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

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Anonymous Referee #2

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 continen-

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tal 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 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 refered 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 tis 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 r paragraph in the manuscript is copied first, followed by my comment

1. Manuscript text: “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.”

Comment by referee#2: 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 resampling individual days or 2-day blocks, from the same season would have been a better strategy.

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Answer to comment: Our analysis evidenced significant differences in ERE10 and ERE25 intensities between years with high (i.e., $\geq$20 ppb) and years with no detectable SO2 concentrations. The rationale of the Monte Carlo procedure consists in establishing quantitatively the probability, p, that rainfall intensity anomalies observed during years with SO2$\geq$20 ppb may derive from a random sampling of the historical record. Specifically, first we compared: i) the differences between ERE10 and ERE25 values recorded after high-SO2 years and ii) ERE10 and ERE25 recorded after pristine atmosphere years from historical records. Then, we reshuffled the historical record like a deck of cards, thus obtaining 10,000 synthetic time series by reassigning ERE10 and ERE25 values to random years of occurrence. Then we repeat the analysis for the 10,000 synthetic time series, thus obtaining the synthetic distribution of the 10,000 differences between the intensities of ERE10 and ERE25 during years with high (i.e., $\geq$20 ppb) and years with no detectable SO2 concentrations. Unlike the historical record, the randomised time series show no significant (or very low) probability, p, dependence of ERE10 and ERE25 on SO2 concentrations (Table 2).

2. Manuscript text: different European climate zones. In the MED area, years with SO2 20 ppb are characterised by ERE25 intensities higher by 13.5 mm on average (standard deviation of the mean, m, 0.8; p<0.03) with respect to pristine atmosphere years. In the TEMO zone,

Comment by referee#2: 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.

Answer to comment: Yes, the mean is calculated by taking all years after volcanic eruptions. The standard deviation of the mean is defined as follow:

Where N is the number of days, i.e., ten and twentyfive for ERE10 and ERE25, respectively and ïĄş is the standard deviation of ERE10 and ERE25 values, as expressed in mm. As defined above, p is the probability that that rainfall intesity anomalies observed during years with SO2≥20 ppb may derive from a random sampling of the historical record.

3. Manuscript text: values of atmospheric SO2. Specifically, in the TEMO zone, the increase of SO2 annual mean concentrations from 11.9 3.5 to 28.5 7.6 ppb is followed by a factor 2.3 Qday increase. This trend is even more marked in the MED region, where an increase of SO2 by

Comment by referee#2: 'by a factor 2.3 Qday is unclear'. I think the authors man an increase by a factor 2.3

Answer to comment: True, in the revised version of the manuscript we will modify this point accordingly.

4. Manuscript text: Overall, it appears that, the response of rainfall and streamflow intensities to atmospheric SO2 concentrations defines a composite yet coherent geographical pattern in Europe. In

Comment by referee#2: I think this conclusion is too far-fetched. The study has analyzed four rain-gauges. Considering that rain is spatially quite variable, it is nut justified to extrapolate these results to whole four regions

Answer to comment: We fully agree that a higher number of rain gauges could significantly improve the statistical significance of the analysis; actually we considered the longest available time series of rainfall and river discharge in Europe, corresponding to four of the largest hydrological basins in the considered European different climate zones. Our results and conclusions refer to the four investigated basins, and consider some general elements (i.e., intensity of rainfall episoded and associated extreme streamflow intensities) to investigate possible links between hydrological events

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and climatic zone trends (as for earlier ice break –up in TEMC zone). We propose a smoother sentence to describe our general findings, as: “Overall, it appears that, the response of rainfall and streamflow intensities to atmospheric SO2 concentrations defines a composite geographical pattern across the considered basins”

5. Manuscript text: 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

Comment by referee#2: 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 ?

Answer to comment: We agree. In the revised version of the manuscript figure 5 should be presented and discussed earlier. Actually, the standard deviation of the mean, ïĄşmean, as reported in our analysis (sea above definition), fully takes into ac-

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count for both the magnitude of the differences of sea surface temperatures and the sample sizes (i.e., being the latter the number of years). We also note that the NAO values reported in the figure 5 are normalised, in order to filter possible multiyear trends in the NAO variations. In this regard, although there is an element of hazard in evidencing general conclusions from the NAO trend reported in figure 5, it makes sense that the effects of radiative forcing produced by SO2 (i.e., as evidenced by statistically significant differences between pristine year and SO2 polluted years) both on SST and NAO are higher during months with highest solar irradiation (i.e., as also evidenced by the referee#2 "it seems significant for August and September". Concerning the possible random effects of the observed trends, we are confident that the NAO standard deviation of the mean, as calculated from multi-decadal records, provides an evaluable threshold well beyond the NAO random variability.

6. Manuscript text: 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

Comment by referee#2: This paragraph is to me quite problematic. The question of whether SST anomalies are driving NAO variability and atmospheric circulation at mid-latitudes 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 may 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 Hurrel 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.

Answer to comment: We fully agree that, even in 2005, the question if whether SST anomalies are driving NAO variability and atmospheric circulation at midlatitudes. Also, we fully agree with referee#2 as our paper does not deal with the driving factors of the NAO. Our research was partially motivated by the results of a paper by Sottili (Constraints on climate forcing by sulphate aerosols from seasonal changes in Earth's spin; Geophys. Journ. Int, 2014 197, 1382–1386) concluding that, if we only consider the Earth's surface cooling induced by aerosol radiative forcing, we largely underestimate the effects of aerosols on atmospheric circulation. Specifically, the energy budget of atmospheric kinetic energy after large eruptions should be coherentely explained only by assuming a strong influence of sulphate aerosols on partitioning the available energy into the atmosphere; for example, by assuming a strong inlfluence of sulphate aerosols on affecting the latent heat release and transport during condensation–evaporation–freezing cycles (Sottili, 2014). The futher steps must also take into account the widely reported decrease, both from historical records and climate modelling, in global mean precipitation by volcanic aerosols, as explained by the stabilization of the atmosphere due to the reduction of short-wave radiation reaching the surface. On these grounds, during the early stages of analysis, by considering the "classical" hydrological parametrs (i.e., mean annual and monthly precipitations and streamflow data, etc.) we found poorly or not significant evidences on volcanic forcing of the European hydrological cycle. After this initial and sterile stage of analysis, when the lack of a sig-

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nificant SO_2 forcing seemed to contradict the hypotheses by Sottili (2014), we decided to explore an alternative path, as we investigated this issue from a kinetics point of view i.e., by considering the short-terms, daily, effects of SO_2 on rainfall and streamflows intensities. This meant to face a much more difficult path for three main reasons: 1) the significant reduction of available data (daily records) from hydrological datasets; 2) no possibilities of comparison of our analysis on short term hydrological paramenters with available climate modelling outputs on rainfall and streamflow data; 3) the need of reconciling the clear contradiction between the reported global-scale "stabilization effects" of SO_2, due to the reduction of short-wave radiation, and our results clearly showing an enhancement of short-term, extreme hydrological events both in terms of rainfall and streamflow intensities. On these grounds, we proposed a mechanism of interaction between SO_2 and short-term extreme hydrological events involving a continental-scale redistribution of atmospheric available energy, with the SST (or, more precisely, with the amount of thermal energy stored in the Ocean) playing a key role in reconciling the contradiction between the reported long-term, global-scale "stabilization effects" of SO_2 and our findings showing a clear enhancement of short-term extreme hydrological events. Although our interpretation about the role of SST and NAO needs further studies, hopefully from climate modelling and independent datasets, our contribution intends to stimulate the scientific debate on this issue.

7. Manuscript text: 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 rainfalland related floods in Europe, with variable effects in different climate zones.

Comment by referee#2: 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.

Answer to comment: As discussed above, we fully agree with the referee#2 about the need of considering the radiative forcing as a key driving factor, which can also play a role in determining the observed pattern of extreme hydrological events. However, we again remark that the response of precipitation and run-off to volcanic sulphate aerosols eruptions does not indicate a stabilization of the atmosphere; rather, we observed a significant increase of extreme events strongly suggesting an increase of hydrological cycle dynamics (i.e., the opposite trend we should expect after the reduction of the radiative heating of atmosphere by sulphate aerosol forcing). In addition, we believe that short term changes of hydrological cycle dynamics (i.e., inter-annual higher extreme precipitations and related river discharge rates), are mechanism which are not yet well constrained by climate models, since they account for phenomena occurring at longer time scales (months rather than hours) and wider spatial scale (i.e., not considering micro-climatic effects occurring at basin scale). On these grounds, we are confident that the triggering mechanism for extreme events by volcanic SO2 is driven by rearrangement of available heat energy into the atmosphere (see for example Sottili, 2014).

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