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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Investigation of the weather conditions during the collapse of the Morandi Bridge in Genoa on 14 August 2018" *by* Massimiliano Burlando et al.

Anonymous Referee #2

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Review of "Investigation of the weather conditions during the collapse of the Morandi Bridge in Genoa on 14 August 2018" by Massimiliano Burlando, Djordje Romanic, Giorgio Boni, Martina Lagasio and Antonio Parodi.

The paper describes the weather conditions in Genoa on the morning of 14 August 2018, when the collapse of a highway bridge resulted in numerous fatalities. Images captured by a security camera close to the bridge show strong winds and lightning during the collapse, which raises the question of a possible contribution of meteorological factors. Based on a diversity of remote sensing observations, including satellite, radar, lightning sensors and Doppler lidar, as well as a dense local network of surface sta-

tions, the paper highlights the presence of a thunderstorm located along the coast and of winds reaching 15 m/s due to the associated outflow. The event is hardly captured by convection-permitting WRF simulations due to its complex and local dynamics.

The paper describes a convective event based on comprehensive observations from a variety of sources. It presents interesting new material and is well written overall. However, it suffers several major flaws in its present form. The scientific topic of the paper is unclear and the main conclusions are not sufficiently supported. Perhaps related to the first issue, the discussion often spreads in diverging directions instead of focusing on relevant content, which renders the interpretation confusing or even speculative. In particular, the section based on model simulations is not convincing. General and specific comments are listed below to help alleviating these flaws. The paper thus requires substantial revision before it can be considered for publication in Natural Hazards and Earth System Sciences.

GENERAL COMMENTS

1. What is the scientific topic of the paper? A hypothetical contribution of meteorological factors to the bridge collapse would involve engineering considerations and goes far beyond the scope of the paper. Furthermore, the results rather suggest that the wind was unexceptional, which is not clearly stated in the abstract and conclusions. The meteorological questions must be better introduced and the general knowledge that is acquired from that specific case study must be better highlighted.

2. The interpretation of meteorological data, albeit interesting overall, tends to be confusing and sometimes speculative. In my opinion, the results suggest that two different gust fronts reached the western and eastern stations. This interpretation may be erroneous but the description of results and their presentation, e.g. the different space and time coordinates used for the different types of observations, prevents a clear picture of what happened. The analysis must be improved by emphasizing important information on figures, better connecting the different types of data and avoiding over interpretation.

3. The configuration of model simulations does not look appropriate for the event and their contribution to understanding its dynamics is very limited. Either run new model simulations and analyze results in details, or remove the section altogether.

SPECIFIC COMMENTS

Abstract

l. 25-26 Operational predictability cannot be discussed without using operational forecasts.

1. Introduction

l. 28-33 Beginning with a discussion of bridge design is not appropriate in a geoscience journal.

l. 33-35, 117-122 Please clarify; I understand you want to be careful but the message is confusing as it is.

l. 49-56 These claims need references.

l. 57-68 The paragraph does not fit with the topic: thunderstorms are not usually associated with cyclones in the mid latitudes; this may be different in the Mediterranean area but needs justification; the cited Zolt et al. (2006) does not mention downbursts.

l. 78-90 The list of publications does not need to be exhaustive and must be shortened; the discussion of the number of Google Scholar publications is not relevant to motivate the study.

l. 99-105 It would be more logical to introduce these historical papers first, then the more recent examples, and finally the systematic bibliography above.

2. Data and Numerical Simulations

l. 181-191 Why use two very different domains? The first one looks unnecessarily large for a thunderstorm case study.

3. Results and discussion: observations

l. 217-230 The two low pressure systems over northern Europe are not relevant for the study (and the dates do not match); better focus on the region of interest and emphasize contributing factors, e.g., instability, cyclogenesis and fronts.

l. 231-239 I do not clearly see the formation of a convective line; changing the color bar (16 km is reached in the tropics only) and improving the overall poor quality of Figure 5 may help.

l. 243-257 This discussion appears speculative.

l. 277-278 This period includes the collapse time and should thus be shown and discussed.

l. 284-310 Figure 9 needs improvement for interpretation: sampling of “20–30 min” and “approximately 2.5 hours” are confusing and time labels every 2:24 h are weird; better zoom on the time of interest and show every single point of measurement, including a time series of wind gusts rather than a single value; the observed impact on pressure is very speculative and is better omitted; finally, the temperature and wind change occurs over a period of about 1 h, which is not “abrupt” and does not support the presence of a macroburst.

l. 305-308 This belongs to the introduction.

l. 311-314 The difference between coastal and terrain station is more striking than between west and east; drop symbols are illustrative but not quantitative, numbers are also needed.

l. 319-341 The interpretation is not convincing: eastern and western stations clearly behave differently, which is consistent with the location of the convective cell over the eastern stations; however, the slow wind turning at western stations does not support the presence of a macroburst; moreover, the claimed association with a gust front is confusing without a spatial representation; what about temperature records at nearby stations?

l. 341-346, 372-374 This belongs to the introduction; a definition of downburst, macroburst and gust front is lacking and associated cold pools should be explained as well.

l. 356-371 This discussion is lengthy and should be streamlined; zooming in would help identifying features on Figure 12, which is white mostly (no data).

l. 374-385 (and 422-424) The eight symbols in Figure 13 depict eight different times, thus their representation is confusing and the computation of a displacement velocity obscure (and of the inclination angle).

l. 386-429 Considering the small contribution of moisture, the uncertainty in k and the very speculative increase in pressure, the computation and discussion should be largely simplified.

l. 430-455 This discussion is largely speculative and should be streamlined or omitted.

l. 456-463 What can be learned from Figure 15?

4. Results and discussion: WRF Numerical Simulations

l. 465-517 The purpose of this section is unclear: even the best run (WRF-IFS) still strongly differs from observations thus would need a much more detailed and systematic analysis to provide useful information on the actual dynamics of the event, while the model configuration is too different in the other two runs to provide useful information about model sensitivity with such a local event.

5. Conclusions

l. 551-556 The study clearly shows the presence of a thunderstorm during the storm collapse but rather suggests that associated winds were not extreme; how these may have or not have affected the bridge is far beyond the scope of the study.

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