Review of "Investigation of the weather conditions during the collapse of the Morandi Bridge in Genoa on 14 August 2018" by Burlando et al. 2019 (nhess-2019-371)

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In this study, the authors describe the weather conditions during the period when the Morandi Bridge in Genoa collapsed. They use observational data from satellite, radar, lightning detection sensors, surface stations and a Doppler lidar. Based on the lidar data, the authors characterize the gust front associated with the passing thunderstorm. In addition they performed model simulations with three different forcing conditions and compared the modeled reflectivity and wind speed at the surface with the observed radar reflectivity and surface wind measurements. The authors conclude that a downburst occurred at the time of the bridge collapse a few kilometers from the bridge. However, there is no clear evidence that atmospheric conditions played a role in the bridge collapse.

The motivation for this study was the collapse of the Morandi Bridge. This is without doubt a very tragic event which needs to be investigated to identify the causes for the collapse. So far there is no evidence that the atmospheric conditions around the time of the collapse played a role in the collapse and the presented analysis does not provide any new insights into this. Neither maximum wind speed nor precipitation amounts were extreme. No substantial conclusions are drawn and considering this the scientific impact of the study is rather weak. The study further lacks a clear scientific objective as the description of atmospheric conditions is not an objective. The model simulations also are not analysed in detail and do not add any new insights into the relation between the bridge collapse and weather conditions. I find it very surprising that the simulation with the data assimilation (WRF GFS-DA) totally misses the storm while the simulation WRF-IFS fits much better to the observation. The analysis of the measurements should be more integrated and needs to be improved at several points (further details are given in the specific comments) e.g. the authors should try to link the observations from the different sources in a better way (e.g. wind measurements from the surface stations and the lidar) and provide a more in depth analysis. Because of all these considerations, I cannot recommend the publication of this article in its current state and I believe that the necessary modifications are too extensive to be done within a major revision. However, the study includes some interesting and new parts (like the Doppler lidar measurements), which I believe deserve publication and I encourage the authors to resubmit a completely revised version of the manuscript with a changed focus. One possible way could be to focus on the gust front observed by the Doppler lidar and use the supplemental measurements from surface stations, radar and satellite to support the hypotheses on the gust front obtained from the Doppler lidar, e.g. propagation speed and direction. The link to the collapse of the bridge could of course be mentioned but should not be the motivation for the study. This could rather be the observational capturing of a gust front by Doppler lidar measurements. In their new manuscript, the authors should try to reduce the number of references to the most relevant (at the moment the number of citations is very large). The number of figures is too large as well and the authors should try to reduce them by combining measurements from different sources. While the English writing is mostly fine, I recommend the manuscript to be checked by a native speaker (e.g. for the correct usage of articles).

1 Specific comments

1. Title: The title is misleading as it suggests a link between the weather conditions and the bridge collapse.

2. l. 41: While the authors state that there is lightning in the picture, the lightning was not detected by the network (as described in Sect. 3.1).

3. l. 53-56: This may be the case but the presented data are not really suitable to quantify the role of weather in the collapse.

4. l. 59-62: The large HyMeX programme should be mentioned in this context.

5. l. 62-66: In my opinion it is not necessary to describe the previous storm in so much detail as it is not decisive for the study.

6. l. 76-78: The comparison between downbursts and the ABL is not adequate. The statement implies that the ABL wind is correctly represented in models. Models still fail to correctly represent the ABL conditions in particular over complex terrain. As the scale of downbursts is not resolved by NWP grid spacing, it is not surprising that the models fail to represent them.

7. l. 79-86: Confine to the most relevant studies.

8. l. 101-106: The detailed description of aircraft crashes is not relevant.

9. l. 111: This is not a scientific objective.

10. l. 115ff: The model simulations are not presented in a way that they complement the observational analysis.

11. l. 140: What does LAMPINET stand for?

12. l. 162: What does THUNDERR stand for?

13. l. 221: Where are Querida I and II in Fig. 4a. I don't see Querida over northwestern Scandinavia but rather northeastern.

14. l. 233: I doubt that the connection of the two clusters over the sea is really relevant for the gust front observed at the coast.

15. l. 237: The cloud tops above Genoa already reached 12.000 m at 09:15 UTC according to Fig. 5a.

16. l. 241-248: The movement of the cells is not very visible in Fig. 6. The x- and y-axis range should be reduced. How do the radar images fit to the satellite observations? The shape of the cells is quite different. How can this be explained? What is the vertical structure of the cells? Instead of VMI the authors could show a MaxCappi.

17. l. 248ff: I cannot distinguish the impact of orographic channeling on the precipatation cell. There is no observational evidence for this in the presented figures.

18. l. 258: The SRI images in Fig. 7 are rather redundant to Fig. 6. How are the translation velocities calculated, i.e. how is the arrow drawn in the figure at 09:00 UTC calculated?

19. l. 264: north-eastward!

20. l. 265ff: The evolution of the cell in Fig. 7 does not agree with the statement in l. 263-264 that the cell is more compact over orography.

21. l. 272-274: The lightning distribution does not fit to the reflectivity images in Fig. 7. Please explain.

22. l. 281-282: This is in contradiction to the statement on lightning in the introduction.

23. l. 287ff and Fig. 9: Wind direction should be plotted as markers and not as a connected line as this is misleading. Markers should also be added to temperature and wind speed as the temporal resolution is low. Wind direction might as well change over north. This is not clear from this measurement as the temporal resolution is too low. How does the northerly wind observed at the surface fit to the northward propagation of the cells? Why is temperature not shown for all stations where it is available (Tab. 1). It would be very interesting to see the spatial distribution of the drop in temperature.

24. l. 308: I do not see a subsequent increase in pressure.

25. l. 318: How are hail and graupel distinguished at the weather stations?

26. l. 320ff: Be more specific. Station 11 shows westerly wind and stations 9 and 8 show southerly

wind. For a clearer presentation of the spatial distribution of wind the authors could show a spatial map with the wind plotted as arrows at the individual sites for specific times before, during and after the downburst.

27. l. 352: Note the recent study of Pantillon et al. (2019) on the observational detection of downbursts with Doppler lidar.

28. l. 358: How is the region with maximum velocity identified? Did the authors use an objective method?

29. Fig. 12: This figure needs to be much improved. The shown range has to be adapted to the actual measurement range and color range also needs be decreased. The coast line should be added and maybe even the surface measurements at the caostal stations. The time stamp format is confusing. Is it really necessary to show the seconds?

30. l. 361ff: It is important to realize that radial velocity is measured by the lidar. This means that the three-dimensional wind vector is projected on the direction of the lidar beam. For example, southerly wind cannot be detected at easterly azimuth angles. This means that the front could be wider, but not be detected anymore by the lidar due to a changing azimuth angle. Also, the decrease of radial velocity at larger elevation angles could be related to a changing wind direction. Overall, the problems and difficulties related to radial velocity mesurements need to be much more discussed.

31. Fig. 13: How is it possible that eight gust front heights are given in Fig. 13, while only 7 are indicated in Fig. 12? From which elevation angles is the displacement velocity determined? The contribution of the horizontal wind to the radial velocity is different at different elevation angles. Could this play a role in the estimated gust front heights, i.e. why is the gust front at 7.5 degree always higher than at 10 degree?

32. l. 398-400: From which station are the surface measurements taken? Are the values similar when using the other stations measuring temperature (Tab. 1)?

33. l. 408-409: The values in Charba (1974) were very different compared to the ones found in the present study. It would be worth discussing the different atmospheric conditions under which the gust fronts occurred.

34. l. 420-421: Here the authors state that the wind speed increases with height in the outflow. However, radial velocity decreases with increasing elevation angle, i.e. with height according to the detected gust front heights (Fig. 13). Please explain.

35. l. 449-450: Where can the two regions with high wind speed be found in Fig. 14?

36. l. 451-452: "decreases with time": At which stage of the gust front? Prior or after the the maximum of the gust front was reached? Distances given are along the beam? This needs to be converted to horizontal distance which is different for different elevation angles. According to Fig. 13 the gust front height is largest for 7.5 degree elevation angle (and not 10 degree).

According to this the distance would decrease with height. This whole issue needs to be analyzed more carefully paying more attention to horizontal and along beam distances and height compared to elevation angle.

37. l. 463-464: How does this easterly wind detected by the Doppler lidar relate to the surface measurements? How does it relate to the northward propagation of the gust front. Please discuss!

38. l. 475-476: Which two peaks? There are no peaks visible in WRF-GFS and WRF-GFS-DA in Fig. 16. Why is the period confined to 08:30 and 09:55 UTC? Why not before 08:30 UTC when the wind speed was just as high in WRF-IFS? How does the wind direction in the model compare to the observed ones?

39. l. 487-490: The storm at 13:25 UTC in the observations also fits to the storm in WRF-IFS at that time.

40. l. 491ff and Fig. 18: Why is the thermodynamic diagram calculated for the grid point in the center of the cell? Thermodynamic diagrams are usually used to describe the pre-convective or post-convective environment and not the conditions within the cell. If the grid point shown in Fig. 18 is within the cell, why is there no saturation?

41. l. 506ff: The analysis of the simulations is rather superficial and at least for the WRF-IFS run which is able to produce some storms in the area a comprehensive evaluation of the model output for temperature and wind should be performed and the shape and existence of a potential gust front should be analysed. I cannot follow the conclusions the authors draw from the model simulations, e.g. I do not see a potential of the WRF runs to provide precursors for storms as two of three simulations fail to produce any kind of storm and the third simulation represents it at the wrong time.

References

Pantillon, F., Adler, B., Corsmeier, U., Knippertz, P., Wieser, A., and Hansen, A.: Formation of Wind Gusts in an Extratropical Cyclone in the Light of Doppler Lidar Observations and Large-Eddy Simulations, Mon. Wea. Rev., doi:https://doi.org/10.1175/MWR-D-19-0241.1, 2019.