

Interactive comment on “Sulphur-rich volcanic eruptions triggered extreme hydrological events in Europe since AD 1850” by Cristina Di Salvo and Gianluca Sottili

Anonymous Referee #2

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The authors present a study on the effects of volcanic eruptions on extreme precipitation and river run-off in Europe over the last 160 years approximately. They apply a superposed epoch analysis to estimate the changes in the number of extreme precipitation events (2-day extremes) in the years following eruptions relative to normal years. They investigate four rain gauges and river discharge data of main rivers in Western Europe. The study finds a response of precipitation and run-off to volcanic eruptions in all four rain-gauges, with higher extreme precipitation in the mediterranean region and temperate oceanic western Europe and weaker extreme precipitation in continental Europe and in the transition zone. My impression of the manuscript is mixed. The first part containing the statistical analysis of the response to volcanic eruptions seems

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to be broadly correct and the results here are interesting, although the magnitude of the signal does not seem to be very strong. There are a few minor points concerning this section that may require the attention of the authors. However, sections 4 and 5 seemed to me rather poor. My first concern is that section 4 (Discussion) still contains results that have not been included in section 3, and this is distracting. For instance, Figure 4 and Figure are first referred to in the Discussion section. These two figures, however, contain important information for the overall interpretation of the results. More importantly, the Discussion on the role of the North Atlantic Oscillation and North Atlantic sea-surface temperatures is rather shallow, with very speculative reasoning that is not supported by the results presented in this study nor by results of previous published studies. I explain in my detail my concerns regarding this section below as well.

My overall recommendation is that this manuscript requires major revisions, and in the case of the last two sections these revisions should, in my opinion, quite substantial.

In the following list, the relevant sentence or paragraph in the manuscript is copied first, followed by my comment

1.

missing values in the Monte Carlo simulations. The statistical significance of the rainfall intensity changes, ERE25 and ERE10, after SO₂-rich eruptions was evaluated by replacing observed rainfall records with data from randomly selected years through 10,000 iterations.

The Monte Carlo procedure is not totally clear to me. Were whole years resampled as a block, or were individual days resampled? The sentence seems to indicate that entire years were resampled, but what is the rationale for this? By resampling entire years, the values of the highest 25th events are not independent, so that the statistical significance is much more difficult to establish. I wonder if re-sampling individual days or 2-day blocks, from the same season would have been a better strategy.

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different European climate zones. In the MED area, years with $\text{SO}_2 \geq 20$ ppb are characterised by ERE25 intensities higher by 13.5 mm on average (standard deviation of the mean, σ_m , 0.8; $p < 0.03$) with respect to pristine atmosphere years. In the TEMO zone,

What is the meaning of 'standard deviation of the mean' ? If I understood properly, the mean is calculated by taken all years after volcanic eruptions. Is the standard deviation of the mean some type of bootstrap estimation or is it the sample standard deviation divided by $n-1$? The reader would appreciate a clearer language here. Also, what is the meaning of p here ? Is it the level of significance of the differences of the means between post-volcanic years and pristine years ? If yes, the language is unclear.

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values of atmospheric SO_2 . Specifically, in the TEMO zone, the increase of SO_2 annual mean concentrations from 11.9 ± 3.5 to 28.5 ± 7.6 ppb is followed by a factor ~ 2.3 ΔQ_{day} increase. This trend is even more marked in the MED region, where an increase of SO_2 by

'by a factor 2.3 ΔQ_{day} is unclear'. I think the authors mean an increase by a factor 2.3

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Overall, it appears that, the response of rainfall and streamflow intensities to atmospheric SO_2 concentrations defines a composite yet coherent geographical pattern in Europe. In

I think this conclusion is too far-fetched. The study has analyzed four rain-gauges. Considering that rain is spatially quite variable, it is not justified to extrapolate these results to whole four regions

5.

Conclusions We found that, since 1850, high SO₂ atmospheric concentrations are followed, during year +1, by significant delayed responses of both the North Atlantic SST and NAO index (Fig.5). This finding suggests a radiative forcing effect of sulphur-rich eruptions, as we

Figure 5 is quite confusing. It is not really well described in the next, and it is distracting that it appears cited in the discussion section, as I mentioned earlier. I have several concerns: why is the standard deviation of the SSTs calculated over the whole period shown ? It seems to me that the authors want to show a statistically significance difference in the mean of the SSTs in post-volcanic years and in pristine years. The standard deviation is not informative because it is the difference in the mean of two populations. The significance of these differences depends on the magnitude of the difference, the pooled standard deviations and the sample size. This third factor is not included in the figure. The results concerning the NAO (lower panel) do not seem to indicate a strong, or even statistically significance systematic response. The red and black lines are pretty close to each other and most of the time within the uncertainty ranges (= standard deviations of the mean?) Again, I think that this figure s not showing how significant the response of the NAO is, and nevertheless, even the sign of the response changes with time lag. For instance, it seems significant for August and September and then not significant in June, Juy and October. Can this be just a random effect ?. What are the physical mechanism by which the sign of the response may change ?

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by climate models (Driscoll et al., 2012; Charlton-Perez et al., 2013). In this regard, the Atlantic sea surface temperature (SST) is one of the most important governing factors for the NAO and the atmosphere dynamics over most parts of the Northern Hemisphere (Hurrell, 1995). Moreover, the lagged decrease of the NAO index following SO₂-induced

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This paragraph is to me quite problematic. The question of whether SST anomalies are driving NAO variability and atmospheric circulation at midlatitudes in general is being discussed for at least 25 years, and there are studies with very opposing views. This section cites some of the studies by Czaja and Frankignoul and by Wang et al, that do indicate some response to SST anomalies, but there are many others that show no response. For instance, the paper by Sutton and Hodson (Science, 2005,doi:10.1126/science.1112666) indicates that ' However, thus far the evidence for an Atlantic link is mainly circumstantial, being derived from observations and showing correlation rather than causality' . This is a sign that, even in 2005, this question was far from settled, and actually it is not settled yet. Yet, the authors discuss this point rather superficially, as if it were widely accepted that NAO variability is driven by North Atlantic SSTs. Even the studies showing a response of the NAO to SSTs admit that the signal is weak. The paper by Sutton and Hodson identifies a response in the low-frequency band of the spectrum, i.e. not at interannual time scales, which would be the relevant time scales here, but at decadal timescales. Furthermore, the paper by Hurrell et al (1995) does not mention that SSTs are the most important driver of the NAO. Actually, if I understood that paper properly, it does not deal with the driving factors of the NAO. The statistical connections between the NAO and SSTs shown there are interpreted as a response of the SSTs to NAO forcing, which is the opposite interpretation given by the authors here.

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We propose a teleconnected mechanism for volcanically induced extreme hydrological events in Europe. Specifically, the triggering mechanism of extreme rainfall and streamflow events in Europe since 1850 after sulphur-rich eruptions can be explained by sulphate aerosol radiative forcing over North Atlantic causing a net decrease of heat exchange between Ocean and atmosphere through evaporation, precipitation and atmospheric- heating processes. The results of this study display how sulphur-rich eruptions have relevant significance in driving the frequency and intensity of rainfall

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and related floods in Europe, with variable effects in different climate zones.

To be honest, I do not think that this study has shown anything of this sort. It has not analysed radiative forcing, nor ocean-atmosphere heat exchange, nor evaporation, and it has analysed only four rain-gauges in Europe. I found the claims contained in this paragraph completely unsubstantiated.

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