Greetings to the authors,

The reviewer begins by thanking them for the additional effort in trying to improve the manuscript.

However, by analysing the revised version, it has emerged that the authors maintain the three fatal errors raised in the previous review:

1) Failure to explicitly mention the caveats of the reconstruction of atmospheric fields up to several centuries back in time from a simple statistical extrapolation from XX century data.

2) Attributing the most dominant principal component of the atmospheric pressure fields over summer to the North Atlantic Oscillation, which is plain wrong.

As noted in the first review:
"
What is then the problem with the "Summer NAO"?

In a nutshell: the first principal component of the geopotential height field at 500 hPa (Z500) only represents the NAO in Winter. In fact, that is the only season in which the NAO is the dominant circulation pattern. Over summer the NAO is definitely not dominant. Instead, other processes take over and it is their imprint, not of NAO, that is seen in the first principal component of the summer climatologies of Z500.

In detail: There is a fundamental problem in the identification of the large scale atmospheric driver North Atlantic Oscillation (NAO). The authors mention a positive anomaly (high-pressure) centre over the North Sea (between Scandinavia and the British Isles), and a negative anomaly (low-pressure) centre over the Mediterranean.
Actually, the centres of action of the NAO lie over the Atlantic, not over the North Sea or the Mediterranean sea. The high-pressure centre is the "Azorean High", over the North Atlantic area around the Azores archipelago, and the low-pressure centre is the "Icelandic Low", over the North Atlantic area around Iceland.
The high-pressure centre close to Scandinavia is known as the Scandinavian High and represented by the Scandinavian Oscillation (SCO) Index (you can read about it in the atmospheric physics literature of at the NOAA websites).

As noted before, when performing Principal Component Analysis for the extraction of EOF of the sea level pressure fields, the NAO pressure anomaly pattern will only be dominant during Winter (e.g. December to February). During Summer other patterns take over, e.g. the SCO. This is why the first EOF over summer is no longer NAO-related, rather having completely different centres of action. Calling that "Summer NAO" is thus plain wrong.

This being said, the solution to this problem is within the authors' reach: all what the authors need to do is to remove the ill-named Summer NAO or SNAO and reinterpret the summer patterns in the light of mechanisms that actually play a dominant role at that time.
"

Just because some ill-founded literature commits the "Summer NAO" mistake, there is no excuse to perpetuate such a senseless error after the problem has been raised in the first review. A benefit of doubt had been given to the authors in the first version, but by now the error should have been corrected. Without this correction, the paper is fatally flawed.

3) On calling EOF of the geopotential height field "atmospheric circulation patterns".

Circulation patterns are wind patterns, not pressure or geopotential anomalies.

The EOF of the geopotential height field are statistical variability patterns in atmospheric geopotential height fields relative to the climatological average taken into consideration for the PCA analysis, as noted in the first review. These patterns do indeed inform indirectly on circulation (wind) patterns but are not circulation patterns per se.
By insisting on presenting pressure anomaly patterns as circulation patterns, the authors are leading the readers into a misunderstanding.

These matters are well-known textbook knowledge in undergraduate atmospheric sciences.
Still, the reviewer acknowledges that these matters may not be obvious to researchers from other fields. This is why the review of the first version had been very constructive and pedagogic.

Addressing atmospheric science matters is more than welcome and enthusiastically encouraged. However, these matters must not be taken lightly. Either they must be properly worked on, or simply removed from the manuscript.

By seeing that the authors insist on the listed mistakes despite the kind recommendations for correction, the reviewer sees no other option than to reiterate the same matters and urge the authors to be open: this is the only way that lessons can be learnt and the good promise raised by this study actually come to fruition.

Aside from the previous notes, an additional major issue has arisen from the second version of the manuscript:

4) PCA of the flood matrix:
The rotation and inversion of factors leads, allegedly, to a visual diagnostic suggestive of the geographic location of the Swiss cantons.
4.1) It should be clarified under which grounds the transformation is made to the principal components, so that it will not be perceived as purely arbitrary by the readership.
4.2) The PCA is only optimal in normally distributed data. There is no statement on the flood datasets being normally distributed (by the way, they are clearly not). Therefore, the plain use of PCA the way it is done in the paper is questionable at best. Care must be exerted when applying such geostatistical techniques, for the patterns they yield have no physical meaning otherwise.
4.3) No dynamics can be inferred from Principal Component Analysis. Only statistics (e.g. statistical variability). The authors' mention of dynamics being inferred is thus completely flawed and misleading.

All things considered, the authors are strongly encouraged to revise the manuscript and address all of the raised matters. The manuscript may only be considered for publication if, and only if, the authors proceed with all the necessary corrections.

Good luck!