Dear Manuela Brunner,

First, we would like to thank for you for handling swiftly this manuscript.

We carefully addressed all the reviewer comments. Notably, we added new analyses about the regional significance of the different findings of the paper, which strengthened the results, thanks to the reviewer comments. They include: a regional test on changes in flood dates, and also on flood characteristics, a Poisson regression to assess the significance of changes in the occurrence of weather types, and a Monte Carlo experiment to show that the choice of classification thresholds has no influence on trends in Excess and Short rain floods occurrence.

The response to the reviewer comments is a bit different now than in the online discussion, since we have included more results since then. Given that the new results required several new figures, we choose to add several ones as supplementary materials to not overburden the manuscript.

About novelty:

- We analyzed not only flood event types but also their characteristics in terms on seasonality, intrinsic events properties such as duration, runoff coefficients, antecedent soil moisture...
- We linked the occurrence of floods in this region to several well-characterized weather types
- We assessed both the evolution in time of a) flood-events characteristics (runoff coefficients, soil moisture...), b) flood event seasonality in relation to large scale atmospheric patterns, c) flood-events types, c) magnitude of floods according to different event types.

There is no equivalent study with such a good quality database in the Mediterranean region, where floods have very strong impacts and are the main natural hazard. We believe this type of study, at the regional level, is complementary, and necessary, to reinforce the findings obtained in large scale studies (using EOBS or model-based data). Notably, changes in flood seasonality, related to changes in weather types leading to rainfall, have not been documented before our study in Southern Europe, to the best of our knowledge.

All figures are made to be readable by color-blind people.

Kind regards

Yves Tramblay

Response to reviewer comments

Reviewer 1

The manuscript "Changes in Mediterranean flood processes and seasonality" by Y. Tramblay et al. is an interesting study analysing changes in flood event characteristics, flood types and their seasonality in 98 catchments in Southern France. The results presented are coherent with other recent literature about flood changes in the Mediterranean and demonstrate and confirm that soil moisture is the primary driver of flood changes in this region. The manuscript is overall well written and logically organised. Please find my comments below.

We would like to thank you for reviewing our work and your positive feedback.

Major comments:

My main concern is about the reliability/suitability of the reanalysis product used for the retrieval of precipitation and soil moisture information for the catchments (L135-136). What is the spatial resolution of this product? Is the spatial resolution fine enough for the relatively small catchments in the analysis? At L394-395 the authors state "[…] despite the large sample of basins considered, the patterns are consistent and homogeneous across different basin sizes and locations". Could this be due to the (coarse) spatial resolution of the reanalysis data compared to the (small) size of the catchments?

It is true we forgot to mention the spatial resolution of the French reanalysis, thank you for pointing this, we added in the revised manuscript: "The SIM product is available at 8x8 km ($64km^2$) and is considered the reference dataset for hydro-climatic analyses over France". SIM reanalysis assimilates the data of about 4000 meteorological stations in the metropolitan France. Actually, most basins are much larger than one pixel size (83% of basins) and the mean catchment area is 480 km^2, so the results are not influenced by a potential scale mismatch notably due to the fact we are using mean areal rainfall.

Specific comments:

L46-47: "[…] the mean flood date being on average advanced by one month". Please specify that the shift refers to two sub-periods.

We added "between 1959-1990 and 1991-2021".

L116-117: I suggest adding in the introduction (and discussion) one recent study by Tarasova et al. (2023) about changes in flood processes in Europe. Tarasova, L., Lun, D., Merz, R. *et al.* Shifts in flood generation processes exacerbate regional flood anomalies in Europe. *Commun Earth Environ* 4, 49 (2023).https://doi.org/10.1038/s43247-023-00714-8

Thanks for this suggestion, we actually discovered this highly relevant paper that has been published online just a couple of days after ours was published in the HESS discussion. Very similar conclusions have been reached, about the decrease of floods induced by wet soil conditions and an increased proportion of short-rain floods in Mediterranean basins. Given that both studies are using different approaches and data, it reinforces the robustness of the findings for the Mediterranean region.

L155: in other words, did you adopt a peak-over-threshold (POT) approach?

Yes, we added it in the text.

L163-167: it's unclear how the maximum precipitation is calculated. Is it the maximum daily precipitation within the same time interval where total precipitation is calculated?

Yes, we added "The maximum daily precipitation is extracted from the same time interval used to compute total event precipitation".

L213: does the first period start in 1959 or in 1950 (as stated in the abstract)? Please check.

1959, thanks for pointing this out, we corrected in the abstract.

L214-215: it is not clear how the pivot year is selected and used in the analysis. Is the extension of the two periods always the same in all catchments (as also shown in all figure legends) or does it vary? Please clarify.

No, it is not exactly the same in the different catchments. We choose different dates, depending on data availability, to ensure that the two sub-periods have the exact same length. As indicated in the method section, this pivot year is for the majority of basins within 5 days around 1991 for all basins. But actually, since we compute the regional significance on the whole time series using a MK test, this pivot year has whatsoever no influence on the results and it is just a matter of presenting the relative changes in maps and tables.

L230: I suggest renaming this section "Results and discussions" as it also contains, alongside with the results, a considerable amount of interpretation of the findings in the context of the literature.

We agree, since we actually merged results and discussion in this section.

L234: please specify that the changes refer to the difference between the two periods. This also applies to subsequent occurrences in the manuscript, especially in the caption of the figures, where it is not always clear what exactly "changes" refers to.

We added: "between the two sub-periods, 1959-1990 and 1991-2021". Also, in the figure captions.

L256: how is the runoff coefficient calculated for each event?

We added in the method section: “the runoff coefficient was computed for each event as the ratio of direct runoff depth and total event precipitation”.

L258-261: Correlations between antecedent soil moisture and runoff coefficients are analysed and reported in the text. I suggest adding a table or adding these results as a panel of figure 3 (or modifying figure 3) to make it easier to follow. The same suggestion (i.e. adding a table/plot) for L395-399 and L401-405.

L258-261: we added a boxplot of the correlations in supplementary Figure S1 since this information is a bit redundant to figure 3.

For L395-399, we are just giving two correlations (ie. between the % of excess rain or short rain with catchment size), that are actually quite low and not very informative (this is why we added the correlation coefficient, to show that despite significant correlations, the values are low). Given the already high number of figures we think it is not necessary here. We also added in the text that this correlation is low and probably not very relevant.

For L401-405, we are talking about the basins where the percentage of excess rain floods is very high, we added a figure S2 in supplementary materials.

Figure 2: I suggest inverting the colours of the colorscale and adding the units of the relative change to the axis.

We changed the figure.

L293: a mountain range is mentioned. To facilitate reader that are not familiar with this area I suggest adding a label to the map of Figure 1 to locate the mountain range.

We changed the figure.

Figure 4: the coloured dots look all a bit brownish and therefore the map is not so easy to read. I suggest making the colours brighter.

We changed the figure.

L330: “Association between flood occurrence and weather patterns”. How is the association done? Is the WT selected based on the date of occurrence the flood peaks or the preceding days? Please specify it here or in the method section.

We added in method section 3.2: “To associate flood events and weather types, for each rainy day corresponding to flood events, the weather type has been extracted from the weather types classification.”.

L340: “Change in seasonality (of what? Of floods?) can be ascribed to changes in the seasonal occurrence of the weather types”

We added “floods”.

L332-347: please check coherence of WT numbers and names in the text and in figure 6. The WT numbers and names seem to be different in some occurrences in the text and in the figure. E.g., WT2 is “Atlantic circulation” in the text but “Steady Oceanic” in figure 6, where Atlantic is WT1 instead.

Thank you for pointing this out. The figure is correct, not the references to WT names in the text. We fixed it.

L340-347: the described changes are tiny in figure 7 and therefore do not seem very significant in the context of the description and interpretation of the results. There are other larger changes in figure 7 that are instead not described. Perhaps figure 7 could be further discussed.

Indeed, statistically these changes are not significant according to the CHI2 test on monthly counts. But they are if considering the seasonal number of events between March-August, using a Poisson regression to assess a potential trend over time. See Supplementary Figure 2.

It must be reminded here that we are dealing with extreme events. So, a small number of events. We argue that even small changes in the frequency of flood-inducing weather types might have an impact on flood frequency. And beyond the “statistical significance”, we don’t know exactly how these changes in numbers may affect seasonal shifts in flood frequency. For example, in August, the frequency of WT4 increase from 8% to 11%, similarly in June from 11% to 14% and overall, this represents an increase of +69 episodes (so 2 per year, that has to be related to the mean occurrence of floods in our study, one per year on average).

We added this discussion in the text, also following recommendations of Reviewer n°2.

L402-404: “For short rain and long rain, the maximum contributions observed are 36% and 32%, respectively, but these maximum values are only found in small basins.” Do these findings refer to the same 30 basins mentioned above?

No. We changed the sentence to be clearer.

Figure 7: please add a label to the vertical axis

There is already a label: “Frequency”

L513: “[…] related to higher evapotranspiration rates” could you add a reference?

We added Tramblay et al. 2020, already in the reference list.

L470: please specify how the regional distributions in fig 12 are obtained.

We added: “Given that there are different flood sample sizes in the different basins corresponding to different flood-generating processes, we pooled regionally the events. To do so, we computed the specific discharge for each event (i.e. the flood magnitude divided by catchment area)…”

L419 and L426: the word “flood drivers” is here introduced and I believe it refers to the flood types mentioned in the rest of the manuscript. Please use consistent terminology to avoid confusion.

We agree and replaced by “flood types”.

L423: is the peak in January or February? (I think February is correct, fig 9)

True, we changed to february

L426: where are the long-term changes shown?

We added a figure S3 in supplementary materials.

Reviewer 2

The manuscript titled "Changes in Mediterranean flood processes and seasonality" by Tramblay et al. investigates how flood types and seasonality evolve in Southern France during the past 50 years and attempts to link the change in flood seasonality to changes in mechanisms. Overall, the logic is clear and the results are well presented. However, deeper scrutiny reveals some issues that, in my opinion, might undermine the quality of the manuscript in its current form. My main concerns are the robustness of the results and the contribution of the study, which are relatively weak in the present manuscript.

We would like to thank you for the review of our paper and the constructive comments to improve the presentation of the results.

Firstly, in the introduction part, the authors argue that "Most of these studies rely on flood classification schemes, with various complexity depending on the type of data available, allowing a data-based separation of floods into their distinct generation mechanisms", which I agreed with. While I assumed that the study would propose an improved approach to partially overcome the current limitations of the flood classification scheme (e.g., the relatively subjective threshold selection, etc.), it seems that the study only used a simple decision tree with hard thresholds, without any justification or discussion of the threshold selection (Tarasova et al. 2020; Zhang et al, 2022). Currently, all the results are based on the seemingly arbitrary threshold and structure of the decision tree. For example, the authors use "50% saturation", "95th percentile of rainfall", etc. as the threshold to distinguish the "excess rainfall", "long rainfall", and "short rainfall", while if some events are distributed around these critical points, the conclusion about their changes might be quite sensitive to these values. I wonder if there are some sensitivity tests to ensure the robustness of the results.

The statement made by the reviewer: "Currently, all the results are based on the seemingly arbitrary threshold" is not correct. The results presented in section 4.1, about changes in flood events characteristics, in section 4.2 about changes in flood dates and in section 4.3 about the association between flood occurrence and weather patterns do not depend upon the flood event classification. Only results from the sections 4.3 and 4.4 are using the flood classification.

We agree with the reviewer that we did not provide enough information about the sensitivity of the results to the threshold values used in the classification. We want to stress that we do not aim in the present study to introduce an improved approach to classify floods events. Instead, we provide a regional focus in the Mediterranean, where floods have very strong impacts, using a well-used classification (to allow inter-regional comparisons) applied globally, in Stein et al. (2020), over the Continental USA in Stein et al. (2021) and over Africa by Tramblay et al. (2022). We indeed used very basic thresholds, yet adapted to the processes analyzed: the 95th percentiles for extreme precipitation and a threshold of 50% for the soil wetness index to define wet/dry conditions (these thresholds, relevant for these processes, are widely used see a recent example in Nanditha, J. S., & Mishra et al. 2022).

Following the reviewer advice, we implemented a Monte Carlo experiment, based on Latin Hypercube sampling to analyze the sensitivity of the regional changes in flood types to the thresholds used in the classification (using 5000 experiments with these threshold ranges: Extreme rainfall [0.7 1], Soil Wetness Index [0.4 1]). The question we want to address here is: are the changes we detected in the regional flood types dependent on the thresholds used in the classification? The response is no, as shown in the new Figure 2 we added in supplementary materials, we have a decrease in excess rain floods and an increase in short rain floods that are not dependent on classification thresholds. Even if considering quite large threshold ranges. On the opposite, for long rain floods, we can see that the confidence interval includes zero, so the changes are not robust. There are indeed interplays between the "excess-rain" and "long-rain" categories, with the soil moisture threshold increasing, less events are classified into "excess-rain" and more are classified into "long-rain". The results of the sensitivity analysis have been included in the revised manuscript.

Nanditha, J. S., & Mishra, V. (2022). Multiday precipitation is a prominent driver of floods in Indian river basins. Water Resources Research, 58, e2022WR032723. https://doi.org/10.1029/2022WR032723

In addition, the manuscript argued that "Yet, beside the trend detection, no study has provided an in-depth analysis of the long-term evolution of flood processes in these regions". This seems to be a very strong statement. However, a recent study by Jiang et al. (2022) has examined the change in flood mechanism in 1000 catchments in Europe, which also include catchments in southern France. The study showed similar trends in flood generation processes as in the present manuscript. I think it would make more sense to compare the trend results with other literature.

We agree and we modified the statement to be more specific: "Yet, beside trend detection or changes in flood types, no study has provided an in-depth joint analysis of the long-term evolution of flood processes in Mediterranean basins, in relation to their drivers such as precipitation, soil moisture and the evolution of synoptic weather patterns associated with floods".

We acknowledge that it is important to compare results in different studies to see if different approaches can provide similar conclusions. Besides large-scale studies, it is also interesting to have a deeper look at the regional level with good quality datasets. For instance, the European dataset EOBS over France includes about 120 meteorological gauges, and the SIM product, used in the present study, over 4000. It is very important to document if the same types of relationships are observed across different scales and different databases, notably to assess the relevance of these large-scale datasets in data-sparse regions.

The reviewer seems to consider that "the contribution of the study is weak", notably in relation to the recent work of Jiang et al. 2022. The study of Jiang et al, 2022 is mostly centered over Central and Northern Europe, using EOBS precipitation and temperature and river discharge. They include about 38 basins in South France (from their map), 13 in the Iberian Peninsula and 1 in Italy, south of the Po basin, so for a total of 1000 basins, Mediterranean basins represent 5.2% of the basins studied. It is interesting to note that we obtained similar results about the reduction of Excess rainfall floods (as in Tarasova et al., 2023), but Jiang et al. (2022) did not consider changes in flood seasonality, flood magnitude depending on event types, or flood characteristics such as runoff coefficients, as we did in the present study.

We added in the introduction: "Recent large-scale studies (Jiang et al., 2022, Tarasova et al., 2023) suggested a reduction of flood driven by soil saturation, including basins in the Mediterranean area."

Also, my concerns about the contribution arise because the conclusion is not very strong currently based on the significance test results. Although the authors argued that it is due to the short records of samples and interannual variability, it would impair the reliability of the conclusion somehow, particularly in the case of lack of sensitivity test for the method.

About significance testing, as shown on figure 11 the regional changes in the frequency of flood event types are significant for the reduction of excess rain floods and the increase in short rain floods. It should be reminded here that we are dealing with extreme events in the Mediterranean hydro-climatic context, with a strong year-to-year variability. Therefore, at the basin scale, dealing with a small number of extreme events, it would be quite difficult to see local 'statistically significant' changes given the small sample sizes. More, a multiplication of local tests is not the best approach (Wilks, 2016). This is why we adopted a regional analysis to check the significance of the regional changes. As noted above, we also included in the revised manuscript a sensitivity analysis to the threshold values.

In addition, to put more strength on the results, since the main question is whether the detected changes are regionally significant, we also added two additional regional significance tests:

1. To assess the regional changes in flood events characteristics (the total and extreme rainfall, runoff coefficients, contribution of baseflow, flood duration and soil moisture), we performed a regional pooling of the events and applied the Mann-Kendall test to detect trends in the regional series of event characteristics. As shown in the table below, all the detected changes are regionally significant except the decrease in base flow contribution to peak discharge during floods. We added this result in the revised manuscript.
2. To assess the regional changes in flood dates, we first separated in two regional samples the stations where floods tend to occur earlier (sample 1) or later (sample 2). Then we used the Watson-William test, previously used to assess changes in flood dates in each station, to compare these two regional samples. The test results indicate that for the 19 stations where floods tend to occur later, the change in flood dates are not significant at the 5% level (p value = 0.0821), on the opposite, for the 79 stations where floods are occurring earlier, the change is significant (p value = 5.34.10^-8).
3. To assess the changes in the Weather types frequency over time, we added a Poisson regression model to test whether there are trends in: the March-August number of weather types n°4 and the January-March number of weather types n°2. In both cases, the increasing trend is significant.

We added these results in the revised manuscript. So, the point raised by the reviewer has been addressed.

Wilks, D. S. (2016). “The Stippling Shows Statistically Significant Grid Points”: How Research Results are Routinely Overstated and Overinterpreted, and What to Do about It. In Bulletin of the American Meteorological Society (Vol. 97, Issue 12, pp. 2263–2273). American Meteorological Society. https://doi.org/10.1175/bams-d-15-00267.1

References:

Tarasova, L., et al. A process-based framework to characterize and classify runoff events: The event typology of Germany. Water Resources Research, 56, e2019WR026951 (2020). https://doi.org/10.1029/2019WR026951

Zhang, S., et al. Reconciling disagreement on global river flood changes in a warming climate. Nat. Clim. Chang. 12, 1160–1167 (2022). https://doi.org/10.1038/s41558-022-01539-7

Jiang, S., et al. River flooding mechanisms and their changes in Europe revealed by explainable machine learning, Hydrol. Earth Syst. Sci., 26, 6339–6359 (2022), https://doi.org/10.5194/hess-26-6339-2022.

Other comments follow:

Abstract: what do the “flood event characteristics” mean? Please specify.

We added: “Further, several flood events characteristics have been computed: flood event durations, base flow contribution to the peak, runoff coefficient, total and maximum event rainfall and antecedent soil moisture”.

L131: What is the spatial resolution of the SIM reanalysis data used? Please clarify if the size of the study catchments is comparable to the spatial resolution of the hydrometeorological data sets.

The spatial resolution is 8x8km (we added this information, that was missing, in the revised manuscript). SIM is the best available product based on observations covering France, since 1958. 83% of basins considered in the study have a size larger than 64km², the mean catchment size considered in the present work is 480 km².

L133: Please clarify how the nival regime is identified from the river discharge hydrographs. It is also not clear why they should be removed, since even snow-covered catchments can also be affected by rainfall, e.g. rain-on-snow events.

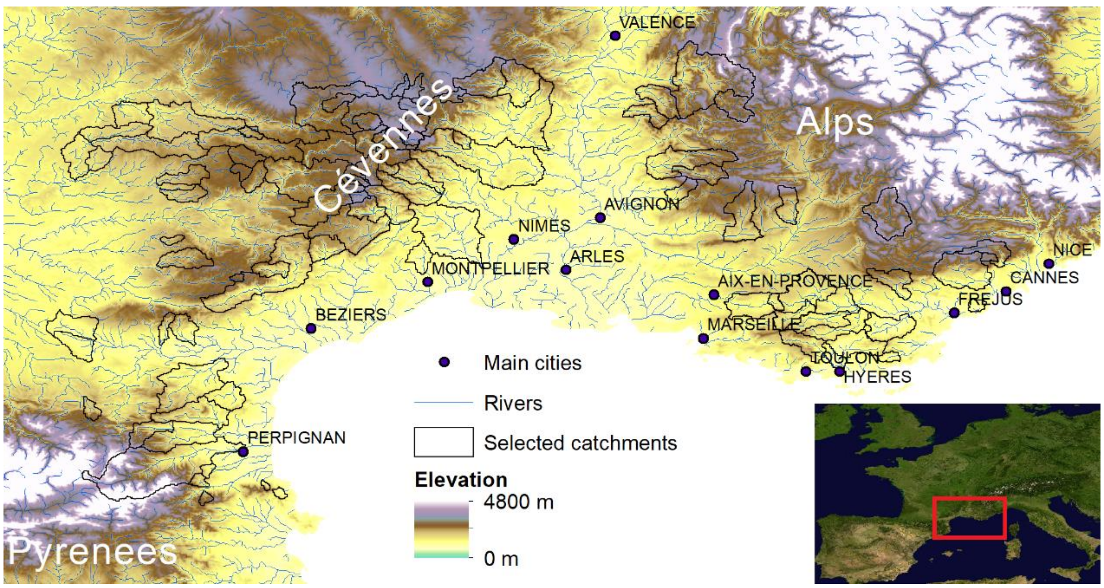
This is explained in the section 2. We excluded basins with a nival type of hygrographs (ie. snowmelt-driven peak discharge in spring) and with more than 20% of precipitation as snow. This is mainly since for French Mountainous basins, the SIM product may not be optimal source of data notably for extreme rainfall, see Gottardi et al 2012 and Blanchet et al. 2021. The focus here is on Mediterranean floods induced by rainfall.

Gottardi, F., Obled, C., Gailhard, J., & Paquet, E. (2012). Statistical reanalysis of precipitation fields based on ground network data and weather patterns: Application over French mountains. In Journal of Hydrology (Vols. 432–433, pp. 154–167). Elsevier BV. https://doi.org/10.1016/j.jhydrol.2012.02.014

Blanchet, J., Blanc, A., & Creutin, J.-D. (2021). Explaining recent trends in extreme precipitation in the Southwestern Alps by changes in atmospheric influences. In Weather and Climate Extremes (Vol. 33, p. 100356). Elsevier BV. https://doi.org/10.1016/j.wace.2021.100356

Figure 1: It would be better to add an inset map to show the location of the study area in Europe.

We provided a modified map:



L155: I understand the reason for using POT1 instead of AM, but please clarify if this will affect the subsequent trend analysis.

It does not have an impact on the results. The resulting distributions are almost identical, and actually numerous papers are using this type of sampling. See some recent ones:

https://doi.org/10.1038/s43247-021-00248-x

https://doi.org/10.1002/wat2.1520

https://doi.org/10.1016/j.jhydrol.2019.05.054

https://doi.org/10.1016/j.jhydrol.2021.126994

L164: Please specify whether rainfall is the precipitation that excludes snowfall.

No, we use total precipitation. But as mentioned above, the contribution of snow is small so it is not irrelevant to use the wording of “rainfall”.

L165: Is the precipitation on the same day as the flood peak considered, please clarify.

Yes, we added it in the text.

L174: How is the duration of the flood event calculated, i.e. how are the start and end points of the event determined? Also for the runoff coefficient calculation process.

We slightly modified the description in the method section to: “Then, for each flood event, we computed the total rainfall and maximum rainfall. The *n*-day previous precipitation is extracted. Total rainfall for each event is estimated by a cumulative sum of precipitation starting the day of the flood and this aggregation stops if there are two consecutive days with precipitation close to zero (1 mm) to account for rainfall intermittency within events. The maximum daily precipitation is extracted from the same time interval used to compute total event precipitation.”

For runoff coefficients we added: “The runoff coefficient was computed for each event as the ratio of direct runoff depth and total event precipitation”.

L183: How is September 1 of the hydrologic year determined?

In the Mediterranean, the summer is dry. It is quite standard practice to consider that the hydrological year in this region starts in September.

L197-206: As I noted earlier, the classification reads rather arbitrary.

See our previous comment.

L207: What does "other" mean?

It is the name of the category of events that cannot be classified. As mentioned, this proportion remains very small.

L220: What test was used to check statistical significance?

As written the line above: the Mann-Kendall test for trends.

Figure 2: what is the unit of "relative change"? I also suggest showing the statistical significance of these changes in the map.

Relative change is unitless. If you multiply by 100 you get a percentage. We added in the caption: [-].

We choose to not add the information about local significance in the maps, with 6 sub-panels, 98 points per panel it would be impossible to see.

Table 1: one-tailed or two-tailed test? please specify.

Two-tailed. We added it in the method.

L262-264: How is the conclusion related to the results?

As shown in Figure 3, there is an increase in runoff coefficient with increased soil moisture.

L269-273: I would expect a figure to support the results.

We added a figure S1 showing the correlations in supplementary materials.

Figure 3: please give the number (of events?) in each bin.

We modified the figure to include the number of events

Figure 4: not quite sure if POT1 events instead of AM events will introduce a bias in the flood dates.

It is not clear whether this is a question or a comment. With POT we have floods in the sample, while with AM it is possible for some very dry years to have an annual maximum that is not really a flood event. Over all, best practice is to avoid introducing this type of events in the sample so POT is preferable (https://doi.org/10.1038/s43247-021-00248-x).

L298-302: it is difficult to follow without showing these basins on the map.

In these lines, we mentioned a few cases where we observe a second minor peak of occurrence for floods, we added the following figure in supplementary materials.

Figure 5: Please also show the results of the significance test in the maps.

Same comment as above, it would be difficult to see. As replied above, we added a regional significance test since the important question here is whether these changes are regionally significant or not.

L304: Which time period (1991-2021 vs. 1959-1990) was referred to?

We added: “1958-1990 and 1991-2021”

L341: I would like to add significance tests on the difference between the proportions, which would be more supportive of the discussion.

We have now added a Poisson regression to assess the significance of the trends in weather patterns occurrence. Se our response above.

L398: I think we should be cautious about this conclusion, given the relatively high p-value and low correlation coefficient.

We agree and changed the text to: **"**There is a significant, yet low, correlation ($\rho = 0.26$, p-value = 0.008) between the ratio of excess rain floods and catchment size, with a larger proportion of excess rain in larger basins, while on the opposite there is an even weaker and negative correlation ($\rho = -0.16$, p-value =0.09) between the ratio of short rain and basin size."

Yet, we would like to stress that we do not provide any strong conclusion here, just showing the results.

L431-434: How statistically significant are these results?

See one of the first comment above. Catchment-by-catchment it would be difficult, if not irrelevant, to compute the 'statistical significance' given the small sample size. See the results of Figure 11 showing the regional changes, showing that regional changes in the frequency of excess rain and short rain floods are significant. We assess the regional significance in section 4.5.

Figure 10: Please also include the significant test results. I am also not clear how the change in frequency is calculated for each catchment. Do you compare the frequency in 1991-2021 with the frequency in 1959-1990?

Yes, we compare the frequency of the different flood types in the two periods. The regional significance of these changes is shown in figure 11. Again, the scope of the study is to assess whether these changes are significant regionally and not necessarily catchment-by-catchment. Given the small numbers, notably of short rains in some basins, the regional approach is much more relevant.

L445: How was the conclusion reached? I don't understand the logic. I am not surprised that the trend level is not consistent. Even a small change in the driver magnitude can lead to a change in the flood type (because the threshold leads to a hard boundary in the classification, that's why I asked for a sensitivity analysis).

As shown above, the results are not strongly dependent on these thresholds, that are actually quite standards to define precipitation extremes or soil moisture state. What we show here, is that trend magnitudes can differ from one catchment to another, given different catchment sizes, land use, the presence/absence of groundwater contribution, so this result seems absolutely expected. As you requested, we included a sensitivity analysis and the main conclusions are not affected by threshold selections.

Figure 11: Please show the 25th and 75th percentiles of the regional frequency to show the spatial variance.

On figure 11 are plotted the annual observed frequency of the different flood types. So, it relies on counting for each year the number of Excess rain floods, Short rain floods and Long rain floods, in all basins, and this count is divided by the total number of events each year. It is not the mean frequency over all basins here, so the computation of 25th and 75th percentiles is not applicable.

L477: "This is mainly due to a decrease in the specific discharge of short rain floods" I can't understand it, because if the short rain has been observed intensified, if we only consider short rain floods, it is more reasonable that the flood magnitude will also increase.

No, the short rain floods are not intensifying, They are just more frequent.

See Figure 12. On average floods induced by short rains have a larger specific discharge than other types of floods. Over time, there is an increase in the number of floods induced by short-rains. But, in the same time, the magnitude (or severity) of this type of floods does show a slight decrease, and not an increase, in terms of specific discharge.

We rephrased this section to =

“Given that there are different flood sample sizes in the different basins corresponding to different flood-generating processes, we pooled regionally the flood events. To do so, we computed the specific discharge for each event (i.e. the flood magnitude divided by catchment area) to analyze the distributions of specific discharge for all the events associated with excess rain, long rain or short rain. Specific discharge is used herein since it is a good indicator of flash floods severity, notably in this Mediterranean region (Delrieu et al., 2005, Ruin et al., 2008). Figure 12 shows that the short rain floods are more severe, in terms of specific discharge, than excess rain or long rain floods at the regional level (as shown also by Tarasova et al., 2023). The regional distributions are different according to the Kolmogorov-Smirnov test. It must be noted that for a given basin the magnitude of the different types of floods may not be very different, showing the strong variability from one event to another that is not solely linked to the flood trigger. When comparing the different flood distributions between the time periods 1959-1990 and 1991-2021, the differences in flood magnitudes between excess rain, long and short rain are reduced. This is mainly due to a slight decrease in the specific discharge of short rain floods, notably for flood events with a return level higher than 10 years, while the excess rain floods show very little changes in intensity over time. “

Reviewer 3

This paper shows that floods in southern France have generally been coming earlier in the wet season (water year in the U.S.) over the last 60 years, and while the associated precipitation extremes have increased, there are no corresponding trends in flood magnitudes. The explanation (which I find reasonably convincing) is reduced antecedent soil moisture, due to the shift of events earlier in the year (and perhaps also a general drying of soils associated with climate warming). The paper constitutes yet another bit of evidence that while precipitation extremes mostly are increasing, there is little evidence of concurrent increases in flooding.

We would like to thank you for the review of our manuscript.

My main misgiving about the paper (which may well not affect the overall results of shifting of floods earlier in the year, without magnitude trends) is their approach to segregating events by flood generating mechanisms. It seems a bit peculiar that the classification of the majority category is based on antecedent soil moisture, but the other two on precipitation intensity only (short and long duration, respectively). I would be inclined to lump all the events together, then examine the role of antecedent soil moisture separately (and as well, whether the flood-associated precipitation is, or is not, sufficient to satisfy the soil moisture deficit by the end of the event.

Actually, this is exactly what we did, in the results section 4.1, we are analyzing the flood event properties, all events together (base flow, maximum and total precipitation, runoff coefficients, antecedent soil moisture…) and similarly in section 4.2 the flood dates without segregating events at first. We notably show that there is regional trend towards a reduction of runoff coefficient, antecedent soil moisture, together with a seasonality shift of floods.

That said, they may have stumbled on something in their finding that the fraction of events in their excess soil moisture category has decreased slightly with time, whereas the short (one-day) events have increased. The question that it would be nice to answer is whether there's been a shift in the distribution of extreme precipitation to shorter events? And if so, could such a shift (doesn't necessarily have to be just one day, could for instance be 1-2 day) becoming sufficient to exceed the initial soil moisture deficit, so that at some point (although clearly not now) this might lead to an increase in extremes. Stated otherwise, while the recent balance between increased extreme precipitation and reduced antecedent soil moisture seems to have shifted so that antecedent dryness is cancelling increased precipitation intensity, is that balance likely to shift in the future?

This is very interesting (and actually inspiring) point. It is true that changes in rainfall properties, including intensity, location, duration and spatial extend may have a strong impact on flood generation. Following your recommendation, we also tested for potential trends in the duration of the rainfall episodes, associated with flood. We plotted the result in the figure below, showing very little changes and a very messy spatial pattern. These changes in the duration of rainfall episodes are only significant in 4 stations (at the 10% significance level), and not at the regional scale. We could possibly draw two conclusions from this analysis =

1- There is likely no trends in multi-day rainfall events associated to floods in this region.
2- About short and intense rainfall events, the daily time step is most likely not appropriate to analyze potential changes.

It is known that climate change impacts the temporal sequence of rainfall events, and these dynamics should be analyzed with instantaneous, or hourly data. As we noted in the conclusion, this can be done for shorter time periods only.

Fowler, H.J., Lenderink, G., Prein, A.F. et al. Anthropogenic intensification of short-duration rainfall extremes. Nat Rev Earth Environ 2, 107–122 (2021). https://doi.org/10.1038/s43017-020-00128-6

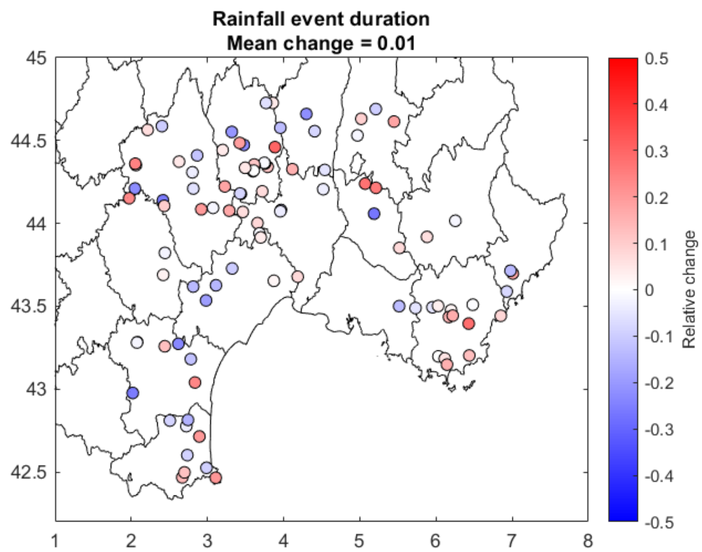


Figure 1: Relative change between 1959-1990 and 1991-2021 in the duration of rainfall events associated with floods.

One final comment: I don't see much value in the weather type discussion. What really matters to floods are a) antecedent soil moisture, b) precipitation intensity and c) precipitation duration (there are in addition factors such as storm extent and movement relative to catchment size and orientation, but these are more difficult to analyze given that the catchments are fixed by prior decisions as to where to locate gauges. So I would stick to the precipitation characteristics that matter to floods, and how they might or might not have changed.

We partly agree, since floods (in this region at least) are induced by rainfall events, and the occurrence of these events is tied to the occurrence of well-defined synoptic conditions, summarized as weather-types. So it makes sense when looking at changes in floods dates to also look at the rainfall triggering mechanisms, and actually there are quite a few studies aiming a relating flood occurrence to weather types (for example, Gilabert and Llasat 2018). We believe that it is a combination of factors, such as changes in the seasonality of soil moisture, precipitation characteristics, but also the inducing synoptic patterns that may explain shifts in flood seasonality.

Gilabert, J. and Llasat, M.C. (2018), Circulation weather types associated with extreme flood events in Northwestern Mediterranean. Int. J. Climatol, 38: 1864-1876. https://doi.org/10.1002/joc.5301