I have carefully read the manuscript entitled “Global to regional overview of floods fatality: the 1951–2020 period” whose main focus is the description of simple analyses of the number of fatal flood events and on the number of flood fatalities per event carried out at nationwide scale. The work is based on the available EM-DAT Emergency Database. The results draw a picture of the global situation of the impact of flood events on the global population even if they limited their analyses to the subset of fatal events with casualties between 10 and 9999. The authors selected this magnitude threshold for the flood fatal events since the research hypothesis is focused only on disastrous floods.

The topic is of extreme importance and the effort made by the authors in trying to compare the various data at a global scale is remarkable, but in my opinion not sufficient to make the manuscript acceptable. A global analysis is certainly very demanding and involves the elaboration of huge amount of data - demographic, economic, social, climatic and physical data - which, really, I did not find in the manuscript. At present, the text is a description of the analyses performed and the true purpose of the work does not emerge at all. Due to the lack of a clear aim, the manuscript has some weak sections including the method and discussion. What is missing is a broader discussion focused on the importance of the results presented in the context of the extensive literature available on these topics in light of the climate change context.

My general comments:

The source of data the authors used is available at global scale, and it is a good point, but it can’t be considered a comprehensive overview of the impact that floods produce to the population for most of the countries. The very frequent but less disastrous fatal flood events occurring every year anywhere in the world were not recorded in EM-DAT since the difficulty in capturing the information does not allow the minor events to be recorded. We
do not know how much these minor events cumulatively amount to. Moreover, the EMDAT casualties estimates are not always consistent with those available in other sources and, frequently, these estimates include among the flood fatalities those due for other factors. The authors do not at all discuss the problem of the estimates uncertainty and how it might affect their results.

I wonder if it makes sense to compare data on flood fatalities on a global scale when the geographical, demographic, social and economic characteristics of different nations are so profoundly different. Can the GDP as unique parameter explain the relation between the flood fatalities and the effort done to increase knowledge and technologies to mitigate the effects of flood events? Maybe the authors should consider additional socio-economic parameters and assess, for example, the social vulnerability of the single countries. I can support the authors in considering administrative limits as important in calculating true fatality coefficient of floods in order to compare them with each other. But this is true only in relation to the economic and political conditions of each country and how government choices can affect flood risk management. However, the administrative limits have nothing to do with the risk zoning where the people, who are most at risk, live. How did the data were spatialized? I bring this question to the attention of the authors since the distribution of the population within very large countries cannot weigh as much as the distribution of smaller ones, where the average density is approximately equal throughout the national territory. Have they considered this aspect? to strengthen the work, it could be helpful the estimation the real number of the population exposed to risk using, for example, the dasimetric maps.

To compare the human losses caused by floods in the different countries, the authors firstly used the fatality coefficient of floods calculated by dividing the total number of death tolls by the number of flood events for each country. I cannot understand if they calculated this coefficient year by year (for the 70-year period) and then they used the average in the long period, or if they simply divided the total number of fatalities by the total number of fatal event.

This consideration points to my second comment concerning the meaning of disastrous event. How can we define disastrous an event at global scale without considering at least the different country population density? Is it possible to quantitatively define “disastrous” merely with the number of fatalities for event, and is this parameter actually useful for comparing data across countries of different size and population density? To define a disastrous event the authors could make a greater effort and estimate a moving thresholds, weighed both on the number of victims per event and on the population density.

Even if the authors attempted to consider the population density and the number of events per unit area (plots in figure 10), it is not clear if they have used the population data by nation and year by year and if the relation they found has changed in time, or if they have counted only the average in the long period. To overcome this weakness, the authors could calculate for the investigated period (1951–2020) the average mortality rates - at nation level - which are given by the number of fatalities recorded every year in a single nation, scaled to the size of the population in the related period of time. The mortality rate is normally used to compare the impact on the population of technological,
health and even natural hazards. The authors could rank the nations on the basis of the mortality rates. They should use the annual population data for each country for the 70-year period. If the annual data are not available for the long period, they could reduce the time period.

In the discussion section the authors state that reducing flood will reduce the number of fatalities. This is conceptually wrong. It is possible reducing the impact, the losses and the intensity of a flood events, with structural and non-structural mitigation measures, but not the number of the physical processes, since it is due to the combination of many physical variables, firstly the rainfall intensity.

I would also like to point out the lack of thoroughness description in the way the authors present their results and outputs. The plots they produced are poorly described and not properly discussed and, in some cases, the citations of the figure numbers in the text are wrong.

Comments on the manuscript structure:

The data and method section is too poor and the method they used is not described anywhere, they only described why they selected the subset of data from EM-DAT.

The list of events they include in section 2 should be removed or moved in an appendix section, together with table 1.

Most of the results are in the discussion section. The authors should better define what they consider as results of their analysis and what they want highlight and discuss

The conclusion should be rewritten.