Reply to Reviewer 1

Review of the paper "Reconstruction and simulation of an extreme flood event in the Lago Maggiore catchment in 1868", by P. Stucki et al.

General comments

This paper describes a disastrous hydro-meteorological event that affected Switzerland 150 years ago. Thanks to the availability of the "Twentieth Century Reanalysis" dataset, meteorological and hydrological state-of-the-art models have been applied, to try to obtain quantitative results that complement the documentary/historical data available.

The approach is interesting, and this is probably the earliest event that has been reconstructed/simulated with models, comparing the model results with the earlier observations. The paper is well written and, in my opinion, basically it is worth of publication in NESS. However, I am a bit concerned with the fact that the reanalysis at that time is based on very scarce data (essentially sporadic surface observations) if compared to nowadays situation, so that it is affected by large uncertainties (particularly at small scales and in atmospheric moisture content) that may adversely reflect on the simulations, especially those at high resolution. The latter can be useful since they provide more accurate description of the orography and land properties and of the atmospheric dynamics, but one cannot rely too much upon detailed results of the model simulations: this limited predictability problem at the convective scale severely affects the quantitative precipitation forecasts even in our days. I can only imagine how severe it is at the time of the event! Therefore, I think that the authors should devote more space to try to quantify or at least describe qualitatively if not quantitatively the uncertainties of their results.

Thank you for the appreciation of language, style and content. We are pleased to read that you judge the manuscript worth publishing after revision.

We understand the concerns of both reviewers about inherent uncertainty in 20CR, which might be passed on to the regional model, and the limited predictability of parameters like precipitation. Similarly, we understand the concern about a large spread and smoothing effects. In the following, we respond to both reviewers regarding these concerns.

In an ideal world, we would assess all uncertainties and include an 'uncertainty bar' for each step, as it would be desirable for a risk assessment study. Unfortunately, this is far beyond the possibilities of this study. Yet, we clearly see the need for describing the limitations and uncertainties of the used tools and the obtained results. In the revised version of the manuscript, we do this for each step of the modeling chain in the respective results sections.

Most prominently, we have added an extended quantitative assessment regarding the 20CR ensemble mean and members in Sect. 3.3. First, we address the spread of the 20CR SLP fields over Central Europe, which is mostly below 1 hPa. We show that the isobars remain within a narrow band, and that the ensemble mean represents a middle scenario; it runs always within the bulk of the members. To further assess the uncertainty of 20CR in the region and time of interest, we analyze specific diagnostics for each observation that went into the assimilation. Specifically, we analyze the ensemble spread of the first guess and of the analysis, and we analyze the background departure (obs-bck). Our analysis shows that the observations reduce the ensemble spread by a factor of 0.5 to 0.75. Observation departures are relatively small and no clear pattern appears. In addition, we analyze the smoothing effect by comparing minimum pressure in the ensemble mean to the minimum pressure values from the full ensemble. The minimum pressure values in the ensemble mean lie between the 54th and 57th percentile of the ensemble members. This illustrates that there is hardly any smoothing effect in the pressure fields of the ensemble mean for our region of interest.

The corresponding analyses are shown in Fig. A2 in the Appendix. The analyses support the choice of the ensemble mean as a plausible and realistic depiction of the meteorological situation in 1868.

We also include descriptions of limitations in the subsequent steps of the model chain. Please refer to the specific comments for more detailed information. In summary, the limitations of the weather and hydrological model setups are described in a semi-quantitative or qualitative way. The choice of the WRF setup is now justified at the top of Sect. 3.4. Uncertainties regarding the hydrological modeling are addressed in a separate paragraph in Sect. 3.5. Specifically, the description of uncertainties regarding E-OBS are included in the data section.

Your general comments had us also think about wha t the choices of tools and model setups mean and what the simulation outcomes represent under these circumstances. Following Compo et al., (2011), we regard the ensemble mean as a 'minimum-error estimate', and the results from the hydro-meteorological simulations can be regarded as a well-reasoned proposal of how the weather evolved during the 1868 event. These notions are elaborated in the article, and also appear in the conclusions.

We hope that we meet the expectations of the reviewers with these revisions and adaptations.

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Punctual comments

The Introduction appears a bit too long and detailed – it contains some treatment that should be postponed to the specific chapters.

According to your suggestion (and the same by reviewer 2), we have moved some specific content to the results sections, i.e. concerning the Swiss legislation after the 1868 event, the mesoscale flow modulation on the south side of the Central Alps, and the hydrological response. Because this is an interdisciplinary study, we think we should shortly introduce some important concepts, e.g., political responses, or the role of IVT, PV-streamers, or the zero-degree line.

Page 1, line 36: if AS stands for Central Alps, it should be CA. But in the context of the paper I had the suspect that it means "Southern Alps" (or Alps South?). I suggest, in any case, to use initials that correspond to the English terms.

We have changed it to "SA", which now stands for "south side of the (Central) Alps"

Page 2, lines 1-7: the paper Malguzzi et al (2006) (see also below "References") should be referenced here, with some short description, because it is very similar to the present paper in various aspects: it describes a major historical (1966) flood event that affected the (Eastern) Alps (and not only – it was considered the century flood in Italy). It applies a very similar approach (meteorological and hydrological model chain), although the used reanalysis (and also the verification) takes advantage from a much better data coverage than available one century before!

This is indeed a very useful article with a number of similarities to our study. We use it as a reference in the introduction and at several places in the article. Here, we speak about the particular floods in 1993 and 2000, this is why it is not included at this stage.

Page 2, line 9: LMR: Lago Maggiore Region?

Yes; the information is added for clarification.

Pages 2-3: somewhere in the Introduction (and not only at page 12), the MAP international project must be mentioned (for example quoting Bougeault et al, 2001): its major objective was to study (from the observational and modelling point of view) the atmospheric processes related to heavy precipitation and flood the Alps, with its largest observational effort concentrated just in the Lago Maggiore area.

Thank you for the reference. We added it in the Introduction in the context of heavy precipitation on the south side of the Alps.

Page 3: lines 5-18: regarding the PV streamers west of the Alps, I think that the main interesting aspect (related to the orographic forcing) is that the (orographic) precipitation occurs more to the east, with respect to the position of the PV anomaly, than expected in the case of flat terrain. In other terms and using more traditional synoptic concepts, while in the flat case precipitation has to be expected ahead of the cold front, more or less in the area of the warm conveyor belt, in the case of the Alps (or similar orography perpendicular to the more-or-less southerly flow) precipitation may be heavy also in the warm sector well in advance of the cold front. It must be remembered, in any case, that the orography can change the synoptic scale flow at scales larger than those of the orography itself.

Thank you for the comment, this is a very interesting hypothesis. We decided not to change the text for the following reason: The only paper to our knowledge that systematically looked at the link between WCBs and heavy precipitation on the south side of the Alps finds that a substantial fraction of the heavy precipitation events are co-located with a WCB:

Pfahl, S., E. Madonna, M. Boettcher, H. Joos, and H. Wernli, 2014: Warm Conveyor Belts in the ERA-Interim Dataset (1979-2010). Part II: Moisture Origin and Relevance for Precipitation. Journal of Climate, 27, 27-40.

And we therefore think that we would need to investigate first in more detail if these WCBs are at an unusual location, i.e. further east in the warm sector, before writing such a statement with confidence. We think that this would be a very interesting research question to address although beyond the scope of this paper.

Page 4, lines 19-25: please give some more info here about the 20CR for those not familiar, in particular for what concerns the input observations (other variables besides surface pressure? T and humidity?), the available variables on the pressure surfaces and at the ground) and the degree of uncertainty as estimated in the literature.

We added some information about 20CR here. As mentioned above, we treat the uncertainty separately in the results section.

Page 5, lines 5-8: the application of the nudging, although justified by the need of constraining the forward meteorological model to run close to the reanalysis "trajectory", has drawbacks that should be mentioned: for example, this is not a real "hindcast" experiment – how the precipitation forecast differs from a pure forward integration, i.e. without nudging? (one test should be made at least for one case).

Unfortunately, running a new simulation without nudging is beyond our capacities at this stage. We are currently running such experiments for a potential future study on a more recent event with complicated flow. Nevertheless, we think we can justify the nudging in the specific setup for the 1868 case with the necessity not to let the simulation run too freely. This is now mentioned, and we also mention that the results cannot be regarded as stemming from a pure hindcast experiment.

Page 5, lines 21-25: please explain better the procedure – include some words about the quality/limitations/uncertainties of the E-OBS dataset.

The according paragraph has been extended and now includes some information about the quality

/ uncertainty.

Page 6, lines 15-17: there is similarity with the 1966 event described in Malguzzi et al: in both cases there is strong enough precipitation at the divide and on the downstream side of the Alps to cause flooding also of the rivers flowing on the north side. It is not clear, however, to what extent such precipitation "originates" on the upstream side (due to transport of cloud condensates and hydrometeors across the Alpine crest) or on the downstream side (including possible thunderstorms). Model results (see Fig. 8) do not seem to represent this aspect.

We agree on this comment. In the article, we mention the transport of condensates with stronger winds (e.g. P19, L7ff) and that the model does not fully capture the precipitation patterns and intensites on the north side of the Alps.

Page 6, line 24: it is not clear if only the "inflation" of the Swiss Franc is considered here or also the fact that the nowadays (economic) damages would be much larger due to the real value and vulnerability of today's infrastructures, resources etc. It seems to me the first is the case here, but the second would be more interesting...

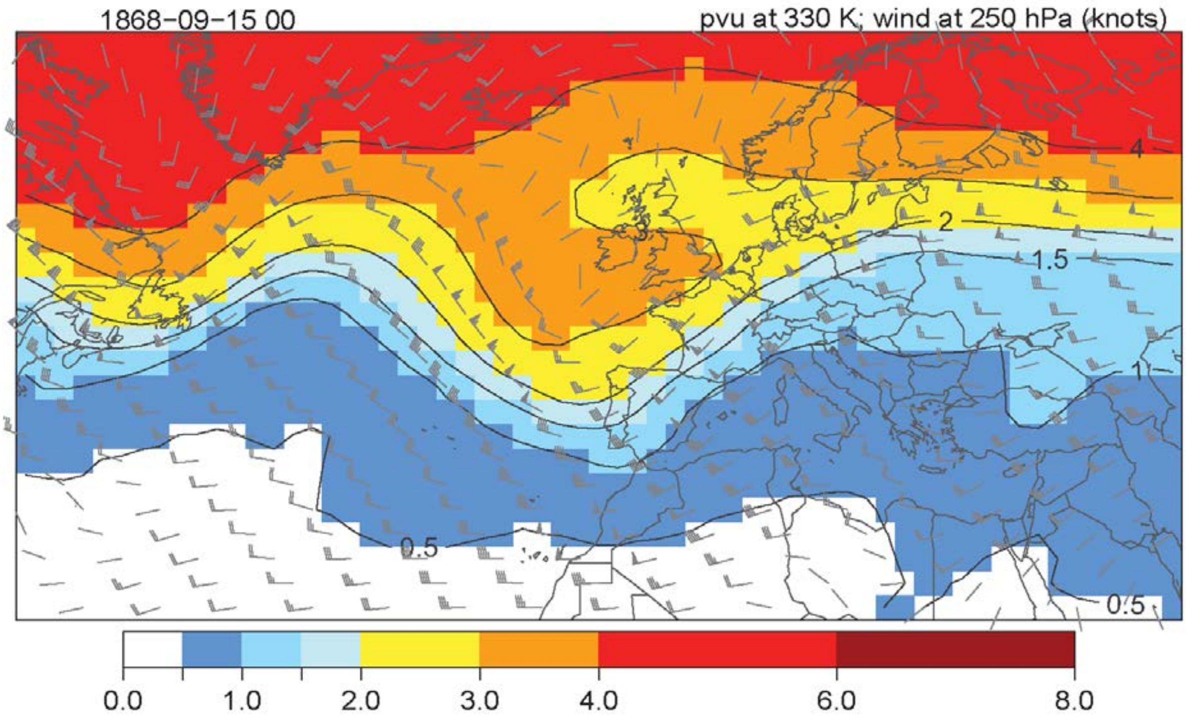
It is inflation only; we added an according clause for clarification, thank you.

Page 8, lines 11-14: I think that the most dramatic aspect, which makes this event so exceptional, is the occurrence of an entire sequence of heavy precipitation cases. Of course, this aspect cannot be "explained" in terms of atmospheric dynamics, even with the help of model simulations.

We agree that the repeated occurrence of similar patterns was decisive for the extreme magnitude of the event. We emphasize this finding in Sect. 3.3 and 4.

Page 8, line 37 (and elsewhere, for ex. page 9, lines 25-26): the expression "Rossby wave breaking" is used several times, but I think that not only it is too technical, it is also ambiguous and should deserve an explanation in more traditional terms.

We agree that for an interdisciplinary audience, this might be very technical. We omit the term in the first occurrence and replace it with a less technical explanation, because we do not actually show the according panel:



In the second occurrence, we leave the term because it is a characteristic feature for such events on the south side of the Alps and shows that even with the limitations regarding resolution and assimilated data, 20CR is capable of detecting such complicated weather variables in a physically consistent way. For the non-specialist reader, we add an explanation of how the breaking manifests in the weather chart (Fig. 6b.)

Page 9, lines 2 and 15; page 10 line 1: a blocking anticyclone (in the traditional meteorological literature) is something different (in brief, an anticyclone located at lat. 50-70 over the Atlantic, Pacific or northern Europe, deviating the westerlies for several days). A ridge downstream of a trough is (I believe) a component of the same Rossby wave, that can become almost stationary. So I think that it not very correct to say that the ridge blocks the easterly propagation of the system.

We deleted this sentence: Until 17 September 1868, a downstream stationary ridge prevented eastward propagation of the system (Figure 6a).

Page 9, line 14: I guess "high-PV", not "low-PV".

high PV trough - thank you for pointing this out

Page 9 line 17: perhaps an "atmospheric river"?

Indeed, this feature resembles an atmospheric river. However, the term is mostly used for structures that are >1500 km long and have constant IVT values of >350 kg m^{-1} s^{-1}. Hence, we prefer not to use the term in our specific context.

Page 9, lines 27-33: qualitatively similar results have been obtained from trajectory computations by Bertò et al, 2004 (among others), for similar events of heavy precipitation on the southern side of the Alps. I suggest to quote this paper (see below the full reference).

Thank you for this reference; their analysis greatly supports our results and we are happy to include it.

Page 9, lines 36-37: I do not think there is a "plausible explanation" – after all, it is not given here.

We can see that this term might be misleading here. The sentence is therefore rephrased.

Page 10, lines 5-8: some more clear should be provided here (or above) about the uncertainties of 20CR.

Two paragraphs and a figure (in the Appendix) are added at the top of Sect. 3.3. See also our reply to your general comments.

Section 3.4 is a bit too speculative, given the uncertainties as mentioned above (see the major comments). If uncertainties affect the low resolution, they can only be larger at higher resolution.

Our line of thought is a little different here. In Sect. 3.3 we come to the conclusion that the ensemble mean is a valid, well-reasoned choice for further analyses (see our reply to your general comment about uncertainty and the reply to the comment on uncertainties in 20CR above). The same, we argue that the chosen model setup offers a valid, well-reasoned proposal of how the weather evolved on a local scale during the 1868 event. The quality of this proposal can then be assessed by comparisons to observations and to analyses of modern analog cases.

Page 10, lines 31 and 33: "well2 and "very good" seem too optimistic, at least in absolute terms (for both amount and spatial distribution of precipitation). I agree that the simulation provide useful meteorological information, perhaps better than expected, but I would use the word "satisfactory" rather than "very good".

We can agree on changing to 'satisfactory' here.

Page 11, lines 7-9: these aspects were investigated during the MAP field experiment (as mentioned below). Here I would quote the paper by Rotunno and Ferretti (2001), providing an interesting explanation of the enhanced convergence due to horizontal inhomogeneities of moisture content at low levels in the incident flow.

This is indeed an interesting investigation, which nicely supports our study, although we do not explicitly look at horizontal differences of moisture content in the low-level flow. We added the reference in the context of the MAP field experiment in the discussion part of Sect. 3.4.

Page 11, line 14: quite typical when convective scale are treated explicitly!

Maybe there is a misunderstanding here. We revised the sentence to:
All these dynamics are consistent with ...

Page 11, line 35: wouldn't it be simpler saying "relatively low relative humidity".

We can see your point, and had discussed it internally before submitting. In conclusion, we would like to keep the current phrasing because the spread is what we actually see in the skew-T diagram.

Page 12, line 32: drop "were".

This was dropped in the final submission.

Page 13, regarding the effects of forests: I agree that forests may not mitigate flooding, however I wonder (but I am not an expert!) if they may act to reduce landslides (a very important cause of casualties and damages - see page 6, line 25) and reduce the amount of mud in flooded water that may worsen the effects.

Thank you for pointing out this aspect. We have added some information about the influence of debris flows.

Page 14, line 8: I think that the moisture flux is not a "precursor" of precipitation like the other factors, since it is almost contemporary to precipitation.

This is correct, thank you for the hint. To keep the metaphor, we added ‘and companions’.

Page 14, line 12: "very well": again too optimistic and not consistent with the results presented here.

We changed the phrasing to ‘are compatible with’.

Figures

Fig. 4: the simulated precipitation seems rather low compared to the observed one. Note also that the Simplon station used to sample the simulation is not included in the station list for the observations – why?

Yes, this underestimation of precipitation is mentioned in Sect. 3.4.
The Simplon observations had been lost somewhere in the process because they cointain NA’s. This is corrected now, thank you for the closer look.

Fig. 6 and 7 are not very clear. This is due mainly to the discontinuous (boxes) coloured fields (shade). This should be avoided using an interpolating graphical package.

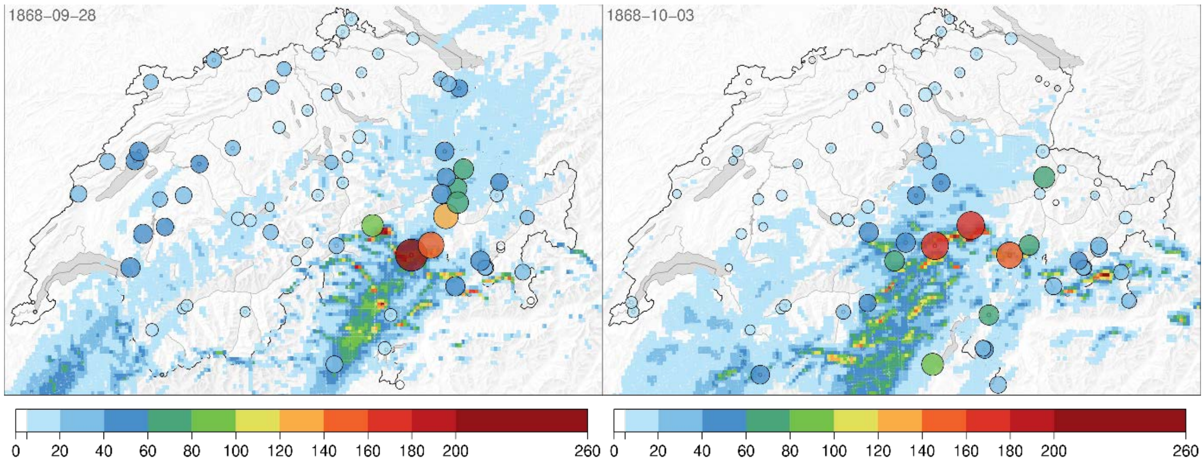
The original shade was used to show the grids of 20CR. However, we can see that this would be at the expense of clarity and readability of the figure. Therefore, we adapted the figures according to the request.

Fig. 7: panel d) suggests the presence of an "atmospheric river".

See the comment above: “Indeed, this feature resembles an atmospheric river. However, the term is mostly used for structures that are >1500 km long and have constant IVT values of >350 kg m^{-1} s^{-1}. We prefer not to use this term.”

Fig. 9a and 9b: it is very difficult to appreciate the differences/similarities with the observations of fig. 5. Perhaps the above figures should be enlarged, plotting on them the observations of fig. 5 with circles.

Thank you for your suggestion. We have indeed tested a number of options for mapping the precipitation obtained from observations and the WRF simulation. One of the options was an overlay map for Fig. 5 (see below). In the end, we decided to refrain from it, for several reasons. We would like to emphasize the aspect of reconstruction and give appreciation to the labor-intense research and digitizing with a stand-alone figure. We would break (and spoil) the narrative of the study, which goes from traditional reconstructions to numerical simulations. An overlay would hide important detail, even with transparent colors. As a workaround, the interested reader may zoom into the two figures side by side on the computer screen.



References

Page 18, line 23: Lago Maggiore (capital).

Adopted, thank you for the correction

Suggested additional references:

Bertò, A., A. Buzzi and D. Zardi, 2004: Back-tracking water vapour contributing to precipitation events over Trentino: a case study. Meteorol. Z., 13, 189-200.

Malguzzi, P., G. Grossi, A. Buzzi, R. Ranzi, and R. Buizza, 2006, The 1966 'century' flood in Italy: A meteorological and hydrological revisitation. J. Geophys. Res., 111, D24106, doi:10.1029/2006JD007111.

Bougeault, P., P. Binder, A. Buzzi, R. Dirks, J. Kuettner, R. Houze, R.B. Smith, R. Steinacker, H. Volkert, 2001: The MAP Special Observing Period. Bull. Am. Meteor. Soc., 82, 433-462.

Rotunno, R., and R. Ferretti, 2001: Mechanisms of Intense Alpine Rainfall. J. Atmos. Sci., 58, 1732-1749.

Thank you for the indications; all requested references are included in the manuscript.