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.

> Ok, we changed Figures 3, 4, 5.

(2) For IC2 and IC3, how significantly different from a tilt the components are?

> To answer this question we performed a Principal Component Analysis (PCA) on IC2, IC3 and IC4 data: we generated a 545x3 matrix of data where each row is associated with a GNSS stations and the three columns are the corresponding longitude, latitude and spatial response (U). Longitude and latitude have been converted into km to avoid distortions and U has been multiplied by a weighting factor, so that its amplitude has the same order of magnitude of the

longitude. The PCA on those data allows us to estimate how well two PCs, which define a tilted plane (Fig. R1), represent U2, U3 and U4.

The variance explained by the plane associated with the first two PCs is:

- 97.7% for U2;
- 97.0% for U3;
- 83.8% for U4.

This shows that U2 and U3 are both well approximated by a tilt; in fact, the percentage of explained variance is very similar and larger than IC4, which does not have any tilt features.

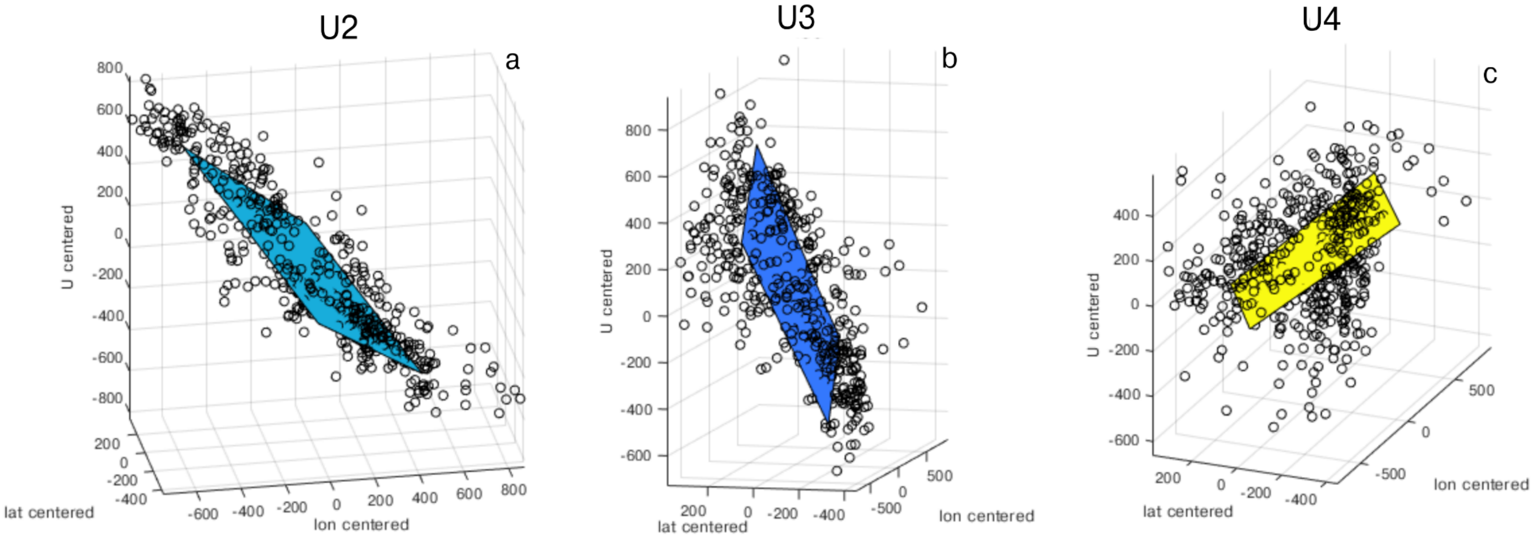

Figure R1: Representation of the tilted planes defined by PC1 and PC2 used to fit U2 (a); U3 (b); U4 (c).

It is worth noting that this result does not depend on how the algorithm works: the relative position of the sites is never taken into account during the analysis.

The presence of N-S and E-W gradients in the ICs of GNSS, NTAL, HYDL and precipitation data is caused by their link to a common, meteo-climatic, source. In fact, precipitation, atmospheric and hydrological loading depend on the climatic conditions, which are spatially and temporally variable. Besides IC1, which is a spatially uniform signal explaining more than the 90% of the total variance in either NTAL or HYDL decomposition, IC2 and IC3 probably reveal the spatio-temporal features of the weather regimes that cause atmospheric and hydrological loading on the surface: the Atlantic Ridge and the North Atlantic Oscillation. In section 5.2 we added the following part:

"It is then likely that weather regimes like the NAO and the Atlantic Ridge influence both NTAL and HYDL, which is mainly forced by precipitation, so that the spatial patterns of the ICs associated with atmospheric and hydrological loading are the same of NAO (N-S) and Atlantic Ridge (E-W)."

(3) the loading models appear to predict mainly very long wavelength features, corresponding to the first three components. Is this true?

Yes, it is true. Since the loading models are global, evaluated over a grid with a spatial resolution of 0.5°, they do not have a great spatial resolution. It follows that it is easier to observe long wavelength features instead of the local ones.

Can you show an example of the predicted load-induced displacement map?

Since the displacements associated with both HYDL+NTAL are not the same over the study area, we cannot show a load-induced displacement map. Nonetheless, we can compute the displacement due to HYDL+NTAL models in some specific GNSS sites. For example, in Figure 7 we compare, at two GNSS sites, the displacements associated with the GNSS_ICs and with the HYDL+NTAL_ICs.

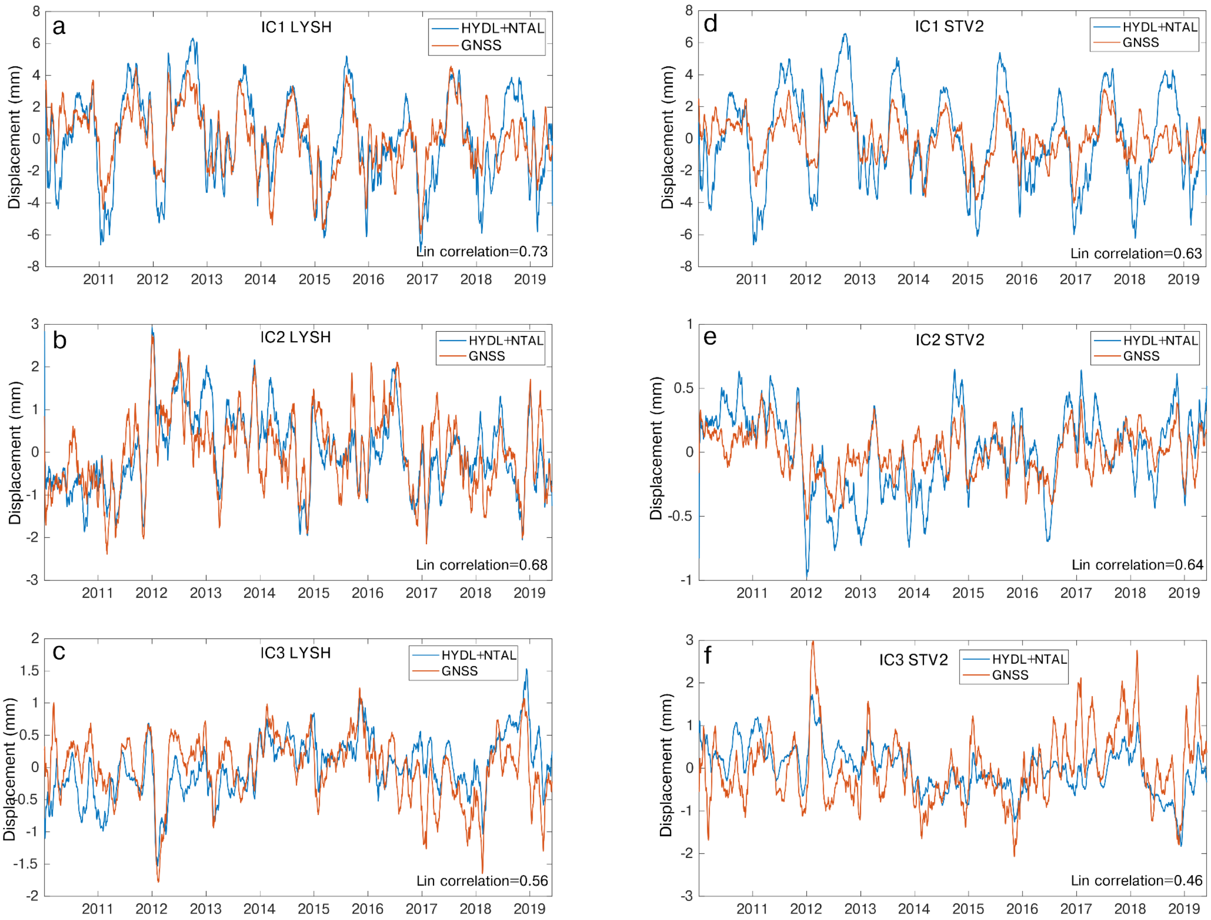

Figure 7: Comparison, at the LYSH (Lon: 18.45°; Lat: 49.55°) site, between the displacements associated with: a) GNSS_IC1 and NTAL+HYDL_IC1; b) GNSS_IC2 and NTAL+HYDL_IC2; c) GNSS_IC3 and NTAL+HYDL_IC3. d), e), f) are the same as a), b), c), respectively, for the STV2 (Lon: 6.11°; Lat: 44.57°) site. A 30-days moving average filter is applied to better visualize the data.

Furthermore, in Figure R2 we show the results of the ICA decomposition of the displacements associated with the combined contribution of atmospheric and hydrological loading (HYDL+NTAL), as you also suggest in a comment below, and in Figure R3 the Lin correlation coefficients between: a) GNSS-IC1 and NTAL+HYDL_IC1; b) GNSS_IC2 and NTAL+HYDL_IC2; c) GNSS-IC3 and NTAL+HYDL_IC3.

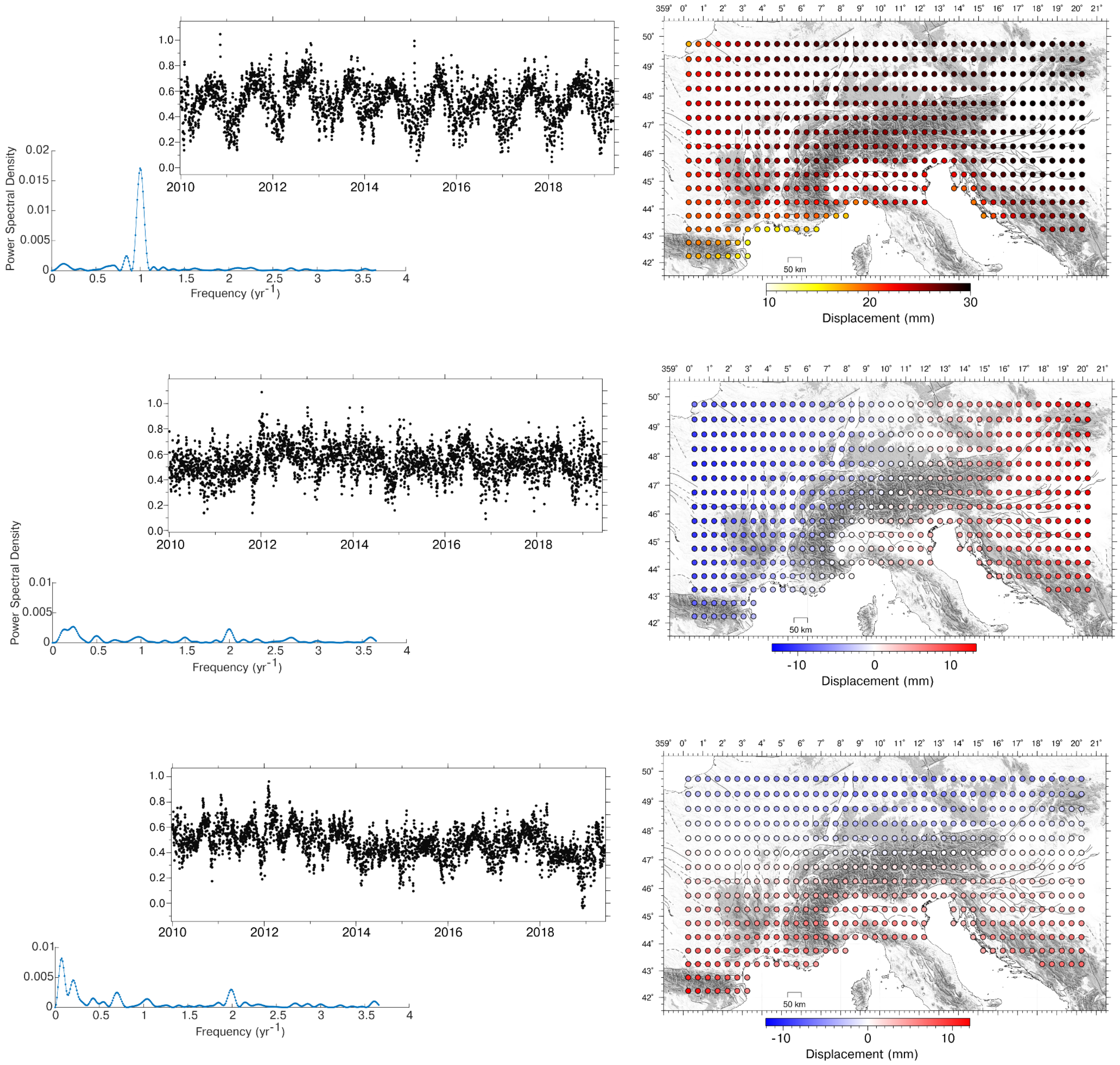

Figure R2: ICA decomposition, using 3 components, of the displacements associated with the combined contribution of atmospheric and hydrological loading (HYDL+NTAL).

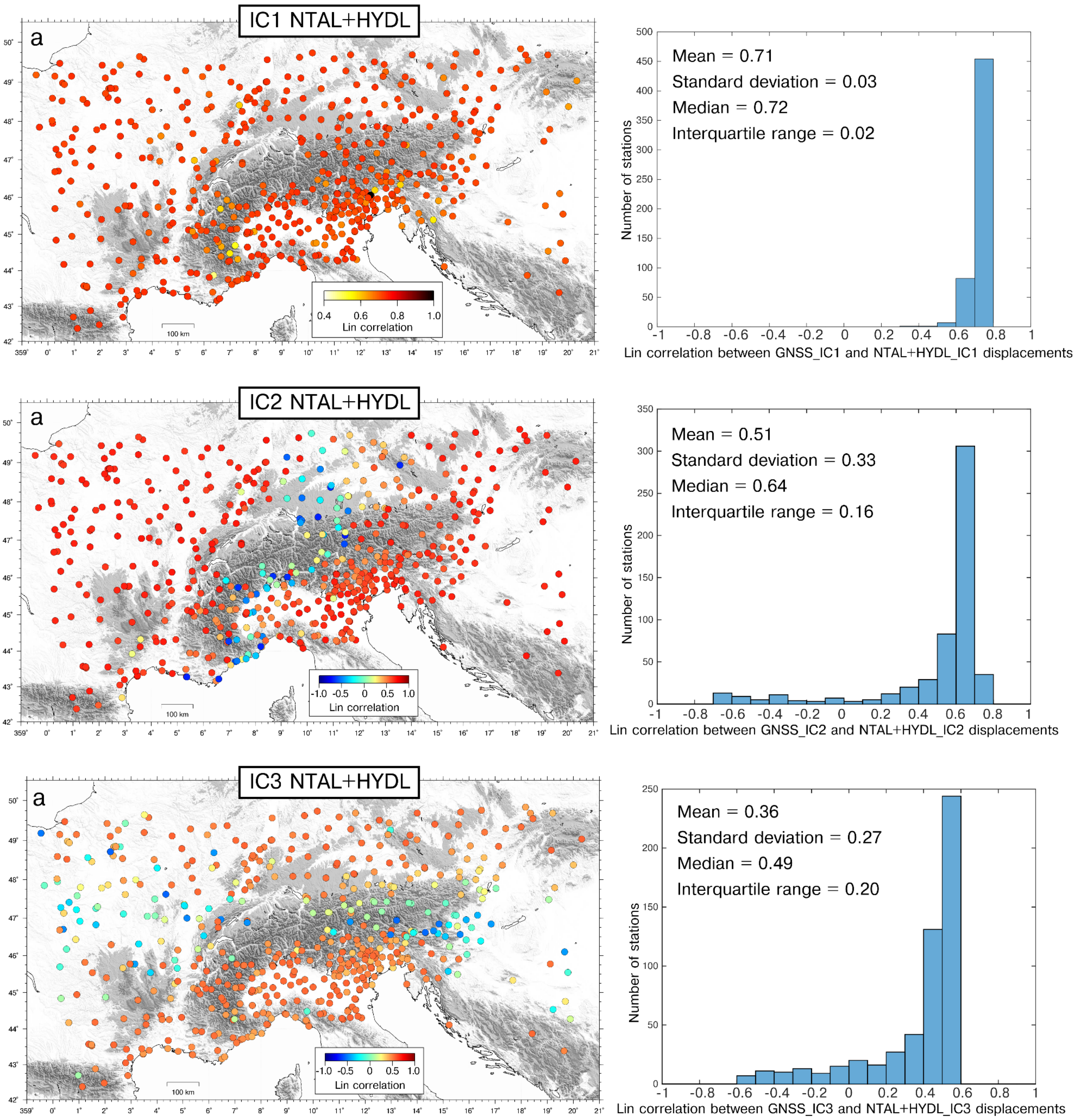

Figure R3: Using the results of the ICA decomposition on the displacements associated with the combined contribution of atmospheric and hydrological loading (HYDL+NTAL) represented in the figure above (Fig. R2), we show the Lin correlation coefficients between: a) GNSS-IC1 and NTAL+HYDL_IC1; b) GNSS_IC2 and NTAL+HYDL_IC2; c) GNSS-IC3 and NTAL+HYDL_IC3. Histograms of the correlation coefficients are also reported.

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 ?

We do agree that hydrological loading is more sensitive to local processes than atmospheric pressure. Nonetheless, we use the results of the global models to estimate the hydrological loading, even though we are aware that some local effects might not be captured. In fact, considering the extension of the study area, it is very complicated to take into account the local features needed to estimate the hydrological loading with a better precision than the one provided by the global models.

*** 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.

We agree that the conclusions on IC4 are too strong. We have modified the abstract, changing a sentence that incorrectly let the reader suppose that temperature might directly cause the displacement associated with IC4.
In fact, it is more correct to state that the displacements could be caused by processes correlated to temperature, which are discussed in section 5.2, than caused by temperature itself.
The lines 504-516 are also updated, discussing some hypothesis about the correlation between temperature and the displacement signal associated with IC4:
"Air temperature increase can induce both positive and negative vertical displacements. In the alpine valleys the water content increases as the temperature increases because of the snow and ice melting. It follows that in those areas the elastic response to hydrological load is higher during summertime than winter, as observed by Capodaglio et al., (2017), so that negative vertical displacements are measured when the temperature increases. Then, it is not surprising that in the alpine valleys the stations affected by large IC4-related displacements move downward as temperature increases. This may be an example of a small-scale hydrological process that is likely badly reproduced by the HYDL displacement dataset, which does not have a spatial resolution fine enough to represent hydrological loading displacements at the scale of the alpine valleys. Other site-dependent processes that can potentially induce uplift during winter are the ice formation, and subsequent melting, in the antenna and antenna mount (Koulali and Clarke, 2020) and soil freezing (Beck et al., 2015).
Conversely, positive vertical displacements as the temperature increases can be caused by monument/bedrock thermal expansion and the drying of the soil, because of the reduction of the hydrological load. While HYDL takes into account the drying of the soil, we cannot exclude that some local, unmodeled, environmental conditions can amplify this effect at some sites. This might explain why most of the sites affected by uplift during temperature increases are located in plain areas, like the northern sector of the Paris Basin and in the Po plain, instead of the mountainous ones.
The relation between IC4 and local processes is also suggested by the heterogeneity of this signal in terms of its spatial distribution, sign, amplitude and relevance in explaining the data variance. In fact, while ~50% of the stations have U4<2mm (Fig. S3d) and explain <1% of the data variance, meaning that IC4 is almost unuseful to reproduce the original data, there is a non-negligible number of stations (~10%) explaining >10% of the data variance and with U4>6mm."

*** 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.

Thanks for this suggestion, we move Tables 1 and 2 in the Supplementary and Figure S4 in the main text.

lines 289 to 292 could be replaced by a more readable text.

We changed lines 289-292, simplifying the text:
"*IC2 and IC3 of both NTAL and HYDL show E-W and N-S gradients in the spatial response, respectively, as observed for IC2 and IC3 of the GNSS dataset (Fig. 3b, d). Since the ICs spatial response of the NTAL and HYDL decomposition are very similar, we also consider the sum of the displacement associated with NTAL and HYDL models, which can be considered as "environmental loading": we use the notation NTAL+HYDL_ICn to indicate the sum of the displacement associated with the n-th component of the NTAL and HYDL decomposition. The amplitude of NTAL+HYDL_IC1, NTAL+HYDL_IC2 and NTAL+HYDL_IC3 are only slightly lower than the ones of GNSS_IC1 , GNSS_IC2 and GNSS_IC3, as shown in Fig. S4 (panels g,h,i) and in Table 1a.*"

*** The part on correlation coefficients is confusing where it should not.**

We tried to make this part more clear

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 ?

If we consider the spatial and temporal correlations separately, we could miss some of the information contained in the data. The station by station computation of the Lin correlation between Xi_ATM(x,y)*Ti_ATM(t) (or Xi_HYDR(x,y)*Ti_HYDR(t)) and Xi_GNSS(x,y)*Ti_GNSS(t) allows us to take into account the amplitude of the displacement associated with each station. We would miss this information if we compared only the temporal evolution of the signals, as Ti_ATM(t) (or Ti_HYDR(t)) with Ti_GNSS(t), by computing the Pearson correlation.
In Fig. 6 we add Xi_ATM(x,y)*Ti_ATM(t) and Xi_HYDR(x,y)*Ti_HYDR(t) and compare it, using the Lin correlation coefficient, with Xi_GNSS(x,y)*Ti_GNSS(t). This allows us to associate the first three ICs of the GNSS decomposition, which have CMS features, with the displacement associated with the combined effect of hydrological and atmospheric loading.

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).

Thank you for this suggestion. The results (Fig. R3) are quite similar to what is shown in Fig. 6 and represent a good validation of what is shown in the main text.
We prefer not to add this in the main text because we decided to compute the HYDL+NTAL contribution only when we found that the ICs resulting from their decomposition have the same spatial patterns of the ICs associated with the GNSS data. We think that explaining why we decide to compute a-priori HYDL+NTAL could be harder to follow than what is written in the manuscript right now.

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.

We do agree, in fact in Section 4.2 we point out that if we consider only the stations with amplitude associated with IC2 and IC3 larger than 3mm, the mean Lin correlation increases to 0.57 and 0.44, respectively.

*** Once ATM and HYDR loads are proven to be good estimators of the common modes, why not use them to correct the time series ?**

Our goal is to remove signals associated with meteo-climatic processes using vbICA, instead of subtracting modeled displacements, such as those made available through loading services like GFZ, from the measured displacements. This approach minimizes biases due to the mismatch between the actual signal caused by atmospheric and hydrological loading and the modeled ones. Larochelle et al. (2018) reached similar conclusions by comparing GRACE measurements and the results from ICA decompositions of GNSS displacements, which resulted to be more accurate in correcting GNSS from seasonal displacements than removing GRACE displacements, which smooth local effects in the data acquisition and processing.
This is now described at the beginning of Section 5.1:
"Our goal is to estimate the tectonic velocity of the GNSS stations, then we seek to remove signals associated with meteo-climatic processes. Instead of subtracting from the IGb14-time series the modeled displacements, such as those made available through loading services like GFZ, we prefer to subtract the displacements associated with the ICs. This approach minimizes biases due to the mismatch between the actual signal caused by atmospheric and hydrological loading and the modeled ones. Larochelle et al. (2018) reached similar conclusions by comparing GRACE measurements and the results from ICA decompositions of GNSS displacements, which resulted to be more accurate in correcting GNSS from seasonal displacements than removing GRACE displacements, which smooth local effects in the data acquisition and processing.
In order to support the approach followed, we estimated the scatter of the GNSS displacement time series by computing the mean standard deviation of 1) the time series given as input to vbICA (IGb14-time series), 2) the IGb14-time series minus the combined displacement associated with the first 3 ICs and 3) the IGb14-time series minus the displacements due to HYDL+NTAL from GFZ models. The resulting standard deviation is 5.32, 4.10 and 4.73, respectively. This demonstrates that removing the displacement associated with the first four ICs is more effective in reducing the scatter than removing the HYDL+NTAL contribution."

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 ?

As now reported in the conclusions, the procedure used in this work to estimate the station velocities does not allow to distinguish the tectonic velocities from the contribution to the velocity induced by climate-related processes, in particular if the linear trend associated with ATML and/or HYDL time series is large. Nonetheless, the small trend associated with HYDL_IC1 is likely the result of an annual signal whose amplitude is not constant over the years, which is captured by GNSS_IC1.

Figures :

ICA figures:

- change color scales of IC1 for all plots to show lateral variations

Ok, done.

- 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.

We added, from line 209, a more detailed explanation on how to interpret the temporal evolution, the spatial distribution and the displacement associated with the ICs.

"Before discussing the vbICA results, we briefly explain how to interpret the temporal evolution and the spatial distribution of the ICs, so that it is possible to retrieve the displacements associated with them.

The color of each GNSS site in Fig. 2 represents the IC2 spatial response (U2), which indicates the maximum displacement associated with the IC2, while the temporal function V2 is normalized between 0 and 1. The displacement associated with IC2 between two epochs (e.g. t_1 and t_2, with $t_2>t_1$) at the station n is computed as $V1(t_2)*U1_n-V1(t_1)*U1_n(t_1)$, where $V1(t_2)$ is the value associated with the temporal evolution of the IC at the epoch t_2.

$U1_n$ depends on the site, but not on the epoch; its unit of measurement is mm, while V has no units of measurement. As a result, $V1*U1_n$ is in mm. It follows that if $U1_n$ is positive, as we observe for each station, and V1 is increasing ($V1(t_2)>V1(t_1)$), the stations move upward during the t_2-t_1 time interval. On the other hand, if $V1(t_2)<V1(t_1)$ the stations move downward during t_2-t_1.

As regards Fig. 2, assuming t_1=2010.0 and t_2=2020.0, the displacements associated with IC2 are ~30 mm upward at the "red" GNSS stations, ~30 mm downward at the "blue" GNSS stations and ~0 mm at the white ones."

We also modify lines 255-256:

> *"IC1 is a spatially uniform signal characterized by an annual temporal signature, as shown by the power spectral density (PSD) plot in Fig. 3a. The mean of the maximum amplitudes is 26 mm, while the histogram showing the distribution of displacement amplitudes is shown in Fig. S3a.*
> *IC2 shows a spatial response characterized by a clear E-W gradient, but, differently from IC1, its temporal evolution has not a dominating frequency. The spatial response U2 of the eastern stations (in blue) is mainly negative, while the U2 of the western stations (in red) is mainly positive."*

Furthermore, Figures 3, 4, 5 are not characterized large outliers and we think that the min/max normalization is the most intuitive to show the displacements associated with the ICs.

Figure 6: change colorscale to see changes in correlation coefficient for IC1 (the colorscale is completely saturated in the red).

Ok, done

Don't use "Lin" abbreviation but linear

With "Lin correlation" we mean the Lin concordance correlation coefficient (Lin, 1989).

Figure 7: panel b is identical to panel a

Yes, we made an error.
We decide to change Figure 7, showing the displacement of two different sites, one located in the south-western part of the study region (STV2), the other in the north-eastern side (LYSH), so that the displacement associated with GNSS_IC2 and GNSS_IC3 have opposite sign.

Abstract

First sentence : too complicated. Simplify and clarify

Ok, done.
"We study the time series of vertical ground displacements from continuous GNSS stations located in the European Alps. Our goal is to improve the accuracy and precision of vertical ground velocities, investigating the spatial and temporal features of the displacements caused by non-tectonic geophysical processes".

line 10: associated with : modeled from

Ok, done.

line 11: processes: drop

Ok, done.

line 16-17 : Atmospheric .... gradients: rewrite

Ok, done. Please note that we also modified lines 11-12:
“Furthermore, while the displacements caused by atmospheric and hydrological loading are apparently spatially uniform, our statistical analysis shows the presence of NS and EW displacement gradients.”

Introduction

First sentence: "active geophysical processes on land, ice and atomosphere": ground displacement on atmosphere. Rewrite.

Ok, done.

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

Ok, we added the following references:

- Ching, K.-E., Hsieh, M.-L., Johnson, K. M., Chen, K.-H., Rau, R.-J., and Yang, M.: Modern vertical deformation rates and mountain building in Taiwan from precise leveling and continuous GPS observations, 2000–2008. Journal of Geophysical Research, 116, B08406, https://doi.org/10.1029/2011JB008242, 2011.
- Dal Zilio, L., Hetényi, G., Hubbard, J. and Bollinger, L: Building the Himalaya from tectonic to earthquake scales. Nature Reviews Earth & Environment, 2, 251–268, https://doi.org/10.1038/s43017-021-00143-1, 2021.

line 117: give principle of CMC Imaging

Ok, done. We added the following part:
“A filtering method similar to CWSF, called CMC Imaging, is developed and used by Kreemer and Blewitt (2021) in western Europe to extract common mode components that are as local as possible. The main difference between CWSF and CMC Imaging is that the former uses as a weighting factor both the distance and the correlation coefficient among the stations, while the latter only the correlation coefficient, showing that it is representative of the distance among the stations.”

line 190: pdfs --> PDFs (and elsewhere)

Ok, done.

line 192: drop "that"

Ok, done.

line 216: a priori any temporal : rewrite

Ok, we rewrote the sentence as follows:
"The advantage of this approach, compared to a trajectory model, is that it is not necessary to assume any temporal evolution of the deformation signals a priori, except for the limited number of functions that make up Eq. (1)"

line 389: k=-2 for both noise and flicker : correct text

Ok, done.

line 391: avoid + in text

Ok, we use "plus" instead of +.

line 506: elastic hydrological load ---> elastic response to hydrological load

Line 506 is deleted in the updated version, but there we use "elastic response to hydrological load" in the text we added.

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

With "Lin correlation" we mean the Lin concordance correlation coefficient (Lin, 1989).