Nilsson and Elgered, or Ning et al.). I suggest removing this part from the manuscript but mention the main results in regard to past studies. An option would be to add the fitted parameters of the seasonal and diurnal cycle in the form a text file or a Table in the supplement.

Using supplemental data is a valuable means for offering the interested reader the possibility to cross-check results and performing complementary analyses of the data. However, the discussion of the main manuscript results should not rely on figures and tables in the supplement. Figures necessary to the discussion (e.g. Figure S1 or a stacked PSD) should be presented in the main text.

Section 3.2 should be reorganised and follow a more logical and sequential description of results. First, the performance of a comprehensive set of different noise models should be compared on the basis of BIC and/or other model selection criteria. Global results should be presented (mean over all sites) as well as some typical examples (Table 2). Then, the trend estimates and their uncertainties can be analysed and results from different noise models inter-compared. I think it is necessary to add statistical significance tests on the estimated parameters (trends, offsets, stochastic model parameters). This would help assessing the relevance of the parameters, especially for the trends, and give an additional criterion for selecting the order of noise models. It would also help clarifying the discussion and interpretation.

The paper is lacking an a priori discussion of the origin and nature of noise in the ZTD

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series. I think at least measurement and processing errors as well as atmospheric variability should be mentioned. As for the nature, it would be interesting to make some assumptions based on the cited literature, e.g. measurement as white noise, atmospheric variability as AR(1), and discuss them later when interpreting the results.

On the question of whether analysing ZTD, ZWD, or IWV time series, it seems to me that this work could stay at the ZTD level since the main goal is describing a methodology for determining an optimal noise model in ZTD time series and assessing the impact on trend estimates and uncertainties. Using ZWD or IWV data would certainly allow more insight into the water vapour origin of variability, but this would pose the problem of using accurate ZHD and  $T_m$  data in order not to introduce spurious trends and noise. Though the main conclusions from the ZTD analysis would very likely also apply to ZWD, this cannot be affirmed without being verified. If only ZTD data are analysed, the authors should thus be cautious in their interpretations. The results cannot be interpreted unambiguously in terms of water vapour variability related to atmospheric processes.

Specific to this work is the presentation of results in climate zones following the Köppen-Geiger classification. Regarding the annual and diurnal components, it seems to me that this classification is not relevant (no coherence emerges in the maps or latitude plots). Maybe, this is due to an excessive reduction to only 5 classes? Did the authors consider using the full classification proposed by Peel et al., 2007?

#### 4. Lack of clarity of presentation

In general I found the manuscript rather difficult to read for the following reasons. The model description uses inconsistent notations among equations (2) to (12). All equations should be introduced in the methodology section and not later, e.g. in the results sections (eq. 10 to 13). Some equations are probably not useful (e.g. 3, 7, 8, 10, 11, 12, 13) and some are incomplete (eq. 5, 9).

A lot of descriptions of properties of GPS time series and results from past studies

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concern coordinate time series and are not relevant to ZTD time series (in the introduction, P2L21-25, P3L6-914, in the homogenization section, P5). Conclusions relevant only to position time series should generally not be included. I also noticed that many of the references to past studies of station position series are improperly cited when discussing properties of ZTD series (e.g. P2L10, P3L11, P5L3) or erroneous results/values are cited (e.g. from Jin et al., 2007, 2008, or from Bock et al., 2014).

The same topics/discussions are repeated in several places (e.g. descriptions of GPS processing, homogenization, climate zone classification in the introduction and in the methodology and results sections; results from past studies on noise models P6, P9 and P12) or are interrupted and restarted (mainly the comparison of results for different noise models P10).

Improper wording is used in some places (e.g. when describing atmospheric propagation in the introduction and GPS processing in section 2).

More specific comments on all these points are given in the annotated PDF.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/amt-2016-385/amt-2016-385-RC3-supplement.pdf>

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-385, 2016.

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