Solid Earth Discussions

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Comment on se-2021-136

Anonymous Referee #3

Referee comment on "Common-mode signals and vertical velocities in the greater Alpine area from GNSS data" by Francesco Pintori et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-136-RC3, 2022

The manuscript "Common mode signals and vertical velocities in the great Alpine area from GNSS data" by Francesco Pintori et al. presents how ICA decomposition of GNSS time series in the alpine area allows to separate sources of deformation and then retrieve with a better uncertainty the velocity field in Europe. The authors process the daily GPS observations with GAMIT/GLOBK software, using subnetworks later tied to IGb14 reference frame. The obtained 2010-2020 time series have then been analysed in order to explore the origin of the common modes, and the potential of Independant Component analysis to extract these modes with a more "physical" basis and filter the time series. The ICA method used in the paper is the vbICA, a bayesian multivariate source separation method. The ICA analysis conducted here is performed in two steps, one, with 8 components, allows to extract and correct the trend (the velocity), the other, with detrended GNSS data as input, contains seven components. In parallel, hydrological and atmospheric loading predictions from two institutes are also analysed with vbICA with three components. These three components corresponds mostly to a uniform spatial pattern, an E-W trend and a N-S trend. The GNSS components appear well correlated to the hydrological plus atmospheric loads components, proving the loading origin of these components. A last component is clearly seasonal and presents spatial variation at small wavelength, in phase with temperature variations. The four vbICA components are used to correct the GNSS time series, which allow a new estimation of the velocity, in very good agreement with the first estimation but with a much smaller error estimation. The authors also compare different methods for common mode estimation, the stacking Filtering method, or weighted stacking filtering method to the filtering obtained by an Independant component analysis.

Overall I found the manuscript interesting and worth of publication, as it shows a convincing correspondance between what is referred as "common modes" and the atmospheric and hydrological loading. However, I think that the paper, although well written, is quite hard to follow, with numerous abbreviations, and comparisons which could be better presented and illustrated. I have also a few scientific comments that can be adressed. I suggest a major revision.

Here are my suggestions:

* I find intriguing that the main three components that are discussed here correspond to a uniform pattern, an E-W tilt and a N-S tilt.
These three components correspond to the largest perpendicular spatially correlated signals possible.
(1) Can you change the color scale of all panels of IC1, to show how uniform it really is ? For example GNSS IC1 should be plotted with a
20-32 scale.
(2) For IC2 and IC3, how significantly different from a tilt the components are ?
(3) the loading models appear to predict mainly very long wavelength features, corresponding to the first three components.
Is this true ? Can you show an example of the predicted load-induced displacement map ? The percentage of the variance do the three components is indicated to be > 97%. For atmosphere, I guess pressure variations are large-scale such that the earth response is also at large-scale. But I would have thought that hydrological loading should be more local. Can you comment on that ?

* The seasonal contribution should not be named temperature contribution. This would suggest a thermal contraction effect which is far from being proven. A lot of signals could be seasonal. Unless you prove that there is a strong correlation between the IC4 and temperature beyond the seasonal term (ie at higher frequency) the correlation appears fortuitous. Fig 8 shows that temperature seems to have higher frequency fluctuations not observed in IC4, but it s hard to tell from the figure only.
I suggest to rewrite the paragraphs and sentences related to this seasonal contribution of unknown origin everywhere in text.

* The statistics shown (mean, median, standard deviation) in tables and discussed in text are not well presented. I suggest to move S4
in the main text, it is quite graphical and shows better the agreement in terms of distribution than Tables 1 and 2, that could be moved to supplementary material. lines 289 to 292 could be replaced by a more readable text.

* The part on correlation coefficients is confusing where it should not.
If you consider that your signal is a sum of IC like Xi(x,y)*Ti(t), then we expect to provide the correlation coefficient
between Ti-GNSS and Ti-HYDR for example, or Ti-GNSS and Ti-ATM, and of Xi-GNSS with Xi-HYDR or XI-ATM. Only two values describing the temporal and spatial correlations would be sufficient. Here, it took me time to understand that, because you add Xi_ATM(x,y)*Ti_ATM(t) and Xi_HYDR(x,y)*Ti_HYDR(t), your spatial and temporal correlations stop being independant from each other. This is why I guess you provide ion Fig6 a spatial map of the temporal correlation of the GNSS and HYDR+ATM. Could you please clarify for the reader why you end up with such a plot ?
In fact, if you had made and ICA on (ATM+HYDR) directly, may be you would have obtained a similar result but easier to compare (ie an independent comparison in space and time). The "blue points" on fig. 6 in the middle of the tilt, in opposite phase, have no real significance, as the spatial patterns of ICs do not exactly correspond to each other. I find more significant the peak in the ditribution, of 0.65 for IC2 and of 0.55 for IC3 which are significant numbers although the PSDs of the Ti do not really match.

* Once ATM and HYDR loads are proven to be good estimators of the common modes, why not use them to correct the time series ?
The advantage is that you can then anticipate that possible decadal trends of ATM and HYDR would then be removed from the time series and thus provide a better displacement rate due to tectonics. Here, the trend is first estimated from a first ICA, removed from GNSS time series, and then a new ICA is performed to extract ICs, that will correct the raw GNSS data, before a new trend estimation. How can you be sure that the last estimation will not be "by construction" biased towards the first ? On the other hand line 219-220 of 3.1 suggests that the separation of tectonics trend from other potential non tectonic trends is already done by the first ICA. Can you clarify this point ?

Figures :
ICA figures:
- change color scales of IC1 for all plots to show lateral variations
- temporal vector: normalisation should be made by variance and not by min/max (if I understood correctly) for the reader to visualie the relative amplitude of each term. Min/max can be outliers.

Figure 6: change colorscale to see changes in correlation coefficient for IC1 (the colorscale is completely saturated in the red).
Don't use "Lin" abbreviation but linear
Figure 7: panel b is identical to panel a

Abstract:
First sentence : too complicated. Simplify and clarify
line 10: associated with : modeled from
line 11: processes: drop
line 16-17 : Atmospheric .... gradients: rewrite

Introduction
First sentence: "active geophysical processes on land, ice and atomosphere": ground displacement on atmosphere. Rewrite.
In general : a lot of references are missing on mountain uplift, both observations and mechanisms. Please provide some refs outside Italy.
Id. for lines 68-80

line 117: give principle of CMC Imaging
line 190: pdfs --> PDFs (and elsewhere)
line 192: drop "that"
line 216: a priori any temporal : rewrite
line 389: k=-2 for both noise and flicker : correct text
line 391: avoid + in text
line 506: elastic hydrological load ---> elastic response to hydrological load

* Don't use "lin" abbrevation but replace by linear correlation coefficient.