

Comment on egusphere-2022-926

Anonymous Referee #2

Referee comment on "Scrutinizing and rooting the multiple anomalies of Nepal earthquake sequence in 2015 with the deviation–time–space criterion and homologous lithosphere–coversphere–atmosphere–ionosphere coupling physics" by Lixin Wu et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-926-RC2>, 2022

This paper collected all the anomalies around the Nepal earthquake in 2015 occurring in lithosphere, atmosphere and ionosphere, and try to establish their physical relationship according their spatial and temporal characteristics. It provides a new idea for comprehensive analysis for multi parameters from different layers. It is interesting and shows some significances in earthquake research and prediction. The paper can be accepted after minor revisions. Main questions: 1. How do the authors estimate the reliability of the anomalies, especially the studies on the same parameter but with the different anomaly occurring time? 2. About the published papers, their studied time scale are not the same, for some with much longer time than years, for some only a few days before the earthquakes that did not mean without long time anomaly in that parameter. According to the statistical study of De Santis, the ionospheric disturbances were also occurred in a quite longer time during the earthquake preparation phase. How do the authors consider them in a same time scale when their studied time period is not same, is it suitable? 3. Most anomalies were not detected in the same day, even after the selection of Figure 7, which illustrate indirect relationship between them? Or the coupling process among the lithosphere, atmosphere, ionosphere all needs a few days? Minor revisions: Line 165: about the figure 3, why only exhibited -20d anomalies around the earthquakes? As described in the text, most parameters exhibited anomalies before that, especially for those in lithosphere? I'd like to suggest a much longer time scale, at least 2-3 months before for a M7 earthquake. Line 265: for figure 5, please add the unit for the parameters used in the model. Line 310-315: This paragraph discussed the P-pole links, but unfortunately there is no direct evidence or observations to prove it. The authors just list its products in atmosphere, not the real detection on the P-pole effects from geoelectric field, geomagnetic field, underground fluid or something like them in that region. The laboratory results cannot replace the field observations. So it is not so convincing here. Line 360: through the ionosphere? Line 352: About the section 5.1, I cannot agree with the point to firstly retain the negative anomalies in ionosphere. (1) The ionospheric perturbations are always modulated, they cannot be easily defined as increase or decrease; the number of positive anomalies in TEC is almost the same as the negative

ones, why the authors remove the positive ones so easily? (2) The disturbances in VLF radio waves are always considered with close relationship of acoustic gravity waves, why the authors connect them to electric field?

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-926/egusphere-2022-926-RC2-supplement.pdf>