

The manuscript “Common mode signals and vertical velocities in the great Alpine area from GNSS data” by Pintori et al. 2021 utilizes variational Bayesian Independent Component Analysis to separate loading signals due to atmospheric and hydrologic sources from vertical GNSS time series to further isolate uplift rates due to mountain building in the greater alpine region. This also serves to provide additional information on the source of the common mode signals in GNSS time series. In this study, they also compare the vbICA method to other methods of constructing the common mode. Finally, they remove the non-mountain building signals from the data and estimate uplift rates. Overall, the article is well written and is a useful contribution and the revisions provided some clarification and improved the manuscript. However, there are a few aspects that would be improved with clarification and a few points the author overstate.

1. In lines 188-189, you mention that the Bayesian approach introduces an approximating PDF for the parameters. Is the PDF chosen by the algorithm or by the authors? If the PDF is chosen by the authors as an input to the vbICA, does changing that PDF significantly change the output?
2. For the NTAL and HDYL models, why are you modeling them at a grid interval rather than at the specific points of the GNSS stations? When you compare the ICs from the models to the GNSS stations (eg Figure 7), are you using the closest grid point or interpolating them to the GNSS station position? Would it not more consistent to just use the NTAL and HYDL models at the GNSS points and then apply the vbICA?
3. This may be out of the scope of this paper, but I’m still wondering why the authors wouldn’t remove known sources of loading, like NTAL, NTOL and HYDL and then apply a vbICA to see if there are any consistent patterns in the data not attributed to those well known signals? This could then highlight additional signals that cannot easily be removed from the data or are missed by the models (eg reference frame jitters). To me, this seems like a preferred method to use the vbICA since it would remove signals we know exist first and then diagnose the resulting signals. Otherwise, your ICs are likely to contain the known signals (NTAL and HYDL) as well as contributions from other signals that have similar temporal or spatial patterns – which the authors acknowledge when explaining why they combine multiple IC from different sources. Thus, the ICs are likely dominated by the loads you are attributing them to but also likely contain other signals that might be more apparent if you removed the known signal first.
4. I still think the authors are overstating the temperature relationship in IC4. In lines 394, you state that it is well correlated with temperature, but couldn’t you find a similar correlation with another annual signal, like NTAL for example or even HDYL? I think the added discussion in line 600 is helpful but still do not provide a strong enough case to be mentioned in the abstract and in the conclusion. Yes, IC4 has a strong annual signal but that does not necessarily mean that it is due to temperature fluctuations especially given that majority of the stations this component explains less than 1% of the data variance as stated in lines 618. Also, the mechanisms provided for temperature would be highly site specific and would be dependent on the type of monumentation (eg is the

monument located in bedrock vs unconsolidated soil monuments)? Additionally, the mechanisms provided are more related to hydrology and site characteristics than temperature. I think the relationship of IC4 to temperature is overstated especially since it is mentioned in the abstract and conclusion and multiple times throughout the text even though the IC is only prevalent in a few stations and the mechanisms provided do not support the claim.

5. Additionally, when calculating the reduction in the standard deviation, you only use the first 3 ICs (lines 477) and not IC4. Why?
6. You state the ICs likely contain a larger component of power-law noise. When you are comparing the different filtering approaches (line 477-479) when you remove the modelled GFZ NTAL and HYDL are you also estimating a noise model for those series as well?
7. Lines 433-435 are a touch confusing. Are you assuming that removing the ICs completely removes all the annual signal? Could there not be other processes that have annual signals that are not captured by the ICs?
8. Figure 6 might be improved by consistent axes.
9. Maybe I missed it, but what precipitation data are you using (lines 557).