**Hydrology and
Earth System
Sciences**

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Discussions

Interactive comment on "Flood history of the
Bavarian Alpine Foreland since the late Middle
Ages in the context of internal and external
climate forcing factors" *by* O. Böhm et al.

Anonymous Referee #4

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General comments:

The authors aimed to provide an analysis on the flood history of the Bavarian Alpine
Foreland, based on the flood series of 6 rivers, from the beginning of the 14th century.
The paper is well structured, the aim is clear and well defined. The topic has high
relevance, the length of the investigated series is rather impressive. I recommend the
paper, after revising and extending certain parts, for publication in HESS. However, I
have a number of major or minor suggestions, and I think a major revision of the paper
is necessary before publication.

Specific comments:

1) In the title of the paper the authors suggests that the paper is about the "flood history" of the Bavarian Alpine Foreland. However, the content of the paper is exclusively related to the identification and analysis of flood-rich flood-poor periods; and other aspects of major significance e.g. seasonality, magnitude, flood types, analysis of greatest floods, any historical approach etc. - which one would expect to be present and discussed in a paper with this ambitious title - are completely missing. Nevertheless, I think this problem can be easily solved by the authors if they change this part of the title, and instead of "flood history" they write "flood-rich and flood-poor periods".

2) Introduction, first paragraph (p. 7411, lines 4-16): I was just wondering whether the authors left out those Central European studies that are based on long-term discharge reconstructions (e.g. Herget et al. 2010, Wetter et al. 2011), or recent Danube-related investigations (e.g. Rohr 2013, Kiss-Laszlovszky 2013) since they think it is not necessary to list them all here in the introduction, or it was only accidental.

3) Introduction, third paragraph (p. 7412, lines 4-16): It is not very common to use in English "descriptive period" for the documentary evidence coming from the pre-instrumental period. And then the sentence continues: "and has been obtained from historical writings such as chronicles and compilations." Please rephrase this sentence, because it is very problematic: here one has the impression that the authors are mixing compilations (that is a collection of data taken/excerpted from historical sources, i.e. it is not a source type) with chronicle (which is an important source type from the group of narrative evidence). Mixing together these two terms suggest in this form as if the authors were not aware of the fundamental difference between these two materials (i.e. also the quality difference of these two materials in their own database), which is - I am sure - not the case. This problem otherwise also appears in Chapter 3 (Database). Out of the 6 rivers studied, four are direct tributaries of the Danube, while the remaining two (Wertach and Salzach) are tributaries of two of the mentioned tributaries (Lech, Inn). Thus it would be important to know: How did you merge (what method did you use)

the 6 flood series? It would be great if you provide here a basic overview figure concerning the number of flood events related to each studied rivers (in one graph, maybe rivers indicated with different colours). This would give basic information to the reader about the overall weight of the individual rivers in your reconstruction (and it becomes especially important concerning the early period when less data are available). It is also important to know how you treat and calculate in the overall series when all your studied rivers were in flood in the same time (i.e. 1 event or 6 events etc.).

4) Chapter 2: I have overall good opinion about this chapter: it is short, concise, and still all the important aspects are included.

5) Chapter 3 Database: first paragraph on the applied source materials (p. 7414, lines 10-26) Based on the short description and the rather strange use of historical terminology, one's first impression is that the authors are not really familiar where their data exactly come from. Although I am sure this is not the case. It is fine that there was a funded research project, and the cooperation with the largest German historical climatology databank, the HISKLID, is impressive. However, the authors have a couple of 'dreadful' sentences here, and without a complete rewriting of these sentences this paper should not be published. E.g. "The evaluated written evidences originated from handwritings and chronicles (e.g. .... the comments to this comes later....), annuals, historical print media, compilations." What do you mean under "handwritings"? Do you mean the term "manuscript"? If you mean all hand-written documentation, then you should specify it more according to types (i.e. narrative sources such as chronicles, annals, diaries; or e.g. letter, charters; or leg-administrative sources such as accounts etc.). Because "hand-written" in itself gives us basically no information about the sources applied (or its strength or weaknesses). Moreover, naturally, all chronicles (and any other written materials) until the 1470s are hand-written, and even in the next centuries most of the chronicles and many other sources are hand-written (often later printed). In brackets you mentioned as an example for the origin of "evaluated written evidence" the publication series of the Historical Commission of the Bavarian Academy

of Sciences, in 37 volumes. I presume you mean the "Die Chroniken der deutschen Städte" series, including narrative source evidence (contemporary, non-contemporary mixed) referring to the 14th-16th centuries. If this is the case, maybe it would be useful to refer to the series title itself here, and also add it to your bibliography (and from this series only some of the volumes actually refer to Bavaria). And it would be useful to refer to 1-2 other major source edition series (e.g. with more critical source evaluation such as the MGH) you most probably also applied in your analysis (in the same way as you also referred to a number of compilations later). What do you mean under "annuals"? Probably it is a mistyping instead of the term "annals", a large source group of narrative evidence. What do you mean under "print media"? This is general and unspecified: please, provide the main source types (or groups) you used. E.g. do you mean newspapers, pamphlets, journals or also printed scientific works, narratives etc.? Please, also check whether the "united leaflet database (...; Ferdinandeum Innsbruck Administration of Inheritance)" is the correct English term of the database and the institute you mention. And finally, again the basic problem of listing (and mixing) primary sources together with compilation. Please make a clear distinction here as it is a totally different thing: so, please, mention the applied compilations in clear separation from original, primary sources. I also recommend to have a language check here. In general, I suggest to rewrite the first long paragraph, and I recommend to be more careful and provide a more elaborated, clear description about the fundamentals of your database, with an appropriate use of basic historical source terminology.

5) Chapter 3 Database: Concerning the rest of the chapter, the homogenisation of instrumental series as well as merging between overlapping periods are well-elaborated, and the brief description of the 3-scaled index classification is clear and appropriate. I only have one little comment here: maybe there is a simple mistyping in line 17 (p. 7415): here most probably you meant "flood descriptions" instead of "weather descriptions". I also find it very positive that the authors in their datasets have an appropriate overlap (1826-1880) between the pre-instrumental/documentary period and the instrumental period. I have some comments to the method described in the last paragraph of

the Chapter (p. 7416): I understand that some researchers use datasets regarding the pre-instrumental period "with and without source-critical evaluation", i.e. no evaluation of individual sources and historical quality-homogenisation (only statistical) have been carried out. It is also true that in this way much less data is filtered out (compared to proper historical critical evaluation), and as a result potentially a larger dataset can be gained. However, this approach has some significant dangers which can significantly alter the analysis results at the end. So, I appreciate the fact that the authors mention that they use the "non critical approach", but with this comment the problem is still not solved. I also agree with the authors that some of the pitfalls can be avoided with "a methodological practice verified inter alia by cross-comparison with verified records". In the (later) periods when great amount of (detailed) evidence is usually available concerning each single flood events (e.g. for medium- and large-scale events) this approach might work. Nevertheless, the early periods with less and less contemporary source evidence and more non-contemporary sources (with copying each-others dating mistakes, and in this way potentially doubling, trippleing flood events; and this is especially true for the Middle Ages), cross-comparison in itself does not really provide much help in identifying wrong dating etc. And this has the consequence that uncertainties greatly increase in the early part of the series, and therefore the validity of the analysis results concerning the early period can be basically questioned. As we could see earlier, the authors did not make a clear differentiation between primary sources and compilations (collections taking data from primary sources). This makes a bit also unclear what they cross-checked with what, i.e. what they mean under "verified records"? For example, if a compilation data is cross-checked with a chronicle data, then it can easily happen that we compare the same information (i.e. the compilation uses the same chronicle, and then we check the same thing with itself - I do not mean that is what the authors actually did, but probably such questions about the methodology could be briefly clarified). I think these raised uncertainties/questions can be partly solved after clarifying the first part of their database chapter, and when the authors provide some more information (i.e. a couple of more sentences) on how they did the

verification in practice.

6) Chapter 4: Methods I have some comments to the end of the chapter: a) Lines 15-20 (p. 7417): The authors specifically refer to the 1501 flood mark in Passau located at the Fischmarkt. Recently, this flood mark was moved significantly higher from its earlier place. Do you use the old maximum water level mark or the new one in your analysis? b) Last sentence: "Different databases and data densities (e.g. 14th/15th century - the period of the Renaissance - beginning of the instrumental period) were thus considered as well as possible." It is not very fortunate to mix dates with cultural periods because from the sentence it is not clear what time span you mean. 14th-15th century is clear, but the Renaissance as a cultural period was already present in the 15th century in Europe (even if not in all parts, of course), and there are other cultural periods in Europe before the beginning of the instrumental period (thus, you have not defined the beginning and end of the referred period). It would be just easier and more clear to give simply centuries/dates.

7) Chapter 5: Results Could you please describe the method ("Polynomial function of the 5th degree") you applied in the identification of flood-rich flood-poor periods? The application of this method has great importance in your overall analysis and fundamentally affects the results. It would be also important to know why the authors chose this particular method (and why not others, why the authors think it is better for their purposes than others), and what are the basic advantages and disadvantages of the applied methodology? The authors properly refer back to the paper written by Glaser and Stangl (2003b), but - checking back that particular paper - the method itself and why that method was chosen are not described there either. And, just by a simple look at your Figs. 3 and 4 (but esp. Fig. 4), it is not obvious at all whether the break points you identified by using the applied method really identify in each case the beginning and the end of a flood rich period (or they identify something else). Lines 16-17 (still on p. 7418): "A rising data density after the mid-15th-century must be seen in a context of the intervention of letterpress." It is not a "must be seen" question. There are also

other important historical-cultural reasons for this change. So, if you want to keep this sentence, then at least please add: "amongst other important reasons" (or something alike).

8) Sub-chapter 5.1: Flood-rich period #1: 1300-1335 Due to very low density of data, I think this period should be discussed in more detail. It would be useful to include briefly: How, based on what flood evidence in your series, did you define the beginning (and the end) of this period? Concerning this flood rich period there is very low number of data available (Fig. 3), and this problem was (I presume on the earlier general information) solved with using interpolation (see also Fig. 4). I have a number of problems here: a) In the early part of the 14th century contemporary sources referring to floods in Bavaria are only exceptionally available (please, correct me if I am wrong): most flood information known (e.g. especially those included in the historical editions series you mentioned "Die Chroniken der deutschen Städte") comes from later chronicles with dating errors etc. If I see it well in Fig. 3, your few data concerning this period is accumulating around the mid-1310s, i.e. the years 1315 and/or 1316 (+-1-2 years), which are also known as the famous flood years on the Danube, and then 1-2 after this period (maybe one in the 1320s? and 1-1 in the 1340s? or around 1350?). So, in the original database, represented in Fig. 3, you basically have a few flood data around the mid-1310s, and then only 1-1 individual events scattered. Just by simple thinking: how can from this data a defined flood rich period of 1300-1335 come out? And then we have not yet even considered such questions that, due to very low density of sources, you might have data doubling or trippleing due to simple misdating of non-contemporary authors, because here there is not too much contemporary to check with (or maybe there is, but then it would be really important to specify, because that would support the validity of you work concerning this early part). And since the analysed series is defined based on 6 flood series, then it can also happen that one single flood (which affected all rivers in Bavaria) and/or its misdatings make up for you this relative flood-rich period. Simply saying: unless the authors prove the validity of this flood-rich period based on their data and the sources, due to the very low number of data

and the high uncertainty quality-level of their data (also due to the applied verification methodology) concerning the early 14th century, the authors can maximum suggest this flood period as a hypothesis, but not as a period based on realities (i.e. you either give more proofs why it is really a flood rich period, esp. concerning the 1320s and early 1330s, or I would recommend to leave it out or mention it only as a hypothesis). It is another question that in the broader neighbourhood of the study area this period is - with the clear exception of the decade of the 1300s and 1310s - not really among the particularly humid periods (e.g. see the related literature referred in the Introduction). To a lesser extent, this is still also valid for the next medieval flood period. But - in general - from the later parts of the 14th, and especially in the 15th century more contemporary sources are available also in Bavaria, and this makes analysis more possible (i.e. no critics here).

9) Chapter 5.3 (p. 7420), last sentences (but also implies on 5.10.2): Which correlation do you use while comparing your series to the NAO indices (and why)?

10) A short question to Fig 3: What does the grey line (with the question mark) mean there?

11) The "6 Discussion" chapter looks rather like a "Conclusions" chapter: maybe it would be useful to rename it accordingly. However, I do agree that a Discussion chapter (with different content) would also have relevance here. For example, the authors have not discussed some important and potentially interesting questions; e.g. they have not compared their results to any other results available in Europe or at least in the neighbouring areas. This, rather reasonable, comparison part is completely missing from the paper, and should be included. The authors compared their results to e.g. sunspot numbers, but - compared to this - other elements, for example, a systematic comparison with volcanic eruptions would be also probably worth for a paragraph in the Discussion (this is only a suggestion; sometimes the authors mention period "triggered by multiple volcanic eruption". However, some of the greatest eruptions happened in their flood poor periods etc.). It is also interesting, for example, that the 1780s -

particularly flood-rich on large sections of the Danube - is part of a flood poor period in the Bavarian Forelands flood reconstruction.

Based on the above-mentioned reasons, as reported at the beginning, I suggest major revision. However, I would like to stress that I find this paper as an important contribution, and - after some necessary changes - clearly worth for publishing in the related special issue of the HESS journal.

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