Review of the manuscript: "Monitoring the geodynamic behaviour of earthquake using Landsat 8-OLI time series data: case of Gorkha and Imphal", by Biswajit Nath et al.

The paper investigates the possible relationship of lineaments (number, length, main orientation) and their changes, extracted from satellite imagery, with geodynamic behavior in seismically active zones, in particular studying scenarios before and after important earthquakes occurred in Nepal and Eastern India.
Authors used commercial software and standard routines and tools to perform imagery analysis claiming their "method worked practically for earthquake monitor and one can apply this new novel combined approach to **predict** the probable earthquake occurrence in advance just a few days before it strikes". The topic is surely of interest but I don't agree with authors' conclusions which, from my point of view, are not confirmed by the presented results nor supported by a methodological approach with sufficient scientific soundness, according to my following comments and remarks. For these reasons, I cannot recommend publication of the paper in its present form.

Main comments:
Generally the English is very poor through all the paper. Many sentences are very long (see for example page 2, lines 28-35(!), but there are many others) and often written in an awful language, making them hardly readable and understandable.

1. Methodology
   Authors generally use commercial softwares and standard routines and tools, often providing description and details (e.g. they spent quite a paragraph to explain what is the "model builder" of ArcGis) which are not very new nor useful. On the other hand, they do not provide details about the theoretical background they have in mind to investigate the geodynamic behavior of the study areas. Why they are studying number, lengths and direction of lineaments? Why do they expect variations in these features (and to what "sign" and extent) and what are the theoretical models explaining their relationship with Earthquakes? Why do they investigate "vertical transect profiles"? What do they exactly means with "vertical profiles" and why they would expect variations in "false color (5, 4, 3)" imagery? Again, what is the theoretical background behind this analysis? If models exist, authors should strictly refer to these and verify if the model hypotheses are confirmed or not by their experimental results. On the other hand, if models do not exist yet, authors should first present their theoretical model and then demonstrate their results are coherent with or not.
   Moreover, the scientific soundness of the presented methodological approach, based only on a few images (3 before and 1 after the quake) is quite poor. How these (very limited) conditions, may drive authors to so firm conclusions? A more extended dataset should be analyzed and it is absolutely required in my opinion to provide more convincing and scientifically rigorous results. For instance, no confutation analysis (lineaments variations in absence of EQ) is provided. To do that authors should analyze similar temporal sequences of

LANDSAT imagery in periods when no EQs occurred in the study area, showing that similar "changes" did not occur in these circumstances.

2. Result Presentation

   The quality of images is very poor and the way they are commented in the text and within the captions is often not very helpful for the reader.

3. Result interpretation

   Authors are interpreting their results only in terms of consequences/effects of EQ. No mention to possible artefacts or unwanted effects due for instance to changes in observation conditions (e.g. clouds, snow, atmospheric or surface changes) is done. In my opinion, the observation/illumination conditions may have a significant impact on feature extraction, as clearly demonstrated by the results shown in Figure 5: the lineaments map of April 5 (central map) is very different from the 20 March (left) and 21 April (right) ones. In particular, looking at the bottom of the 5 April map, no lineaments are extracted for this day, whereas a number of features is clearly visible a few days before and after. Do really the authors may assert that such changes are TOTALLY due to geodynamic causes only? May they categorically exclude any other cause (including simple observation conditions)? Moreover, how much the errors and inaccuracies of the processing steps (e.g. lineaments extraction, segmentation and length estimation) impact on the achieved results? Error impacts and algorithm inaccuracies are not discussed at all. These are just some examples of how authors seem to get conclusions and interpretations not really in line with the actually achieved results.

   Other examples are in results reported in:

   - Table 1: authors achieve different behavior of lineament changes (decreasing-increasing and then decreasing after the EQ for Nepal, but decreasing-increasing and then increasing again after EQ for India) but they still interpret these data as showing "similar trend".
   - Figures 13 and 15: where do authors find "anomalies" on profiles? Why should they occur? What do they mean and how do they can be related to geodynamic processes?
   - Figure 17: again, a different behavior in the buffer analysis for the two test cases: how do authors may explain this?