Answers to Reviewer #2

Summary and overview: 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 stations, 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.

Answer: The reviewer's summary of our study in the first paragraph is accurate.

The overview provided in the second paragraph criticizes the scientific objective of this work and insufficient support of some of the conclusions. The model performances are also pointed out as being suspicious. We would like to emphasize that we have addressed all of these specific points below in the reviewer's general and specific comments. In the nutshell, the scientific objectives are strengthened, numerical simulations are additionally discussed and some of the conclusions will be reformulated in the revised manuscript. We thank the reviewer for the insightful comments provided in this review.

General comments:

Q1: 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.

A1: The objective of this study is to investigate the weather conditions during the collapse of the Morandi Bridge from different spatiotemporal scales. Besides inspecting the weather at local scale—namely above Genoa and around the collapsed bridge—the paper also analyzes the larger scale weather scenarios in order to identify the main contributors of this high impact weather, and to which extent these contributors were predictable in an operational setup of the Weather Research and Forecasting (WRF) model. The authors are convinced that this is a valid scientific objective because the paper does not only presents measurement data, but rather also intends to interpret and provide the physical background of the presented observations whenever possible.

Our objectives will be additionally strengthened in the abstract as well as the conclusions. Although wind speeds were much higher in comparison to the annual average value in Genoa, we agree with the reviewer that the wind speed was not exceptionally high in terms of wind speed magnitude. Nevertheless, it was a thunderstorm event that occurred during the collapse and the unique lidar and other surface measurements deserve to be published and interpreted in order to provide a more complete picture of the circumstances surrounding the bridge collapse.

Lastly, we fully agree that a structural wind engineering study is beyond the scope of this paper, as well as this journal and that is why we opted not to present this analysis here. It might be carried out in the future research. We have also elaborated on this topic (i.e., the inclusion of wind engineering study) in our response to the general comment of Reviewer #1.

Q2: 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.

A2: We thank the reviewer for stating that the overall interpretation of meteorological data is interesting.

We have now tried to remove all the speculations that were present in the original manuscript. In particular, some parts related to the interpretation of gust front structure from lidar measurements were now omitted (e.g., see also our answer to the question Q36 of Reviewer #1) and citing literature in the Introduction is limited to the most relevant studies for this research.

Our interpretation of data does not indicate the existence of two different gust fronts in the region. We are assuming that the reviewer is referring here to Figure 11 and the specific comment Q19 below. Different temporal signature of the gust front at different anemometer stations does not indicate that the event was characterized with several gust fronts, but rather a single gust front that evolved over space and time. The examples of this are numerous in literature. For instance, Burlando et al. (2017) analyzed a downburst in Livorno, Italy, on 1 October 2014 using a network of three anemometers installed along the coastline (two of them) and further in Livorno (one anemometer). While all three anemometers recorded the same event, the velocity (speed and direction) time histories were different at each anemometer. The reasons for different wind velocity signatures at different locations in the outflow are numerous, but some of the most obvious are that the outflow could be not perfectly symmetric in respect to downdraft touchdown due to the background atmospheric boundary layer winds and cloud translation, different surface roughness, as well as different radial distances of anemometers from the downburst center which results in naturally different evolution of the outflow at each anemometer.

One of the goals of this manuscript is to make the use of multiple data sources in order to describe and interpret weather conditions during the bridge collapse. Therefore, we have attempted to make multiple connections between different measurements throughout the manuscript. The radar reflectivity and precipitation data are linked, as well as the radar and satellite observations. Furthermore, we have connected the interpretation of lightning data with radar observations of precipitation zone. The high-frequency anemometer data positioned along the coastline are also interpreted in conjunction with Fig. 9, which shows the meteorological observations from the Genoa Airport weather station. The evolution of the precipitation zone and gust front are interpreted using multiple surface observations in Figures 10 and 11, respectively. Furthermore, the lidar measurements are discussed in terms of their relationship with the location of cloud cells and precipitation zone shown in Figures 6 and 7, respectively. Of course, the WRF numerical simulations were compared against the available in-situ and remote sensing measurements. However, we have additionally strengthened this aspect of the manuscript and additionally discussed the discrepancy between lightning data from LAMPINET and the video observation of lightning strike hitting the bridge prior to the collapse. Furthermore, the reasons for choosing the Genoa Airport weather station for determining the gust front displacement velocity instead of other weather stations will be also discussed in the revised manuscript. Also, the location of the precipitation zone in Figure 7 is now linked with the absence of lidar data in Figure 15.

Q3: 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

A3: The WRF configuration set chosen is the same of the operational model runs for this region by CIMA Research Foundation since the end of 2018 (https://www.cimafoundation.org/foundations/research-development/wrf.html). Furthermore, the schemes and model dynamics described in Section 2.2 demonstrate that we did not use any untested configuration of WRF. We fully agree with the reviewer that the model results presented in the previous version of the manuscript do not provide a good forecast of this event. In comparison to the original manuscript version, we will now add to the discussion the simulation including the data assimilation on the IFS driven forecast, namely WRF-IFS-DA. This prediction in comparison to the WRF-IFS experiment shows two main advantages: (1) wind peaks up to 12-13 m/s over the 8 wind stations used in the study, and (2) simulated event is one hour closer to the observed wind maxima (in comparison to the WRF-IFS experiment). Those improved wind forecast performances are due to the better reproduction by the WRF-IFS-DA simulation of the observed thunderstorm activity (Figure A below) close to Genoa in the late morning of 14 August 2018. The WRF-IFS-DA storms in the period 11–12 UTC resulted in a maximum wind gust around 15–20 m/s (Figure B below) and are located only 8–10 km far apart the wind stations locations. The estimated location error of the predicted storms is just 4–6 times the grid spacing. This model accuracy is deemed as very good (Grasso 2000).

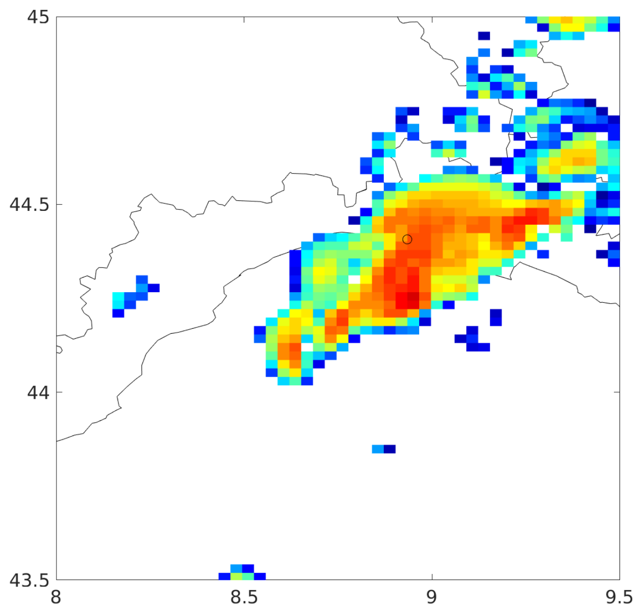

Figure A. VMI map from WRF-IFS-DA simulation at 11:30 UTC.

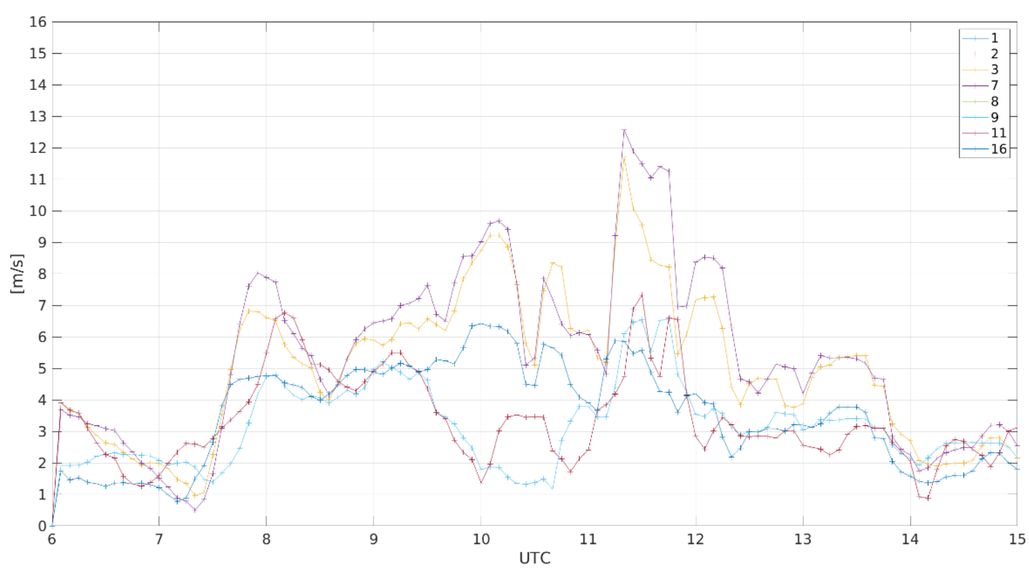

Figure B. Maximum 5-min wind speeds at 10 m above ground from the WRF-IFS-DA simulation at the locations of eight anemometers situated in the Port of Genoa. See manuscript for additional information on anemometers location.

Lastly, we strongly believe that demonstrating that only one of the members of operational WRF suite properly capture this event is as scientifically significant as if the WRF provided a good comparison against the observations in all cases. It is a truth that the model simulations are not able to perfectly reproduce the timing and location of the observed event. However, these simulations are performed in an operational framework and the location and timing error is in line with the state-of-the-art numerical weather modelling prediction. Furthermore, even with slightly different timing and location, the WRF-IFS-DA simulation is reproducing the event well enough to provide a deeper insight into the physics of this event.

Specific comments:

Q4: Abstract l. 25-26 Operational predictability cannot be discussed without using operational forecasts.

A4: As mentioned in A3, all the setups used in this study are operationally run by CIMA Research Foundation with the GFS initialization since the end of 2018 (https://www.cimafoundation.org/foundations/research-development/wrf.html) on behalf of ARPAL (Liguria Region Environment Protection Agency). The main aim of this section was, in fact, to investigate the predictability of this thunderstorm event in the operational weather forecasting model setups.

Q5: Introduction l. 28-33 Beginning with a discussion of bridge design is not appropriate in a geoscience journal.

A5: We decided to start the Introduction by introducing the reader with some basic information about the Morandi Bridge. We do not consider this to be a discussion on bridge design, but a very brief overview of the Morandi Bridge history and its state prior to the collapse. After all, the paper is analyzing the weather conditions during the collapse of this bridge, so it is appropriate to provide the basic information about the structure. However, we will remove the two sentences describing the bridge design in this paragraph, as the reviewer suggested.

We agree with the reviewer that the journal is not suited for structural analyses and technical terms in this field. That is why we have omitted this type of analysis in this manuscript (see also our answer A1 above).

Q6: l. 33-35, 117-122 Please clarify; I understand you want to be careful but the message is confusing as it is.

A6: As we stated in our answer A1, the goal of the manuscript is to document and investigate weather conditions during the collapse of the Morandi Bridge. The motivation for this meteorological study is the fact that the bridge collapse occurred during a severe thunderstorm event that was captured by several in-situ and remote measuring instruments. Besides only presenting the data, the study also aims to interpret the observations and provide the weather contributors for this event at different spatiotemporal scales.

However, we also want to emphasize that this study does not intend to explicitly demonstrate that the collapse occurred (or did not occur) due to the high impact weather. As we elaborated above, as well as in our answer to the general comment of Reviewer #1, a comprehensive structural wind engineering study would have to be conducted in order to quantify the likelihood of bridge collapse due to wind (or lightning). However, this type of study is beyond the scope of this paper and we are happy to notice that the reviewer agrees with us on this matter (Reviewer #2 question Q1 above).

Q7: l. 49-56 These claims need references.

A7: All this paragraph is based on newspaper articles (in Italian). A collection of these articles can be found in Wikipedia, see the Notes of the webpage https://it.wikipedia.org/wiki/Viadotto_Polcevera

Q8: 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.

A8: Cyclonic troughs are typically associated with frontal lines, which in turn can produce thunderstorms. We agree that this situation does not always occur, but the Alps orographic cyclogenesis that manifests around Genoa is usually associated with thunderstorms and downbursts (Burlando et al., 2017). The study of Zolt et al. (2006) as well as Burlando et al. (2018) further confirm this observation. We have accordingly modified our sentence in the revised manuscript to reflect the above clarification.

Zolt et al. (2006) describes the severe thunderstorm over Italy and while there is no explicit mention of a downburst, thunderstorms are inherently associated with downdrafts in the precipitation zone. These downdrafts further produce the radially advancing outflow known as the gust front. Furthermore, our citation of Zolt et al. (2006) is not related to the phenomena of downburst, but it is required in order to present other high impact weather thunderstorms that were investigated in this region. Conceptually, their study is somewhat similar to our paper and it is also published in the same journal.

However, kindly note that this paragraph in the revised manuscript will be drastically reduced and the discussion related to Zolt et al. (2006) shortened. We have limited the content to the most relevant studies and findings.

Q9: 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.

A9: We thank both reviewers for this suggestion. This part of the manuscript will be significantly shortened. Most of the citations are now excluded and the Google Scholar segment is also removed.

Q10: 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.

A10: This section will be also drastically shortened in accordance with Reviewer #1 comments. The segment related to aircraft accidents is removed from the revised manuscript.

Q11: 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.

A11: The domains in this study are the same as the domains used in the operational WRF forecasting for Liguria Region and the north-western Italy. The main purpose of the numerical simulations that we conducted is to investigate how good the operational WRF predicted this event. We agree that, being the focus a single thunderstorm event, a such larger domain would be unnecessary. Also see A3 and A4 for the details.

Q12: 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.

A12: This paragraph is a very short summary of the macro-meteorological conditions that ultimately led to the formation of the thunderstorm cloud on 14 August 2018 over Genoa city. Macro-meteorological conditions are not irrelevant for the local analysis and every meteorological office starts from analyzing the weather conditions at the synoptic scale and then focus on the local scale, because commonly the local weather is mainly due to the larger-scale conditions. In this case, the trough of Roswitha (see Fig. 4) caused a secondary cyclogenesis over the Gulf of Lion, which in turn was responsible of the atmospheric instability that led to the thunderstorm under study. As far as the dates mentioned are concerned, the lifetime of extratropical cyclones is usually in the order of some days: Roswitha was born on 16 August, but it actually affected the northern Mediterranean and Italy only a few days later.

As explained in A8, apart from single-cell thunderstorms which occur randomly during fair weather conditions, typically in summer when synoptic high-pressure systems prevent organized thunderstorm systems to develop and only thermally-driven cumulonimbus clouds generate thunderstorms, most often thunderstorms are related to unstable conditions linked to larger-scale disturbances, like troughs, cut-offs, or frontal areas associated to primary or secondary cyclones. In this paragraph, which is actually already very short, we describe the link between the "Weather conditions at larger scales" (title of Section 3.1) to the thunderstorm that is described later, also in Section 3.2 in terms of local observations. We cannot just start the description stating that on 14 August there were some clouds without explaining the reason behind the instability that triggered their formation.

Q13: 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.

A13: While the color bar goes up to 16 km, we state in that paragraph that the cloud tops above Genoa did not exceeded about 12 km. The quality of Figure 5 is now improved, but the color bar stayed the same because some of the clouds in the bottom-right in Figure 5a,b reached almost 16 km. Kindly note that we have improved Figure 5c with the zoom-in of the cloud tops above Genoa (also see our answers A15 and A16 to Reviewer #1 in relation to Figure 5).

The convection that stretches in the north-south direction above Genoa (Figure 5) resembles the main features of a convective line. The cloud tops are organized along the line that is very well structured. In addition, the convection in this region around 10:00 UTC is located below the tropopause height cutoff around 12:00 UTC (Figure 4b). This also reinforce the link between synoptic-scale conditions (Fig.4) and meso-scale weather (Fig. 5).

Q14: l. 243-257 This discussion appears speculative.

A14: As we stated above, this manuscript intends to go beyond the simple presentation of observational data and tries to interpret the observations using some physical arguments, whenever possible. We do agree that some of the statements might appear as speculative, but they are based on the observational evidence presented in data. Indeed, some other processes might have contributed and govern the cloud propagation, but the orographic influence that we "speculated" in this paragraph is also credible. Some relevant studies are also cited.

However, we will address this comment by relaxing our claim that orography likely influenced the cloud propagation. In this way we convey the uncertainty with this claim. In addition, please see our answer A17 to Reviewer #1 because it is also related to this discussion.

Q15: l. 277-278 This period includes the collapse time and should thus be shown and discussed.

A15: Figure 8c shows the number and spatial distribution of lightning strikes between 09:15 and 09:45 UTC, which encompasses the bridge collapse time. This discussion is provided later in the same paragraph. We observe that the lightning measuring network did not detect any strikes around the bridge in this period.

Q16: 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.

A16: This figure is now improved and considerably modified. Firstly, we have used symbols instead of line for wind direction, and symbols were also added to air pressure and air temperature lines. This edit was also proposed by Reviewer #1 in the comment Q23.

The measurements are available in a non-standard hours (i.e., not in the full hour) and that is why the time labels are not full hour. However, we have modified the time labels to a full hour and not 24 min into an hour in order to facilitate this comment (2-h interval shown in the *x*-axis). However, kindly note that not all measurements are available in the full hour timestamp.

There is not time series of wind gust, but only this single observed gust that is shown in Figure 9a. The gusts are recorded if the wind characteristics satisfy given conditions over a given period of time. The only significant gust recorded at the weather station was the one shown in Figure 9a that occurred around the bridge collapse time.

Unfortunately, the time resolution of air pressure data is much lower than the air temperature and wind velocity. Despite this, however, we still observe an overall positive trend in the air pressure which is in accordance with thunderstorm passage. Moreover, the pressure increase associated with thunderstorm passage is more gradual than wind speed and temperature jumps (e.g., Markowski and Richardson, 2010). We fully agree with the reviewer that the high-frequency time history of surface air pressures would probably deviate from the data shown in Figure 9c, but unfortunately such data are not available. However, we have reformulated our statements in the revised manuscript and excluded the terms "rapid" and "abrupt" when talking about air temperature and pressures. The kindly disagree with the reviewer's statement that the data do not resemble a downburst-like signature. The velocity and air temperature data alone do indicate a downburst passage over the area.

Q17: l. 305-308 This belongs to the introduction.

A17: This part was included in order to demonstrate that the observational data fit some conceptual models of thunderstorm downbursts. However, we will remove it from the revised manuscript. The proper description of downbursts and gust fronts will be included in introduction, which is also in accordance with the reviewer's comment Q20 below.

Q18: 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.

A18: We will include numbers next to drop symbols and station labels (the units are mm h^{-1}). However, it still seems that the increase of precipitation along the coast (towards the east) is more pronounced than moving further inland from the coast. However, we also notice that the west-east span of stations is approximately two times larger than the south-north span, which could bias these conclusions. We will add this statement in the revised manuscript.

Q19: 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?

A19: This part of the paper was not clear to Reviewer #1 as well in **Q26**, which means for sure that the analysis that we have described based on wind speed and direction data is not clear enough. The point is that the time series have a behavior which is absolutely coherent with the passage of a gust front that is spreading northward (away from the cloud downdraft) but, in turn, is also travelling northeastward together with the cloud itself. Because this process might not easy to visualize for the reader, this part has to be properly clarified. In the revised paper, new pictures/panels will be added to Fig. 11 (not to increase the overall number of figures) with the wind plotted as arrows and the text will be changed accordingly.

Q20: 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.

A20: The downburst definition and origins are already provided in the Introduction. However, we will additionally address this comment and also include the definitions of dry and wet downbursts, microbursts and macrobursts, as well as the thunderstorm gust front in the new version of this manuscript.

Q21: 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).

A21: Figure 12 is now substantially improved (also see A29 in rebuttal for Reviewer #1). Namely, the color bar is restricted to -7 m s^{-1} to +17 m s^{-1}, which is the range of radial velocities captured during this event. Secondly, the figure is saved with much higher resolution (600 dpi). Thirdly, the radial distance is now limited to 5492 m away from the lidar because the data beyond that radius are either unreliable or non-existent due to precipitation (i.e., mostly white as the reviewer correctly noticed).

Q22: 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).

A22: In addition to displacement velocity calculation that is provided in the manuscript, we kindly direct the reviewer to our answer A31 to Reviewer #1. We have clarified how the gust front height and displacement velocities were calculated including the treatment of lidar scanning time and the time needed to move to the next elevation. This lidar makes one elevation scan in 49 s and it takes 2 s to move to the next elevation. For example, this scanning velocity is much higher than that of a Doppler radar. Yet, in almost all practical analyses, the weather radar data (i.e., volume scans) are considered to be instantaneous despite the fact that the radar actually need some finite amount of time to perform the scan.

Here, an analogy can be drawn to rainfall data in order to further clarify this comment. Namely, concerning radar rainfall, it is usually assumed that the rainfall estimates from the "instantaneous" reflectivity represent the average rainfall in the interval 5–10 minutes around the scan. This is why in about 5-10 minutes (time taken for one complete scan, depending on the radar characteristics) the decorrelation in time is reasonably low and an instantaneous observation in space, at the resolution of the radar, taken any time inside the 5 minutes can be representative of the average rain rate in a 1 km x 1 km x 5 (10) min cell (assuming the radar resolution of 1 km). The similar assumption of the "frozen flow field" over the lidar scan period was invoked in the lidar wind analyses.

Q23: 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.

A23: While the reviewer might disagree, the authors believe that this discussion is simplistic. Namely, the authors' goal is to be as transparent as possible and to explain the reader how the displacement velocity was obtained. The objective is to show the method that can be easily replicated if the reader is interested. That is why we have included all the steps and explained them—however, the steps are arguably quite simple.

The uncertainty of k and low temporal resolution of pressure data are highlighted in order to inform the reader that the analysis contains uncertainties. The Genoa Airport station was used due to its proximity to the sea (i.e., lidar is also located on the coastline). We have highlighted this in the revised paper. Furthermore, our results are quite similar to the results reported in Goff (1976) and Mueller and Carbone (1987), but also very different to the results presented in Charba (1974). We believe that this comparison to other literature is very important in order to justify our findings, but also to demonstrate there is a large discrepancy between different papers. Both information are valuable. In addition, we have emphasized now that the reported displacement velocity is only the projection of the overall displacement velocity vector in the direction of lidar. Kindly also see our answer A32 to Reviewer #1 due to its similarity with this comment.

The smaller influence of relative humidity to the results is not limited to our data, but to the physic of this phenomenon. This quantity is less important factor in the gust front propagation velocity and dynamics, and we wanted to demonstrate that in our analysis. We believe that such an analysis is very instructive for the readers.

Q24: l. 430-455 This discussion is largely speculative and should be streamlined or omitted.

A24: We agree that this discussion might seem speculative in some parts, but in reality it is steered by the analytical results that were obtained using observational data and/or published literature. This is the authors' interpretation of observational evidences and we agree that perhaps some other interpretations would be possible due to the lack of higher-quality data that would provide the clearer picture of this event.

We would like to emphasize once more that the authors believe that the additional value of this research is in its attempt to provide physical background for the presented observational data. Otherwise, the paper becomes a technical report and this is not our intent. However, we will address this comment by excluding the most speculative parts in this section (e.g., the segment related to the variability of spatial separation between the high-speed regions in Figure 12).

Q25: l. 456-463 What can be learned from Figure 15?

A25: There are few reasons for the inclusion of Figure 15 in this manuscript. Firstly, we wanted to present to the readers that the lidar did not observe any significant phenomena during the bridge collapse due to the precipitation zone that was located close to the lidar. The overall goal here is to present what was the weather like during the collapse and it is important to document that this lidar could not provide a significant insight into it.

Secondly, the limited data that are available from the lidar show that the easterly winds were present at the lidar locations during the bridge collapse. This result is in agreement with the location of precipitation zone shown in Figure 7. This observation is also in accordance with the reviewer's comment Q2 that recommends more connection between different data sources in the interpretation of results. Kindly note that this figure is significantly improved in the revised manuscript in terms of color bar limits (-7 m s^{-1} to 9 m s^{-1}) and the radial distance of data is limited to 2852 m away from the lidar.

Q26: 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.

A26: The revised manuscript contains new numerical simulations. We have added WRF-IFS-DA run to supplement previous three simulations. This newest simulation provides much better agreement with the observations. While the onset of this thunderstorm is still delayed in the model, the delay is significantly smaller than in the previous simulations.

The purpose of this section is to analyze the accuracy and reliability of operational WRF forecasts in the case of this particular event. The authors are convinced that "imperfect" results from the operational weather forecast are very valuable addition to this manuscript because they demonstrate that the event could not have been accurately predicted on the morning of the disaster. In fact, we believe that the slightly negative results from the numerical model are more valuable than if the model perfectly predicted this event.

However, note that the new version of this manuscript contains additional WRF outputs that are now used to describe thunderstorm dynamics, microphysics processes (including the spatial distribution of water species). For instance, the figure below shows the vertical cross-section of the WRF-IFS-DA simulated radar reflectivity along the convective cell that developed in the Genoa region at 11:30 UTC. The shaded colors in the horizontal plan show the air temperature at 2 m above ground. As we mentioned earlier, the WRF still delayed this thunderstorm for approximately 2 hours. The revised manuscript contain discussions that describe this and other figures from the new WRF simulations.

To conclude, we now added to the discussion the simulation that includes the data assimilation on the IFS driven forecast, namely WRF-IFS-DA, to complete the simulations set. The aim of the numerical simulations was to analyze the behavior of operational setups two different initialization. As a result, the

IFS and GFS gave different results and the IFS simulations resulted in a good reproduction of the event especially when the DA is used. The use of an ensemble of simulations at very high resolution using different microphysics and dynamical setup is beyond the scope of this paper. However, recently Parodi et al. (2019) analyzed a macroburst that took place over the same region on 14 October 2016. Their analysis employed an ensemble of kilometer-scale simulations using different microphysics and planetary boundary layer schemes. Despite this, still only few members performed well and those are the one that used a setup similar to the one used in the operational forecast that were replicated in this study. Finally, the revised version of the paper will contain a deeper analysis on the best performing simulation, and the paper by Parodi et al. (2019) will be cited.

Q27: 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.

A27: As already stated, a detailed investigation of the causes of bridge collapse from the engineering point of view is beyond the scope of this paper and this journal. However, it is also true that at this moment, after 1.5 years from the collapse, the cause of this disaster is still not clear. The structure was surely old and not well maintained, therefore it is possible that we are not facing a disaster due to an extreme and exceptional meteorological event, but rather a collapse due to a strong, even if not extreme, weather condition. It might be, therefore, that the common paradigm that a disaster caused by a natural hazard can be triggered only by an extreme event, has to be widened to include non-extreme events when considering elder and weaker structures.

Supplementary references

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