Comment on “Investigating rupture directivity effects on macroseismic intensity data in Italy” by Antonucci et al., submitted to NHESS.

The manuscript has critical issues that preclude its publication in its current form.

First, the approach used to determine whether the intensity distribution exhibits directivity is purely qualitative and, furthermore, not precisely defined in the text.

Second, looking at the azimuthal distributions of aleatory residuals δW_0, in most cases I am not able to see the evidence of directivity that the authors assert to see. I suggest the author to develop some quantitative criteria able to distinguish between directive and non-directive macroseismic fields. To this end, one could start for example from the Kuiper and Rayleigh statistical tests, which are employed by Gasperini et al. (1999, 2010) in the Boxer code to assess the azimuthal uniformity (or non-uniformity) of intensity distributions.

Another critical aspect, completely ignored by the authors, is the spatial uncertainty associated with the instrumental locations that may be of the order of some tens of km for earthquakes occurred before the first Italian telemetered seismic network was installed at ING starting from 1984. The point is particularly evident for the Friuli earthquake of 1928, for which the instrumental epicenter appears displaced 10-15 km southward with respect to the real one, which is reasonably located closer to the cluster of highest intensities.

I argue that only for the last two earthquakes in Table 2, the spatial uncertainty of instrumental epicenter becomes of the order of a few km, while for the other ones it is of the order of 5-10 km at least. The authors must discuss the role of such uncertainty on the effectiveness of theirapproach.

Minor comments:

It would be advisable to include the estimated directivity azimuths for each earthquake in Table 2, to allow for direct comparison; similarly, an arrow indicating the estimated direction of rupture propagation could be added to the left-hand panels of Figures 3–6. Finally, in the right-hand panels of Figures 3–6, it would be more meaningful to report the standard errors of the mean residuals (calculated by dividing the standard deviations by the square root of the number of residuals in each azimuthal bin) rather than the sample standard deviations.

The paper has problems that prevents its publication in the present form. First of all, the approach used for deciding if the intensity distribution present directivity or not is fully qualitative and neither well defined in the text. Second, looking at the azimuthal distributions of aleatory residuals δW_0 I am not able to see what the authors asserts.

I suggest the author to try to find some quantitative criteria able to distinguish between directive and non-directive macroseismic fields.

One idea might be to start from the Kuiper and Rayleigh tests used by Gasperini et al., (1999, 2010) for assessing the azimuthal uniformity or non-uniformity of the intensity distributions in Boxer code.

It would be useful to report in table 2, the estimated directivity azimuths of each earthquake for a direct comparison as well as in the figures 3-6 left panels an arrow indicating the estimated rupture propagation direction. Furthermore, it would be more significant to plot in Figs. 3 to 6 right panels, rather than the sample standard deviations the errors of the mean residuals (dividing the standard deviations by the square root of the number of residuals in each azimuthal interval).