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Interactive comment on "Frequency and intensity of palaeofloods at the interface of Atlantic and Mediterranean climate domains" *by* B. Wilhelm et al.

B. Wilhelm et al.

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We thank reviewer #2 for his constructive comments. We answer below each of his comments.

MAJOR REMARKS: 1) Deposit thickness as proxy for flood intensity: The approximation of flood intensity using the thickness of the deposits does not seem to hold for Lake Foréant. As you indicate on lines 4949-2to7 grain size would be the best proxy for flood intensity. On lines 4953-20to23 and 4959-6to9 you mention that there is no significant grain-size variability that could give an indication on flood intensity, and that you therefore use the deposit thickness as proxy for intensity. As a result, you find

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that the intensity of the events during the warmer MCA is higher than during the cool LIA (Fig. 8). This is not a legitimate conclusion, as you have no proof (or calibration) showing that deposit thickness is really proportional to flood intensity. During the MCA, floods occur less frequent than during the LIA and a sediment storage effect in the catchment could therefore become highly important. For MCA floods, which occur at longer time intervals, more sediment is thus available for mobilization by the river because the catchment has not been 'emptied' for a longer period of time than during the LIA. In this context, please also see my second-last comment on the discussion part. The study with the fungal spores is helpful for ruling out anthropogenic influences on sediment mobilization, but you still have to take into account natural sedimentary processes acting in the catchment. In my opinion, the thickness of the deposits can therefore not be used as a proxy for flood intensity. As a consequence you would need to adapt the focus of this study ('Frequency and intensity : : :') and text passages referring to intensity. An additional remark regarding the grain-size results: The results are only shown as a small inset in Figure 2, and you mention it shortly on lines 4953-20to23 and 4959-6to9. However, at these positions in the text you do not show or describe your results but you add many references. The effect is that it looks like the grain-size results were published in the indicated references. Instead, I propose that you show the grain-size results more prominent in this paper.

- We appreciate this comment of Reviewer 2, as he touches a complex aspect in our study. However, we prefer to stick to the use of thickness as proxy for the following reasons: 1. We did not indicate that grain size may be the 'best' proxy. Actually there is no universal 'best way' but there are only 'site-specific' ways depending on the sedimentary environment of the studied catchment-lake system. Having said this, it is true that there are other means to reconstruct flood intensity, i.e. by assessing the amount of flood-sediment volume, which indeed was not indicated in the 'methods' section. We thus reworked the section 3.2. to clarify i) that two distinct proxies (grain size and flood-sediment volume of flood layers) have been shown by previous works to be relevant to reconstruct flood intensity and ii) that, as a result, these two proxies

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were explored in our study. 2. Previous studies in Alpine environments (e.g. Wilhelm et al., 2012b; Jenny et al., 2014; Wilhelm et al., 2015) showed through calibrations that the intensity of past floods may be reliably assessed from the flood-layer thickness when i) there is no significant variability of grain size in the flood layers and ii) when the depositional pattern of the flood layers is stable over time. As these conditions were observed in Lake Foréant, we are confident about identification of the high-intense events. These preconditions that make our study reliable are now clearly indicated in section 3.2. and 5.2. 3. Coarse particles (> 100 μm) are transported by processes acting close to the river bed (saltation, rolling, etc.; e.g. Passega, 1964) and, thereby, they are sensitive to a threshold flow velocity below which these coarse particles are deposited (cf. Hjulström curve). In contrast, fine particles (i.e. clays and fine silts that composed the Foréant flood layers) are transported by suspension in the river. As a result, trapping and storage of these particles in the small Foréant alluvial plain is unlikely making flood-layer thickness a meaningful parameter. This is supported by the relatively stable sedimentation rate of the silty sedimentary background deposits (Fig. 6) that suggests an uninterrupted sediment transport to the lake over the studied period. The section 5.2.2. was reworked to clarify this point. 4. The negligible storage effect of fine particles during floods is supported by other studies. For instance, a calibration study of the Lake Le Bourget palaeoflood record showed that the amount of fine particles transported and deposited in a lake during floods is proportional to the river discharge, even in a large catchment area where places for sediment storage are present and dams have been built during the calibration period (Jenny et al., 2014). 5. Grain-size data are used, shown and commented as other results from this study. References have been deleted from the section 'Results' (lines 4953-20to23) to avoid confusion between data of this study and others studies.

2) 'Turbidites', 'debrites' etc: My impression is that the use of the terms 'turbidites, debrites, event layers, and flood deposits' may be a bit confusing and is not consistently used throughout the manuscript. Furthermore, for the MMIT (mass-movement induced turbidite) I do not really agree why you have to separate the deposit into a turbidite

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and a debrite. Sedimentologically I might understand your aim, but still, it is one single event that leads to the deposition of the debrites and the turbidite on top. So why not simply call the complete deposit MMIT and describe in chapter 4.1 how it is composed. This would leave you with 168 flood deposits and 3 MMITs. Instead of 171 turbidites minus the 3 turbidites belonging to the MMITs. In addition, in the methods section you already speak of event layers (4948-20) that were detected by density anomalies, and of flood deposits (4949-13) that were detected by geochemical data. Is there a difference here between event layers and flood deposits? In section 4.1 you then speak of coarsegrained layers or graded beds or turbidites. In total, this is a bit confusing and I propose that you speak of detrital layers or event layers (if necessary with, for instance, the addition 'coarse-grained' or 'homogeneous') as long as you haven't interpreted the trigger. Afterwards, you can speak of flood deposits and MMITs. The term turbidite: you mention that the grading is very weak in the detrital layers. I am therefore not convinced that the term 'turbidite' is sedimentologically appropriate here. Maybe it would be better to omit the term 'turbidite' and speak of detrital layers or event layers, as outlined above.

- We followed the comment of the reviewer to avoid confusion. We deleted the terms 'turbidite' and 'debrite' and only speak of 'event layers' in the method section, 'graded layer' and 'coarse-grained layer' in the result section and, 'mass-movement-induced layer' (MMIL) and 'flood-induced layer' (FIL) in the discussion section. In addition, some parts were reworked to describe the MMILs as the stratigraphic succession of a coarse-grained layer and a graded layer.

3) XRF counts as quantitative indication of element concentrations: You use the low XRF core scanning Ca counts in the sediments as direct indication that Ca concentrations are indeed low compared to the concentration of other elements. XRF core scanning results are qualitative and many factors (e.g. sediment matrix, machine-specific properties) influence the counts. Have you calibrated the results via e.g. ICP-MS? The assumption that the counts are proportional to the element concentrations is specula-

tive, unless you have additional data to show it.

- The reviewer's comment is correct in a sense that scanning XRF counts should not be confused with absolute concentrations measured by calibrated quantitative methods. The use of the term 'content' leads to some confusion here. We are well aware of the fact that count rates/intensities derived from scanning XRF are only the reflection of relative/semi-quantitative changes in element intensities, which are mostly driven by changes in element concentration with typically minor bias by changes in sediment matrix (i.e. water content, grain-size, sediment composition). We used the same measurement conditions for all scanning XRF measurements conducted here. Tube aging- a common issue, which needs to be corrected for when using very long integration times or when measuring several 100 m long sediment successions should not be an issue given the comparably short total measurement times used in this study. Machine-specific property changes should not be an issue here. Sediment matrix effects can be a challenge in sediment successions with strong changes in sediment composition- i.e. rapid changes from calcareous to diatomaceous to clastic muds. We conducted a careful reevaluation of every XRF spectra after measurement in order to minimize bias induced by changes in sediment matrix. During reevaluation we found that changes in sediment matrix, in case of the Foreant sediment successions studied here, are very small. This is because changes in sediment composition are rather small- the sediment is primarily composed of detrital clasts with only low contents of OM, endogenic mineral phases, and siliceous fossils. In addition we only focus on elements that are typically abundant in high concentrations (>1000ppm) and that can easily be measured with the measurement set up we used in our study. Therefore we believe that our approach is far from speculation and that we can use downcore changes in element intensities measured with a state-of-the-art ITRAX XRF Scanner (acquired in 2013) as indicators for semi-quantitative changes in element concentration. In order to minimize confusion we changed the term 'content(s)' to 'intensities'.

DETAILED REMARKS: TITLE According to my point 1 above you might have to adapt.

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- See response to comment 1.

ABSTRACT You should mention in the abstract that you reconstruct mid-June to mid-Nov (i.e. summer-fall) events, this seems important.

- This has been added.

4944-5: See my comment #2 about the term turbidite.

- See response to comment 2.

4944-6: See my comment #1 about thickness as intensity proxy.

- See response to comment 1.

4944-9: What do you mean with 'typical of both climatic influences'? You need to explain a bit more here.

- This is now better explained: "typical of both Atlantic (local events) and Mediterranean (meso-scale events) climatic influences"

4944-13to14: You might want to indicate the age range of the MCA and the LIA in years AD.

- Age ranges were added.

4944-15: It is not intuitively clear that you refer to high-intensity events with 'these events'. Please reformulate. Also see my comment #1.

- This was changed to: "there is a tendency towards higher frequencies of high-intensity flood events"

4944-18: What do you want to express here? You need to specify more what you mean with 'uncertainties', 'extremes' and 'forcing factors' that you list.

- What we mean with 'uncertainties' is now explained: "Uncertainties in future evolution of flood intensity". For 'extremes', this refers to the word 'extremes' used two sentences

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above. For ‘forcing factors’, this is now explained: ‘the different climate forcing factors between the two periods’.

INTRODUCTION 4945-1: Replace ‘trigger’ with ‘lead to’.

- This has been changed.

4945-18: Delete ‘the’ in front of ‘climate variability.

- This has been deleted.

4945-27: ‘: : :: Atlantic in the north and Mediterranean in the south.’

- This has been changed.

4945-27: ‘north-western’ part of what? Of the Alps, of the Mediterranean?

- This is now indicated: ‘north-western part of the Alps’

4946-3: Please specify. It is not clear what you mean with ‘changes in atmospheric circulation’ and ‘pathways and intensity’. As a reader I would like to see here a short explanation and do not want do extract the necessary information from the references.

- This has now been rewritten: ‘. . .strongly depend on pathways and intensity of storm-tracks’.

4946-10: Rather ‘in this context’ or ‘in this framework’.

- This has been changed.

4946-14: Add the country.

- This has been added.

REGIONAL SETTING Title 2.1: ‘Hydro-climatic setting and historical flood record’

- This has been changed.

4946-17: ‘located between the northern and southern French Alps’ -> Why not ‘central

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French Alps'?

- The French Alps are divided in two parts: the northern French Alps and the southern French Alps. The term 'central French Alps' does not exist.

4946-26: Rather 'to the Queyras massif' instead of 'until'.

- This has been changed.

4947-7: 'was' instead of 'have been'.

- This has been changed.

4947-16: ': : :, whose hydro-climatic settings are characterized by the south-western and north-western flood pattern, respectively (: : :).'

- This has been changed.

4947-22: 'alluvial plain' 4947-22: 'meandering'? This seems rather characteristic for downstream river reaches in settings with a very low slope gradient and not for a mountainous area. Maybe delete the addition about the 'meandering branches' and only use 'alluvial plain'.

This has been changed.

4947-25: 'They enter the lake through only small deltas compared to the Bouchouse inflow area, suggesting limited detrital input.'

This has been changed.

4947-26: What do you mean here with 'glacial deposits'? I expect that this catchment was glaciated in the past, thus I have difficulties to believe that there are no 'glacial deposits' in the forms of sediments (moraines etc.).

There are no evidences of glacial landforms or glacial deposits in the catchment that would suggest the presence of a glacier in the past. In addition, the geological maps published by the BRGM (French Geological Institute; http://infoterre.brgm.fr/) do not

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show any glacial deposits in the catchment area (cf. Fig. 1). The sentence was modified for clarity: “There is no evidence that the catchment was glaciated in the past, i.e. no moraine or other glacial deposits.”

METHOD 4948-8: ‘was’ instead of ‘has been’.

This has been changed.

4948-9: Looking at the lake map it rather seems to be the axis between the main inflow (Bouchouse stream) and the outlet.

We changed to: “along a north-south transect in the axis of the two main inlets of the Bouchouse stream”

4948-20: ‘Bulk density was used as a proxy for identifying event layers, : : :’ The information about the time (‘deposited in a short time’) is not necessary in this context, i.e. for detecting the event layers via density, and can be deleted.

This has been deleted.

4949-1to2: What was the concentration of the hydrogen peroxide? What was the temperature of the bath? Did you control after treatment if all organic matter was dissolved (e.g. microscope, organic carbon analysis)?

The sentence was modified for clarity: “. . .in a bath of diluted (30%) hydrogen peroxyde during 3 days to remove organic matter. After treatment, microscopic observations were performed to control that organic matter was totally dissolved.”

4949-3to4: Please specify what you mean with ‘transport-deposition dynamics’.

We considered that this term was not useful and we deleted it.

4949-6: ‘Grain-size variability’ does not reflect the ‘maximum discharge volume’. Rather, the grain size is assumed to be proportional to the river discharge. Please reformulate. The sentence was reworded according to the reviewer 1 comment: “Grain-size

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variability is assumed to be related with the stream flow energy of the river entering the lake and, thereby, with the peak discharge reached during the flood event”

4949-12to13: The proposed proportionality of XRF core scanning counts and element concentration is not given for all sediments. Tachikawa et al. (2011) could show this, but it does not necessarily apply to your sediments and the XRF core scanner you used. Without quantitative measurements (e.g. calibration via ICP-MS) I therefore recommend to not interpret element quantities from XRF counts. See also my main comment #3.

See response to comment #3.

4949-23: Rather ‘grazing activity’ or ‘grazing intensity’ than ‘grazing pressure’.

This has been changed (however, the authors do not understand the difference between these terms).

4949-24: ‘: : : from the sedimentary abundance of coprophilous: : :’

This has been changed.

4949-27: What is the depth interval of the samples?

This is now indicated in the manuscript: “with an approximate step of 3 cm”

4949-28: Potentially erosive ‘event layers’ and then on the next line ‘turbidites’. This is already a lot of interpretation for a method sections. See also main comment #2.

See the response to the comment #2. The term ‘turbidte’ was deleted from the manuscript and in this method section, we only speak of ‘event layer’.

4950-6: What does ‘nb’ stand for? I assume number but it is not explained.

‘nb’ was changed to ‘number’.

4950-11: Rather ‘For dating the lake sequence: : :’

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This has been changed.

4950-13: What do you want to say with ‘matching the facies boundaries’? It is the first time that you use these terms here.

The sentence was rewritten for clarity: “The non-regular step aims at matching the facies (i.e. sedimentary background or event layer) boundaries for homogeneous samples.”

4950-27: The information about how you modeled the age-depth model should only be given after the paragraph on the paleomagnetic chronological markers.

This has been changed.

4951-3 and following: This is quite a long and detailed explanation on the paleomagnetic chronological markers. Any chance to make this shorter? Or move details into a supplement? What I am missing in this paragraph is actually how you attributed an age to your measurements.

The methods of the palaeomagnetic investigations were shorten in the main manuscript and details were moved into the supplementary material.

RESULTS 4952 first three paragraphs: See my main comment #2.

See response to comment #2.

4952-21: ‘: : : over the entire lake basin with a consistent deposition pattern.’ Delete ‘indeed’. The deposition seems not to be ‘regular’ in terms of thickness.

‘Indeed’ was deleted.

4952-24: Please specify what you mean with ‘over time’.

‘over time’ was changed to ‘over the studied period’

4953-10: What do you mean with ‘well-measured’?

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‘well-measured’ was changed to ‘other’.

4953-13: Can you provide evidence or ideas where the Fe is incorporated?

X-ray diffraction analyses were performed in this purpose. However, Fe seems to be associated to many types of clays and the measurements did not appear helpful.

4953-20to23: ‘However, since grain-size variability is insignificant, the information that can be won from this proxy in regard flood-intensity reconstruction is minor.’ Or similar.

This has been changed as suggested.

4953-24: ‘Relative Ca intensities’. Relative to what? See also my main comment #3.

‘Relative’ indeed appeared inappropriate here and was deleted.

4954-5: You have not yet introduced the abbreviation ‘FIT’.

At this stage, the trigger of the deposits is undiscussed. The abbreviations ‘MMIL’ and ‘FIL’ are only used as label in figures. This part of text and the figure was reworked in that sense.

4955-26: Abbreviated formulation with parenthesis is not intuitively clear. ‘3 (2) declination (inclination)’

The sentence was reworked for clarity.

4956-6: ‘(without event layers)’

This has been changed.

DISCUSSION Title 5.1: ‘Different triggers for event layers’

This has been changed.

4956-18: ‘Ca counts’

This has been changed.

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4956-19: Please specify here what you mean with ‘other turbidites’.

This is now specified: ‘the 168 graded layers that not overlain a coarse-grain layer’

4956-24: What would you like to implicate with these angles of delta and littoral slopes? Do you have numbers for Lake Foréant?

The sentence was changed to address the reviewer’s comment: ‘Slope angles of $< 10^{\circ}$ and $\sim 15^{\circ}$ for delta and littoral slopes of Lake Foréant, respectively, point to a littoral origin as suggested by many studies showing that slope angles $> 10^{\circ}$ are favourable for the generation of mas-movements’.

4956-4-6: These first two sentences do not belong here as they discuss event deposits in general and MMITs, but not flood events.

This has been changed.

4957-9: ‘Ca counts’. See also main comment #3.

See response to comment #3.

4957-17: So what is the effect of oxygen reaching the deeper parts of the basin? I think you should add here the information that Mn-oxides or Mn-hydroxides may precipitate because of this oxygen source.

This is now specified: ‘where it is oxidized and precipitated likely in form of an Mn-oxyhydroxide’

4957-24 and following: This last sentence of the chapter is not clear. E.g. ‘to trigger them’, not clear what is triggered?

This is now specified: ‘flood events are the most probable candidate to trigger the 168 graded layers’

4958-7to8: A lowering of the lake level still seems to be possible. This would lead to slope destabilization.

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Lowering of such alpine lake located at high-elevation would imply a negative precipitation/evaporation balance. This is extremely unlikely at this elevation (wet and rather cold climate).

4958-23: 'Only a few earthquakes' seems not to be a good expression here, since you wish 'one single strong earthquake' in the database in order to be able to attribute a specific age to the deposit.

The sentence was rewritten: 'For the older period of the third deposit, data of documented earthquakes are sparser in the catalogue, precluding a reliable assignment'

4959-7: You should specify what you mean here with 'related proxy' (in parenthesis).

We deleted 'related proxy' to avoid confusion.

4959-10 and 4959-8to11: As described under comment #1 I am not convinced that this intensity approximation via flod-layer thickness holds.

See response to comment #1.

4959-20: 'flood events' 4959-20 and following: I do not understand what you mean with 'almost absence' in regard to documented events of the Bouchouse stream and how that affects your comparison with historical records.

See Table S1, there is only one mention of flood specific to the Bouchouse stream: 'almost absence'. Hence, an event-to-event comparison between the sedimentary and the historical records cannot be undertaken.

4960-18to19: What exactly should be the difference between a 'catchment-lake system' and a 'river-lake system'? Please specify what you mean and maybe it is clearer when you omit these abbreviations.

Abbreviations were deleted: 'Erosion processes in the Foréant catchment may be affected by modifications in the river system and/or by land-use changes'

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4960-22: Rather ‘alternating activation’? What ‘record’ will be disturbed? This does not become clear from the sentence.

This is now specified: ‘may disturb the flood record’

4960-28 to 4961-11: Here you address quite well what I mentioned in my comment #1. This alluvial plain is able to store large sediment volumes, which would impact your flood-intensity record. I am particularly concerned because of the very limited grainsize variation in the flood deposits. Hence, no information at all about the hydraulic force of the river can be won (i.e. about the river discharge volume).

See response to the comment #1.

4961-21: Here you write ‘cells cm2 yr-1’, in the method section you wrote ‘nb cm2 yr-1’.

This has been changed to be consistent.

4962-8to15: Before you can draw this conclusion you should show that your record relates to the Giguet-Covex record and the former Wilhelm data. Here, it is too early to bring this proposition forward. You should therefore continue here directly with the next paragraph (line 16).

This part was deleted.

4963-3to4: See my main comment #1.

See response to the comment #1.

4963-18to19: Rather ‘: : : due to moisture advection from the North Atlantic.’

This has been changed as suggested.

4963-19 to 4964-19: You will have to reconsider this discussion on flood intensity. See my main comment #1. Most importantly, if you calculate the ratio of the number of events per century during the LIA (17) and the MCA (10) and you compare it to the ratio of the thickness (LIA: 2.4 mm; MCA: 3.8 mm) you would get very similar values:

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1.7 for the number of events and 1.6 for the thickness. Thus, what I want to point out here is that the larger thickness of MCA events could simply be due to the longer storage (thus residence) time of sediment in the catchment. Hence, you would really need the grain size as a flood intensity proxy to make a case here. In addition, you bring forward that the thickness of MCA events (3.8 mm) is 50% larger than the thickness of LIA events (2.4 mm). These '50%' depend on the perspective; one could also argue that LIA events are one third (only) thinner than MCA events.

See response to the comment #1.

4964-23to29: This discussion on possible forcing factors such as solar activity and volcanic eruptions, as well as the possibly different situation during the 20th century due to the increase in greenhouse gas concentrations, comes a bit sudden here. If you wish to keep it you have to elaborate more on the potential influence of the different forcings – even if you say that deeper analysis is necessary.

We appreciate this comment, however, we think that our concise statement should be clear. An elaboration of this in more details would be out of the scope of this paper and require a lot of space. Hence, we did not modify this paragraph.

CONCLUSIONS 4965-3: Delete 'show'.

This has been deleted.

4965-11to13: Even if there is a consistent depositional pattern in the lake basin, this is unfortunately no proof that the thickness is proportional to flood intensity.

See response to comment #1.

4965-24 to 4966-6: Again, you will have to reconsider your argumentation in regard to flood intensity.

See response to comment #1.

FIGURES Figure 2: This figure would profit from a zoom into an interval of about 10 cm

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length in order to better demonstrate how geochemical and sedimentological proxies fluctuate between normal sediment and event layers. As it is, it is hard to see how in particular the geochemical proxies vary with the occurrence of event deposits. The unit of the sediment depth is wrong: it should be 'cm' and not 'm'. I assume 'thinner' should be 'finer' (Fe/K colun).

The CPD format induced a reduced figure format that was designed to be shown on a whole page (horizontal format). 'Thinner' was changed to 'finer' and 'm' to 'cm'. Figure 3: The caption could profit from a short explanation why you are plotting this data. (I assume for illustrating the different geochemical characteristics of the different event layers.) This has been added: 'To illustrate the different geochemical characteristics of the sedimentary background and the graded layers,'

Figure 4: What is 'MP' standing for? This was indicated in the caption here '(C corresponds to the historic 137Cs peak of Chernobyl (AD 1986) and MP to the maximum 137Cs peak of the nuclear fallout (AD 1963)'. Figure 5: Here you need to indicate what the abbreviations D-1 etc. and I-1 etc. stand for. stands for. This is also necessary because the abbreviation appear in the following figures.

This has been added: 'The well-correlated declination and inclination features are labelled D-x and I-x, respectively.'

Figure 7: Please indicate in the caption that the question mark refers to a possible gap (am I correct?) in the historical data. This has been added as suggested.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/11/C2731/2015/cpd-11-C2731-2015-supplement.pdf

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